Investment Tax Credits and the Response of Firms*

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Abstract

This paper studies the effects of investment tax credits on firms' input choices by exploiting a sudden shift in the tax credit rate by firm size for manufacturing firms in Germany in 1999. I find that more generous tax credits lead to a significant increase in both investment and employment, with implied elasticities with respect to capital costs of 2.8 and 1.1, respectively. Local spillovers between firms generate an additional positive effect. The employment effect is due to the increased hiring of new employees rather than a decrease in separations, with direct flows out of unemployment constituting about half of the inflow of workers. A heterogeneity analysis reveals that firms with larger capital cost shares are more responsive to tax credits and that spillovers tend to be stronger for firms operating in the same industry. While there is little evidence that the average firm adjusts its skill mix or occupational structure, firms in industries with higher investment shares into information and communications technology (ICT) are more likely to shift towards highly educated labor and high-skilled occupations.

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Governments have long used tax policy in an effort to stimulate economic activity. Because the accumulation of capital is thought to be key to the creation of economic growth, there is frequent reliance on investment tax credits and similar tax incentives that reduce investment costs.[1] Proponents of such tax policies argue that a reduction in investment costs encourages additional investments that lead to the expansion of production, higher labor demand and positive spillover effects between firms. Others warn that such tax benefits provide economic rents to firm owners who would have invested anyway, generate real effects only for the initially targeted firms and prompt the substitution of workers with capital.[2]

For the evaluation of the impact of investment tax credits, therefore, it is necessary to analyze not only the investment behavior and input choices of targeted firms but also the additional adjustment processes that may arise throughout the economy at large. In this context, policy-makers often express a particular interest in the role of tax credits for the creation of jobs, both in targeted firms and through spillovers. Because of the difficulty of separately identifying direct and spillover effects and the scarcity of sufficiently detailed micro-level data, there is so far limited empirical evidence on the adjustment behavior of firms.

In this paper, I investigate the effect of investment tax credits on firm investment, employment and workforce composition, and quantify the relevance of spillover effects between firms. To this end, I consider a tax policy in Germany that was introduced in 1991 immediately after reunification to mitigate the considerable economic differences that had developed over the previous decades. For the period 1991–1994, East Germany had about half the GDP per capita and a considerably higher unemployment rate of 13%. The program provided *refundable* investment tax credits that reduced firms' investment costs substantially independent of actual tax liabilities, allowing them to recover up to 27.5% of these costs. The scale of the program was significant, with annual expenses of €1–2 billion per year.

I exploit variation in the tax credit rate by firm size and time. During the period 1995–2004, initially manufacturing firms with up to 250 employees at the beginning of a business year were eligible for a tax credit rate of 10% while those with more than 250 employees were eligible for a rate of only 5%. In 1999 a change in the rates amplified this differential treatment in favor of firms below the cutoff, increasing the tax credit rate to 20% for firms below the cutoff and 10% for firms above the cutoff, therefore generating a relative decrease in capital costs for smaller firms.

[1] Investment tax credits in the U.S. on a national level were first introduced with the Revenue Act of 1962 and played a prominent role until its repeal in 1986. In 1985, government expenses for this policy totaled $21 billion (Chirinko, 2000). In 2004, 40% of U.S. states had their own investment tax credit program (Chirinko and Wilson, 2008).

[2] For example the discussions surrounding Bill Clinton's proposal for investment tax credits in 1992 illuminate both sides well.

The empirical analysis relies on two distinct administrative datasets that collect detailed panel information on investment and employment for a large share of German firms.[3] I implement difference-in-differences estimations for the period 1995–2004 and estimate the direct effects of the policy change by comparing the behavior of a group of firms below and above the firm size cutoff over time. I then augment the regression model by a difference-in-differences approach comparing firms across labor markets according to the share of firms below the firm size cutoff to estimate spillover effects. To mitigate concerns about the interference of time-varying shocks and bunching behavior, I include time-varying industry and labor market fixed effects, and exclude observations close to the cutoff.

The estimation strategy is guided by a theoretical firm production framework in which an increase in investment tax credits leads to an unambiguously positive effect on investment. The effect on employment and workforce composition is however ambiguous, depending on the degree of substitution between capital and different types of labor. Furthermore, spillovers between firms within local labor markets may generate an additional impact on investment and employment decisions, and this effect is larger within labor markets where a higher share of firms receive a reduction in capital costs.

My first empirical finding is that tax credits have a substantial direct effect on investment. The increase in the relative tax credit rate leads to 23.4 log points higher overall investment and 25.1 log points higher equipment investment at the intensive margin. Given the underlying relative change in capital costs of 8.2%, the overall investment response corresponds to an elasticity with respect to capital costs of 2.8. Considering the dynamic effects, there is an increase in the intensive margin investment response immediately after the policy change. The effect gets stronger in the subsequent year and stabilizes at a higher level thereafter. There is, however, no discernible effect on the probability of investing, likely because most firms invest in any given year independent of their capital costs.

I next consider the employment effects of the policy change and find a positive effect of tax credits on overall employment. Based on the preferred estimate, the change in the tax credit rate leads to 8.7 log points higher employment, equivalent to an elasticity with respect to capital costs of 1.1. This effect is similar when considering full-time workers, workers in regular employment or full-time workers in regular employment. Most of the increase materializes in the year after the policy change and there is a slight further increase in subsequent years. I further find that the increase in employment is almost exclusively driven by hiring additional employees rather than fewer separations. There

[3] I use the term "firm" interchangeably with "establishment" throughout. The main analysis is conducted at establishment level due to the aggregation of the datasets. The policy cutoff, however, is at firm level. I will show evidence that this simplification does not influence the qualitative results by considering single-establishment firms in a robustness test.

is actually a relative increase in separations for the treated firms, although the estimates are small and statistically insignificant. Among the additional hires, a share of 49% was unemployed one year prior which is similar to the average share among all hires in the data.

The investment and employment response translate into more output, measured in terms of revenue. Estimates for the revenue effect, however, tend to be more volatile and less precisely estimated. The response to the tax credit rate change for domestic revenue amounts to 8.3 log points. Taking these results together, tax credits seem to be an effective tool to induce higher investment among targeted firms, and the change in investments then translates to more employment and output within the same firms.

When turning to the regression model that estimates both direct and spillover effects, the estimate of the direct employment effect does not change markedly, with the estimate indicating an increase by 10.1 log points. On top of this direct effect, there are positive spillover effects. They depend on the share of firms in a labor market below the cutoff, as predicted by the theoretical model. In a labor market with only treated firms, the spillover effects lead to an additional increase of employment by 12.0 log points, meaning that spillovers amount to 54% of the combined effect. This result suggests that the benefits from tax credits among targeted firms propagate locally and spillovers have important implications when considering the cost-effectiveness of the policy.

These results mask important heterogeneity in the direct and spillover effect. First, firms with larger capital cost shares measured by relating average annual investment costs to the average annual wage bill have a stronger investment and employment response, in line with the theoretical predictions. Second, the spillover effect tends to be stronger for firms within the same industry, suggesting that similarities between firms are important for creating spillovers as is the case for agglomeration economies. In contrast, there is no significant effect on the service industries, which counters the idea that an increase in demand for local goods and services creates spillovers through local multipliers.

Capital-skill complementarity would suggest that highly educated labor profits more relative to less educated labor from tax credits. I consider the ratios of various skill measures: college-educated versus non-college-educated employees (all or those with full-time employment), and high-skilled occupations including engineers and managers versus low- and medium-skilled occupations like machine operators. All point estimates are close to zero and statistically insignificant. Given the magnitude of the standard errors, it is possible to reject estimates of all but modest skill composition changes. Thus, the added capital in firms does not shift employment opportunities towards highly educated labor or high-skilled occupations. When considering the possibility that effects are confined to manual labor by examining changes in the ratio of medium-skilled to low-skilled manual occupations, and the ratio of manual to service occupations, there is again a zero effect.

The literature on technological change considers information and communication technology (ICT) strongly complementary with skill. When analyzing heterogeneous effects across industries, I find that industries with a higher share of investment and capital in ICT are more likely to shift towards college-educated employees and high-skilled occupations.

To verify that the research design is appropriate, I perform various robustness checks. First, plotting the raw data year by year and checking pretreatment year estimates in the dynamic specifications, I find no differential pretreatment behavior between treatment and control group which supports a common trends assumption. Second, adjustments in inputs occur directly in the year after the policy change providing a link between the policy and firm behavior. Third, the results are robust to different sample selection procedures. Average effects are stable for the selection of narrower and wider firm size intervals of the treatment and control group, and to the exclusion of different firm size intervals around the cutoff. Finally, in a placebo test where I select only treated firms and assume various policy-irrelevant firm size cutoffs, I obtain insignificant estimates.

This paper contributes to several strands of the literature. Starting with Hall and Jorgenson (1967) a long literature has emerged that empirically quantifies the effect of capital cost changes on firm investment. Earlier studies rely on aggregate time-series data and find surprisingly small responses in investment (Abel, 1980; Summers, 1981; Abel and Blanchard, 1986).[4] In an attempt to overcome measurement bias, subsequent studies use firm-level data and cross-sectional variation in tax policies, and generally find larger effects (Cummins et al., 1994; Edgerton, 2010). A survey of such studies by Hassett and Hubbard (2002) concludes that the elasticity of investment with respect to capital costs is between 0.5 and 1.0. Although these studies consider investment tax credits, capital cost reductions are calculated as the total of all available tax incentives at any given time. The recent literature predominantly focuses on the analysis of specific tax incentive programs that introduce cross-sectional variation. House and Shapiro (2008), Maffini et al. (2016) and Zwick and Mahon (2017) examine special depreciation allowances and find large responses in investment behavior with elasticities of investment of around eight.

To the best of my knowledge, I am the first to estimate causal effects of the impact of investment tax credits on firm investment using plausibly exogenous cross-sectional variation. The main advantage of the analysis of investment tax credits is their clear link to capital costs, that in contrast to depreciation allowances do not depend on assumptions for the discount factor and depreciation schedules. Since investment tax credits in the German case were refundable, there is also no influence of the firms' profit situation on capital costs. I find an elasticity of investment of 2.8, which is in between the lower values

[4] The elasticities are considered *too* small, since they imply extremely high capital adjustment costs.

from earlier studies and the large values of recent studies.

I use this result as a starting point for a comprehensive analysis of the impact of investment tax credits on secondary outcomes. As a novelty, I exploit variation within and across labor markets to separately estimate direct effects and indirect spillover effects. On the one hand, this approach adds to the literature by connecting the firm-level evidence on the investment response to related firm outcomes like employment, that are influenced by a capital cost reduction as well. On the other hand, the existence of spillover effects reveals an adjustment mechanism that operates on an aggregate level and their estimation is a step towards the decomposition of the total effect of tax policy found for example in macro-level tax policy studies (Blanchard and Perotti, 2002; Romer and Romer, 2010; Mertens and Ravn, 2013).

With this approach, I also contribute to the literature on spillover effects between firms. My results relate to Greenstone et al. (2010) and Gathmann et al. (2018) that exploit exogenous variation across labor markets in firm openings and closings respectively.[5] These studies focus on relatively specific events for large firms. I add to this literature by analyzing a far-reaching policy that focuses on smaller firms, and I exploit continuous treatment assignment across labor markets. My results suggest that small firms create spillovers as well, as long as they add up to a sufficient share of a labor market. I consider the importance of agglomeration economies and local multipliers as mechanisms for spillovers and in contrast to Moretti and Thulin (2013) do not find evidence of local multipliers.

Furthermore, my paper speaks to the large literature on place-based policies and their effects on regions (Becker et al., 2010, 2013; Busso et al., 2013; Kline and Moretti, 2014a; Criscuolo et al., 2016; Dettmann et al., 2016; Etzel and Siegloch, 2018) and firms (Bronzini and de Blasio, 2006; Cerqua and Pellegrini, 2014). In these studies, there is a stronger focus on employment, but the evidence on the employment effect is inconclusive. This may be because the analyzed policies often combine a mix of regional and firm-specific incentives, including reductions in capital costs. I add to this literature by studying one particular incentive, investment tax incentives, and study adjustments in input and output independent of other influences.

Finally, by considering heterogeneous effects across labor types, my findings speak to the often voiced concern that tax policies and place-based policies can lead to unwanted redistribution in welfare (Glaeser and Gottlieb, 2008; Kline and Moretti, 2014b; Neumark and Simpson, 2015). The literature on the shift of production technology towards automation (e.g. Acemoglu and Autor, 2011) and capital in general (Krusell et al., 2000) suggests that there can be an advantage for high-skilled over low-skilled labor. The tax credit program does not have adverse effects on the skill composition on average and cre-

[5] The overall literature on spillovers is much larger. For an overview particularly concerning agglomeration economies see Combes and Gobillon (2015).

ates employment opportunities for unemployed individuals. However, I find an influence of ICT on a shift towards high-skilled labor. Similar to Akerman et al. (2015), this result points to potential adverse effects of government programs supporting ICT investments.

The structure of the paper is as follows. Section 1 explains the policy intervention in more detail focusing on the relevant regulations for the empirical analysis. Section 2 introduces a theoretical framework that provides intuition for expected firm behavior with heterogeneous labor types and spillovers. Section 3 explains the estimation strategy. Section 4 provides detail on the data including descriptive statistics and sample selection. Section 5 presents the main results and Section 6 relates the results in a back-of-the-envelope cost-benefit analysis. Section 7 concludes.

1 Policy Intervention

After the second world war, Germany split into two countries, West Germany and East Germany.[6] While West Germany experienced continued growth with a market-based economy, East Germany faced large war reparations and inefficiencies in its communist economic system. The fall of communism throughout Eastern Europe led to the reunification of Germany in 1990. The diverging prior development however created a country with economically disparate regions. Over the period 1991 to 1994, East Germany had on average 46% lower GDP per capita, 47% lower capital per worker, 30% lower earnings per worker and an unemployment rate of 13.4% compared to 7.1% in West Germany (see Figure 1). To speed up economic convergence, the government provided considerable financial support to regions in East Germany. Besides cash transfers to private households and large infrastructure investments, efforts were focused on increasing the capital stock of firms. The most salient policy in this respect was an investment tax credit program (*Investitionszulagengesetz*) which is at the center of this paper.[7]

The program started immediately after reunification in 1991 and lasted until 2013. It provided tax credits for equipment investments to firms located in East Germany and West Berlin. From 1999, it also covered investments in structures. At the beginning of the program, firms of all industries were eligible for the program but over time access became more restricted and by 1997 coverage applied almost exclusively to manufacturing firms.[8] Tax credits typically reduced investment costs by around 10% but depending on

[6] During the time of separation the official designation for West Germany was *Federal Republic of Germany* and for East Germany *German Democratic Republic*. I use the common names since they are still used to refer to the respective parts of Germany after reunification.

[7] Appendix A provides information on competing programs and how the unique characteristics of the policy change in 1999 can causally identify the effects of tax credits.

[8] Retail businesses continued to have limited eligibility until 2001. Manufacturing service businesses like construction design or research gained access to tax credits in 1999. Accommodation businesses were eligible from 2007.

the exact location and firm size, the reduction could be as high as 27.5%.[9] Tax credits were fully paid even if they exceeded tax liabilities of a firm and did not depend on the life span of the investment good.[10]

Importantly, all details on firm eligibility and the tax credit rate were precisely defined by law without room for discretion on a case by case basis. This led to an entitlement to tax credits for eligible firms and thus certainty for the planning of long-term investment projects.[11] The tax credits therefore can be considered a pure capital cost reduction for firms. The administrative cost for receiving tax credits was small. Firms filled out a tax credit claim form describing the investment good and the value of investment. Tax officers would check the correctness of the claim after the end of the business year and a positive assessment would trigger the transfer of tax credits. To reduce adverse incentives, a number of further eligibility criteria needed to be satisfied. Assets had to stay within the firm for at least 3 years to prevent East German firms from becoming pass-through companies of buying and reselling fixed assets.[12] Planes, passenger cars and low value assets such as office equipment or basic tools were never eligible because verifying their continued presence within an eligible firm in East Germany (or West Berlin) would entail large monitoring costs.

The program was costly. Figure 2 summarizes overall government expenses for tax credits by year based on available information in the official subsidy reports. From 1992 to 1995 expenses totaled around €2 billion per year. After 1993, expenses declined steadily, which can be explained by the reduction in eligible industries. They reached a low of €645 million in 1998 and stabilized thereafter at around €1 billion per year. Starting in 2000, expenses from tax credits for investments in structures contributed to the total and generally made up around 15% each year.

In the empirical analysis, I focus on manufacturing firms as the main recipients of tax credits and consider the time period between 1995 and 2004, comparing their behavior around a sudden change in tax credit rates for equipment investments in 1999. From July 1994, manufacturing firms with up to 250 employees received a tax credit rate of 10% on equipment investments. Firms with more employees instead received 5%. The program defined firm size as the number of employees at the beginning of a business year without differentiating full-time and part-time employment, and excluding vocational trainees since they are employed through special educational contracts. At the beginning of 1999,

[9] The highest rate applied to equipment investments of manufacturing firms with at most 250 employees in regions close to the Czech and Polish borders from 2002 to 2009.

[10] This is in contrast to special depreciation allowances where the decrease in cost of capital depends on the profit situation of a firm and on the years of depreciation due to the present value of future tax deductions. House and Shapiro (2008) and Zwick and Mahon (2017) provide detailed explanations.

[11] This is in contrast to various place-based policies that distribute grants to investment projects via a competitive application process with the final outcome based for example on the perceived success of the project or a commitment to hire additional employees.

[12] After 1999 the minimum time period was extended to 5 years.

tax credits were raised for a broad range of equipment investments, with firms below the employment cutoff now receiving a rate of 20% and firms above the cutoff receiving 10%.[13] The announcement of the policy change was published in August 1997 but because of disputes with EU law, it got approval only by the end of 1998. The adjustments led to a decrease in capital costs for all manufacturing firms. However, the increase in the tax credit rate was larger for firms below the cutoff and granted them a relative decrease of capital costs of equipment compared to firms above the cutoff.

The change applied to so-called modernization investments, which included among others any investment that could potentially increase production, change the production process or produce different products. Any investment in new equipment that did not directly replace a similar asset fell within this category. Even (high value) office equipment could be part of this category as long as it was bought in connection to a specific modernization investment.

Figure 3 summarizes the general tax credit changes for equipment investments of manufacturing firms in East Germany between 1995 and 2004.[14] Apart from the adjustment in 1999, there was another increase for modernization investments in 2000 that further strengthened the relative advantage of firms below the cutoff to those above. Tax credits for non-modernization equipment investment remained unchanged during the policy update in 1999 but were reduced in 2002. However, this reduction did not change the differential treatment and maintained a higher rate of 5 percentage points for firms below the cutoff before and after the change. Since the definition for firm size changed markedly in 2005, I exclude those years from my analysis. From then on the cutoff value followed the definition of small and medium firms by the European Union that takes ownership structure into account and defines the cutoff with respect to the number of employees, revenue and total assets.[15]

2 Theoretical Framework

To understand firm behavior after capital cost changes, it is helpful to outline a simple model of firm production. The literature has already developed detailed models of firm investment behavior in which adjustment costs and corporate taxation play an integral

[13] At the same time an investment limit for receiving the higher tax credit rate of €2.56 million per year for firms below the employment cutoff was eliminated. The additional tax credit rate for investments in structures was independent of firm size and thus, did not lead to relative differences.

[14] Berlin had generally lowered rates throughout the time period. Regions close to the Polish and Czech border received slightly higher rates between 2001 and 2009. For long investment projects, additional rules applied around changes of the tax credit rate.

[15] The definition can be found in the "Commission recommendation of 6 May 2003 concerning the definition of micro, small and medium-sized enterprises" (ABl.EU, #L 124 pp36).

part.[16] Since I rely on a reduced-form approach in the empirical analysis, I abstract from both these issues and focus on a static model with capital costs consisting of a universal capital rental rate and a firm-specific tax credit rate.[17] I further focus on labor as input to the production process, consider heterogeneous labor types and capture the influence of spillovers in regional firm production with a regional productivity shifter similar to Greenstone et al. (2010) and Gathmann et al. (2018).[18]

The model assumes many firms i within many regions r. Each firm produces one differentiated good according to the nested CES production function

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

where output Y_i is produced using capital K_i, low-skilled labor U_i and high-skilled labor S_i as inputs. The nesting of the three input factors follows Krusell et al. (2000) to allow for differential adjustment of the two labor inputs to a change in capital costs. In particular, the elasticity of substitution between low-skilled labor and capital is $\frac{1}{1-\mu}$ and the elasticity of substitution between high-skilled labor and capital is $\frac{1}{1-\rho}$. Production also depends on a firm-specific production parameter A_i and a productivity shifter A_{ir}. Although the productivity shifter is firm-specific, it depends on aggregate outcomes in region r. I consider the behavior of A_{ir} in detail once I turn to the effect of spillovers.

Each firm chooses inputs according to the rental rate of capital r, wage w_U for low-skilled and wage w_S for high-skilled labor. There is fully elastic capital and labor supply which leads to equalization of input prices throughout the economy and firms take the input prices as given. The cost of capital still differs between firms since there is a firm-specific reduction through tax credits with rate τ_i. Firms set the product price p_i facing monopolistic competition with a downward sloping inverse demand curve

$$p_i = B Y_i^{-\frac{1}{\eta^D}}, \tag{2}$$

where the price depends on the elasticity of demand $\eta^D > 1$ and a demand shifter B.[19]

The profit maximization problem for a firm is well-defined and the first-order condi-

[16] Important examples are Hall and Jorgenson (1967), Hayashi (1982) and Abel and Eberly (1994). Hassett and Hubbard (2002) and Bond and Reenen (2007) summarize basic model assumptions and survey further approaches.

[17] The qualitative equilibrium results hold true nonetheless. An important difference is that in my model firms choose capital stock and not the investment rate as is the case when including adjustment costs.

[18] Details on solving the model are provided in Appendix B.

[19] Monopolistic competition leads to decreasing returns to scale. In this case, there is a unique interior solution. Other approaches introduce a fixed input factor or restrict full elasticity of labor supply.

tions fully explain the production decisions of a firm. The first-order conditions are

$$(1-\tau_i)r = \left(1-\frac{1}{\eta^D}\right) Ba_K Y_i^{1-\mu-\frac{1}{\eta^D}} X_i^{\frac{\mu-\rho}{\rho}} K_i^{\rho-1}(A_i A_{ir})^\mu \tag{3}$$

$$w_S = \left(1-\frac{1}{\eta^D}\right) Ba_S Y_i^{1-\mu-\frac{1}{\eta^D}} X_i^{\frac{\mu-\rho}{\rho}} S_i^{\rho-1}(A_i A_{ir})^\mu \tag{4}$$

$$w_U = \left(1-\frac{1}{\eta^D}\right) Ba_U Y_i^{1-\mu-\frac{1}{\eta^D}} U_i^{\mu-1}(A_i A_{ir})^\mu \tag{5}$$

where $X_i = a_K K_i^\rho + a_S S_i^\rho$.

To show the impact of a change in the cost of capital through adjustments in the tax credit rate on optimal capital and labor, I totally differentiate the production function (1) and all first order conditions (3), (4) and (5). By reformulating the results, the price elasticity of capital and the cross-price elasticity of high-skilled and low-skilled labor respectively are

$$e_{K_i} = \left[\eta^D s_{K_i} + \frac{1}{1-\mu}s_{U_i} + \frac{1}{1-\rho}s_{S_i} - \left(\frac{1}{1-\mu} - \frac{1}{1-\rho}\right) s_{U_i}\frac{a_S S_i^\rho}{X_i}\right] Z \quad + \quad (\eta^D - 1)e_{A_{ir}} \tag{6}$$

$$e_{S_i} = \left[\left(\eta^D - \frac{1}{1-\rho}\right) s_{K_i} + \left(\frac{1}{1-\mu} - \frac{1}{1-\rho}\right) s_{U_i}\frac{a_K K_i^\rho}{X_i}\right] Z \quad + \quad (\eta^D - 1)e_{A_{ir}} \tag{7}$$

$$e_{U_i} = \underbrace{\left(\eta^D - \frac{1}{1-\mu}\right) s_{K_i} Z}_{\text{direct effect}} \quad + \quad \underbrace{(\eta^D - 1)e_{A_{ir}}}_{\text{indirect effect}}, \tag{8}$$

where each elasticity $e_\# = \frac{d\#}{d\tau_i}\frac{1-\tau_i}{\#}$ is with respect to the net of tax rate,[20] $s_{K_i} = \frac{(1-\tau_i)r}{p_i}\frac{K_i}{Y_i}$ is the share of capital cost (after tax credit) in revenue, $s_{U_i} = \frac{w_U}{p_i}\frac{U_i}{Y_i}$ is the share of low-skilled labor cost in revenue, $s_{S_i} = \frac{w_S}{p_i}\frac{S_i}{Y_i}$ is the share of high-skilled labor cost in revenue and $Z = \frac{\eta^D}{\eta^D - 1}$ is an additional scaling term.

Each elasticity consists of a firm-specific direct effect and an indirect regional effect. Turning first to the direct effect of tax credits, the elasticities mimic those in a model of firm production with just one labor type (e.g. Hamermesh, 1993). The elasticities of capital and labor largely depend on two effects, a scale effect from changes in input prices and a substitution effect between capital and the two types of labor. In the case of capital, these effects work in the same direction. An increase in tax credits leads to more demand for capital because of an expansion of production and a shift from labor towards capital. On the other hand, for each labor type the overall effect is ambiguous since there is higher demand from expansion in production but lower demand from the shift towards capital. The net effect depends on the relative magnitude of the elasticity of product demand and the elasticities of substitution. The effect can be different for each

[20]It is straightforward to show that this elasticity is equivalent to $-\frac{d\#}{dc_K}\frac{c_K}{\#}$, the capital cost elasticity.

firm because of differences in the capital cost and labor shares. Because of monopolistic competition and the nesting of the CES production function, additional terms show up. First, less than fully elastic product demand amplifies adjustments of all inputs through the term Z. Second, for the elasticity of capital, there is an additional term that reweighs the impact of each elasticity of substitution. Third, for the elasticity of high-skilled labor with respect to the net of tax rate, there is a dependence on the elasticity of substitution of low-skilled labor. Independent of the parameter choices, the elasticity of capital is larger than the elasticity for either labor type.

To better judge the effect on the composition of labor inputs, it is useful to consider the elasticity of the ratio between high-skilled and low-skilled with respect to the net of tax rate. It is

$$\frac{d\frac{S_i}{U_i}}{d\tau_i}\frac{1-\tau_i}{\frac{S_i}{U_i}} = \left(\frac{1}{1-\mu} - \frac{1}{1-\rho}\right)\frac{a_K K_i^\rho}{X_i}. \tag{9}$$

The sign and magnitude of this elasticity depend on the relative magnitude of each elasticity of substitution. If the elasticity of low-skilled labor is higher than the one of high-skilled, there is capital-skill complementarity implying a shift towards high-skilled labor.

The indirect effect changes each elasticity of input by the same additive term consisting of the elasticity of regional productivity and the elasticity of product demand. Since the regional productivity depends on aggregate outcomes, a change for one firm will lead to adjustments for all firms in the same region. By redoing the maximization problem for a firm j, the elasticities with respect to the net of tax rate of firm i are

$$\frac{dK_j}{d\tau_i}\frac{1-\tau_i}{K_j} = \frac{dS_j}{d\tau_i}\frac{1-\tau_i}{S_j} = \frac{dU_j}{d\tau_i}\frac{1-\tau_i}{U_j} = (\eta^D - 1)\frac{dA_{jr}}{d\tau_i}\frac{1-\tau_i}{A_{jr}}, \tag{10}$$

which closely resembles the indirect effects of firm i.

The productivity shifter captures spillovers between firms. I follow the literature and assume that the productivity shifter depends on the overall economic activity within a region. I define

$$A_{ir} = \sum_{j \in S_r} Y_j^{\lambda_{ij}}, \tag{11}$$

where the set S_r contains all firms in region r and λ_{ij} is the elasticity of agglomeration between firm i and j.[21] This definition encompasses many of the characteristics of spillovers discussed in the literature.

Aggregate output measures the degree of economic activity in a region, relating to the advantages found from clustering economic activity in proximity such as reduced transportation and communication costs in the supply chain of production, knowledge

[21] I use output as the measure for economic activity since the according elasticity is unambiguously greater or equal zero.

spillovers and thick labor markets (e.g. Moretti, 2011). These mechanisms suggest that firms may not profit equally from these advantages. By including firm-specific elasticities of agglomeration, I capture differences in the reliance of firms on local production networks. The measure of regional productivity also permits local multiplier effects as an alternative explanation for spillovers (Moretti, 2010). An increase in the employment within one industry may boost the demand for local goods and services and thereby impact employment in the non-tradable sector.

To derive intuitive closed-form solutions, I assume the same productivity shifter for all firms within a region by setting the elasticity of agglomeration $\lambda_{ij} = \lambda$. With this simplification, the elasticity of regional productivity with respect to a change in tax credit rate of firm i is

$$\frac{dA_r}{d\tau_i}\frac{1-\tau_i}{A_r} = \frac{\lambda\eta^D}{1-\lambda\eta^D}\frac{s_{K_i}Y_i^\lambda}{\sum Y_j^\lambda}Z > 0, \tag{12}$$

where I assume that $\lambda\eta^D < 1$.

The elasticity importantly depends on the interaction of the elasticity of agglomeration and the elasticity of product demand. Larger values for both elasticities imply a more pronounced impact on regional productivity. Furthermore, firms with a higher share of output in total regional output and larger capital cost shares impact regional productivity more. An initial increase in output of one firm due to tax credits will spread and lead to an unambiguous positive output effect due to spillovers in the whole region. The additional assumption on the magnitude of the elasticities rules out boundary cases in which spillover effects lead to infinite aggregate output.

If several firms in the same region experience a tax credit rate change, then the initial increase in aggregate output will be larger and will magnify spillover effects. Intuitively, the elasticity with respect to the tax credit rate of multiple firms comes about by sequentially calculating the equilibrium adjustments. Considering infinitesimally small tax rate changes, this reduces to summing up all elasticities of regional productivity for firms with changing capital costs. For notational simplicity, I consider the case where firms start out with the same tax credit rate $\tau_i = \tau$ and a subset receives the exact same tax credit rate change $d\tau_i = d\tau$. As result, the elasticity of regional productivity is

$$\frac{dA_r}{d\tau}\frac{\tau}{A_r} = \sum_{i|d\tau_i\neq 0}\frac{dA_r}{d\tau_i}\frac{1-\tau_i}{A_r} = \frac{\lambda\eta^D}{1-\lambda\eta^D}Z\frac{\sum_{i|d\tau_i\neq 0}s_{K_i}Y_i^\lambda}{\sum Y_j^\lambda}. \tag{13}$$

The elasticity is comparable to the one before. However, the adjustments of multiple firms lead to a summation of the output of all firms with tax credit rate change weighted by their share of capital costs. This means that there are larger spillover effects in regions where a tax credit rate change affects more firms.

In sum, the theoretical framework predicts the following. An increase in the tax credit rate leads to the use of more capital. The sign of the direct effect on labor depends importantly on the magnitude of the substitution effect between capital and the two labor types. Whether there is a shift towards high-skilled or low-skilled labor depends on the relative magnitude of both elasticities of substitution. On top of these direct effects, spillovers between firms lead to an additional positive effect on capital and employment. The spillover effects are larger in labor markets where more firms experience an increase in the tax credit rate. Finally, the proposed mechanisms for spillover effects suggest that the effect can vary firm by firm.

3 Estimation Strategy

The estimation strategy is guided by the theoretical framework and uses the described change of tax credit rates in 1999. I implement a difference-in-differences estimation approach and compare the adjustment of firms below and above the firm size cutoff before and after the change. I start by estimating average effects using the regression model

$$Y_{ibt} = \beta \text{Treated}_b \times \text{Post}_t + X'_{it}\gamma + \psi_i + \psi_{nt} + \psi_{lt} + \epsilon_{ibt}, \tag{14}$$

where Y_{ibt} is the outcome variable for each firm i with treatment b in year t. The variable Treated_b classifies firms into treatment and control group. I consider firms with up to 250 employees in 1998 as treated and those above as untreated to reflect the relative advantage for firms below the cutoff. The classification is fixed over time. I interact this variable with the dummy variable Post_t, which categorizes years 1999 to 2004 as treatment period to reflect the change in tax credit rate. ψ_i, ψ_{nt} and ψ_{lt} are firm, industry-year and labor market-year fixed effects, respectively. Firm fixed effects control for level differences in firm characteristics that stay constant over time such as those correlated with average firm size, industry and location. Industry-year and labor market-year fixed effects can control for the possibility that industry-specific shocks, and labor market-specific policies and economic developments coincide with the update of the tax credit program. I include various control variables X'_{it} depending on the specification and with log average firm wage being used for all main estimations.[22]

The coefficient of interest is β. Without confounding factors, it provides a causal estimate for the effect of the reduction of capital costs caused by the policy change on each outcome variable. The set of fixed effects already controls for many confounding factors. However, the firm size cutoff introduces additional firm incentives. Firms can adjust their size over time and thus are able to cross the cutoff. Such movements imply that these firms receive a different tax credit rate than assigned by the treatment status in the regressions. If this change in firm size is unrelated to the tax credit program,

[22] Using pretreatment wage growth trends instead of log wage leads to qualitatively the same results.

for example because of a general decline of a specific firm, this would not influence the causality claim. It would affect the interpretation of the coefficients though since it partly captures intent to treat effects. To minimize this issue, I exclude firms that are close to the cutoff in 1998 and thus those that are more likely to cross the cutoff in either direction. On the other hand, firms may intentionally move just below the cutoff or delay moving above to take advantage of the higher tax credit rate. To prevent biases of such behavior in the estimation, I further exclude observations in any year for which firm size is close to the cutoff. Since the exclusion of particular observations is directly connected to employment of a firm, it will bias the estimations with employment as outcome variable. In this case I select firms only based on their firm size in 1998.

I study several outcomes. First, I am interested in the effect on capital inputs. As is common in the literature, I use investment as a directly measured variable in the dataset to proxy capital adjustment. I consider the intensive margin, extensive margin and a combined measure. I also specifically consider equipment investment. In a second step, I focus on labor inputs and analyze the effect on the number of total employees and for various subcategories separately. To assess the impact of the policy on the skill composition of a firm's workforce, I also use employment ratios by education and occupation as an outcome variable.

The error term ϵ_{ibt} includes all other omitted factors. I cluster standard errors at the regional level allowing for heteroscedasticity and arbitrary correlation between firms within the same region over time. I consider German *Landkreise* as regions in my analysis. There are 76 regions in East Germany.[23] The regions are then divided into 56 labor markets following a classification used by Dustmann and Glitz (2015).

Since the average effect can mask interesting adjustment patterns, I estimate the dynamic regression model

$$Y_{it} = \sum_{p=1995}^{1997} \delta_p D_{bp} + \sum_{p=1999}^{2004} \delta_p D_{bp} + X_{it}'\gamma + \psi_i + \psi_{nt} + \psi_{lt} + \epsilon_{ibt} \tag{15}$$

where in comparison to specification (14) the interaction term is omitted and instead a set of yearly dummies D_{bp} is introduced. Each dummy variable assigns a value of one to firms in the treatment group for the corresponding year p and zero otherwise. The coefficients δ_p for the years 1999 to 2004 capture the dynamic treatment effect. If the treatment effect is indeed causal, then treatment and control group have parallel trends absent the policy change. This would not be the case if anticipation effects from the announcement of the policy or long-term influences from similar policy changes before 1995 influence firm behavior. For this reason, I also examine the pre-treatment effect

[23] Various regions merged due to reforms at the state level. I use the regional disaggregation as of 2014 to ensure consistency over time.

by including dummies for the years 1995 to 1997. Observing statistically insignificant estimates close to zero before the start of the treatment provides an indication that the identifying parallel trends assumption indeed holds true.

Both regression approaches so far only consider the direct effect of the policy change. For the estimation of spillover effects, I use the regression model

$$Y_{iblt} = \beta \text{Treated}_b \times \text{Post}_t + \alpha \text{ShareBelow250}_{l,98} \times \text{Post}_t + X_{it}^{'}\gamma + \psi_i + \psi_t + \epsilon_{iblt}, \quad (16)$$

where $\text{ShareBelow250}_{l,98}$ is the share of employees working in firms with up to 250 employees in labor market l for year 1998. The variable is interacted with the treatment period dummy Post_t to set up a difference-in-differences estimation with continuous treatment status. There are now two coefficients of interest, β, the direct effect of receiving a higher tax credit rate as in specification (14) and α. The latter coefficient provides an estimate of the difference in the outcome variable by the share of firms below the cutoff that is due to the change in tax credit rate. This setup mimics the results of the theoretical framework with α corresponding to an estimate of the spillover effects. Since I use variation at labor market level, I cannot control for the same set of fixed effects as before. Instead, I include firm and year fixed effects in the baseline and add industry-year, area (federal state)-year and regional pre-treatment growth trends as robustness tests.

The analysis of dynamic effects is again helpful to better understand adjustment behavior and to check the parallel trends assumption for the estimation of spillovers. The regression model is

$$\begin{aligned} Y_{iblt} = \sum_{p=1995}^{1997} \delta_p D_{bp} + \sum_{p=1999}^{2004} \delta_p D_{bp} + \sum_{p=1995}^{1997} \theta_p \text{ShareBelow250}_{lp,98} \\ + \sum_{p=1999}^{2004} \theta_p \text{ShareBelow250}_{lp,98} + X_{it}^{'}\gamma + \psi_i + \psi_t + \epsilon_{iblt}, \quad (17) \end{aligned}$$

where $\text{ShareBelow250}_{lp,98}$ are a set of variables measuring the share of employees working in firms with up to 250 employees in labor market l in year 1998. Each variable takes on this value in year p and is zero otherwise. The set of coefficients δ_p still estimates the direct impact of the policy change over time. The set of coefficients θ_p estimates the dynamic effect of spillovers over time. If the estimation for spillovers is causal, then there should not be any differential effect on firms between labor markets before the policy change. I check for this assumption by including coefficients for pre-treatment periods.

4 Data

4.1 Data Sources

The empirical analysis relies on two data sets, the AFID Establishment-Panel by the Federal Statistical Office of Germany and the Establishment History Panel (BHP) by the Institute for Employment Research (IAB).

The AFID dataset has a broad coverage of variables for investment, employment and output for the universe of manufacturing and mining firms with more than 20 employees in Germany. With its unusual richness it perfectly fits the needs for the general empirical analysis. Firm variables are collected through various administrative surveys and are used to inform the government and the public about key economic statistics like aggregate output and investment. Because of the importance of these statistics, firms are required by law to provide truthful information. The AFID dataset merges the underlying surveys through a unique firm identifier and aggregates information from monthly and quarterly surveys by year. Information is available since 1995 and new waves are continuously added. The dataset is especially suited for the investment analysis since there is separate information for equipment. Further subcategories distinguish different modes of acquisition such as self-production, leasing and purchase. For measures of output, there are revenue, production value, orders and the number of distinct products. Revenue is divided into domestic and foreign. There is however only limited information on labor inputs, with total employment and wage bill being most informative.[24] For each firm the 4-digit industry code and location at regional level is provided as well.

For a more detailed employment analysis, I use the BHP dataset which provides information on overall employment, employee composition, employee inflows and outflows, and wages. The data are based on the employment histories of the entire labor force covered by social security. They are collected from mandatory communication between firms and the Federal Employment Agency on changes in employment. The BHP aggregates this information at firm level for 30 June of every year for West Germany since 1975 and for East Germany since 1992. I focus on the years 1995 and 2004. The final sample consists of a 50% random draw of firms and all available years of selected firms are included. For employment there are counts for the total and for subcategories of education, occupation types and age and full-time employment and vocational training. For a better understanding of changes over time, inflow and outflow information provides the number of employees that did not work in the same firm one year before and one year after, respectively. These flows are again divided into subcategories. I use information for incoming and leaving employees that were unemployed one year before and after, respec-

[24]There is a distinction of employees by contract type. I do not use this information since it does not translate well to other economic concepts.

tively. Average wage is based on full-time employees and available for quartiles and by education. The dataset includes firms of all industries. Information at 3-digit industry level and a region variable allows the selection of relevant firms.

The AFID and BHP dataset are distinct and it is not possible to link them. Therefore, it is necessary to calculate key policy variables separately and take into account changes in the data collection over time independently. I explain the general adjustments here but relegate further details to Appendix C.

One important variable in the analysis is firm size, since it is the basis for classifying treatment and control group. The program definition requires information on the head count of overall firm employment and vocational trainees.[25] The AFID dataset lacks information on the latter. To address this issue, I match the number of vocational trainees at firm level for the years 1999 to 2001 from the cost structural panel (KSE) by the Federal Statistical Office.[26] For observations that are unmatched, I impute values assuming a constant share of trainees within firms or if unknown within industries. For the BHP dataset, on the other hand, information on the marginally employed is missing for years before 1999. I impute missing observations by assuming a constant share of marginally employed within firms or if unknown predict the share within industries for different firm size.[27] The change in reporting of marginally employed unfortunately coincides with the policy change. To reduce the risk that the imputation of marginally employed influences the estimation, I drop firms with an average share of more than 25%, which is above the 95$^{\text{th}}$ percentile within the manufacturing industry.

Another issue concerns the continuity of firm identifiers. In the AFID data, firm identifiers are constant even if the ownership structure of a firm changes, in the BHP this is not the case. However, in the BHP the firm identifier can change for relatively simple reasons such as changes in the legal structure. In the analysis, firm structure possibly influences decision-making processes and thus leading to abrupt changes in production behavior. To have firm identifiers that exclude such changes, I separate firm identifiers in the AFID dataset when there are changes in overall firm structure and I exclude firms with more than one-hundred establishments in a given year. For the BHP, I follow Hethey-Maier and Schmieder (2013) and create unique identifiers for firms that are connected through employee flows.

Further adjustments are the reclassification of regions as of 2014, the classification of

[25] The definition considers the business year start for calculations. Employment figures in the AFID and BHP dataset do not have this same timing, however, an auxiliary analysis using the IAB establishment survey does not show a systematic difference of employment levels in manufacturing firms within a given year.

[26] The KSE is a yearly firm survey of a stratified random sample in the manufacturing and mining industry and focuses on the production process.

[27] Since I rely on broad firm size intervals in the analysis, and vocational trainees and marginally employed constitute a small share of a firm, measurement error from both imputations should lead to relatively few misclassifications into treatment and control group.

regions into labor markets according to Dustmann and Glitz (2015) and a classification of 2-digit industries that further aggregates uncommon industries.

4.2 Sample Selection

The sample selection follows the eligibility criteria of the program. First, I select firms in manufacturing industries[28] for years 1995 to 2004 located in East Germany excluding Berlin. To avoid peculiar behavior of entering and exiting firms, I also condition on them being economically active throughout the period of the analysis.

Second, I check for bunching behavior around the cutoff. Figure 4 displays the size distribution of manufacturing firms in East Germany and West Germany around the cutoff for 1999 to 2004. For West Germany a decrease in the density with increasing firm size is apparent which is as expected (e.g. Axtell, 2001). For East Germany this pattern is generally true as well, however, just below the cutoff there is excess mass. This points to bunching of East German manufacturing firms. I therefore exclude all firms with a size of between 226 and 274 employees in 1998.[29] I further exclude observations within that size interval in any other year.[30]

As a last step, I include only firms with at least 40 employees and a maximum of 1,500 in 1998 and observations that lie within the same interval for any other year. This reduces the problem from biases due to heterogeneous effects among observationally different firms (Heckman et al., 1997).[31] To check whether the choice of the excluded interval and the size interval has an impact on estimation results, I run robustness test that vary these interval boundaries.

Figure 5 considers the impact of the exclusion around the cutoff on firms moving outside their assigned treatment status. As a comparison, I include the case without restriction. The specific sample selection has little impact on the treatment group. At most 3.5% of firms move above the cutoff and once observations around the cutoff are excluded this share reduces to a maximum of 1.5%. There is more movement within the control group. Without the exclusion of firms around the cutoff, 8.6% of firms in the control group fall below the cutoff already in 1999. In 2003 this share is at 26.4%. It is likely that some of these firms move on purpose to take advantage of higher tax credit rates. For the sample with excluded bandwidth, the share of firms moving below is considerably lower. After the policy change the share increases slowly over time,

[28]These include all industries with a WZ 1993 classification of D.

[29]I determine this cutoff by implementing a structural approach that adapts ideas from Garicano et al. (2016). The maximum likelihood estimation leads to a value of 274.6. Implementation details are available upon request.

[30]The exclusion of single observations is not appropriate for estimating the effect on employment since then there would be a selection based on the dependent variable. In this case, I only condition on firm size in 1998.

[31]I implemented a propensity score matching approach between treatment and control group and found qualitatively similar results to those in the main text.

reaching 6.8% in 2001 and is highest in 2004 with 16.9%. Even though the share in 2004 is non-negligible, in this case, a movement below the cutoff is less problematic since these observations are not affected by bunching behavior.

4.3 Descriptive Statistics

Table 1 presents a selection of firm variables from the AFID dataset in Panel A and the BHP dataset in Panel B for the years 1995 to 2004.[32] I show descriptive statistics for all manufacturing firms in West and East Germany (excluding Berlin), and for treatment and control group of the empirical analysis. Overall, firms in West Germany are larger in many respects compared to those in East Germany. The average number of employees in the AFID dataset is 155.19 in West Germany and 89.42 in East Germany. They have the same likelihood of investing in any given year but in West Germany the investment value is larger. These differences in input factors translate to higher revenue with €30.75 million compared to €13.03 million. Panel B reports very similar differences for the number of employees. On top, it shows that full-time employees earn considerably more in West Germany. In terms of employee composition, there are actually more employees with college degree, more high-skilled occupations and more vocational trainees in East German manufacturing firms.

Turning to the estimation sample, there are again clear differences in size. This is not surprising given the definition of treatment and control group. The treatment group is similar to the average East German firm when comparing means, but the control group is far larger in every respect.[33] The groups have similar employee composition. For example the share of college graduates is 11.16% in the treatment group compared to 14.82% in the control group.

As a step towards the actual estimation, Figure 6 presents the raw means for treatment and control group over time for different investment and employment outcomes. Encouragingly, in each plot treatment and control group have a similar pre-treatment trend. In the upper left panel, there is a continuous decrease in the log of total investment that continues after the policy change in 1999. For subsequent years the investment level stays higher for the treatment group reflecting a positive investment response. The decision to invest, shown in the upper right panel, is relatively stable with a value close to 100% and there is little differential movement before or after 1999. I plot log employment using AFID data in the lower left panel and BHP data in the lower right panel. The evolution in both graphs is quite similar. This is remarkable given that the actual data collection was independent of each other and speaks to the quality of both datasets.

[32] I provide descriptive statistics for the complete list of relevant variables for the estimation sample in the Appendix.

[33] This does not mean that there is no overlap. Standard deviations are usually large, especially for investment and employee composition.

Until 1999, employment increases for the treatment and control group. Subsequently, employment stays constant or decreases in the control group whereas there is continued growth for the treatment group until 2001. After 2001, employment decreases for the treatment group as well at similar levels as the control group. This pattern indicates a positive employment response to the relative reduction in capital costs.

5 Results

5.1 Investment

As a first outcome, I study firm investment. Table 2 summarizes the average effect for various investment measures. In column (1), I consider the log of total investment at the intensive margin and find that the policy change leads to 23.4 log points higher investment for the treatment group compared to the control group. Since the policy change only affected tax credits on equipment investment, column (2) reports the estimate for this subcategory. The estimate is slightly higher with 25.1 log points and statistically significant. Both outcomes measure only the intensive margin. In column (3), I check for differences in the probability of investing. However, there does not seem to be any response with a point estimate of zero and small standard errors. This is likely the case because of the high rate of firms that invest in any given year independent of treatment. In the literature, one outcome of interest is the investment rate (I_t/K_{t-1}) which combines intensive and extensive margin. I do not observe capital in my datasets. As proxy, I consider investment divided by the average total investment during the period of analysis. I find a positive and statistically significant response of 0.171 for total investment and 0.167 for equipment investment.

Taken together, these results show a positive investment response to tax credits. To compare the effect to previous findings in the literature, I calculate the elasticity with respect to capital costs. The change in capital costs for treatment and control group is equal to $\Delta t_i/(1 - t_i)$. Taking into account the changes in tax credit rate in 1999 and 2000, and assuming that all investments are modernization investments, capital costs decreased by 15.74% for the treatment group and 7.46% for the control group. This leads to an intensive margin elasticity of total investment of 2.825.[34] The consensus range for the elasticity of investment proposed by Hassett and Hubbard (2002) is 0.5 to 1.0 although recent studies by House and Shapiro (2008), Maffini et al. (2016) and Zwick and Mahon (2017) find much larger elasticities of around 8. Zwick and Mahon (2017) provide evidence that smaller firms have larger elasticities which could explain the rather large elasticity in my case as well.

[34]For cases where there are structures and replacement investments, the calculated elasticity is a lower bound. The extensive margin elasticity of total investment with respect to the tax credit rate is zero.

Table 3 considers a few robustness tests for the investment specification. Column (1) reproduces the results from before. Given that investment is highly volatile, in column (2), I consider a sample where I exclude firms with investment growth in 1997 above the 95$^{\text{th}}$ percentile. The response is slightly lower for log of total investment and log of equipment investment but there is no response for the probability of investing. In column (3), I only control for average firm wage, firm fixed effects and year fixed effects. The coefficients are again smaller but lead to qualitatively similar results. Finally, in column (4), I only consider single-establishment firms to eliminate inconsistencies between tax credit eligibility and the level at which production decisions are taken. I again exclude firms with high volatility in their investments. In this case, the average effect becomes slightly larger although there is still no response in the extensive margin. Overall, the investment response is similar throughout distinct specifications and for different sample selections.

For a better understanding of investment behavior over time, I study the dynamic specification. Figure 7 presents the coefficients and their 95% confidence intervals for the log of investment, the log of equipment investment and the probability of investing. For both measures of the intensive margin, there is an upturn directly in the year after the policy change. In the subsequent year, investment further increases with coefficients of 20.6 log points for total investment and 23.2 log point for equipment investment. Afterwards, there is notable fluctuation in the effect size with a short period of smaller coefficients followed by increases in 2003 and 2004. Standard errors are relatively large so that not all coefficients after the policy change are statistically significant. For pretreatment periods, coefficients are close to zero which suggests that treatment and control group follow the same trend. For the extensive margin, there is not any clear dynamic pattern. Coefficients move around zero for periods before and after the policy change leading to the zero result on average.

5.2 Employment

Given the positive response in investment, I then study adjustments in the use of labor inputs. Table 4 presents the estimation results for the effect of the tax credit rate change on employment. For the regression of column (1), I use employment information from the AFID dataset. I find a positive and statistically significant effect of 11.3 log points on total employment. The response is smaller than for investment but still sizable. In column (2), I consider the same employment measure based on the BHP data. In this case the coefficient is 8.7 log points which is similar in magnitude and confirms the positive employment response. The corresponding elasticity is 1.051. To analyze whether the employment effect applies to different subgroups of workers, in column (3) to (5), I consider regular employees, full-time employees and full-time regular employees.[35] The average effects are 7.6 log points on regular employees, 8.8 log points on full-time employees and 8.7 log points on full-time regular employees which is very close to the effect on total employment.

Figure 8 presents the according dynamic effects for total employment and full-time employment. For the case of the AFID data, there is a continuous increase in total employment during the treatment period. In the first year after the policy change employment is 3.3 log points higher. After three years it reaches 9.9 log points and then levels off. For the case of BHP data, the effect on total employment is more immediate. After one year, employment is 7.7 log points higher. Subsequently, there is a small drop, that is followed by a slow increase over time reaching 10.9 log points in 2004. When considering only full-time employees, there is again an immediate response of 5.5 log points. Even though there is again a drop in the subsequent year, there is a stronger increase in the effect over time, reaching 12.3 log points in 2004. When checking for parallel trends in employment for pretreatment periods, coefficients are again close to zero and mostly statistically insignificant. For total employment with the AFID data there seems to be some movement already in 1998, however, it is small in magnitude.

For these employment regressions, I exclude firms that are close to the firm size cutoff in 1998, but I do not exclude observations for firms that moved close to the cutoff in prior or subsequent years. Thus, my estimates potentially pick up bunching at the cutoff. As robustness test, I estimate the dynamic specification with the yearly change in the log of total employment as outcome variable and present the results using BHP data in Figure 9. For this regression, I exclude firms around the cutoff in 1998 and observations close to the cutoff in any other year, thereby eliminating observations with bunching. The effect is close to zero and statistically insignificant for all years except 1999. For 1999, the effect on the log growth rate is 7.9 log points. Even though statistically insignificant, the coefficients in subsequent years are still in line with the level results suggesting a decrease in employment in 2000 and small positive growth over time in subsequent years.

5.3 Revenue

In the previous subsections, I find that firms increase investments and employment due to tax credits. A natural extension to these results is the analysis of output. Table 5 provides estimation results for various revenue measures and the number of distinct products as proxy for output.[36] In column (1), I use total revenue as outcome variable and find a small effect of 1.1 log points. However, the effect on domestic revenue in column (2) is 8.3

[35] Since these measures mostly exclude marginally employed workers, using them as outcome variable can check whether reporting issues of the marginally employed in the BHP dataset are driving the results on total employment.

[36] The revenue measures will elicit the same effects if there is no change in output prices.

log points and on domestic revenue of manufacturing-specific goods in column (3) is 8.0 log points. This is puzzling since this implies a reduction in exports to compensate for changes in domestic production, although exports equal a small share of production on average. I therefore consider the possibility that the results for total revenue are driven by outliers among exporting firms. In column (4), I restrict the sample to firms with an average export share of not more than 15%. In this case, the effect is 7.6 log points which is close to the effect on domestic revenue (though statistically insignificant). Thus, the discrepancy between the effect on total revenue and domestic revenue seems to be confined to those firms that already export sizable amounts. As additional outcome, I include the number of distinct products in column (5) but I do not find an effect of tax credits on the number of products.[37]

5.4 Robustness Tests

Taken together, these results provide a positive assessment for investment tax credits. Not only do they increase firm investment, but they also lead to more employment overall, and thereby to higher revenue. The results are all based on a specific selection of the firm size sample. I check the robustness of these results to the exclusion of different firm size intervals around the cutoff and the selection of different lower and upper firm size bounds. Additionally, I implement a placebo test by estimating the effect on the sample of the treated firms and selecting several policy irrelevant cutoffs.

Table 6 reports coefficients for the most relevant measures of investment, labor and revenue. The interval of excluded firms around the cutoff varies in each column. In column (1), I do not exclude any firm. Column (3) follows the main specification with an excluded interval of 225 to 275 employees. In addition, I report an intermediate case in column (2) and larger intervals in column (4) and (5). Reassuringly, estimates for all outcomes are robust throughout the different intervals. This applies especially to log overall investment, the extensive margin investment decision and employment. For log equipment investment there are slight differences although there is no clear pattern as a function of the size of the excluded interval. For log domestic production, coefficients grow somewhat by the size of the excluded interval, but qualitatively, the results are the same. Taken together, there is little evidence that the exclusion of firms around the cutoff has a notable impact, possibly because the number of firms that bunch at the cutoff is small in comparison to the whole sample.

Table 7 keeps the excluded bandwidth constant but instead varies the smallest and largest included firm size. In this table, column (4) reproduces the main specification and columns to the left use smaller intervals whereas columns to the right use larger intervals.

[37] It is possible that this result reflects the introduction of novel products and a discontinuation of old ones at the same time.

Again, independent of the chosen interval, estimates lead to qualitatively similar results with limited fluctuations in the main coefficient of interest. The largest differences apply to the investment variables for which the coefficients in column (1) are somewhat smaller and those in column (6) larger than the rest. It should be noted that for the smallest firm interval, the number of observations is considerably smaller which may lead to higher susceptibility to outliers.

Finally, Table 8 reports the estimation result for the placebo cutoffs. The sample only consists of firms in the treatment group. I focus on the cutoffs 80, 100 and 125 to have a reasonable number of firms below and above these new cutoffs. I still exclude firms around the cutoffs as in the main specification. For the majority of outcomes, coefficients are close to zero and statistically insignificant. This holds true for all placebo cutoffs. For the extensive margin investment decision, coefficients are negative and statistically significant. However, given the high probability of firms investing in any given year, the coefficient is not economically significant.

5.5 Spillover Effects

All results so far speak to the direct effect of investment tax credits. In this subsection, I exploit the labor market-level variation in the number of employees working in firms below the cutoff. I start with the analysis of specification (16) where the treatment intensity is equal for all firms in the same labor market. Since this specification estimates direct and indirect effects simultaneously, I still exclude firms close to the cutoff in 1998. However, compared to previous estimations, I additionally include firms with 20–40 employees to have a larger sample size for more precision in the spillover analysis. The outcome variable is the log of total employment in all estimations.

Table 9 reports the according results. In the baseline in column (1), the direct effect on treated firms is 10.7 log points which is close to the previously estimated employment effect. The coefficient for the spillover effect is 0.118. Both the direct and indirect effect is stable to the inclusion of additional control variables. I include pretreatment growth trends in average firm size at the labor market level in column (2), industry-year fixed effects in column (3), industry-year and area (federal state)-year fixed effects in column (4) and all of the previous controls together in column (5). The direct effect fluctuates between 9.3 and 11.3 log points and the indirect effect is between 0.116 and 0.126. The average firm is in a labor market where 78% of employees work in a firm below the cutoff, translating to spillover effects on employment of 9.0 to 9.8 log points. In contrast to the direct effect, the spillover effects boost employment for firms in the treatment and control group.

In Figure 10, I plot the dynamic spillover effects estimated from specification (17). I find an immediate response in employment after the policy change. However, the effect gets larger over time and has not stabilized by 2004 which suggests that the complete propagation of spillovers takes extended time. For the pretreatment periods, the spillover effect is close to zero and statistically insignificant, suggesting parallel trends between firms in different labor markets.

5.6 Flow Information

I continue with an analysis of employee flow information. The BHP dataset reports the number of employees within each firm that did not work there one year prior or that do not work there one year after. For my empirical analysis, I first relate the inflow and outflow information to overall employment one year prior. This is equal to the yearly growth rate for net flows and the hypothetical growth rate for inflows and outflows assuming the other flow value to be zero. I then accumulate these rates over time and take logs to have a measure which is similar to log employment.[38] I apply the same procedure to flow information of employees within firms that were unemployed one year prior or are unemployed one year after.

Table 10 reports the results for each of the accumulated flow variables. In column (1) to (3), the dependent variables include flows from all employees. The result for net flows in column (1) is nearly identical to the result using log employment with an increase of cumulated net flows by 8.6 log points. This serves as a check on the validity of the analysis of flow information. In column (2), I focus on the inflow rate and find an increase by 10.2 log points. Since the additional inflows in treated firms are larger than net flows, there must be an increase in outflows as well. This is confirmed in column (3), although the coefficient is small and statistically insignificant. These results show that firms that received relatively more tax credits after the policy change increased firm size predominantly through hiring additional employees. Importantly, the point estimate does not suggest that the control group has higher outflows which counters the concern that the employment effect is due to a shift of employees from the control group to the treatment group. If anything, the higher cumulated outflow among treated firms suggests changes in employment structure.

From a policy perspective, it is of interest whether unemployed individuals gain from tax policies. I analyze the flows related to unemployment in column (4) to (6). Even though the coefficients are statistically insignificant, their magnitudes are economically significant. The effect of inflows from unemployment in column (5) is 5.0 log points. When comparing this estimate to the one for overall inflows, the hiring of unemployed people constitutes 49% among the additionally hired employees. This is sizable and similar to the share of all hires of 60%. The effect on the accumulated outflow rate into unemployment is 1.8 log points which is almost as high as for overall outflows. Thus,

[38]This is the case since firm fixed effects control for average firm employment.

it seems that the additional separations are of employees who have problems finding re-employment.

I consider the dynamic effects of the flow variables in Figure 11. Taken together, the dynamic results confirm the previous findings. It is of interest that the net flow and inflow for unemployment are statistically significant in this specification. It is also the case that the share of inflows from unemployment is larger at the beginning with 71.4% and reduces over time. Furthermore, the increase in outflows, although still statistically insignificant, slowly increases over time which suggest that firms first hire additional employees and only let go of unnecessary employees over time.

5.7 Heterogeneous Effects

Based on the theoretical model, there is reason to believe that the direct effect of investment tax credits varies by the capital cost share of firms. Even though the theoretical model suggests a measure relating capital costs to revenue, in practice such a measure is biased by the more volatile reaction of revenue to economic changes and exceeds one in many cases. Therefore, I relate the average yearly investment costs to the average yearly wage bill to have a measure between zero and one, which still reflects differences in capital costs. Table 11 provides evidence that there are indeed differences empirically. The coefficients for the direct effect are based on firms with zero capital costs. Although this is an unrealistic boundary case, coefficients are close to zero and statistically insignificant as expected. The larger the capital cost share becomes the larger the response for investment and employment. The coefficients mirror the average findings from before, that there is a stronger response for investment rather than employment.

To learn about the underlying mechanisms of spillovers, I examine two additional specifications. First, if spillover occur due to advantages in the production network or input sharing, then the share of firms below the cutoff in related industries should have a larger effect on employment than the share in unrelated industries. Aggregated input and output statistics and job to job movements indicate a strong dependence of firms within industry classes. I therefore split the share of employees in firms below the cutoff into a within industry share and an across industry share:

$$\underbrace{\frac{\sum_{i\in S_l, L_i\leq 250} L_i}{\sum_{i\in S_l} L_i}}_{\text{Whole labor market}} = \underbrace{\frac{\sum_{i\in S_l, i\in S_n, L_i\leq 250} L_i}{\sum_{i\in S_l} L_i}}_{\text{Within industry}} + \underbrace{\frac{\sum_{i\in S_l, i\notin S_n, L_i\leq 250} L_i}{\sum_{i\in S_l} L_i}}_{\text{Across industry}} \tag{18}$$

Table 12 reports the results of estimating direct effects, spillover effects within an industry and spillover effects across industries. As before, I report results for a baseline specification and for specifications with various additional control variables. Since I just split the previous share variable into two, there is no change on the direct effect. For the spillover effect, it is the case that the coefficient for the within industry share is larger than for the across industry share. In column (1) and (4) the value is nearly double. This suggests that spillovers are stronger between firms in the same industry, possibly because of their links in the production network. It should be noted that the standard errors are too large to statistically test for the differences in coefficients.

The policy applied mainly to manufacturing firms. Local multipliers can create spillovers to the service industry because of changes in local demand for goods and services. To check for such an effect, I use a regression sample consisting of firms in the service industry with employment between 20 and 1,500 employees in 1998 and analyze the effect of the policy by the share of manufacturing workers in firms below the cutoff. Table 13 presents the results. Independent of the chosen control variables, estimates are close to zero, ranging from -0.028 to 0.012, and statistically insignificant. These results suggest that spillover effects are confined to firms in the manufacturing industry. The investment tax credits do not seem to create multiplier effects.

5.8 Capital-Skill Complementarity

The economic literature extensively discusses the importance of capital-skill complementarity. I examine this adjustment mechanism in the context of tax credits by analyzing various skill ratios. First, I consider the education level of workers comparing shifts in the ratio of the college educated vs. the non-college educated. I use this measure based on all employees and conditional on being a full-time employee. Second, I analyze the skill level of occupations. I build on the categories provided in the BHP dataset and use the ratio of high-skilled vs low- and medium-skilled occupations. In this definition, technicians, engineers, semi professions, professions and managers count as high-skilled occupations. Manual, service and sales occupations count as low- and medium-skilled occupations. As another dimension, I explore shifts to or away from manual occupations. I build the ratio comparing manual to service and sales occupations. Finally, within manual labor, I analyze the ratio of medium-skilled (qualified) vs. low-skilled occupations.

Table 14 reports the average effect on the log of each ratio. All regression coefficients are close to zero and statistically insignificant. I can reject the null hypothesis that there are large shifts in employment composition. For example, for the ratio of the college educated vs non-college educated, the upper bound of the 95% confidence interval includes a coefficient of 0.052, which translates to a shift of the ratio from 0.131 to just 0.138.

This zero result is surprising. The literature considers ICT as an important mechanism for these shifts, which became an important driver of production technology changes in the 1990s. It is possible that in my setting, ICT does not play a big enough role and therefore does not influence the average employment composition. This however does not mean that there is no influence in particular circumstances. To explore this point in more detail, I study heterogeneous effects of tax credits by the intensity of ICT usage. Since the main datasets do not have information on this type of capital, I match information of ICT usage at the industry level from several data sources. First, Sauer and Strobel (2015) provide investment information for 2014 based on data by the Federal Statistical Office and a firm survey by the Ifo Institute. Second, the EU KLEMS project, in collaboration with the DIW, provides various capital input measures since 1991 for Germany. I rely on the real fixed capital stock in 1998. Third, the IAB establishment survey includes extensive margin investment decisions at firm level for 1993-2014. I aggregate this information to 2-digit industries after controlling for broad regional areas, firm size, average wage and year as possible confounding factors. Finally, the Economic Census collects capital expenditure information for U.S. manufacturing firms. I select the year 2002 as it is the earliest publicly available one. Using information at industry level from several data sources is advantageous for thinking about the heterogeneous results as causal. Because of the aggregation, within industry correlations of firm characteristics and ICT usage are excluded. Using information from data sources of different time periods or countries helps to exclude temporary influences and permanent region specific correlations.

Table 15 presents the estimation results using each measure of ICT usage. I find that firms in industries with more intensive use of ICT change their employment composition more towards high-skilled labor and high-skilled occupations when receiving investment tax credits. This heterogeneous response is remarkably stable for each ICT measure, even though they are from quite distinct sources. This speaks to the fact that the effect may be indeed causally related to ICT itself. For the ratio of college-educated vs. non-college educated and using the IAB establishment survey, the coefficient for the heterogeneous effect is 0.793 with a standard deviation of the ICT measure of 0.056. The effect size does not seem large, however, it should be noted that there is larger variation at firm level and the industry measure introduces measurement error on the actual firm-level ICT usage.

The effects on shifts of manual occupations and on shifts within manual occupations are noisy. Only when using information from the Economic Census, I find a statistically significant effect for a more intensive use of medium-skilled compared to low-skilled manual labor among firms in industries with higher ICT shares.

6 Discussion

The results show that investment tax credits increase investment and employment. For policy decisions, it is crucial to relate these benefits to the incurred government expenses. Given that one stated goal is the increase in employment, I focus on a measure that relates the government expenses needed for increasing employment among the average manufacturing firm in East Germany. To highlight the contribution of spillovers, I separately calculate this measure for only the direct effects and including spillover effects.

I first provide a back-of-the-envelope calculation of government expenses when considering only the direct effect. Based on the full sample of East German manufacturing firms in Table 1, the average firm invests €1.17 million per year including zero investment among 14% of firms and employs 81.1 employees. The estimation results in Table 2 and 4 elicit an elasticity of investment of 2.825 and an elasticity of employment of 1.051.

Assuming a tax credit rate of 10% for all firms independent of size has the following effect. The average firm increases the intensive margin of investment by 28.3 log points which translates to €381.9 thousand in additional average investments reaching €1.552 million per year. The government therefore spends €155.2 thousand in government expenses on tax credits. At the same time, the average firm increases employment by 8.99 employees. Taken together, government expenses of €17,264 lead to one additional employee.[39]

This result changes when including spillover effects. Based on Table 9 column (5), first, there is a slight increase in the direct effect implying an elasticity of employment of 1.219. Adopting the same investment response as before, this implies government expenses of €14,761 per additional employee. The difference between both numbers reflects partly the variation of the estimated employment effects.

Second, if all firms receive the tax credit of 10%, then the share of employees working in such firms is by construction one. Thus, the estimate of 0.120 translates to an additional employment effect of 12.0 log points and to an increase of the elasticity of employment to 2.668. Spillovers account for 54.3% of this overall employment response. Due to spillovers, government expenses of only €6,258 per additional employee are needed.

7 Conclusion

In this paper, I empirically assess an investment tax credit program in Germany to estimate the causal impact on firm input choices. To evaluate the success of this program, I go beyond a firm investment analysis and study the effect on employment of different labor types and spillover effects.

I find that firms increase both their investment and employment substantially and that this translates to higher revenue. For employment, the effect occurs through hiring additional employees, including a sizable share that were unemployed before. I do not find a shift in the employment composition by skill types on average. Nevertheless, industries with higher dependence on ICT technology are more likely to shift to high-skilled labor. Finally, spillovers further increase the employment effect.

[39] This calculation does not take into account that government receive additional income tax or reduce expenses for unemployment benefits.

These results are encouraging for the use of investment incentive programs in fiscal policies. The fact that there is a benefit for unemployed and low-skilled individuals is important from a welfare perspective since it is believed that these individuals profit the most from support policies. The existence of spillovers are further important for the cost-effectiveness of the policy. However, more research is needed to assess whether investment incentives are the most efficient way of improving economic outcomes and how programs directly targeted to employees compare during the short-run and the long-run.

The influence of ICT on the workforce composition provides a cautionary tale. Given that ICT has become pervasive in the production process and robots are starting to take over many simple production tasks, incentivizing investments in the future may lead to less beneficial outcomes for unemployed and low-skilled individuals.

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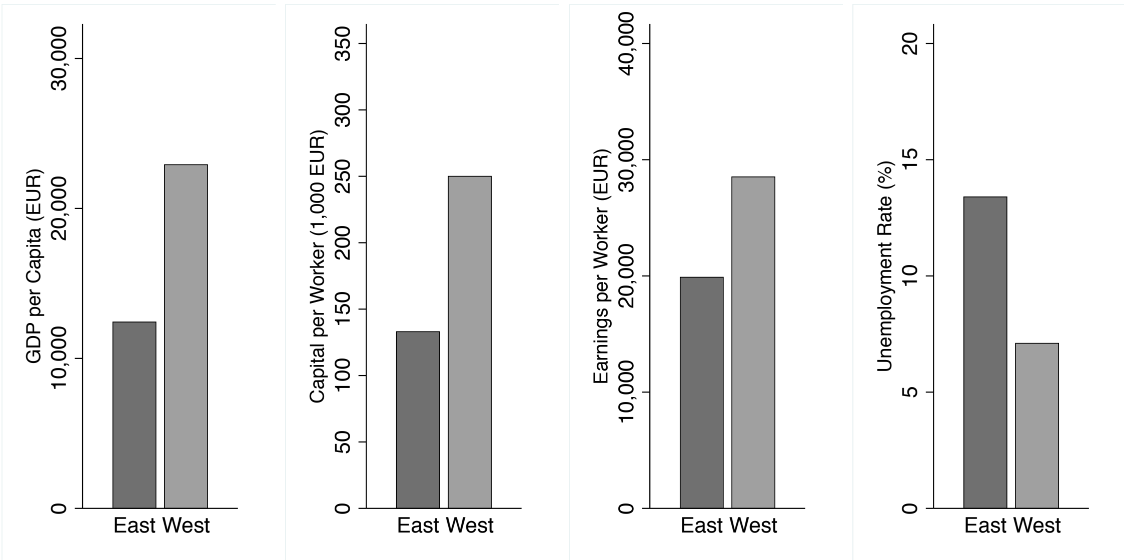


Figure 1: Economic Indicators for West and East Germany for 1991-1994

Note: The bars represent averages over the years 1991-1994. Data source for Panel (a)-(c) is the Federal Statistical Office. Data source for Panel (d) is the Federal Employment Institute. The unemployment rate is based on figures of June of each year. All of Berlin is counted towards the statistics of East Germany.

Figure 2: Government Expenses for Tax Credits by Year and Investment Type

Note: Information taken from subsidy reports of the German government. Values that are available only in Deutsche Mark were converted to euro using the official conversion rate. Figures on government expenses get revised over time due to additional information. The presented figures are taken from the most recent report available for each year respectively to reflect the most current information status. Expenses after 2016 excluded.

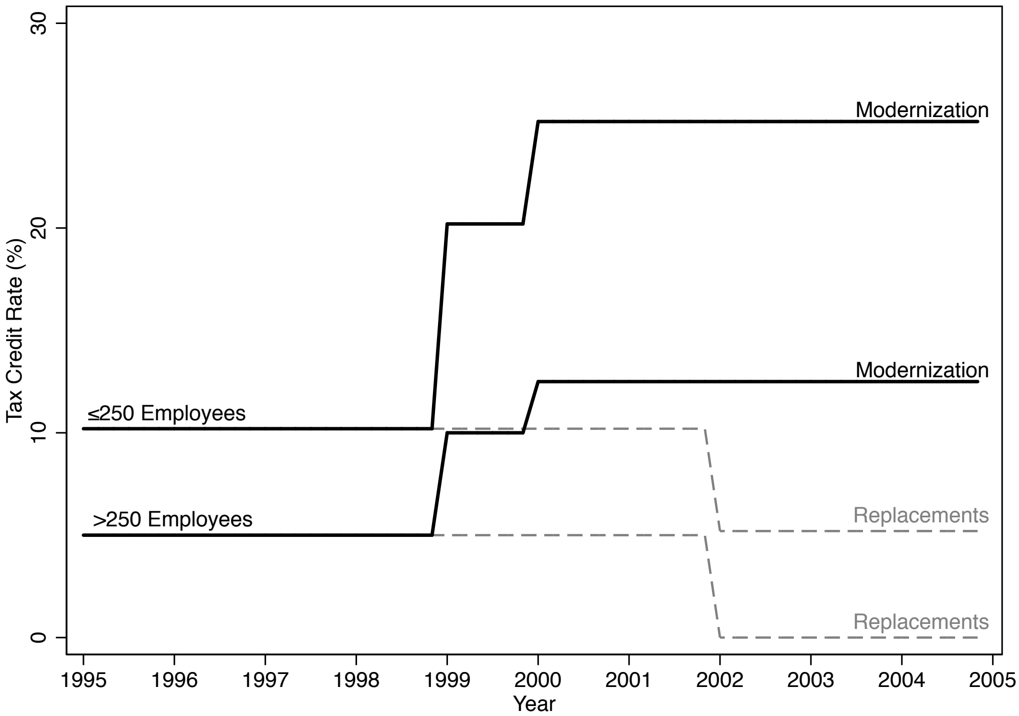
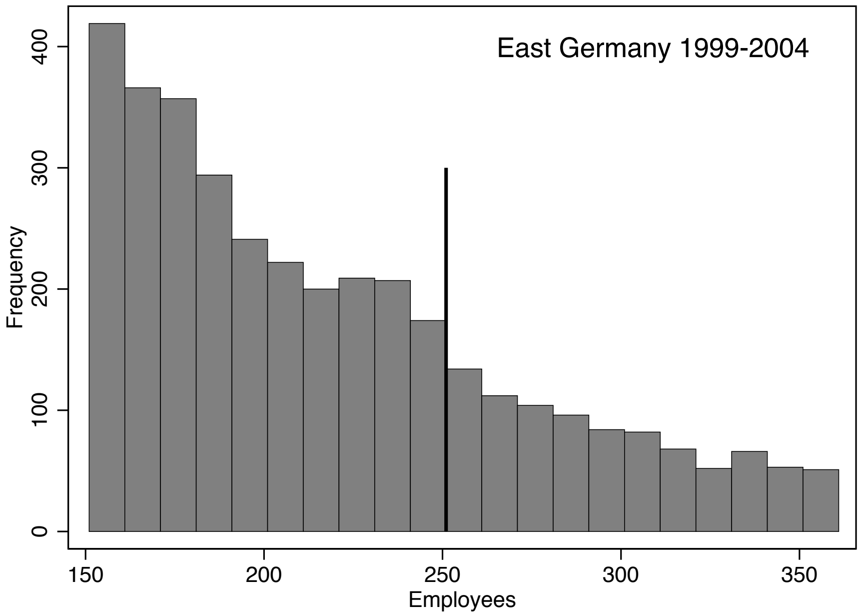


Figure 3: Equipment Tax Credit Rates for Manufacturing Firms

Note: The shown tax credit rates apply to manufacturing firms in most parts of East Germany excluding Berlin for equipment investment that start and end at the same day. For Berlin and in some years for areas close to Berlin rates were lower. There was a slight increase in the tax credit rate for modernization investments in regions close to the Polish or Czech border starting in 2001. Changes in tax credit rate were usually accompanied by phase-out periods to allow a constant rate for longer-lasting investment processes.



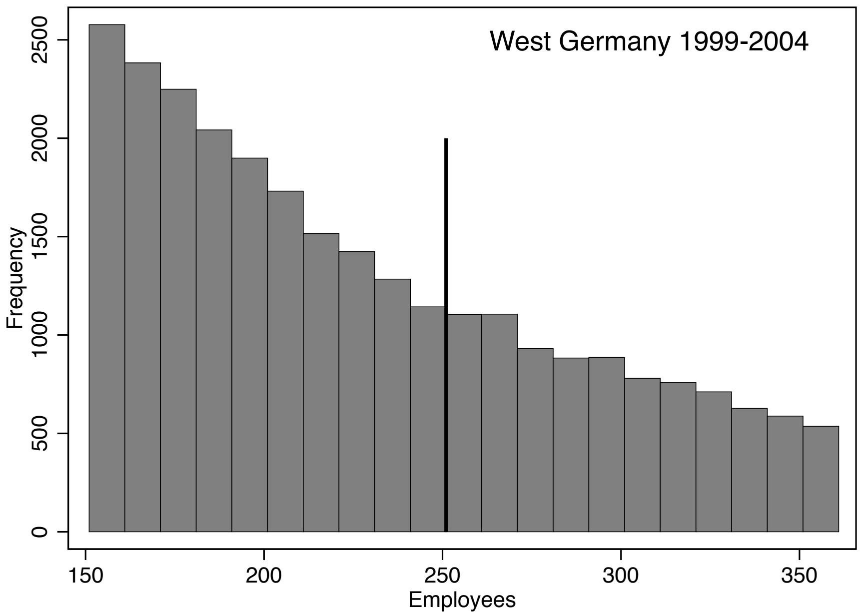


Figure 4: Firm Size Distribution of Manufacturing Industry

Note. Data from AFID establishment panel. The sample consist of all observations of manufacturing firms for East and West Germany excluding Berlin between 1999 and 2004. Firm size is based on total number of all employees but excludes vocational trainees.

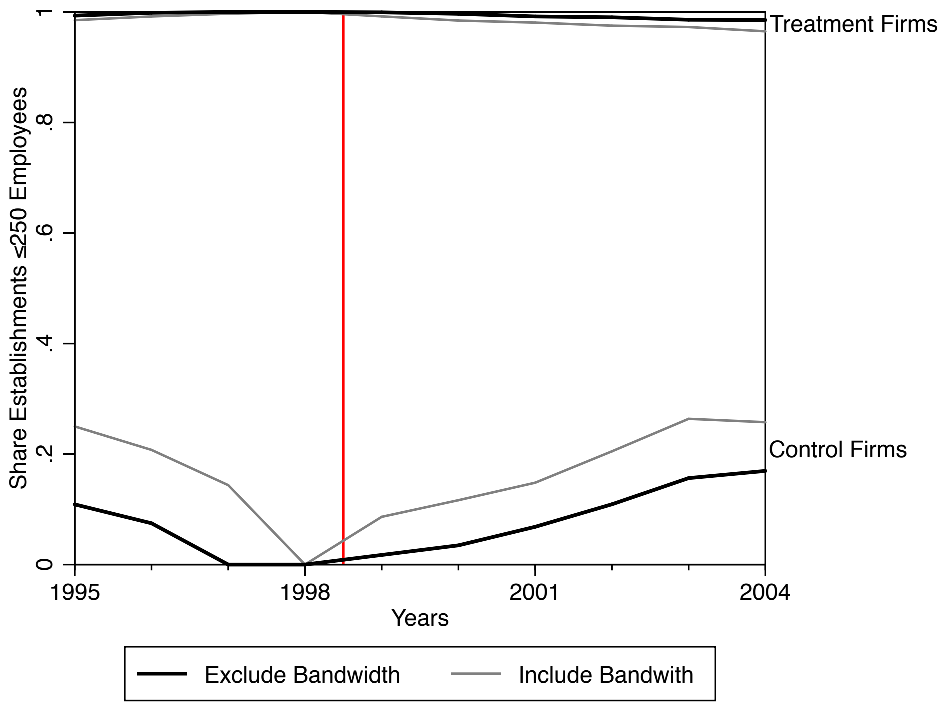


Figure 5: Movement of Treatment and Control Group Around Firm Size Cutoff

Note: Data from AFID establishment panel. The figure shows the share of firms in the treatment and control group below the firm size cutoff. Firm size is defined as total employee head count minus vocational trainees. The treatment group consists of firms with 250 or fewer employees. The sample selection for 'Exclude Bandwidth' is according to main text and specifies the exclusion of firms and observations around the firm size cutoff. The case of 'Include Bandwidth' follows the same sample selection, but keeps firms and observations around the size cutoff.

Table 1: Descriptive Statistics for Manufacturing Firms

Panel A. AFID Data

	West Germany	East Germany		
	All	All	Treatment	Control
No. of Employees (mean)	155.19	89.42	91.88	508.19
No. of Employees (median)	57	47	75	414
Investing (%)	86.0	86.0	94.0	97.0
Investments (thousand EUR)	1,267.50	1,172.35	805.96	8,605.11
Revenue (million EUR)	30.75	13.03	11.39	101.73
Part of Multi-Establishment Firm	22.0	22.0	9.98	15.76
Observations	357,662	64,154	16,374	1,266

Panel B. BHP Data

	West Germany	East Germany		
	All	All	Treatment	Control
No. of Employees (mean)	129.24	81.09	89.89	468.66
No. of Employees (median)	47	41	72	404.5
Full-time employees (mean)	111.63	70.95	80.56	424.02
Average Daily Wage Full-time	85.86	58.16	59.54	78.23
Share College Degree (%)	6.43	10.58	11.27	14.70
Share High-Skilled Occupation (%)	12.63	13.06	13.08	16.87
Share Vocational Trainees (%)	3.89	5.30	4.33	4.13
Observations	212,924	37,239	9,180	1,000

Note: Statistics are based on firms in the manufacturing sector for the years 1995-2004 excluding Berlin. The number of observations is based on the according statistic for number of employees. The group of all manufacturing firms includes those with more than 20 employees to allow for a better comparison between AFID and BHP data. Treatment and control group are according to the estimation sample.

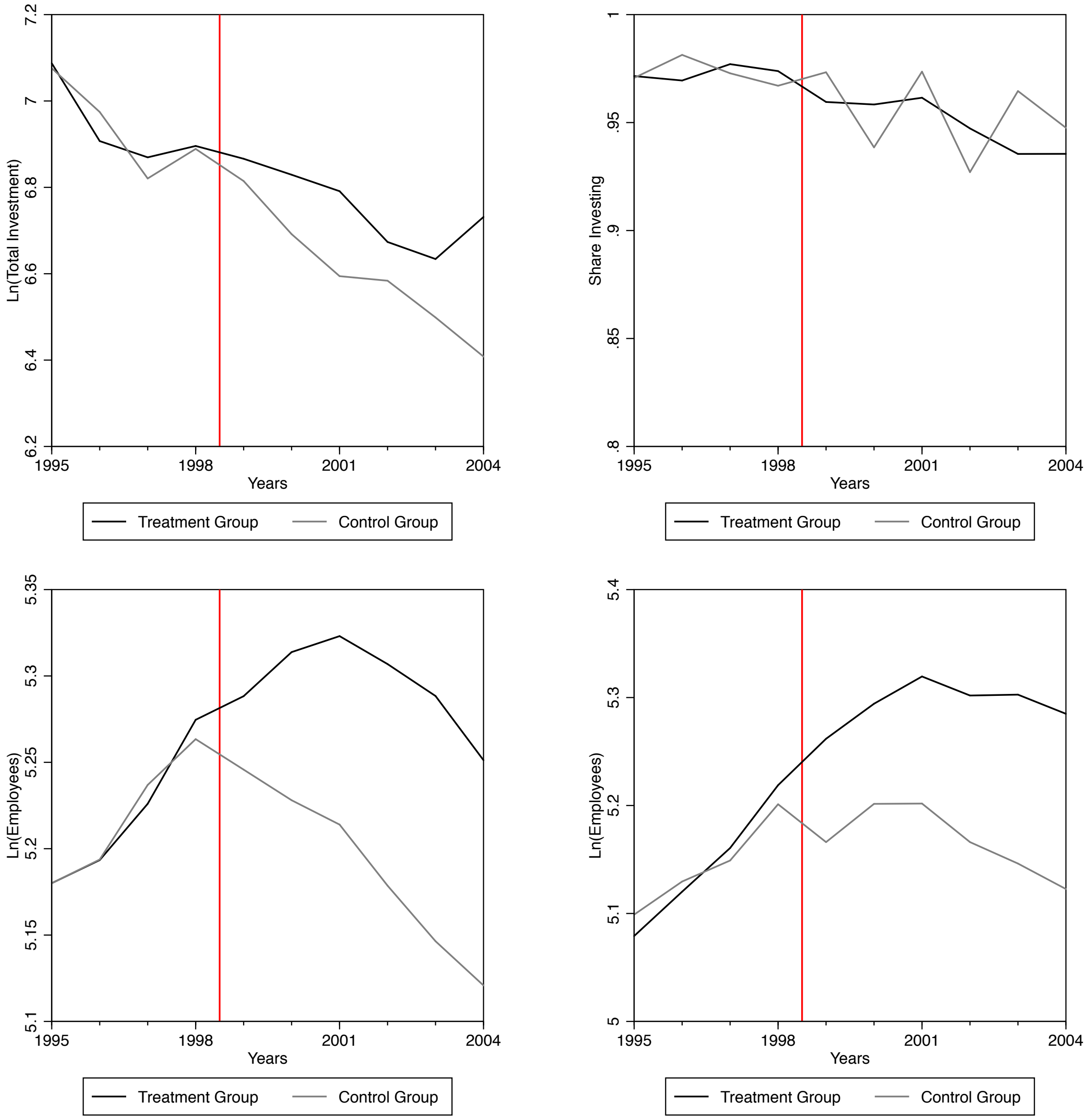


Figure 6: Average Firm Investment and Employment By Treatment Status

Note. The figures report raw means for each outcome by treatment and control group over the period 1995 to 2004. Outcome variables are the log of total investment in the upper left panel, share of firms with positive total investment in the upper right panel, log of employment based on AFID data in the lower left panel and log of employment based on BHP data in lower right panel. For each data point, I subtract the group mean for the pre-treatment period (1995-1998) and add the pooled mean for the same period for facilitating a comparison of trends.

Table 2: Difference-in-Differences - Investment

	Dependent Variable:				
	$\log\left(\begin{array}{c}\text{Total}\\ \text{Investment}\end{array}\right)$	$\log\left(\begin{array}{c}\text{Equipment}\\ \text{Investment}\end{array}\right)$	Investing (1/0)	$\frac{\text{Total Invest}_t}{\text{Avg Tot Invest}}$	$\frac{\text{Equip Invest}_t}{\text{Avg Tot Invest}}$
	(1)	(2)	(3)	(4)	(5)
Direct Effect	0.234**	0.251***	-0.000	0.171**	0.167**
	(0.089)	(0.092)	(0.008)	(0.081)	(0.068)
Observations	15,275	15,071	15,900	15,865	15,865

Note. Data from AFID establishment panel. Each coefficient is estimated from different regression following main specification (14). The dependent variables are the log of total investment in column (1), log of equipment investment in column (2), a dummy for having positive total investment in column (3), total investment over the average total investment between 1995-2004 in column (4) and equipment investment over the average total investment between 1995-2004 in column (5). Additional controls are log average firm wage, firm fixed effects, industry-year fixed effects and labor market-year fixed effects. Standard errors in parentheses are clustered at the regional level: * p<0.10, ** p<0.05, *** p<0.01

Table 3: Difference-in-Differences - Specification Robustness Investment

	(1)	(2)	(3)	(4)
	Dependent Variable: Log(Total Investment)			
Direct Effect	0.234**	0.218**	0.201**	0.270***
	(0.089)	(0.090)	(0.090)	(0.094)
Observations	15,275	14,547	15,275	13,047
	Dependent Variable: Log(Equipment Investment)			
Direct Effect	0.251***	0.231**	0.227**	0.285***
	(0.092)	(0.093)	(0.092)	(0.099)
Observations	15,071	14,347	15,071	12,880
	Dependent Variable: Investing (1/0)			
Direct Effect	-0.000	-0.000	-0.004	-0.002
	(0.008)	(0.008)	(0.007)	(0.009)
Observations	15,900	15,148	15,900	13,555

Note. Data from AFID establishment panel. Each coefficient is estimated from different regression following main specification (14). The dependent variables are the log of total investment in the upper panel and the log of equipment investment in the lower panel. Column (1) reproduces the coefficient from Table 2. Column (2) excludes firms with volatile investment measured by growth of total investment in 1997 above the 95$^{\text{th}}$ percentile, column (3) only includes log average firm wage, firm fixed effects and year fixed effects as control and column (4) is conditional on a a sample of single-establishment firms and excluding firms with volatile investment measured by growth of total investment in 1997 above the 95$^{\text{th}}$ percentile. Standard errors in parentheses are clustered at the regional level: * p<0.10, ** p<0.05, *** p<0.01

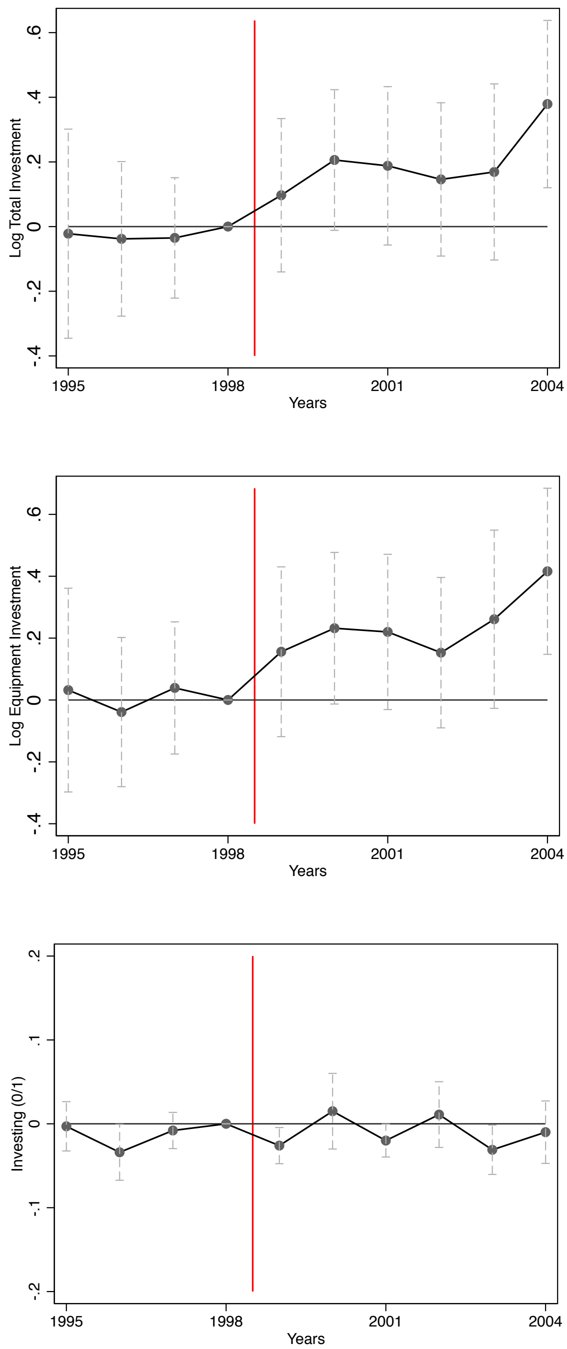


Figure 7: Dynamic Effect of Policy Change on Investment Behavior

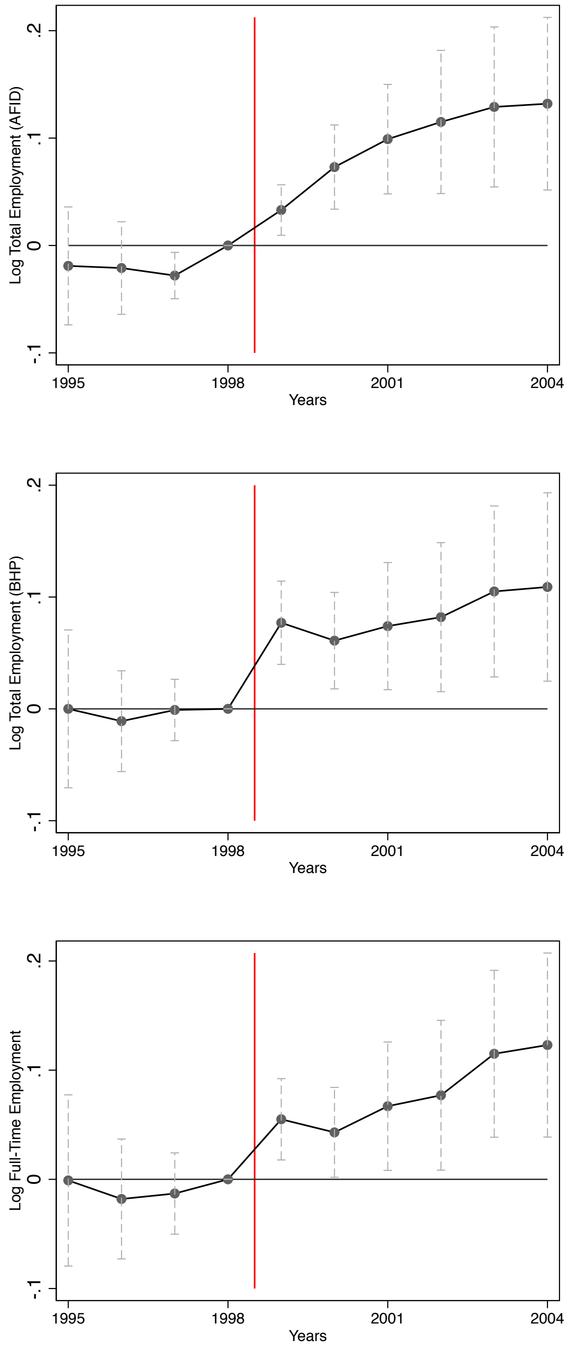
Note. Coefficients from estimation of dynamic specification (15). The dependent variables are the log of total investment in the upper panel, log of equipment investment in the middle panel and a dummy for having positive total investment in the lower panel. Additional controls are log average firm wage, firm fixed effects, industry-year fixed effects and labor market-year fixed effects. Firms with volatile investment, measured by growth of total investment in 1997 above the 95$^{\text{th}}$ percentile, are excluded. 95% confidence intervals are displayed for each coefficient using clustered standard errors at the regional level.

Table 4: Difference-in-Differences - Employment

	Dependent Variable: Log Employment of				
	Total (AFID)	Total (BHP)	Regular	Full-Time	Full-Time-Regular
	(1)	(2)	(3)	(4)	(5)
Direct Effect	0.113***	0.087**	0.076**	0.088**	0.087**
	(0.031)	(0.037)	(0.036)	(0.038)	(0.038)
Observations	17,637	10,116	10,116	10,116	10,116

Note. Each coefficient is estimated from different regression following main specification (14). The dependent variables are the log of total employment in column (1) and column (2), log of regular employees in column (3), log of full-time employees in column (4) and log of regular full-time employees in column (5). Data from AFID in column (1) and BHP in column (2)-(5). Additional controls are log average firm wage, firm fixed effects, industry-year fixed effects and labor market-year fixed effects. Standard errors in parentheses are clustered at the regional level: * p<0.10, ** p<0.05, *** p<0.01

Figure 8: Dynamic Effect of Policy Change on Firm Employment



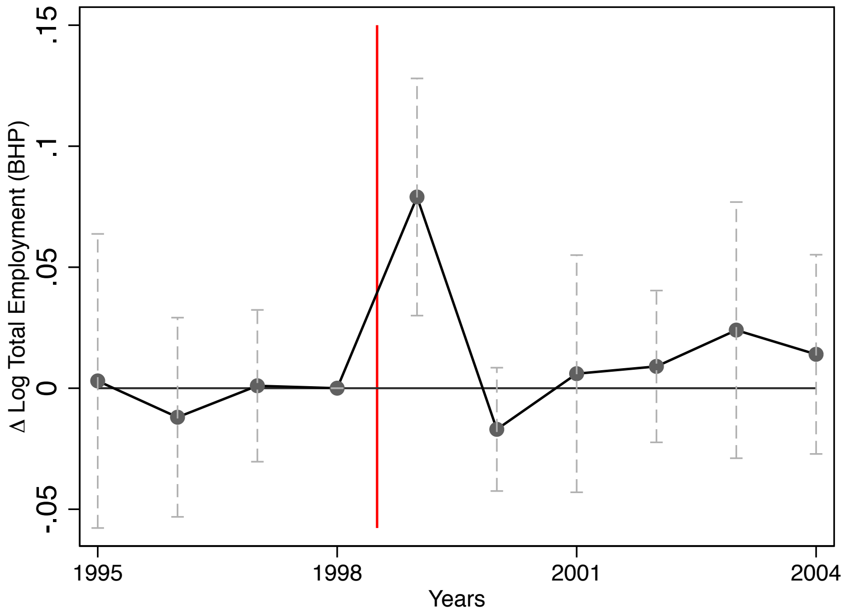
Note. Coefficients from estimation of dynamic specification (15). The dependent variables are the log of total employment. Additional controls are log average firm wage, firm fixed effects, industry-year fixed effects and labor market-year fixed effects. 95% confidence intervals are displayed for each coefficient using clustered standard errors at the regional level.

Table 5: Difference-in-Differences - Effect on Output

	Dependent Variable:				
	Log Revenue	Log Domestic Revenue	Log Domestic Manuf Revenue	Log Revenue (low exporting)	Log Products
	(1)	(2)	(3)	(4)	(5)
Direct Effect	0.011	0.083**	0.080*	0.076	0.022
	(0.037)	(0.038)	(0.040)	(0.050)	(0.032)
Obs	15,906	15,898	15,897	11,689	15,547

Note. Each coefficient is estimated from different regression following Specification 14. The dependent variables are log of total revenue in column (1), log of domestic revenue in column (2), log of domestic revenue for manufacturing output in column (3), log of revenue in column (4) and log of the number of distinct products in column (5). The sample in column (4) is conditional on an exporting share of below 0.15. Additional controls are log average firm wage, firm fixed effects, industry-year fixed effects and labor market-year fixed effects. Standard errors in parentheses are clustered at the regional level: * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Figure 9: Dynamic Effect of Policy Change on Firm Employment Growth



Note. Coefficients from estimation of dynamic specification (15). The dependent variables are the log of total employment. Additional controls are log average firm wage, firm fixed effects, industry-year fixed effects and labor market-year fixed effects. 95% confidence intervals are displayed for each coefficient using clustered standard errors at the regional level.

Table 6: Impact of Excluded Bandwidth Around Cutoff

	Excluded Bandwidth:				
	None	[238,262]	[225,275]	[213,287]	[200,300]
	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Log(Total Investment)				
Direct Effect	0.209**	0.208**	0.218**	0.212**	0.210**
	(0.082)	(0.085)	(0.090)	(0.096)	(0.098)
Observations	15,559	15,014	14,547	14,168	13,862
	Dependent Variable: Log(Equipment Investment)				
Direct Effect	0.207**	0.189**	0.231**	0.218**	0.215**
	(0.087)	(0.090)	(0.093)	(0.102)	(0.103)
Observations	15,354	14,813	14,347	13970	13,674
	Dependent Variable: Investing (0/1)				
Direct Effect	-0.000	-0.004	-0.000	-0.002	-0.001
	(0.009)	(0.008)	(0.008)	(0.008)	(0.008)
Observations	16,186	15,625	15,071	14,763	14,446
	Dependent Variable: Log(Employees) AFID				
Direct Effect	0.091***	0.105***	0.113***	0.108***	0.109***
	(0.030)	(0.031)	(0.031)	(0.033)	(0.034)
Observations	18,377	18,007	17,637	17,287	16,997
	Dependent Variable: Log(Employees) BHP				
Direct Effect	0.080**	0.080**	0.087**	0.107**	0.114**
	(0.036)	(0.036)	(0.037)	(0.044)	(0.046)
Observations	10,406	10,256	10,116	9,876	9,706
	Dependent Variable: Log(Domestic Revenue)				
Direct Effect	0.058*	0.074*	0.083**	0.088**	0.089**
	(0.032)	(0.038)	(0.038)	(0.040)	(0.039)
Observations	16,988	16,382	15,898	15,510	15,191

Note. Data from AFID establishment panel and BHP. Each coefficient is estimated from different regression following main specification (14). The excluded firm and observations (for employment only firm) are according to the column titles. Additional controls are log average firm wage, firm fixed effects, industry-year fixed effects and labor market-year fixed effects. Standard errors in parentheses are clustered at the regional level: * p<0.10, ** p<0.05, *** p<0.01

Table 7: Impact of Firm Size Interval

	Firm Size Interval					
	[80,225], [275,750]	[54,225], [275,1125]	[45,225], [275,1350]	[40,225], [275,1500]	[34,225], [275,1800]	[20,225], [275,3000]
	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variable: Log(Total Investment)					
Direct Effect	0.165*	0.244***	0.211**	0.218**	0.284***	0.327***
	(0.093)	(0.091)	(0.091)	(0.090)	(0.089)	(0.096)
Observations	6,307	10,459	12,821	14,547	17,043	23,945
	Dependent Variable: Log(Machinery Investment)					
Direct Effect	0.163*	0.262***	0.224**	0.231**	0.273***	0.300***
	(0.092)	(0.093)	(0.092)	(0.093)	(0.092)	(0.095)
Observations	6,218	10,332	12,657	14,347	16,799	23,566
	Dependent Variable: Investing (0/1)					
Direct Effect	0.008	0.013	0.003	-0.000	-0.003	-0.020***
	(0.010)	(0.010)	(0.008)	(0.008)	(0.008)	(0.007)
Observations	6,464	15,625	13,308	15,148	17,819	25,494
	Dependent Variable: Log(Employees) AFID					
Direct Effect	0.091**	0.101***	0.105***	0.113***	0.135***	0.128***
	(0.039)	(0.033)	(0.032)	(0.031)	(0.034)	(0.033)
Observations	7,964	12,968	15,626	17,637	20,455	27,717
	Dependent Variable: Log(Employees) BHP					
Direct Effect	0.097**	0.097**	0.089**	0.087**	0.101***	0.115***
	(0.045)	(0.040)	(0.037)	(0.037)	(0.035)	(0.034)
Observations	4,540	7,406	9,086	10,116	11,826	19,244
	Dependent Variable: Log(Domestic Revenue)					
Direct Effect	0.090**	0.096**	0.082**	0.083**	0.111***	0.121***
	(0.045)	(0.041)	(0.040)	(0.038)	(0.041)	(0.042)
Observations	6,731	11363	13,987	15,898	18,701	26,796

Note. Data from AFID establishment panel and BHP. Each coefficient is estimated from different regression following main specification (14). The sample consists of firms according to the size intervals given in the column titles. Except for log employment these intervals apply to observations as well. Additional controls are log average firm wage, firm fixed effects, industry-year fixed effects and labor market-year fixed effects. Standard errors in parentheses are clustered at the regional level: * p<0.10, ** p<0.05, *** p<0.01

Table 8: Placebo Cutoffs Within Treatment Group

	Dependent Variable				
	Log Total Investment	Log Machinery Investment	Investing (1/0)	Log Employees (AFID)	Log Employees (BHP)
	(1)	(2)	(3)	(4)	(5)
Cutoff: 80					
Direct Effect	-0.043	-0.038	-0.030*	0.033	0.006
	(0.078)	(0.076)	(0.016)	(0.026)	(0.030)
Observations	6,552	6,448	6,803	9,939	5,410
Cutoff: 100					
Direct Effect	-0.027	0.023	-0.024**	0.039	0.009
	(0.068)	(0.072)	(0.012)	(0.025)	(0.027)
Observations	8,623	8,510	9,011	12,237	7,036
Cutoff: 125					
Direct Effect	0.010	0.040	-0.020*	0.020	0.026
	(0.079)	(0.076)	(0.011)	(0.027)	(0.036)
Observations	10,385	10,247	10,886	14,025	7,916

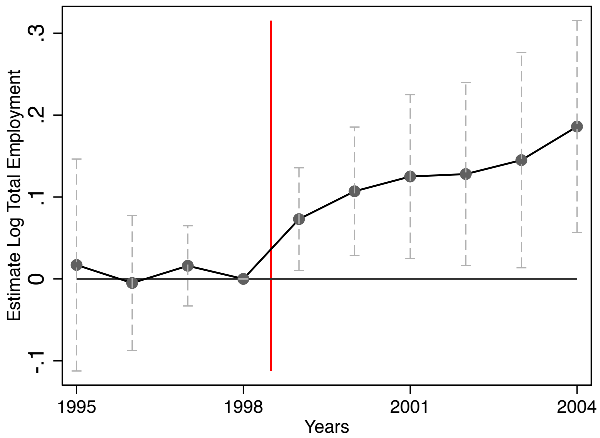
Note. Data from AFID establishment panel and BHP. Each coefficient is estimated from different regression following a modified version of specification (14). I change the firm size interval to between 50 and 250 employees. I set a cutoff for treatment and control group to 80 in the first panel, 100 in the second panel and 125 in the third panel. I exclude firms in an interval between -24 and +24 of the cutoff. Except for log employment these intervals apply to observations as well. Additional controls are log average firm wage, firm fixed effects, industry-year fixed effects and labor market-year fixed effects. Standard errors in parentheses are clustered at the regional level: * p<0.10, ** p<0.05, *** p<0.01

Table 9: Spillover Effects on Labor Inputs

	Dependent Variable: Log Total Employment				
	(1)	(2)	(3)	(4)	(5)
Direct Effect	0.107***	0.093**	0.112***	0.113***	0.101***
	(0.037)	(0.036)	(0.038)	(0.038)	(0.038)
Regional Share Firms Below Cutoff	0.118	0.116*	0.126*	0.117*	0.120**
	(0.075)	(0.067)	(0.072)	(0.062)	(0.056)
Observations	19,328	19,328	19,328	19,328	19,328
Controls					
Growth trends	-	X	-	-	X
Industry-Year FE	-	-	X	X	X
Area-Year FE	-	-	-	X	X

Note. Each column is estimated from different regression following specification 16. The dependent variable is log total employment. Additional controls are log average firm wage, firm and year fixed effects, and the controls specified in each column. The sample includes firms within firm size of [20,225],[275,1500] in 1998. Standard errors in parentheses are clustered at the regional level: * p<0.10, ** p<0.05, *** p<0.01

Figure 10: Dynamic Effect of Spillovers on Labor Inputs



Note. Coefficients θ_p from estimation of dynamic specification (17). The dependent variable is the log of total employment. The firm size interval is [20,225],[275,1500]. Additional controls are log average firm wage, firm fixed effects, industry-year fixed effects and area (Bundesland)-year fixed effects. 95% confidence intervals are displayed for each coefficient using clustered standard errors at the regional level.

Table 10: Difference-in-Differences - Analysis of Flow Data

	Dependent Variable:					
	Flows All Employees			Flows Unemployment		
	Net	In	Out	Net	In	Out
	(1)	(2)	(3)	(4)	(5)	(6)
Direct Effect	0.086**	0.102**	0.022	0.031	0.050	0.018
	(0.037)	(0.041)	(0.032)	(0.019)	(0.034)	(0.026)
Observations	10,099	10,099	10,099	10,099	10,099	10,099

Note. Each coefficient is estimated from different regression following main specification (14). The dependent variables are the log of cumulated net flows growth in column (1), the log of cumulated inflow growth in column (2), the log of cumulated outflow growth in column (3), the log of cumulated net flow growth from or to unemployment in column (4), the log of cumulated inflow growth from or to unemployment in column (5) and the log of cumulated outflow growth from or to unemployment in column (6). Growth rate is defined as a flow over past year total employment. Additional controls are log average firm wage, firm fixed effects, industry-year fixed effects and labor market-year fixed effects. Standard errors in parentheses are clustered at the regional level: * p<0.10, ** p<0.05, *** p<0.01

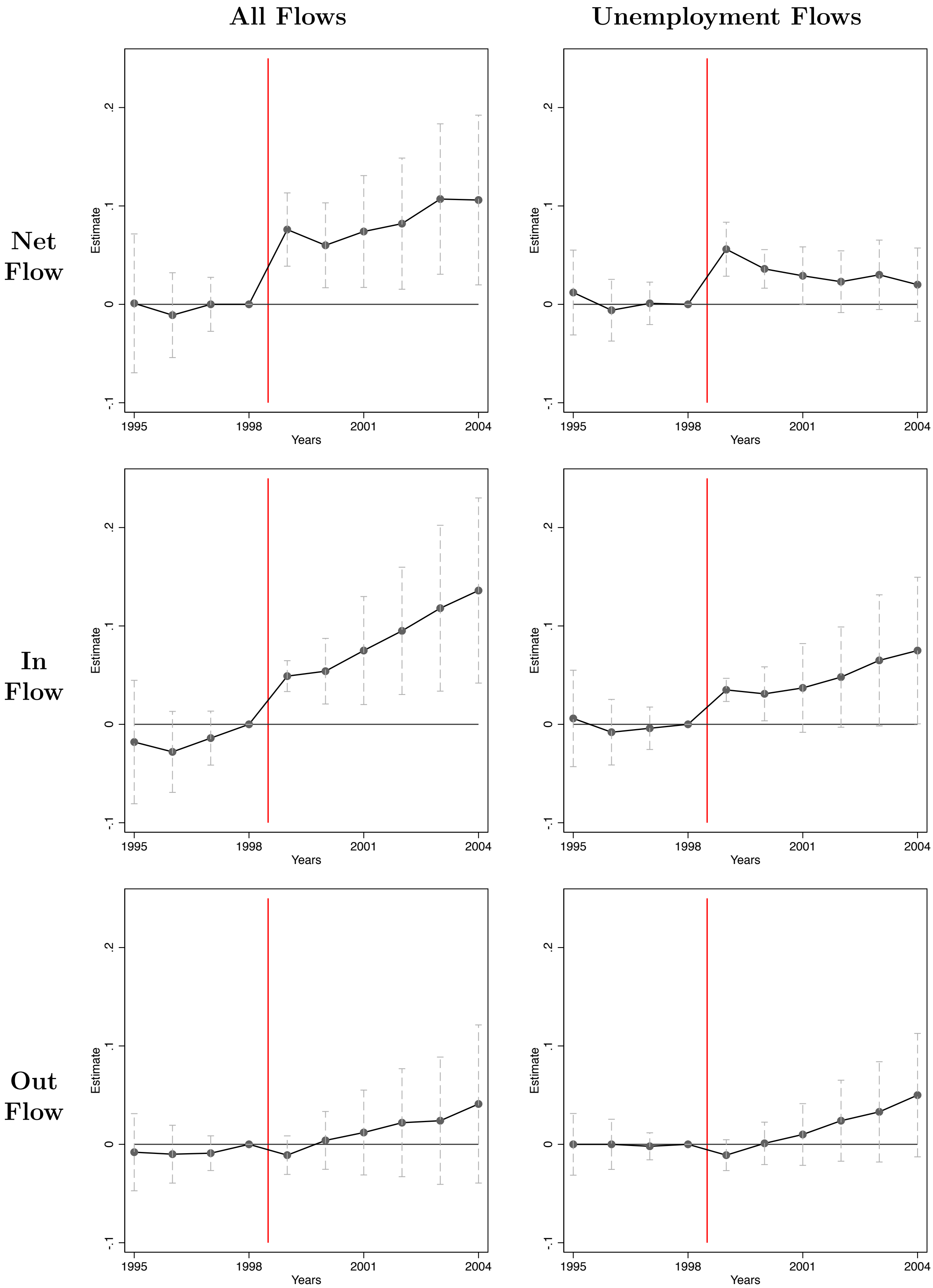


Figure 11: Dynamic Effect By Flow Type

Note. Coefficients from estimation of dynamic specification (15). The dependent variables are the log of cumulated flow growth. Additional controls are log average firm wage, firm fixed effects, industry-year fixed effects and labor market-year fixed effects. 95% confidence intervals are displayed for each coefficient using clustered standard errors at the regional level.

Table 11: Differences-in-Differences - Effect by Capital Cost Share

	Log Total Investments (1)	Investing (0/1) (2)	Log Employment (3)
Direct effect	0.020	0.004	0.041
	(0.145)	(0.017)	(0.055)
By capital cost share	0.880*	-0.015	0.301*
	(0.485)	(0.049)	(0.156)
Obs	14,453	15,792	17,412

Note. Each column is estimated from different regression following main specification (14) where the main interaction term is further interacted with a firms average capital cost share measured as average yearly investment costs over average yearly investment costs and average yearly wage bill. The dependent variables are the log of total investment in column (1), dummy of investing in column (2) and log of employees in column (3). Data is from AFID dataset. Additional controls are log average firm wage, firm fixed effects, industry-year fixed effects and labor market-year fixed effects. Standard errors in parentheses are clustered at the regional level: * p<0.10, ** p<0.05, *** p<0.01

Table 12: Spillover Effects on Labor Inputs By Industry

	Dependent Variable: Log Total Employment				
	(1)	(2)	(3)	(4)	(5)
Direct Effect	0.106***	0.093**	0.112***	0.113***	0.101***
	(0.037)	(0.036)	(0.038)	(0.038)	(0.038)
Regional Share Same Industry Below Cutoff	0.201	0.116	0.132	0.200	0.178
	(0.151)	(0.137)	(0.193)	(0.178)	(0.172)
Regional Share Other Industry Below Cutoff	0.104	0.116*	0.125*	0.105*	0.112**
	(0.074)	(0.067)	(0.071)	(0.062)	(0.056)
Observations	19,328	19,328	19,328	19,328	19,328
Controls					
Growth trends	-	X	-	-	X
Industry-Year FE	-	-	X	X	X
Area-Year FE	-	-	-	X	X

Note. Each column is estimated from different regression following specification 16. The dependent variable is log total employment. Additional controls are log average firm wage, firm and year fixed effects, and the controls specified in each column. The sample includes firms within firm size of [20,225],[275,1500] in 1998. Standard errors in parentheses are clustered at the regional level: * p<0.10, ** p<0.05, *** p<0.01

Table 13: Spillover Effects to Service Industry

	Dependent Variable: Log Total Employment				
	(1)	(2)	(3)	(4)	(5)
Regional Share Manuf Firms Below Cutoff	-0.028	0.004	-0.000	0.001	0.012
	(0.031)	(0.029)	(0.028)	(0.022)	(0.022)
Observations	75,965	75,965	75,965	75,965	75,965
Controls					
Growth trends	-	X	-	-	X
Industry-Year FE	-	-	X	X	X
Area-Year FE	-	-	-	X	X

Note. Each coefficient is estimated from different regression following specification 16 excluding the interaction term of the firm size cutoff on a sample of firms in the service industry. The dependent variable is log total employment. Additional controls are firm-level pre-treatment wage growth trends and the fixed effects specified in each column. Standard errors in parentheses are clustered at the regional level: * p<0.10, ** p<0.05, *** p<0.01

Table 14: Differences-in-Differences - Skill Ratios

	Dependent Variable: Log Ratio of				
	College Educated	Full-time College Educ	High-Skilled	Manual Labor	Qualified Within Manual Labor
	(1)	(2)	(3)	(4)	(5)
Direct Effect	-0.011	0.014	-0.027	-0.019	0.004
	(0.032)	(0.031)	(0.043)	(0.045)	(0.043)
Average Share (%)	11.6	12.3	13.4	74.2	42.2
Obs	8,521	8,466	8,623	8,968	8,008

Note. Each coefficient is estimated from different regression following main specification (14). The dependent variables are the log of college educated vs. non-college educated in column (1), the log of full-time college educated vs. full-time non-college educated in column (2), the log of high-skilled occupations vs. low-skilled occupations in column (3), the log of manual occupations vs. service occupations in column (4) and the log of qualified manual occupations vs. unqualified manual occupations in column (5). Additional controls are log average firm wage, firm fixed effects, industry-year fixed effects and labor market-year fixed effects. Standard errors in parentheses are clustered at the regional level: * p<0.10, ** p<0.05, *** p<0.01

Table 15: Differences-in-Differences - Employment Shares By Skill Level

	Dependent Variable: Log Ratio of				
	College Educated	Full-time College Educ	High-Skilled	Manual Labor	Qualified Within Manual Labor
	(1)	(2)	(3)	(4)	(5)
Direct Effect	-0.021	0.005	-0.041	-0.009	-0.006
	(0.029)	(0.028)	(0.041)	(0.045)	(0.043)
Direct Effect x ICT Share Investing IAB-LIAB (demeaned)	0.793**	0.710*	0.880**	-0.358	0.439
	(0.374)	(0.389)	(0.439)	(0.582)	(0.46)
Observations	8,521	8,466	8,623	8,968	8,008
Direct Effect	-0.013	0.014	-0.032	-0.022	0.002
	(0.030)	(0.029)	(0.041)	(0.045)	(0.045)
Direct Effect x ICT Investment Share ifo (demeaned)	0.745*	0.800*	1.081**	0.390	-0.322
	(0.435)	(0.418)	(0.426)	(0.682)	(0.720)
Observations	8,521	8,466	8,623	8,968	8,008
Direct Effect	-0.015	0.011	-0.036	-0.012	-0.010
	(0.030)	(0.029)	(0.042)	(0.045)	(0.043)
Direct Effect x ICT Capital Share EU KLEMS (demeaned)	1.984	1.775	2.877*	-1.084	1.591
	(1.371)	(1.420)	(1.677)	(2.201)	(2.082)
Observations	8,521	8,466	8,623	8,968	8,008
Direct Effect	-0.001	0.024	-0.019	-0.030	0.015
	(0.033)	(0.031)	(0.044)	(0.044)	(0.048)
Direct Effect x ICT Share CapEx U.S. Economic Census (demeaned)	1.378	1.113	1.514	-2.525	3.568*
	(1.388)	(1.276)	(1.378)	(1.988)	(2.142)
Observations	8,521	8,466	8,623	8,968	8,008

Note. Each coefficient is estimated from different regression following main specification (14) including interaction term. The dependent variables are the log of college educated vs. non-college educated in column (1), the log of full-time college educated vs. full-time non-college educated in column (2), the log of high-skilled occupations vs. low-skilled occupations in column (3), the log of manual occupations vs. service occupations in column (4) and the log of qualified manual occupations vs. unqualified manual occupations in column (5). The interaction term is the share of firms investing ICT in a given year based on the IAB-LIAB dataset in panel 1, the share of ICT investment based on Sauer, Strobel (2015) in panel 2, the ICT capital share for Germany based on EU KLEMS data and the ICT capital expenditure share based on U.S. Economic Census. Interaction terms are demeaned. Additional controls are log average firm wage, firm fixed effects, industry-year fixed effects and labor market-year fixed effects. Standard errors in parentheses are clustered at the regional level: * p<0.10, ** p<0.05, *** p<0.01

Appendices

A Competing Programs

The main text interprets the results of the empirical analysis as the effect of the investment tax credit program described in section 1. During the period of analysis, further support was available to firms in East Germany through other programs. There might be the concern, that these programs confound the analysis. I describe the two most important competing programs and the differences to the tax credit program. I discuss how relying simultaneously on cross-sectional and time series variation can control for the changes in the other programs. I also provide additional robustness tests that suggest little influence if any from the competing programs. Taken together, the evidence supports the claim that the results in the main text are caused by the tax credit program.

A.1 Special Depreciation Allowances

The German government introduced special depreciation allowances to all firms in East Germany in 1991.[40] In general, depreciations distribute the purchasing costs of fixed assets over the life span of the asset to match them to their generated profits. As an expense they reduce the amount of taxes on profits. The policy allowed the depreciation of 50% of an asset's value directly after purchase. The remaining value followed standard depreciation practices by depreciating equal amounts each year over the life span of the asset. This change did not influence the total amount of depreciation for the asset, however it moved the resulting tax benefit to earlier years. This reduces capital costs since temporal discounting makes a current benefit more valuable than an equally sized future one.

Given that there was only a shift in timing, the seemingly large special depreciation allowance converted to a smaller capital cost reduction. House and Shapiro (2008) calculate the capital cost reduction for a 50% special depreciation allowance of a similar program in the U.S. They find that for short-lived assets of 5 years, there is a reduction in capital costs by 1.26%. For long-lived assets of 20 years, the capital cost reduction is close to 5%. Equipment typically has a life span of 10 years and therefore the allowances provided relatively small benefits.

The special depreciation program lasted until the end of 1998 and thus coincided with the change in the tax credit rates. However, the percentage of special depreciation allowances was the same for all firms and did not induce any differential treatment be-

[40]The program is very similar to those studied in House and Shapiro (2008), Maffini et al. (2016) and Zwick and Mahon (2017). Eichfelder and Schneider (2014) analyze the German program comparing firm in East Germany with those in West Germany.

tween firms below and above the tax credit rate cutoff. Since I include time fixed effects in my main specification, I control for the discontinuation of the special depreciation allowances.[41]

Nevertheless, firms from different industries likely differ in their capital structure, particularly in the life span of their fixed assets. This leads to a different capital cost reduction from special depreciation allowances, with industries with longer-lived assets receiving a larger reduction. If the capital structure of industries correlates with firm size, special depreciation can influence treatment along the same dimensions as the tax credit program. However, since this type of influence is likely industry-specific, the use of industry-year fixed effects can control for this type of influence.[42]

The simultaneity of the changes in both tax policies was not a coincidence. The special depreciation allowances were stopped with the goal of incorporating a similar capital cost reduction through tax credits. This is the reason for the increase in the tax credit rate for all firms in 1999. At the same time, the policy shifted the focus towards small firms, introducing the additional differential treatment along firm size. There is no evidence that the focus on small firms was due to different pre-policy trends or anticipated differential shocks by firm size. The focus on small firms seems to rather reflect a general preference for small firms, which is common for policies worldwide. Arguments brought forward are the large share of small firms in an economy and unfair competition advantages of large firms.

A.2 German Joint Task Program

The German Joint Task Program was another program through which firms in East Germany could receive a reduction in investment costs. The program already existed before reunification of Germany and is currently the main place-based policy instrument in Germany.[43] After reunification, the main focus was put on regions in East Germany. The program allocated funds for infrastructure investments and firm-specific investments to each eligible federal state in Germany. Based on a general set of rules, each state then decided on the specific criteria for the selection and support of investment projects. Since funds were limited, decisions on allocation was based on a competitive basis. Important criteria were the expected profitability of firms and the commitment to keep or hire

[41] I consider the differential percentage change in capital costs from the equal reduction in special depreciation allowances due to a differentially distorted capital cost between firms below and above the tax credit rate cutoff as negligible.

[42] Any remaining interactions between the two programs should not hamper the qualitative results assuming that special depreciation allowances were less important since both programs were pure investment cost shocks. However, since the exact size of the shock would be unclear, it would be difficult to state the effects in terms of an elasticity.

[43] Dettmann et al. (2016) analyze the program using regional variation in West Germany and applying RDD techniques. Stierwald and Wiemers (2003) and Bade and Alm (2010) study the program using firm-level variation.

employees. Firms had to apply to these grants before initiating their investment project and were uncertain about the success of their application. Thus, this program mixed a reduction in capital costs with a selection on firm characteristics and specific employment decisions with uncertainty in the success and the value of a given grant.

Overall, during the years in question the joint task program was not as important as tax credits. Using information from the IAB-establishment survey for 1997, 76% of firms state that they used investment tax credits, whereas 34% used GRW grants. Most firms with GRW grants used tax credits at the same time as well. The value of funds distributed to East German manufacturing firms were roughly half of the tax credits. The program also focused on large investment projects with a priority for the opening of new firms.

Similar to the investment tax credits, the program provided larger incentives for smaller firms. However, the differential treatment stayed stable over time, with constant maximum subsidy rates for small and large firms respectively and little evidence for changes in applying this maximum subsidy rate. Thus, comparing firms below and above the tax credit rate cutoff before and after the policy change identifies the investment tax credits independent of the joint task program. For a few regions, maximum rates were adjusted at the end of 1996 and at the end of 1999 (Etzel and Siegloch, 2018), but the dynamic results in the main text suggest no effect for years before 1999 and a direct response to the investment tax credit change in 1999.

Table A.1: Difference-in-Differences - Effect on Investment (AFID)

	Dependent Variable:		
	$\frac{\text{Total Invest}_t}{\text{Avg Tot Invest}}$	$\frac{\text{Equip Invest}_t}{\text{Avg Tot Invest}}$	$\frac{\text{Structure Invest}_t}{\text{Avg Tot Invest}}$
	(1)	(2)	(3)
Treatment	0.167**	0.174**	-0.007
	(0.080)	(0.067)	(0.031)
Observations	15,865	15,865	15,865

Note. Data from AFID establishment panel. Each coefficient is estimated from different regression following main specification (14). The dependent variables are total investment over the average total investment between 1995-2004 in column (1), equipment investment over the average total investment between 1995-2004 in column (2) and investment in structures over the average total investment between 1995-2004 in column (3). Additional controls are log average firm wage, firm fixed effects, industry-year fixed effects and labor market-year fixed effects. Standard errors in parentheses are clustered at the regional level: * p<0.10, ** p<0.05, *** p<0.01

To further verify my claims, I consider a robustness test. In contrast to the tax credits, the GRW grants had preferential rates for small firms for investments in structures. Table A.1 compares the estimation result for investments in structures and equipment investments for manufacturing firms in East Germany. For better comparability, I focus on the investment measure in the main text that combines extensive and intensive margin effects. The first two columns reproduce the results for scaled total investment, with an estimate of 0.167 and scaled equipment investment, with an estimate of 0.174. In comparison, the effect for scaled investments in structures of -0.007 is economically and statistically insignificant. The difference in the effect for equipment and structures is in line with the regulations of the investment tax credit program but would be surprising for the GRW program.

B Theoretical Framework

In this section I provide detailed steps for solving the firm production model. I leave out general explanations that are covered in the main text.

In the model there are many heterogeneous firms i in many regions r with each firm producing one differentiated good. The production function for each firm is

$$F(K_i, U_i, S_i) = Y_i = A_i A_{ir} \left[(a_K K_i^\rho + a_S S_i^\rho)^{\frac{\mu}{\rho}} + a_U U_i^\mu \right]^{\frac{1}{\mu}}, \tag{19}$$

where output Y_i is produced using capital K_i, low-skilled labor U_i and high-skilled labor S_i as input factors. The nested CES-production function is governed by the parameters a_K, a_S and a_U. The elasticity of substitution between low-skilled workers and capital (or high-skilled workers) is $\frac{1}{1-\mu}$ and the elasticity of substitution between high-skilled and capital is $\frac{1}{1-\rho}$. Production also depends on a firm-specific production parameter A_i and a region-specific productivity shifter A_{ir}.

Each firm maximizes their profit facing the downward sloping inverse demand curve

$$p_i = B Y_i^{-\frac{1}{\eta^D}}, \tag{20}$$

and has a capital rental rate r, wage to low-skilled labor w_U and wage to high-skilled labor w_S. Each firm receives a tax credit τ_i on their capital.

The firm maximization problem then is

$$\max_{K_i, S_i, U_i} p_i Y_i - (1 - \tau_i) r K_i - w_S S_i - w_U U_i, \tag{21}$$

subject to equations (19) and (20).

I plug in equations (19) and (20) to obtain an unconstrained maximization problem. The resulting first order conditions from the maximization problem are

$$(1 - \tau_i) r = \left(1 - \frac{1}{\eta^D}\right) B a_K Y_i^{1-\mu-\frac{1}{\eta^D}} X^{\frac{\mu-\rho}{\rho}} K_i^{\rho-1} (A_i A_{ir})^\mu \tag{22}$$

$$w_S = \left(1 - \frac{1}{\eta^D}\right) B a_S Y_i^{1-\mu-\frac{1}{\eta^D}} X^{\frac{\mu-\rho}{\rho}} S_i^{\rho-1} (A_i A_{ir})^\mu \tag{23}$$

$$w_U = \left(1 - \frac{1}{\eta^D}\right) B a_U Y_i^{1-\mu-\frac{1}{\eta^D}} U_i^{\mu-1} (A_i A_{ir})^\mu, \tag{24}$$

where

$$X_i = a_K K_i^\rho + a_S S_i^\rho, \tag{25}$$

and where I substitute parts of the expression with the production function.

The FOCs serve to study how firms adjust their production process after a change in tax credit rate. Considering that tax credits are a form of reduction in capital costs, it is straightforward to show that

$$d \ln c_K = \frac{d(1-\tau_i)r}{(1-\tau_i)r} = -\frac{r d\tau_i}{(1-\tau_i)r} = \frac{d\tau_i}{1-\tau_i}, \tag{26}$$

where c_K are capital costs of a firm.

With that in mind, the total derivative of equation (22) written as elasticity with respect to the tax credit rate is

$$-1 = \left(1 - \mu - \frac{1}{\eta^D}\right) \frac{dY_i}{d\tau_i}\frac{1-\tau_i}{Y_i} + \frac{\mu-\rho}{\rho}\frac{dX_i}{d\tau_i}\frac{1-\tau_i}{X_i} + (\rho-1)\frac{dK_i}{d\tau_i}\frac{1-\tau_i}{K_i} + \mu\frac{dA_{ir}}{d\tau_i}\frac{1-\tau_i}{A_{ir}}. \tag{27}$$

To simplify the calculation of the total derivative based on equation (23), first I divide left- and right-hand side with equation (22). The resulting elasticity is

$$\frac{dS_i}{d\tau_i}\frac{1-\tau_i}{S_i} = \frac{dK_i}{d\tau_i}\frac{1-\tau_i}{K_i} - \frac{1}{1-\rho}. \tag{28}$$

The total derivative of equation (24) written as elasticity is

$$\frac{dU_i}{d\tau_i}\frac{1-\tau_i}{U_i} = \left(1 - \frac{1}{(1-\mu)\eta^D}\right) \frac{dY_i}{d\tau_i}\frac{1-\tau_i}{Y_i} + \frac{\mu}{1-\mu}\frac{dA_{ir}}{d\tau_i}\frac{1-\tau_i}{A_{ir}}. \tag{29}$$

The equations (27)–(29) depend on the elasticity of Y_i and X_i with respect to the tax credit rate. I totally differentiate equation (19) and (25) to receive

$$\frac{dY_i}{d\tau_i}\frac{1-\tau_i}{Y_i} = \frac{dA_{ir}}{d\tau_i}\frac{1-\tau_i}{A_{ir}} + \left(\frac{A_i A_{ir}}{Y_i}\right)^{\mu} \left(a_U U_i^{\mu}\frac{dU_i}{d\tau_i}\frac{1-\tau_i}{U_i} + a_K X_i^{\frac{\mu-\rho}{\rho}} K_i^{\rho}\frac{dK_i}{d\tau_i}\frac{1-\tau_i}{K_i} + a_S X_i^{\frac{\mu-\rho}{\rho}} S_i^{\rho}\frac{dS_i}{d\tau_i}\frac{1-\tau_i}{S_i}\right) \tag{30}$$

$$\frac{dX_i}{d\tau_i}\frac{1-\tau_i}{X_i} = \rho a_K \frac{K_i^{\rho}}{X_i}\frac{dK_i}{d\tau_i}\frac{1-\tau_i}{K_i} + \rho a_S \frac{S_i^{\rho}}{X_i}\frac{dS_i}{d\tau_i}\frac{1-\tau_i}{S_i}. \tag{31}$$

With equations (27)–(31), there are six unknown elasticities and five equations. For now I keep the elasticity of regional productivity and solve the system of equations for the rest of the elasticities. I do so by plugging in equation (28) into equation (31) and subsequently equation (31) into equation (27). I also plug equations (28) and (29) in equation (30). This leads to two equations and by subtracting one from the other, the elasticities for output and capital are

$$\frac{dY_i}{d\tau_i}\frac{1-\tau_i}{Y_i} = \eta^D \left(\frac{A_i A_{ir}}{Y_i}\right)^{\mu} X_i^{\frac{\mu}{\rho}} \left(1 - \frac{a_S S_i^{\rho}}{X_i}\right) + \eta^D \frac{dA_{ir}}{d\tau_i}\frac{1-\tau_i}{A_{ir}} \tag{32}$$

$$\frac{dK_i}{d\tau_i}\frac{1-\tau_i}{K_i} = \eta^D \left(\frac{A_iA_{ir}}{Y_i}\right)^{\mu} X_i^{\frac{\mu}{\rho}} \frac{a_K K_i^{\rho}}{X_i} + \frac{1}{1-\mu}\left(1-\left(\frac{A_iA_{ir}}{Y_i}\right)^{\mu} X_i^{\frac{\mu}{\rho}} \frac{a_K K_i^{\rho}}{X_i}\right) - \left(\frac{1}{1-\mu}-\frac{1}{1-\rho}\right)\frac{a_S S_i^{\rho}}{X_i} + (\eta^D-1)\frac{dA_{ir}}{d\tau_i}\frac{1-\tau_i}{A_{ir}}. \quad (33)$$

Following from that, the elasticities for low-skilled and high-skilled labor are

$$\frac{dS_i}{d\tau_i}\frac{1-\tau_i}{S_i} = \left(\eta^D - \frac{1}{1-\mu}\right)\left(\frac{A_iA_{ir}}{Y_i}\right)^{\mu} X_i^{\frac{\mu}{\rho}} \frac{a_K K_i^{\rho}}{X_i} + \left(\frac{1}{1-\mu}-\frac{1}{1-\rho}\right)\frac{a_K K_i^{\rho}}{X_i} + (\eta^D-1)\frac{dA_{ir}}{d\tau_i}\frac{1-\tau_i}{A_{ir}} \quad (34)$$

$$\frac{dU_i}{d\tau_i}\frac{1-\tau_i}{U_i} = \left(\eta^D - \frac{1}{1-\mu}\right)\left(\frac{A_iA_{ir}}{Y_i}\right)^{\mu} X_i^{\frac{\mu}{\rho}} \frac{a_K K_i^{\rho}}{X_i} + (\eta^D-1)\frac{dA_{ir}}{d\tau_i}\frac{1-\tau_i}{A_{ir}}. \quad (35)$$

To facilitate the interpretation of these elasticities, I substitute parts of the equations with the relationships given in the FOCs. I rearrange equations (22)–(24) as

$$\left(\frac{A_iA_{ir}}{Y_i}\right)^{\mu} X_i^{\frac{\mu}{\rho}} \frac{a_K K_i^{\rho}}{X_i} = \frac{\eta^D}{\eta^D-1}\frac{(1-\tau_i)r}{p_i}\frac{K_i}{Y_i} = Zs_{K_i} \quad (36)$$

$$\left(\frac{A_iA_{ir}}{Y_i}\right)^{\mu} X_i^{\frac{\mu}{\rho}} \frac{a_S S_i^{\rho}}{X_i} = \frac{\eta^D}{\eta^D-1}\frac{w_S}{p_i}\frac{S_i}{Y_i} = Zs_{S_i} \quad (37)$$

$$\left(\frac{A_iA_{ir}}{Y_i}\right)^{\mu} a_U U_i^{\mu} = \frac{\eta^D}{\eta^D-1}\frac{w_U}{p_i}\frac{U_i}{Y_i} = Zs_{U_i}, \quad (38)$$

where $Z = \frac{\eta^D}{\eta^D-1}$, $s_{K_i} = \frac{(1-\tau_i)r}{p_i}\frac{K_i}{Y_i}$, $s_{S_i} = \frac{w_S}{p_i}\frac{S_i}{Y_i}$ and $s_{U_i} = \frac{w_U}{p_i}\frac{U_i}{Y_i}$, omitting the index i on the shares for .

With these terms the elasticities can be written as

$$\frac{dY_i}{d\tau_i}\frac{1-\tau_i}{Y_i} = \eta^D s_{K_i} Z \qquad +\eta^D \frac{dA_{ir}}{d\tau_i}\frac{1-\tau_i}{A_{ir}} \quad (39)$$

$$\frac{dK_i}{d\tau_i}\frac{1-\tau_i}{K_i} = \left[\eta^D s_{K_i} + \frac{1}{1-\mu}s_{U_i} + \frac{1}{1-\rho}s_{S_i} - \left(\frac{1}{1-\mu}-\frac{1}{1-\rho}\right)s_{U_i}\frac{a_S S_i^{\rho}}{X_i}\right]Z \qquad +(\eta^D-1)\frac{dA_{ir}}{d\tau_i}\frac{1-\tau_i}{A_{ir}} \quad (40)$$

$$\frac{dS_i}{d\tau_i}\frac{1-\tau_i}{S_i} = \left[\left(\eta^D - \frac{1}{1-\rho}\right)s_{K_i} + \left(\frac{1}{1-\mu}-\frac{1}{1-\rho}\right)s_{U_i}\frac{a_K K_i^{\rho}}{X_i}\right]Z \qquad +(\eta^D-1)\frac{dA_{ir}}{d\tau_i}\frac{1-\tau_i}{A_{ir}} \quad (41)$$

$$\frac{dU_i}{d\tau_i}\frac{1-\tau_i}{U_i} = \left(\eta^D - \frac{1}{1-\mu}\right)s_{K_i} Z \qquad +(\eta^D-1)\frac{dA_{ir}}{d\tau_i}\frac{1-\tau_i}{A_{ir}}. \quad (42)$$

To consider changes in the composition of the labor force by skill type, I use the ratio between high-skilled and low-skilled. We have

$$\frac{S_i}{U_i} = \left(\frac{w_s}{w_u}\frac{a_u}{a_s}\right)^{\frac{1}{\rho-1}} X_i^{\frac{\rho-\mu}{\rho(\rho-1)}} U_i^{\frac{\rho-\mu}{1-\rho}} \quad (43)$$

The elasticity with respect to the tax credit can be calculated using the previous elasticities as

$$\frac{d\frac{S_i}{U_i}}{d\tau_i}\frac{1-\tau_i}{\frac{S_i}{U_i}} = \frac{\mu-\rho}{(1-\mu)(1-\rho)}\frac{a_K K_i^{\rho}}{X_i} \tag{44}$$

The above results summarize the effect of a change in tax credit rate on firms that experience the rate change. Before I define A_{ir} more specifically, I first consider the effect of a change in tax credit rate on firms that do not experience the rate change.

In this case, taking into account that τ_j is not affected by changes in τ_i, the total derivatives from the FOCs are

$$0 = \left(1-\mu-\frac{1}{\eta^D}\right)\frac{dY_j}{d\tau_i}\frac{1-\tau_i}{Y_j} + \frac{\mu-\rho}{\rho}\frac{dX_j}{d\tau_i}\frac{1-\tau_i}{X_j} + (\rho-1)\frac{dK_j}{d\tau_i}\frac{1-\tau_i}{K_j} + \mu\frac{dA_{ir}}{d\tau_i}\frac{1-\tau_i}{A_{ir}} \tag{45}$$

$$\frac{dS_j}{d\tau_i}\frac{1-\tau_i}{S_j} = \frac{dK_j}{d\tau_i}\frac{1-\tau_i}{K_j} - \frac{1}{1-\rho} \tag{46}$$

$$\frac{dU_j}{d\tau_i}\frac{1-\tau_i}{U_j} = \left(1-\frac{1}{(1-\mu)\eta^D}\right)\frac{dY_j}{d\tau_i}\frac{1-\tau_i}{Y_j} + \frac{\mu}{1-\mu}\frac{dA_{ir}}{d\tau_i}\frac{1-\tau_i}{A_{ir}} \tag{47}$$

$$\frac{dY_j}{d\tau_i}\frac{1-\tau_i}{Y_j} = \frac{dA_{ir}}{d\tau_i}\frac{1-\tau_i}{A_{ir}} + \left(\frac{A_j A_{ir}}{Y_j}\right)^{\mu}\left(a_U U_j^{\mu}\frac{dU_j}{d\tau_i}\frac{1-\tau_i}{U_j} + a_K X_j^{\frac{\mu-\rho}{\rho}} K_j^{\rho}\frac{dK_j}{d\tau_i}\frac{1-\tau_i}{K_j} + a_S X_j^{\frac{\mu-\rho}{\rho}} S_j^{\rho}\frac{dS_j}{d\tau_i}\frac{1-\tau_i}{S_j}\right) \tag{48}$$

$$\frac{dX_j}{d\tau_i}\frac{1-\tau_i}{X_j} = \rho a_K \frac{K_j^{\rho}}{X_j}\frac{dK_j}{d\tau_i}\frac{1-\tau_i}{K_j} + \rho a_S \frac{S_j^{\rho}}{X_j}\frac{dS_j}{d\tau_i}\frac{1-\tau_i}{S_j}. \tag{49}$$

I solve again for the five elasticities as before. I plug equations (46) and (49) into equation (45), and I use this result together with equation (46) and (47) in equation (48).

The elasticities are

$$\frac{dK_j}{d\tau_i}\frac{1-\tau_i}{K_j} = \frac{dS_j}{d\tau_i}\frac{1-\tau_i}{S_j} = \frac{dU_j}{d\tau_i}\frac{1-\tau_i}{U_j} = (\eta^D-1)\frac{dA_{ir}}{d\tau_i}\frac{1-\tau_i}{A_{ir}}, \tag{50}$$

and

$$\frac{dY_j}{d\tau_i}\frac{1-\tau_i}{Y_j} = \eta^D\frac{dA_{ir}}{d\tau_i}\frac{1-\tau_i}{A_{ir}}. \tag{51}$$

Focusing now on the indirect effect through changes in the regional productivity shifter, I first define the productivity shifter as

$$A_{ir} = \sum_{j\in S_r} Y_j^{\lambda_{ij}}, \tag{52}$$

where productivity for firm i in region r depends on the accumulated output of each firm i in region r weighted by the agglomeration elasticity λ_{ij}.

To simplify exposition, I assume that $\lambda_{ij} = \lambda$ and thus the productivity shifter is the same for all firms in a particular region, $A_r = A_{ir}$.

With this dependence, it is possible to provide a closed form solution for the elasticity of regional productivity with respect to the tax credit rate τ_i:

$$\frac{dA_r}{d\tau_i}\frac{1-\tau_i}{A_r} = \frac{d\sum Y_j^\lambda}{d\tau_i}\frac{1-\tau_i}{\sum Y_j^\lambda} = \sum\left(\lambda Y_j^{\lambda-1}\frac{dY_j}{d\tau_i}\right)\frac{1-\tau_i}{\sum Y_j^\lambda} = \frac{\lambda}{\sum Y_j^\lambda}\sum Y_j^\lambda\frac{dY_j}{d\tau_i}\frac{1-\tau_i}{Y_j}. \quad (53)$$

By plugging in equations (32) and (51), the elasticity of regional productivity to a change in tax credits of firm i is

$$\frac{dA_r}{d\tau_i}\frac{1-\tau_i}{A_r} = \frac{\lambda\eta^D}{1-\lambda\eta^D}\frac{s_{K_i}Y_i^\lambda}{\sum Y_j^\lambda}Z. \quad (54)$$

For the case that several firms receive a change in the tax credit rate, it is possible to consider the total derivative

$$dA_r = \sum_i\frac{dA_r}{d\tau_i}d\tau_i = \sum_i\frac{dA_r}{d\tau_i}\frac{1-\tau_i}{A_r}\frac{A_r}{1-\tau_i}d\tau_i \quad (55)$$

With the result in equation (54), this transforms to

$$dA_r = \sum_i\frac{\lambda\eta^D}{1-\lambda\eta^D}\frac{s_{K_i}Y_i^\lambda}{\sum Y_j^\lambda}Z\frac{A_r}{1-\tau_i}d\tau_i = \frac{\lambda\eta^D}{1-\lambda\eta^D}Z\sum_i s_{K_i}Y_i^\lambda\frac{d\tau_i}{1-\tau_i} \quad (56)$$

To get to a meaningful expression in terms of elasticity, I make simplifying assumptions. I assume that the change in tax credit rate applies to a subset of all firms and is the same for this subset. Thus, $d\tau_i = d\tau$ for some firms and for the rest $d\tau_i = 0$. For the subset of firms experiencing change, I also assume that $\tau_i = \tau$. With that it is possible to rearrange terms to get

$$\frac{dA_r}{d\tau}\frac{1-\tau}{A_r} = \frac{\lambda\eta^D}{1-\lambda\eta^D}Z\sum_{i|d\tau_i\neq 0}\frac{s_{K_i}Y_i^\lambda}{\sum Y_j^\lambda} \quad (57)$$

C Data

This section complements the data section in the main text. I provide descriptive statistics for the complete set of variables used in the analysis. Further, I provide details on creating the final datasets, imputations of variables and specific sample selection. The following information pertains to the AFID Establishment-Panel[44], the Panel of Cost Structure[45] and the Establishment Panel Schmucker et al. (2016).

C.1 Descriptive Statistics

Table C.1: Descriptive Statistics AFID-Panel for Regression Sample

	Mean	P10	Median	P90	Count
Investment variables					
Investing (%)	95.15	100.00	100.00	100.00	15,900
Investments all types (1,000)	1244.40	26.01	351.13	2,547.56	15,900
Investments equipment (1,000)	976.48	15.71	252.82	1,933.44	15,900
Investments all over average investments	1.01	0.10	0.71	2.26	15,865
Investments equipment over average investments	0.81	0.09	0.59	1.76	15,865
log (investments all types (1,000))	5.86	3.73	5.94	7.88	15,275
log (investments equipment (1,000))	5.58	3.42	5.65	7.61	15,071
Employment variables					
No. of total employees	121.76	44	79	218	17,640
log (total employees)	4.50	3.78	4.37	5.38	17,640
Other firm variables					
Policy-relevant firm size	115.60	42	75	206	17,640
Labor market level variables					
Share employees in firm with ≤250 employees (%)	71.10	52.14	72.85	89.60	51
Share employees in firms of specific industry with ≤250 employees (%)	5.57	0.55	4.10	11.90	679
Share employees in firms of other industries with ≤250 employees (%)	65.38	47.16	65.81	83.22	679

Note: Statistics are based on firms in the manufacturing sector for the years 1995-2004 excluding Berlin. The descriptives of investment and employment variables are additional based on the associated regression sample in the main text. The descriptives of Other firm variables are based on the regression sample of employment. Labor market level variables are based on all manufacturing firms.

[44] Source: RDC of the Federal Statistical Office and Statistical Offices of the Länder, AFiD Establishment-Panel, 1995–2004, own calculations.

[45] Source: RDC of the Federal Statistical Office and Statistical Offices of the Länder, Panel of Cost Structure, 1999–2001, own calculations.

Table C.2: Descriptive Statistics BHP-Panel for Regression Sample

	Mean	P10	Median	P90	Count
Employment variables					
No. of total employees	127.10	42	78	253	10,180
No. of full-time employees	114.33	37	70	225	10,180
No. of regular employees	118.69	39	73	239	10,180
No. of regular full-time employees	114.29	37	70	225	10,180
log (total employees)	4.51	3.74	4.36	5.53	10,180
log (full-time employees)	4.39	3.61	4.25	5.42	10,180
log (regular employees)	4.44	3.66	4.29	5.48	10,180
log (regular full-time employees)	4.39	3.61	4.25	5.42	10,180
Δ log(total employees)	0.03	-0.11	.02	0.18	9,060
log (flow measure net all)	0.35	-0.31	0.19	1.06	10,163
log (flow measure hires all)	0.98	0.16	0.75	1.94	10,163
log (flow measure separations all)	0.61	0.14	0.53	1.20	10,163
log (flow measure net unemployment)	0.15	-0.22	0.06	0.58	10,163
log (flow measure hires unemployment)	0.55	0.09	0.43	1.11	10,163
log (flow measure separations unemployment)	0.40	0.08	0.35	0.76	10,163
Shares and ratios					
Share college (%)	11.68	1.52	8.70	25.00	9,106
Share college among full-time (%)	12.35	1.59	9.30	26.74	9,106
Share high-skilled occup (%)	13.42	2.00	10.20	26.98	9,106
Share manual occup among low-/mid-skilled (%)	74.06	41.48	80.65	88.24	9,105
Share mid-skilled occup among manual (%)	42.23	4.44	34.27	95.00	9,015
log (college / non-college)	-2.31	-3.77	-2.27	-1.06	8,560
log (college full-time / non-college full-time)	-2.23	-3.66	-2.20	-0.96	8,505
log (high-skilled occup / other occup)	-2.15	-3.53	-2.10	-0.97	8,666
log (manual occup / service occup)	1.30	-0.22	1.44	2.58	8,997
log (mid-skilled manual / low-skilled manual)	-0.53	-2.66	-0.69	1.97	8,030
Other firm variables					
Average daily wage full-time	61.64	40.68	59.05	85.12	9,106
log (average daily wage full-time)	4.08	3.71	4.08	4.44	9,106
Policy-relevant firm size	121.52	40	75	243	10,180
Labor market level variables					
Share employees in firm with $\leq$250 employees (%)	79.53	–	82.25	–	55
Share employees in firms of specific industry with $\leq$250 employees (%)	6.13	0.74	4.38	14.24	707
Share employees in firms of other industries with $\leq$250 employees (%)	73.41	51.69	75.95	92.90	707

Note: Statistics are based on firms in the manufacturing sector for the years 1995-2004 excluding Berlin. The descriptives of employment variables, and shares and ratios are additional based on the associated regression sample in the main text. The descriptives of Other firm variables are based on the regression sample of employment. Labor market level variables are based on all manufacturing firms.

C.2 Prediction of Apprentices in AFID Establishment-Panel

For determining the firm size in context of the policy all employees count as one person independent of their employment contract or their working hours. The only exception are apprentices that have special contracts and therefore are not employees in legal terms. In the AFID establishment panel there is no information about apprentices. In order to have a better measure of the firm size, I use information from the Panel of Cost Structure (KSE). In this survey, firms were asked about their number of apprentices for the years 1999–2001 for a subsample of manufacturing firms. I create the share of apprentices within this dataset and then link this information to the AFID establishment panel with their firm identifier. For observations in which information on the share of apprentices is matched, I use the matched information. For observations that are part of a firm with matched apprentices information, I assume the share to be equal to the average share of matched information. I use information on the total number of employees in the AFID establishment panel in each year to impute the number of apprentices for these cases. For firms that have no matched apprentices information, I calculate the average share within 3-digit industry classifiers and assume it to be constant for all observations with missing information. For industries that have no single firm with matched apprentice information, I calculate the average share in the whole sample and assume it to be constant for all observations with missing information.

C.3 Predicting Marginally Employed Workers in the BHP

On the other hand, the firm size definition included marginally employed workers. The BHP records information about the number of marginally employed only since 1999 though. For classifying firms into small firms with at most 250 employees and large firms with more than 250 employees, it is important to impute marginally employed for prior years.

I use the following approach. First, I calculate the share of marginally employed for each establishment averaging over the years 1999 and 2000. For establishments that existed in these years, I assume that this share is constant over time and impute the number of marginally employed for years before 1999 directly. For establishments that did not exist in 1999 or afterwards, this direct method does not work. For these cases, I run a simple prediction model. I include all establishments in 1999 and 2000 with more than 20 total employees. I run the model separately for East and West Germany and include 3-digit industry classifiers and firm size. I allow the coefficient for firm size to vary within each industry. Since the prediction model is linear, the predicted share can be negative. In these cases, I set the share to zero. I also winsorize at the 99th-percentile to exclude extreme values in the prediction. I then proceed as before and assume a constant share within each establishment.

Table C.3 provides information for the actual share, the calculated share and the predicted share.

C.4 Entry and Exit of Firms in the BHP

Since the difference-in-difference design makes use of the panel component, it is important to understand how firms enter and exit into the dataset. The BHP aggregates social security information at establishment level and draws a 50% random sample of establishment identifiers. As noted in the data report by Schmucker et al. (2016), establishment identifiers can change when ownership changes or the establishment is merged with or split into different companies. Thus, some changes in the establishment identifier do not lead to changes in economic activity. To overcome this shortcoming, I merge an extension file provided by the IAB based on the methodology of Hethey and Schmieder (2010, 2013). In this file, entries and exits are classified according to flow information of coworkers from one establishment to another.[46] Essentially, if most employees move together from one establishment during its last year and constitute a large share of a newly created establishment, it is likely a change in the identifier without change in the economic structure. In contrast, if most workers move into unemployment or to various other establishments, it is likely an exiting firm. Table C.4 provides information from the extension file for the overall dataset and the specific sample in the data analysis. ID changes do not happen that often. In the regression sample 0.59% of entering establishments and 0.6% exiting establishments are affected.

The dataset provides information on the preceding / succeeding establishment identifier for entering / exiting establishments with identifier change. I use this information to match these establishments. I take into account that the establishment identifier can change more than once over the observation period. In case the information from identifier changes of entering and exiting establishments differs I choose the largest component. With this algorithm, I end up with 60 newly merged establishments in the regression sample which is a share of 3.24% of all establishments. There is also information on merges and split ups. However, these changes are more likely to influence production and I therefore consider them as distinct.

There is a second dimension to entry and exit that to the best of my knowledge has not been addressed in the official data description. It is concerned with the question whether an entry and exit into the dataset coincides with actual start and end of economic production. On various occasions, an establishment starts to exist with one or few employees and only in the next or a later year there is a jump to a distinctively larger number of employees. The same pattern in reverse happens before the exit of various establishments.

[46] This methodology uses matched employer-employee data that is not accessible to me.

Table C.3: Marginally employed in BHP Data

Regression Sample	Mean	P10	P50	P90	Count
Actual Shares 1999 - 2004	0.045	0.000	0.013	0.098	9,271
Actual Shares 1999 - 2000	0.040	0.000	0.012	0.086	3,380
Imputed within establishment 1995-1998	0.040	0.000	0.012	0.083	6,516
Imputed with prediction model 1995-1998	0.052	0.000	0.051	0.106	231
Absolute Difference Actual Value vs Imputed within Establishment 2001-2004	0.027	0.000	0.009	0.055	5,890
Absolute Difference Actual Value vs Prediction Model 2001-2004	0.057	0.004	0.037	0.102	5,891

Note: Statistics are based on establishments in the manufacturing sector for the years 1995-2004 excluding Berlin. The number of observations is based on the statistic for number of employees.

Table C.4: Descriptive Statistics - IAB-BHP Data

ID Changes Entering Establishments	
Overall (%)	0.17
Regression sample (%)	0.35
Regression sample (count)	56
ID Changes Exiting Establishments	
Overall (%)	0.16
Regression sample (%)	0.48
Regression sample (count)	77
Matching Regression Sample	
Affected establishments (%)	8.23
Affected establishments (count)	148
Matched establishments (%)	5.92
Matched establishments (count)	109

Note:

Table C.5: Descriptive Statistics

	Mean	Min	P10	P50	P 90	Max	Obs
Yearly employment growth rate	0.24	-1	-0.72	0.01	1.54	368	15,400
Yearly employment growth rate AFID	0.02	/	-0.11	0.01	0.15	/	15,867
Employment before increase by 200%		1	1	9	96	96	89
Employment after decrease by 67%		0	0	3	341	1,337	180
Share inflow from other establishments normal growth	0.39	0	0.0	0.38	0.71	1	14,802
Share outflow to other establishments normal growth	0.34	0	0.0	0.33	0.67	1	14,849
Share inflow from other establishments high growth	0.72	0	0.36	0.80	0.96	1	124
Share outflow to other establishments low growth	0.67	0	0.22	0.77	0.95	1	170

Table C.5 summarizes the incident of these patterns. Examining the percentiles of yearly employment growth, the median firm has zero growth. At the 10th and 90th percentile, growth rates are already large with a decrease by 72% and an increase by 154%. The smallest and largest growth rates are extreme with either a fall to zero employees or an increase of 36,800%. This is not the case for the AFID data. Interestingly, many of these changes coincide with movement of employees from or to other (not necessarily the same) employers. The share of inflowing employees that worked before is 72% during growth of more than 200% but only 39% during lower growth excluding marginally employed. The share of outflowing employees that work afterwards is 67% during growth of less than -67% but only 34% during higher growth. This could potentially point to additional ID changes.

Because of these patterns, I assume that periods before growth of 200% or higher and periods after growth of -67% or lower are periods of non-production and I exclude them as observations.[47] These cutoffs are guided by establishment behavior in the AFID dataset. To provide an understanding how different cutoffs affect the empirical analysis, Table C.6 provides estimates for employment using various cutoffs. Figure 12 shows the dynamic profile.

Table C.6: Static Estimation - Difference of Growth Rate Cutoff of Outliers

	1.5 (1)	2.0 (2)	2.5 (3)	3.0 (4)	5.0 (5)	10.0 (6)	none (7)
	Dependent Variable: Log(Number Employees)						
β	0.102***	0.076**	0.082**	0.080**	0.096**	0.063	0.125
	(0.029)	(0.030)	(0.035)	(0.035)	(0.046)	(0.052)	(0.089)
Obs	7,900	9,550	9,920	10,070	10,370	10,450	11,192
	Dependent Variable: Log(Ratio High-Skilled to Low-Skilled Occupation)						
β	-0.012	0.001	-0.005	-0.011	-0.020	-0.017	-0.030
	(0.033)	(0.031)	(0.033)	(0.032)	(0.029)	(0.030)	(0.031)
Obs	6,776	8,099	8,374	8,478	8,678	8,731	9,285

Note. Each coefficient is estimated from different regression following specification 14. The dependent variables are machinery investment over lagged revenue in column (1), machinery investment over lagged imputed capital in column(2), the natural logarithm of machinery investment in column (3) and a dummy for positive machinery investment in column (4). The upper panel shows estimates for regressions including firms with 40 to 1,500 employees in 1998, the lower one for firms with 80 to 750 employees. Further, the sample consist of single-establishment manufacturing firms in East Germany excluding Berlin with observations in 1995 and 2004. For all regressions, firms with imputed negative capital are excluded. In Column (1) and (2), values are truncated at the largest 5%. Standard errors in parentheses are clustered at the regional level: * p<0.10, ** p<0.05, *** p<0.01

[47] Although I consider it unlikely, it is possible, that the changes in employment happen because of large genuine adjustments in economic production from one year to another.

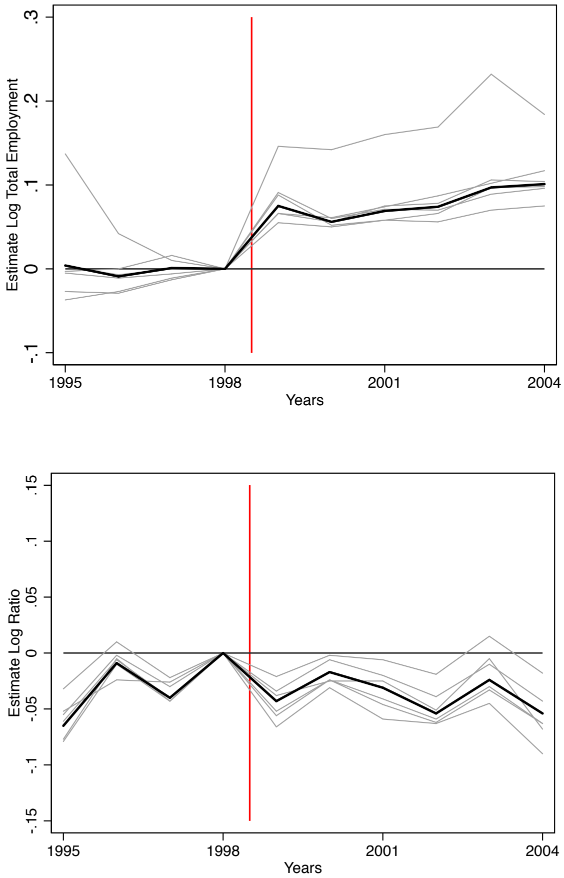


Figure 12: Dynamic Estimation - Difference of Growth Rate Cutoff of Outliers

Note. Coefficients from estimation of dynamic specification (15). The dependent variables are the log of total investment in the upper panel, log of equipment investment in the middle panel and a dummy for having positive total investment in the lower panel. Additional controls are log average establishment wage, establishment fixed effects, industry-year fixed effects and labor market-year fixed effects. Establishments with volatile investment, measured by growth of total investment in 1997 above the 95$^{\text{th}}$ percentile, are excluded. 95% confidence intervals are displayed for each coefficient using clustered standard errors at the regional level.