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ABSTRACT

Investment Tax Credits and the Response of Firms*

This paper estimates the direct effects of investment tax credits on firms' production behavior and the additional indirect effects arising from agglomeration economies. Exploiting a change in tax credit rates by firm size in Germany, I find that manufacturing firms increase capital and employment, with labor demand in information and communication technology-intensive industries shifting towards college-educated workers. Using geolocation data, I show that agglomeration benefits lead to a sizable further firm production expansion with these benefits materializing within distances of 5 kilometers. Worker flows from the service sector and from non-employment, rather than between manufacturing firms, explain the employment effects.

JEL Classification: D22, H25, H32, J23, R11

Keywords: investment tax incentives, capital, labor demand, agglomeration

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1 Introduction

Governments have long used tax policy to stimulate economic activity and with the accumulation of capital thought to be key for economic growth, they often rely on investment tax credits and similar tax incentives that reduce capital costs. Their belief is that cheaper capital investments encourage the expansion of overall firm production, and consequently lead to the creation of new jobs. Apart from such direct effects, policymakers often argue that indirect effects can generate substantial additional capital and employment growth throughout the economy, with regional agglomeration spillovers from intensified production considered as one important channel.

Recent findings in the tax literature suggest that reducing capital costs indeed increases firm investment (Zwick and Mahon, 2017; Maffini et al., 2019; Ohrn, 2019; Liu and Mao, 2019). However, the effects of investment tax incentives on other firm outcomes, such as labor demand, have thus far received limited attention. With new production technology having the potential to replace overall labor (Karabarbounis and Neiman, 2014; Acemoglu and Restrepo, 2018) or that of particular skill groups (Autor et al., 2003; Lewis, 2011; Goos et al., 2014; Michaels et al., 2014), the impact on employment outcomes may be very different from policymakers' expectations. But even broad positive direct effects and additional indirect agglomeration effects may be misleading at the firm level. Given that tax policies are often targeted at specific groups of firms, the effects may instead occur due to shifts in production capacities.[1] Firms with more favorable tax provisions may benefit at the expense of others, creating small aggregate effects or even efficiency loss due to misallocation (Hsieh and Klenow, 2009; Garicano et al., 2016).

To assess these different adjustment mechanisms, I estimate the effects of investment tax incentives on a broad set of firm outcomes, including capital stock, employment, employment composition, and sales. I not only quantify the direct effects of a capital cost reduction but also investigate the additional indirect effects arising from agglomeration economies, and explore the redistribution of workers as a measure for production shifts across firms.

[1]For example, some tax policies provide better tax provisions for small firms (Maffini et al., 2019; Benzarti and Harju, 2020; Moon, 2020), or implicitly favor specific industries, as with accelerated depreciation policies (Zwick and Mahon, 2017). Similarly, place-based tax policies target firms within particular regions (Slattery and Zidar, 2020).

To do so, I analyze an investment tax credit policy introduced in 1991, directly after German reunification. This policy was one of the leading support programs for firms in the former East Germany—with annual government expenses of around 1–2 billion euros per year ($1.15–2.3 billion)—and aimed at mitigating the considerable economic differences that had developed during Germany's division.[2] A policy change in 1999 allows me to identify causal effects. Before 1999, manufacturing firms with up to 250 employees were eligible for a tax credit rate of 10%, while those with more than 250 employees received a rate of only 5%. In 1999, changes to these rates amplified this differential treatment in favor of firms below the cutoff. The tax credit rate increased to 20% for firms below the cutoff and to 10% for firms above it, generating a relative decrease in capital costs for smaller firms.

This policy change allows me to separately estimate the direct and indirect effects of investment tax credits. Guided by a theoretical framework that incorporates agglomeration economies and local labor supply in a basic model of labor demand, I quantify the direct effects by comparing the differential behavior of firms below and above the firm size cutoff in a difference-in-differences setup. For the indirect effects, the framework establishes a link between the effect size and the regional employment share of firms below the cutoff receiving a relative cost reduction. I thus combine the direct effect estimation with a difference-in-differences approach that compares firm behavior across regions according to this share. Because both agglomeration economies and local labor supply impact firms at the regional level, this indirect effect estimation leads to a combined effect, which I further break down by analyzing the responsiveness of local wages.

The empirical analysis relies on unusually rich data, including detailed information from administrative survey and matched employer-employee data on variables such as employment structure and worker flows for almost the entirety of German manufacturing establishments. Firm identifier information facilitates the aggregation of the establishment-level data. To obtain the precise location of establishments, I further draw on geolocation information from address data. With this information, I estimate the influence of indirect effects not only within predetermined administrative regions but also flexibly across space.

[2]In the U.S., investment tax credits played a prominent role at the national level between 1962 and 1986, with government expenses totaling $21 billion in 1985 (Chirinko, 2000). At the state level, 40% of U.S. states offered investment tax credits in 2004 (Chirinko and Wilson, 2008).

The empirical results show substantial positive effects of investment tax credits. Manufacturing firms below the cutoff with a relative reduction of capital costs increase their capital stock by 14.5 log points, employment by 10.3 log points, and domestic sales by 9.6 log points compared to those above the cutoff. As labor inputs and domestic sales increase in similar magnitudes, the effect on labor productivity is close to zero. To investigate whether skill-biased and routine-biased technological change leads to different demand effects by worker types, I analyze employment according to the skill level of workers and the routine-task content of occupations, and find similar effects across all groups. When I divide firms by their industries' reliance on information and communication technology (ICT), I find a sizable gap of 8.8 log points in the demand response for college-educated compared to non-college-educated workers in industries with high ICT capital shares. The employment effect for non-college-educated workers remains positive in this case, highlighting a broad labor demand response.

In addition to these direct effects, all firms expand their production through the considered indirect adjustment channels. For the average East German region in terms of the employment share of firms below the cutoff, the policy change leads to indirect firm capital growth by 15.8 log points and employment growth by 6.6 log points. To determine the impact of inelastic labor supply on the indirect effects, I examine firm wages across regions. The effect is small and statistically insignificant, pointing to perfectly elastic local labor supply and to the indirect effects being completely driven by agglomeration economies. Redoing the analysis for employment by using distance measures instead of administrative regions reveals that the agglomeration effects are highly localized, occurring only in intervals of 0 to 2 kilometers and 2 to 5 kilometers ($\sim$1 to 3 miles).

In a final analysis, I investigate the redistribution patterns of labor across firms. I build counterfactual employment measures for different groups of worker flows and assess how much of the total effect each can explain. The results show that job-to-job transitions are economically important, explaining 63% of the direct effect and 17% of the indirect effect. Flows to and from non-employment are important as well, as they explain the remaining share of the direct and indirect effects. As for job-to-job transitions, worker flows to and from the service sector constitute a sizable share of the direct effect, whereas flows between manufacturing firms are not important. Most importantly, I find no evidence that shifts between large and small manufacturing firms or across regions drive the employment effect, countering the concern that unequal tax incentives lead to a redistribution of production in the manufacturing sector.

The research setting allows for various robustness checks to the empirical strategy. First, when I estimate the effects by year, firms show no differential pre-treatment behavior for either the direct or indirect effects, thereby supporting the common trends assumption underlying the difference-in-differences approach. Second, results are robust to different sample selections and fixed effects, and remain unaffected when I control for changes in a competing firm support program. Third, in a placebo test in which I redo the analysis with a set of comparable firms in West Germany, coefficient estimates are close to zero and statistically insignificant.

This paper contributes to several strands of the economic literature. It follows advances in the tax literature in studying the investment responses of firms by exploiting cross-sectional variation (Cummins et al., 1994; House and Shapiro, 2008; Yagan, 2015; Zwick and Mahon, 2017; Maffini et al., 2019; Ohrn, 2019; Liu and Mao, 2019; Moon, 2020).[3] I add to these studies by extending the set of outcome variables beyond capital to create a more detailed picture of firm adjustments to capital cost changes. By analyzing different labor types and interactions with ICT at the industry-level, I also add to the literature on skill-biased and routine-biased technological change (e.g., Katz and Murphy, 1992; Autor et al., 2003; Acemoglu and Autor, 2011; Goos et al., 2014; Jaimovich and Siu, 2020). I show that whereas investment tax credits can shift the relative labor demand for skill, the expansion of production prevails and leads to positive employment effects for all types of workers.

A further strand of the tax literature uses aggregate data to examine tax policy (Romer and Romer, 2010; Mertens and Ravn, 2013; Suárez Serrato and Zidar, 2016). Using accelerated depreciation allowances in the U.S., Garrett et al. (2020) study the effects of investment tax incentives on labor market outcomes. However, their use of aggregate data masks individual firm responses and does not disentangle the adjustment margins that are relevant at the labor market level. I complement this research by showing the importance of agglomeration economies for linking firm-level evidence with findings at aggregate levels. I further exploit worker flows to shed light on reallocation patterns

[3]The general literature on the investment effects of tax policy is much broader. Important early contributions are Hall and Jorgenson (1967), Abel (1980), Summers (1981), Hayashi (1982), Chirinko et al. (1999) and Desai and Goolsbee (2004).

across firms as another important factor in the aggregate response.[4]

At the firm level, recent evidence for the direct effects of investment tax incentives on employment at the firm level comes from accelerated depreciation allowances in the U.S. Results are mixed, with Tuzel and Zhang (2021) finding a shift towards skilled workers but no effect on total employment. In contrast, Curtis et al. (2021) find a positive response on total employment. I broaden the understanding of firm adjustments in a setting outside the U.S. context and additionally determine the indirect effects of a capital cost reduction.

This paper is further related to the literature on agglomeration economies, which are central to the understanding of localized production and urbanization.[5] Yet few studies have exploited quasi-experimental variation to identify these effects, given the difficulty of finding appropriate settings. At the firm level, Greenstone et al. (2010) and Gathmann et al. (2020) take advantage of large firm openings and closings, respectively, to explore the impact on firm production in affected regions. I contribute to this literature by exploiting variation from tax policy, thereby focusing on a context that has not been considered before. By analyzing agglomeration effects not only within administrative boundaries but also with distance-based measures, I additionally provide new evidence on the attenuation of such indirect effects (Rosenthal and Strange, 2003; Arzaghi and Henderson, 2008; Ahlfeldt et al., 2015).

Finally, with the regional focus of the German investment tax credits, this paper is related to the literature on place-based policies (e.g., Glaeser and Gottlieb, 2008; Kline and Moretti, 2014b; Neumark and Simpson, 2015), and their empirical evaluation (Busso et al., 2013; Kline and Moretti, 2014a; Criscuolo et al., 2019; Siegloch et al., 2021). The combined evidence of this literature points to sizable positive effects of such policies on regional employment and the existence of agglomeration spillovers across sectors. However, place-based policies commonly combine various regional and firm-specific incentives with an allocation of funds through application procedures and discretionary decision making. In contrast, by examining investment tax credits, I identify the effects of a reduction of capital costs through tax deductions. In this context, my results show that agglomeration spillovers within a sector can be an additional channel for shaping regional employment

[4] Similarly, Giroud and Rauh (2019) show the importance of reallocation within firms across state borders for explaining aggregate effects of state taxation on business activity.

[5] See, for example, Ciccone and Hall (1996), Glaeser and Maré (2001), Ellison et al. (2010), and De la Roca and Puga (2017). Combes and Gobillon (2015) provide an overview of empirical strategies and the difficulties involved in identifying agglomeration effects.

effects.

The remainder of the paper is structured as follows. Section 2 introduces the policy and the variation used for identification in the empirical analysis. Section 3 describes the theoretical framework that lays out expected firm behavior. Section 4 explains the estimation strategy. Section 5 provides an overview of the data, sample selection, and descriptive statistics. Section 6 presents the empirical results, and Section 7 concludes.

2 The German Investment Tax Credit Program

In 1990, Germany garnered worldwide attention by reuniting West Germany and East Germany. However, stark differences in their political and economic development throughout their separation led to sizable regional inequality. In 1991, East Germany had 43% of the GDP per capita of West Germany, 46% of the capital stock per worker, 57% of the earnings per worker, and an unemployment rate of 9.5% compared to 5.8% in West Germany.[6] To mitigate economic differences, the German government provided considerable financial support. The investment tax credit program (*Investitionszulagengesetz*) started in 1991, immediately after reunification, providing tax credits to firms located in East Germany to reduce capital costs for equipment and structures. While at the beginning, firms in all industries were eligible, by 1997 the program almost exclusively targeted the manufacturing sector.[7]

Tax credits were generous, with rates ranging from 5% to 27.5%. For example, a firm that was eligible for the highest rate would receive a 275,000 euro ($320,000) cost reduction on investments of 1 million euros ($1.15 million). Tax credits are usually deducted from tax liabilities and cannot exceed them. However, in the German program, tax credits were refundable, meaning that firms received payments at the end of the business year irrespective of tax liabilities. The tax credit rate therefore provides a simple and comprehensive metric for the reduction of capital costs in this context.

Figure 1 summarizes the related government expenses. From 1992 to 1995, they amounted to roughly 2 billion euros per year ($2.3 billion). In line with restricting

[6] The figures are based on official statistics from the German Federal Statistical Office and the Federal Employment Agency.

[7] Retail businesses continued to have limited eligibility until 2001. Manufacturing-related service businesses, such as industrial design services or laboratories, gained access to tax credits in 1999. Businesses in accommodation services (e.g., hotels) were eligible from 2007.

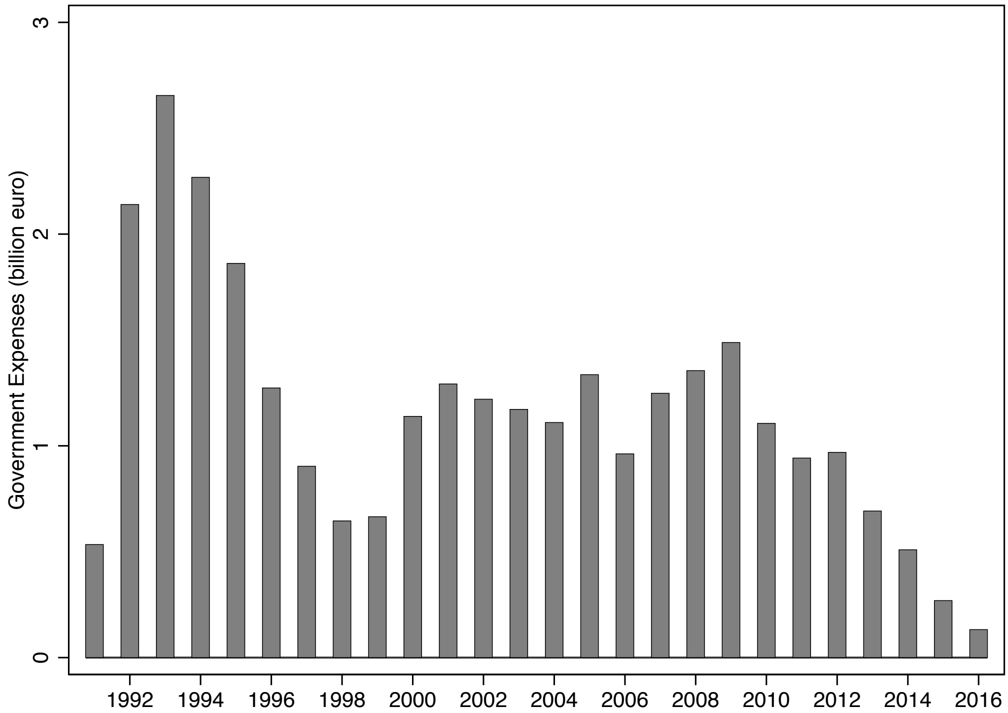

Figure 1: Government Expenses for Investment Tax Credits in Germany by Year
Note. Data collected from the subsidy and tax relief reports of the German government. For 1991–1998, expenses are converted to euros based on the official fixed exchange rate. When several reports provide information for the same year, I select expenses from the most recent report.

eligible industries, yearly expenses declined over time, dropping to 600 million euros ($690 million) in 1999. These expenses fluctuated around 1 billion euros ($1.15 billion) per year thereafter and faded after the program ended in 2013.

In the empirical analysis, I focus on manufacturing firms in East Germany, excluding Berlin. For these firms, a change in tax credit rates for equipment investments occurred at the beginning of 1999. Although this change was announced in August 1997, disputes with the European Union (EU) delayed final approval until the end of 1998.[8] Figure 2 depicts the typical equipment tax credit rate for this subgroup for 1995–2004. Even before the policy change, tax credit rates differed by firm size, defined as the headcount of all employees at the beginning of a business year, excluding vocational trainees. Manufacturing firms with up to 250 employees received a tax credit rate of 10% on all equipment investment, whereas those with more than 250 employees received only 5%.

In 1999, tax credit rates were raised for modernization investments, with firms below the employment cutoff receiving a rate of 20% and firms above the cutoff receiving 10%.[9]

[8]Using a flexible estimation specification in the results section, I find no evidence for anticipatory behavior of firms.

[9]At the same time, an investment limit for receiving the higher tax credit rate of 2.56 million euros per

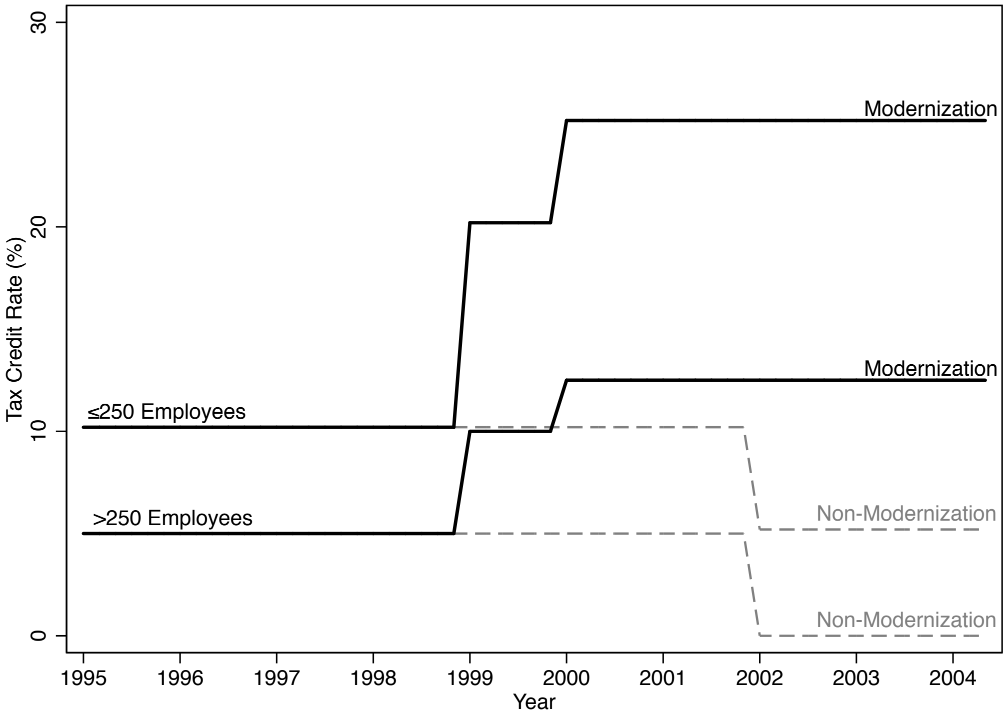

Figure 2: Equipment Tax Credit Rates for Manufacturing Firms

Note. The tax credit rates pertain to manufacturing firms in East Germany, excluding Berlin, and are based on the standard rates of the official government act for investment projects completed within a business year. Firms in regions close to the Polish or Czech borders received an additional 2.5% in 2001–2009. Small firms in regions close to Berlin were not eligible for the additional increase in 2000. For investment projects spanning several years, the change in the tax credit rate was smoothed.

One year later, these rates increased to 25% and 12.5%, respectively. Modernization investments were broadly defined and covered a wide range of equipment, including those that could *potentially* increase production, change the production process, or produce different products. All investment that did not directly replace a similar asset fell within this large category.[10] The larger increase of the investment tax credit rate for firms below the cutoff implies a relative decrease of their capital costs after the policy change compared to firms above it. With the rate changes for modernization investments, the reduction amounts to 8.29% on average.[11] Tax credit rates for non-modernization equipment investment had no discernible differential impact on capital costs by firm size, with rates remaining constant during the policy change in 1999 and moving in parallel in 2002.

year ($2.94 billion) for firms below the employment cutoff was eliminated. Tax credits for investments into structures started in 1999 but rates did not differ by firm size.

[10]See German Federal Ministry of Finance regulations from June 28, 2001.

[11]From a standard definition of the user costs of capital: $d \ln c_k = -d\tau_i/(1-\tau_i)$, where τ_i is the tax credit rate. For 1999, the relative reduction is $11.11\% - 5.26\% = 5.85\%$ and for 2000–2004, $16.67\% - 7.89\% = 8.78\%$.

3 Theoretical Framework

To explain the impact of a (relative) reduction of capital costs on firms' production behavior, I set up a simple theoretical framework. While firms that receive a favorable tax credit rate may change their factor use and production output as a direct response, these changes can result in general equilibrium adjustments for all firms, independent of their capital cost change. The framework includes local labor market mechanisms as channels for such indirect effects.

As a first indirect factor, I take agglomeration economies into account. A long-standing literature investigates the benefits of agglomeration to explain spatially concentrated economic activity despite the associated disadvantages, such as higher costs for labor and land. In a classification still used today, Marshall (1890) divides cost and productivity advantages from agglomeration into three broad categories: thick labor markets, which reduce search frictions and facilitate better worker-firm matches; knowledge spillovers as a result of intensified interaction between firms or workers; and production advantages from being located close to upstream suppliers and downstream customers (for an overview of theories, see, e.g., Duranton and Puga, 2004). In the context of the German investment tax credits, an initial expansion of economic activity from lower capital costs may attract a larger labor force and facilitate greater interactions between closely located firms. These agglomeration advantages may generate additional production expansion shared by all firms in a region.

The second indirect channel highlights the role of local labor supply adjustments. If local labor supply is not perfectly elastic, due to limited movement of workers across regions or in and out of non-employment, changes in labor demand would affect wages and thus influence production costs for all firms in a region.

I introduce both channels into a static firm model with capital and labor. Given the static setup, I refrain from using time indices.[12] Following the previous literature, I capture agglomeration economies as a reduced-form region-specific productivity shifter A_r in firms' production functions (e.g., Moretti, 2011; Gathmann et al., 2020). The shifter

[12]For details on various derivation steps, see Appendix A. The static model abstracts from capital adjustment costs that are important for explaining dynamic investment behavior and investment inactivity (Cooper and Haltiwanger, 2006). Bond and Van Reenen (2007) summarize the literature on dynamic investment models.

is defined as

$$A_r = L_r^{\lambda}, \tag{1}$$

where L_r is the total employment within region r and λ is the elasticity of agglomeration. Changes in the employment of one firm, therefore, impact the production capabilities of all firms within the same region.[13] Local labor supply is modeled as a function of wages both in region r and outside of it, and depends on the local labor supply elasticity $\varphi = (dL_r/L_r)/(dw_r/w_r)$, which determines how responsive labor is to changes in local wages. In equilibrium, wages will adjust such that local labor demand and supply equalize. I assume many regions that are each small relative to the overall economy. Adjustments in one region therefore imply negligible effects on outcomes in other regions.

Firms produce according to the constant elasticity of substitution (CES) production function

$$F(K_i, L_i) = Y_i = A_r A_i \left(a_K K_i^{\rho} + a_L L_i^{\rho}\right)^{\frac{1}{\rho}}, \tag{2}$$

where the output Y_i of firm i is produced from capital K_i and labor L_i, with the production parameters a_K and a_L and the elasticity of substitution between capital and labor $\sigma = \frac{1}{1-\rho} \in [0, \infty)$. Production also depends on a firm-specific productivity parameter A_i.

Monopolistic competition leads to firms facing the downward sloping inverse demand curve

$$p_i = BY_i^{-\frac{1}{\eta^D}}, \tag{3}$$

where the price depends on the elasticity of demand $\eta^D > 1$ and a demand shifter B. Firms take the rental rate of capital r and local wages w_r as given. Tax credits allow firms to reduce their cost of capital by rate τ_i. Each firm maximizes current profits by choosing optimal capital and labor.

[13] With agglomeration spillovers, a change in the employment of one firm leads to productivity gains for all others in the same region. Firm adjustments from this initial push create additional agglomeration spillovers that benefit the original firm as well. With many firms receiving a capital cost reduction, this chain of reaction occurs multiple times, making agglomeration effects roughly equal for all firms in a region.

Under these conditions, optimal factor inputs at baseline can be written as

$$K_i = a_K^{\frac{1}{1-\rho}} Y_i \left(\frac{(1-\tau_i)r}{p_i(1-\frac{1}{\eta^D})} \right)^{\frac{1}{\rho-1}} (A_i A_r)^{\frac{\rho}{1-\rho}} \tag{4}$$

$$L_i = a_L^{\frac{1}{1-\rho}} Y_i \left(\frac{w_r}{p_i(1-\frac{1}{\eta^D})} \right)^{\frac{1}{\rho-1}} (A_i A_r)^{\frac{\rho}{1-\rho}}. \tag{5}$$

To derive the overall impact, I totally differentiate the production function and factor input equations (4) and (5). For simplicity, I consider the case in which firms do not initially receive tax credits ($\tau_i = 0$, $\forall i$), leading to

$$\frac{dK_i}{K_i} = \left(\eta^D s_r^K + \frac{1}{1-\rho} s_r^L \right) H d\tau_i \quad + \quad (\eta^D - 1)\frac{dA_r}{A_r} - (\eta^D - \frac{1}{1-\rho}) s_r^L H \frac{dw_r}{w_r} \tag{6}$$

$$\frac{dL_i}{L_i} = \underbrace{\left(\eta^D - \frac{1}{1-\rho} \right) s_r^K H d\tau_i}_{\text{direct effect}} \quad + \quad \underbrace{(\eta^D - 1)\frac{dA_r}{A_r} - (\eta^D s_r^L + \frac{1}{1-\rho} s_r^K) H \frac{dw_r}{wr}}_{\text{indirect effect}}, \tag{7}$$

where s_r^K is the capital share and s_r^L the labor share. With wages and the rental rate of capital being constant at the regional level, firms in a region have the same capital-labor ratio and equal capital and labor shares. $H = \eta^D/(\eta^D - 1)$ is an additional term arising from the monopolistic competition assumption.

Both the capital and labor response depend on a direct effect, which only influences firms receiving tax credits, and an indirect effect that is identical for all firms of a region. A comparison of firms according to the tax credit rate change within regions can therefore elicit the direct effect of a capital cost reduction. Closely mimicking the standard labor demand model (Hamermesh, 1993), the direct effect combines two forces, a scaling effect of production and a substitution effect between capital and labor. For capital, these effects work in the same direction, with an increase in the tax credit rate thus implying an unambiguous expansion of the capital stock. This relationship is driven on the one hand by a general stimulation of production through cheaper production costs, and on the other hand by a shift from labor towards capital due to the relative cost reduction. For labor demand, these factors oppose one another. While the expansion of production increases the demand for labor, at the same time relatively more capital is employed. The combined impact then depends on the relative magnitude of both forces and is positive if the elasticity of product demand is larger than the elasticity of substitution.

The indirect effect depends on the two channels introduced at the regional level: agglomeration economies and local labor supply. While agglomeration benefits have a factor-neutral positive impact in the model, the influence of local labor supply through adjustments of local wages is more complex. An increase in wages leads to higher costs of production and therefore a reduction in output. Firms can mitigate some of the impact by shifting to the relatively cheaper capital inputs. Consequently, higher wages lead to an unambiguous decrease in labor but an ambiguous effect on capital, depending on the magnitude of the scale and substitution effect.

Using the definition of the regional productivity shifter and the local labor supply elasticity, I solve for the equilibrium adjustment of capital and labor. The indirect effects for firms within region r are

$$\left(\frac{dK_i}{K_i}\right)_{indirect} = \frac{(\eta^D-1)\lambda - (\eta^D - \frac{1}{1-\rho})s_r^L H\frac{1}{\varphi}}{1-(\eta^D-1)\lambda + (\eta^D s_r^L + \frac{1}{1-\rho}s_r^K)H\frac{1}{\varphi}} \sum_{j\in j_r} \frac{L_j}{L_r}\left(\frac{dL_j}{L_j}\right)_{direct} \tag{8}$$

$$\left(\frac{dL_i}{L_i}\right)_{indirect} = \frac{(\eta^D-1)\lambda - (\eta^D s_r^L + \frac{1}{1-\rho}s_r^K)H\frac{1}{\varphi}}{1-(\eta^D-1)\lambda + (\eta^D s_r^L + \frac{1}{1-\rho}s_r^K)H\frac{1}{\varphi}} \sum_{j\in j_r} \frac{L_j}{L_r}\left(\frac{dL_j}{L_j}\right)_{direct}, \tag{9}$$

where the parameters need to be bounded by $(\eta^D - 1)\lambda - (\eta^D s_r^L + 1/(1-\rho)s_r^K)H\varphi < 1$ for a stable equilibrium. The indirect firm response is governed by a first term of model parameters, including the agglomeration elasticity and labor supply elasticity as central parameters in shaping the indirect effect. With a positive direct labor demand effect, an agglomeration elasticity above zero ($\lambda > 0$) means that agglomeration creates additional firm growth in both capital and labor.[14] A perfectly elastic local labor supply ($\varphi = \infty$) implies no change to wages and therefore no impact on indirect adjustments. In all other cases, the labor effect is dampened. With a perfectly inelastic local labor supply ($\varphi = 0$), regional employment stays constant, and the indirect employment effect—which is equal for all firms within a region—completely offsets the direct effect of those firms receiving tax credits.

The second multiplicative term comprises the sum of the direct effects within a region, weighted by firm employment. In the German investment tax credit program, firms either received a large tax credit rate change ($d\tau_i = d\tau$) or a small one, which for simplicity I assume to be zero ($d\tau_i = 0$). Under these assumptions, the direct effect of a capital cost

[14]In principle, the model allows for an agglomeration elasticity $\lambda < 0$, which could be interpreted as the negative impacts of agglomeration (e.g., congestion) prevailing.

reduction are the same across affected firms in a region. The indirect effects can thus be written as

$$\left(\frac{dK_i}{K_i}\right)_{indirect} = \frac{L_{d\tau,r}}{L_r} f(s_r^L) d\tau, \quad \left(\frac{dL_i}{L_i}\right)_{indirect} = \frac{L_{d\tau,r}}{L_r} g(s_r^L) d\tau, \tag{10}$$

where $L_{d\tau,r}/L_r$ is the regional employment share of firms receiving a tax credit rate change, and $f(s_r^L)$ and $g(s_r^L)$ summarize all remaining terms, which depend on both the model parameters and the regional labor share as a varying term.[15] The tax credit policy leads to differences in the indirect effects across regions according to the employment share of firms receiving tax credits. The effect size will depend on the model parameters of the two indirect mechanisms, agglomeration economies and local labor supply. Additionally, the regional labor share interacts with these mechanisms and may increase or decrease the observed effect across regions. As long as the local labor share is independent of the regional firm size distribution, I can separate this additional impact and use the employment share of firms receiving a tax credit rate change to elicit the average indirect effects in a comparison across regions.

4 Estimation Strategy

The estimation strategy builds on the regularities of the theoretical framework and exploits the differential change of tax credit rates in 1999 by firm size. For the estimation of direct effects, I compare the response of firms below the firm size cutoff to that of firms above the cutoff over time. Differences in the firm size distribution across regions further create variation in the regional employment share of firms receiving the relative capital cost reduction. To estimate indirect effects, in a second step, I extend the regression model by comparing firm behavior across regions according to this share.

To begin, I focus solely on the direct effects, using the difference-in-differences specification

$$Outcome_{it} = \beta Small_{i,98} \times Post98_t + X_{it}'\gamma + \psi_i + \psi_{nt} + \psi_{lt} + \epsilon_{it}, \tag{11}$$

where the dependent variable $Outcome_{it}$ is one of several outcomes of firm i in year t. To capture general input and output variables of firm production, I analyze capital

[15] The indirect effects also depend on the regional capital share, which in turn is a function of the regional labor share.

stock, employment and sales. As an additional outcome, labor productivity can provide evidence for long-term firm growth. To reveal workforce composition changes important in the context of skill-biased and routine-biased technological change, I also examine the effects on the employment of different skill and occupation groups.

The variable $Small_{i,98}$ classifies firms according to their policy-relevant firm size in 1998 into small firms below the firm size cutoff and a comparison group of those above it. This variable is interacted with the dummy $Post98_t$, which categorizes the years after 1998 as the treatment period. This design exploits the differential treatment of small and large firms, with the parameter of interest β capturing the effect of a reduction of capital costs caused by the policy change. I include differential pre-treatment wage trends X_{it}, which linearly interact wage growth between 1995 and 1998 with time, to proxy for firm-specific trends in productivity from before the policy change. I further add firm (ψ_i), industry-year (ψ_{nt}) and labor market-year (ψ_{lt}) fixed effects. Industry-year and labor market-year fixed effects can control for the common concern that industry-specific shocks or region-specific policies and trends may coincide with the policy changes. Labor market-year fixed effects also capture all differences arising from indirect effects across labor markets. The identifying assumption is that any differences in indirect effects within labor markets are uncorrelated with the direct effects. By explicitly defining the functional form of indirect effects in a second step, I can test this assumption.

The classification of firms by their policy-relevant firm size in 1998 leads to time-invariant comparison groups in a standard difference-in-differences design. Yet firms may later move across the firm size cutoff. The firm size dependence may also introduce incentives for firms to stay small and receive larger tax benefits. To deal with both possibilities, I exclude firms close to the cutoff, as they are most likely affected. Firms far from the cutoff may instead affect the estimation through heterogeneous responses. I therefore restrict the sample to within a firm size minimum and maximum. For the main analyses, I include firms with a policy-relevant firm size between 40 and 1,500, excluding those above 225 and below 275 in 1998. To assess the influence of these design choices, I provide robustness tests that vary the overall firm size interval and the excluded firms close to the cutoff.

To estimate the indirect effects, I adjust the regression model to

$$Outcome_{it} = \beta Small_{i,98} \times Post98_t + \eta ShareBelow250_{-ir,98} \times Post98_t + X_{it}^{'}\gamma + \psi_i + \psi_{nt} + \psi_{st} + \epsilon_{it}, \quad (12)$$

where the additional term $ShareBelow250_{-ir,98}$ is the share of employees working in firms with up to 250 employees in region r in 1998, excluding employees in firm i. I exclude firms' own employees to prevent interactions between the firm size dummy and the employment share measure in regions with a small number of firms. To set up a difference-in-differences estimation with continuous treatment intensity, I interact the share with the treatment period dummy $Post98_t$. The estimation includes two coefficients of interest: β, the direct effect of higher tax credit rates as in specification (11), and η, which identifies the indirect effects of investment tax credits within a region in line with the theoretical framework.

For the main specification, to capture indirect effects in small regional units, I calculate the share measure within districts. Given the broad overlap of districts with labor markets, I rely on state-year fixed effects (ψ_{st}) to control for broad regional shocks. To counter within-state differences that could correlate with the employment share measure, I add differential pre-treatment employment trends by linearly interacting employment growth between 1995 and 1998 with time. As industries tend to be spatially clustered (see e.g. Duranton and Overman, 2005; Ellison et al., 2010)), industry-year fixed effects allow me to capture industry-specific firm trends such as technology adoption, which would otherwise be ascribed to the indirect effects. The theoretical framework shows that interaction effects between the employment share of firms below the cutoff and the regional labor share can potentially bias the estimation. To assess the influence of the labor share on firm outcomes, I compare the main specification with a version in which the indirect effect estimation is interacted with the demeaned regional labor share.

With the district level distinguishing relatively confined regions, the corresponding employment share of firms below the cutoff likely captures a large percentage of the indirect effects. While varying the level of the administrative region would allow me to explore changes to the indirect effects for larger or smaller areas, such an approach is still limited by the available regional units. Additionally, these regions may not coincide with the relevant unit for each firm. For example, firms located at administrative boundaries may interact more often with firms in the neighboring district than with those on the other side of their own district. To investigate the spatial propagation and attenuation of indirect effects, I therefore use distance intervals around each firm and calculate the share measure based on firms within these intervals. In this way, I can estimate indirect effects flexibly across space, choosing distances independent of administrative units.

I focus on the capital stock, employment and sales as the main production outcomes. To disentangle the mechanisms of the indirect effects, I add firm wage. The strength of the wage response is informative about the labor supply elasticity. If the labor supply is perfectly elastic, then wages should not react to changes in regional labor demand and agglomeration economies would therefore remain as the explanatory channel.

The average effect estimations mask any firm adjustment patterns for the years around the policy change. To analyze short-term and medium-term effects, I set up a dynamic specification as follows:

$$Outcome_{it} = \sum_{p=1995}^{2004} \beta_p Small_{i,98} \times \mathbb{1}(t=p) + X_{it}^{'}\gamma + \psi_i + \psi_{nt} + \psi_{lt} + \epsilon_{ibt} \tag{13}$$

$$Outcome_{it} = \sum_{p=1995}^{2004} \beta_p Small_{i,98} \times \mathbb{1}(t=p) + \sum_{p=1995}^{2004} \eta_p ShareBelow250_{-ir,98} \times \mathbb{1}(t=p) + X_{it}^{'}\gamma + \psi_i + \psi_{nt} + \psi_{st} + \epsilon_{it}, \tag{14}$$

where compared to specification (11) and (12), $Small_{i,98}$ and $ShareBelow250_{-ir,98}$ are interacted with dummies for all years of the period of analysis and the coefficients $\beta_{1995}, ..., \beta_{2004}$ and $\eta_{1995}, ..., \eta_{2004}$ capture the direct and indirect effects for each year, respectively. To measure the estimated dynamic effects relative to the baseline period, I set the coefficients for 1998 to zero ($\beta_{1998} = 0$ and $\eta_{1998} = 0$). This specification provides insights into the dynamic response of firms, which may not completely adjust production within a year. It also sheds light on pre-treatment trends or anticipatory behavior. Observing statistically insignificant estimates close to zero before the start of the treatment period lends support to the parallel trends assumption.

The differential change in investment tax credit rates across firms allows me to identify the direct and indirect effects. However, any expansion of production of targeted firms could be at the expense of the remaining firms, which could face, for example, tougher price competition. Equally, for labor demand, the creation of jobs in one firm may not translate to aggregate changes but to a shift across firms. Given a fixed pool of workers, tracking job-to-job movers is a convenient way of exploring the dependence between firms. Observing directed movements of employed workers to targeted firms would be evidence that jobs shift across firms.[16]

In a final analysis, I therefore investigate shifts in employment across firms by analyzing the employment status of workers before entry into and after exit from a firm. I do so in a counterfactual exercise, in which I accumulate yearly employment growth of firms from different worker flow types according to

$$\ln L_{it}^{G} = \ln \left[\prod_{p=1996}^{t} \left(1 + \frac{\Delta L_{ip}^{G}}{L_{ip-1}} \right) \right], \tag{15}$$

where ΔL_{ip}^{G} is the change in the number of employees of subgroup G for year p compared to one year before, and L_{ip-1} is actual firm employment one year before. For overall worker flows, this expression is equivalent to the log of total employment in the estimation, as firm fixed effects subsume the difference from initial employment in 1995. I divide the overall employment change into two groups: (a) flows in and out of non-employment, and (b) flows of job-to-job movers. Comparing the employment effect of both groups can shed light on the relative importance of investment tax credits for both non-employment and direct employment shifts across firms.

I then further subdivide job-to-job movers and examine movements between firms of the estimation sample. First, I estimate the effects for transitions from and to firms above the firm size cutoff. Finding a positive effect for this type of flow would imply employment shifts towards the firms that receive a relative capital cost reduction, potentially limiting the aggregate effect on production. Similarly, to quantify spatial shifts in production that may be driven by differences in agglomeration benefits, I select job-to-job movers across regions. The analysis further includes flows to and from the service sector for an assessment of the impact of the policy change on outside sectors.

[16] Although firms losing workers could compensate by hiring from non-employment or an outside sector, they incur search and hiring costs, and may end up with less productive replacements.

5 Data

The empirical analysis relies on two comprehensive administrative data sources, the *AFiD-Panel Industriebetriebe* (AFiD) from the German Federal Statistical Office and matched employer-employee data from the *Beschäftigten-Historik* (BeH) from the Institute for Employment Research.

The AFiD dataset, which covers the universe of manufacturing and mining firms with more than 20 employees, is particularly suited for the general analysis of firm capital, employment, and sales. The data are collected through administrative surveys at the establishment level and form the basis for the official statistics of Germany. To align the analysis with the relevant level of the investment tax credits, I aggregate the data by using firm identifiers. To obtain capital stock measures, I rely on capital depreciation information from the supplementary *Kostenstrukturerhebung* and combine this information with industry-level depreciation rates by Müller (2017) and firm investment in the AFiD data.[17]

For a detailed analysis of employment effects, I use the BeH data, which comprise the universe of employees covered by the German social security system.[18] Schmucker et al. (2016, total population) provide aggregate data at the establishment level for June 30 of each year, for a standard set of employment and wage outcomes. For East Germany, I expand this dataset with employment information by skill groups: (a) employees with a college degree and (b) all others with either vocational qualifications or none. I further classify occupations according to their main task content into (a) a group of routine task occupations and (b) a group of non-routine manual and abstract task occupations. The classification of occupations relies on Dengler et al. (2014), who apply the approach by Autor et al. (2003) to a representative database with occupational job task requirements for Germany. The employer-employee data also allow me to follow workers across firms. To calculate the counterfactual employment growth measures of equation (15), I categorize inflows and outflows in the yearly dataset according to the employment status of workers one year prior to entry to or one year after exit from a firm.

[17]The *Kostenstrukturerhebung* is a yearly firm survey of a stratified random sample in the manufacturing and mining sector, conducted by the German Federal Statistical Office. Appendix B describes the calculation of capital stock and other data processing steps in detail.

[18]The German social security system covers roughly 80% of the workforce. Civil servants, the self-employed, and military personnel are not included.

Given German data protection laws, the BeH data cannot be linked with the AFiD data. To make both datasets comparable, I aggregate the BeH data at the firm level by using firm names available through German Federal Employment Agency registries (Schäffler, 2014) and restrict the sample to firm observations with at least 20 employees.

Both datasets include industry and municipality information. I build industry groups at the 2-digit level and assign districts to labor markets according to Dustmann and Glitz (2015). I end up with 14 industry groups within the manufacturing sector and divide 111 districts into 66 labor markets in East Germany (32 districts constitute their own labor market). To determine the policy-relevant firm size, I use vocational trainee information to adjust total employment. Wage is measured as the average monthly wage of all employees in the AFiD data and as the daily average wage of full-time employees in the BeH data. For better comparability, I transform daily wage to monthly. Establishments in multi-establishment firms may have distinct industry identifiers and location. I set firms' industry and location equal to those of the establishment with the highest number of workers over time. I calculate the regional employment share of firms below the firm size cutoff by determining the share for each establishment separately and aggregating at the firm level weighting by establishment size.

To investigate the spatial propagation of agglomeration economies, I extend the analysis of indirect effects to flexible distance intervals around each firm. The calculation of these intervals builds on precise geolocation data (IEB GEO) from the Institute for Employment Research. Starting in 1999, the dataset includes geographic coordinates for establishments and is generated from address data of the German Federal Employment Agency registries with information on both street name and building number. Using that data, I determine the shortest (great-circle) distance between establishments and calculate the employment share of firms below the policy-relevant firm size cutoff for different distance intervals for 1998. Appendix B.4 describes the steps I take to infer establishment location for 1998.

In the main analysis I include manufacturing firms in East Germany between 1995 and 2004. By 1995, the privatization of state-owned firms was nearly completed and the convergence with West Germany had slowed down significantly despite remaining economic differences (Sinn, 2002). As 1995 is the first year with available AFiD data, this year constitutes a natural starting point. After 2004, the firm size definition changed

Table 1: Descriptive Statistics for Manufacturing Firms

	All Manufacturing		Estimation Sample	
	West	East	Small Firms	Large Firms
	(1)	(2)	(3)	(4)
Panel A. AFiD Data				
Capital (million)	29.04	21.16	12.59	100.62
	(354.36)	(138.55)	(19.08)	(145.25)
Employees	168.52	91.28	94.28	516.62
	(1,444.84)	(369.92)	(54.91)	(314.30)
Total sales (million)	35.45	13.31	11.46	88.77
	(479.55)	(72.07)	(15.74)	(93.68)
Average monthly wage	2,451.70	1,680.30	1,719.42	2,218.03
	(1,104.96)	(541.87)	(477.34)	(634.58)
Multi-establishment firms (%)	9.26	5.16	3.83	15.30
	(28.99)	(22.12)	(19.19)	(36.02)
Share below cutoff region (%)	41.90	65.80	66.94	62.41
	(13.10)	(16.32)	(15.64)	(16.96)
Observations	335,778	60,386	14,970	1,319
Panel B. BeH Data				
Employees	141.32	77.00	91.65	470.77
	(759.87)	(144.78)	(56.14)	(279.28)
Average monthly wage full-time	2,597.73	1,738.21	1,790.59	2,267.93
	(744.10)	(555.76)	(508.54)	(678.07)
Share employees college (%)	14.85	10.46	10.79	14.13
	(26.74)	(11.89)	(10.51)	(10.97)
Share non-routine occupations (%)	35.44	35.08	32.78	33.69
	(38.20)	(26.58)	(23.66)	(21.33)
Multi-establishment firms (%)	7.96	5.35	6.79	10.61
	(27.06)	(22.50)	(25.16)	(30.81)
Share below cutoff region (%)	46.63	73.86	74.88	69.67
	(14.17)	(16.38)	(15.67)	(17.91)
Observations	380,582	71,272	18,960	1,630

Note. The descriptive statistics are the means with standard deviations reported in parentheses for selected outcomes of the AFiD data in panel A and the BeH data in panel B. Columns 1 and 2 include all observations of manufacturing firms for 1995–2004 for West Germany and East Germany, respectively, excluding Berlin. Columns 3 and 4 include all observations from the main estimation sample split into those of small firms with a policy-relevant firm size in 1998 of at most 225 employees (column 3) and large firms with at least 275 (column 4). In both panels, the number of observations pertains to the employee information.

markedly, so that interpreting comparisons with prior years is difficult.[19] I exclude firms in Berlin, where different tax credit rate rules applied, and restrict the sample to firms that were active throughout the period of analysis for a balanced panel. As previously stated for the estimation strategy, the sample includes firms with policy-relevant firm size in 1998 between 40 and 1,500, and excludes those with more than 225 employees and fewer than 275.

Table 1 summarizes key variables for (a) manufacturing firms in West and East Ger-

[19]From 2005, the cutoff value followed the definition of small and medium firms by the European Union, i.e., taking the ownership structure into account and defining the cutoff jointly by the number of employees, sales and total assets.

many, and (b) for the estimation sample divided into small and large firms for 1995–2004.[20] Panel A, which shows descriptive statistics for the AFiD data, reveals clear differences between manufacturing firms in the West (column 1) and the East (column 2). Firms in West Germany on average use 37.2% more capital, employ 84.6% more workers, have 166.3% higher sales, and pay 45.9% higher wages. These figures support the well-documented fact that economic differences between West and East Germany persisted after German reunification. For the estimation sample (columns 3 and 4), as expected, both groups show differences in average firm outcomes. In contrast, the regional employment share of firms below the cutoff does not markedly depend on the firm size groups, with shares of 67% and 62% for small and large firms, respectively.

Panel B summarizes the BeH data. The availability of firm employment and wage in both datasets facilitates a comparison of these outcomes. For each subgroup, mean employment and wage compare well with their counterparts from panel A, likely a result of the high data collection standards for both datasets. While these figures confirm the size and wage differences across West and East Germany, and by firm size, the employment composition is similar across subgroups. The average share of college-educated workers varies between 10–15% and the share of non-routine occupations between 32–36%.

Both panels also include information on the share of multi-establishment firms. For panels A and B, the share is 9% and 8% in West Germany and 6% and 5% in East Germany, respectively. Both panels also point to a higher share of multi-establishment firms among the large firms in the estimation sample. The alignment of these outcomes across datasets indicates a high reliability of the firm identifiers generated from the BeH firm name data.

6 Results

6.1 Direct Effects

To begin, I focus on the estimation results for the direct effects from specification (11). Table 2 reports estimates for the main firm outcome variables, with column 1 summarizing the results for the log of capital stock. The estimate implies that the relative reduction of capital costs leads to an increase in the capital stock by 14.8 log points for small firms

[20] Appendix B.5 provides descriptive statistics of the estimation sample for a complete list of variables.

Table 2: Direct Effects of Investment Tax Credits

	Log Capital	Log Investment	Log Employment	Log Domestic Sales	Log Labor Productivity
	(1)	(2)	(3)	(4)	(5)
Small firm × After 1998	0.148** (0.058)	0.202** (0.089)	0.103*** (0.036)	0.096** (0.046)	-0.007 (0.029)
Observations	14,931	15,527	16,289	16,281	16,281

Note. Each column reports estimates based on specification (11). The dependent variables are the log of total capital stock in column 1, log investment in column 2, log employment in column 3, log of domestic sales in column 4, and log of labor productivity in column 5. Additional controls are pre-treatment wage growth, firm fixed effects, industry-year fixed effects, and labor market-year fixed effects. Results are based on the AFiD data. Standard errors in parentheses are clustered at the district level: * p<0.10, ** p<0.05, *** p<0.01.

relative to large ones. A positive effect on capital is in line with the predictions from the theoretical framework. While I prefer using capital stock for linking the empirical evidence to the theoretical framework, to compare the response more directly with findings in the previous literature, in column 2, I also show results for log investments. The estimate corresponds to an investment effect of 20.2 log points. With a relative cost reduction of 8.3%, this effect implies an elasticity of investment with respect to the net of tax costs of 2.4.[21] This value compares to investment elasticities for accelerated depreciation allowances of between 6 and 14 (House and Shapiro, 2008; Zwick and Mahon, 2017; Maffini et al., 2019; Ohrn, 2019). The distinct settings may explain the difference, with East Germany facing many structural problems, such as failing infrastructure and the outflow of high-skilled workers.

Differences in the firms' perceptions of investment tax credits compared to accelerated depreciation allowances may play a role as well. While investment tax credits are close to a discount on the purchase price, accelerated depreciation allowances reduce capital costs through a shift of depreciations to the first year, for which tax deductions have the highest per dollar present value. Because deductions are only shifted across years, substantial tax deductions in the first year are counterbalanced with later years, and imply a considerably smaller capital cost reduction (Zwick and Mahon, 2017). But if firms have a preference for short-term tax savings, then accelerated depreciation allowances may generate benefits beyond the pure capital cost reduction.

Column 3 presents the employment estimate. The effect on overall employment is 10.3

[21] From German tax records, I find that 70% of manufacturing firms in East Germany with a firm size between 40 and 1,500 received investment tax credits in 2004, with credits amounting to on average 290 thousand euros. Adjusting the elasticity to this gap in take-up, I obtain a value of 3.5.

log points, implying that firms increase their labor inputs after a capital cost reduction. The employment response turns out to be smaller than for capital, suggesting substitution between both factors with a shift towards capital-intensive production.[22] Nevertheless, as the overall effect on employment remains positive, the theoretical framework implies that the overall expansion of production from cheaper production costs prevails any substitution effect. The results therefore suggest that investment tax policies work as intended, by creating jobs rather than destroying them.

Given the expansion of input factors in production, a natural next step is the analysis of output. I use domestic sales as the main output measure, as this outcome limits the influence of volatile exports in the data, and find an effect of 9.3 log points, reported in column 4. This effect is similar to the input response, albeit somewhat smaller. I provide additional evidence for log total sales in panel A of Appendix Table C.1, finding a positive (but statistically insignificant) effect on total sales when excluding firms with volatile pre-treatment export shares, although the coefficient estimate is statistically insignificant. As a related finding, I show that investment tax credits lead to a decrease in the firms' export shares, indicating benefits from a reduction in exporting costs.

Column 5 shows the relationship between inputs and outputs more directly, with log labor productivity (log of domestic sales over employment) as the dependent variable. The estimate is -0.008 and statistically insignificant at conventional levels. This result suggests that firms do not improve productivity and might even experience an efficiency loss. Adjustment processes in the short and medium term could explain this finding. Investment tax credits may also create limited incentives for substantially changing production technology.

Taken together, the results point to firm expansion that is driven mainly by the scaling of production processes. The increase in labor demand supports policymakers who promise to create more jobs through investment tax incentives. However, the productivity estimate casts doubt on the goal of generating long-term growth for the firms involved. Without productivity improvements, firms may return to their prior production levels once the benefits end.

[22] A comparison of the capital and employment effect allows me to calculate the elasticity of substitution: $\sigma = \frac{d\ln(K_i/L_i)}{d\ln(w/r)} = (0.145 - 0.103)/0.083 = 0.506$. The magnitude is in line with that of the previous literature, which generally finds elasticities below 1 at the firm level (Chirinko et al., 2011; Raval, 2019; Oberfield and Raval, 2021). With negative wage effects, the value is a lower bound, as the change in relative factor costs would be smaller.

Figure 3 summarizes the related dynamic regression results from specification (13). Similar firm response patterns emerge across outcome variables. First, in all panels, firms below and above the firm size cutoff move in parallel before the policy change, with statistically insignificant estimates that fluctuate around zero. This finding constitutes important evidence in favor of the parallel trends assumption and counters the argument that firms reacted in anticipation of the policy change. After the policy change, firms below the cutoff expand their production relative to those above it, as evident for log capital in panel A, for log employment in panel B, and for log domestic sales in panel C. The adjustments take place over several years, with both the capital and employment effect stabilizing after 4–5 years. For domestic sales, the effect remains small in the first year after the policy change but increases in the subsequent period before stabilizing. Panel D shows the labor productivity response for completeness. In line with the average result, estimates in the dynamic specification stay close to zero throughout the sample period.

Thus far, the analysis on employment speaks to the overall response. However, the literature on skill-biased and routine-biased technological change highlights the possibility of differential effects by both skill level and occupational task content (Katz and Murphy, 1992; Acemoglu and Autor, 2011). The predictions are that novel technology, particular ICT, shifts demand towards skilled labor with abstract tasks that complement the use of technology and away from routine tasks that machines can easily codify.[23] The investment tax credits cover a period with significant change in available technology. By encouraging capital investments in such technology, investment tax credits could therefore induce shifts in labor demand.[24]

Table 3 presents estimation results for different skill and task groups based on the BeH data. Panel A shows the average effects and starts with log total employment in column 1. This estimation serves as a baseline for subsequent outcome variables and allows for comparing the results of the BeH and AFiD data. The average employment effect is 11.3 log points and close in magnitude to the result for the AFiD data, again highlighting the high quality and comparability of these datasets. Columns 2–4 report the effects by skill, distinguishing between log college-educated and log non-college-educated

[23]During the sample period, technology could not replace non-routine manual tasks. I therefore group them with abstract tasks.

[24]An extension of the theoretical framework with two types of labor and different elasticities of substitution between labor types and capital can incorporate skill-bias (see Appendix A.5 for details).

Figure 3: Evolution of Direct Tax Credit Effects

Note. Each panel reports estimates based on specification (13). The dependent variables are the log of total capital stock in panel A, log employment in panel B, log of domestic sales in panel C, and log of labor productivity in panel D. Additional controls are pre-treatment wage growth, firm fixed effects, industry-year fixed effects, and labor market-year fixed effects. Results are based on the AFiD data. The dashed lines show the 95% confidence interval for each estimate, with standard errors clustered at the district level.

workers. The coefficient estimates translate to an effect on college-educated workers of 12.8 log points and a slightly smaller effect on non-college-educated workers of 10.7 log points. The difference in effect size between both groups is in the expected direction and suggests a labor demand shift towards skilled workers, even though this shift is of limited magnitude. In line with this finding, combining both outcomes as the log ratio—which restricts the analysis to firms with both skill types—leads to a positive but statistically insignificant effect of 1.9 log points.

Columns 5–7 complement these results by reporting the coefficient estimates for the effects on either predominately abstract or routine task occupations. Similar patterns emerge, with the labor demand increasing by 12.4 log points for abstract occupations

Table 3: Effects By Employment Types and Industry ICT Intensity

Panel A: Average Employment Effects							
	Log All (BeH)	Log College	Log Non-College	Log Skill Ratio	Log Abstract	Log Routine	Log Task Ratio
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
After 1998 ×	0.112***	0.128***	0.107***	0.019	0.124***	0.112***	0.011
Small firm	(0.029)	(0.030)	(0.031)	(0.021)	(0.035)	(0.036)	(0.029)
Observations	20,590	19,145	20,589	19,144	20,423	20,528	20,361

Panel B: Employment Effects By ICT Intensity Of Industries							
	Log All (BeH)	Log College	Log Non-College	Log Skill Ratio	Log Abstract	Log Routine	Log Task Ratio
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Low ICT	0.115***	0.080*	0.118***	-0.038	0.132***	0.133**	0.000
Reliance	(0.041)	(0.041)	(0.041)	(0.030)	(0.049)	(0.055)	(0.045)
High ICT	0.107**	0.186***	0.094**	0.088***	0.114**	0.086**	0.024
Reliance	(0.043)	(0.052)	(0.046)	(0.031)	(0.048)	(0.042)	(0.030)
Observations	20,590	19,145	20,589	19,144	20,423	20,528	20,361

Note. Each column reports estimates based on specification (11). In panel B, heterogeneous effects are presented by industry groups with ICT capital stock shares below (low reliance) and above (high reliance) the median (3.96%). The dependent variables are the log employment in column 1, the log of college-educated employees in column 2, the log of non-college-educated employees in column 3, the log ratio of college-educated to non-college-educated employees in column 4, the log of abstract (including non-routine manual) occupation employees in column 5, the log of routine occupation employees in column 6, and the log ratio of abstract to routine occupation employees in column 7. Additional controls are pre-treatment wage growth, firm fixed effects, industry-year fixed effects, and labor market-year fixed effects. Results are based on the BeH data. Standard errors in parentheses are clustered at the district level: * p<0.10, ** p<0.05, *** p<0.01.

compared to an increase of 11.2 log points for routine occupations. The estimate for the log ratio is 1.1 log points and statistically insignificant. Overall, the estimates highlight a broad positive employment response across different labor types. While small shift in the employment composition are evident, confidence intervals are too large to make any strong statements about skill composition shifts.

To examine the role of new technology more directly, I investigate the importance of ICT capital for the employment response of firms in more detail. I use auxiliary data on ICT capital stock for 13 manufacturing industry groups from EU KLEMS, and divide firms into below and above median according to the ICT capital share at the industry level in 1998. Panel B of Table 3 presents the results of the heterogeneous effect analysis by these ICT reliance groups. The estimates in column 1 show that the effect on total employment is similar for both industry groups. When focusing on different skill groups in columns 2–4, I find important differences across industries. For firms in industries with low ICT reliance, the effect on college-educated employment is 7.7 log points, whereas on non-college-educated it is 10.3 log points. Thus, non-college-educated workers actually have more favorable outcomes, although the effect on the skill ratio is statistically insignificant. In the high ICT reliance group, this pattern is reversed. The effect on college-educated workers is 17.7 log points, compared to an effect of 8.2 log points on non-college-educated workers. This difference in employment effects by skill translates to a statistically significant effect on the skill ratio of 8.8 log points, suggesting composition changes in these industries.

The effects by occupation group in columns 5–7 are broadly consistent with those by skill. For the low ICT reliance group, the effects on abstract and routine occupations are about the same, while for high ICT reliance, the effect is larger for abstract occupations. However, the estimates for the employment ratio are statistically insignificant. Overall, the adjustment patterns from panel B, particularly the shift towards college-educated workers among firms in industries with high ICT reliance, support the common concern that the employment response can favor highly educated workers when new technologies are involved. Nonetheless, overall composition changes are muted in the context of the German investment tax credits, with employment effects being positive for all labor types, possibly because the policy does not encourage firms to fundamentally adjust their production to new technologies. In industries with a higher reliance on ICT technology, existing skill bias could explain the composition changes.

6.2 Indirect Effects

I now turn to the indirect effects of investment tax credits. For each dependent variable, Table 4 presents the results from specification (12) and when I further include the demeaned regional labor share as interaction term. The first row of estimates captures the direct effects of tax credits for this specification. For log capital, log employment, and log domestic sales, the coefficient estimates of the direct effect are all close in magnitude to results from specification (11). The identification of indirect effects at the district level (compared to the broader labor market-year fixed effects) therefore does not show a material impact on the direct effect estimation.

The second row of estimates captures the indirect effects based on the comparison of firms by the regional employment share of firms below the cutoff. The estimate for log capital in column 1 is 0.240, implying that firms in regions with a higher employment share are increasing their capital inputs more. This result provides evidence that investment tax credits generate positive indirect effects for firms, independent of their capital cost reduction. The actual magnitude of the effect depends on the employment share itself. For the average district in East Germany with an employment share of 65.8%, the indirect effect leads to an increase in capital by 15.8 log points relative to a scenario of no capital cost change. Compared to a direct effect of 15.1 log points, this additional firm response is large. Column 2 shows the corresponding estimation when I control for the regional labor share, proxied by the wage bill over total sales at the district level in 1998. The results for the direct and indirect effect remain nearly unchanged, suggesting that the regional labor share does not bias the indirect effect estimation.[25] In addition, I do not find any clear relationship between the labor share and the capital response more generally, with estimates being statistically insignificant.

Columns 3 and 4 summarize the employment results. The estimates of the indirect effect are 0.100 and 0.111 for the specification without and with the labor share as a control, respectively. Translated to firms in the average East German region, these estimates imply a sizable indirect employment effect of 6.6 and 7.3 log points, compared to the direct effect of 9.3 and 9.2 log points. While I find a statistically significant effect of the regional labor share on employment in column 4, this relationship is weak, with only a minor influence on the main estimates. Finally, columns 5 and 6 show the indirect effect on domestic sales with point estimates of 0.054 and 0.055, respectively. These results suggest a corresponding output adjustment through indirect adjustment channels. However, the implied effects are smaller than those for inputs and statistically insignificant.[26] Taken together, the results suggest substantial positive indirect effects due to investment tax credits, particularly for production inputs. The direct effects of capital cost changes alone therefore understate the overall impact of such policies.

The theoretical framework suggests two mechanisms through which indirect effects manifest. Agglomeration economies form the main channel for positive effects in both capital and employment, shifting production capacities upward. Inelastic local labor supply may have a further impact through changes in wages. For this second channel, columns 7 and 8 present the results from wage estimations. For the direct wage effect, the estimates suggest a decrease of average wages by 2.5 log points for firms that receive

[25] Using the labor share at the firm level, I obtain estimates close in magnitude.

[26] Panel B of Appendix Table C.1 shows that the coefficient estimate for the indirect effect on total sales is positive and larger than that for domestic sales. While the results are statistically insignificant, they support the general adjustment patterns of the main analysis.

Table 4: Direct and Indirect Effects of Investment Tax Credits

	Log Capital		Log Employment		Log Domestic Sales		Log Avg. Firm Wage	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
After 1998 × Small firm	0.151***	0.151***	0.093***	0.092***	0.089**	0.089**	-0.025***	-0.025***
	(0.054)	(0.054)	(0.027)	(0.027)	(0.041)	(0.040)	(0.009)	(0.009)
After 1998 × Share small (district)	0.240***	0.234**	0.100**	0.111***	0.054	0.055	0.016	0.017
	(0.090)	(0.093)	(0.042)	(0.041)	(0.062)	(0.063)	(0.013)	(0.014)
After 1998 × Labor share (demeaned)		0.579		-0.740*		0.052		0.035
		(0.923)		(0.394)		(0.510)		(0.161)
After 1998 × Share below × Labor share (demeaned)		-0.950		0.918		-0.164		-0.136
		(1.518)		(0.608)		(0.852)		(0.235)
Observations	14,931	14,931	16,289	16,289	16,281	16,281	16,289	16,289

Note. Each column reports estimates based on specification (12) with either the standard set of controls or additionally including interaction terms for the demeaned labor share (proxied by wage bill over total sales in 1998) at the district level. The dependent variables are the log of total capital stock in columns 1 and 2, log employment in columns 3 and 4, log of domestic sales in columns 5 and 6, and log of average firm wage in columns 7 and 8. Additional controls are pre-treatment wage growth, pre-treatment employment growth, firm fixed effects, industry-year fixed effects, and federal state-year fixed effects. Results are based on the AFiD data. Standard errors in parentheses are clustered at the district level: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

a capital cost reduction. One explanation for this result is that the expansion of employment coincides with firms hiring less-productive workers (Caliendo et al., 2015). For the indirect wage effect, I find statistically insignificant and small estimates of 0.016 and 0.017 in columns 7 and 8, respectively. These results suggest that wages are unresponsive at the regional level and, given the increase in labor demand, would imply a perfectly elastic local labor supply. In this case, agglomeration economies remain as the mechanism for generating the indirect effects. Again, firms could hire less-productive workers who earn lower wages, in which case the indirect effect would be a lower bound.

To investigate how changes in observable worker and job characteristics may interfere in the estimation of the wage response, I analyze log wage residuals as dependent variables, with results shown in Appendix Table C.2. The log wage residuals are aggregated at the firm level from individual-level regressions of log wage on worker characteristics in the BeH employer-employee data. I obtain residuals for different sets of worker characteristics and for a specification that interacts all controls with industry fixed effects. Compared to the results for log average firm wages, controlling for observable characteristics in the log wage residuals reduces the direct wage effect. The strongest impact comes from the workers' tenure at their current establishment, with estimates being reduced by roughly half.

Other characteristics, such as education and the type of occupation, have instead only a minor impact on the estimates. Thus, the negative direct wage effect is driven by hiring new employees who systematically earn lower wages than established workers, possibly because of lower firm-specific skills. The coefficient estimate for log wage residuals remains negative and statistically significant, even when interacting all observable characteristics with industry fixed effects. In this case, the effect amounts to -0.8 log points, possibly because of differences in unobservable characteristics.

For the indirect effect, the wage residual estimations lead instead to no substantial changes. Estimates remain close to zero and statistically insignificant, suggesting no indirect wage effect even when I account for observable characteristics. The results therefore reinforce the interpretation that local labor supply is highly responsive and that agglomeration economies explain all of the indirect effects.

Figure 4 summarizes the yearly estimates for the indirect effects from specification (14), which provides evidence for the indirect dynamic firm behavior before and after the policy change. For log capital and log employment in panels A and B, respectively, firms move in parallel before the policy change, with estimates being close to zero and statistically insignificant. Starting in 1999, estimates turn positive and statistically significant. For capital, the effect is evident in the first year after the policy change, with small additional increases in subsequent years. For employment, the adjustments occur more gradually over a four-year period, after which estimates stabilize. The response for log domestic sales in panel C shows comparable adjustment patterns, with estimates increasing after the policy change and stabilizing after three years. The results, however, are statistically insignificant. For log average firm wage in panel D, estimates are close to zero and statistically insignificant throughout, in line with the small average indirect wage effect in Table 4.

The analysis thus far identifies indirect effects at the district level. To analyze the spatial propagation and attenuation of these effects, in Table 5 I present estimation results for flexible distance intervals around each firm. As I can calculate these measures only for the BeH data, I am restricted to the analysis of employment. As a useful comparison with the previous results from the AFiD data, column 1 summarizes the results when I use district boundaries. The estimates of the direct and indirect effects correspond well with the previous results. I find a direct effect of 12.0 log points and additional employment

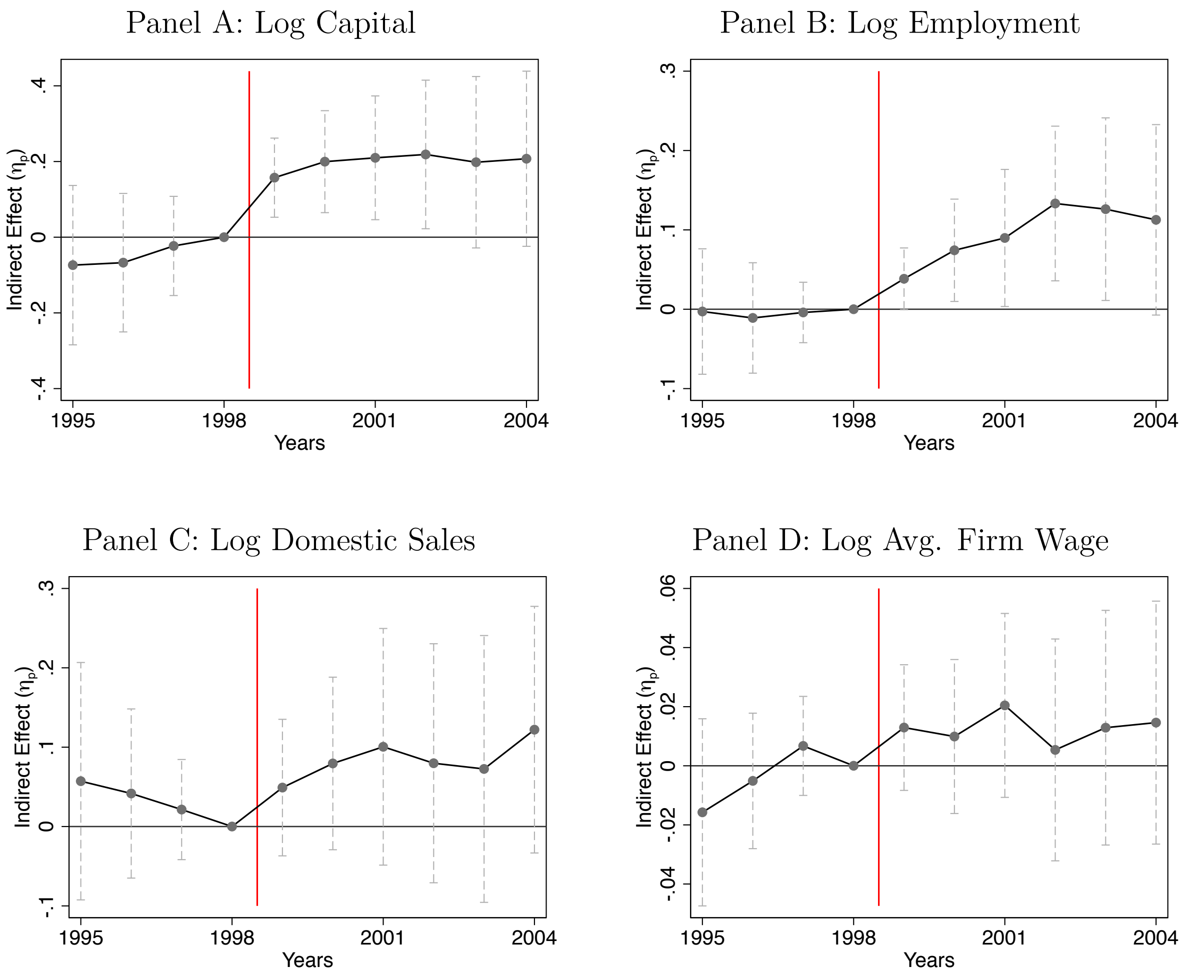

Figure 4: Evolution of Indirect Tax Credit Effects

Note. Each panel reports the estimates for the indirect effect based on specification (14). The dependent variables are the log of total capital stock in panel A, log employment in panel B, log of domestic sales in panel C, and log of average firm wage in panel D. Additional controls are pre-treatment wage growth, pre-treatment employment growth, firm fixed effects, industry-year fixed effects, and federal state-year fixed effects. Results are based on the AFiD data. The dashed lines show the 95% confidence interval for each estimate, with standard errors clustered at the district level.

growth from indirect effects of 7.0 log points for the average regional employment share below the cutoff for East Germany in the BeH data (73.9%). Columns 2–6 show, one by one, different distance intervals around each firm. The estimates for the direct effect do not vary markedly, fluctuating between 0.118 and 0.128. This finding again shows evidence that the model specification has a minor influence on the estimates of the direct effect. However, for the indirect effects, the results are sensitive to the specific distance interval. For a distance interval of up to 2 kilometers (∼1.2 miles) in column 2, the estimate is 0.052. The magnitude amounts to more than half of the estimate at the district level, pointing to the importance of nearby firms for generating indirect effects.

Table 5: Agglomeration Effects By Distance

	Dependent Variable: Log Employment						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
After 1998 × Small firm	0.120***	0.118***	0.127***	0.128***	0.124***	0.124***	0.122***
	(0.023)	(0.025)	(0.024)	(0.024)	(0.024)	(0.024)	(0.025)
After 1998 × Share small							
Within district	0.095**						
	(0.040)						
Within (0km, 2km]		0.052**					0.057**
		(0.024)					(0.025)
Within (2km, 5km]			0.059***				0.063**
			(0.022)				(0.025)
Within (5km, 10km]				-0.014			-0.010
				(0.026)			(0.027)
Within (10km, 25km]					-0.012		-0.038
					(0.053)		(0.056)
Within (25km, 50km]						0.023	0.026
						(0.082)	(0.084)
Observations	20,590	18,760	19,120	20,350	20,590	20,590	17,520

Note. Each column reports estimates based on specification (12), with the indirect effect estimation using the employment share of firms up to 250 employees either at the district level or for different distance intervals. The dependent variable is the log of total employment throughout. Additional controls are pre-treatment wage growth, pre-treatment employment growth, firm fixed effects, industry-year fixed effects, and federal state-year fixed effects. Results are based on the BeH data. Standard errors in parentheses are clustered at the district level: * p<0.10, ** p<0.05, *** p<0.01.

For distances of 2 to 5 kilometers (∼1.2 to 3.1 miles) in column 3, the estimate is similar in magnitude, taking the value of 0.059.

In contrast, for subsequent distance intervals in columns 4–6, estimates are small and statistically insignificant, corresponding to -0.014 for 5 to 10 kilometers, -0.012 for 10 to 25 kilometers and 0.023 for 25 to 50 kilometers. Such firms appear too far away to create any agglomeration benefits. Finally, to control for any dependence, column 7 combines all previous distance measures in a single estimation. As the estimates hardly change, they support the finding that indirect effects materialize in distances up to 5 kilometers. These results suggest a strong gradient of agglomeration economies with outsized importance for nearby firms. Policymakers may therefore target investment tax credits to small regional units without affecting surrounding firms through agglomeration economies.

As a final analysis of indirect effects, I study shifts in production across firms. This analysis relies on the counterfactual measures of cumulative growth from equation (15) as dependent variables, determining the influence of different types of labor flows on the overall employment effect. Table 6 summarizes the results for both the direct effect specification in panel A and for the combined specification with indirect effects in panel

Table 6: Effects By Employment Flows

	Dependent Variable: Log of Cumulative Employment Growth By Flow Groups						
	All	Non-Employed	Job-to-job	Large Firms	Within District	Other East	Services
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Only Direct Effects							
After 1998 × Small firm	0.112***	0.040***	0.071***	-0.007	0.008	0.002	0.040***
	(0.029)	(0.013)	(0.022)	(0.014)	(0.015)	(0.004)	(0.011)
Observations	20,590	20,590	20,590	20,590	20,590	20,590	20,590
Panel B. Including Indirect Effects							
After 1998 × Small firm	0.120***	0.046***	0.073***	-0.006	0.010	0.002	0.040***
	(0.023)	(0.011)	(0.019)	(0.013)	(0.014)	(0.003)	(0.010)
After 1998 × Share small (district)	0.095**	0.079***	0.014	-0.001	0.016	-0.014*	-0.008
	(0.040)	(0.024)	(0.025)	(0.007)	(0.011)	(0.008)	(0.019)
Observations	20,590	20,590	20,590	20,590	20,590	20,590	20,590

Note. Each column reports estimates based on specification (11) in panel A and specification (12) in panel B. The dependent variables are the log of cumulative employment growth, using all types of worker flows in column 1, flows to and from non-employment in column 2, flows to and from other establishments in column 3, flows to and from establishments of the estimation sample that are part of the group of large firms, located in the same district, and located in other districts in East Germany in columns 4–6, respectively, and flows to and from establishments in the service sector in column 7. Additional controls are pre-treatment wage growth, firm fixed effects and industry-year fixed effects. Panel A includes labor market-year fixed effects and panel B includes pre-treatment employment growth and federal state-year fixed effects. Results are based on the BeH data. Standard errors in parentheses are clustered at the district level: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

B. The estimates based on the cumulative growth of total inflows and outflows in column 1 are identical to the employment results from the BeH data, as is expected from the equivalence of specifications. Column 2 reports the effects for flows to and from non-employment. In panel A, the estimate translates to a direct effect of 4.0 log points, indicating that 35.7% of the direct employment effect is driven by either hiring more workers who were non-employed one year earlier or separating from fewer workers who would be non-employed one year later.

Column 3 of panel A shows the results for job-to-job transitions, which generate an effect of 7.1 log points. The direct employment effect, therefore, is driven by a sizable share of labor mobility across firms. This mobility between firms could indicate shifts across firms in the estimation sample. In this case, the effects of the main analysis would not translate to aggregate effects. However, column 4 of panel A provides no evidence for a shift towards small firms, which receive the relative cost reduction, with a statistically insignificant estimate of -0.006 based on flows from large to small firms within the estimation sample. In columns 5 and 6 of panel A, when distinguishing flows between firms in the estimation sample in the same or in different East German districts, I likewise find no statistically significant results either. Firms with a capital cost reduction thus do not appear to benefit at the expense of other manufacturing firms more generally. Rather than movements within the manufacturing sector, in column 7 of panel A, the estimate provides evidence that changes in mobility with the service sector drive the job-to-job transition effect, explaining 56.3% of the direct employment effect. These results point to changes in the overall production structure across sectors.[27]

In panel B, the estimates for the direct effect remain qualitatively unchanged. I therefore focus on the estimates of the indirect effect. The indirect effect is largely driven by transitions to and from non-employment with the estimate of 0.079 in column 2 explaining 83.2% of the total. The estimate for job-to-job transitions in column 3 is 0.014 and statistically insignificant. Furthermore, when analyzing different subgroups of job-to-job transitions in columns 4–7, I do not find any clear evidence for shifts towards the expanding firms. The only statistically significant effect occurs for worker flows from manufacturing firms across different East German districts in column 6. But the estimate is small and negative. Rather than firms expanding in regions with large indirect effects on the expense of firms in other regions, this result suggests that the opposite may hold true.

By using labor flows that capture direct movements across firms, the analysis inevitably disregards additional adjustment margins. Potentially, one set of firms could reduce employment while others hire from a different pool of workers as a result of general equilibrium adjustments. In this case, I would mistakenly attribute shifts across firms to movements with non-employment. However, given that firms within the same region generally share a similar worker pool and that job-to-job transitions are observably important even across regions, such hiring patterns appear unlikely without evidence on direct movements. The results therefore point to aggregate employment effects in the manufacturing sector, with effects facilitated by movements to and from non-employment and the service sector.

[27] Labor flows away from the service sector could be offset by flows from non-employment. Even then, firms in the service sector would incur additional hiring costs.

6.3 Robustness Tests

The main analysis builds on a particular estimation sample and estimation specification. In a first robustness test, I show that the results are not driven by these choices. Figure 5 summarizes estimates for the direct and indirect effects on log capital, log employment and log domestic sales for a variety of control variables and sample selections. As a baseline, row 1 reproduces the main estimates in Table 4. I start with the influence of the control variables on the estimates. Rows 2 and 3 present results for the exclusion of pre-treatment trends and a further exclusion of industry-year and federal state-year fixed effects, respectively. Both changes have a limited impact on point estimates but generally decrease precision as is evident from larger confidence intervals. Thus, neither industry-specific and regional shocks, nor pre-treatment differences show any clear correlation with the policy change. Nonetheless, to benefit from higher precision in the estimation, I still include these controls in the preferred specification.

For the remaining robustness specifications, I vary the sample selection. Rows 4 and 5 show results for changes to the included smallest and largest firms. A wider interval of firms with policy-relevant firm sizes of 20 to 3,000 (while firms around the cutoff are still excluded) in row 4 has a limited influence on point estimates. The same conclusion holds for a narrower interval of firm sizes between 60 to 1,000 in row 5. A comparison of the direct effects of both size intervals with the main specification shows that the employment and sales effects tend to be larger when I select wider firm size intervals. For capital, however, the relationship between the effect size and the number of firms is not monotonous in the sample, with direct effects being larger for the small and large intervals than for the main specification. The more evident differences occur for standard errors. For the indirect effects in particular, a wide firm size interval leads to narrower confidence intervals. As a result, the estimate for the indirect effect on domestic sales turns statistically significant when I use the wide firm size interval. In the preferred specification, I include firms between 40 and 1,500, as this interval provides a balance between the similarity of characteristics among firms and having enough power for the identification of the main effects.

Rows 6 and 7 report results for different intervals of excluded firms close to the cutoff. Excluding firms around the cutoff in a wider firm size interval of 200 to 300 or excluding

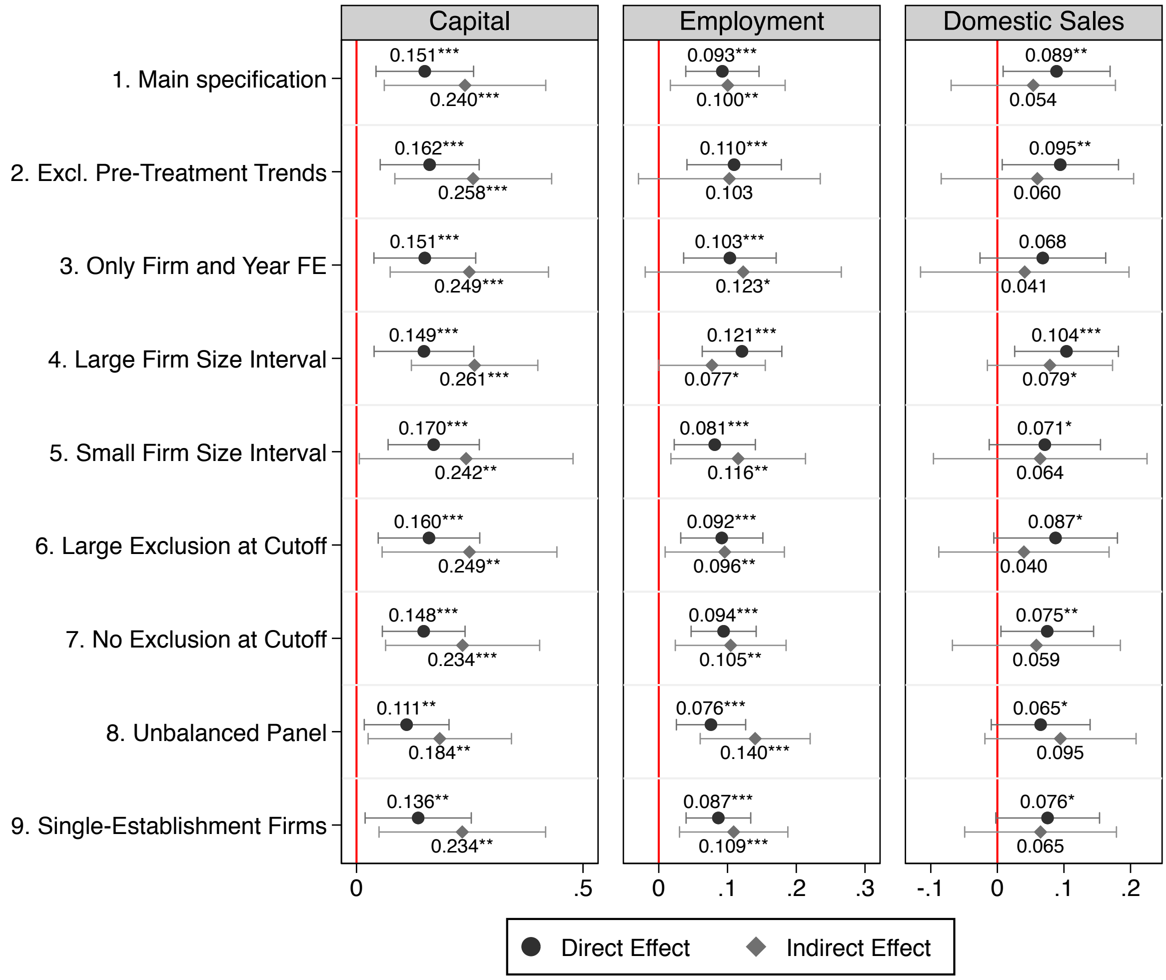

Figure 5: Robustness Tests of the Empirical Specification and Sample Selection

Note. Each row reports estimates based on specification (12) with adjustments to either the included control variables or the sample selection. The dependent variables are the log of total capital stock in the left panel, log employment in the middle panel, and log of domestic sales in the right panel. Compared to the main specification in row 1, row 2 excludes pre-treatment trend controls; row 3 excludes pre-treatment trend controls, industry-year fixed effects, and federal state-year fixed effects; row 4 selects firms with policy-relevant firm size in 1998 between 20 and 3,000 (excluding firms between 225 and 275); row 5 selects firms with policy-relevant firm size in 1998 between 60 and 1,000 (excluding firms between 225 and 275); row 6 excludes firms with policy-relevant firm size in 1998 between 200 and 300; row 7 excludes no firms close to the cutoff; row 8 drops the restriction of firms being active throughout 1995–2004; and row 9 only includes single-establishment firms. Results are based on the AFiD data. The solid lines around the coefficients show the 95% confidence interval.

no firms has a limited impact on estimates, possibly because the number of firms close to the firm size cutoff is small relative to the overall sample. In row 8, the sample includes firms entering the data after 1995 or exiting before 2004 in an unbalanced panel. While the estimates for the direct and indirect effects are qualitatively consistent with the firm adjustment patterns of a balanced panel, they tend to be smaller. With new firms assumed to enter as small firms, lower estimates could point to a weaker response among this group of firms. Sample composition changes, however, make a comparison of the coefficient estimates difficult. The estimation approach does not capture the effects on firm entry and exit decisions either, making the balanced panel my preferred choice. Finally, as a check for misclassifications of multi-establishment firms to specific industries and regions, row 9 shows estimates for a specification with only single-establishment firms. I find no evidence that single-establishment firms react markedly differently to the relative capital cost change. In sum, the estimates of all robustness specifications are broadly consistent with those of the main analysis and support the conclusion that investment tax credits lead to an expansion of firm production through both the direct effects of a capital cost reduction and the indirect effects at the regional level.

In a second robustness analysis, I deal with the concern that the empirical analysis may pick up effects from other support programs. Government support for East Germany was widespread after reunification, with additional programs targeting, for example, infrastructure and housing projects. As these programs did not directly target firms, they likely create only aggregate influences that I capture with the included fixed effects. For East German firms, the *Gemeinschaftsaufgabe Verbesserung der regionalen Wirtschaftsstruktur* (GRW) was an important program that provided investment subsidies at the discretion of state governments (Brachert et al., 2018; Siegloch et al., 2021). While the GRW prioritized large-scale investment projects and plant openings, its overall goal to create stronger local economies was similar to that of the investment tax credits.

During the sample period, changes to the maximum subsidy rates occurred for a subset of East German districts in 1997 and 2000 (Siegloch et al., 2021), potentially biasing the main analysis. From the official government reports, I identify all districts and years affected by the decrease in GRW subsidy rates, and include a dummy as a control. While the GRW did not differentially adjust subsidy rates by firm size, to check for any impact on my main estimates, I further interact this control variable with a dummy for small firms according to the policy-relevant firm size in 1998.

Appendix Table C.3 summarizes the results for the direct effects in panel A and for when I include indirect effects in panel B. Compared to the main analysis, estimates for the investment tax credits show only minor deviations and stay qualitatively the same. The largest difference occurs for the direct effect of the sales estimations, for which point estimates are 0.113 (compared to 0.096) in panel A and 0.106 (compared to 0.089) in panel B. These findings suggest that the GRW does not create any clear biases in the main analysis, nor do the estimates of the control variables indicate any clear impact of

Table 7: Placebo Analysis - West Germany

	Log Capital	Log Employment	Log Domestic Sales	Log Avg. Firm Wage
	(1)	(2)	(3)	(4)
Panel A. Only Direct Effects				
After 1998 × Small firm	0.035 (0.049)	0.020 (0.033)	-0.002 (0.043)	-0.009 (0.010)
Observations	15,903	16,290	16,284	16,289
Panel B. Including Indirect Effects				
After 1998 × Small firm	0.012 (0.041)	0.014 (0.022)	-0.009 (0.034)	-0.008 (0.010)
After 1998 × Share small (district)	0.054 (0.152)	0.033 (0.053)	0.026 (0.068)	-0.007 (0.026)
Observations	15,903	16,290	16,284	16,289

Note. Each column reports estimates based on specification (11) in panel A and specification (12) in panel B. The dependent variables are the log of total capital stock in column 1, log employment in column 2, log of domestic sales in column 3, and log of average firm wage in column 4. Additional controls are pre-treatment wage growth, firm fixed effects and industry-year fixed effects. Panel A includes labor market-year fixed effects, and panel B includes pre-treatment employment growth and federal state-year fixed effects. Results are based on the AFiD data. Standard errors in parentheses are clustered at the district level: * p<0.10, ** p<0.05, *** p<0.01.

the GRW on firms in the estimation sample either, possibly because each program targets a different set of firms.

Finally, the focus of the tax policy on East German firms allows for a placebo analysis. If changes in investment tax credit rates are driving my results, then comparable West German firms, which were not eligible for investment tax credits, should not show the same differential production behavior. However, East German firms generally do not have comparable characteristics with their West German counterparts, as previously highlighted by the differences in the descriptive statistics, thereby creating difficulties in selecting appropriate firms.

I resolve this issue by relying on matching on observables to select firms closest in their characteristics. I use Mahalanobis matching, which calculates the Euclidean distance of all matching variables standardized by their variance. This approach aims to balance all included covariates and works well for small numbers of covariates (Stuart and Rubin, 2008). To match firms of comparable size and with similar production trends before the policy change, I use average employment and worker wage, and employment and wage growth over the period 1995–1998. To make firms comparable at the regional level, I also match on average firm size within labor markets during the same period. After obtaining the Mahalanobis distance, I select for each East German firm in the estimation sample the closest West German firm fulfilling the same sample restrictions. I then apply the difference-in-differences strategy from the main analysis to this matched sample of West German firms.

Table 7 summarizes the results for both the direct effect specification and the combined specification with indirect effects. For both specifications and all outcomes, estimates are statistically insignificant and close to zero. These findings suggest that firms in West Germany did not change production differently by firm size or across regions, in line with these firms being excluded from investment tax credits. Furthermore, these results suggest that no aggregate trends during the sample period affect firms differently by either firm size or the regional employment share of firms below the cutoff. Therefore, without the policy change, East German firms would likely not have the differential behavior found in the main analysis.

7 Conclusion

In this paper, I investigate the impact of investment tax credits in Germany on the response of firms. I go beyond the analysis of capital and study the effects on employment, employment composition, sales, labor productivity, and wages. By exploiting a relative capital cost change by firm size in a difference-in-differences setting, I not only causally estimate the direct effects of a capital cost reduction but also analyze the indirect effects at the regional level. The indirect effect estimation relies on the link between the effect size and the regional employment share of firms receiving the relative cost reduction, a relationship that I establish in a model of labor demand with agglomeration economies and local labor supply.

I find that, after receiving a capital cost reduction, firms increase their capital stock, employment, and domestic sales. Labor demand increases for all worker types, independent of skill level or occupational task content, although the impact is larger for college-educated than for non-college-educated workers in industries with a high ICT capital share. Indirect effects lead to additional benefits for all firms within a region, with empirical results showing positive effects on capital and employment. Statistically insignificant results for the wage response across regions suggest that local labor supply is perfectly elastic and that agglomeration economies explain the entire indirect effect. An additional analysis of the indirect effects by distance shows that the employment effects are confined to a radius of 5 kilometers ($\sim$3 miles). Finally, I show that the employment effects materialize through net inflows from non-employment and from the service sector. In contrast, movements between differently treated manufacturing firms do not explain the effects.

Overall, these results provide a favorable assessment of investment tax incentives. Many of the intended effects, such as higher labor demand and indirect adjustments through agglomeration economies, are realized. A positive labor demand response for low-skilled workers and movements from non-employment even suggest beneficial welfare effects. Given this positive assessment, an open question is how investment tax incentives compare to other fiscal policies, with the answer to this question likely depending on the particular circumstances. While investment tax incentives may be cost-effective for compensating negative output shocks in the short term, I find that firms do not improve their productivity. For upholding the effects in the long term, governments may therefore face increasingly higher costs.

Another avenue for future work concerns the policy choice between investment tax credits and accelerated depreciation allowances. I show broad positive effects for investment tax credits. However, in a comparison with previous studies on accelerated depreciation allowances, I find a significantly smaller elasticity of investment. Preferably, both types of policies should be compared in the same setting, as a way to exclude outside influences. Without such an opportunity, improving the understanding of the firms' perceptions of the different incentive structures could help policymakers in the choice between both investment tax policies.

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Appendix

A Theoretical Framework

A.1 Maximization Problem of Firms

$$\max_{K_i,L_i} p_i Y_i - (1-\tau_i) r K_i - w_r L_i, \tag{16}$$

subject to

$$p_i = B Y_i^{-\frac{1}{\eta^D}} \tag{17}$$

$$F(K_i, L_i) = Y_i = A_r A_i \left(a_K K_i^\rho + a_L L_i^\rho\right)^{\frac{1}{\rho}} \tag{18}$$

A.2 Total Derivatives of Output and FOCs

$$\frac{dY_i}{Y_i} = \frac{dA_r}{A_r} + \frac{a_K K_i^\rho}{a_K K_i^\rho + a_L L_i^\rho} \frac{dK_i}{K_i} + \frac{a_L L_i^\rho}{a_K K_i^\rho + a_L L_i^\rho} \frac{dL_i}{L_i} \tag{19}$$

$$\frac{dK_i}{K_i} = \left(1 - \frac{1}{\eta^D} \frac{1}{1-\rho}\right) \frac{dY_i}{Y_i} + \frac{\rho}{1-\rho} \frac{dA_r}{A_r} + \frac{1}{1-\rho} \frac{d\tau_i}{1-\tau_i} \tag{20}$$

$$\frac{dL_i}{L_i} = \left(1 - \frac{1}{\eta^D} \frac{1}{1-\rho}\right) \frac{dY_i}{Y_i} + \frac{\rho}{1-\rho} \frac{dA_r}{A_r} - \frac{1}{1-\rho} \frac{dw_r}{w_r} \tag{21}$$

A.3 Labor and Capital Share

With the production function equation and the FOCs, it is possible to rewrite

$$\frac{a_L L_i^\rho}{a_K K_i^\rho + a_L L_i^\rho} = a_L L_i^\rho \left(\frac{A_i A_r}{Y_i}\right)^\rho = \frac{w_r L_i}{p_i Y_i} \left(\frac{\eta^D}{\eta^D - 1}\right). \tag{22}$$

With the assumption of zero tax credits at baseline, the capital-labor ratio depends on the production function parameters, wages and the rental rate of capital, which are constant at the regional level. The previous term therefore is equal for all firms in a region and can be simplified to

$$\frac{a_L L_i^\rho}{a_K K_i^\rho + a_L L_i^\rho} = s_r^L H. \tag{23}$$

The derivations are analogous for the capital share and lead to

$$\frac{a_K K_i^\rho}{a_K K_i^\rho + a_L L_i^\rho} = s_r^K H. \tag{24}$$

A.4 Indirect Effects

In a first step, using the definitions from the main text, I relate the change in the productivity shifter and wage to the change in regional employment

$$\frac{dA_r}{A_r} = \frac{d(\sum_{j\in j_r} L_j)^\lambda}{A_r} = \lambda \sum_{j\in j_r} \frac{L_j}{L_r}\frac{dL_j}{L_j}, \qquad \frac{dw_r}{w_r} = \frac{1}{\varphi}\frac{dL_r}{L_r} = \frac{1}{\varphi}\sum_{j\in j_r}\frac{L_j}{L_r}\frac{dL_j}{L_j}, \tag{25}$$

where j_r represents the set of firms in region r.

Using these terms in the equations for the firm response, I obtain the relation for the indirect employment response

$$\left(\frac{dL_i}{L_i}\right)_{ind} = \left[(\eta^D - 1)\lambda - (\eta^D s_r^L + \frac{1}{1-\rho}s_r^K)H\frac{1}{\varphi}\right]\sum_{j\in j_r}\frac{L_j}{L_r}\left[\left(\frac{dL_j}{L_j}\right)_{dir} + \left(\frac{dL_j}{L_j}\right)_{ind}\right]. \tag{26}$$

Since indirect effects are equal for firms within regions, I rearrange for the final result

$$\left(\frac{dL_i}{L_i}\right)_{indirect} = \frac{(\eta^D - 1)\lambda - (\eta^D s_r^L + \frac{1}{1-\rho}s_r^K)H\frac{1}{\varphi}}{1-(\eta^D - 1)\lambda + (\eta^D s_r^L + \frac{1}{1-\rho}s_r^K)H\frac{1}{\varphi}}\sum_{j\in j_r}\frac{L_j}{L_r}\left(\frac{dL_j}{L_j}\right)_{direct}. \tag{27}$$

The corresponding capital response is

$$\left(\frac{dK_i}{K_i}\right)_{ind} = \left[(\eta^D - 1)\lambda - (\eta^D - \frac{1}{1-\rho})s_r^L H\frac{1}{\varphi}\right]\sum_{j\in j_r}\frac{L_j}{L_r}\left[\left(\frac{dL_j}{L_j}\right)_{dir} + \left(\frac{dL_j}{L_j}\right)_{ind}\right], \tag{28}$$

which simplifies with the result for the indirect labor response to

$$\left(\frac{dK_i}{K_i}\right)_{indirect} = \frac{(\eta^D - 1)\lambda - (\eta^D - \frac{1}{1-\rho})s_r^L H\frac{1}{\varphi}}{1-(\eta^D - 1)\lambda + (\eta^D s_r^L + \frac{1}{1-\rho}s_r^K)H\frac{1}{\varphi}}\sum_{j\in j_r}\frac{L_j}{L_r}\left(\frac{dL_j}{L_j}\right)_{direct}. \tag{29}$$

In the case that firms either receive a tax credit rate change ($d\tau_i = d\tau$) or no change

$(d\tau_i = 0)$, the indirect effects can be written as

$$\left(\frac{dK_i}{K_i}\right)_{indirect} = \frac{(\eta^D - 1)\lambda - (\eta^D - \frac{1}{1-\rho})s_r^L H \frac{1}{\varphi}}{1 - (\eta^D - 1)\lambda + (\eta^D s_r^L + \frac{1}{1-\rho} s_r^K) H \frac{1}{\varphi}} \left(\eta^D - \frac{1}{1-\rho}\right) s_r^K H \frac{L_{\tau,r}}{L_r} d\tau \tag{30}$$

$$\left(\frac{dL_i}{L_i}\right)_{indirect} = \frac{(\eta^D - 1)\lambda - (\eta^D s_r^L + \frac{1}{1-\rho} s_r^K) H \frac{1}{\varphi}}{1 - (\eta^D - 1)\lambda + (\eta^D s_r^L + \frac{1}{1-\rho} s_r^K) H \frac{1}{\varphi}} \left(\eta^D - \frac{1}{1-\rho}\right) s_r^K H \frac{L_{\tau,r}}{L_r} d\tau. \tag{31}$$

A.5 Employment Ratio with Two Types of Labor

For the impact of investment tax credit on different labor types, the substitution elasticity of capital with each labor type is important. I consider a nested CES production function with two types of labor that allows for differences in the elasticity of substitution. I further allow for different wages and labor supply elasticities for each labor type.

With these adjustments, the maximization problem is

$$\max_{K_i, S_i, U_i} p_i Y_i - (1 - \tau_i) r K_i - w_r^S S_i - w_r^U U_i, \tag{32}$$

subject to

$$p_i = B Y_i^{-\frac{1}{\eta^D}} \tag{33}$$

$$F(K_i, U_i, S_i) = Y_i = A_i A_r \left[(a_K K_i^\rho + a_S S_i^\rho)^{\frac{\mu}{\rho}} + a_U U_i^\mu \right]^{\frac{1}{\mu}} \tag{34}$$

The first order conditions from the maximization problem are

$$(1 - \tau_i) r = \left(1 - \frac{1}{\eta^D}\right) B a_K Y_i^{1-\mu-\frac{1}{\eta^D}} (a_K K_i^\rho + a_S S_i^\rho)^{\frac{\mu-\rho}{\rho}} K_i^{\rho-1} (A_i A_{ir})^\mu \tag{35}$$

$$w_r^S = \left(1 - \frac{1}{\eta^D}\right) B a_S Y_i^{1-\mu-\frac{1}{\eta^D}} (a_K K_i^\rho + a_S S_i^\rho)^{\frac{\mu-\rho}{\rho}} S_i^{\rho-1} (A_i A_{ir})^\mu \tag{36}$$

$$w_r^U = \left(1 - \frac{1}{\eta^D}\right) B a_U Y_i^{1-\mu-\frac{1}{\eta^D}} U_i^{\mu-1} (A_i A_{ir})^\mu. \tag{37}$$

Totally differentiating the FOCs leads to

$$-\frac{d\tau_i}{1-\tau_i} = \left(1-\mu-\frac{1}{\eta^D}\right)\frac{dY_i}{Y_i} + (\mu-\rho)(X_i^K\frac{dK_i}{K_i} + X_i^S\frac{dS_i}{S_i}) + (\rho-1)\frac{dK_i}{K_i} + \mu\frac{dA_r}{A_r} \tag{38}$$

$$\frac{dw_r^S}{w_r^S} = \left(1-\mu-\frac{1}{\eta^D}\right)\frac{dY_i}{Y_i} + (\mu-\rho)(X_i^K\frac{dK_i}{K_i} + X_i^S\frac{dS_i}{S_i}) + (\rho-1)\frac{dS_i}{S_i} + \mu\frac{dA_r}{A_r} \tag{39}$$

$$\frac{dw_r^U}{w_r^U} = \left(1-\mu-\frac{1}{\eta^D}\right)\frac{dY_i}{Y_i} + (\mu-1)\frac{dU_i}{U_i} + \mu\frac{dA_r}{A_r}, \tag{40}$$

where $X_i^K = 1 - X_i^S = \frac{a_K K_i^\rho}{a_K K_i^\rho + a_S S_i^\rho}$.

Based on the total derivatives of the FOCs, the change in inputs can be written as

$$\frac{dK_i}{K_i} = \left(1 - \frac{1}{(1-\mu)\eta^D}\right)\frac{dY_i}{Y_i} + \left(\frac{1}{1-\mu} - \frac{\mu-\rho}{(1-\mu)(1-\rho)}X_i^S\right)\frac{d\tau_i}{1-\tau_i}$$
$$+ \frac{\mu}{1-\mu}\frac{dA_r}{A_r} - \frac{\mu-\rho}{(1-\mu)(1-\rho)}X_i^S\frac{dw_r^S}{w_r^S} \tag{41}$$

$$\frac{dS_i}{S_i} = \left(1 - \frac{1}{(1-\mu)\eta^D}\right)\frac{dY_i}{Y_i} + \frac{\mu-\rho}{(1-\mu)(1-\rho)}X_i^K\frac{d\tau_i}{1-\tau_i}$$
$$+ \frac{\mu}{1-\mu}\frac{dA_r}{A_r} - \left(\frac{1}{1-\rho} + \frac{\mu-\rho}{(1-\mu)(1-\rho)}X_i^S\right)\frac{dw_r^S}{w_r^S} \tag{42}$$

$$\frac{dU_i}{U_i} = \left(1 - \frac{1}{(1-\mu)\eta^D}\right)\frac{dY_i}{Y_i} - \frac{1}{1-\mu}\frac{dw_r^U}{w_r^U} + \frac{\mu}{1-\mu}\frac{dA_r}{A_r} \tag{43}$$

To consider changes in the composition of the labor force, I start out with the implicitly defined ratio of the FOCs

$$\frac{S_i}{U_i} = \left(\frac{w_r^S}{w_r^U}\frac{a_u}{a_s}\right)^{\frac{1}{\rho-1}}(a_K K_i^\rho + a_S S_i^\rho)^{\frac{\rho-\mu}{\rho(\rho-1)}} U_i^{\frac{\rho-\mu}{1-\rho}}. \tag{44}$$

The total derivative is

$$\frac{d\frac{S_i}{U_i}}{\frac{S_i}{U_i}} = \frac{\mu-\rho}{1-\rho}\left(X_i^K\frac{dK_i}{K_i} + X_i^S\frac{dS_i}{S_i}\right) - \frac{\mu-\rho}{1-\rho}\frac{dU_i}{U_i} - \frac{1}{1-\rho}\frac{dw_r^S}{w_r^S} + \frac{1}{1-\rho}\frac{dw_r^U}{w_r^U}. \tag{45}$$

Plugging in the equations for the change in inputs, the total derivative can be written as

$$\frac{d\frac{S_i}{U_i}}{\frac{S_i}{U_i}} = \left(\frac{1}{1-\mu} - \frac{1}{1-\rho}\right)X_i^K d\tau_i - \left(\frac{1}{1-\mu}X_i^S + \frac{1}{1-\rho}X_i^K\right)\frac{dw_r^S}{w_r^S} + \frac{1}{1-\mu}\frac{dw_r^U}{w_r^U}. \tag{46}$$

Changes in employment composition depend on a direct and an indirect component similar to the results on inputs. The direct component notably depends on the magnitude of each elasticity of substitution between labor type and capital. Assuming two types of skill, if the elasticity for unskilled labor is higher than the one for skilled, there is capital-skill complementarity and a capital cost reduction leads to a shift towards skilled labor. The magnitude is determined by the absolute difference of the elasticities.

The indirect component does depend only on the wage changes in a labor market. Increasing wages for skilled (unskilled) labor shift employment towards unskilled (skilled). Since agglomeration economies are modeled as factor-neutral production changes, it does not influence the skill ratio.

B Data

This section complements the data section in the main text. I provide additional information on creating the final datasets and descriptive statistics for the complete set of variables used in the empirical analysis. The following information pertains to the *AFiD-Panel Industriebetriebe* (AFiD),[28] *Kostenstrukturerhebung* (KSE),[29] and the *Beschäftigten-Historik* (BeH).

B.1 Calculation of Capital Stock in the AFiD

I calculate firms' capital stock from capital depreciation information in the KSE. Depreciation is related to capital stock by

$$D_{it} = \delta K_{it-1}, \tag{47}$$

where D_{it} is the value of depreciation of firm i in year t, δ is the depreciation rate, and K_{it-1} is the firm capital stock in year $t-1$. I determine capital stock by combining the depreciation information with industry-level depreciation rates from average economic life calculations by Müller (2017). As the KSE uses a stratified random sample each year, depreciation information is not available for all firm-year observations. Of the firms in the estimation sample, 6.7% never participated in the survey between 1995 to 2005. Another 0.8% provided depreciation information with implausibly high changes across years (increase or decrease in values by a factor of 100), or implausibly low values below EUR100. To avoid any bias from outliers, I exclude these firms. For all other firms, I obtain a capital stock measure directly from depreciation information for 52% of the observations.

To impute further values, I build on the motion of capital

$$K_{it} = (1-\delta)K_{it-1} + I_{it}, \tag{48}$$

where the current capital stock depends on the previous one adjusted by depreciation

[28]Source: RDC of the Federal Statistical Office and Statistical Offices of the Länder, AFiD-Panel Industriebetriebe, 1995–2005, own calculations.

[29]Source: RDC of the Federal Statistical Office and Statistical Offices of the Länder, Kostenstrukturerhebung, 1995–2005, own calculations.

and capital investment I_{it}. Any year with capital stock information serves as a potential starting point for imputing capital stock for the entire sample period of a firm whose investment information is in the AFiD data.

For missing values at the beginning or end of the sample period, I take the imputed capital stock from the closest starting year. For missing values in between two capital stock measures, I average across both according to

$$K_{it}^{imp} = \frac{j}{k+j} K_{it}^{imp,t-k} + \frac{k}{k+j} K_{it}^{imp,t+j}, \tag{49}$$

where K_{it}^{imp} is the imputed capital stock, $K_{it}^{imp,t-k}$ is the imputation, when only relying on starting capital in period $t-k$, and $K_{it}^{imp,t+j}$ is the imputation, when only relying on starting capital in period $t+j$. I obtain capital stock for an additional 48% of the observations.

B.2 Creating Firm Identifiers in the BeH

The regularities of the policy depend on variables at the firm level. However, the BeH data only include establishment identifiers. I aggregate the BeH data at the firm level by using the methodology of Schäffler (2014), who uses firm names recorded in the German Federal Employment Agency registries to construct firm identifiers based on the uniqueness of firm names. I take advantage of the standardized procedure for generating firm identifiers in this dataset.[30] With this dataset, I obtain a firm identifier for 94% of establishment observations between 1998 and 2004. For earlier years, missing firm name information leads to lower rates. The rate is 57% for 1997 and 4% for earlier years.

I impute firm information for earlier years by using within-establishment information. This approach is possible because the allocation of establishment identifiers follows official guidelines, with a change of the firm stipulating that a new establishment identifier should be assigned. Conversely, a constant establishment identifier over time means that the firm stayed the same. After the imputation, 90% of establishment observations for 1995–1997 have an assigned firm identifier. In a final step, I equalize firm identifiers over time. An implausibly high number of firm identifier changes within establishments indicate artificial breaks introduced during the data generation. For example, spelling errors in

[30]The procedure generates additional information not available in the BeH data. I use the generated postal codes as additional location information for establishments.

firm names over time could explain such changes.

I again build on the official guidelines that, in principle, a firm identifier should stay constant within establishment identifiers. If a change in firm identifier occurs without an establishment identifier change, I equalize the firm identifiers over time. However, I do so only if this adjustment keeps the firm structure unchanged. For example, if two establishments have the same firm identifier in one year and switch to a new but congruent firm identifier in the next year, I assume that no firm change has occurred. Without this restriction, multi-establishment firms consist of an unreasonably large number of establishments and frequently change their establishment structure, in ways inconsistent with the evidence from the AFiD data.

B.3 Imputation of Vocational Trainees in the AFiD

The investment tax credit rate granted to a firm depends on the policy-relevant firm size measured as the headcount of all employees (independent of their employment contract or their working hours). This measure does not include vocational trainees, as they are not legally considered employees in Germany. The AFiD data do not separately account for the number of trainees, preventing the direct calculation of the policy-relevant firm size.

To obtain an accurate measure, I use vocational trainee information from the KSE, available for 1999–2001, and link it to the AFiD data via firm identifiers. I calculate the share of trainees for observations with available information and impute missing trainee information within firms by assuming a constant trainee share over time. For firms that have no trainee information, I take the average share within 3-digit industry codes. This approach leads to information on vocational trainees for all observations, 83% of which are imputed (43% within firms and 40% within industries). I determine the policy-relevant firm size by excluding vocational trainees according to these shares. While the imputation may lead to a misclassification of firms into small and large for the empirical analysis, the exclusion of firms close to the firm size cutoff in the main estimation sample considerably reduces this risk.

B.4 Geolocation Data

As the geolocation information from address data in the BeH is only available starting in 1999, simply assigning the same information to earlier years would not account for relocation. To obtain accurate location measures for 1998—the year preceding the policy change—I combine the geolocation data with a mix of additional data sources. I use municipality information from the BeH dataset and postal code information from the firm identifier data (see Appendix B.2), both of which are available for establishments in 1998. I add geographic coordinates either from the list of municipalities by the German Federal Statistical Office or from postal code information collected by the OpenStreetMap project. In both cases, the coordinates represent the center of these areas. Whether the municipality or the postal code is more precise depends on the areas in which an establishment is located. In rural areas, several municipalities are usually part of the same postal code, whereas large cities comprise several postal codes. I assign the geographic coordinates for each establishment according to the most precise source.

Given that municipalities or postal codes distinguish relatively confined areas, I obtain a relatively accurate measure for establishment location in 1998. To improve location information, I combine this dataset with the geolocation data from address data. I do so by identifying establishment relocations according to the following steps: If establishments are located in the same municipality in 1998 and in the year of the earliest availability of the geolocation data (which is 1999 for the majority of establishments), and if the distance between the geographic coordinates from address data and the municipality/postal code data is below 25 kilometers, I assume that no relocation occurred. In such cases, I use the geographic coordinates of the establishment address. For all other cases, I assume that the establishment relocated and use the geographic coordinates from the municipality/postal code data. For manufacturing firms in East Germany, I obtain geographic coordinates based on the address data for 88% of establishments and the municipality/postal code data for 12% of establishments. Almost none of the establishments have missing location information.

B.5 Descriptive Statistics

Table B.1: Descriptive Statistics of the Estimation Sample for the AFID Data

	Mean	P10	Median	P90	Count
Time-varying					
Log capital stock (million)	2.089	0.524	2.074	3.689	14,931
Log investment (thousand)	5.848	3.671	5.932	7.907	15,527
Log employment	4.541	3.784	4.419	5.451	16,289
Log total sales (million)	2.151	0.933	2.023	3.633	16,285
Log domestic sales (million)	1.982	0.785	1.854	3.444	16,281
Share export (%)	11.8	0.0	1.3	40.7	16,285
Log labor productivity	-2.559	-3.348	-2.586	-1.745	16,281
Log firm wage	7.432	7.059	7.437	7.788	16,289
Time-constant					
Pre-treatment wage growth	0.072	-0.072	0.072	0.208	1,629
Pre-treatment employment growth	0.094	-0.227	0.076	0.445	1,629
Share below cutoff district (%)	66.6	44.7	70.0	85.4	1,629
Labor share district (Wage bill over sales, %)	18.4	12.0	18.7	24.3	1,629

Note. Descriptive statistics are based on firms in the main estimation sample, consisting of East German manufacturing firms active throughout 1995–2004 with policy-relevant firm size in 1998 between 40 and 1500 employees, excluding those in Berlin and with policy-relevant firm size in 1998 of more than 225 and fewer than 275.

Table B.2: Descriptive Statistics of the Estimation Sample for the BHP Data

	Mean	P10	Median	P90	N
Time-varying					
Log employment	4.498	3.761	4.382	5.416	20,590
Log college educated	2.041	0.693	2.079	3.497	19,145
Log non-college educated	4.361	3.584	4.248	5.293	20,589
Log ratio education	-2.347	-3.714	-2.303	-1.168	19,144
Log abstract/manual jobs	3.046	1.792	3.045	4.357	20,423
Log routine jobs	3.912	2.833	3.892	5.011	20,528
Log ratio tasks	-0.869	-2.269	-1.053	0.868	20,361
Log net flows total	0.133	-0.283	0.059	0.643	20,590
Log net flows non-employment	0.039	-0.219	0.000	0.340	20,590
Log net flows movers	0.112	-0.095	0.056	0.412	20,590
Log net flows large manuf. firms	0.003	-0.015	0.000	0.018	20,590
Log net flows same district manuf. firms	0.008	-0.026	0.000	0.047	20,590
Log net flows other district East manuf. firms	0.005	-0.018	0.000	0.030	20,590
Log net flows service sector	0.077	-0.047	0.038	0.273	20,590
Log firm wage	7.472	7.121	7.469	7.817	20,590
Avg. log residual (tenure)	-0.143	-0.474	-0.135	0.167	20,590
Avg. log residual (other observables)	-0.068	-0.304	-0.071	0.183	20,590
Avg. log residual (all observables)	-0.094	-0.319	-0.096	0.138	20,590
Avg. log residual (interacted with industry)	-0.090	-0.308	-0.085	0.120	20,590
Time-constant					
Pre-treatment wage growth	0.083	-0.026	0.084	0.192	20,590
Pre-treatment employment growth	0.143	-0.218	0.103	0.577	20,590
Share below cutoff district (%)	74.5	49.8	77.5	92.3	2,059
Share below cutoff $(0\text{km}, 2\text{km}]$ (%)	83.6	39.7	100.0	100.0	1,876
Share below cutoff $(2\text{km}, 5\text{km}]$ (%)	81.6	41.4	100.0	100.0	1,912
Share below cutoff $(5\text{km}, 5\text{km}]$ (%)	78.6	44.5	84.7	100.0	2,035
Share below cutoff $(10\text{km}, 25\text{km}]$ (%)	73.5	55.0	74.7	91.2	2,059
Share below cutoff $(25\text{km}, 50\text{km}]$ (%)	70.2	60.5	70.7	81.4	2,059

Note. Descriptive statistics are based on firms in the main estimation sample, consisting of East German manufacturing firms active throughout 1995–2004 with policy-relevant firm size in 1998 between 40 and 1500 employees, excluding those in Berlin and with policy-relevant firm size in 1998 of more than 225 and fewer than 275.

C Results

Table C.1: Effects on Sales and Trade

		Exclusion of Volatile Exporters		
	Log Total Sales	Log Total Sales	Log Domestic Sales	Export Share
	(1)	(2)	(3)	(4)
Panel A. Only Direct Effects				
After 1998 ×	0.009	0.055	0.102**	-0.026***
Small firm	(0.044)	(0.044)	(0.046)	(0.009)
Observations	16,285	15,465	15,465	15,465
Panel B. Including Indirect Effects				
After 1998 ×	0.006	0.048	0.091**	-0.024**
Small firm	(0.039)	(0.043)	(0.043)	(0.010)
After 1998 × Share	0.097	0.092	0.051	0.024*
small (district)	(0.067)	(0.067)	(0.061)	(0.013)
Observations	16,285	15,465	15,465	15,465

Note. Each column reports estimates based on specification (11) in panel A and specification (12) in panel B. The dependent variables are the log of total sales in columns 1 and 2, log of domestic sales in column 3, and the export share in column 4. Firms with differences in export rate during 1995–1998 above the 95th percentile are excluded in columns 2–4. Additional controls are pre-treatment wage growth, firm fixed effects and industry-year fixed effects. Panel A includes labor market-year fixed effects and panel B includes pre-treatment employment growth and federal state-year fixed effects. Results are based on the AFiD data. Standard errors in parentheses are clustered at the district level: * p<0.10, ** p<0.05, *** p<0.01.

Table C.2: Wage Effects of Investment Tax Credits

		Log Residual			
	Log Avg. Wage	Tenure	Other observables	All observables	Interacted with industry
	(1)	(2)	(3)	(4)	(5)
After 1998 × Small firm	-0.022***	-0.012**	-0.018***	-0.010**	-0.008*
	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
After 1998 × Share small (district)	-0.002	0.005	0.009	0.008	0.006
	(0.008)	(0.009)	(0.008)	(0.009)	(0.009)
Observations	20,590	20,590	20,590	20,590	20,590

Note. Each column reports estimates based on specification (12). The dependent variables are the log of average full-time worker wage in column 1, and average residuals from individual-level regressions of log wage on worker characteristics in all other columns. The residual in column 2 controls for a cubic functional form of tenure. Column 3 controls for gender, secondary education, post-secondary education, type of employment contract, a cubic functional form of worker age, and occupation (2-digit) fixed effects. For the residual in column 4, the cubic functional form of tenure is combined with all characteristics in the previous column. The residual in column 5 interacts all characteristics with industry (2-digit) fixed effects. For the firm-level specifications, additional controls are pre-treatment wage growth, pre-treatment employment growth, firm fixed effects, industry-year fixed effects, and federal state-year fixed effects. Results are based on the BeH data. Standard errors in parentheses are clustered at the district level: * p<0.10, ** p<0.05, *** p<0.01.

Table C.3: Robustness - Controlling for GRW Subsidies

	Log Capital	Log Employment	Log Domestic Sales	Log Labor Productivity	Log Avg. Firm Wage
	(1)	(2)	(3)	(4)	(5)
Panel A. Only Direct Effects					
After 1998 ×	0.139**	0.101***	0.113**	0.012	-0.021**
Small firm	(0.056)	(0.036)	(0.047)	(0.032)	(0.010)
GRW subsidy decrease	-0.066	0.030	0.050	0.018	0.013
	(0.109)	(0.065)	(0.088)	(0.054)	(0.016)
GRW subsidy decrease ×	0.043	0.004	-0.072	-0.074	-0.016
Small firm	(0.106)	(0.060)	(0.072)	(0.048)	(0.016)
Observations	14,931	16,289	16,281	16,281	16,289
Panel B. Including Indirect Effects					
After 1998 ×	0.144***	0.089***	0.106**	0.018	-0.021**
Small firm	(0.052)	(0.029)	(0.043)	(0.031)	(0.010)
After 1998 × Share	0.241***	0.099**	0.048	-0.051	0.018
below (district)	(0.091)	(0.042)	(0.061)	(0.043)	(0.014)
GRW subsidy decrease	-0.018	-0.019	0.045	0.062	0.018
	(0.086)	(0.042)	(0.066)	(0.048)	(0.014)
GRW subsidy decrease ×	0.026	0.017	-0.068	-0.083*	-0.014
Small firm	(0.098)	(0.044)	(0.061)	(0.047)	(0.015)
Observations	14,931	16,289	16,281	16,281	16,289

Note. Each column reports estimates based on specification (11) in panel A and specification (12) in panel B, with additional controls for firms in regions that received a GRW subsidy rate reduction. The dependent variables are the log of total capital stock in column 1, log employment in column 2, log of domestic sales in column 3, log of labor productivity in column 4, and log of average firm wage in column 5. Additional controls are pre-treatment wage growth, firm fixed effects and industry-year fixed effects. Panel A further includes labor market-year fixed effects and panel B includes pre-treatment employment growth and federal state-year fixed effects. Results are based on the AFiD data. Standard errors in parentheses are clustered at the district level: * p<0.10, ** p<0.05, *** p<0.01.