Committing to Grow: Employment Targets and Firm Dynamics*

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Abstract

We examine effects of government-imposed employment targets on firm behavior. Theoretically, such policies create "polarization," causing low-productivity firms to exit the market while others temporarily distort their employment upward. Dynamically, firms are incentivized to improve productivity to meet targets. Using novel data from East German firms post-privatization, we find that firms with binding employment targets experienced 25% higher annual employment growth, a 1.1% higher annual exit probability, and 10% higher annual productivity growth over the target period. Structural estimates reveal substantial misallocation of labor across firms and that subsidizing productivity growth would yield twice the long term increases in employment.

Keywords: industrial policy, productivity, size-dependent regulations, privatizations.

JEL classification: D22, D24, J08, L25.

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1 Introduction

Industrial policies place the promotion and protection of employment at the heart of their design. Policies often achieve this by either directly inducing labor hiring through subsidizing the cost of labor at the firm level, or alternatively by promoting firms' investment in productivity, which can lead to an organic long term increase in employment through higher returns on labor. In choosing among these strategies, policymakers weigh the costs of creating distortions due to misallocating resources against their potential short- and long-term benefits. In this context, an important and pervasive policy that directly affects firm employment levels is the use of employment targets or commitments by governments. This policy tool is highly relevant in policy debates worldwide, especially during periods of rapid economic transition when social backlash pressures governments to safeguard employment. For instance, the Paycheck Protection Program, one of the largest firm-based policy initiatives in U.S. history, offered forgivable loans conditional on maintaining employee counts during and after the COVID-19 pandemic. Employment considerations also play a pivotal role in government approval of M&A deals, as demonstrated by the acquisition of Alstom by General Electric in 2014. The French government decided not to veto the acquisition after General Electric pledged to create 1,000 jobs, backed by penalties of 50,000 Euros for each unfulfilled job. Despite clear empirical importance and policy interest, the implications of labor commitments for misallocation and productivity dynamics across firms remain unexplored due to a lack of systematic data.

In this paper, we study, theoretically and empirically, the economic implications of employment targets for firm dynamics. On the theory side, we study a dynamic model of firms with endogenous productivity growth. The key intuition of our model is that a firm's maximization problem with labor targets is equivalent to the firm receiving a wage subsidy and incurring a fixed cost. These changes in the cost structure of the firm lead to a static "polarization effect" and a dynamic "out-of-necessity" improvement of firm productivity. The "polarization effect" either pushes firms up or out of the market: firms with very low productivity stop hiring and exit the market, while the remaining firms make an upwardly distorted employment choice. This higher employment is temporary in nature, as employment reverts back once the policy expires. The dynamic effect leads firms to "necessity-driven" productivity improvements as they have added incentives to align their productivity with the employment target to reduce the cost of the distortion. We confirm the empirical relevance of these mechanisms by leveraging the data of East German firms operating under employment targets after their privatization. Importantly, our setting uniquely allows us not only to obtain systematic data on employment commitments, but also to address the endogeneity of employment targets by exploiting the quasi-random allocation of privatizers to firms as an Instrumental Variable. We find that, over an average commitment period of 3 years, firms with binding employment targets experience not only a 25% percentage points higher annual employment growth rate and a 1.1% percentage points higher exit probability but also a 10% percentage points higher annual productivity growth. Finally, the structural estimates of the model enable us to quantify the cost of labor misallocation introduced by employment targets and decompose their employment implications into transitory employment growth due to the “polarization effect” as opposed to longer-term growth arising from “necessity-driven” productivity improvements by firms. We estimate that the policy results in 15% higher long-term employment gains stemming from the latter effect, but at the cost of substantial misallocation of labor across firms. We also show that direct subsidies for productivity improvements, aimed at organic firm employment growth, could lead to twice the permanent employment gains without inducing static misallocation of labor across firms.

Our analysis proceeds in three steps. First, we introduce a tractable dynamic model with endogenous productivity growth at the firm level to study the impact of labor commitments. In this model, firms operate using a standard Cobb-Douglas production function between productivity and labor without any fixed operational costs. Consequently, firms of any productivity level find it worthwhile to operate and generate positive profits at all times. In an equilibrium without labor commitments, both labor and profits increase linearly with productivity. Next, we incorporate labor commitments into the model. Under this policy, in line with our empirical setting, firms that fall short of their labor target incur a penalty for each worker they are missing. This introduction of labor commitments leads to rich implications within the model. Intriguingly, this policy reduces variable costs for firms at the expense of introducing a fixed cost into their maximization problem. The static maximization for firms is now divided into multiple productivity regions. First, we observe a "polarization" effect: firms with the poorest productivity opt not to hire any workers due to the new implied fixed cost and exit the market, while all other firms that initially had fewer workers than their employment target now increase their labor due to the implied lower variable costs. Moreover, this policy leads some firms to operate with negative profits, with the hope that future productivity improvements will help them recover their losses. Second, in the dynamic equilibrium, firms intensify efforts to enhance their productivity "out of necessity" to align their productivity so that the committed labor matches the firm’s equilibrium hiring. This reflects the additional benefits the firms get from improving their productivity as it reduces the costs associated to the penalty. Thus, the model produces both a transitory impact due to the “polarization effect” and a permanent impact from “necessity-driven” productivity improvement.

In the second step of the analysis, we take these predictions to the data. The empirical setting exploits the fact that following the German reunification, policymakers required that new owners of privatized firms in East Germany commit to employment targets, with penalties imposed for falling below the committed employment level. In total, these labor commitments were applied to over 18,000 privatization contracts, covering more than 900,000 workers in East Germany. Our empirical analysis relies on a novel dataset from the German archives that contains all the documentation produced by the *Treuhandanstalt* (THA), the government agency responsible for the privatization process in East Germany. Our data therefore contains detailed contract-level information on employment targets and deadlines, as well as the dates and results of each audit of the employment commitment.

Identifying and measuring the impact of industrial policy on firm-level reactions is challenging (Juhász, Lane, and Rodrik 2023). In the context of employment targets, the German reunification provides a unique setting to disentangle the impact of the policy from the endogenous allocation mechanism of labor commitments to firms. In the spirit of the literature on judge leniency (Bhuller, Dahl, Løken, and Mogstad 2020; Dobbie and Song 2015; Bernstein, Colonnelli, Giroud, and Iverson 2019), we develop an instrumental variable (IV) approach that exploits heterogeneous preferences of privatizers and their quasi-random assignment to firms. To do so, we estimate the propensity of a privatizer to require binding labor commitments, i.e., in which the target is higher than the initial employment level. We show that: (i) the probability of receiving a binding contract increases continuously along the labor preference measure, (ii) these preferences are heterogeneous across privatizers, and (iii) they are persistent across time. Importantly, we provide evidence consistent with the quasi-random assignment mechanism of firms to privatizers. To do so, we use information from the balance sheets of firms before their privatization. Consistent with anecdotal evidence about the organization of THA, we find no evidence of an economically or statistically significant correlation of our instrument with a wide range of sectoral characteristics, employment and revenue measures, and other individual characteristics of the privatizers.

Our empirical analysis finds evidence supporting both the "polarization" effect of labor commitments, and their dynamic implications in terms of "necessity-driven" productivity improvements. Consistent with the model's predictions about the up or out consequences of the policy for firms, during the commitment period of on average 3 years, our IV estimates reveal a 25% points higher annual employment growth rate for firms with binding labor contracts compared to those without. At the same time, firms with binding contracts exhibit, on average, a 1.1% points higher annual probability of exiting over the commitment period. Consistent with the dynamic implications of the firm model, we show that "necessity-driven" productivity improvements are also significantly higher for firms remaining in the market. For these firms, binding labor contracts lead to an additional yearly productivity growth of approximately 10% points. To complement the dynamic analysis, we provide additional evidence related to patenting activity and commitment horizons. We document a higher patenting activity for firms under binding labor commitments and show that this effect is especially strong for firms with initially lower productivity levels. We further show that "necessity-driven" productivity improvement effects are significantly stronger for longer commitment horizons.

Our empirical findings are robust to a wide range of alternative specifications. In particular, we address two specific challenges to our identification strategy. The first one relates to the fact that privatization decisions are multidimensional as privatizers also decide on sales price, investment commitments, and financial support. We examine this threat to the exclusion restriction, by following Bhuller, Dahl, Løken, and Mogstad (2020), and augmenting our baseline model by estimating and including privatizer preferences in these other contract dimensions. A second challenge in estimating the impact of labor commitments on employment and productivity growth is related to the role of firm exit. We address concerns about sample selection affecting our estimates of the policy effect in several ways. We implement a Heckman correction procedure and estimate a selection equation predicting the likelihood of firm exit by the end of the commitment period. We also provide estimates for specifications in which we assign an employment and productivity growth rate of -2 if firms exit by the end of the commitment period. Finally, in all specifications, we normalize employment and productivity growth by controlling for initial values of these variables.

In the last step of our analysis, we calibrate the model to the data and run several counterfactual scenarios in order to quantitatively assess the different margins of distortion on firm behavior and the resulting misallocation of labor across firms. To identify the parameters of the model, especially the penalty of not meeting the target, we match the effects of binding employment commitments on firm outcomes uncovered in our empirical analysis. The calibrated model is able to reproduce the main patterns in the data well. Importantly, the model replicates firm-level growth patterns across the employment commitment distribution and after the expiration of the policy.

The structural estimates of the model allow us to study three counterfactual economies. We first simulate an economy without employment targets and find that aggregate employment would persistently be 15% lower after 10 years. Despite the higher aggregate employment induced by the commitment policy, labor is severely misallocated across firms. We calculate that the loss of output due to misallocation of labor induced during the commitment policy is around 1.8%. Next, we decompose the employment implications of commitments into transitory employment growth due to the "polarization effect" as opposed to persistent growth arising from "necessity-driven" productivity improvements by firms at different horizons. In the short run, our calibrated model attributes one-third of the employment growth to necessity-driven productivity effects. In the long run, the entire increase in employment is driven by these dynamic effects. Lastly, we consider an alternative policy of subsidizing investment into productivity. We calibrate the subsidy rate to achieve the aggregate employment growth in the data by the end of the commitment period. The subsidy policy generates a more gradual but higher persistent employment gains without inducing static misallocation of labor across firms. Our results therefore highlight the differential impact of alternative industrial policies at different horizons.[1]

Our analysis provides novel insights into a wide range of public policies that contain firm-specific employment commitments. In both the United States and Europe, numerous place-based policies tie tax credits and grants directly to pledges of job creation, as illustrated by initiatives like the North Carolina Job Development Investment Grant (Economic Investment Committee 2022) and the German *Gemeinschaftsaufgabe Verbesserung der regionalen Wirtschaftsstruktur* (Titze 2007). Public procurement laws often incorporate provisions, such as the "social clause" found in Italian law (Art. 57 of the Law Decree dated March 31, 2023), by which the bidding process prioritizes firms with the greatest employment safeguards. Similarly, within bankruptcy proceedings, such as those in France (Code de Commerce L.642-2 and L.642-5), policymakers mandate judges to prioritize acquirers based on their employment commitments rather than on the acquisition price of distressed firms. Finally, employment considerations play a pivotal role in governmental scrutiny and approval of significant M&A deals, as shown by the acquisition of Alstom by General Electric in 2014 (General Electric's

[1]Throughout this paper, we abstract from general equilibrium considerations, such as endogenous wages, and focus on the behavior of the individual firm under employment target policies. In the context of East Germany, we argue that this is a reasonable approach given the high prevailing unemployment and the rigid wage-setting framework. While our mechanism at the firm level are robust under endogenous wages, the counterfactual results could be quantitatively affected, the magnitude of which would depend on the scale of the policy and the state of the labor market.

Commitments Monitoring Committee 2019) or Kraft's Acquisition of Cadbury in 2009 (Secretary of State for Business, Innovation and Skills 2010).[2]

The paper contributes to the extensive and important literature examining the merits and costs of industrial policies, which is comprehensively surveyed by Juhász, Lane, and Rodrik (2023).[3] Similar to our paper, several contributions in this literature exploit historical settings to causally identify the impact of policies. Lane (2022) and Choi and Levchenko (2021) use historical data to study the dynamic impact of the South Korean heavy and chemical industry drive from 1973 to 1979. Giorcelli and Li (2021) estimate the long-term effects of technology and know-how transfers on China's structural transformation using data from the Sino-Soviet alliance in the 1950s. Methodologically, our paper is related to recent studies leveraging a quantitative model-based framework to evaluate industrial policies. Kalouptsidi (2018) and Barwick, Kalouptsidi, and Zahur (2021) study the Chinese intervention in the shipbuilding industry. Acemoglu, Akcigit, Alp, Bloom, and Kerr (2018) demonstrate that strategic industrial policies have significant influence over firms' composition, with the potential to harness the economy's firm selection process to amplify overall productivity gains. Liu (2019) embeds industrial policy in a production network setting and applies it to interventions in South Korea in the 1970s and modern-day China. Criscuolo, Martin, Overman, and Van Reenen (2019) exploit changes in area-specific eligibility criteria to evaluate the impact of place based policies. Bustos, Caprettini, and Ponticelli (2016) and Bustos, Garber, and Ponticelli (2020) study the impact of the adoption of new agricultural technologies on structural transformation in Brazil. Our contribution to this literature is to show how government policies can "polarize" firm trajectories and dynamically generate "necessity-driven" productivity improvements. These effects also illustrate how a time-limited distortion such as a labor commitment can have a lasting impact on firm dynamics.

The paper also contributes to the recent literature on the consequences of size-dependent regulations that frequently favor smaller firms. These policies can potentially create distortions in the economy that affect aggregate productivity by misallocating resources toward less-productive firms (Restuccia and Rogerson 2008; Hsieh and Klenow 2009; Bartelsman, Haltiwanger, and Scarpetta 2013; Syverson 2011). Garicano, Lelarge, and Van Reenen (2016) leverage size-contingent laws in France to identify the equilibrium and welfare effects of labor regulation. Braguinsky, Branstetter, and Regateiro (2011) document how the entire Portuguese firm size distribution has shifted over time to the left. They attribute this process to strong protections for regular workers. Martin, Nataraj, and Harrison (2017) use the elimination of small-scale industry promotion in India to study firm dynamics. The dismantling of these policies leads not only to increased entry, but also higher output growth in more exposed districts. We also analyze a policy distorting the firms' employment decisions, but focus on its dynamic implications, similar to recent papers by Aghion, Bergeaud, and Van Reenen

[2]In the case of Cadbury the House of Commons monitored and requested employment commitments from Kraft's executive vice president Marc Firestone.

[3]The objectives and tools of industrial policies can be wide-ranging. In the context of innovation policies, a combination of taxes, subsidies, and regulation can be directed at promoting technological change and specific industries (Aghion, Dechezleprêtre, Hemous, Martin, and Van Reenen 2016; Acemoglu, Akcigit, Hanley, and Kerr 2016). Similarly, persistent gaps in economic performance across regions have prompted governments to create a variety of place-based economic development policies (Greenstone, Hornbeck, and Moretti 2010; Kline and Moretti 2014).

(2023) and Akcigit, Alp, Akgunduz, Cilasun, and Quintero (2023).[4] We contribute to this literature by analyzing a unique policy creating distortions that are not only firm-specific and time-limited but also actively push firms to grow or operate at a larger size. Consequently, the trade-offs generated by this policy are distinct from those introducing barriers to growth. We show that firms operating under labor targets are "polarized" into up or out dynamics that either pushes them up or out of the market, and that firms dynamically react by implementing "out-of-necessity" productivity improvements to comply with the target. The heterogeneity in imposed targets and richness of our data allow us to also document that, consistent with the model, the dynamic effects are larger for i) initially low productivity firms, and ii) longer commitment horizons.

Finally, our paper contributes to the literature studying the German reunification. The process of economic convergence between East and West Germany was extensively analyzed in terms of labor reallocation (Dauth, Lee, Findeisen, and Porzio 2021; Fuchs-Schündeln and Schündeln 2005), migration (Uhlig 2008; Hunt 2006; Peters 2022; Redding and Sturm 2008), capital investments from West Germany (Sinn 2002), collective bargaining agreements (Burda and Hunt 2001; Burda 2010), as well as social and cultural attitudes (Alesina and Fuchs-Schündeln 2007; Burchardi and Hassan 2013; Laudenbach, Malmendier, and Niessen-Ruenzi forthcoming).[5] Mergele, Hennicke, and Lubczyk (2020) study the privatization process and show that firms with higher baseline productivity were more likely to be privatized and yield higher prices. We show how the implementation of strategic industrial goals by the government – in the form of labor commitments – dynamically distorts firms by modifying their labor, productivity and exit choices.

The remainder of this paper is organized as follows. Section 2 introduces a simple model of firm growth to study the dynamic implications of employment targets. Section 3 describes the institutional framework under which employment commitments were introduced in East Germany after reunification. In Section 4, we describe the data and provide descriptive statistics. Section 5 shows the identification strategy and reports our empirical results. The structural model and the quantitative estimation are shown in Section 6. Section 7 concludes.

2 A Model of Firms with Employment Targets

In this section, we build a simple model of firm growth to study the implications of employment targets. We consider an economy in continuous time, populated by a large number of heterogeneous firms in productivity producing a homogeneous good. One of the main features of our model is that firms can improve their productivity through investment, which allows us to study the dynamic consequences of operating under such targets. At any point in time, firms choose (i) the amount of

[4] These papers study, through the lens of endogenous growth models, size-dependent regulations that introduce barriers to growth. While the first paper focuses on a developed country setting like France, the second paper studies a developing country setting and concerns how regulations interact with informality in labor markets.

[5] The persistent economic differences between the East and West German economies are studied by Snower and Merkl (2006) and Burda (2006). More recently, Heise and Porzio (2021) provide evidence for low labor mobility between East and West Germany. Bachmann, Bayer, Stüber, and Wellschmied (2022) relate higher monopsony power to lower productivity convergence.

labor to hire for production, (ii) how much to invest in improving firm productivity, and (iii) whether to exit the economy or not. In this section, our main analysis concerns the decision of an individual firm, therefore we abstract from general equilibrium considerations such as labor supply decisions, wage setting and firm entry.[6]

2.1 Static Environment

Firms are endowed with a constant returns to scale Cobb-Douglas production function of productivity and labor:

$$y_{t,j} = z_{t,j}^{1-\alpha} l_{t,j}^{\alpha}, \quad 0 < \alpha < 1$$

where $z_{t,j}$ denotes the level of productivity at firm j at time t, which is heterogeneous across firms, and $l_{t,j}$ is the amount of labor hired.[7] Firms take the wage rate w_t as given. Firms operate under perfect competition and the price of the homogeneous good is normalized to be one, without loss of generality.[8] In what follows, we drop the time and firm subscripts t, j whenever it does not cause any confusion.

Firms operate under employment targets, l_*, which are heterogeneous across firms and exogenously set by the policy makers. Consistent with the institutional framework we study in the empirical part of the paper, firms pay a penalty if they operate below the target level of employment and the penalty is proportional to the missing amount of employment:

$$\gamma \left(l_* - l\right)^+ w$$

where γ is a parameter that controls the amount of penalty per missing employee as a fraction of the wage rate.

Given this structure, the firm's static profit maximization problem is given by

$$\Pi(z, l_*) = \max_{l \geq 0} \left\{ z^{1-\alpha} l^{\alpha} - wl - \gamma(l_* - l)^+ w \right\}.$$

The next lemma describes the solution to the static profit maximization problem.

Lemma 1 The static profits and the respective optimal labor decision for a firm with productivity level z and employment target l_* are given by

[6] We argue that our assumptions on labor supply and wages are reasonable given that employment commitments covered approximately 20% of the East German workforce and the high prevailing unemployment rates. Moreover, this is a period when the wage setting was mainly driven by political considerations rather than market forces (Krueger and Pischke 1995; Hunt 2001), which makes the wage impact of the employment target policies less relevant.

[7] We consider labor as the only factor of production for simplicity. Our results are qualitatively robust to including capital in the production function.

[8] Assuming monopolistic competition would not change the main predictions of the model.

$$\pi(\tilde{z}, l_*) = \begin{cases} \alpha^{\frac{\alpha}{1-\alpha}}(1-\alpha)\tilde{z} & > 0 \quad \text{if } \tilde{z} \in [\tilde{z}_*, \infty) & \text{(Undistorted)} \\ \tilde{z}^{1-\alpha} l_*^{\alpha} - l_* & > 0 \quad \text{if } \tilde{z} \in [\tilde{z}_{**}, \tilde{z}_*) & \text{(Distorted, Bunching)} \\ \left(\frac{\alpha}{1-\gamma}\right)^{\frac{\alpha}{1-\alpha}} (1-\alpha)\tilde{z} - \gamma l_* & > 0 \quad \text{if } \tilde{z} \in [\tilde{z}_{***}, \tilde{z}_{**}) & \text{(Distorted, No Bunching)} \\ \left(\frac{\alpha}{1-\gamma}\right)^{\frac{\alpha}{1-\alpha}} (1-\alpha)\tilde{z} - \gamma l_* & \leq 0 \quad \text{if } \tilde{z} \in [\tilde{z}_{****}, \tilde{z}_{***}) & \text{(Distorted, No Bunching)} \\ \quad exit & \quad\;\; \text{if } \tilde{z} \in (0, \tilde{z}_{****}) & \text{(No Production)} \end{cases} \tag{1}$$

and

$$l(\tilde{z}, l_*) = \begin{cases} \alpha^{\frac{1}{1-\alpha}} \tilde{z} & if\ \tilde{z} \in [\tilde{z}_*, \infty) & (Undistorted) \\ l_* & if\ \tilde{z} \in [\tilde{z}_{**}, \tilde{z}_*) & (Distorted,\ Bunching) \\ \left(\frac{\alpha}{1-\gamma}\right)^{\frac{1}{1-\alpha}} \tilde{z} & if\ \tilde{z} \in [\tilde{z}_{****}, \tilde{z}_{**}) & (Distorted,\ No\ Bunching) \\ 0 & if\ \tilde{z} \in (0, \tilde{z}_{****}) & (No\ Production) \end{cases} \tag{2}$$

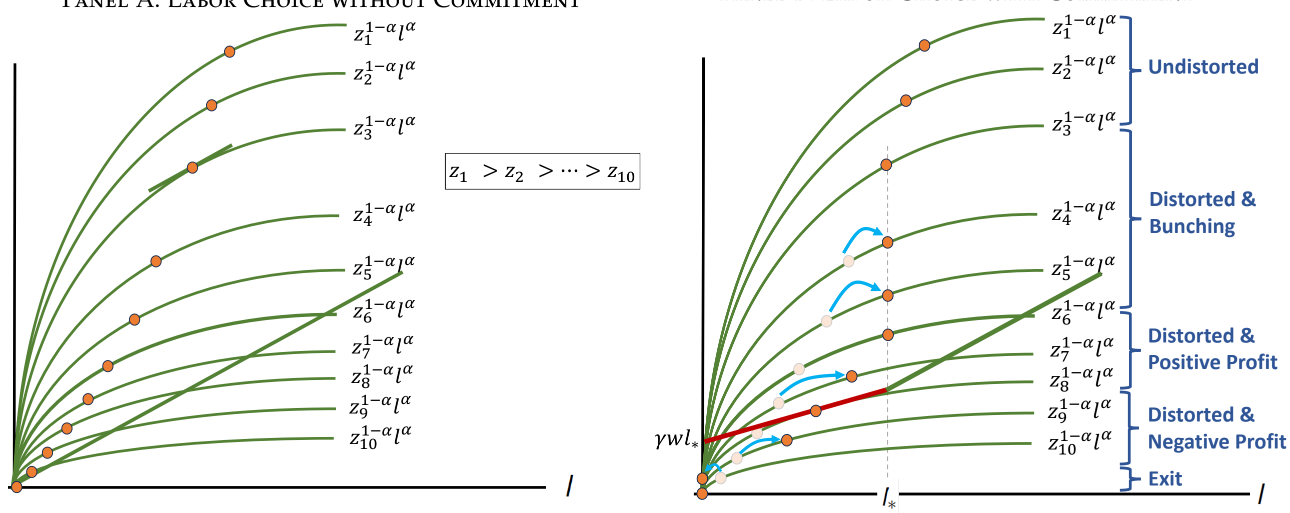
where $\tilde{z} \equiv z/w^{\frac{1}{1-\alpha}}$ is the normalized productivity level with respect to the wage rate and the implied profits are $\Pi(\tilde{z}, l_*) = \pi(\tilde{z}, l_*)w$.

The above lemma suggests that firms' optimal employment choice and the resulting profits are non-decreasing in productivity, $\tilde{z}$, and can be characterized based on five productivity regions as illustrated in Figure 1. Figure 1A illustrates various firms with differing productivity levels, with z_1 being the highest and z_{10} the lowest. Additionally, the orange circles represent the undistorted optimal choices of labor, where the marginal cost of hiring labor (w) equals the marginal product of labor. As economic intuition suggests, more productive firms hire more workers, which is reflected in the rising position of the orange circles as firm productivity increases.

FIGURE 1: THE IMPACT OF COMMITMENT ON LABOR CHOICES

PANEL A: LABOR CHOICE WITHOUT COMMITMENT

PANEL B: LABOR CHOICE WITH COMMITMENT



The above lemma and Figure 1B show that the introduction of employment targets distorts firms' labor decisions and create misallocation of labor across firms by changing the cost structure which can be seen by rewriting the profit function as

$$\Pi(z, l_*) = \max_l \begin{cases} z^{1-\alpha} l^{\alpha} - \underbrace{wl}_{\text{variable cost}} & for\ l \geq l_* \\ \\ z^{1-\alpha} l^{\alpha} - \underbrace{(1-\gamma)wl}_{\text{variable cost}} + \underbrace{\gamma w l_*}_{\text{fixed cost}} & for\ l_* \geq l_j \geq 0 \end{cases} \tag{3}$$

Comparing the second line to the first, two significant differences are evident. First, the introduction of employment targets effectively adds a fixed cost to the cost function. The firm must now pay $\gamma w l_*$ regardless of the number of workers it decides to hire in equilibrium. This acts as if the government imposes a certain fixed cost upfront by making the firm pay a cost proportional to l_*. The second difference is that the policy introduces a "wedge" in the *marginal* cost of labor by reducing the variable cost of hiring l_j workers. Since the government has already covered a γ fraction of the cost as part of the fixed cost, the situation now appears as if the government reimburses γ fraction of the variable costs, effectively making it cheaper by γw. This implies that employment target policies generate heterogeneity in the marginal cost of labor across firms, creating misallocation of labor.

This change is visually represented by the red cost line in Figure 1B. By transforming the linear cost line into a V-shaped cost function with a kink at l_*, the labor commitment categorizes firms into five distinct groups based on their productivity levels. For the first "undistorted" group of highest-productivity firms, where $\tilde{z} > \tilde{z}_*$, the labor choices remain undistorted since the employment target is not binding. Second, firms with high levels of productivity, $\tilde{z} \in [\tilde{z}_{**}, \tilde{z}_*)$, decide to increase their labor due to lower variable costs but also cluster at the target employment, as variable costs revert to their original level beyond l_*. Third, medium-productivity firms, $\tilde{z} \in [\tilde{z}_{***}, \tilde{z}_{**})$, increase their labor thanks to lower variable cost, yet their new optimum remains below l_*. This results in a new interior solution that falls short of the employment target. Fourth, low-productivity firms with $\tilde{z} \in [\tilde{z}_{****}, \tilde{z}_{***})$ cannot achieve positive profit even at their new distorted optimal labor level. Despite making negative profits, these firms continue to operate and hire labor at the new lower marginal cost, hoping to achieve positive profits in the future. This highlights the significance of their dynamic considerations regarding the future of their firm, an important dynamic feature that will be described further in the next section. Finally, the fifth group, consisting of firms with the lowest productivity levels, $\tilde{z} < \tilde{z}_{****}$, finds it optimal not to hire any workers since their productivity and resulting profit levels are too low to sustain the firm, even when future prospects are considered.

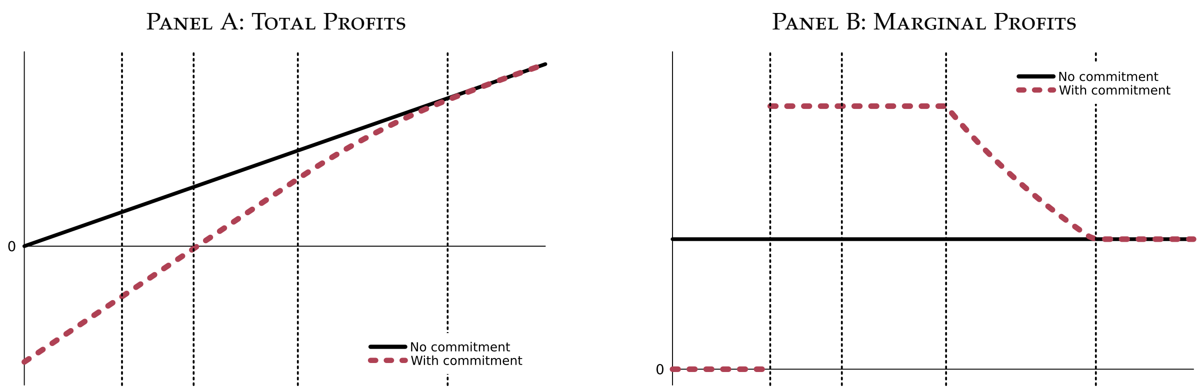
The discussion makes it clear that even though the baseline model lacks a fixed cost and every firm is viable at all times, the introduction of employment targets creates a "polarization" effect. Firms above a certain productivity level increase their labor hiring even when they begin to incur negative profits following the policy implementation. Firms that start making negative profits on the margin choose to remain in the market, hoping that future positive profits will offset their current losses. Conversely, firms with very low productivity levels stop hiring and decide to exit the market, effectively pushing the firms either up or out.

The implications of the employment targets discussed so far primarily arise from the static maximization problem of the firm. In the next section, the dynamic decision of the firm will reveal a new incentive to enhance firm productivity "out of necessity" under the policy, aiming to align employment decisions with the employment targets. Before moving on to these dynamics, it is crucial to discuss the shape of the profit function, which will directly influence the firms' productivity improvement decisions.

Figure 2 illustrates key implications of the existence of employment targets on firm profits. The left panel plots total profits with respect to firm-level productivity. The black line provides the benchmark for firms with no employment targets, while the dashed red line plots profits for firms under employment targets. The plot shows that distorted firms have lower total profits. As Equation 3 clarifies, although the penalty for not meeting the target is proportional to the number of missing employee, it shows up as a fixed cost in the profit function. This cost increases in magnitude as the level of the target rises. This will have important dynamic implications for the exit decision, which will be discussed in the next subsection.

The right panel of Figure 2 plots *marginal* profits across firm productivity and shows that distorted firms have a higher marginal profit with respect to productivity due to the lower variable cost of labor. This reflects the additional benefits the firms get from improving their productivity: it reduces the amount of distortion (if the firm is bunching) or penalty paid (if the firm is not bunching) for those firms with binding employment targets. In a dynamic setting, this implies that the increase in profits from productivity improvements will be higher for distorted firms relative to undistorted ones i.e., distorted firms would be more willing to invest in productivity improvements.

FIGURE 2: PROFITS ACROSS FIRM PRODUCTIVITY



Notes: The left and right panels plots total and marginal profits across firm-level productivity, respectively. The black line provides the benchmark for firms with no commitment, while the dashed red line plots profits for firms under commitment. Dashed vertical lines show the threshold productivity levels, z_* and z_{**}.

2.2 Dynamics

Next, we describe the dynamic decisions of the firms. At any point, the owner decides whether to stay in the economy or exit. If she decides to exit, she needs to pay an exit cost, net of outside option, which we parameterize with C_e.[9] If she stays in the economy, she makes the optimal labor choice, as described above, and decides how much to invest in productivity growth by choosing the Poisson arrival rate of improving the productivity, x, with the following cost function (in terms of the homogeneous good)

$$c(x|\tilde{z}) = \frac{\phi}{2} x^2 \tilde{z} w$$

which is convex in the success probability x, and ϕ is the scale parameter for the cost. This cost function assumes that the higher the current level of productivity, the higher the cost of investment. The particular normalization of the current level of productivity implies that firm growth is consistent with Gibrat's law in the absence of employment targets: the growth rate of sufficiently large firms (high productive firms) is independent of their size.[10] If the investment is successful, the productivity improves from z to $(1+\lambda)z$, where λ is the parameter that controls the step size in productivity improvement. Finally, we assume that the labor commitment contracts expire at the firm level at the rate μ, i.e., the employment target becomes zero and no longer binding.[11]

Given this structure, the dynamic problem of the firm can be represented by the following value function:

$$rV(\tilde{z}, l_*) - \frac{\partial V(\tilde{z}, l_*)}{\partial t} = \max \left\{ -C_e w, \max_{x \geq 0} \left[\begin{array}{c} \pi(\tilde{z}, l_*) w - \frac{\phi}{2} x^2 \tilde{z} w \\ +x \left[V(\tilde{z}(1+\lambda), l_*) - V(\tilde{z}, l_*) \right] \\ +\mu \left[V(\tilde{z}, 0) - V(\tilde{z}, l_*) \right] \end{array} \right] \right\} \tag{4}$$

where $V(\tilde{z}, l_*)$ is the firm value. The outer maximization problem determines the endogenous exit decision of the firm. The value of staying is determined in the second maximization problem where the firm chooses how much to invest on productivity growth.[12] The first line includes the instantaneous profits, minus the cost of investment on productivity. The second line expresses the change in firm value when the firm is successful with its investment in improving productivity at the rate x. The last line represents the change in value when the employment target expires at the rate μ.

The extensive margin choice above gives rise to the standard optimal stopping problem. Firms follow a cutoff rule under which they choose to exit when their productivity falls below a certain threshold. The threshold productivity for exit is higher for firms with higher employment targets due to the associated larger fixed costs. In other words, conditional on initial productivity, firms with higher employment targets would be more likely to exit the economy.

For firms that choose to stay in the economy, optimal level of investment in productivity is given

[9] This cost reflects not only the penalties from missing the target as employment becomes zero upon exiting, but also any other, implicit or explicit, costs due to impaired relations between the acquirer of the firm and the government.

[10] These particular assumptions on cost function makes the firm problem very tractable under no employment targets, but are not essential for our main results. This is because the main channel through which employment targets influence firm decisions is by affecting the returns to investment in productivity improvements rather than the cost of such improvements.

[11] Stochastic expiration of the employment targets is assumed for tractability. Specifically, it allows us to obtain time-independent decision rules for the firms. In practice, firms in our dataset do know about the length of the commitment periods. Assuming deterministic expiration of the contracts would not change our main results qualitatively.

[12] Employment choice was characterized above, so it is taken as given here.

by (the arrival rate of improving productivity):

$$x(\tilde{z}, l_*) = \frac{V(\tilde{z}(1+\lambda), l_*) - V(\tilde{z}, l_*)}{\phi \tilde{z}^{\frac{1}{1-\alpha}}} \tag{5}$$

which depends on the increase in the value of the firm in the case of a successful improvement in productivity. Since the value function inherits the properties of the profit function, the investment rate on productivity mimics the pattern of marginal profits similar to the case illustrated in Panel B of Figure 2: it is higher for firms that are distorted by the binding employment targets, i.e., the "out-of-necessity" effect of productivity improvements. Moreover, the longer the firm operates under the employment target (the smaller the value of μ), the steeper the value of the firm with respect to productivity becomes. This implies that productivity improvement incentives are stronger for longer policy horizons.

2.3 Taking Stock

We finish the model discussion by summarizing the main insights. Our analysis clarifies two channels through which employment targets affect firm decisions. First, employment targets distort static labor decision of the firms and create a "polarization effect": firms with low productivity exit the market due to the new implied fixed cost, while the remaining firms make an upwardly biased employment choice (Equation 2), due to the implied lower variable cost of labor. The latter effect is transitory in nature, with firms reverting to their undistorted employment size once the targets expire.[13] Second, our model suggests a "necessity-driven" improvements in productivity, induced by higher marginal profits. This dynamic effect arises as firms have distorted incentives to align their productivity with the employment target to reduce the cost of the distortion imposed by the employment target. Furthermore, since the level of optimal labor is increasing in the level of productivity, this dynamic effect also contributes to higher employment. Importantly, the employment gains resulting from these dynamic improvements in productivity are persistent.

In the next section, we will empirically test these theoretical implications of the model in the data. In particular, we study the impact of employment targets on (i) employment growth, (ii) exit decision, and (iii) productivity growth at the firm level.

3 Institutional Background

We test the empirical relevance of these mechanisms by leveraging the data of East German firms operating under employment targets after their privatization. In this section, our aim is to outline the key institutional characteristics under which these employment commitments were put in place during the privatization process. To accomplish this, we leverage insights from interviews with practitioners, reports and books about the organizational design of the THA. We also exploit information

[13]For simplicity, our model abstracts from labor adjustment costs. Incorporating such costs would not affect the transitory nature of this channel but would imply some transition phase occurring when the commitment expires.

from the THA archives consisting of official handbooks distributed to THA employees for organizational and sales purposes. The main takeaways are that: i) the inclusion of employment commitments in sales contracts was pervasive, ii) they were negotiated with considerable autonomy by privatizers, and iii) enforced through reporting obligations and penalties.

Creation and Organization of the Treuhandanstalt Established in March 1990 under the last Communist regime in East Germany, the THA was given greater authority in July 1990 through the *Treuhandgesetz*.[14] Tasked with managing and privatizing the companies that had previously been owned by the state of East Germany, the THA became the largest holding company in the world, overseeing a portfolio of around 12,000 companies and employing approximately 4.5 million people, which made up about 50% of the total workforce population. The THA officially commenced its duties on July 1, 1990.

The THA management inherited a diverse and often disjointed portfolio of activities structured within large centrally planned conglomerates (*Kombinate*). The initial step taken towards transforming these companies involved the splitting up of these large conglomerates into firms organized under private law (*Entflechtung*). In a second step, the THA required these enterprises to submit an opening balance sheet in Deutsche Mark (*Eröffnungsbilanz*) and a business plan for review. The privatization process further streamlined business activities, as firms divested through asset sales.

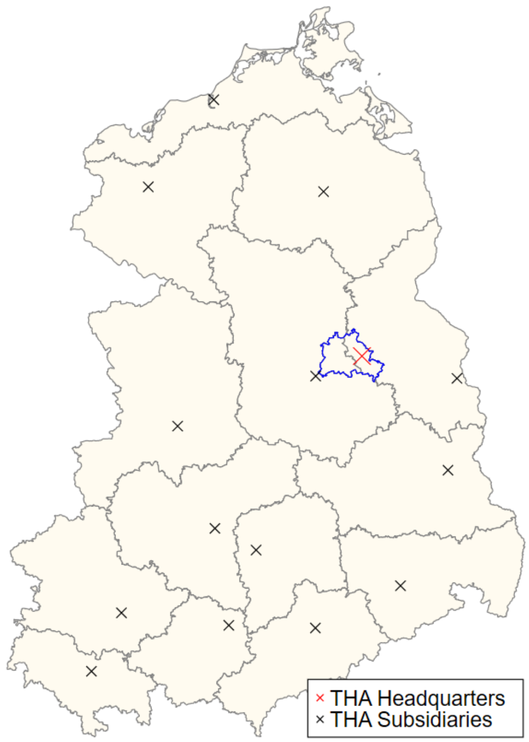
The THA itself built up rapidly from an initial staff of about 200 mostly East German employees to an institution of around 4,000 employees plus 800 full-time consultants. These people were divided approximately equally between the central office in Berlin and the 15 branch offices distributed among the major cities of the new federal states. Figure 3 represents the former German Democratic Republic (GDR) districts and the location of THA branch offices. Smaller firms with fewer than 1,500 employees were assigned to local branch offices, while larger firms were assigned to industry-based divisions in the Berlin headquarters. In particular, the THA headquarters organized firms with more than 1,500 employees or with revenue or balance sheet values above 1.5 million Deutsche Mark (DM).[15]

The Privatization Process The THA utilized cash sales to privatize assets, which included both privatizations of entire firms and divestitures/spin-offs resulting from firm restructuring and liquidation. The standard procedure of privatization was through direct sales to interested acquirers. Structured auctions happened more on an exceptional basis (Fischer, Hax, and Schneider 1993b). Sales contracts were structured to include the sales price as well as guarantees made by purchasers regarding minimum levels of employment and investment. In practice, the goal of the sales process was not necessarily to maximize the sales price but rather to ensure employment levels at the privatized plant. As stated by Dodds and Wächter (1993) "the urgency of the Treuhand's need to maintain and create jobs cannot be overestimated."

[14]Gesetz zur Privatisierung und Reorganisation des volkseigenen Vermögens of June 17, 1990.

[15]Exceptions from the cutoff rules are (i) if the total sum of firm subsidiaries is above 1,500 employees and (ii) if the firm belongs to one of the following sectors: foreign commerce business, financial institutions, printing and newspaper, DEFA, hotels and travel agencies, circuses, water and sewage, energy and mining, transportation.

FIGURE 3: THA HEADQUARTERS AND SUBSIDIARIES



Notes: The figure shows the location of the THA headquarters and local subsidiaries across East Germany. Including East Berlin, the former GDR consisted of 15 district. Each district possesses a local THA office. The headquarters is located in East Berlin, which is indicated by the red cross.

Within this process, the role and freedom assigned to the privatizer was considerable. In the official handbooks used by THA employees it is explicitly stated that the sales process is to be steered by the privatizer and that the weighting of the components of each bid was not subject to a rigid formula but rather left "to the acumen of the privatizer."[16] The delegation of responsibilities to the privatizers was natural given the breakneck speed of the sales process and the organizational challenges. The THA was described as “an adolescent bureaucracy, born of chaos and destined to be phased out without ever functioning normally. It is a human creation, whipped together quickly and then put under extreme pressure without time to prepare” (Dodds and Wächter 1993).[17]

Employment commitments The urgency to maintain and create jobs in the former GDR was reflected in THA’s emphasis on inserting employment commitments into the sales contract. This mirrored the obligation placed on the THA to take account of the social costs of unemployment. Following the reunification, East Germany experienced a sharp increase in the unemployment rate, reaching 10.2% by 1991 and further rising to 15.7% in 1994, which led to significant social unrest. In April

[16]More specifically the *“Generally binding principles that prescribe how each individual case should be assessed are not intended to be specified. In this core area, your (the privatizer) knowledge and negotiating skills are primarily required.”*

[17]The agency officially terminated its operation at the end of 1994 and its mission was taken over by a successor agency entitled *Bundesanstalt für vereinigungsbedingte Sonderaufgaben* (Böick 2018).

1991, the first president of THA, Detlev K. Rohwedder, was assassinated. Similarly, after the THA decided to close the former VEB Kaliwerk Bischofferode, employees went on an 81-day-long hunger strike (Bernhard 2011).

The employment commitment consisted of an agreed number of full-time equivalent jobs that should be maintained. While such commitments could result in discounts on the sales price, the valuation process varied for each case, and there was no fixed formula followed by the THA. The commitment was specific to the acquired establishment and could not be fulfilled by employing individuals in other establishments of the acquirer (Siebert 1991; Fischer, Hax, and Schneider 1993a). Similarly, these commitments constituted corporate contracts and therefore continued to be valid in the event of the sale of the company.

In terms of monitoring, the contract also established the obligation of the acquirer to provide information for the audit of the commitment. The contract managers would approach the contracting party and conduct audits either through physical visits to the firm or via documentation requests. Importantly, the THA office responsible for auditing the commitment (Vertragsabwicklung) was distinct from the operational offices that organized the sale.

To ensure enforceability, penalty clauses were included in the contracts, stipulating payments to the THA if the agreed-upon employment levels were not met. The penalties were designed to approximate the cost of retaining an employee and were proportional to the shortfall in employment and prevailing industry wages. The penalty per missed employee was also proportional to the time that the commitment was violated (*pro rata temporis* condition). This means that, for example, if a firm is missing continuously one employee over the course of three years, the firm misses, in total, three commitments and needs to pay three times the per missed employee penalty. Note that the effective penalty could be lower because of "conditions beyond the purchaser's control," renegotiation, minor violations of targets (*Bagatellfall*), and judicial decisions.

4 Data and Descriptive Statistics

To test the model predictions regarding the impact of employment commitments on firm dynamics, we rely on a unique dataset from the German Federal Archives (*Bundesarchiv*) containing all contracts and documentation produced by the THA. These data are confidential, and, thanks to an institutional cooperation with the IWH Institute, we are among the first to gain access to them. Importantly, the agency digitally recorded all contracts for monitoring and enforcement purposes in more than 500 data tables (*ISUD System*). These tables provide comprehensive contract-level information on the privatization of assets including all the employment commitments that have been agreed upon as well as dates and audits associated with each commitment. To be able to measure post-privatization dynamics, we combine this contract-level data with information from the Mannheim Enterprise Panel (MUP) and a firm survey called SOESTRA.[18]

[18]Thanks to the ownership information contained in the MUP data, we are able to track contracts after the privatization year. Appendix Section B provides a detailed description of the explored ISUD data tables. For a detailed description of the data merges, please refer to Supplementary Appendices C and D.2.

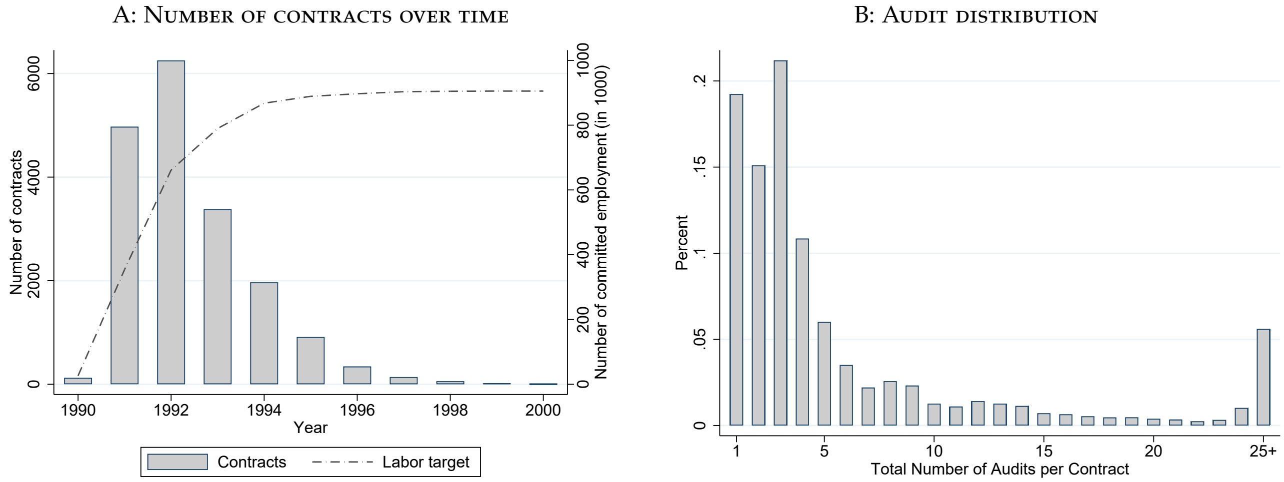
4.1 Contract Data

In total, the ISUD dataset contains 18,235 contracts with labor commitments that amount to more than 900,000 committed workers and represent about 20% of the initial workforce population of THA firms. Figure 4 provides evidence on the aggregate importance of employment commitments, their timing, and the associated monitoring by THA.

Panel A of Figure 4 shows that 90% of employment commitments are signed between 1991 and 1994.[19] Panel B of Figure 4 shows that these contracts underwent regular audits conducted by contract managers employed by the THA. On average, each contract was audited 6.3 times, with a minimum of 1 audit and a maximum of 84 audits. About 82% of all contracts were audited at least twice.

Table 1 provides descriptive statistics for the 14,726 privatization contracts featuring an employment commitment with at least two audits. Panel A of Table 1 shows contract-level employment information at the start date of the contract, the final level of employment, and the target level of employment. With, on average 66 employees, firms had been relatively sizable at the onset of privatization. Over the course of the commitment period of, on average, three years, firms decreased in size.

Figure 4: Contracts and Labor Audits



Notes: Panel A plots the total number of signed contracts with labor commitments between 1990 and 2000 as well as the accumulated number of commitment employment. Panel B plots the distribution of labor audits per contract.

Panel B relates the initial size of the firm to the final target. The fraction of firms initially below their target is 22%, while about 20% of the firms receive a target that is equal to their initial size.[20] In Panel C, we observe for a subset of 1,272 firms the total amount of penalties claimed by the THA due

[19] Less than 2% of all labor contracts are written out in 1996 or later (15 contracts are observed in 2002). For 168 contracts we do not observe the date of the contract.

[20] In principle, the contract could stipulate multiple employment targets for different deadlines over the course of the commitment period. However, in more than 80% of cases the employment target is constant.

Table 1: Summary Statistics

	N	Mean	SD	Minimum	Maximum
	(1)	(2)	(3)	(4)	(5)
A: Average firm size					
Initial employment	14,726	66.20	319.57	0.00	23,691
Final employment	14,726	60.67	194.26	0.00	8540
Final employment target	14,726	52.98	183.25	1.00	6906
B: Initial size relative to target					
Fraction initially below target	14,726	0.22	0.42	0.00	1.00
Fraction initially at target	14,726	0.20	0.40	0.00	1.00
C: Penalties					
Number of observed violation	1,272	2.24	1.29	1.00	12.00
Total number of violated labor	1,272	111.58	393.22	0.24	8,567.47
Penalty per missed employee (in 1000 EUR)	1,272	10.77	10.67	0.10	58.52
D: Productivity					
Initial productivity	3,387	9.99	1.43	3.51	16.27
Final productivity	3,584	12.02	0.786	10.46	14.81
Initial TFP	3,118	6.81	1.23	2.70	9.79
Final TFP	2,219	7.32	1.08	3.53	10.31
E: Market exit					
Exit until final commitment year	4,622	0.055	0.22	0.00	1.00

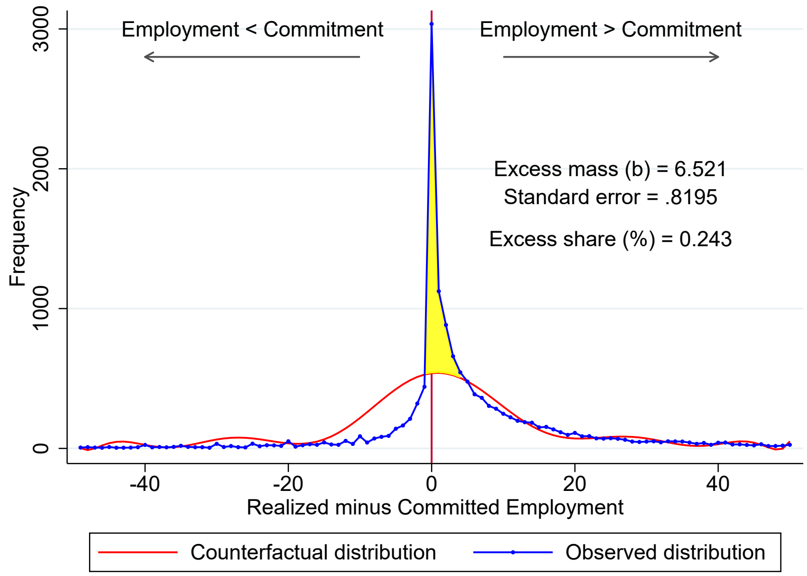
Notes: The table shows summary statistics of privatization contracts with at least two audit observations. Panel A provides information on the initial and final level of employment and the committed level of employment at the end of the commitment period. Initial employment is measured using the first audit. Final employment is measured as the last audit at the end of the contract period. Panel B shows the share of firms with an initial employment level being below and at the committed target level. Both variables are indicator functions. Initially below the target is measured as $\mathbb{1}(empl_{initial} < empl_{target})$. Initially at the target is measured as $\mathbb{1}(empl_{initial} = empl_{target})$. Panel C provides information on 1,272 contracts with at least one observed labor commitment violation. Panels D and E are based on the linkage between contracts and external firm-level data described in Appendices C and D. Panel D provides model-consistent productivity and TFP measured in logs. Panel E shows the exit indicator at the end of the labor commitment period.

to violations of labor commitments as well as the total number of violations. Conditional on having at least one labor violation, the average firm deviates 2.2 times. The cumulative number of missed employment over multiple violations corresponds, on average, to 111 workers.[21] Finally, consistent with documentation on THA policy, our calculations suggest that the average stipulated penalty per missed employee amounts to 10,768 EUR.

Figure 5 empirically shows the importance of employment targets in distorting firm's labor choices. The bunching patterns constitute the first evidence in line with the predictions of the model. The horizontal axis measures the difference between the realized employment measured at the last audit of a commitment and the final employment target. Firms below 0 are smaller in terms of their realized employment relative to the committed level, whereas firms above 0 have a larger employment with respect to their committed level. The figure plots the bin counts around the normalized target shown by the red vertical line at zero, with each bin representing a unit of employment devi-

[21] The maximum cumulative missed employment amounts to 8,567 workers. This is above the maximum of the final employment target in Panel A, as there can be multiple violations. In addition, the maximum number in Panel A corresponds to the final commitment level.

Figure 5: Employment Distribution around the Commitment Level



Notes: The figure shows the employment distribution around the committed employment (demarcated by the vertical red line at 0) for contracts between 1990-1995. The blue line in dots is a histogram of actual employment relative to the commitment target in the final commitment year. Each point shows the number of observations in employment count bins (deviation between the target and the realized employment). The solid line beneath the empirical distribution is a twelve-degree polynomial fitted to the empirical distribution excluding the area of missing one employee and having three employees more than committed. The shaded region in yellow is the estimated excess mass, which is 652% of the average height of the counterfactual distribution beneath. Standard error is calculated using a parametric bootstrap procedure. Estimation based on Chetty, Friedman, Olsen, and Pistaferri (2011).

ation. A striking feature of the data is the large spike exactly at the committed level of employment, suggesting the importance of these constraints for firms' labor choices. Following Chetty, Friedman, Olsen, and Pistaferri (2011), we estimate an excess mass around the threshold of 652% relative to the average height of the counterfactual distribution.[22]

We next provide in Figure 6 descriptive evidence on the distribution of employment growth rates over the course of the contract period. We approximate the initial employment levels using the first labor audit, which typically took place between three to six months after the contract was signed with the notary. Importantly, this contract level approach to measuring employment levels allows us to mitigate measurement error due to the importance of piece-wise asset sales during the privatization process.[23]

In the left panel, the dashed line plots the distribution of growth rates for the entire sample. On average, firm employment grows by 6% between the initial and final audits. In addition, the panel

[22] The red line in Figure 5 plots the estimated counterfactual density based on a twelve-degree polynomial (p = 12) and an asymmetric window around the threshold $R = [3, -1]$. $R = [3, -1]$ denotes the omitted bunching range including firms having up to three more employees than their committed target. The yellow shaded region depicts the estimated excess mass around the threshold. Figures A.5 to A.7 provide robustness checks with respect to the degree of the polynomial, the bunching window, and the bin definition. Table A.10 shows the results by sub-samples.

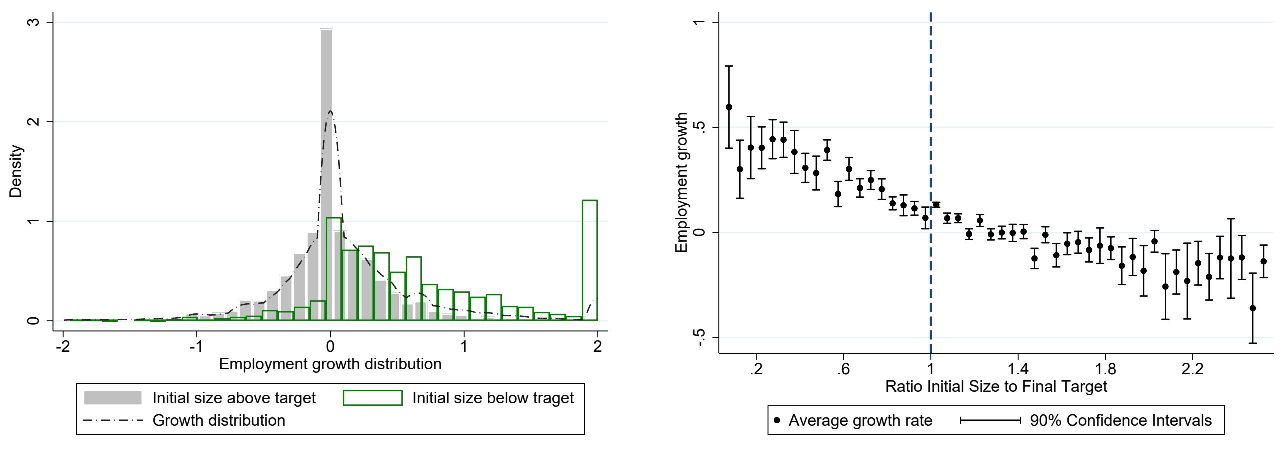
[23] More specifically, we follow Davis and Haltiwanger (1999) and construct the firm-level growth rate between the first audit and the final audit of the commitment as $(L_{iFinal} - L_{iInitial})/0.5(L_{iFinal} + L_{iInitial})$, where L_i denotes the level of employment of firm i. Subscript *Final* refers to the date of the final commitment audit and, *Initial* refers to the date of the first commitment audit.

distinguishes between firms according to their initial size and final target. Firms initially at or above their committed target (grey bars) shrink, on average, by -6.8%, whereas firms initially below their target (empty bars) grow, on average, by 54%. The right panel of Figure 6 provides the full distribution of employment growth according to the ratio of initial size over final employment target. A striking negative relationship emerges between the distance to the final target and subsequent employment growth. Firms that have high targets relative to their initial size grow their workforce significantly more than firms with targets close to their initial size. Firms with lax targets relative to their initial employment had leeway to adjust and subsequently shrunk significantly. Overall, the figure suggests the importance of firm employment targets as a determinant of firm employment policies.

FIGURE 6: EMPLOYMENT DYNAMICS BY INITIAL SIZE TO TARGET

A: EMPLOYMENT GROWTH

B: EMPLOYMENT GROWTH BY INITIAL SIZE TO TARGET



Notes: Panel A shows the overall employment growth distribution as well as the employment growth distribution distinguishing by firms initially below or above (including firms initially at their target) their commitment employment level. Panel B shows average growth rates by the distance of the initial size to the final target.

4.2 Firm Data on Exit and Productivity

To construct market exit and productivity measures for privatized firms we rely on data from the Mannheim Enterprise Panel (MUP). The data stems from *Creditreform e.V.*, the largest credit rating agency in Germany, and provides firm-level information for the population of East German companies since 1993 (Bersch, Gottschalk, Müller, and Niefert 2014).[24] To connect THA contracts to post-privatization outcomes, we merge the contract partners' names with the ownership information in the MUP. For a detailed description of the data merge between the two datasets, please refer to Supplementary Appendix C.

[24]Bersch, Gottschalk, Müller, and Niefert (2014) provide detailed information on data collection, processing and the definition of variables. We use wave 56 of the data with the latest available year being 2019.

We compute two measures of productivity growth for firms under employment commitments. First, we consider a model-consistent productivity measure given by sales per worker adjusted by the labor share in the production function.[25] To assess initial productivity, we use information from the opening balance sheets of THA firms with contract data regarding employment at the time of privatization. To measure productivity at the end of the employment commitment, we merge the sales information from MUP with the final employment audit. Second, we use information on investment contained in the SOESTRA survey of THA firms to calculate firm-level Total Factor Productivity (TFP) as described in Appendix D.

Panel D of Table 1 reveals a substantial increase in productivity during the commitment period. This productivity improvement aligns with the documented convergence process observed in the years following reunification. Within the first decade after reunification, approximately half of the measured labor productivity gap and over one-third of the GDP per capita gap between East and West have been closed (Burda 2006).[26] Starting in 1990, our calculation suggests an increase in productivity of 2 log points. The calculated improvements in TFP between the initial contract year and the final commitment period amounts to 0.51 log points. Finally, Panel E of Table 1 provides information on market exit for the matched contracts with the MUP. The sample size for this analysis is larger compared to the productivity assessment, because of missing data in terms of the sales variable. Over the commitment period, which spans an average of 3.3 years, we observe an exit share of 5.5%.

5 Empirical Analysis of Labor Commitments and Firm Dynamics

We now turn to the empirical framework for testing the predictions derived from the dynamic firm model in Section 2. We outline how we leverage the unique contract data presented in Section 4 to construct an instrumental variable based on the exogenous allocation process of privatizers with heterogeneous labor preferences. Sections 5.2, 5.3, and 5.4 provide the empirical results for employment growth, exit, and productivity growth, respectively.

5.1 Identification Strategy

Addressing the empirical challenge of non-random allocation of labor commitments is crucial when analyzing subsequent firm-level responses. For example, if high labor targets are assigned to low-growth firms, it may lead to an underestimation of the impact of employment commitments. To tackle this issue, we develop a framework for reduced-form identification inspired by methods used in studies on judge leniency (Bhuller, Dahl, Løken, and Mogstad 2020; Dobbie and Song 2015; Bernstein, Colonnelli, Giroud, and Iverson 2019) and patent evaluators (Sampat and Williams 2019). These studies typically estimate the fixed traits or preferences of decision makers – refered to as leniency or

[25] Our measure of model-consistent measure of productivity is given by $sales/employment^{\alpha}$. In the baseline analysis, we set $\alpha = 0.8$, consistent with the aggregate labor share during this period. For robustness we provide our results with different values of α in the Appendix.

[26] Based on aggregate statistics, Bachmann, Bayer, Stüber, and Wellschmied (2022) show that GDP per worker increased by about 0.7 log points between 1991 and 2000.

toughness/stringency – regarding outcomes under their control. By combining this estimate of the fixed trait with the quasi-random allocation of decision makers, we obtain an exogenous shifter that helps mitigate potential biases.

The proposed empirical framework for the analysis is well-suited to our institutional setting for several reasons. The number of privatizations in those years meant that THA agents typically worked on multiple cases. Importantly, the breakneck speed of privatizations generated, within offices, randomness in the assignment of these cases. A consultant with the THA in those years described the process as "an exceptional situation where there was a lot of improvisation." Finally, at the moment of privatization, each THA agent possessed significant discretion in establishing the conditions for the firm to be privatized, thus leaving room for privatizer traits to matter in the process. In our data, we observe the name of the privatizer for 11,194 signed contracts. These contracts are handled by 1,659 different individuals with an average of 6.7 cases per privatizer. Figure A.1 in Appendix A shows the distribution of cases per privatizer. We condition our baseline sample on having at least five privatizations per privatizer. This generates a final sample of 9,363 privatizations as our baseline.[27]

Instrument Construction The first step in the empirical analysis is to construct a measure for privatizers' stringency in assigning labor commitments. Our measure is the average propensity of privatizers to require binding labor commitments, i.e., labor targets that force the firm to grow.

The construction of the instrument must address the own-observation issue that arises due to a mechanical correlation in finite samples between the ongoing privatization case and the estimation of the privatizer's preference. We follow the literature by estimating a leave-one-out measure of binding commitment, i.e., by removing the considered privatization case from the estimation of the stringency measure. The measure of privatizer stringency must also correct for the fact that THA offices differed in the underlying assets for sale. To address this concern, we normalize privatizer preferences within offices. In other words, we measure a privatizer's stringency on labor commitments relative to the average stringency of cases handled in that office. We thus obtain:

$$Z_{ioj}^{empl} = \frac{1}{n_{oj}-1}\left(\sum_{k=1}^{n_{oj}}(Binding_k) - Binding_i\right) - \frac{1}{n_o-1}\left(\sum_{k=1}^{n_o}(Binding_k) - Binding_i\right),$$

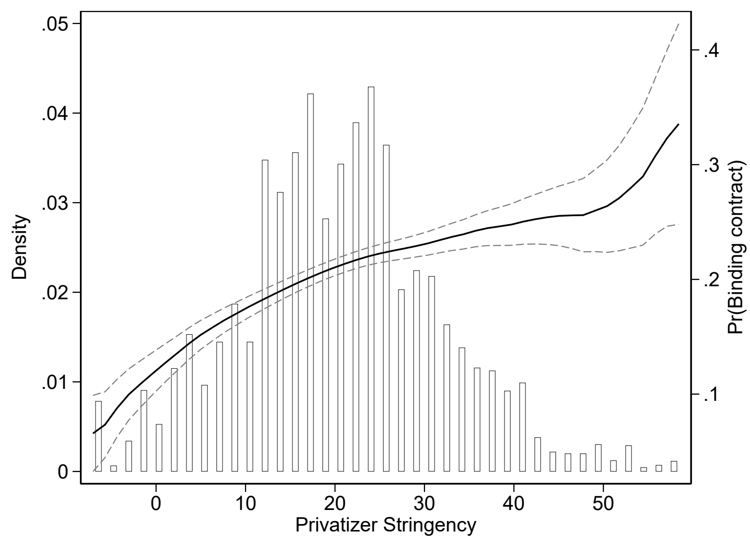
where i denotes the firm, o the THA office, and j the assigned privatizer.[28] $Binding_i$ is an indicator variable equal to 1 if initial employment is smaller than the final employment target, i.e., the firm is constrained to grow. n_{oj} is the number of cases handled by the privatizer in THA office o, and n_o is the number of cases handled by the local THA office. Note that the second term in the formula normalizes the measure by taking into account the average office propensity of writing out binding commitments. Z_{ioj}^{empl}, therefore, measures the leave-one-out measure of binding labor requirements of privatizer j assigned to firm i.

[27] We provide robustness estimates using 10 cases per privatizer in Appendix A.

[28] We observe the assignment of firms to THA offices and the assignment of contracts to firms. For a subset of privatizers we observe several offices. In these cases, we estimate the mode within each privatizer to assign a THA office, and, if the mode is a draw, we assign the respective headquarters.

Figure 7 plots the relationship between binding labor commitments and the estimated privatizer preferences. The density plot is accompanied by a local linear regression highlighting considerable variation in how privatizers impose labor commitments. The probability of receiving a binding contract increases continuously along the stringency measure e.g., moving from the lowest decile to the highest decile increases the probability of assigning a binding contract by 21% points.[29]

Figure 7: First-stage analysis



Notes: The figure plots the probability of having a binding contract (initial firm size < final committed size) against the leave-one-out mean privatizer stringency ($\times$ 100) on the right y-axis. The plotted solid line corresponds to a local linear regression of binding contracts on the privatizer stringency. The two dashed lines show the corresponding 95% CI. All plotted values in the local linear regression are mean-standardized residuals from regressions on THA subsidiary times year of privatization fixed effects. The histogram shows the density of privatizer stringency (left y-axis). The figure is constructed by conditioning of having handled at least five privatization contracts and excludes top and bottom 1% of the stringency measure. Total number of contracts is 9,363.

Random Assignment In a second step we show that labor preferences are uncorrelated to ex-ante characteristics of the assets to be sold. The test exploits pre-assignment information on 12,500 firms that submitted their opening balance sheets in July 1990. We link the labor contracts to these firms and test whether pre-privatization firm-level characteristics correlate with our measure of privatizer stringency. In column (1) of Table 2 each coefficient represents a regression, with the independent variable being our measure of labor preferences (conditional on fully interacted year and office fixed effects). Column (2) provides p-values with two-way clustered standard errors at the privatizer and local office level. Finally, we provide adjusted p-values for multiple testing using the procedure proposed by Romano and Wolf (2005a) and Romano and Wolf (2005b) with 1,000 bootstrap replications.

[29]Figure A.2 shows that preferences for binding labor commitments are consistent within the individual privatizer, i.e., the correlation coefficient between the leave-one-out measure in the previous case and the leave-one-out measure of the current case (the order of the cases is defined by the date the contract is signed) is 0.91.

Estimates in Table 2 provide strong evidence that cases are randomly assigned to privatizers in our sample. Consistent with the institutional evidence, the assignment of privatizers with preferences for stringent labor targets is uncorrelated to i) firm characteristics, ii) privatizer characteristics, iii) buyer characteristics. For example, the results indicate that a 1% point increase in labor preferences is associated with an insignificant 0.1% increase in production workers. Similarly, we find no economically or statistically significant correlation of our instrument with a wide range of employment and revenue measures, including initial labor productivity. In terms of sectoral affiliation, only 2 out of 16 coefficients are individually significant at the 5% level. Adjusting for multiple testing, however, shows no statistically significant relationship between the privatizer stringency measure and sector affiliation.

The data also includes individual-level characteristics of the privatizer. We leverage this information to examine whether the stringency measure predicts the number of cases, the gender of the privatizer, and whether they hold a PhD degree. If there is systematic variation in the stringency measure based on academic qualifications or the ability to handle privatizations, as reflected in the number of cases, it could suggest that decisions regarding labor commitments are influenced by heterogeneity in skills rather than preferences (Chan, Gentzkow, and Yu 2022). However, our findings, presented in the table, indicate that privatizer characteristics do not have any predictive power over our instrument. This suggests that heterogeneity in decision-making stems from preferences for strict labor contracts.

The next two rows of the table present regression results of our stringency measure on the probability of renegotiating contract conditions. Firms were able to renegotiate if they failed to meet their committed targets, and this might lead to an effective reduction in the stringency of contracts. However, the table demonstrates that the instrument is not correlated with future renegotiations. This is consistent with the fact that the organization of privatizations and contract management were handled by different units within the THA.

Finally, in Table A.1 we investigate whether labor preferences of privatizers can influence buyer selection. To check whether this is the case, we link investor names from the contract data with external MUP data via record linkage. This allows us to obtain investor level information relative to size, credit rating, location (East/West), and industry. After the record linkage, we observe investor characteristics for 4,993 privatization contracts. Similar to Table 2, Table A.1 shows the correlation between our instrumental variable and investor characteristics. Again, we fail to observe any systematic evidence for buyer selection.[30]

IV Model The final step of the analysis is to embed this instrument into a 2SLS equation relating THA's employment targets to the main outcome variables predicted by our model: employment

[30] Our instrumental variable estimates rely not only on the validity of the exclusion restriction but also on the accompanying monotonicity condition. We discuss and provide empirical evidence that supports the validity of the monotonicity assumption in Appendix A.

Table 2: Test of Random Assignment of Firms/Contracts to Privatizers

	Indep. variable: Stringency			Dep. variables	
	Coefficient (1)	p-value (2)	Adj. p-value (3)	Mean (4)	Standard deviation (5)
Employment					
Accounting	-0.0039	0.2776	0.9830	2.2540	1.4640
Purchasing	0.0019	0.6773	1.0000	1.6380	1.5600
HR	-0.0003	0.9397	1.0000	1.8840	1.7680
Production	-0.0010	0.8725	1.0000	4.4340	2.4600
R&D	0.0004	0.8956	1.0000	1.2500	1.8300
Sales	-0.0012	0.8321	1.0000	2.2520	1.8720
Administration	-0.0039	0.3806	0.9970	3.2680	1.8720
Firm size above 2000	0.0005	0.4863	0.9990	0.1060	0.3100
Revenue					
Revenue	-0.0110	0.1926	0.9211	8.0840	3.3820
Revenue upper 80p	-0.0007	0.4295	0.9980	0.1920	0.3940
Share of revenue West Europe	0.0007	0.1548	1.0000	0.2560	0.4360
Productivity					
Labor productivity	-0.0013	0.5481	1.0000	3.3440	1.2640
Productivity upper 80p	-0.0006	0.3947	1.0000	0.1960	0.3960
Sector affiliation					
Agriculture, forestry, fishing	-0.0001	0.6859	1.0000	0.0140	0.1140
Energy and water	0.0000	0.9144	1.0000	0.0160	0.1280
Mining and quarrying	0.0001	0.4999	1.0000	0.0080	0.0840
Chemical industry and petroleum	0.0005	0.1356	0.9970	0.0480	0.2160
Plastics and rubber	0.0001	0.8000	1.0000	0.0100	0.1040
Extraction of cut-stone and sand	0.0001	0.7342	1.0000	0.0240	0.1560
Iron, casting, steel forming	0.0000	0.9654	1.0000	0.0240	0.1520
Steel construction, mechanical engineering	0.0021	0.0207	0.4635	0.1660	0.3720
Electrical engineering, optics	0.0008	0.2174	0.9600	0.0680	0.2520
Wood, paper, print industry	0.0000	0.9417	1.0000	0.0440	0.2040
Textile and clothing	0.0001	0.7956	1.0000	0.0580	0.2340
Food and beverage industry	-0.0006	0.1089	0.9770	0.0460	0.2100
Construction and buildings trades	-0.0008	0.2166	0.9311	0.0580	0.2320
Wholesale and foreign trade	-0.0003	0.5554	1.0000	0.0580	0.2320
Retail trade	-0.0007	0.2162	0.8821	0.0340	0.1780
Service	-0.0006	0.0419	0.9311	0.0380	0.1900
Privatizer characteristics					
Number of cases	0.1280	0.2705	0.4635	30.2820	25.1260
Gender	-0.0001	0.9338	1.0000	0.8640	0.3420
PhD degree	0.0009	0.4499	0.9970	0.2660	0.4420
Renegotiation attempt					
Labor renegotiation	-0.0002	0.4142	1.0000	0.0640	0.2440
Any renegotiation	-0.0009	0.1788	0.9980	0.3820	0.4860

Notes: The sample is based on 7,152 contracts with employment, revenue and sector information at the year of reunification 1990. Employment, revenue, and productivity is measure in logs. All explanatory variables refer to the THA initial firm. Each line represents a single regression of the explanatory variable on the stringency measure that takes values between 0 (minimum) and 100 (maximum) controlling for THA office and year of privatization fixed effects. Standard errors are two-way clustered at privatizer and THA office level. p-values in column (2) correspond to the regression model and are two-way clustered at the privatizer and THA office level. p-values in column (3) adjust for multiple testing using Romano-Wolf procedure (Romano and Wolf 2005a; Romano and Wolf 2005b) with 1,000 bootstrap replications. *p<0.1, **p<0.05, ***p<0.01.

growth, productivity growth, and exit. The regression model can be written as:

$$y_i = \beta \mathbb{1}(Binding_i) + X_i'\theta + \epsilon_i, \tag{6}$$

with y_i, for example, indicating the growth rate of employment between the first and the final audits. X_i includes log initial employment measured at the first audit to account for pure size effects of the privatized firms as well as industry dummies. The empirical model further includes the number of months between the time the contract is signed and the first audit, as well as the number of months between the first and final audits to capture differences in commitment length across contracts. The parameter of interest is β, which measures the effect of assigning a binding contract on firm dynamics.

Our research design exploits the quasi-random assignment of cases to THA privatizers with different preferences for binding labor commitments. We specify our first-stage equation for binding labor targets, $Binding_i$, as:

$$Binding_i = \gamma Z_{i(j)}^{empl} + X_i' \lambda + \kappa_i \tag{7}$$

One empirical challenge to our identification strategy relates to the fact that privatization decisions are multidimensional as privatizers also decide on sales price, investment commitments, and financial support. A similar situation arises in the literature on judge decisions as they may not only decide on incarceration but also fines, community service, probation, or guilt. We examine this threat to the exclusion restriction, by following Bhuller, Dahl, Løken, and Mogstad (2020), and augmenting our baseline model by including privatizer preferences in these other contract dimensions. That is, if $Z_{i(j)}^{empl}$ denotes the privatizer stringency instrument for the assignment of a binding labor commitment, we construct and include preference measures $Z_{i(j)}^{price}$, $Z_{i(j)}^{investment}$, $Z_{i(j)}^{financial}$ respectively for sales prices, investment commitments, and financial support.

A second challenge in estimating the impact of labor commitments on employment and productivity growth is related to the role of firm exit. We address the concerns related to sample selection in several ways. First, we implement a Heckman correction procedure and estimate a selection equation predicting the likelihood of firm exit by the end of the commitment period. Second, we keep exiting firms in our estimation sample and assign an employment and productivity growth rate of -2 if the firm exited by the end of the commitment period. Finally, in all specifications we normalize employment and productivity growth by controlling for initial values of these variables.

5.2 Labor Commitments and Firm Employment Growth

Table 3 presents results on the labor distortions induced by employment commitments. The specifications analyze labor growth dynamics throughout the commitment period. Columns (1) to (3) provide OLS estimates starting from the baseline specification and sequentially adding control variables for industry and privatizer characteristics. Columns (4) to (9) provide IV estimates. Standard errors are two-way clustered at the privatizer and office level.

Columns (1) to (3) confirm model predictions suggesting that firms under binding labor commitments upwardly distort their employment decisions. Conditional on the set of baseline controls, industry dummies, and privatizer characteristics, the association between binding contracts and employment growth is on average 52% points until the final commitment date. At a yearly level, the point estimate corresponds to about 13% points differential growth.[31]

IV estimates starting in column (4) support the causal interpretation of the impact of the policy on firm employment growth. In these specifications, following the random assignment of a binding labor contract, firms are estimated to grow their workforce by approximately 25% points annually during the years under commitment. The effect is not only economically large but also precisely estimated at the 1% significance level. The direction of the OLS bias suggests that, consistent with the institutional mandate of the THA, binding labor targets were allocated more frequently to low-growth firms.[32] In line with the previous evidence, the first-stage statistics for weak instruments is large (Panel B). The Kleinbergen-Paap F-tests reject the hypothesis of weak instruments with statistics ranging between 11 and 18. Economically, the first-stage estimates imply that a 10% increase in labor preferences of privatizers result in a 2% points higher likelihood of a binding labor contract. In line with the quasi-random assignment mechanism, the inclusion of additional covariates does not affect the first-stage coefficients.

The results are robust to a series of alternative specifications. Starting in column (5) we consider whether our IV estimates are affected by the inclusion of privatizer preferences on other contract dimensions such as prices, investment commitments and financial support. Consistent with the exclusion restriction, our estimates are unaffected. Columns (6) and (7) consider whether survivorship bias could account for the impact of employment commitments on firm employment. Column (6) specifies a Heckman selection equation and includes the inverse mills ratio into the 2SLS estimation.[33] In column (7), we assign a -2 growth rate for firms that exit by the end of the commitment period. Estimates are again robust to both specifications. Finally, columns (8) and (9) provide estimates for specifications in which the labor commitment policy is defined as a continuous treatment measure, the ratio of initial firms size over final target. Similar to our previous estimates, a less constraining target results in a smaller increase in employment growth over the commitment period.[34]

[31]We approximate the annual growth rates: $[(1+\hat{\beta})^{(1/avg.\ maturity)}]-1$, with average contract maturity of 3.3 years.

[32]Table A.6 provides a way to test whether the differences between the OLS and the IV estimates are driven by treatment effect heterogeneity. We follow Bhuller, Dahl, Løken, and Mogstad (2020) and first perform a principal component analysis using one component based on pre-determined employment (see employment categories in Table 2) and revenue figures measure in 1990, as well as initial employment at contract date, and the sector affiliation. We then separate the predicted component into quartiles and separately estimate the complier share for each quartile group using the first-stage regression specification. Finally, we re-weight the full estimation sample by using the sub-sample complier shares as weights. Panel B of Table A.6 shows that re-weighting based on observed characteristics increases the OLS estimate slightly from 0.49 to 0.52. The difference between the re-weighted OLS and IV estimate, however, remains stark. This suggests that effect heterogeneity is unlikely to explain the differences.

[33]Along with X_i as in equation (7), the selection equation specification includes the exogenous preferences $Z_{i(j)}^{empl}$ and $Z_{i(j)}^{other}=\{Z_{i(j)}^{price}, Z_{i(j)}^{investment}, Z_{i(j)}^{financial}\}$. To mitigate possible collinearity across the system of equations, we use additional information about firms determined before the reunification to predict selection.

[34]Table A.4 further presents a set of robustness checks based on the specification in column (4) of Table 3. In Panel A, we provide different ways of constructing the instrument. Panel B includes additional control variables. Panel C estimates our model using alternative measures of firm growth.

Table 3: Regression Results, Employment Growth

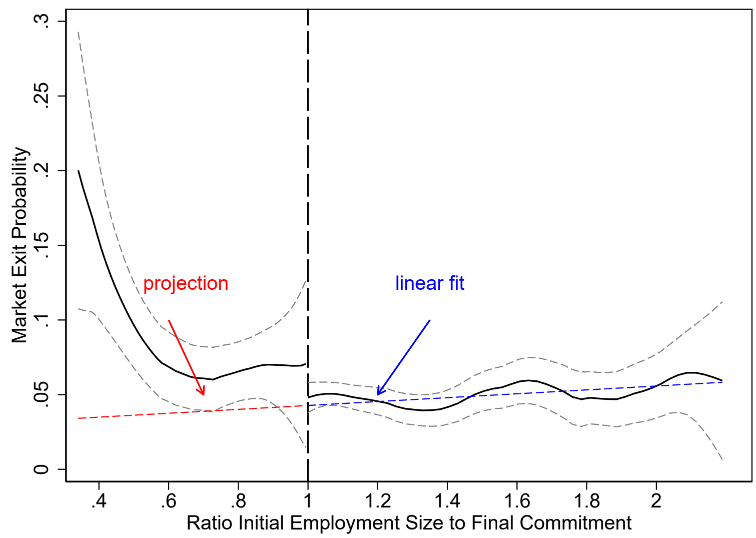
	OLS Model			2SLS-Model					
								Continuous treatment	
			Survivor			Heckman	-2 Growth	Survivor	-2 Growth
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Second-stage results									
Binding contract (0/1)	0.5284***	0.5257***	0.5260***	1.0649***	1.1267***	1.1962***	0.7184***		
	(0.025)	(0.025)	(0.025)	(0.269)	(0.269)	(0.270)	(0.240)		
						[.75;2.77]			
Contract leniency								-0.0092***	-0.0072***
								(0.002)	(0.002)
Panel B: First-stage results									
Stringency				0.0019***	0.0019***	0.0022***	0.0018***	-0.238***	-0.186***
				(0.000)	(0.000)	(0.001)	(0.0005)	(0.067)	(0.075)
						[.0006;.0028]			
Observations	9,051	9,051	9,051	9,051	9,051	9,051	9,363	9,051	9,363
Initial size	60.604	60.604	60.604	60.604	60.604	60.612	60.064	60.604	60.064
Mean growth (non-binding)	-0.002	-0.002	-0.002	-0.002	-0.002	-0.002	-0.063	-0.002	-0.063
F-Statistic				18.27	16.92	11.39	13.91	12.56	6.13
Set of control variables									
Baseline	✓	✓	✓	✓	✓	✓	✓	✓	✓
Industry FE		✓	✓	✓	✓	✓	✓	✓	✓
Individuals			✓	✓	✓	✓	✓	✓	✓
Leave-out dimensions									
Price					✓	✓	✓	✓	✓
Investment					✓	✓	✓	✓	✓
Financial					✓	✓	✓	✓	✓
IMR						✓			

Notes: The table shows OLS and IV regression results of employment growth on binding contracts. Panel A shows the second-stage results. Panel B shows the reduced form regressing the binding contract indicator on the stringency measure. All specifications control for fully interacted THA agency and year fixed effects and are conditional on having at least five privatizations per privatizer. Binding contracts are defined as initial firm size below the committed target level. Contract leniency is defined as initial firm size over the committed target level (winsorized at 4). Instrument refers to the leave-one-out measure of assigning binding contracts. *F*-Statistic refers to the Kleibergen-Paap *F*-Statistic. Baseline controls are time between the first and last audits measured in months, time between contract date and first audit measured in months, and log initial employment level measured at the first audit. Industry FE are 2-digit industry dummies. Individual controls refer to the gender of the privatizer and a dummy for a PhD degree. Standard errors are two-way clustered at privatizer and THA office level. Squared brackets indicate 95% bootstrap CI clustered at the privatizer and THA office level using 2,500 replications. *p<0.1, **p<0.05, ***p<0.01.

5.3 Labor Commitments and Firm Exit

The theoretical discussion highlighted that the introduction of employment targets creates a "polarization" effect across firms, pushing them either up or out of the market. In Section 5.2, we empirically confirmed the "up" mechanism by which firms that remain in the market increase their labor hiring. Conversely, we now turn to empirically investigating the "out" mechanism by which firms under binding employment targets decide to exit the market. To measure firm exit, we use the merged sample of contracts to the MUP data that contains information on the exit year. We also employ a second measure of exit based on the final labor audit reporting zero workers.

FIGURE 8: MARKET EXIT AND THE DEGREE OF BINDING CONTRACTS



Notes: The figure plots market exit rates against the ratio of initial employment relative to the final commitment level. Contracts below 1 have initially lower employment than committed. Contracts above 1 have initially higher employment than committed. The plotted values in the local linear regression are mean-standardized residuals from a regression on initial employment and industry-fixed effects. The two grey dashed lines correspond to the 90% CI. The blue line shows a linear fit of a regression of market exit on the ratio of initial size to final commitment among contracts to the left of or at 1. The red line projects the linear fit into the area where the initial size is below the committed level (to the left of 1). The figure excludes top and bottom 3% of the tightness measure. Total number of firms is 4,596.

Figure 8 starts by describing the relationship between the exit probability of firms at the end of the commitment period and the ratio of initial employment relative to the final target. The figure plots local linear regressions on both sides of the vertical line separating initially binding and non-binding contracts. A first observation is that the proportion of firms exiting the market remains relatively constant for non-binding firms above the vertical line of one. The exit rate for these firms averages 4.8% over the commitment period. Strikingly, firms with binding contracts experience a significantly higher level of market exit, with an average rate of 8.4%. Moreover, the probability of firm exit appears to notably increase with the tightness measure of the labor commitment.

Table 4 provides a regression version of Figure 8, controlling for the same variables as in the

Table 4: Regression Results, Exit Probability at Final Commitment

	MUP exit indicator			ISUD zero employment		
	OLS	2S2SLS		OLS	2S2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Binding contract (0/1)	0.0260**	0.0776		0.0213**	0.0362*	
	(0.011)	(0.089)		(0.011)	(0.023)	
		[-.13;.25]			[-.005;.08]	
Contract leniency			-0.0003			-0.0003**
			(0.0009)			(0.0001)
			[-.0007;.0003]			[-.0002;-.00001]
Observations	4,563	2,804	2,804	4,563	2,804	2,804
Exit share (non-binding)	0.05	0.048	0.048	0.036	0.032	0.032
Share with binding contracts	0.171	0.168	0.168	0.171	0.168	0.168
Set of control variables						
Baseline controls	✓	✓	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓	✓	✓
Leave-out price		✓	✓		✓	✓
Leave-out investment		✓	✓		✓	✓
Leave-out financial		✓	✓		✓	✓

Notes: The table shows OLS and 2S2SLS regression results of exiting probabilities at the end of the commitment period. The outcome variable takes the value of 1 if the firm is exiting by the end of the commitment period and 0 otherwise. All specifications control for fully interacted THA agency and year fixed effects. Binding contracts are defined as initial firm size below the committed target level. Contract leniency is defined as initial firm size over the committed target level (winsorized at 4). Instrument refers to the leave-one-out measure of assigning binding contracts. Controls are as in the baseline specification. Additional control variable is the exporting status to the Eastern block before 1990. Standard errors are two-way clustered at privatizer and THA office level. Squared brackets indicate 95% bootstrap CI clustered at the privatizer and THA office level using 2,500 replications. *p<0.1, **p<0.05, ***p<0.01.

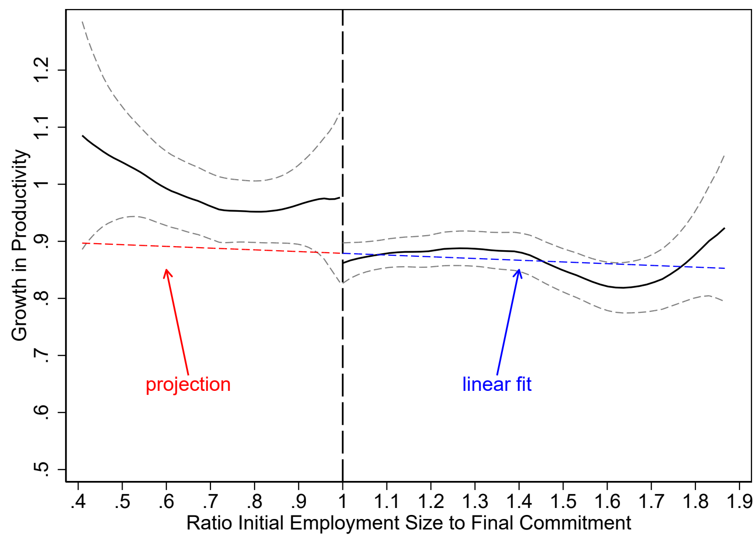
employment growth regressions. The first three columns provide the results using the MUP exit indicator, whereas the last three columns are based on zero employment in the ISUD data.

OLS estimation in columns (1) and (4) both generate similar estimates across our exit measures. In column (4) binding contracts are associated with an increase in market exit of around 2.1% points over the commitment period, or approximately 0.7% points annually. The IV estimates in the remaining columns implement a two-sample 2SLS estimation instrumenting the labor commitment with privatizer preferences. To calculate standard errors in these specifications we perform 2,500 bootstrap replications clustered at the THA office and privatizer level and present the 95% CI in squared brackets. Although with lower precision, the IV results confirm the higher propensity to exit for firms with binding labor commitments. In column (5) binding labor commitment increase firm exit by about 3.6% points at the end of the commitment period. This represents a 1.1% points higher likelihood of exit at the yearly level. Columns (3) and (6) again provide estimates in terms of the continuous treatment variable, defined as the ratio of initial firm size over final target. Consistent with the previous evidence, less stringent labor targets decrease the probability to exit the market. The results are in line with the model's prediction and show that labor commitments negatively impact firm total profits and result in higher firm exit.

5.4 Labor Commitments and Firm-Level Productivity Growth

A contribution of our theoretical framework on labor commitments was to reveal a "necessity-driven" channel for improvements in firm productivity. This dynamic channel is induced by higher marginal profits of distorted firms and arises as they have higher incentives to align their productivity with the new employment target. To provide evidence on this mechanism, we now extend the empirical analysis to productivity dynamics. The analysis relies on two measures of productivity introduced in data Section 4.2: a model-consistent measure of productivity, and a TFP measure taking capital investment into account.

Figure 9: Productivity Growth and the Degree of Binding Contracts



Notes: The figure plots the growth in productivity between the initial year of the contract and the final commitment year against the ratio of initial employment relative to the final commitment level. Contracts below 1 have initially lower employment than committed. Contracts above 1 have initially higher employment than committed. The plotted values in the local linear regression are mean-standardized residuals from a regression on initial productivity, employment and industry-fixed effects. The two grey dashed lines correspond to the 90% CI. The blue line shows a linear fit of a regression of productivity growth on the ratio of initial size to final commitment among contracts to the left of or at 1. The red line projects the linear fit into the area where the initial size is below the committed level (to the left of 1). The figure excludes top and bottom 4% of the tightness measure. Total number of firms is 2,414.

Figure 9 again starts by describing the relationship between model-consistent productivity growth and the ratio of initial employment relative to the final target.[35] As before, the figure plots local linear regressions on both sides of the vertical line separating initially binding and non-binding contracts. The figure provides two major insights. First, growth in productivity is relatively constant for firms above the threshold for binding contracts. The average growth rate between the initial year and the end of the commitment period amounts to 86.8%, which indicates substantial improvements in productivity during the first years after reunification. Second, productivity growth is significantly

[35]Plots based on TFP measures are similar.

higher for firms initially below their committed employment.

Table 5 provides OLS and IV estimates for the "necessity-driven" productivity growth effect. In Panel A, we provide the results using the model-implied productivity measure, whereas Panel B shows the results for TFP growth. Columns (1) to (3) provide OLS estimates for the same specifications as in the case of employment growth. The IV estimates in columns (4) to (9) are obtained in a two-sample 2SLS estimation method similar to that used in the exit analysis. The bootstrapped 95% confidence intervals are shown in square brackets.

Columns (1) to (3) of Table 5 provide OLS evidence that firms with binding labor contracts experience higher productivity growth of around 9 to 11% points. This corresponds to approximately 2% points higher annual growth rates. Column (4) provides IV evidence confirming that firms with binding labor contracts experience higher productivity growth over the commitment period. The point estimate of 0.61 corresponds to approximately 10% points differential growth at the annual level and is consistent across the different productivity measures. In line with the results on employment and market exit, the OLS estimates for productivity also display a downward bias with respect to the IV estimates. Columns (5) to (7) again confirm the robustness of our results to the inclusion of other privatizer preferences as well as accounting for survival bias. Columns (8) and (9) provide estimates in which the labor commitment policy is defined as the ratio of initial firms size over final target. As before, a less constraining target results in a smaller increase in productivity growth over the commitment period. We provide further robustness checks for our productivity growth results in Appendix Table A.7 by varying the labor share coefficient for the model-implied productivity measure.

Additional Evidence, Patenting: To complement the analysis, Table A.8 provides supporting evidence for the dynamic productivity channel by analyzing patenting activity during the commitment period. The outcome variable measures the number of patents produced during the commitment period transformed by the inverse hyperbolic sign transformation. OLS estimates in columns (1) to (3) show a positive and significant correlation between patenting activity and binding employment commitments. Binding labor targets are associated to 1.3% points higher number of patents during the commitment period. Although estimated with more noise, the 2S2SLS coefficient starting in column (4) highlight again a downward bias of the OLS point estimate. Columns (5) and (6) suggest that the impact of binding labor targets on patenting is especially large for firms that were initially less productive and larger.

Additional Evidence, Commitment Horizon: One of the unique features of the East German implementation of labor commitments is not only its heterogeneity in terms of employment targets, but also the range of contract horizons that were attached to them. This naturally lends itself to test whether the dynamic"out-of-necessity" channel is related to the duration of distortions. Table A.9 provides estimates of firm-level productivity growth according to the contracts' commitment horizon. Across all specifications it is clear that "necessity-driven" productivity improvements were significantly stronger for longer contract horizons.

Table 5: Regression Results, Productivity Growth

	OLS Model			2SLS-Model					
								Continuous treatment	
	Survivor					Heckman	-2 Growth	Survivor	-2 Growth
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Model-implied ($\alpha = 0.8$)									
Binding contract (0/1)	0.0856***	0.0858***	0.0865***	0.6135*	0.6928**	0.8400**	0.6587*		
	(0.023)	(0.023)	(0.023)	(0.316)	(0.319)	(0.389)	(0.370)		
				[-.11;1.31]	[-.06;1.38]	[.03;1.55]	[-.39;1.50]		
Contract leniency								-0.0077***	-0.0098***
								(0.002)	(0.003)
								[-.01;-.001]	[-.01;-.001]
Observations	2,414	2,414	2,414	1,624	1,624	1,624	1,669	1,624	1,669
Mean growth (non-binding)	0.852	0.852	0.852	0.854	0.854	0.854	0.776	0.854	0.776
Panel B: TFP growth									
Binding contract (0/1)	0.1185***	0.1118**	0.1109**	0.6268**	0.6717***	0.6842***	0.8232***		
	(0.038)	(0.041)	(0.039)	(0.239)	(0.232)	(0.224)	(0.277)		
				[.17;1.04]	[.19;1.08]	[.15;1.09]	[.27;1.36]		
Contract leniency								-0.0027**	-0.0035**
								(0.001)	(0.001)
								[-.0045;-.0006]	[-.0058;-.0011]
Observations	1,825	1,825	1,825	1,825	1,825	1,825	1,863	1,825	1,863
Mean growth	0.352	0.352	0.352	0.352	0.352	0.352	0.281	0.352	0.281
Set of control variables									
Baseline	✓	✓	✓	✓	✓	✓	✓	✓	✓
Industry FE		✓	✓	✓	✓	✓	✓	✓	✓
Individuals			✓	✓	✓	✓	✓	✓	✓
Leave-out dimensions									
Price					✓	✓	✓	✓	✓
Investment					✓	✓	✓	✓	✓
Financial					✓	✓	✓	✓	✓
IMR						✓			

Notes: The table shows OLS and 2S2SLS regression results of measures of productivity growth on binding contracts. All specifications control for fully interacted THA agency and year fixed effects. Binding contracts are defined as initial firm size below the committed target level. Contract leniency is defined as initial firm size over the committed target level (winsorized at 4). Instrument refers to the leave-one-out measure of assigning binding contracts. Controls are as in the baseline specification. Additional controls are log initial productivity. Standard errors are two-way clustered at privatizer and THA office level. Squared brackets indicate 95% bootstrap CI clustered at the privatizer and THA office level using 2,500 replications. *p<0.1, **p<0.05, ***p<0.01.

6 Quantitative Analysis

In this section, we present the calibration of the model using firm-level data and provide several counterfactual analyses to quantify the various channels by which employment targets distort firm dynamics. The calibrated structural model will allow us to (i) quantify the cost of misallocation of labor introduced by employment targets and (ii) decompose their employment implications into transitory employment growth due to the "polarization effect" as opposed to longer-term growth arising from "necessity-driven" productivity improvements by firms.

6.1 Calibration

We start by setting some of the parameter values externally. We choose the labor share parameter in the production function, α, equal to 0.8 to match the labor earning share. Consistent with the average contract length of around three years in the data, we set the arrival rate of contract expiration, μ, to 1/3. Annual wage growth rate is set to 10% to match the average real wage growth rate over 1990 and 1996 in East Germany (Hunt 2001). The rest of the parameters are calibrated internally by minimizing the distance between the moments from the firm-level data we used in the empirical part of the paper and their model implied counterparts.[36] In particular, let M^E denote the vector of empirical moments and let $M(\Omega)$ denote the vector of model-simulated moments and Ω is the set of parameters to be calibrated internally. We then search Ω to minimize the absolute relative deviation between the model and data; that is, we solve

$$\min_{\Omega} \sum_{m} \frac{|M_m^E - M_m(\Omega)|}{|M_m^E|}.$$

We use the point estimates of the effect of binding contracts on employment growth and productivity growth presented in Section 5 to discipline the cost of not hitting the target, γ. We further use regression results on the impact of binding contracts on exit rates of firms to inform the exit cost parameter, C_e. Finally, we include the growth rate of total employment for firms with binding contracts over the commitment period to pin down the investment cost parameter, ϕ.

We use the following procedure to calibrate the model: For given values of parameters, we first solve the value function in equation 4 and use the implied optimal decisions to simulate a cohort of firms. We initialize the cohort by using the sample of firms used in the empirical part of the paper and take the employment target as given in the data. Crucially, each firm is simulated in line with the time span from its initial audit to its final audit. Finally, we use the simulated data to construct the targeted moments. We repeat this process and search over the parameter space until we minimize the distance between model-implied moments and the data.

[36] In our setting, we cannot separately identify the step size and the cost scale parameter in productivity improvements, λ and ϕ, respectively. Therefore, we fix the value of the step size at 0.25 and calibrate the cost scale parameter internally.

6.2 Calibration Results and Goodness of Fit

Table 6 and 7 contain the calibrated parameters and the targeted moments, respectively. As seen from Table 6, the model is able to replicate the targeted moments well. In particular, we were able to fit higher employment and productivity growth of firms with binding contracts with a relatively parsimonious model. Our calibration suggests that for every missing employee relative to the committed labor target, firms pay a fine that corresponds to 68% of the average wage, given by γ.

Table 6: Moments Used in Calibration

#	Description	Model	Data
M_1	Employment growth regression	0.489	0.498
M_2	Productivity growth regression	0.083	0.083
M_3	Exit rate regression	0.030	0.027
M_4	Total empl. growth rate of firms with binding contracts	0.614	0.672

Table 7: Internally Calibrated Parameters

Description	Model	Estimate
Penalty for not hitting target employment	γ	0.676
Scale for investment cost parameter	ϕ	0.030
Cost of exit	C_e	58.39

The calibrated model also performs well in matching some important patterns in the data that were not targeted. In Figure 10, we depict the employment growth over the policy horizon by the ratio of initial employment relative to target employment, analogous to Panel B of Figure 6. The black and red dots show the model-implied employment growth rates and the data, respectively. Although we only target the *average* excess growth rate of binding-contract firms (employment regression coefficient) in the calibration, our model successfully matches employment growth rates across the entire range of employment-to-target ratios.

The calibrated model also captures well the post-commitment employment dynamics, which is not targeted. Table 8 illustrates the growth rate of total employment during and after the commitment period both in the model and data. The results suggest that while firms with binding employment targets experienced higher employment growth during the commitment period, these employment gains were partially reversed after the commitment policy ends. The reversal confirms the temporary nature of the "polarization" channel through which labor commitments operate. However, consistent with the "necessity-driven" dynamic productivity gains implied by the model, part of the employment gains are persistent.

6.3 Counterfactuals

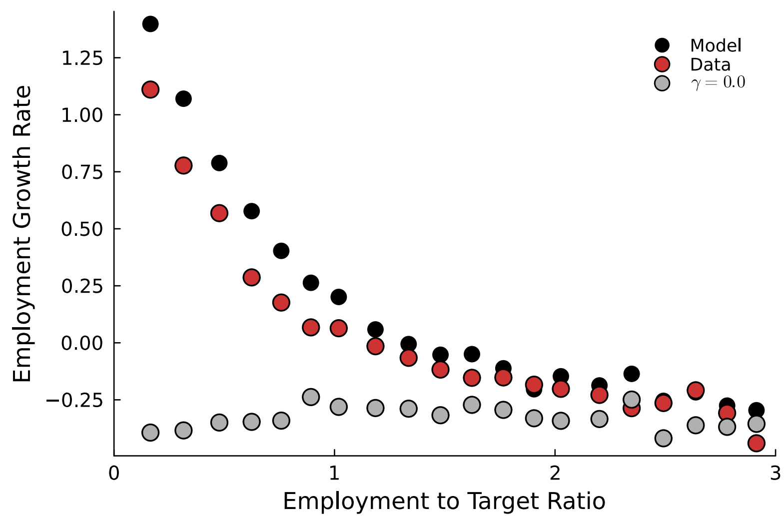
To quantify the importance of the different channels through which employment targets distort firm dynamics, we start with a simple exercise where we simulate a counterfactual economy under which

Table 8: Employment Growth During and After the Commitment Policy

	Model	Data
During Commitment	57.5%	64.1%
After Commitment	-44.5%	-35.5%

Notes: The table shows the growth rate of total employment for firms with initially binding commitments during and after the commitment period. The sample is for a subset of firms for which the post-commitment data is available. Post-commitment period refers to the 6 years after the commitment policy ends.

Figure 10: Employment Growth



Notes: The figure depicts the employment growth at the firm level by the ratio of initial employment relative to target employment. The black and red dots show the model-implied employment growth rates and the data, respectively. Gray dots show the employment growth rates under the counterfactual economy where there are no employment targets. The x-axis is divided into 20 quantile bins and each dot represents average value within that bin.

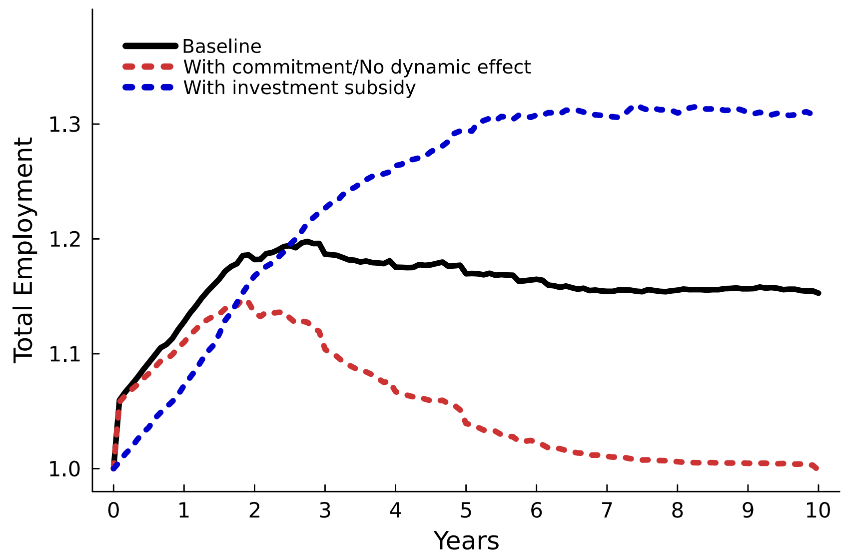
there are no employment targets.[37] For this, we keep all other parameters of the model as in the baseline values and set the cost of commitment parameter to zero, $\gamma = 0$. Gray dots in Figure 10 show the employment growth over the policy horizon by the ratio of initial employment relative to target employment in this counterfactual economy. As seen from the figure, the employment growth rate is substantially reduced in the absence of targets, especially for those firms that have more binding contracts initially. This counterfactual economy implies a 20% drop in the aggregate employment across firms under employment target, rather than the 5% drop we observe in the data, suggesting that this policy had a non-trivial role in shaping aggregate employment dynamics.

Despite the induced higher employment, the policy distorts the allocation of labor across firms. To isolate and quantify the cost of this misallocation of labor, we calculate a counterfactual total output by efficiently reallocating labor across distorted firms immediately after the firms respond to the employment targets. This exercise suggests that total output would have been 1.8% higher if the policy didn't distort the static labor choice of the firms.

[37]Throughout this section, the counterfactual simulations reflect the full dynamic responses of firms including the exit decision.

Our next exercise decomposes the impact of employment targets on aggregate employment dynamics into "static" and "dynamic" effects. To isolate the static effect, we simulate a counterfactual economy where firms still operate under employment targets but we shut down the "out-of-necessity" productivity growth effect by assuming that marginal profits are not affected by the employment targets and are set to the value under the case of no employment targets for all firms. In other words, this counterfactual economy only includes the direct, static effect of employment targets on employment choices over the policy horizon. Figure 11 summarizes the results by plotting the total employment

Figure 11: Total Employment under Counterfactual Economies



Notes: The figure shows total employment implied by the model under three different cases: (i) baseline economy (solid black line), (ii) counterfactual economy with no dynamic effect (red dashed line) and (iii) counterfactual economy with investment subsidies. "No dynamic effect" counterfactual is obtained by setting the marginal profits to the value under the case of no employment targets for all firms. The investment subsidy level is set to match the total employment growth in the data through the period the original policy was implemented. All series are normalized to 1 at the beginning of the period.

over time under commitment (black line) and under no dynamic effects (red dotted line), relative to the total employment under no commitment counterfactual. Our calibrated model attributes one-third of the employment growth over three years to dynamic effects, as can be seen from the distance between the black line and the red dotted line at year 3. After 10 years, the static effect of the policy completely disappears (red dotted line goes back to 1), as by that time there are no more firms with employment commitment left. That is, all the employment gains shown by the black line are due to dynamic effect after 10 years, implying a 15% permanent employment increase relative to a no commitment scenario.

Lastly, we consider an alternative policy where firms are provided uniform subsidies for productivity improvements aiming at a more organic growth in firm employment. Such an intervention can include policies that encourage more R&D investment, or technology licensing. We choose the subsidy level such that the total employment growth is the same as in the data through the period the original commitment policy was implemented. The blue dotted line in Figure 11 depicts the total employment growth under such a productivity subsidy. The comparison between the subsidy policy and the commitment policy in terms of employment dynamics reveals two important distinctions. Firstly, the subsidy policy leads to a slower initial rise in employment but ultimately achieves double the long-term employment gains compared to the commitment policy. Secondly, the subsidy policy achieves these higher employment gains without causing labor misallocation across firms, as it does not distort static labor decisions.

7 Conclusion

In this paper, we study the implications of a policy that imposed employment targets to push firms to grow or limit their downsizing. Our three-step analysis involved the construction of a dynamic model, an empirical assessment, and counterfactual simulations based on a calibrated model. The model highlighted the dual effect of binding employment targets. Statically, imposing a labor target generates a "polarization effect" across firms: those with the poorest productivity opt not to hire any workers due to the implied fixed cost and exit the market, while all other firms that initially had fewer workers than their employment target now increase their labor due to the implied lower variable costs. Dynamically, firms intensify efforts to enhance their productivity "out of necessity" to align their productivity so that the committed labor matches the firm's equilibrium hiring. Empirical validation, using a rich German dataset and an instrumental variable approach, confirmed these theoretical predictions. We find that, over the commitment period, firms with binding employment targets experience not only 25% points higher annual employment growth rate and 1.1% points higher exit probability but also 10% points higher annual productivity growth. Finally, the structural estimates of the model allow us to quantify the extent of misallocation resulting from both the temporary impact of labor commitments, known as the "polarization effect," and the permanent impact arising from "necessity-driven" productivity improvements. While we estimate 15% of long-term employment gains to stem from the latter effect, we also show that direct subsidies for productivity improvements could lead to even higher permanent employment levels.[38]

This paper aims to take the first steps in understanding a widely-used yet understudied public intervention where governments use employment commitments to affect firm-level employment. The analysis provides key insights into the trade-offs policymakers face in promoting firm employment growth. Industrial policies can stimulate growth through "induced" or "organic" strategies. We show that "induced" strategies, which reduce hiring costs at any productivity level, can lead to rapid job creation but often lack long-term sustainability. In contrast, "organic" strategies, which foster innovation and productivity, may create jobs more slowly but result in more sustainable and resilient employment growth.

[38]Our analysis focuses on the positive implications of this policy. As such, the model we employ focuses on firm-level decisions, analyzing how the policy distorts firm behavior statically and dynamically, rather than exploring the policy's welfare implications. It is important to recognize that a comprehensive normative analysis would necessitate to specify various other details of the economic environment. This encompasses factors such as externalities present within the economy and the interactions between the policy and these externalities. We leave this interesting avenue for future research.

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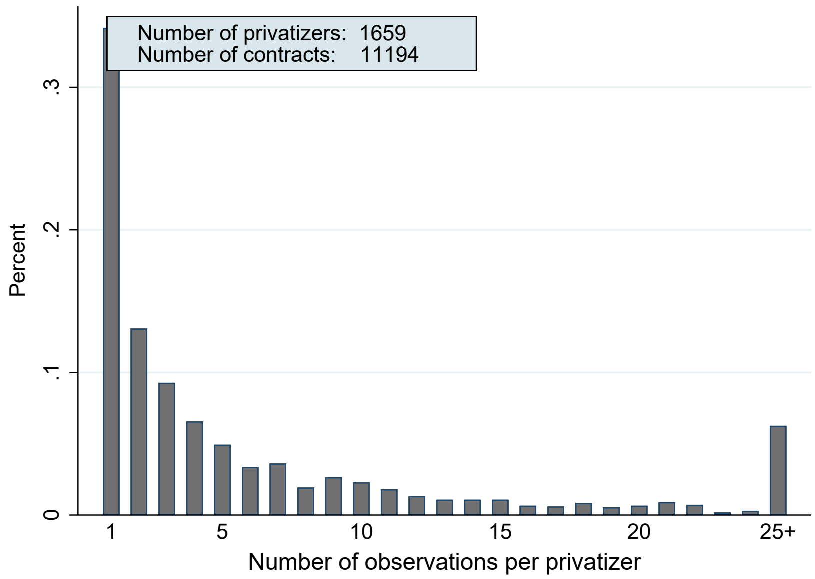
Supplementary Appendix

A Further Empirical Results

Figure A.1: Privatizations per Privatizer



Notes: The figure plots the number of privatization handled per individual privatizer (winsorized at 25). The total number of privatizations is 11,194. These cases are handled by 1,659 individuals. 5.04% of all cases are organized by privatizers only observed once in the sample. This corresponds to 652 individuals.

Table A.1: Test of Random Assignment of Investors to Privatizers

	Indep. variable: Stringency			Dep. variables	
	Coefficient	p-value	Adj. p-value	Mean	Standard deviation
	(1)	(2)	(3)	(4)	(5)
Employment					
Log investor size	-0.0033	0.1228	0.9740	2.4600	1.8220
Investor size > 100 employees	-0.0008	0.1135	0.9181	0.1400	0.3480
Credit rating					
Creditworthiness investor	0.0768	0.3334	0.9990	284.38	101.58
High rating	-0.0004	0.1101	0.9800	0.0640	0.2460
Location					
West German investor	0.0001	0.8356	0.9990	0.6780	0.4680
Sector affiliation					
Agriculture, forestry, fishing	0.0002	0.5666	0.9930	0.0120	0.1120
Mining and quarrying	-0.0001	0.4517	0.9980	0.0040	0.0700
Manufacturing	-0.0006	0.1441	0.9860	0.1740	0.3800
Energy	-0.0003	0.0505	0.0859	0.0040	0.0680
Water	0.0001	0.7685	0.9990	0.0380	0.1940
Construction	-0.0005	0.3395	0.9860	0.1260	0.3320
Retail trade	0.0012	0.0113	0.5375	0.2000	0.4000
Transportation	-0.0003	0.2162	0.9940	0.0540	0.2280
Hospitality	0.0001	0.3576	0.9990	0.0400	0.1940
ICT	-0.0001	0.6517	0.9990	0.0420	0.2000
Baning and Insurance	0.0000	0.9975	1.0000	0.0320	0.1760
Real Estate	0.0001	0.7775	0.9990	0.0580	0.2340
Technical services	-0.0003	0.5271	0.9980	0.1020	0.3040
Economic services	0.0002	0.2320	0.9940	0.0340	0.1800
Other	0.0003	0.6932	0.9940	0.0760	0.2640

Notes: The sample is based on 4,993 contracts matched to investor characteristics. Each line represents a single regression of the explanatory variable on the stringency measure that takes values between 0 (minimum) and 100 (maximum) controlling for THA office and year of privatization fixed effects. Standard errors are two-way clustered at privatizer and THA office level. p-values in column (2) correspond to the regression model and are two-way clustered at the privatizer and THA office level. p-values in column (3) adjust for multiple testing using Romano-Wolf procedure (Romano and Wolf 2005a; Romano and Wolf 2005b) with 1,000 bootstrap replications. *p<0.1, **p<0.05, ***p<0.01.

Monotonicity The interpretation of our instrumental variable estimates relies not only on the validity of the exclusion restriction but also on the accompanying monotonicity condition. In our context, the monotonicity condition implies that firms with a strict labor commitment assigned to a lenient privatizer would have also received a strict commitment if they were assigned to a tough privatizer, and vice versa. Frandsen, Lefgren, and Leslie (2023) show that it is possible to relax the strict (pairwise) monotonicity assumption to an average monotonicity assumption and still recover a weighted average of individual treatment effects. This average monotonicity assumption requires that the data contain only complier groups where the covariance between privatizer stringency and binding labor commitments is positive.

To test this condition, we first test whether the first stage regression of binding commitments on privatizer stringency is non-negative across various observable subgroups (Bhuller, Dahl, Løken, and Mogstad 2018; Dobbie, Goldin, and Yang 2018). In Table A.2, we present the results of first-stage regressions for different firm size groups based on the 1990 measurements and also for sub-samples grouped by sector affiliation. We construct our instrument using the entire sample and perform the first-stage regressions on the sub-samples.[39] As expected under the assumption of average monotonicity, all first-stage coefficients are positive and statistically significant. Finally, following Frandsen, Lefgren, and Leslie (2023), we implement and fail to reject in Table A.3 the joint null hypothesis that pairwise monotonicity and exclusion hold. The test for the joint null hypothesis is performed for different numbers of knots and Bonferroni weights using the suggested quadratic spline (controlling for THA office times year fixed effects).

Table A.2: First-Stage Regression Results by Sub-Samples

	Baseline	Employment in 1990		Revenue in 1990		Sector affiliation	
		< p(75)	< p(50)	< p(75)	< p(50)	Tradeable	Non-tradeable
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Privatizer stringency	0.0015***	0.0012**	0.0013**	0.0013**	0.0012*	0.0017***	0.0007
	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)
Observations	10,252	5,919	3,934	5,829	3,873	5,083	2,927
Average employment at contract date	63.772	58	46.646	55.93	49.616	69.218	70.88
Average growth rate	0.136	0.082	0.07	0.088	0.086	0.108	0.058
Share with binding contracts	0.206	0.182	0.168	0.19	0.182	0.218	0.144
Set of control variables							
Baseline controls	✓	✓	✓	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓	✓	✓	✓
Industry controls	✓	✓	✓	✓	✓	✓	✓

Notes: The table shows IV regression results. All specifications control for fully interacted THA agency and year fixed effects and are conditional on having at least 2 privatizations per privatizer. All strata variables (e.g., employment in 1990) refer to the initial firm from where the contract was generated. There are 335 contracts affiliated with the agriculture sector not presented in the table. F-Statistic refers to the Kleibergen-Paap F-Statistic. Baseline controls are the time between the first and last audits measured in day, time between contract date and first audit measured in days, and log initial employment level (+1) measured at the first audit. Individual controls are the gender of the privatizer and academic degree (PhD). Industry controls are 2-digit industry dummies. Standard errors are two-way clustered at privatizer and THA office level. Instrument refers to the leave-one-out measure of assigning binding contracts. *p<0.1, **p<0.05, ***p<0.01.

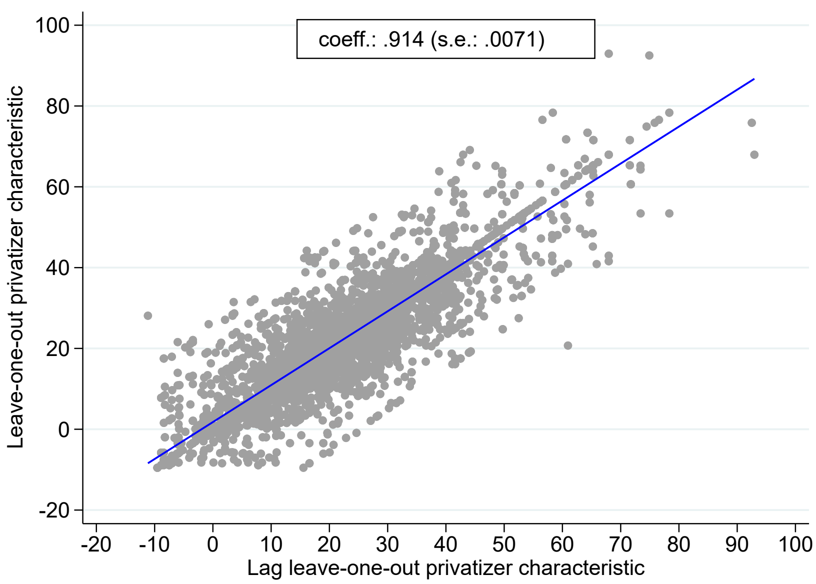
[39] To ensure an adequate sample size, these regressions are conducted on sub-samples with a minimum of two privatizations per privatizer.

Table A.3: Test of Joint Null of Monotonicity and Exclusion

	15 knots				20 knots			
	$\omega = 1$	$\omega = 0.8$	$\omega = 0.5$	$\omega = 0.3$	$\omega = 1$	$\omega = 0.8$	$\omega = 0.5$	$\omega = 0.3$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Test statistic	516	516	516	516	536	536	536	536
d.f.	(500)	(500)	(500)	(500)	(495)	(495)	(495)	(495)
P-value	[0.296]	[0.371]	[0.593]	[0.988]	[0.099]	[0.124]	[0.198]	[0.330]

Notes: The table presents results from the test proposed in Frandsen, Lefgren, and Leslie (2023) for the joint null hypothesis that the monotonicity and exclusion restrictions hold. We test this null using THA office times year-of-privatization effects conditional on having handled at least 5 privatizations. Columns (1) to (4) provide the results imposing 10 knots in the quadratic spline function. Columns (5) to (8) provide the results imposing 15 knots in the quadratic spline function. Each column is associated with different weighting schemes between the fit and slope components of the test. A failure to reject the null implies that we cannot reject the hypothesis that the monotonicity and exclusion restrictions jointly hold. The test was implemented in Stata via the package `testjfe`.

Figure A.2: Persistence of Privatizer Characteristics



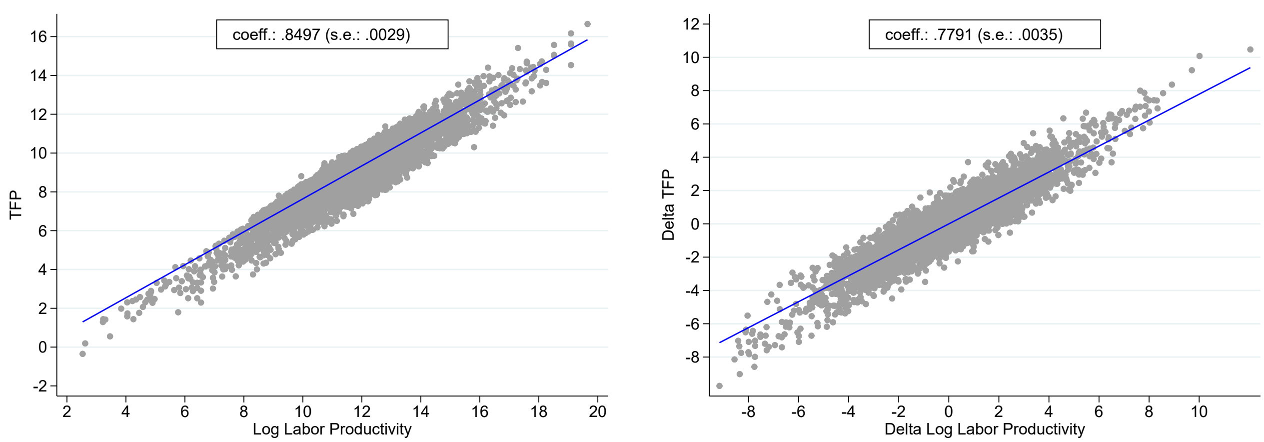
Notes: The figure plots the leave-one-out rate of a tight contract (initial firm size < final committed size) in the previous case against the leave-one-out rate of a binding contract in the current case. All plotted values are mean-standardized residuals from regressions on fully interacted THA office and year of privatization fixed effects. The blue line corresponds to a linear regression. The figure is constructed by conditioning of having handled at least five privatization contracts. Total number of observations is 8,759.

Figure A.3: Labor Productivity across Firm Size

Notes: The figure plots labor productivity across the firm size distribution among 7,620 initial GDR firms with sales and employment information in 1990. The figure exclude the top and the bottom 1% of the productivity measure.

Figure A.4: Correlation Productivity Measures

Productivity in Level / Yearly Change in Productivity



Notes: The figure plots two measures of firm-level productivity using a matched sample of contracts with THA survey data (Soestra). The left panel shows the correlation between the log labor productivity and log TFP. The right panel shows the correlation between the yearly change in log labor productivity and the yearly change in log TFP. TFP is calculated based on a Cobb Douglas production function with log revenue as measured output and log employment and log capital as inputs.

Table A.4: Robustness Tests, Employment Growth

	Dep. variable: Firm growth			Random Assignment
	Coefficient (1)	First-stage (2)	F-Statistic (3)	Joint F-test (p-value) (4)
A: Instrument construction				
Only past decisions for instrument	1.2486***	0.0008***	9.553	0.245
	(0.349)	(0.000)		
Above 10 cases per privatizer	1.2179***	0.0026***	17.50	0.362
	(0.245)	(0.001)		
Very tight contracts	1.4533***	0.0016***	11.14	0.254
	(0.480)	(0.000)		
B: Control variables & sample selection				
Control for renegotiation attempts	1.0113***	0.0019***	20.81	0.463
	(0.237)	(0.000)		
Control for penalty clause	1.0694***	0.0017***	17.12	0.463
	(0.297)	(0.000)		
Control for initial employment bins (14 dummies)	1.0172***	0.0018***	17.15	0.463
	(0.301)	(0.000)		
MUP subsample	0.5583**	0.0017***	12.73	0.654
	(0.253)	(0.000)		
C: Manipulation of the outcome variable				
Log employment differences	0.8934**	0.0019***	12.32	0.463
	(0.331)	(0.000)		
Annualized firm growth, $(L_t/L_{t-1})^{1/\#year} - 1$	0.4691***	0.0019***	18.27	0.463
(*trimmed at the upper percentile*)	(0.138)	(0.000)		
Growth rate < 2 & > -2	0.7426**	0.0015***	12.32	0.399
	(0.313)	(0.000)		

Notes: The table shows IV regression results. All specifications control for fully interacted THA office and year fixed effects and are conditional on having at least five privatizations per privatizer. For sample size reasons, the MUP subsample is conditional on having at least three observations per privatizer. Column (1) shows the point estimate of the main variable of interest (except the specification with at least 10 observations per privatizer). Column (2) shows the corresponding first-stage coefficient. F-Statistic in column (3) refers to the Kleibergen-Paap F-Statistic (first-stage). All specifications condition on the full set of control variables including baseline controls (log) time between the first and last audits (+1) measured in days, log time between contract date and first audit (+1) measured in days, and log initial employment level (+1) measured at the first audit), individual controls (gender of the privatizer and academic degree (PhD)), and 2-digit industry controls. Column (4) shows the F-Statistic of a joint F-test of random assignment. The dependent variable is always the instrument regressed on log initial employment variables (accounting, purchasing, HR, production, sales, administration, R&D), and log initial revenue measured in 1990 (conditional on industry-fixed effects and fully interacted THA office and time fixed effects). Standard errors are two-way clustered at privatizer and THA office level. *p<0.1, **p<0.05, ***p<0.01.

Table A.5: Heckman Selection Equations: Probit Regression Results

	P(Zero employment at contract end)	
	(1)	(2)
1(Soviet Revenue)	-0.2496***	-0.2394***
	(0.001)	(0.078)
Mean outcome	0.033	0.035
Observations	9,369	8,687
THA office FE	✓	
Year FE	✓	
THA office times year FE		✓

Notes: The table reports Heckman selection equation regression results based on probit specifications predicting the outcome variable of zero employment at the end of the commitment period. Control variables include all controls used in the main specification (baseline, industry, individual controls). The instrument refers to a dummy variable equal to 1 if the initial GDR firm was exporting to the Soviet bloc before unification. Column (1) includes THA office and year FE, whereas column (2) includes THA office $\times$ year FE. Standard errors are clustered at the privatizer level. Robust standard errors in parenthesis. *p<0.1, **p<0.05, ***p<0.01.

Table A.6: OLS Regression Results with Different Control Variables & Weighting

	OLS-Model Results		
	(1)	(2)	(3)
A: Baseline			
Binding contract	0.5284***	0.5287***	0.5260***
	(0.025)	(0.025)	(0.025)
B: Complier re-weighting			
Binding contract	0.5625***	0.5629***	0.5593***
	(0.024)	(0.023)	(0.024)
Observations	9,051	9,051	9,051
Average employment at contract date	60.604	60.604	60.604
Average growth rate	0.136	0.136	0.136
Share with binding contracts	0.206	0.206	0.206
Set of control variables			
Baseline controls	✓	✓	✓
Individual controls		✓	✓
Industry controls			✓

Notes: The table shows OLS regression results. All specifications control for fully interacted THA agency and year fixed effects and are conditional on having at least five privatizations per privatizer. Baseline controls are log time between the first and last audits (+1) measured in days, log time between contract date and first audit (+1) measured in days, and log initial employment level (+1) measured at the first audit. Individual controls are the gender of the privatizer and academic degree (PhD). Industry controls are 2-digit industry dummies. Standard errors are two-way clustered at privatizer and THA office level. *p<0.1, **p<0.05, ***p<0.01.

Table A.7: Robustness Tests, Productivity Growth with Different α

	$\alpha = 0.7$		$\alpha = 1.0$	
	OLS	2S2SLS	OLS	2S2SLS
	(1)	(2)	(3)	(4)
Binding contract	0.1059***	0.8676**	0.0754***	0.8552**
	(0.021)	(0.409)	(0.025)	(0.400)
		[.01;1.57]		[.002;1.50]
Observations	2,395	1,612	2,395	1,612
Mean outcome binding contracts	1.014	1.014	1.014	1.014
Mean outcome non-binding contracts	0.842	.842	0.842	0.842
Set of control variables				
Baseline controls	✓	✓	✓	✓
Industry controls	✓	✓	✓	✓
Privatizer controls	✓	✓	✓	✓
Leave-out other dimension		✓		✓
IMR		✓		✓

Notes: The table shows OLS and 2S2SLS regression results of measures of productivity growth on binding contracts. All specifications control for fully interacted THA agency and year fixed effects. Binding contracts are defined as initial firm size below the committed target level. Standard errors are two-way clustered at privatizer and THA office level. Squared brackets indicate 95% bootstrap CI clustered at the privatizer and THA office level using 2,500 replications. *p<0.1, **p<0.05, ***p<0.01.

Table A.8: Regression Results, Cumulative Patents during Commitment Period

	OLS			2S2SLS		
					Init. Prod < Median	Prod < Med. & Size > 10
	(1)	(2)	(3)	(4)	(5)	(6)
Binding contract	0.013*	0.0134**	0.0125*	0.0897	0.1640**	0.2350**
	(0.006)	(0.006)	(0.007)	(0.146)	(0.076)	(0.097)
				[-.08;.29]	[-.11;.37]	[-.11;.56]
Observations	4,313	4,313	4,313	2,080	793	511
Mean of Y of binding contracts	0.046	0.046	0.046	0.016	0.011	0.014
Mean of Y of non-binding contracts	0.02	0.02	0.02	0.007	0.009	0.009
Set of control variables						
Baseline controls	✓	✓	✓	✓	✓	✓
Individual controls		✓	✓	✓	✓	✓
Industry controls			✓	✓	✓	✓
Leave-out other dimension				✓	✓	✓
IMR				✓	✓	✓

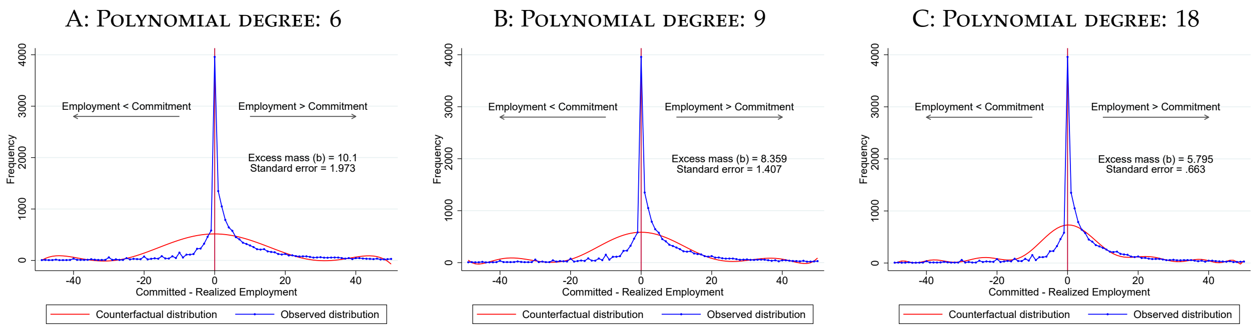
Notes: The table shows OLS and 2S2SLS regression results of the number of patents produced (inverse hyperbolic sign transformed) during the commitment period. All specifications control for fully interacted THA agency and year fixed effects. Binding contracts are defined as initial firm size below the committed target level. Leave-one-out variables in other dimensions and the inverse mills ratio enter as discussed in the text. Column (5) selects firms with initial productivity levels below the media in the sample. Column (6) in addition to column (5) shows the results for firms with initially above 10 employees. Standard errors are two-way clustered at privatizer and THA office level. Squared brackets indicate 95% bootstrap CI clustered at the privatizer and THA office level using 2,500 replications. *p<0.1, **p<0.05, ***p<0.01.

Table A.9: Regression Results, Productivity Growth by Contract Length

	Model-implied ($\alpha = 0.8$)				TFP growth			
	OLS		2S2SLS		OLS		2S2SLS	
	Short	Long	Short	Long	Short	Long	Short	Long
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Binding contract	0.0725	0.0939***	-0.4049	0.9483**	-0.0004	0.1263**	0.1208	0.7910**
	(0.067)	(0.027)	(1.567)	(0.359)	(0.123)	(0.051)	(0.557)	(0.307)
			[-3.62;3.32]	[.08;1.72]			[-1.56;.94]	[.26;1.30]
Observations	357	2,011	161	1,434	350	1,458	350	1,458
Set of control variables								
Baseline controls	✓	✓	✓	✓	✓	✓	✓	✓
Privatizer controls	✓	✓	✓	✓	✓	✓	✓	✓
Industry controls	✓	✓	✓	✓	✓	✓	✓	✓
Leave-out other dimension			✓	✓			✓	✓
IMR			✓	✓			✓	✓

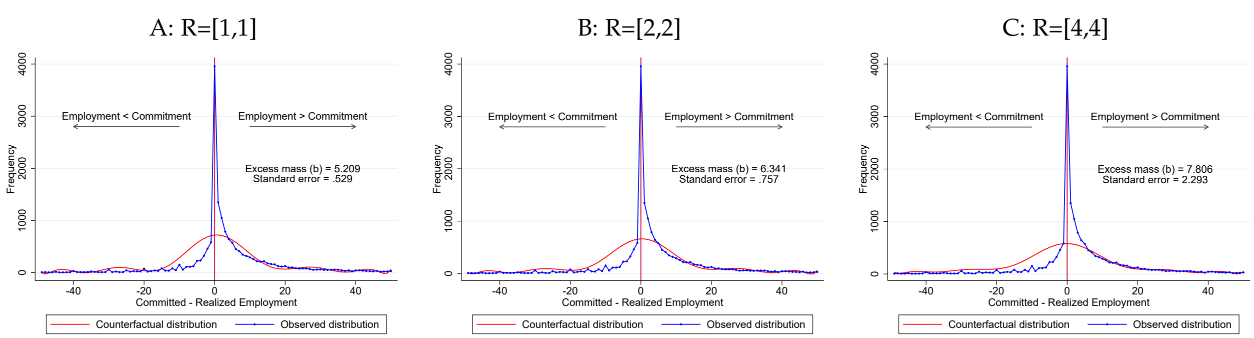
Notes: The table shows OLS and 2S2SLS regression results on measures of productivity growth on binding contracts by contract length. Short contracts are defined with a during between the first and the last audit below or at 12 months. Long contracts have a length between the first and the last audit above 12 months. All specifications control for fully interacted THA agency and year fixed effects. Binding contracts are defined as initial firm size below the committed target level. Leave-one-out variables in other dimensions and the inverse mills ratio enter as discussed in the text. Standard errors are two-way clustered at privatizer and THA office level. Squared brackets indicate 95% bootstrap CI clustered at the privatizer and THA office level using 2,500 replications. *p<0.1, **p<0.05, ***p<0.01.

Figure A.5: Bunching with different polynomials



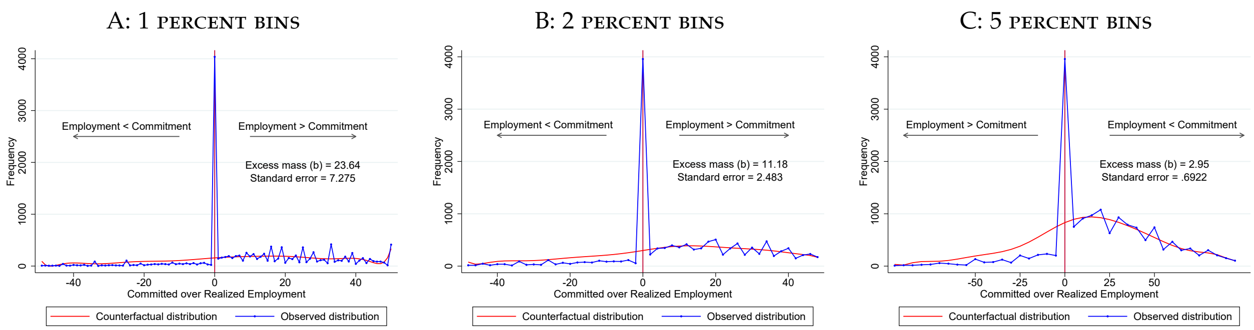
Notes: The figures show the employment distribution around the committed employment (demarcated by the vertical red line at 0) for contracts between 1990-2002. The blue line in dots is a histogram of actual employment relative to the commitment target in the final commitment year. Each point shows the number of observations in employment count bin (deviation between the target and the realized employment). The solid line beneath the empirical distribution is a twelve-degree polynomial fitted to the empirical distribution excluding the area of missing one employee and have 4 employees more than committed. The shaded region in yellow is the estimated excess mass. Standard error is calculated using a parametric bootstrap procedure. Estimation based on Chetty, Friedman, Olsen, and Pistaferri (2011). Panel A shows the results using a six-degree polynomial order. Panel B shows the results using a ninth-degree polynomial order. Panel C shows the results using a eighteenth-degree polynomial order.

Figure A.6: Bunching with symmetric R



Notes: The figures show the employment distribution around the committed employment (demarcated by the vertical red line at 0) for contracts between 1990-1995. The blue line in dots is a histogram of actual employment relative to the commitment target in the final commitment year. Each point shows the number of observations in employment count bin (deviation between the target and the realized employment). The solid line beneath the empirical distribution is a twelve-degree polynomial fitted to the empirical distribution excluding the area of missing one employee and have 4 employees more than committed. The shaded region in yellow is the estimated excess mass. Standard error is calculated using a parametric bootstrap procedure. Estimation based on Chetty, Friedman, Olsen, and Pistaferri (2011). Panel A shows the results excluding -1 and 1. Panel B shows the results excluding -2 and 2. Panel C shows the results excluding -4 and 4.

Figure A.7: Bunching with percent deviation bin



Notes: The figures show the employment distribution around the committed employment (demarcated by the vertical red line at 0) for contracts between 1990-1995. The blue line in dots is a histogram of actual employment relative to the commitment target in the final commitment year. Each point shows the number of observations in employment count bin (deviation between the target and the realized employment). The solid line beneath the empirical distribution is a twelve-degree polynomial fitted to the empirical distribution excluding the area of missing one employee and have 4 employees more than committed. The shaded region in yellow is the estimated excess mass. Standard error is calculated using a parametric bootstrap procedure. Estimation based on Chetty, Friedman, Olsen, and Pistaferri (2011). Panel A shows the results by constructing 1 percentage bin deviations. Panel B shows the results by constructing 2 percentage bin deviations. Panel C shows the results by constructing 5 percentage bin deviations.

Table A.10: Bunching by Sub-Samples

	Excess mass (b)	Standard error
	(1)	(2)
A: Industry affiliation		
Agriculture, energy, mining	9.076	3.220
Chemistry, plastics	4.952	0.9231
Extraction of cut-stone, iron, casting, steel forming	7.842	3.351
Steel construction, mechanical & electrical engineering, automobile	6.699	1.023
Paper, print, textile, food	7.617	1.070
Construction and buildings trades, wholesale, retail	7.257	1.227
Transportation, communication, insurance	5.799	1.325
B: Contract maturity		
16 to 31 months	6.574	1.198
Below 16 months	8.697	3.010
Above 31 months	7.212	1.118
C: Number of audits		
Multiple audits	6.521	0.773
D: Penalty condition		
Exclude contracts without penalty clause	6.627	0.889
E: Initial size		
Below target	6.473	0.989
Above target	5.151	0.540

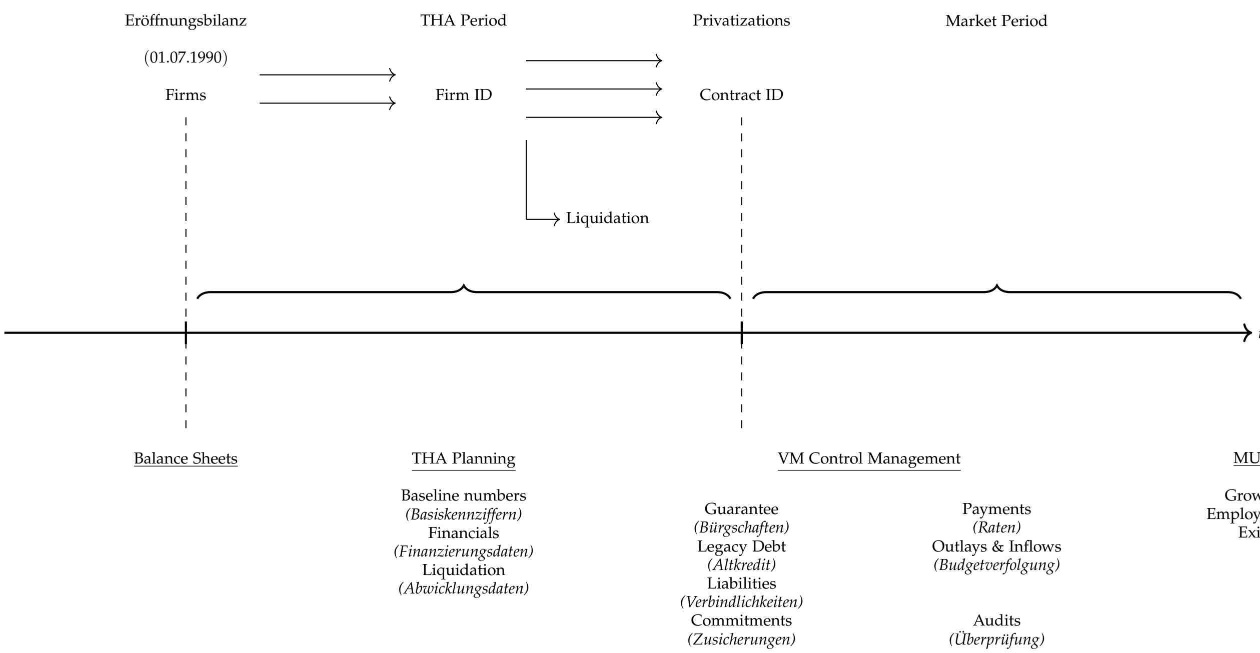
Notes: The table shows bunching estimates of the employment distribution around the committed employment for contracts between 1990-1995 by different groups. The counterfactual distribution is a based on a twelve-degree polynomial fitted to the empirical distribution excluding the area of missing one employee and having three employees more than committed. Standard errors are calculated using a parametric bootstrap procedure with 100 replications. Estimation based on Chetty, Friedman, Olsen, and Pistaferri (2011). Panel A shows the results by industry. Panel B shows the results by contract maturity cutting at 25th (16 months between contract date and final commitment) and 75th (31 months between contract date and final commitment) percentile. Panels C and D select only contracts with multiple audits and with a penalty clause, respectively. Panel E distinguishes by initial contract size (measure at the first audit) relative to the final target.

B Data Addendum - ISUD Data Environment

This section provides an overview and a description of data used in the empirical analysis. The data were provided to the authors on the basis of an agreement between the IWH (Halle) and the German Federal Archives (*Bundesarchiv*). This agreement involved the transfer of more than 500 separate data tables in digitized format (csv) on activities of Treuhand.

The timeline in Figure B.1 visualizes the level and timing of observations. The main identifiers in the ISUD environment are at the firm level and at the contract level. The former is constituted by information from firms submitting a balance sheet (DM Eröffnungsbilanz) and transitioning into the THA portfolio. The THA assigns initial IDs to each firm, and, in the case of restructurings and firm separations, new IDs are created. Once assets are sold out of the firms, we observe contract IDs. These contracts are organized and used by the contract management teams (VM) to follow up on payments and obligations of buyers.

FIGURE B.1: TIMELINE FROM REUNIFICATION TO THE MARKET PERIOD



Two tables are used to measure firm-level information: `basis_kennziffern` and `basis_kennziffern_91`. The table `basis_kennziffern_91` comprises most of the information and, therefore, is the main table. In case of missing values, we search for information in `basis_kennziffern` to complement and to construct a comprehensive cross-section of firm information for the year 1990.[40] The information relates to employment (including a breakdown into production workers, HR, and administration),

[40]The information can be combined to construct a yearly panel with information at the firm level between 1989 and 1994. This dataset cannot be used to study the evolution of firms over time because the firm disappears from the dataset once the firm transitions out of the THA portfolio either because of a privatization or liquidation.

revenues (including a breakdown of revenues in East and West Europe), and the assignment of firms to THA offices (headquarters or local subsidiary). The data contains a total of 13,552 legal firm entities, out of which 93.3% are observed for the first time in 1990.[41] We complement the data with additional industry information from the SOESTRA survey (see Mergele, Hennicke, and Lubczyk 2020). The final data set is used in the analysis to study random assignment of firms to privatizers in Table 2, to calculate labor productivity growth between 1990 and the final commitment year in Table 5, market exit effects in Table 4, and to construct Figure A.3.

A second set of data tables provides information on ownership changes of firms: `besitz_91` and `besitz`. Similarly, `besitz_91` comprises most information, and `besitz` is used to fill missing values. Combining the two tables generates a dataset with information on 13,051 firms about partial sales, privatizations and liquidation decisions. These data allows us to not only track changes in ownership, but also to calculate the share of firms privatized or liquidated.

One of the main challenges of the ISUD data environment is to link information at the firm level with contract-level information. This link is important for two reasons. First, it allows us to study random assignment, productivity growth, and market exit. Second, it provides us information on which THA division handles the privatization of the firm. We first describe the data tables used to construct the link between firms and contracts. Table B.1 provides an overview of the data tables and a short description.

The data table `ASVA01T` forms the main source of information for contracts. It provides us with information on the contract ID and the contract date. It does not, however, provide information on the link between the contracts and the firm. For this reason, we search for this information across the *ISUD* system. The tables `ASVA02T`, `VATVT`, `ASVA22T`, `ASVA50T`, and `FE3_VT` are identified to be candidates that possess the link. Due to the degree of non-missing information, the two most important tables are `ASVA02T` and `VATVT`. The search process generates 48,086 unique contracts with a firm link.

Another advantageous feature of `ASVA01T` is that it contains not only the contract ID but also the string names of privatizers who handle the contracts and communicate/negotiate with potential investors. We clean the variable "*PNAME*" which is labeled as "*Name d. zuständigen Privatisierers*". In the overall file, we generate 3,521 unique names for 58,544 contracts after name cleaning. The main reason for losing contracts is missing values in this name variable. Out of the 256,842 contracts in the data table, 147,060 do not have information on the name of the privatizer. The reason why most of the contracts do not possess a name of a privatizer is because the contracts are not related to firms but represent estate, machinery or land deals. Therefore, these contracts are not related to firms and consequently do not have a privatizer attached to it. Linking contracts to contain privatizer information, labor commitment contracts, and firm links generates a sample of 11,194 contracts as shown in Section 5.

After this preparation of baseline tables, we obtain information on labor commitments and labor audits. We start with the original files that are called `VAPST` for commitment information and `VAPIT`

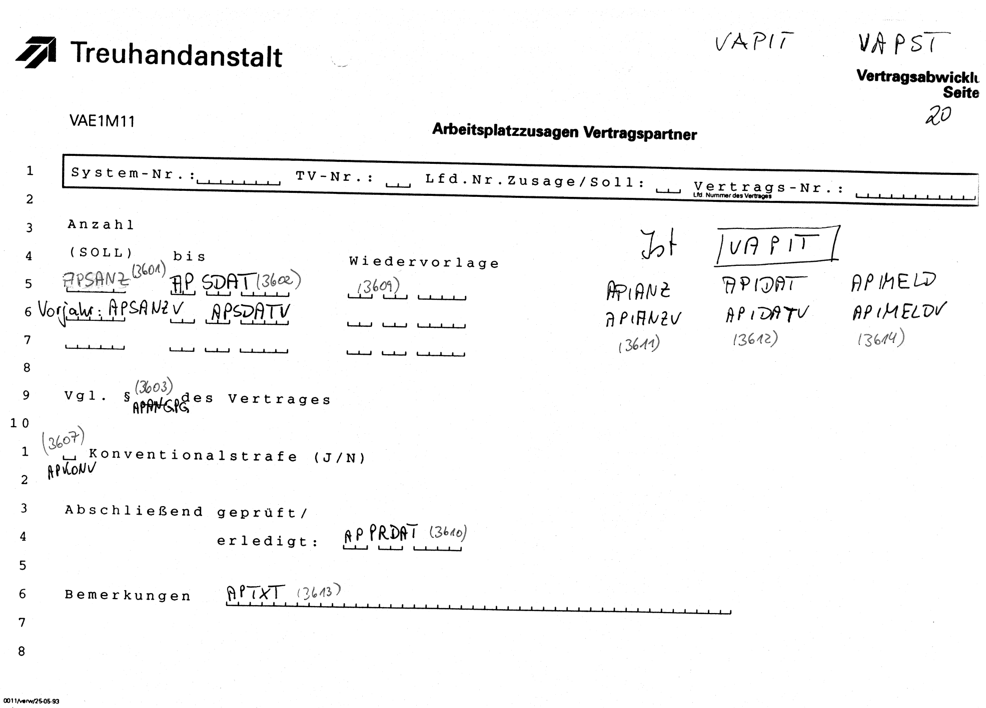
[41] THA created legal entities over time, and, as a result, 5.1% of firms are observed for the first time in 1991, and 1.2% in 1992, and 0.48% in 1993.

Table B.1: Contract-Level Data Tables

Table names	Description
A: Baseline tables	
ASVA01T	The tables contains master data and status information for contracts signed with the THA. It combines many variables from different tables. The table contains the contract ID (sysnr), the date of the contract signed with the notary, and the name of the privatizer. Total number of unique contracts: 256,842.
ASVA02T	The table provides information on partial contracts. It contains the link between the contracts and the firms, the fixed price payed by the contract partner, and the assignment to THA offices. Total number of unique contracts: 213,052. Unique contracts with a non-missing contract-firm link: 22,837.
VATVT	The table provides information on partial contracts. It contains the link between the contracts and the firms. Total number of unique contracts: 37,967. Unique contracts with a non-missing contract-firm link: 30,745.
ASVA22T	This table provides information on mappings. It contains the link between the contracts and the firms. Total number of unique contracts: 40,036. Unique contracts with a non-missing contract-firm link: 9,784.
ASVA50T	This table provides header data for concerted action. It contains the link between the contracts and the firms. Total number of unique contracts: 82. Unique contracts with a non-missing contract-firm link: 82.
FE3_VT	This table provides information on processes/operations of main tables related to financials. It contains the link between the contracts and the firms. Total number of unique contracts: 1,723. Unique contracts with a non-missing contract-firm link: 1,710.
B: Labor Commitments & Audits	
VAPST	This table provides information on labor commitments of the contract partner. Total number of unique contracts: 17,753. Total number of observations: 52,438.
VAPIT	This table provides information on labor audits. Total number of unique contracts: 16,583. Total number of observations: 116,619.
VAPITH	This table provides information on labor audits and is labeled as history in the documentation. Total number of unique contracts: 19,052. Total number of observations: 102,933.
ASVA12T	This table, among others, provides information on labor commitments. Total number of unique overall contracts: 275,054. Total number of unique contracts with positive number of committed labor: 22,535. Total number of observations: 322,829.
ASVA13T	This table, among others, provides information on labor audits. Total number of unique overall contracts: 47,111. Total number of unique contracts with positive number of audited labor: 15,702. Total number of observations: 153,155.
C: Investment Commitments & Audits	
VAZST	This table provides information on investment commitments of the contract partner. Total number of unique contracts: 18,120. Total number of observations: 20,366.
VAZIT	This table provides information on investment audits. Total number of unique contracts: 16,806. Total number of observations: 32,096.
VAZITH	This table provides information on investment audits and is labeled as history in the documentation. Total number of unique contracts: 26,195. Total number of observations: 60,159.
ASVA15T	This table, among others, provides information on investment commitments. Total number of unique overall contracts: 274,375. Total number of unique contracts with positive number of committed investment: 24,220. Total number of observations: 280,370.
ASVA16T	This table, among others, provides information on investment audits. Total number of unique overall contracts: 47,111. Total number of unique contracts with positive number of audited investment: 15,619. Total number of observations: 64,725.

for information on audits (see Panel B of Table B.1). These two tables can be seen as the original tables as suggested from the delivered pdf documentation by the German Federal Archives. The pdf file for labor commitments is shown in Figure B.2. It shows the template how the data was collected in the first place by THA employees. The top right corner corresponds to the tables VAPST and VAPIT, respectively. In these two data tables we observe 17,753 unique contracts with labor commitments and 16,583 contracts with at least one audit. As presented in Panel B, the total number of observations in both tables is higher because there can be multiple commitments for different years of the commitment period as well as several audits per commitment.

FIGURE B.2: PAPER FILE: LABOR COMMITMENT



Treuhandanstalt

VAPIT VAPST

Vertragsabwicklu
Seite
20

VAE1M11

Arbeitsplatzzusagen Vertragspartner

System-Nr.: TV-Nr.: Lfd.Nr.Zusage/Soll: Vertrags-Nr.:

Anzahl
(SOLL) bis Wiedervorlage Ist |VAPIT|
APSANZ (3601) AP SDAT (3602) (3609) APIANZ APIDAT APIMELD
Vorjahr: APSANZV APSDATV APIANZV APIDATV APIMELDV
(3611) (3612) (3614)

Vgl. § (3603) APANGPG des Vertrages

(3607) Konventionalstrafe (J/N)
APKONV

Abschließend geprüft/
erledigt: APPRDAT (3610)

Bemerkungen APTXT (3613)

Notes: The figures show the original template used by the THA to document labor commitments.

We perform the following steps to clean the data. First, we drop observations without date information in both tables and select the first contract within the contract ID in case there are several partial contracts per ID. Out of the 116,619 contract-audit observations, these selection steps reduce the sample by 36 and 674, respectively. Out of the 52,438 contract-commitment observations, these selection steps reduce the sample by 1,414 and 367, respectively. Within the VAPIT file we also drop observations where the number of employees at the audit is zero, but the variable that states whether employee information is reported is set to zero. This reduces the sample further by 2,536 observations. In order to obtain an initial firm size measure at the contract level, we select the first audit. The last audited labor information provides a measure of the size at the final commitment time. We further perform basic data cleaning steps: (i) we drop contracts if the date of the last commitment is before the date of the contract with the notary (7 observations), (ii) if the time between two consecutive commitments is negative, and (iii) if the final employment commitment is zero (224 observations). This generates a sample with 15,538 labor commitment contracts with at least one matched employment audit.

The ISUD environment further contains a table called ASVA12T with labor commitment contracts. The original table has 322,829 observations. The majority of these observations are labeled as having no labor commitments. We compare this data table with the original VAPST table. Conditional on observing one contract ID in both tables (VAPST and ASVA12T) shows that the information is identical. However, ASVA12T has 5,125 additional contracts with labor commitments that are not included in VAPST. These additional contracts are, on average, later written out and are entered into the ISUD data system mainly in 2003 and 2004. After following the same data cleaning steps, we end up with 3,385 additional contracts. In terms of labor audits, however, these contracts are not observed in VAPIT. There exists another data table that is a natural suspect and is called ASVA13T. But again, this table does not contain audit information for the additional contracts with observed labor commitments.[42] After searching for possible contracts with additional audit information, we found that the history version of VAPIT, called VAPITH, is suitable to fill parts of the missing audits from ASVA12T. Among the 3,385 additional contracts after basic data cleaning steps, we are able to merge the audit information for 2,702 contracts. Together, these data tables generate our final sample of 18,235 contracts with labor commitments.

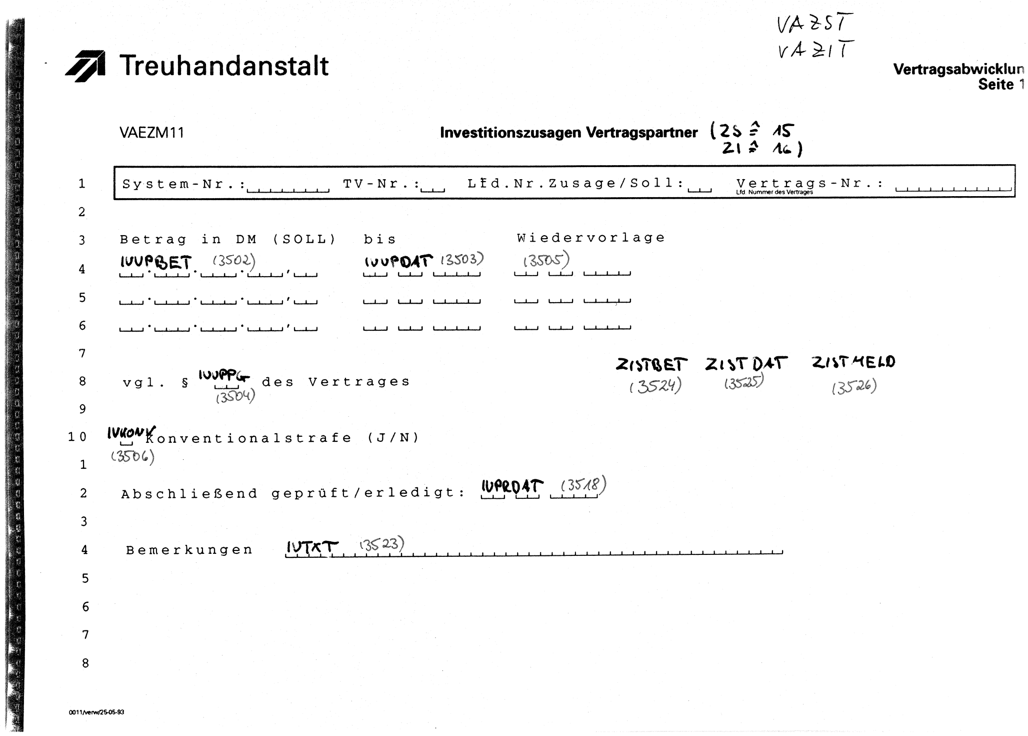
For the empirical specifications accounting for privatizer preferences in other dimensions, we make further use of investment commitment contracts. The logic and steps in the data cleaning process apply similarly to investment commitment contracts. Figure B.3 shows the template used for the documentation of investment commitments. The baseline data table for investment with information on investment commitments is called VAZST, whereas the table for investment audits is called VAZIT. Panel C of Table B.1 provides a list and short description of the investment commitment related data tables.

After basic data cleaning steps and combining commitment information in VAZST with audit information in VAZIT, we obtain a dataset with 15,086 investment commitments. The data table ASVA15T has 7,127 additional contracts that are not observed in the baseline files. Similar to the additional employment contracts, ASVA16T does not contain audits to these additional contracts. Again, exploiting VAZITH, the history file of VAZIT, we are able to add 4,978 contracts. Together, these data tables generate our final sample of 20,062 contracts with investment commitments.

One remarkable difference between investment and labor commitment contracts is the number of audits. While the share of contracts with only one audit is about 17% among the labor commitment contracts, this share is 65.2%. Due to the flow nature of investment commitment, there are fewer audits during the commitment period. Combining labor with investment contracts results in a sample of 23,662 unique contract-level observation. Among them, 14,635 contracts have both, labor and investment commitments, 5,427 only have investment commitments, and 3,600 contracts only have labor commitments. In order to calculate extensive margin preferences i.e., writing contracts with any labor commitment condition we merge this combined dataset with the 58,544 contracts with cleaned privatizer names.

[42]Out of the 5,125 additional contracts with labor commitments ASVA12T, 17 contracts are found in VAPIT and 22 contracts are found in ASVA13T.

Figure B.3: Paper File: Investment Commitment


VA ≥ 5 T
VA ≥ 1 T

Treuhandanstalt

Vertragsabwicklun
Seite 1

VAEZM11

Investitionszusagen Vertragspartner (25 ≙ 15
21 ≙ 16)

System-Nr.: TV-Nr.: Lfd.Nr.Zusage/Soll: Vertrags-Nr.:
Lfd. Nummer des Vertrages

Betrag in DM (SOLL) bis Wiedervorlage
IVVPBET (3502) IVVPDAT (3503) (3505)

ZISTBET ZISTDAT ZISTMELD
(3524) (3525) (3526)

vgl. § IVVPPG (3504) des Vertrages

IVKONV Konventionalstrafe (J/N)
(3506)

Abschließend geprüft/erledigt: IVPRDAT (3518)

Bemerkungen IVTXT (3523)

Notes: The figures show the original template used by the THA to document investment commitments.

C Data Addendum - Merging Contracts to Mannheim Enterprise Panel Data

This section describes the merge between our baseline contract-level data and the Mannheim Enterprise Panel data, which cover firms in East Germany starting from 1993 to 2019 (the most recent wave). The Mannheim Enterprise Panel (MUP), is the most comprehensive micro database of companies in Germany outside of administrative data. Official administrative data is usually not accessible to the public. The data contains detailed information on the firm-level that is often hard to come by in administrative records such as, for instance, the date of creation and closure of a company, ownership structures, and credit rating scores. Besides that, the dataset comprises employment, sales, and industry affiliation information. The MUP is based on the firm data pool of Creditreform e.V., which is the largest credit rating agency in Germany. While it has broad overall coverage it does not offer 100% coverage (for further details, see Bersch, Gottschalk, Müller, and Niefert (2014)).

At the level of the contracts, we do not observe firm names that would allow a string matching based on these names. Instead, we explore the ownership information in both datasets. In the MUP data, we observe for each firm owners. In the contract-level data, we have access to the contract partner, who ususally becomes the new owner of the company after the contract is signed with the notary.

Among the 18,235 contracts in the baseline data, we start off with 9,538 that can be linked via name matching between the owners in the MUP and contract partners in the contract data. These observations correspond to 11,199 contract partners. These individuals usually have multiple links to firms at different points in time and across space. In order to select the correct firm to the contract, we perform the following pre-selection:

- Drop if firm is located in West Germany
- Drop if original firm under Treuhand is located in different Federal State than MUP firm
- Drop if firm/contract location, date of incorporation, contract date is missing
- Drop if date of incorporation/ownership start is after 2000
- Drop if contract date is five years after date of incorporation

The first two selection criteria are based on regional information. We assume that the contract does not belong to the privatized eastern firm or asset if the firm in the MUP dataset is located in West Germany. We also drop observations if the former GDR firm and the MUP firm are located in different Federal States (within East Germany). Moreover, if we do not observe the region, the contract date or the date of incorporation, we drop the entire observation. We also drop observations if the date of incorporation or the start of the ownership period is post 2000. As a last step of the pre-selection procedure, we drop contracts if the contract date is more than five years after the date of incorporation. The reason behind this is that the contract date should mark the creation of a firm and therefore should be close to the date of incorporation. This leaves us with 7,415 contracts and 8,952 contract partners. Per contract partner, we find about two owners with the same name in the MUP data at the median. The 99th percentile corresponds to 51 potential matches, which are rather common names that are matched several times in the MUP data. We therefore exclude the upper decile (more than nine different IDs in the MUP data) of the matches as a further pre-selection step.

With these potential matches at hand, we need to select the firm that matches best. In order to perform the selection and exclude MUP firms that are likely not behind the privatization contract, we construct three indicator variables based on the region (county, state), the dates (incorporation, contract), and the employment deviation. At the regional level, we construct an indicator equal to 1 if the regional information in both dataset coincide. The date indicator is equal to 1 if the absolute difference between the two available dates is at most three years. For the indicator for employment deviation, we first calculate observed employment deviations for all the year where we observe employment numbers in both datasets. It is possible to have more than one observation per firm because audits happen at different points in time. The employment deviation measure, naturally, can only be calculated among contracts with labor commitments. The employment deviation indicator is set to be 1 for the match with the smallest difference.

We then drop potential matches if regional and date information do not coincide with each other. In cases where we only observe date information, we select the MUP firm with the closest date of incorporation to the contract date. If, for example, there are two possible matches of MUP firms in the same region and incorporated in the same year, we need to drop the contract entirely from the sample as we cannot select the bast match. Our final match consists of 4,735 firms with labor commitment contracts that are observed in a panel structure.

Table C.1 provides an overview on the selection criteria. It states that 38% of our matches are based on the exact county, date (date of incorporation and contract date) and audit information. Another 15.5% of the matches are selected based on the Federal State information, the date and audit information. This indicates that slightly more than 50% are based on region, date and employment information available in both dataset. Then, there are some few matches of around 10% that are only based on region and date or region and audit information. About a quarter of the matches are based only on the information of the contract date and the date of incorporation, whereas 2.6% are only based on audit information. Finally, 8.1% of the selected MUP firms are selected because there is only one possible match, i.e., the matched owner has only one firm ID attached.

Given the pre-selection criteria, all observed matches are in the same state. Conditional on non-missing county information, our final matched MUP firms to contracts that come out of former GDR firms are in 73% of all cases located in the same county as the MUP firms. Moreover, the average absolute difference between the date of the contract and the date of the incorporation in the MUP data is 1.12 years (median is equal to 1 year).

Based on the firm-year observation, we are able to merge employment audits from the contract management system of the ISUD environment. Note that in the selection procedure, we have used the match with the smallest deviation. We will now be able to justify the match by studying employ-

Table C.1: Sources of Selected Matches

	Share
Selected based on county, date & audit information	0.378
Selected based on state, date & audit information	0.155
Selected based on county & date	0.016
Selected based on state & date	0.009
Selected based on county & audit information	0.061
Selected based on state & audit information	0.024
Selected based on only state	0.006
Selected based on only date	0.247
Selected based on only audit information	0.026
Selected based only 1 possible merge	0.081

Notes: The table shows the source of selected matches between the ISUD data and the MUP dataset. The majority of selected matches are based on county, date and audit information. About 25% are only selected base on date information and 16% are based on the same state, date and audit information.

ment number differences between the two datasets. For 3,609 firms, we observe at least one audit (with positive employment information) which allows us to calculate employment differences. The median (mean) estimated difference in employment is 0 (2.67). However, we observe large tails in the distribution of employment differences. For this reason, we further drop matches with absolute employment differences above 500. After this adjustment, our sample consists of 4,735 firms. At this stage, we do not drop firms if precise calculations of employment differences are not possible, which means that we rely on date and regional information for the merge.

Figure C.1 provides a comparison between the contract-level employment information and the MUP data. Panel A provides a visualization of count differences with a median of zero (maximum of 500 by construction). Panel B shows the inverse hyperbolic sine transformed employment numbers between the MUP firm-level data in black and the contract-level data in grey. These results suggests that the MUP firm-level data shows slightly more mass among smaller firms.

To evaluate the quality of the match, we calculate the share of firms that are "close" to each other in terms of employment figures. To arrive to such a statement, we first calculate the relative employment differences as:

$$employment_{diff} = \frac{(empl_{MUP} - empl_{ISUD})}{(empl_{MUP} + empl_{ISUD})},$$

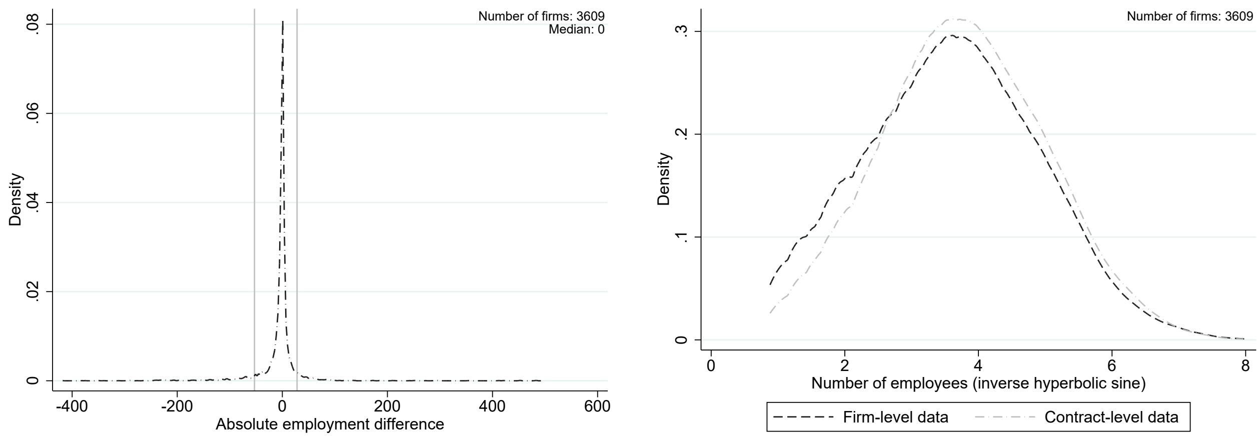
where $empl_{MUP}$ and $empl_{ISUD}$ refer to the respective employment figures in both datasets. We then define a match to be close or acceptable if the employment difference is smaller or equal to following threshold value:

$$abs(employment_{diff}) \leq \frac{1}{\sqrt{(min[empl_{MUP}, empl_{ISUD}] + 1)}}.$$

This equation takes into account the level of employment and allows for higher relative deviations among small firms. To provide an example, consider the following case with $empl_{ISUD} = 1$ and $empl_{MUP} = 3$. This generates a relative employment difference, $employment_{diff}$, equal to 0.5, which is smaller than the threshold value of 0.707 and therefore considered to be close enough to be acceptable.

Figure C.1: Comparison of Employment Figures between Contracts and MUP

A: Employment Differences　　　　B: Employment Distributions

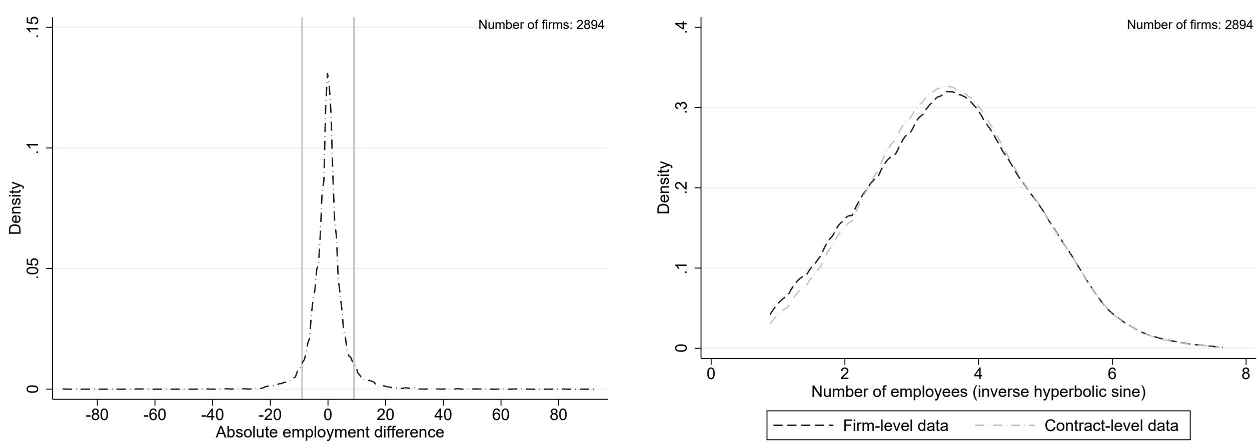


Notes: Panel A shows employment differences between matched contracts and firms in the MUP data that is centered around 0. Panel B shows the log employment distribution of matched contracts and the employment distribution in the MUP dataset. Number of observations with employment information in both datasets is 3,609.

The case where, for example, $empl_{ISUD} = 100$ and $empl_{MUP} = 300$ also provides a measure of $employment_{diff}$ equal to 0.5. However, the threshold value becomes 0.099 and therefore labels this merge as not close enough to be acceptable. Figure C.2 shows the same distributions among firms that are considered to be close i.e., have employment differences below the defined threshold value. At the firm level, 2,894 out of 3,609 firms are below the defined threshold value, which corresponds

Figure C.2: Close Matches between Contracts and MUP

A: Employment Differences　　　　B: Employment Distributions

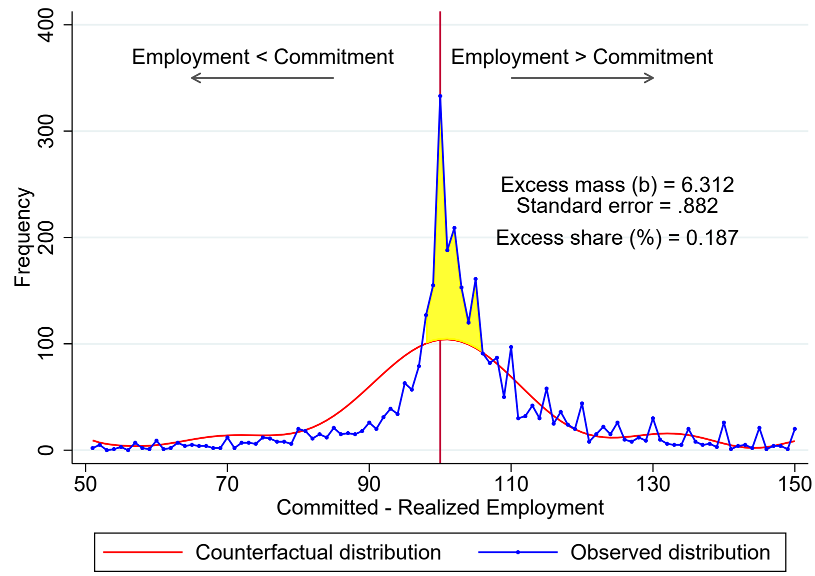


Notes: Panel A shows employment differences between matched contracts and firms in the MUP data that is centered around 0. Panel B shows the log employment distribution of matched contracts and the employment distribution in the MUP dataset. The sample is conditional on fulfilling the threshold rule. Number of observations with employment information in both datasets is 2,894.

to an acceptance rate of 80%. We therefore judge the success of the merge to be relatively high.

Based on this sample, we can re-calculate the employment distribution around the commitment level as shown in Figure 5. Figure C.3 shows the bunching estimate using the employment information in the MUP for the year of the final commitment. We adjust the bunching window slightly by excluding the area of 6 and less above the committed level as well as 1 and 2 employment below the committed level.

FIGURE C.3: EMPLOYMENT DISTRIBUTION AROUND THE COMMITMENT LEVEL USING FIRM-LEVEL DATA



Notes: The figure shows the employment distribution around the committed employment (demarcated by the vertical red line at 0) for firms with matched contracts. The blue line in dots is a histogram of actual employment relative to the commitment target in the final commitment year. Each point shows the number of observations in employment count bin (deviation between the target and the realized employment). The solid line beneath the empirical distribution is a twelve-degree polynomial fitted to the empirical distribution excluding the area of missing two employee and having six employees more than committed. The shaded region in yellow is the estimated excess mass, which is 631% of the average height of the counterfactual distribution beneath. Standard error is calculated using a parametric bootstrap procedure. Estimation based on Chetty, Friedman, Olsen, and Pistaferri (2011).

Similar to the baseline bunching estimates, bunching with the matched sample between the contracts and the MUP is estimated to be 6.312. The estimated standard error is 0.88, indicating a significance level of 1%. This value is, furthermore, relatively close to 6.52 presented in Figure 5. Overall, these results suggest that the merge between the two datasets can be considered highly reliable.

D Data Addendum - Treuhand Firm Survey Data

This section describes how we construct firm-level capital stock and TFP estimates and the merge between our baseline contract-level data and the THA firm survey data. The bi-annual survey was conducted by the the SOESTRA institute with its first wave in April 1991. The survey data has been used and analyzed, among others, by Wahse, Dahms, Schäfer, and Kühl (1996) and Mergele, Hennicke, and Lubczyk (2020).

The focus of the questionnaire was on employment and most of the survey waves also contain questions on firm revenue. Important for our purpose to construct the firm-level capital stock is the fact that some waves also contain information on investments. Apart from these main variables, the survey contains baseline information on the sector affiliation, the location of the firm, and end dates of THA ownership and labor commitments (if any). Out of these waves, we first construct an (unbalanced) monthly firm panel between 1991 and 2000. This initial panel contains 11,105 Treuhand firms.

D.1 Constructing Firm-Level Capital Stock and TFP Measures

The first aim is to convert the monthly panel into a yearly panel. Out of 36,735 revenue observations over the years between 1991 and 2000 belonging to 9,596 firms, 69% of the information belongs to an end-of-year question. Thus, most of the revenue information is related to a full calendar year. Further, 15% of the revenue questions ask for revenue numbers during the first half of the year, and the remaining belongs either to the first quarter of the year (9.4%) or to the third quarter of the year (6.6%). Likewise, the survey covers 17,896 investment information belonging to 6,743 firms. The majority of 95.3% of the investment numbers are related to the full calendar year, and the remaining 4.6% relate to the first six months of the year. Therefore, we harmonize the data to the yearly level by assuming linearity e.g., if we only observe revenue/investment information for the first six month of the year, we multiply by 2 to construct the number for the year. In most cases, however, information are typically available for the full year and for a fraction of the year. We finally impute for 652 firms revenue information and for 834 firms investment information to the end of the year. Regarding employment, we construct the average employment level out of the monthly information. We complement the survey data on yearly employment and revenue with `basis_kennziffern` as described in Appendix B.

The initial capital stock is constructed using balance sheet information submitted by the firms for the year 1990. The data table is called `DM_BIL_N`. The initial capital stock consists of tangible assets, including mainly properties, (technical) equipment, and machinery. These tangible assets represent 97% of the initial capital stock. The remaining fraction comes from breeding stock, concessions, and soil improvement. Initial capital stock information is available for 7,182 firms. We then clean the dataset and drop firms entirely if the firm does not have a single employment or sales information, which drops the initial sample of 11,105 firms to 10,390 firms. In the occurrence that employment and revenue information within the firm contain gaps, we linearly impute these gaps of up to two years.

In order to calculate the yearly capital stock at the firm level, we start with the initial capital stock measured in 1990, add investments, and assume a 10% depreciation rate. All Deutsch Mark (DM) values are deflated by the CPI measured in 2016 prices. The capital stock can only be estimated if investment information is available. Table D.1 shows in column (4) that the question on investment primarily exists for the years between 1992 and 1995. Coverage is particularly low towards the end of the sample period and in 1991. For example, there are only 560 firms with full investment information between 1991 and 1994, and only 160 always have investment numbers. Likewise, but to a lower extent, column (3) shows the number of firms with revenue information. In the first two years, around 98% of all firms do have information on revenue, whereas this share decreases to 65% in 2000.

Table D.1: Actual and Imputed Investment Information

Year (1)	N (2)	N with investment (3)	N with imputed investment (4)
1991	6,764	682	5,767
1992	6,764	3,572	3,130
1993	6,707	2,428	3,694
1994	6,583	1,364	4,198
1995	6,003	1,145	2,962
1996	5,187	500	2,383
1997	4,369	535	1,963
1998	3,633	555	1,447
1999	2,711	515	1,293

Notes: The table shows the number of firms in the final survey data as well as the number of firms with actual and imputed revenue and investment information.

To construct the capital stock, we first employ a machine-learning assisted imputation approach by predicting investment numbers and use the predicted values in case actual numbers are missing. We employ a standard least absolute shrinkage and selection operator (lasso) with an optimal tuning parameter using a 10-fold cross-validation. The covariates used in the baseline lasso regression include revenue and employment, both measured in size bins and 259 4-digit sector dummies. We perform the prediction exercise separately for every year. We provide the results for the investment imputation also using ln(employment) and ln(revenue) as well as these variables introduced with a second degree polynomial. Due to the fact that Treuhand firms got restructured (to different degrees) until privatization, we also use a proportional imputation approach. For this approach, we approximate the initial capital stock by mimicking the faction of employment at privatization relative to the initial firm size. For example, if a firm gets privatized with 50 employees and the initial firm size in 1990 was 500 employees, we assume the initial capital stock to be 10% of the actual capital stock measured in 1990.

Table D.2 provides baseline information for each lasso specification measuring employment and revenue in bins. Specifically, we introduce 11 employment size bins [1-4; 5-19; 50-99; 100-149; 150-249; 250-499; 500-749; 740-1449; 1450-2999; 3000+] and 9 (ln) revenue size bins [<12.51356; 12.51356-13.26366; 13.26366-14.36855; 14.36855-15.50374; 15.50374-16.67438; 16.67438-17.80855; 17.80855-18.57818; 18.57818-20.10738; 20.10738+]. The number of non-zero covariates decreases as the sample size de-

Table D.2: Lasso Results: ln(investment)

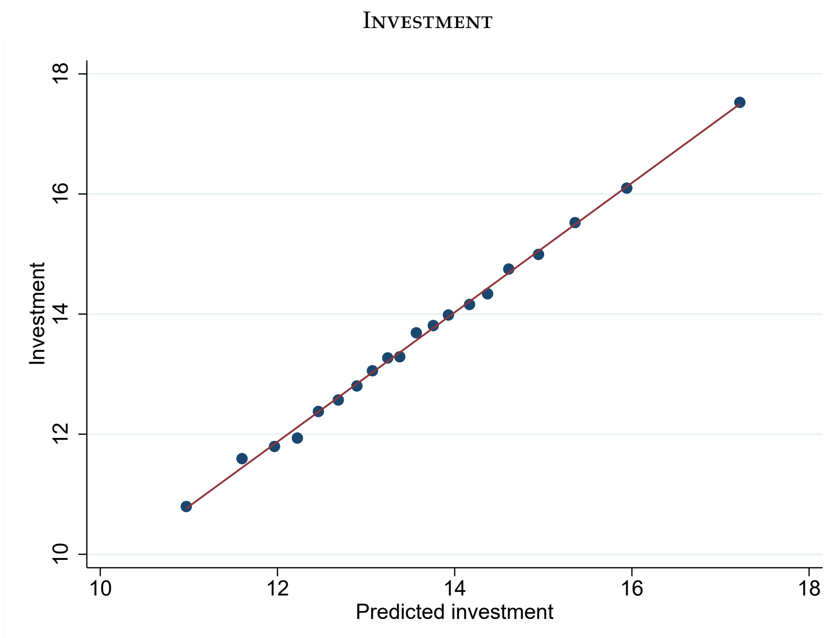
	N	Optimal lambda	Number of non-zero coefficients	Cross-validated minimum prediction error
	(1)	(2)	(3)	(4)
1991	4,908	0.015	163	2.131
1992	3,665	0.021	142	2.174
1993	2,112	0.032	99	2.041
1994	1,864	0.035	95	2.294
1995	750	0.057	65	2.322
1996	825	0.038	78	2.077
1997	851	0.039	94	2.046
1998	833	0.046	69	2.099
1999	739	0.033	86	1.848

Notes: The table shows summary results from yearly lasso regressions with ln(investment) as the outcome variable.

creases, indicated by a higher optimal cross-validated penalty parameter.

Figure D.1 shows actual vs predicted investment numbers pooled over the whole time period. On average, actual and predicted numbers line up at the 45 degree line. Based on these predictions, we impute investment information in case actual investment information is missing and the selected covariates are not missing. Column (4) of Table D.1 shows the number of imputed observations over time.

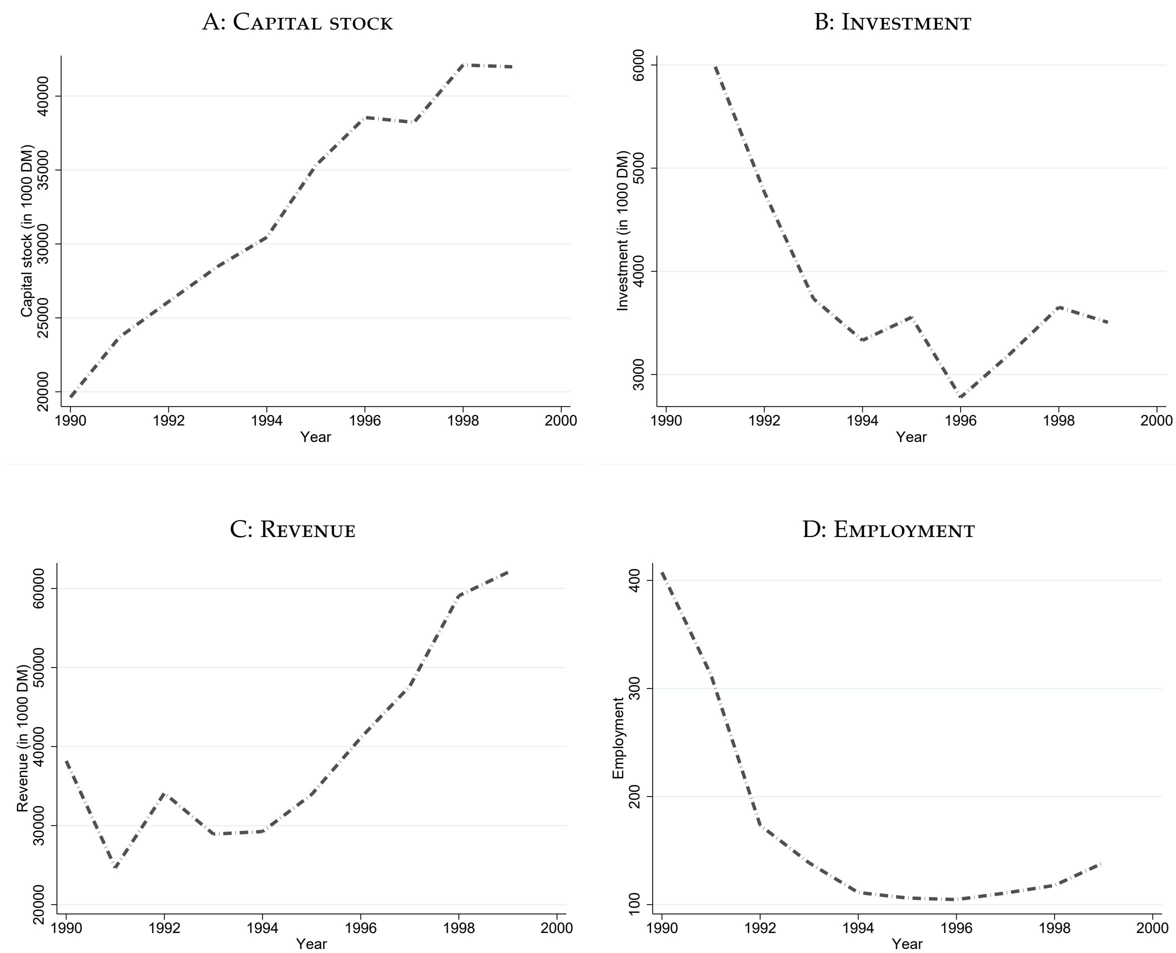
Figure D.1: Correlation Actual and Predicted Values



Notes: The figure plots actual vs. predicted investment numbers pooling all years between 1991 and 1999 with the cross-validated lambda.

In a next step, we construct the capital stock at the firm level starting with the initial capital stock in 1990 and add these (actual and imputed) investment numbers and subtract a 10% depreciation rate. Figure D.2 provide firm-level averages over the period between 1990 and 1999. Although these numbers might not be representative for the East Germany economy due to selectivity and panel attrition, the panels A and C of the figure show an increasing trend in the constructed capital stock measure and firm revenue. Average investment amounts decrease over time ,indicting a disproportional high investment need. Average firm-level employment decreases over time. The drop in firm level employment is consistent with total employment in the economy, with the largest decreased happening between 1990 and 1991.

Figure D.2: Main variables used from Soeastra firm survey



Notes: The figures plot average firm-level capital stock, investment, revenue, and employment numbers between 1991 and 1999.

In a next step, we aim to construct a measure of total factor productivity (TFP). Due to the fact that we have no information on intermediate inputs such as material, we run a simple Cobb-Douglas regression specification for each year, with input factors being firm-level employment and the constructed measure of capital. Output is measured by revenue. All variables are deflated by the CPI.

Specifically, we estimate

$$y_i = \alpha + \beta_l l_i + \beta_k k_i + \epsilon_i$$

where y_i is the logarithm of the firm's output, in our case, revenue. l_i and k_i are the logarithm of the firm inputs, in our case, the number of employees and the capital stock. We construct TFP as $\omega_i = exp(y_i - \hat{\beta}_l l_i - \hat{\beta}_k k_i)$. Table D.3 provides the regression results separately for each year between 1991 and 1999. In Panel A, we provide the results using the baseline imputation approach of firm-

Table D.3: Regression Results: ln(revenue)

	1991	1992	1993	1994	1995	1996	1997	1998	1999
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: baseline, covariates: employment & revenue dummies									
Ln(Empl.)	0.4899***	0.6679***	0.6937***	0.6482***	0.6683***	0.5895***	0.6070***	0.6887***	0.7290***
	(0.020)	(0.015)	(0.013)	(0.012)	(0.014)	(0.019)	(0.019)	(0.023)	(0.025)
Ln(Capital)	0.4582***	0.3497***	0.3454***	0.3859***	0.3654***	0.4019***	0.4407***	0.4093***	0.4002***
	(0.018)	(0.015)	(0.014)	(0.014)	(0.014)	(0.018)	(0.019)	(0.021)	(0.023)
N	6,448	6,449	5,823	5,251	3,847	2,677	2,296	1,771	1,502
R^2	0.560	0.538	0.577	0.583	0.623	0.597	0.634	0.672	0.699
Panel B: covariates: ln(employment) & ln(revenue)									
Ln(Empl.)	0.4413***	0.6505***	0.6832***	0.6378***	0.6622***	0.5794***	0.5977***	0.6796***	0.7251***
	(0.020)	(0.016)	(0.013)	(0.012)	(0.014)	(0.019)	(0.020)	(0.023)	(0.026)
Ln(Capital)	0.4927***	0.3613***	0.3502***	0.3897***	0.3634***	0.3988***	0.4347***	0.4056***	0.3927***
	(0.018)	(0.015)	(0.014)	(0.013)	(0.014)	(0.018)	(0.018)	(0.021)	(0.022)
N	6,448	6,449	5,823	5,251	3,847	2,677	2,296	1,771	1,503
R^2	0.566	0.542	0.580	0.587	0.625	0.599	0.635	0.673	0.699
Panel C: covariates: ln(employment) & ln(revenue) with second polynomial order									
Ln(Empl.)	0.4293***	0.6509***	0.6843***	0.6383***	0.6628***	0.5805***	0.5996***	0.6763***	0.7214***
	(0.020)	(0.016)	(0.013)	(0.012)	(0.014)	(0.019)	(0.019)	(0.023)	(0.026)
Ln(Capital)	0.5007***	0.3603***	0.3499***	0.3909***	0.3633***	0.3947***	0.4300***	0.4092***	0.3984***
	(0.018)	(0.015)	(0.014)	(0.013)	(0.014)	(0.018)	(0.018)	(0.021)	(0.023)
N	6,439	6,440	5,816	5,244	3,840	2,669	2,290	1,766	1,499
R^2	0.563	0.538	0.577	0.584	0.621	0.592	0.629	0.671	0.699
Panel D: baseline with proportional initial capital stock									
Ln(Empl.)	0.3012***	0.5456***	0.6002***	0.5460***	0.5764***	0.4876***	0.4872***	0.5961***	0.6467***
	(0.022)	(0.020)	(0.017)	(0.015)	(0.017)	(0.024)	(0.024)	(0.029)	(0.033)
Ln(Capital)	0.6537***	0.4454***	0.4109***	0.4728***	0.4569***	0.4836***	0.5351***	0.4759***	0.4508***
	(0.020)	(0.021)	(0.018)	(0.018)	(0.018)	(0.023)	(0.024)	(0.027)	(0.029)
N	4,279	4,280	4,004	3,683	2,748	1,919	1,649	1,283	1,063
R^2	0.600	0.528	0.569	0.572	0.622	0.579	0.618	0.653	0.671
Mean revenue	15.33	15.474	15.434	15.55	15.66	15.744	15.684	15.602	15.556
Mean employment	4.636	3.966	3.676	3.484	3.394	3.338	3.436	3.526	3.714
Mean capital	15.674	15.748	15.81	15.878	15.95	15.948	15.922	15.906	15.85

Notes: The table shows production function estimation results of ln(revenue) for each year between 1991 and 1999 with inputs ln(employment) and ln(capital). Different panels indicate different lasso specifications to impute investment for constructing firm-level capital stock. Panel A (baseline) uses as covariates group size bins in employment and revenue. Panel B uses as covariates ln(revenue) and ln(employment). Panel C uses as covariates ln(revenue) and ln(employment) with a polynomial degree of order 2. Panel D uses as covariates the baseline revenue and employment introduced with size dummies. All lasso specification include 259 4-digit sector dummies.

level investment. Except for the first and the last year of the sample, we estimate β_l to be around 0.65 and β_k to be around 0.35. Towards the end of the sample, both coefficients increase with a significant decrease of the size of the sample. The estimates' elasticities in the year 1991 are rather of equal size, and both are below 0.5. This might be the results of distorted firm sizes under socialism.

Panels B and C provide the estimation results for the different lasso specifications. Panel D provides the results with the baseline imputation procedure using the proportionality approximation of the initial capital stock. While Panels B and C show rather similar results, Panel D shows that β_k is higher by a magnitude of around 0.1, whereas β_l is lower by about the same magnitude. The reason might be that the imputed investment numbers are relatively large, relative to the approximated initial capital stock, which increases the elasticity of capital in the production function estimation.

D.2 Merging Contracts to Treuhand Firm Survey Data

The section describes the linkage between the contracts and the survey data. This combined dataset allows us to estimate the effects of binding labor commitment contracts on TFP growth. The main challenge of linking the two datasets come from the fact that the survey data covers initial firm units, whereas the contracts might belong to only part of the firm assets. This becomes apparent because we observe multiple contracts within initial Treuhand firms.

The initial firm survey sample covers 11,105 Treuhand firms with information on employment, revenue, and investments measured at different points in time at the monthly level. The ISUD data environment contains 47,322 contracts merged to 10,023 Treuhand firm IDs. In order to select the contracts that belong to the legal unit of the Treuhand firm, we merge contracts with labor commitments at the level of the Treuhand firm ID and month of the year. For example, in the case of two labor commitment contracts belonging to the same initial Treuhand firm, we can compare employment information from the survey and the audits and select the best match.

Similar to Appendix Section C, we calculate the relative employment differences as:

$$employment_{diff} = \frac{(empl_{survey} - empl_{ISUD})}{(empl_{survey} + empl_{ISUD})},$$

where $empl_{survey}$ and $empl_{ISUD}$ refer to the respective employment figures in both datasets and keep the contract with the smallest absolute deviation. In addition, we drop matched pairs if the absolute difference in both employment numbers is above 1000 employees (30 observations) and also drop 71 observations because two or more contracts generate the same deviation in employment, making it impossible to select the correct one. This generates a sample of 5,221 Treuhand firms with selected labor commitment contracts.

To judge the success of the linkage, we define a match to be close or acceptable if the employment difference is smaller or equal to the following threshold value:

$$abs(employment_{diff}) \leq \frac{1}{\sqrt{(min[empl_{survey}, empl_{ISUD}] + 1)}}.$$

Out of the 5,221 linked contracts, 73.07% fulfill this condition.

We combine this dataset with the TFP measure at the firm level calculated and described in Section D.1. At the contract level, we merge information related to the labor commitment (first and last labor audit information including the timing, the final commitment level, the date of the contract signed with the notary) and related to the contract in general (privatizer information, THA office information, sales price, investment target). This generates a sample of 2,185 firms with information on the change in TFP between the initial contract year and the final year of the labor commitment.