Committing to Grow: Privatizations and Firm Dynamics in East Germany*

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Abstract

This paper investigates a unique policy designed to maintain employment during the privatization of East German firms after the fall of the Iron Curtain. The policy required new owners of the firms to commit to employment targets, with penalties for non-compliance. Using a dynamic model, we highlight three channels through which employment targets impact firms: distorted employment decisions, increased productivity, and higher exit rates. Our empirical analysis, using a novel dataset and instrumental variable approach, confirms these findings. We estimate a 22% points higher annual employment growth rate, a 14% points higher annual productivity growth, and a 3.6% points higher probability of exit for firms with binding employment targets. Our calibrated model further demonstrates that without these targets, aggregate employment would have been 15% lower after 10 years. Additionally, an alternative policy of productivity investment subsidies proved costly and less effective in the short term.

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

JEL classification: D22, D24, J08, L25.

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1 Introduction

Industrial policy is often designed during times of significant structural change caused by shocks related to competition (e.g., China shock), disruptive innovations (e.g., IT revolution), or political turbulence (as seen in the post-Soviet era in Eastern Europe). During these periods, policymakers consider the immediate costs of labor market disruption and how to mitigate them by introducing policies centered around employment considerations. However, there remains a lack of evidence on how such interventions dynamically affect reallocation and firm behavior.

In this paper, we study a novel policy designed to preserve employment during the privatization of East German firms following the fall of the Iron Curtain. The privatization process represented a period of intense structural change and raised significant concerns about the social costs associated with high unemployment. In response, policymakers required that new owners of East German firms 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 analysis proceeds in three steps. First, we introduce a dynamic model where firms operate under employment targets. An important feature of the model is that firms invest resources to improve their productivity, allowing us to study the endogenous response of productivity to such targets. The model highlights three channels through which a firm is affected by an employment target that is *binding* i.e., in which the target is higher than the current employment level. The first channel stems from the firm's static labor decision, which induces an upwardly distorted employment choice. The second channel arises dynamically as binding targets induce higher productivity growth. This is because more productive firms hire more workers in our model, and this structure creates additional incentives to invest in productivity improvements to avoid the penalties. These two channels imply that firms with binding employment targets experience higher employment and productivity growth. The third channel operates through the extensive margin choice of the firm to exit. Firms with binding employment targets are more likely to exit as binding targets introduce a fixed-cost-like structure in the cash flow of the firm.

In the second step, we take these predictions to the data. The 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. Our data contains detailed contract-level information on employment targets and deadlines, as well as the dates and results of each on-site audit of the employment commitment. To measure firm-level productivity, we merge our contract-level information with data from the Mannheim Enterprise Panel (MUP) and the SOESTRA survey of East German firms.

The empirical identification of the link between employment targets and firm dynamics is a challenging task. The reason is that employment targets are not randomly allocated and might thus bias our empirical estimates. 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. 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 also 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.

Consistent with the model's predictions, we find that binding labor targets are associated with higher employment and productivity growth, as well as increased firm exit over the labor commitment period. Our IV estimates reveal a 22% points higher annual employment growth rate for firms with binding labor contracts compared to those without. Binding labor contracts also lead to an additional yearly productivity growth of approximately 14% points. Additional evidence based on firms' patenting activity during the commitment period also supports these findings. Furthermore, firms with binding contracts exhibit, on average, a 3.6% points higher probability of exiting by the end of the commitment period. Relative to the baseline exit rate of 5.5%, this represents an economically sizable increase in the exit margin. We show that these results are robust to alternative specifications in terms of the measurement of the dependent variables, the construction of the instrumental variable, and the inclusion of additional contractual characteristic as control variables.

In the last step of our analysis, we calibrate our model to the data and run several counterfactual scenarios in order to quantitatively assess the importance of the different channels on firm behavior. 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 as well as the post-commitment employment dynamics, which are not targeted in the calibration process.

We study three counterfactual economies. We first simulate an economy without employment targets and find that aggregate employment would be 15% points lower permanently after 10 years. Next, we decompose the impact of employment targets on total employment into its "static" and "dynamic" components by shutting down its impact on productivity improvements. Our calibrated model attributes one-third of the employment growth to dynamic effects in the short run. In the long run, the entire permanent increase in employment is driven by the 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 during the commitment period. The implied cost of such a policy is high, amounting to 5% of the output. While this policy results in higher permanent employment levels relative to the employment target policy, the increase is more gradual over the commitment period. In other words, the subsidy policy is less effective in the short run to preserve employment.

The paper contributes to the recent literature revisiting the merits and costs of industrial 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. Lane (2022) shows that this temporary drive shifted Korean manufacturing into more advanced markets, creating durable industrial change. Choi and Levchenko (2021) link the associated firm-level subsidies to persistent effects on firm size due to a combination of learning-by-doing and financial frictions. Kalouptsidi (2018) and Barwick, Kalouptsidi, and Zahur (2021) study the Chinese intervention in the shipbuilding industry. Kalouptsidi (2018) estimates that policy interventions reduced shipyard costs by 13-20% and reallocated international market shares. Barwick, Kalouptsidi, and Zahur (2021) disentangle the various subsidies during the intervention and estimates their impact. 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. Finally, 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.[1] Similar to these studies, we leverage unique historical microdata to develop identification strategies and causally estimate the dynamic impact of an industrial policy. At the same time, the type of policy we analyze is unique, insofar as it primarily puts constraints on firms as opposed to using subsidies. By temporarily constraining firms to keep a larger size, the policy produces strong incentives to improve productivity.[2]

The paper contributes to the important and long-standing debate on state versus private ownership (Megginson and Netter 2001). From the point of view of economic efficiency, the case for privatization lies in the concentration of control rights and cash flow rights in the hands of outside investors. In this way, a firm's new owners are provided with optimal incentives to discipline management and restructure activities (Barberis, Boycko, Shleifer, and Tsukanova 1996; Djankov and Murrell 2002; Dyck 1997; López-de Silanes 1997). At the same time, the case for privatisation leaves open the possibility for a welfare-maximizing government to address social and strategic goals through contracting and regulation (Shleifer 1998; Besley and Ghatak 2001). Our paper provides novel evidence on this latter point, as we study how the German government used a contractual approach to force buyers to internalize concerns about employment.

The paper also contributes to the recent literature on the consequences of size-dependent regu-

[1] Glitz and Meyersson (2020) studies how industrial espionage by East Germany affected TFP gaps with West Germany. See also Harrison and Zaksauskienė (2016) for a study of the role of the secret police as a market regulator in Soviet Lithuania.

[2] Note that 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 (Criscuolo, Martin, Overman, and Van Reenen 2019; Greenstone, Hornbeck, and Moretti 2010; Kline and Moretti 2014). See Juhász, Lane, and Rodrik 2023 for a comprehensive literature review on developments in the economics of industrial policy.

lations 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 2023 and Akcigit, Alp, Akgunduz, Cilasun, and Quintero 2023.[3] We contribute to this literature by analyzing a unique policy creating distortions that are not only firm-specific 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 exploit this unique institutional setting to provide a comprehensive evaluation of its dynamic impact on firm employment, productivity and exit.

Finally, our paper contributes to the understanding of the transition of former Eastern Block economies and the process of economic convergence with Western countries (De Loecker and Konings 2006).[4] Similar to other Eastern European countries, East Germany started out with a lower level of a variety of macroeconomic indicators such as economic freedom, GDP per worker, nominal wages, and labor productivity (Lipschitz and McDonald 1990; Akerlof, Rose, Yellen, Hessenius, Dornbusch, and Guitian 1991; Fuchs-Schündeln, Nicola and Schündeln, Matthias 2020). Over the years following reunification, the Eastern German economy started to converge in many dimensions. The process of economic convergence studied in the literature ranges from its implications for management practices (Dyck 1997), to labor reallocation (Dauth, Lee, Findeisen, and Porzio 2021; Fuchs-Schündeln and Schündeln 2005) and 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 ties (Alesina and Fuchs-Schündeln 2007; Burchardi and Hassan 2013).[5] We show in this paper that the implementation of strategic goals – in the form of labor commitments – set by the government can contribute to higher productivity growth by generating a dynamic catch-up in productivity of firms.

[3]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.

[4]Johnson and Papageorgiou (2020) provide a recent survey about the large literature in economic convergence between countries in general.

[5]The convergence process started to level off and stagnate at the end of the 1990s. This observed non-convergence over the last 20 years received increasing attention in the literature. Snower and Merkl (2006) emphasize the role of government transfers in explaining persistent unemployment gaps between East and West Germany, while Burda (2006) argues for capital accumulation frictions as a driver of slow labor productivity convergence. 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.

The remainder of this paper is organized as follows. Section 2 provides an overview of the German institutional framework after reunification. Section 3 introduces a simple model of firm growth to study the dynamic implications of employment targets. In Section 4, we describe the data and provide descriptive statistics. Section 5 reports our empirical results. The structural model and the quantitative estimation are shown in Section 6. Section 7 concludes.

2 Institutional Background

Established in March 1990 under the last Communist regime in East Germany, the THA was given greater authority in July 1990 through the *Treuhandgesetz*.[6] 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.

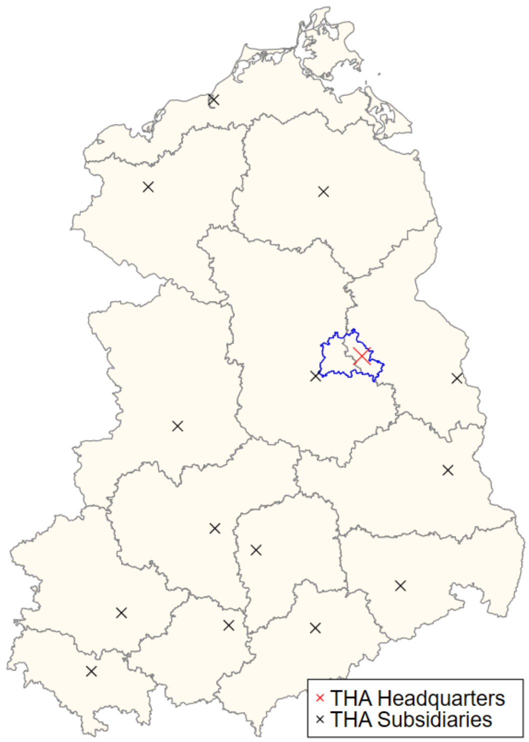
THA itself built up rapidly from an initial staff of about 200 mostly East German employees to an institution of around 4,000 employees plus 800 full-time consultants. These people were divided approximately equally between the central office in Berlin and the 15 branch offices distributed among the major cities of the new federal states. Figure 1 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).[7]

The THA utilized direct cash sales to privatize assets, which included both privatizations of entire firms and divestitures/spin-offs resulting from firm restructuring and liquidation. Sales contracts were structured to include the sales price as well as potential guarantees made by purchasers regarding minimum levels of employment and investment. The imposition of employment targets reflected 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 1991, the first president of THA, Detlev K. Rohwedder, was assassinated. Similarly, after the THA closed the

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

[7] 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 1: 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.

former VEB Kaliwerk Bischofferode, the employees went on an 81-day-long hunger strike (Bernhard 2011).

The employment commitment typically consisted of an agreed number of full-time equivalent jobs that should be maintained for an agreed period of time. This 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 1993). While these targets could result in discounts on the sales price, the valuation process for these commitments varied for each case, and there was no fixed formula followed by the THA (Dodds and Wächter 1993). 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 employment target was subsequently monitored through multiple audits organized within the contract management system of the THA organization.

3 A Model of Firms with Employment Commitments

In this section, we build a simple model of firm growth to study the implications of such employment targets implemented by THA. 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 grow through improving 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 labor to hire for production, (ii) how much to invest in improving firm productivity, and (iii) whether to exit the economy or not. We further assume that labor supply is perfectly elastic and wage growth is exogenous.[8]

3.1 Static Environment

Firms are endowed with a production technology that features decreasing returns to scale with respect to 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. 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. In what follows, we drop the time subscript t whenever it does not cause any confusion.

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

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

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

$$\Pi(z_j, l_j^*) = \max_{l_j \geq 0} \left\{ z_j^{1-\alpha} l_j^{\alpha} - w l_j - \gamma (l_j^* - l_j)^+ w \right\}.$$

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

Lemma 1 The optimal labor decision for a firm with productivity level z and employment target l^* is given by

$$l(\tilde{z}, l^*) = \begin{cases} \alpha^{\frac{1}{1-\alpha}} \tilde{z} & if\ \tilde{z} > \tilde{z}_* \equiv \frac{l_*}{\alpha^{\frac{1}{1-\alpha}}}\ (Undistorted) \\ \left(\frac{\alpha}{1-\gamma} \right)^{\frac{1}{1-\alpha}} \tilde{z} & if\ \tilde{z} < \tilde{z}_{**} \equiv \frac{l_*}{\left(\frac{\alpha}{1-\gamma} \right)^{\frac{1}{1-\alpha}}},\ (Distorted,\ No\ Bunching) \\ l_* & if\ \tilde{z}_{**} \leq \tilde{z} \leq \tilde{z}_*,\ (Distorted,\ Bunching) \end{cases} \tag{1}$$

[8] We think this is a reasonable assumption 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.

and the implied profits are $\Pi(\tilde{z}, l^*) = \pi(\tilde{z}, l^*)w$ where

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

where $\tilde{z} \equiv z/w^{\frac{1}{1-\alpha}}$ is the normalized productivity level with respect to the wage rate.

The above lemma suggests that firms' optimal employment choice and the resulting profits are positively correlated with the level of (relative) productivity, $\tilde{z}$, and can be characterized based on three productivity regions. For high-productivity firms, $\tilde{z} > \tilde{z}_*$, the labor choices are not distorted, since the employment target is not binding. Firms with intermediate levels of productivity, $\tilde{z}_{**} \leq \tilde{z} \leq \tilde{z}_*$, decide to bunch at the target employment, which is higher than their optimal level of employment if there were no employment targets. Finally, low-productivity firms, $\tilde{z} < \tilde{z}_{**}$, find it too costly to operate at the target level of employment, but still their labor choices are distorted towards the target level. These labor choices underline the first channel through which firms with binding contracts i.e., having an employment target larger than the optimal employment level under no target experience a higher employment growth through the contract period, as their employment is simply distorted upward towards the target. We refer to this channel as the "direct" effect of binding labor contracts on employment growth.

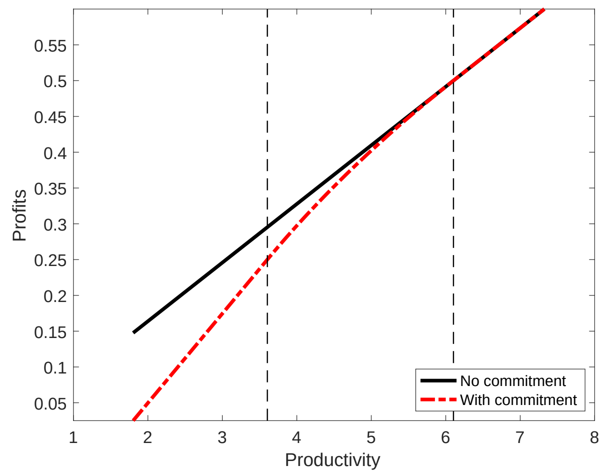
Figure 2 illustrates key implications of the existence of employment targets on firm profits. The left panel plots total profits with respect to firm-level productivity. The black line provides the benchmark for firms with no commitment, while the dashed red line plots profits for firms under commitment. Dashed vertical lines show the threshold productivity levels, z_* and z_{**}. The plot shows that distorted firms have lower profits. As Equation 2 clarifies, these lower profits are attributable to the presence of penalties that introduce a fixed-cost-like structure. These penalties increase 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.

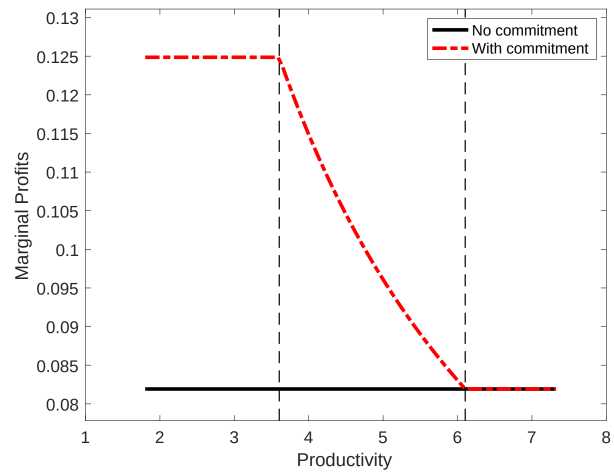
The right panel of Figure 2 plots *marginal* profits across firm productivity and shows that distorted firms have a higher marginal profit with respect to productivity. This is intuitive: increasing productivity not only increases profits but also reduces the amount of distortion (if the firm is bunching) or penalty paid (if the firm is not bunching) for those firms with binding contracts. In a dynamic setting, which will be introduced later, 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. Since the labor choice is positively correlated with the level of productivity, this higher productivity growth by binding-contract firms constitutes the second channel for higher employment growth through its dynamic implications on productivity growth.

FIGURE 2: PROFITS ACROSS FIRM PRODUCTIVITY

PANEL A: TOTAL PROFITS

PANEL B: MARGINAL PROFITS





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_{**}.

3.2 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. 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.

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

[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.

$$rV(\tilde{z},l_*) - \frac{\partial V(\tilde{z},l_*)}{\partial t} = \max\left\{ -C_e w, \max_{x\geq 0} \begin{bmatrix} \pi(\tilde{z},l_*)w - \frac{\phi}{2}x^2\tilde{z}w \\ +x\left[V(\tilde{z}(1+\lambda),l_*) - V(\tilde{z},l_*)\right] \\ +\mu\left[V(\tilde{z},0) - V(\tilde{z},l_*)\right] \end{bmatrix} \right\} \tag{3}$$

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.[10] 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 labor commitment contract 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 by (the arrival rate of improving productivity):

$$x(\tilde{z},l_*) = \frac{V(\tilde{z}(1+\lambda),l_*) - V(\tilde{z},l_*)}{\phi\tilde{z}^{\frac{1}{1-\alpha}}} \tag{4}$$

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.

3.3 Taking Stock

We finish the model discussion by summarizing the main insights and predictions. Our model clarifies three channels through which binding employment commitments affects firm decision. The first channel stems from the firm's static labor decision under binding contracts, which induces an upwardly biased employment choice (equation 1) i.e., *labor hoarding* towards the target employment level. This results in a transitory expansion in employment, with firms reverting to their undistorted size once the policy expires.[11] The second channel arises dynamically as firms operating under these binding commitments witness higher productivity growth induced by higher marginal profits fostering investment in productivity improvements. This dynamic effect arises as firms seek to "escape" from contractually imposed penalties. Unlike the transitory nature of the first effect, the employment gains resulting from these dynamic improvements in productivity are persistent and continue beyond

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

[11]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.

the commitment period. The third channel operates through the extensive margin choice of the firm to exit. Firms with binding employment targets are more likely to exit over the commitment period as binding targets introduce a fixed-cost-like structure in the cash flow of the firm.

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

4 Data and Descriptive Statistics

4.1 Contract Data

The analysis relies 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. Appendix Section B provides a detailed description of the explored ISUD data tables.

The dataset contains 18,235 contracts with labor commitments. For each contract, we observe the contract date e.g., the date the contract is signed with a notary and the committed level of employment along with the date of the final commitment. As shown in Panel A of Figure 3, 90% of employment commitments are signed between 1991 and 1994.[12] In total, all labor contracts amount to more than 900,000 committed workers, representing about 20% of the initial workforce population of THA firms.

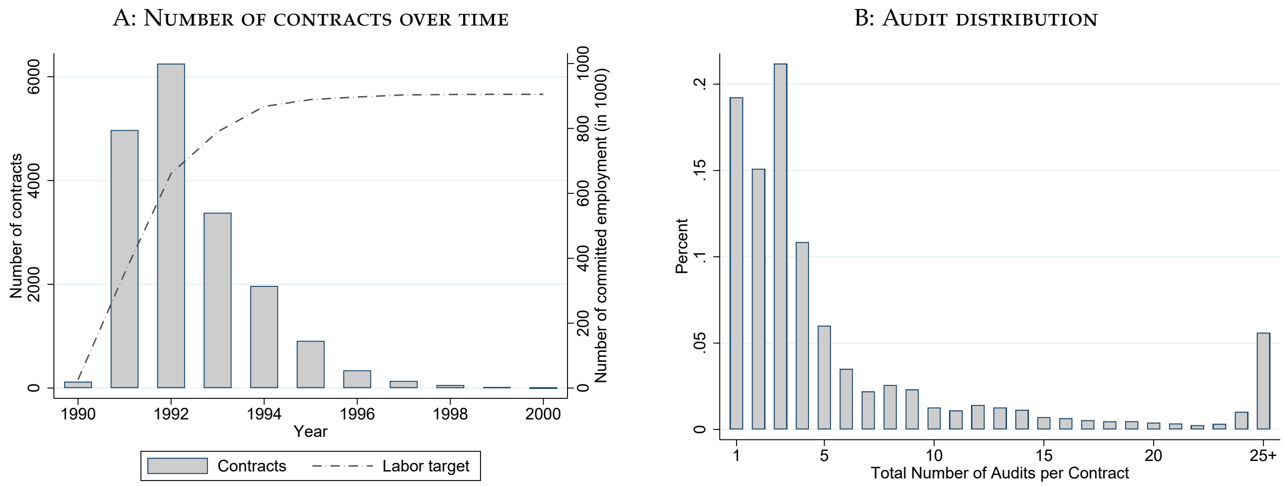
These contracts underwent regular audits conducted by contract managers employed by the THA. These audits serve as the basis for the realized employment levels analyzed in this paper. The contract managers would approach the contracting party and conduct audits either through physical visits to the firm or via documentation. On average, each contract was audited 6.3 times, with a minimum of 1 audit and a maximum of 84 audits. Figure 3, Panel B, illustrates that approximately 82% of all contracts were audited at least twice.

We focus on these contracts to measure employment growth during the commitment period. While we always have data on employment levels at the final commitment date, 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. Panel A of Table 1 shows contract-level employment information at the start date of the contract, the final level, and the target level. We provide descriptive statistics for the 14,726 contracts with at least two audits. 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 their size. Panel B relates the initial

[12]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.

size 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. In 10% of cases the firm stays at the committed size in the first and last audits.

Figure 3: 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.

For a subset of 1,272 firms, we observe the total amount of penalties claimed by the THA due to violations of labor commitments as well as the total number of violations. Based on these numbers, we calculate the penalty per missed employee taking into account the *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 one employee. 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.[13] Finally, consistent with documentation on THA policy, our calculations suggest that the average penalty per missed employee amounts to 10,768 EUR.[14]

Figure 4 empirically assesses the importance of employment targets in affecting firm's labor choices. 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

[13]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.

[14]The effective penalty can be lower because of "conditions beyond the purchaser's control" (Dodds and Wächter 1993), renegotiation, minor violations of targets (*Bagatellfall*), judicial decisions, and bankruptcy.

Table 1: Summary Statistics

	N (1)	Mean (2)	SD (3)	Minimum (4)	Maximum (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
Fraction initially & finally at target	14,726	0.10	0.30	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. In Panel A, initial employment level is calculated for contracts with at least two observations. This corresponds to 14,726 contracts. In Panel C, we observe 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.

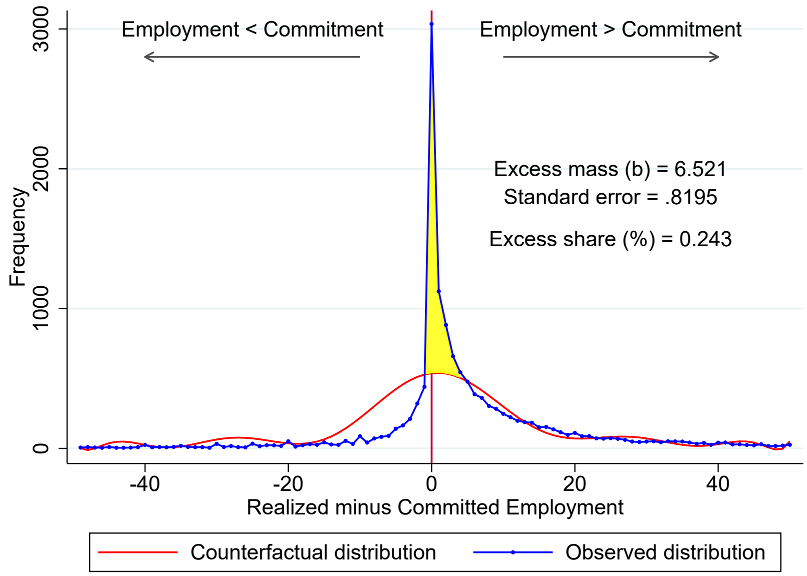
employment deviation. A striking feature of the data is the large spike exactly at the committed level of employment, suggesting the importance of these constraints for firms' labor choices. Following Chetty, Friedman, Olsen, and Pistaferri (2011), we estimate an excess mass around the threshold of 652% relative to the average height of the counterfactual distribution.[15]

4.2 Matching Contracts to Firms

The audits conducted on the contract-level data do not provide information regarding firm-level sales and post-privatization market exit. To construct productivity measures, we utilize data from the Mannheim Enterprise Panel (MUP). By merging the contract partners' names with the ownership information in the MUP, we can generate productivity measures starting from 1993. This merging process enables us to measure firm sales at the end of the commitment period for nearly all linked firms. For a detailed description of the data merge between the two datasets, please refer to Supple-

[15]The red line in Figure 4 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.11 shows the results by sub-samples.

Figure 4: 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).

mentary Appendix C. Overall, we identify the respective legal unit behind 4,622 contracts.

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. 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 the Soestra firm-level 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 noteworthy 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).[16] 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

[16]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.

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. At the end of the commitment period, an exit share of 5.5% is observed.

5 Empirical Analysis of Labor Commitments and Firm Dynamics

5.1 Identification Strategy

Addressing the empirical challenge of non-random allocation of labor commitments to firms is crucial when analyzing 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 regarding outcomes under their control, such as leniency or toughness. 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 in future cases.

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.[17]

Instrument Construction. In our setting, 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.[18]

The first step in the analysis is to construct a measure for privatizers' stringency in assigning binding labor commitments i.e., labor targets that force the firm to grow. Our measure is the average propensity of privatizers to require binding labor commitments. To address the own-observation problem and control for the THA office level effects, we follow the literature by estimating the following leave-one-out measure of binding commitment:

[17]The organizational stress and complexity of privatizing the East German economy cannot be underestimated. 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). 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).

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

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

where i denotes the firm, o the THA office, and j the assigned privatizer.[19] $Binding_i$ 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. This is important as the characteristics of firms to be privatized are different across THA offices. Z_{ioj}, therefore, measures the leave-one-out measure of binding labor requirements of privatizer j assigned to firm i.

Figure 5 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.[20]

Random Assignment. We use this measure of privatizer labor preference to indirectly test the assignment mechanism of cases to privatizers. 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.

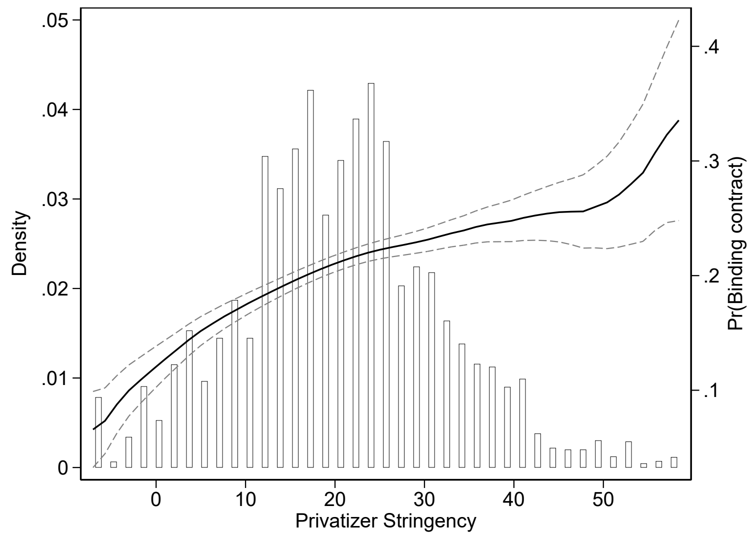
Table 2 tests the random assignment mechanism. Each coefficient in column (1) represents a single regression, with the independent variable being our measure of labor preferences (conditional on fully interacted year and local office fixed effects). Column (2) provides p-values with two-way clustered 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.

Estimates in Table 2 provide strong evidence that, conditional on fully interacted year and local office fixed effects, cases are randomly assigned to privatizers in our sample. 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

[19] 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.

[20] 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.

Figure 5: 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.

at the 5% level. Adjusting for multiple testing, however, there is 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 last 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, which may indicate a potential reduction in the effective 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.

An additional empirical challenge relates to how labor commitments can influence buyer selection. High-quality buyers might find it easier to agree to binding labor commitments, thereby generating 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.

correlation between buyer types and labor targets. 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. Overall, we do not observe any systematic evidence for buyer selection.

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 conduct an examination of the correlation between privatizer stringency and binding commitments 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.[21] 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 also implement and fail to reject the joint null hypothesis that pairwise monotonicity and exclusion hold.[22]

IV Model. The next step of the analysis is to embed this instrument into a 2SLS equation relating THA's employment targets to outcome variables such as employment growth, productivity growth, and exit. The regression model can be written as:

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

with y_i, for example, indicating the growth rate of employment between the first and the final audits.[23] 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 and the number of months between the first and final audits to capture differences in commitment length across contracts. The parameter

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

[22] Table A.3 provides the test for the joint null hypothesis that the exclusion and monotonicity assumptions hold for different numbers of knots and Bonferroni weights using the suggested quadratic spline (controlling for THA office times year fixed effects).

[23] This measure of employment growth bounds the growth rate between -2 and 2 and reduces the possibility that results are driven by outliers. We provide robustness checks using log differences and $(L_{it}/L_{it-1})^{1/\#years} - 1$ as the growth measure. Note also that the final audit corresponds closely to the date of the final commitment.

of interest is β, which measures the effect of assigning a binding contract on the growth rate of the firm.

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:

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

where Z_i denotes labor preferences of privatizer j assigned to case i as defined above. The model therefore estimates the local average treatment effect of THA labor requirements on firm outcomes.

5.2 Labor Commitments and Firm Growth

We begin by documenting how employment commitments dynamically affected firm policies in terms of labor. The description of employment dynamics by initial contract conditions uses the sample of 11,194 contracts for which we have multiple audits as well as information on privatizer names and offices. 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 $(L_{it} - L_{it-1})/0.5(L_{it} + L_{it-1})$, where L_i denotes the level of employment of firm i. Subscript t refers to the date of the final commitment and, consequently, $t-1$ refers to the first audit that approximates firm size when the contract is signed.[24]

Figure 6 provides descriptive evidence on the distribution of growth rates over the course of the contract period. 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 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%.

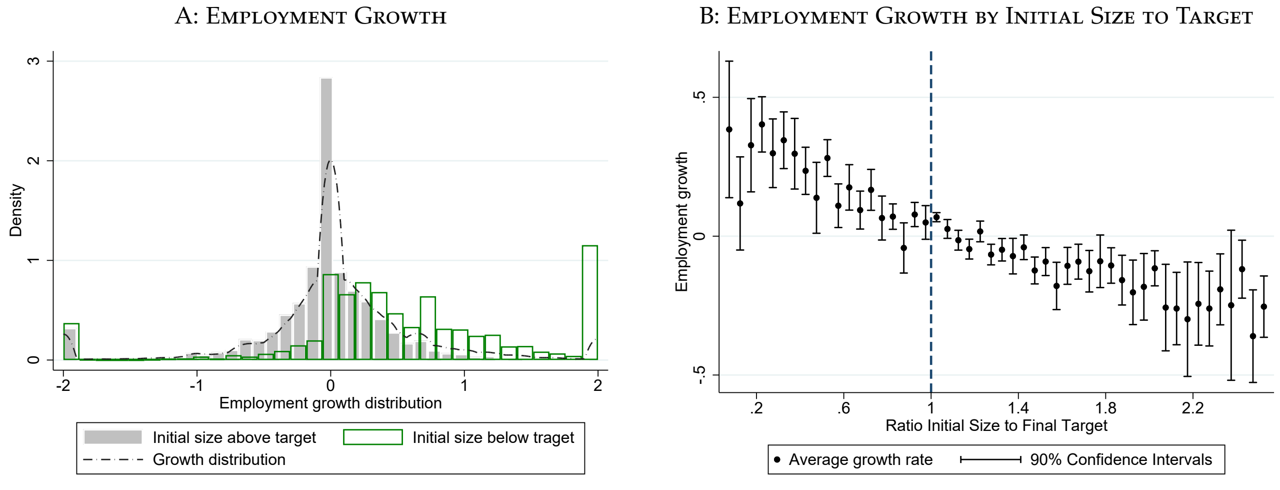
The right panel of Figure 6 provides the full distribution of employment growth according to the ratio of initial size over final employment target. A striking negative relationship emerges between the distance to the final target and subsequent employment growth. Firms that have high targets relative 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.

Table 3 presents the estimates for the labor growth equation. Columns (1) to (3) provide OLS estimates, while columns (4) to (6) provide IV estimates. Standard errors are two-way clustered at the privatizer and office level. Columns (1) to (3) suggest that firm growth is positively correlated with binding labor commitments. Conditional on the set of baseline controls, industry dummies, and privatizer characteristics, the association between binding contracts and employment growth is on

[24] The length of the employment commitment is heterogeneous across contracts. On average commitment length is three years. However, we observe commitments that can last for 11 months to 98 months at the 1st and 99th percentiles of the distribution. In the empirical specification we control for differences in commitment length.

average 49% points until the final commitment date. The estimated OLS coefficient is unaffected by the inclusion of additional control variables to those in the baseline specification.

Figure 6: Employment Dynamics 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.

IV estimates in columns (4) to (6) suggest that the causal effect of binding labor targets is significantly larger with respect to OLS estimates. In these specifications, firms are estimated to grow their workforce by 68% points in the three years following the random assignment of a binding labor contract. The effect is not only economically large but also precisely estimated at the 1% significance level. Consistent 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 15 and 17. 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. Consistent with the quasi-random assignment mechanism, the inclusion of additional covariates does not affect the first-stage coefficients.[25]

The results are robust to a series of alternative specifications. A challenge to our identification strategy is that privatizer decisions are multidimensional. The exclusion restriction requires that pri-

[25]Table A.7 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.7 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.

Table 3: Regression Results, Employment Growth

	OLS Model			IV-Model		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Second-stage results						
Binding contract	0.4992***	0.4973***	0.4975***	0.7016***	0.6740***	0.6873***
	(0.031)	(0.030)	(0.030)	(0.219)	(0.231)	(0.235)
Panel B: First-stage results						
Privatizer stringency				0.0020***	0.0018***	0.0018***
				(0.000)	(0.000)	(0.000)
Observations	9,363	9,363	9,363	9,363	9,363	9,363
Average employment at contract date	60.064	60.064	60.064	60.064	60.064	60.064
Average growth rate (non-binding contracts)	-0.063	-0.063	-0.063	-0.063		
Share with binding contracts	0.207	0.207	0.207	0.207	0.207	0.207
F-Statistic				17.01	14.67	14.76
Sample condition						
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry controls	No	Yes	Yes	No	Yes	Yes
Individual controls	No	No	Yes	No	No	Yes

Notes: The table shows OLS and IV regression results of employment growth on binding contracts. Panel A shows the reduced form regressing the binding contract indicator on the stringency measure. Panel B shows the second-stage results. All specifications control for fully interacted THA agency and year fixed effects and are conditional on having at least five privatizations per privatizer. *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 controls 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. Instrument refers to the leave-one-out measure of assigning binding contracts. *p<0.1, **p<0.05, ***p<0.01.

vatizers affect firms' outcomes only through binding labor commitments. THA privatizers, however, negotiated not only on labor commitments, but also on associated penalties, investment commitments, and sales price. Following Bhuller, Dahl, Løken, and Mogstad (2020), we address this issue by augmenting our baseline model with controls for these dimensions of privatization contracts. Consistent with the exclusion restriction, Table A.4 shows that adding extensive and intensive investment preferences does not affect our baseline results qualitatively and, further, does not have any explanatory power in the first and second stages. In Panel A of Table A.5, we also control for subsequent renegotiation attempts initiated by buyers. Panel B varies the sample according to the number of cases handled and the construction of the instrument. Panel C estimates our model using alternative measures of firm growth. Finally, Table A.6 addresses potential sample selection of GDR firms into labor commitments by including the estimated inverse mills ratio from a Heckman model. Again, estimates are unaffected.[26]

5.3 Labor Commitments and Productivity Growth

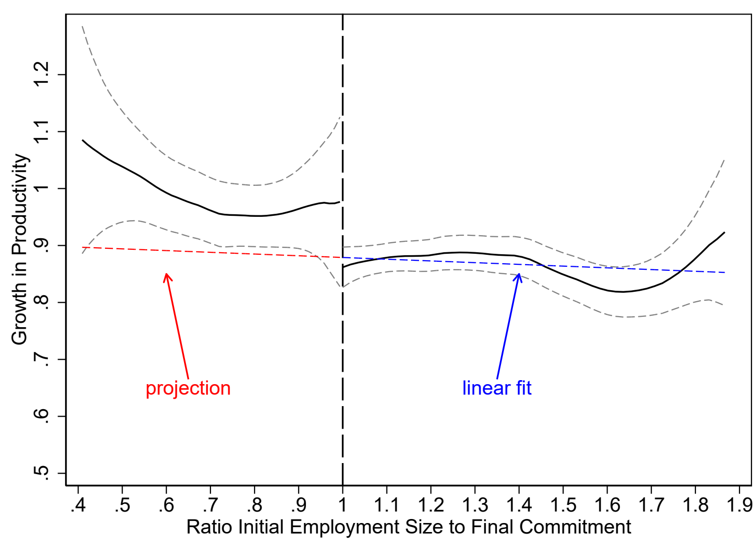
To disentangle the mechanism behind the growth in employment we extend the empirical analysis to productivity dynamics in the matched sample of 2,395 privatization contracts with complete informa-

[26] The Heckman selection equation is based on a probit regression with the outcome variable being equal to 1 if the initial GDR firm is observed among the contracts with labor commitments and zero otherwise. We use as explanatory variables log initial firm size and log initial sales over employment measured in 1990 as well as sector- and THA office fixed effects. Results for employment growth, productivity growth and firm exit in A.6.

tion. We construct the model-consistent measure of productivity as $sales/employment^{\alpha}$, taking into account the labor share in the production. In the baseline analysis, we set $\alpha = 0.8$, consistent with the aggregate labor share during this period. We also construct measures of productivity based on TFP by matching the contracts to a survey that contains information on THA firm investments. This allows us to obtain the associated capital stock of the firm and estimate a Cobb-Douglas production function.

Figure 7 describes the relationship between productivity growth 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. 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 in the data amounts to 86.8% which indicates substantial improvements in productivity during the first years after reunification. Second, productivity growth is significantly higher for firms initially below their committed employment.

Figure 7: 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 labor 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 2,395.

Table 4 provides OLS and IV estimates for productivity growth following the same growth rate formula as for employment. The specification controls again for fully interacted THA office and year fixed effects, industry dummies, log initial employment, log initial productivity, the purchasing price and the time between the first and last audits as well as between the contract date and the first audit.

Columns (1) and (2) of Table 4 provide OLS evidence that firms with binding labor contracts experience higher productivity growth of around 8 to 9% points. Column (2) adds controls for decile dummies of the purchasing price. Column (3) provides IV evidence on productivity growth. To do so, we implement a two-sample 2SLS estimation by using the predicted values from the first-stage regression of the full sample in the second-stage regression of the sub-sample that includes information on productivity growth. To calculate the standard errors, we perform 2,500 bootstrap replications presented in parentheses. Over the course of five years measured between July 1990 and the end of the commitment period, on average, firms with binding labor contracts experience a total productivity growth of roughly 73% points. Columns (4) to (6) present our results based on the TFP growth measure. Again, we find that TFP increased by 66% points more for firms with binding contracts over the commitment period. As in the employment growth regressions, the OLS estimates for productivity display a downward bias with respect to the IV estimates. We provide extensive robustness checks for our productivity growth results in Appendix Tables A.9 and A.10. Amongst others, we demonstrate that the estimates remain robust when varying the labor share parameter, and when including additional controls such as purchasing price, the presence of investment targets, and the inclusion of exiting firms.

Finally, Table A.8 provides supporting evidence for the productivity channel by analyzing patenting activity. The outcome variable is equal to 1 if the firm has at least one patent during the commitment period and 0 otherwise. OLS results show a positive and significant association between binding contracts and patenting. Although imprecisely estimated, the 2S2SLS coefficient shows again a downward bias of the OLS point estimate.

Table 4: Regression Results, Productivity Growth

	Productivity, $\alpha = 0.8$			TFP		
	OLS	OLS	2S2SLS	OLS	OLS	2S2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Binding contract	0.0965***	0.0835***	0.7109**	0.1107***	0.1239***	0.6608***
	(0.022)	(0.023)	(0.363)	(0.039)	(0.041)	(0.213)
Observations	2,395	2,395	1,612	1,825	1,825	1,825
Average productivity at contract date	10.599	10.599	10.633	6.813	6.813	6.813
Average productivity growth (non-binding)	0.851	0.851	0.854	0.332	0.332	0.332
Share with binding contracts	0.171	0.171	0.155	0.162	0.162	0.162
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Purchasing price	No	Yes	Yes	No	Yes	Yes

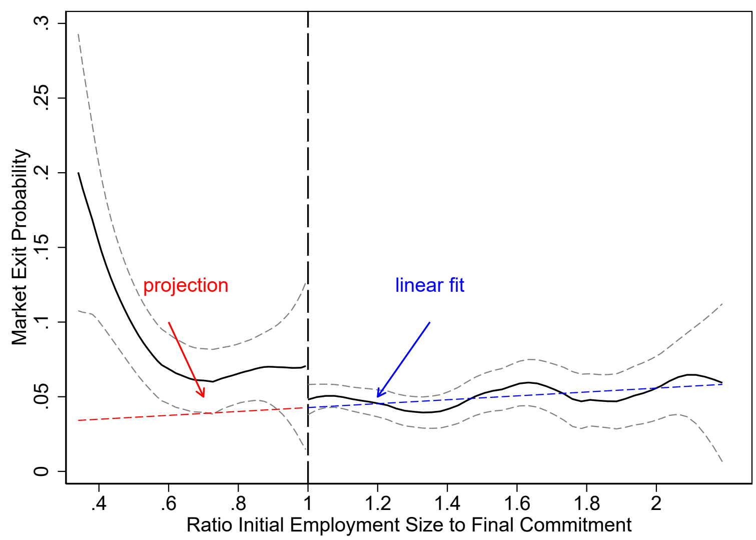
Notes: The table shows OLS and 2S2SLS regression results of measures of productivity growth on binding contracts. All specifications control for fully interacted THA agency and year fixed effects. Binding contracts are defined as initial firm size below the committed target level. Controls are as in the baseline specification. Additional controls are log initial productivity and the purchasing price. Standard errors in columns (1)-(3) are two-way clustered at privatizer and THA office level. The standard errors in columns (4)-(6) are bootstrapped using 2,500 replications. *p<0.1, **p<0.05, ***p<0.01.

5.4 Labor Commitments and Market Exit

We now turn to the analysis between binding labor commitments and market exits. To measure firm exit we again use the merged sample of contracts to the MUP data. We also use a second measure of exit based on the final labor audit reporting 0 workers.

Figure 8 describes the relationship between the exit probability of firms and the ratio of initial employment relative to the final target. Similar to the productivity growth patterns, the share of firms exiting the market is relatively constant for non-binding firms above the vertical line of 1. The

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.

exit rate for these firms amounts to 4.8% on average. With an average exit rate of 8.4%, firms with binding contracts show a higher level of market exit that is increasing in the tightness measure.

Table 5 provides a regression version of Figure 8, controlling again for the same variables as before. 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 (conditional on the same sample). Both regression specifications generate similar OLS results. Binding contracts are associated with an increase in market exit of around 2.5% points. The IV estimation, although with lower precision, confirms the higher propensity to exit of firms with binding labor commitments. These results are consistent with the model's prediction that lower profits associated with the penalties lead firms to exit at a higher rate.

Table 5: Regression Results, Exit Probability at Final Commitment

	MUP exit indicator			ISUD 0 employment		
	OLS		2S2SLS	OLS		2S2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Binding contract	0.0262**	0.0248**	0.1302	0.0216*	0.0193*	0.0358*
	(0.011)	(0.012)	(0.108)	(0.011)	(0.010)	(0.021)
Observations	4,563	4,563	2,804	4,563	4,563	2,804
Exit share (non-binding)	0.049	0.049	0.047	0.035	0.035	0.030
Share with binding contracts	0.171	0.171	0.171	0.171	0.171	0.171
Sample condition						
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry controls	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Purchasing price	No	Yes	Yes	No	Yes	Yes

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. Controls are as in the baseline specification. Additional control variable is the purchasing price. Standard errors in columns (1), (2), (4) and (5) are two-way clustered at privatizer and THA office level. The standard errors in columns (3) and (6) are bootstrapped 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 the binding employment targets impact firm dynamics.

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 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.[27] 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

[27] 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.

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 3 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.

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

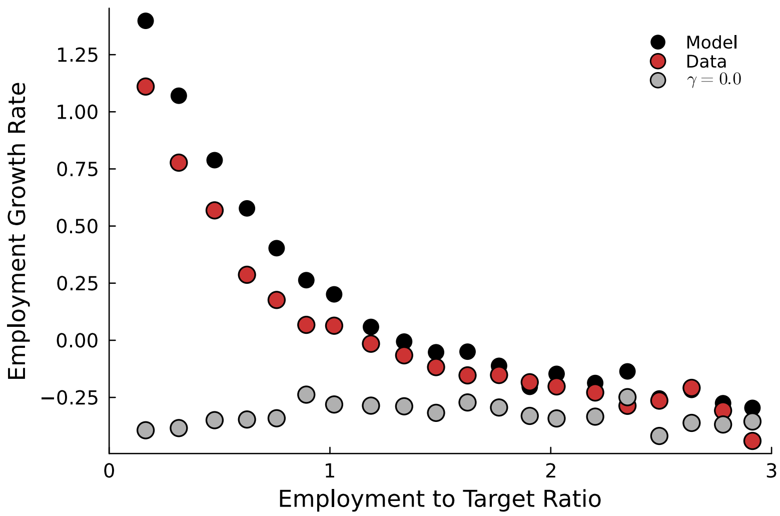
Table 7: Internally Calibrated Parameters

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

Non-targeted Moments The calibrated model also performs well in matching some important patterns in the data that were not targeted. In Figure 9, we depict the employment growth 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.

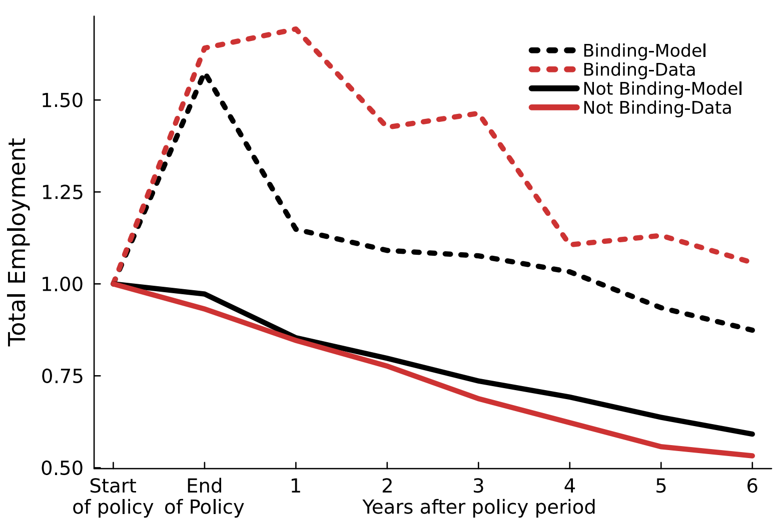
The calibrated model also captures the post-commitment employment dynamics fairly well. Figure 10 illustrates the evolution of total employment during and after the commitment period both in the model and data. The results suggest that not only do these firms with binding contracts experience higher employment growth during the commitment period, but these employment gains are persistent at least six years subsequent to the commitment period. This persistent employment effect is consistent with the dynamic productivity gains implied by binding labor targets in the model.

Figure 9: 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.

Figure 10: Total Employment in the Post-Commitment Period



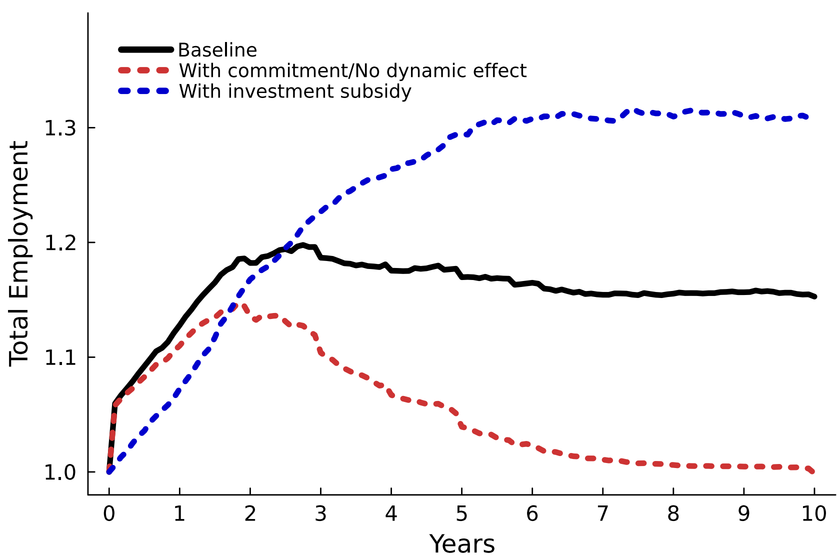
Notes: The figure shows the evolution of total employment during and after the commitment period both in the model (black lines) and data (red lines). Dashed and solid lines show the total employment for binding and not binding firms, respectively. All series are normalized to 1 at the beginning of the policy period.

6.3 Counterfactuals

To quantify the importance of the different channels through which employment targets affect firm dynamics, we start with a simple exercise where we simulate a counterfactual economy under which there are no employment targets. In particular, 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 9 show the employment growth rate 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 total employment, rather than the 5% drop we observe in the data, suggesting that employment targets had a non-trivial role in shaping the aggregate employment dynamics.

Our next exercise decomposes the impact of employment targets on total employment 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 "escape" 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, the second counterfactual economy only includes the direct, static effect of employment targets on employment choices. Fig-

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.

ure 11 summarizes the results by plotting the total employment over time under commitment (black line) and under no dynamic effects (red dotted line), relative to the total employment under no commitment counterfactual. The decomposition results suggest that the dynamic effect of employment targets on productivity growth contributed to the employment dynamics significantly. As can be seen from the opening distance between the black line and the red dotted line, the importance of the dynamic channel increases over time. Our calibrated model attributes one-third of the employment growth over three years to dynamic effects. 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 investment subsidies instead of employment targets. We choose the subsidy level such that the total employment growth is the same as in the data through the period the original policy was implemented. The blue dotted line in Figure 11 depicts the total employment growth under such an investment subsidy. The comparison between the subsidy policy and the commitment policy in terms of employment dynamics reveals two important distinctions. Firstly, the subsidy incurs a substantial cost to attain the same aggregate employment growth, equivalent to around 5 percent of total output. Secondly, while the subsidy policy results in higher levels of permanent employment, it falls short in addressing short-term employment concerns compared to the commitment policy.

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. The type of policy intervention we analyze is unique, insofar as it primarily puts constraints on firms' labor choices as opposed to using subsidies. 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, simultaneously inducing higher employment and productivity growth, yet increasing the probability of firm exit due to lower profitability. Empirical validation, using a rich German dataset and an instrumental variable approach, confirmed these theoretical predictions. We find that a 22% points higher annual employment growth rate was achieved under binding labor contracts, along with a substantial 14% points annual increase in productivity. However, these gains were somewhat counterbalanced by a 3.6% points higher firm exit probability. In a series of counterfactual scenarios, the absence of employment targets indicated a potential 15% drop in aggregate employment after 10 years, emphasizing the policy's significant role in the labor market. We explore an alternative policy of productivity investment subsidies and find that it could potentially yield higher permanent employment, albeit at a high cost and a slower employment response.

In this paper, our primary objective is to understand the positive implications of the policy, rather than exploring its detailed welfare implications. As such, the model we employ focuses on firm-level decisions, analyzing how the policy shapes and influences firm behavior statically and dynamically. It's important to recognize that a comprehensive normative analysis would necessitate to specify various other details of the economic environment. This encompasses factors such as the 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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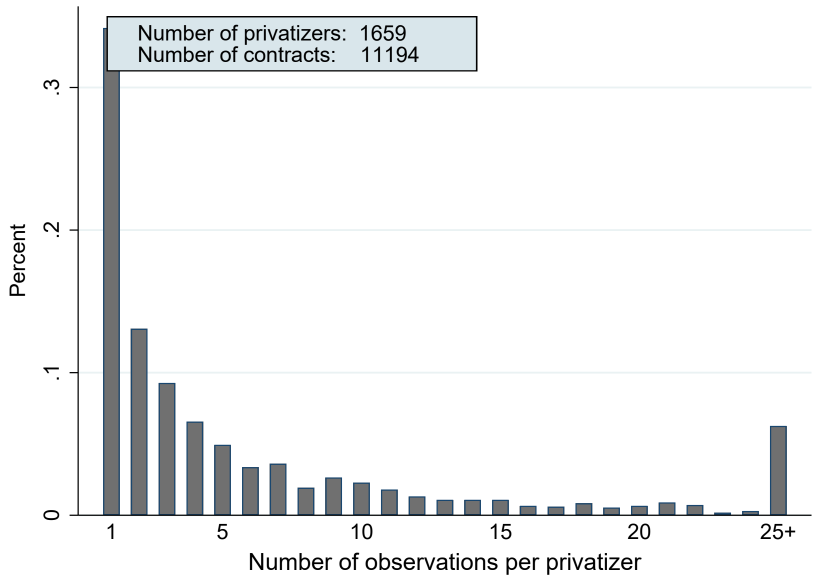
Supplementary Appendix

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 (1)	p-value (2)	Adj. p-value (3)	Mean (4)	Standard deviation (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.

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***	0.0011**	0.0015***	0.0012**	0.0012*	0.0016***	0.0009*
	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)
Observations	10,616	6,077	6,545	5,985	3,982	5,230	3,003
Average employment at contract date	63.22	57.75	73.638	72.39	70.166	69.032	70.554
Average growth rate	.062	.028	.084	.038	.038	.048	.006
Share with binding contracts	.208	.184	.232	.194	.194	.22	.146
Sample condition							
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

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.

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

	10 knots				15 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	535	535	535	535	484	484	484	484
d.f.	(509)	(509)	(509)	(509)	(504)	(504)	(504)	(504)
P-value	[0.204]	[0.255]	[0.408]	[0.680]	[0.733]	[0.916]	[1.000]	[1.000]

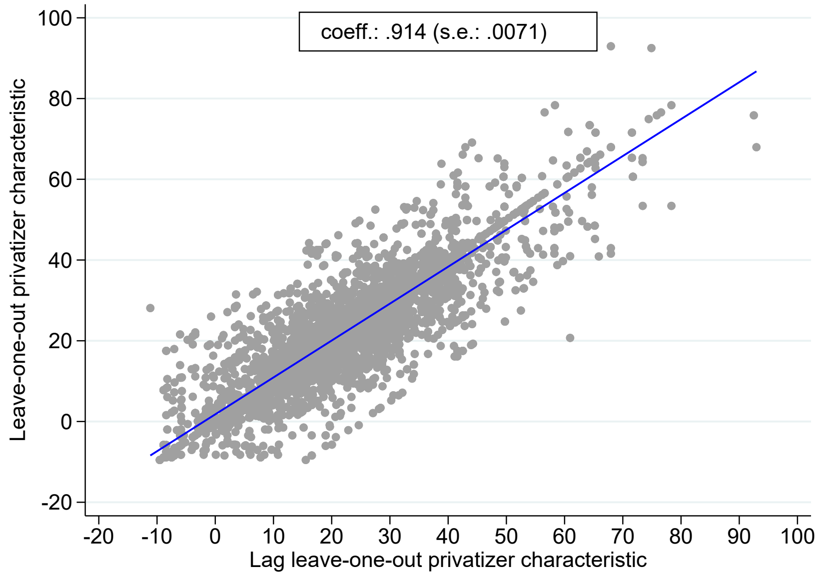
Notes: The table presents results from the test proposed in Frandsen, Lefgren, and Leslie (2023) for the joint null hypothesis that the monotonicity and exclusion restrictions hold. We test this null using THA office times year-of-privatization effects conditional on having handled at least 5 privatizations. 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`.

Table A.4: Regression Results Accounting for Extensive/Intensive Margin Preferences

	IV-Model Results			First stage
	(1)	(2)	(3)	(4)
Binding contract	0.7246***	0.7317***	0.7070***	
	(0.223)	(0.225)	(0.249)	
Investment preferences				
Extensive margin	-0.0005	-0.0005	-0.0004	-0.0005
	(0.001)	(0.001)	(0.001)	(0.000)
Intensive margin	-0.0005	-0.0004	-0.0004	-0.0004
	(0.001)	(0.001)	(0.001)	(0.000)
Privatizer stringency (instrument)				0.0018***
				(0.000)
Observations	9,363	9,363	9,363	9,363
Average employment at contract date	60.064	60.064	60.064	60.064
Average growth rate	.064	.064	.064	.064
Share with Binding contracts	0.207	0.207	0.207	0.207
F-Statistic	17.67	17.03	14.47	
Sample condition				
Baseline controls	Yes	Yes	Yes	Yes
Individual controls	No	Yes	Yes	Yes
Industry controls	No	No	Yes	Yes

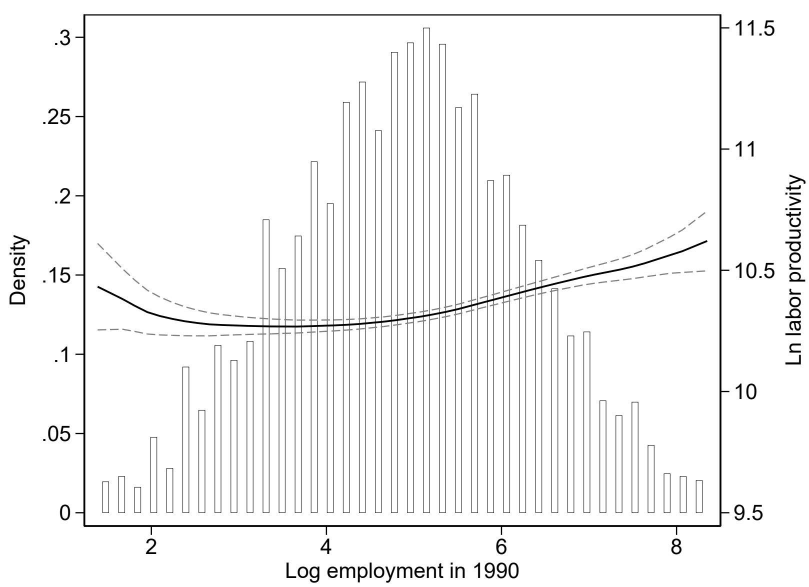
Notes: The table shows IV regression results. All specifications control for fully interacted THA agency and year fixed effects and are conditional on having at least 5 privatizations per privatizer. Extensive margin investment preference refer to contracts with any investment commitments. Intensive margin preference refer to contracts with the investment target over initial employment in upper decile of the distribution. *F*-Statistic refers to the Kleibergen-Paap *F*-Statistic. Baseline controls are the time between the first and last audits measured in days, the time between contract date and first audit measured in months, and initial employment level 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 tight contracts. **p<0.1, **p<0.05, ***p<0.01.

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.

Table A.5: 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	1.1690***	0.0008***	9.505	0.226
	(0.354)	(0.000)		
Above 10 cases per privatizer	0.9589**	0.0023***	11.38	0.355
	(0.346)	(0.001)		
Very tight contracts	1.0160**	0.0015***	8.511	0.237
	(0.424)	(0.001)		
Full tightness distribution	-0.0097***	0.1285***	21.58	0.848
	(0.003)	(0.028)		
Contract with zero employment in first & last	0.6380**	0.0016***	14.83	0.392
	(0.254)	(0.000)		
B: Control variables & sample selection				
Control for renegotiation attempts	0.6588***	0.0018***	16.24	0.408
	(0.222)	(0.000)		
Control for penalty clause	0.7621***	0.0016***	13.65	0.408
	(0.264)	(0.000)		
Control for purchasing price & investment target	0.5524**	0.0017***	13.65	0.408
	(0.222)	(0.000)		
Years between contract signed & first audit < 2	0.6558**	0.0020***	16.86	0.617
	(0.273)	(0.001)		
Month between first & last audit > 12	0.7387**	0.0018***	14.26	0.695
	(0.269)	(0.000)		
MUP subsample	0.5435**	0.0018***	10.45	0.486
	(0.256)	(0.001)		
C: Manipulation of the outcome variable				
Log employment differences	0.8953**	0.0018***	12.33	0.408
	(0.331)	(0.000)		
Annualized firm growth, $(L_t/L_{t-1})^{1/\#year} - 1$	0.2822**	0.0018***	14.76	0.408
(*trimmed at the upper percentile*)	(0.114)	(0.000)		
Growth rate < 2 & > -2	0.7443**	0.0015***	12.33	0.404
	(0.314)	(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.6: OLS Regression Results, Adjustment for Sample Selection

	Employment		Productivity		Exit	
	(1)	(2)	(3)	(4)	(5)	(6)
Binding contract	0.4313***	0.4302***	0.0938***	0.0884***	0.0229*	0.0223*
	(0.025)	(0.025)	(0.022)	(0.023)	(0.011)	(0.011)
Mills ratio		-0.0444		-0.1695**		-0.0194**
		(0.030)		(0.076)		(0.008)
Observations	8,333	8,333	2,399	2,399	3,877	3,876
Average employment at contract date	70.926	70.926	47.336	47.336	47.336	47.336
Mean outcome (non-binding contracts)	-0.062	-0.062	0.852	0.852	0.051	0.051
Share with binding contracts	0.192	0.192	0.188	0.188	0.188	0.188
Sample condition						
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry controls	No	No	No	No	No	No
Individual controls	No	No	No	No	No	No

Notes: The table shows OLS regression results of employment growth, productivity growth and firm exit on binding contracts with and without the inverse mills ratio. The inverse mills ratio is calculated based on a probit specification with the outcome variable being equal to 1 if the GDR initial firms is observed with privatizations contracts that include labor commitment. Explanatory variable in the selection equation are log employment measured in 1990, log sales over employment measured in 1990, THA office FE and industry FE. The selection equation controls for missing values in employment and sales over employment by introducing dummy variables. All specifications in the second stage control for fully interacted THA agency and year fixed effects. 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. Standard errors are two-way clustered at privatizer and THA office level. *p<0.1, **p<0.05, ***p<0.01.

Table A.7: OLS Regression Results with Different Control Variables & Weighting

	OLS-Model Results		
	(1)	(2)	(3)
A: Baseline			
Binding contract	0.4992***	0.4975***	0.4975***
	(0.031)	(0.030)	(0.030)
B: Complier re-weighting			
Binding contract	0.5336***	0.5341***	0.5298***
	(0.030)	(0.030)	(0.030)
Observations	9,363	9,363	9,363
Average employment at contract date	60.064	60.064	60.064
Average growth rate	.064	.064	.064
Share with binding contracts	.207	.207	.207
Sample condition			
Baseline controls	Yes	Yes	Yes
Individual controls	No	Yes	Yes
Industry controls	No	No	Yes

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.8: Regression Results, Cumulative Patents during Commitment Period

	OLS			2S2SLS
	(1)	(2)	(3)	(4)
Binding contract	0.0050**	0.0049*	0.0061**	0.0081
	(0.002)	(0.002)	(0.003)	(0.057)
Observations	4,563	4,563	4,563	1,430
Mean of Y of binding contracts	.012	.012	.012	.012
Mean of Y of non-binding contracts	.008	.008	.008	.006
Sample condition				
Baseline controls	Yes	Yes	Yes	Yes
Industry controls	Yes	Yes	Yes	Yes
Privatizer controls	No	Yes	Yes	Yes
Purchasing price	No	No	Yes	Yes

Notes: The table shows OLS regression results of patenting probabilities during the commitment period. The outcome variable takes the value of 1 if the firm has at least one patent during the period under commitment. 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. Baseline controls are the timing variable as in the baseline specification, log initial firm size, and an indicator if the firm has at least one patent before the contract date. Industry controls are 2-digit industry dummies. Standard errors are two-way clustered at privatizer and THA office level. The standard error in column (4) is bootstrapped using 2,500 replications. *p<0.1, **p<0.05, ***p<0.01.

Table A.9: Robustness Tests, Productivity Growth

	OLS			2S2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
A: Baseline & industry controls	$\alpha = 1$	$\alpha = 0.7$	$\alpha = 0.8$	$\alpha = 1$	$\alpha = 0.7$	$\alpha = 0.8$
Binding contracts	0.0753***	0.1059***	0.0965***	0.6378*	0.6195*	0.6307*
	(0.025)	(0.021)	(0.022)	(0.359)	(0.368)	(0.362)
Average productivity growth rate	0.884	0.868	0.875	0.884	0.870	0.876
Observations	2,395	2,395	2,395	1,612	1,612	1,612
B: Investment commitment control	$\alpha = 1$	$\alpha = 0.7$	$\alpha = 0.8$	$\alpha = 1$	$\alpha = 0.7$	$\alpha = 0.8$
Binding contracts	0.0647**	0.0909***	0.0828***	0.6793*	0.7004*	0.6989*
	(0.026)	(0.022)	(0.023)	(0.370)	(0.380)	(0.374)
Average productivity growth rate	0.884	0.868	0.875	0.884	0.870	0.876
Observations	2,395	2,395	2,395	1,612	1,612	1,612
C: Including exits	$\alpha = 1$	$\alpha = 0.7$	$\alpha = 0.8$	$\alpha = 1$	$\alpha = 0.7$	$\alpha = 0.8$
Binding contracts	0.0827**	0.1074***	0.0999***	0.5173	0.5650	0.5522
	(0.032)	(0.029)	(0.030)	(0.497)	(0.509)	(0.503)
Average productivity growth rate	0.808	0.792	0.799	0.822	0.810	0.816
Observations	2,480	2,480	2,480	1,656	1,656	1,656
D: July 1990 productivity		$\alpha = 1$			$\alpha = 1$	
Binding contracts		0.0564**			0.6701*	
		(0.026)			(0.370)	
Average productivity growth rate		0.882			0.882	
Observations		2,414			1,624	

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. Baseline controls are as in the baseline specification. Panel A controls for the baseline control variables. Panel B includes as further controls a dummy if the contract has investment commitments. Panel C includes exiters when calculating productivity growth rates. Panel C performs a ϵ-transformation. Panel D uses the actual measure numbers of sales and employment in 1990. Standard errors in columns (1)-(3) are two-way clustered at privatizer and THA office level. The standard errors in columns (4)-(6) are bootstrapped using 2,500 replications. *p<0.1, **p<0.05, ***p<0.01.

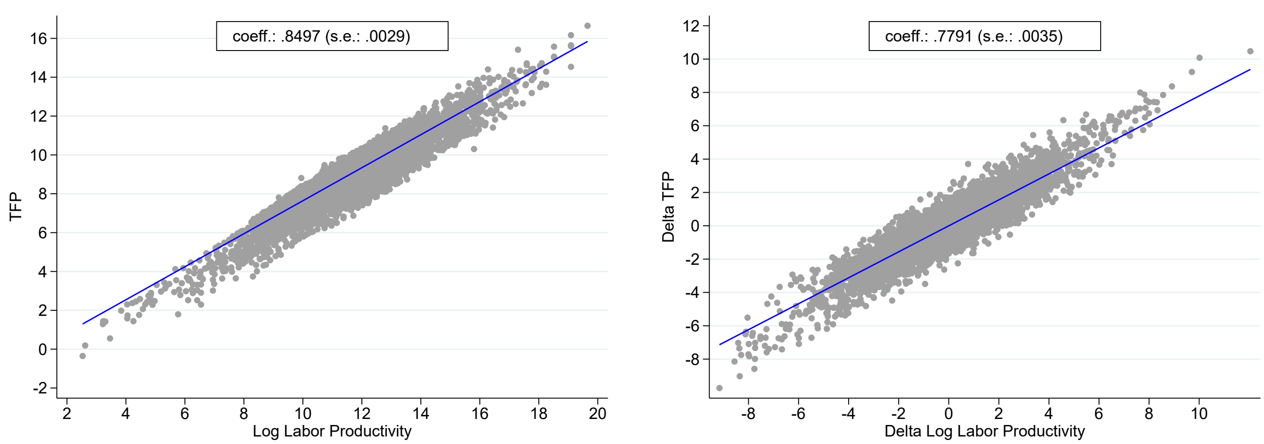
Table A.10: Robustness Tests, TFP Growth

	OLS			2S2SLS
	(1)	(2)	(3)	(4)
A: Baseline				
Binding contracts	0.1108**	0.1239***	0.1266***	0.6608***
	(0.039)	(0.041)	(0.042)	(0.213)
Observations	1,825	1,825	1,825	1,825
B: Including exits				
Binding contracts	0.0784	0.0931*	0.0978*	0.6159***
	(0.052)	(0.052)	(0.054)	(0.213)
Observations	1,835	1,835	1,835	1,835
TFP growth	.348	.348	.348	.348
Labor prod. growth	.53	.53	.53	.53
Baseline controls	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Industry controls	Yes	Yes	Yes	Yes
Purchasing price	No	Yes	Yes	Yes
Investment target	No	No	Yes	No

Notes: The table shows OLS and 2S2SLS regression results of TFP growth on binding contracts. Panel A provides the baseline results conditional on survival in the final commitment year. Panel B induces a -2 for firms that exit in the year of the final commitment. 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. Baseline controls are as in the baseline specification. Column (1) controls for the baseline control variables. Column (2) includes the purchasing price (flexibly introduced using decile dummies). Columns (3) and (4) include a dummy for investment targets. Standard errors in columns (1)-(3) are two-way clustered at privatizer and THA office level. The standard error in column (4) is bootstrapped using 2,500 replications. *p<0.1, **p<0.05, ***p<0.01.

Figure A.4: Correlation Productivity Measures

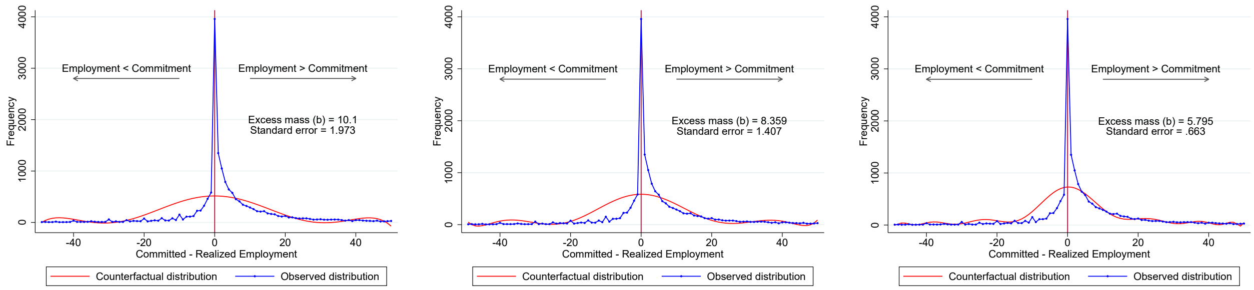
Productivity in Level | Yearly Change in Productivity



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.

Figure A.5: Bunching with different polynomials

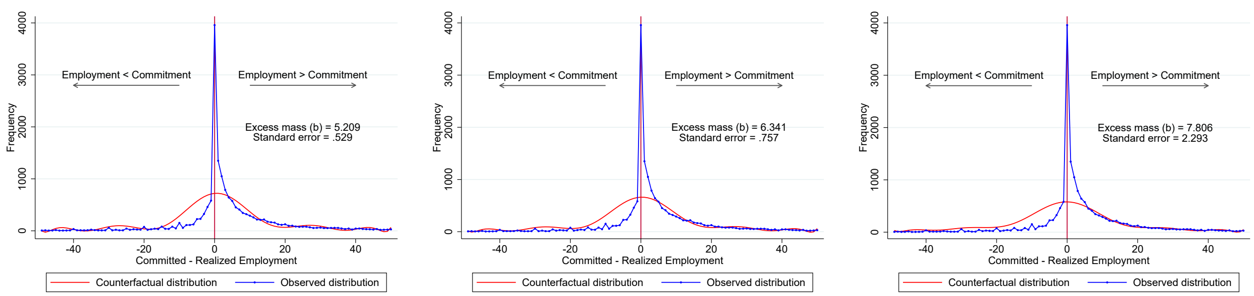
A: Polynomial degree: 6 | B: Polynomial degree: 9 | C: Polynomial degree: 18



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.

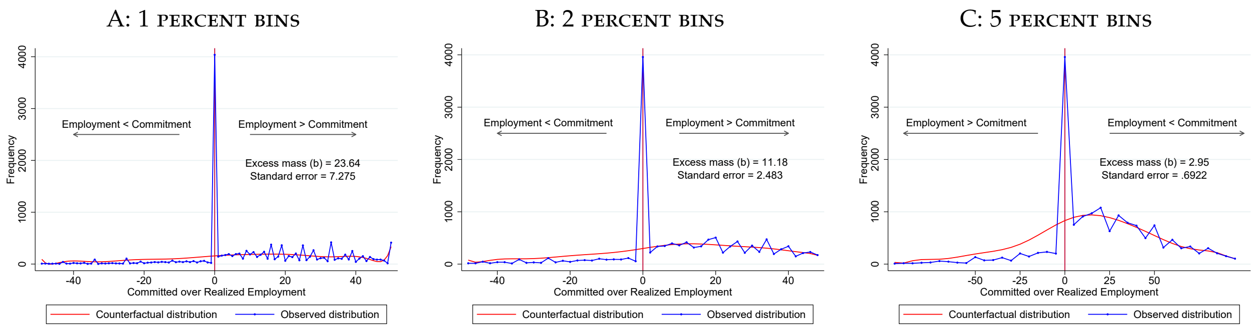
Figure A.6: Bunching with symmetric R

A: R=[1,1] | B: R=[2,2] | C: R=[4,4]



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.11: Bunching by Sub-Samples

	Excess mass (b) (1)	Standard error (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

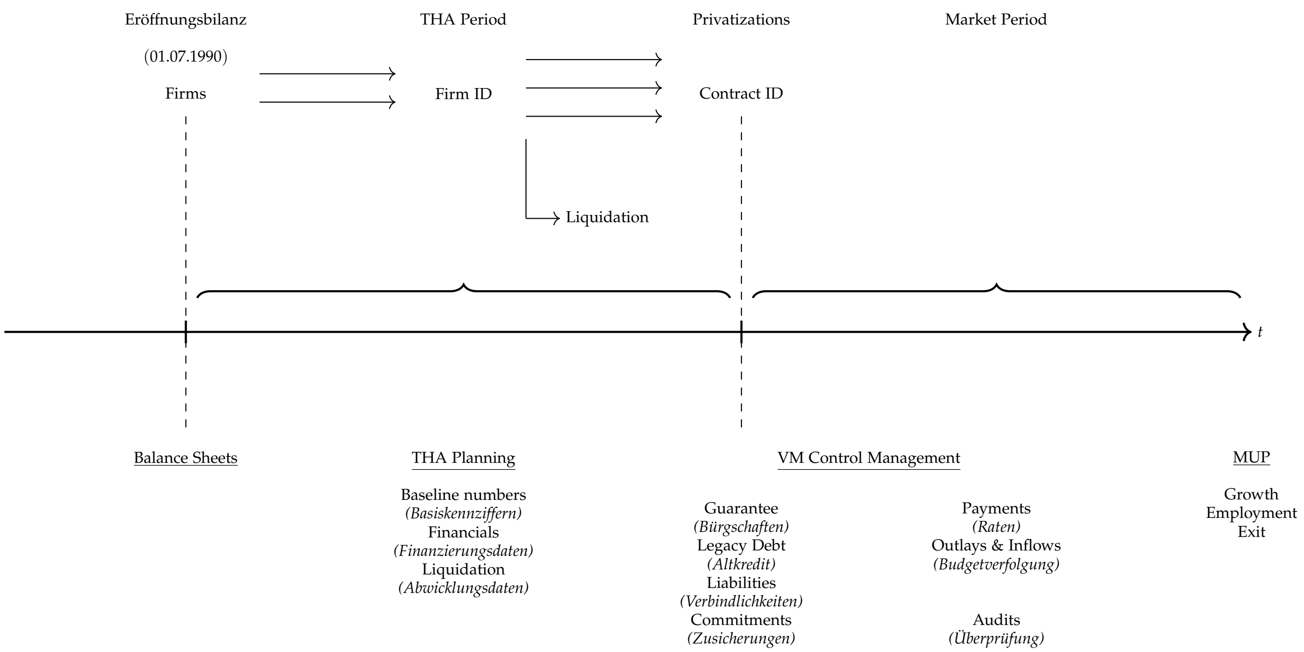
Notes: The table shows bunching estimates of the employment distribution around the committed employment for contracts between 1990-1995 by different groups. The counterfactual distribution is a based on a twelve-degree polynomial fitted to the empirical distribution excluding the area of missing one 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.[28]

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.[29] The information

[28]Section C describes the merge between contracts and external firm-level data, the Mannheim Enterprise Panel (MUP), to study dynamics beyond the commitment period.

[29]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.

relates to employment (including a breakdown into production workers, HR, and administration), 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.[30] 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 4, market exit effects in Table 5, 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. We refer to this estimated share in Section 2.

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.

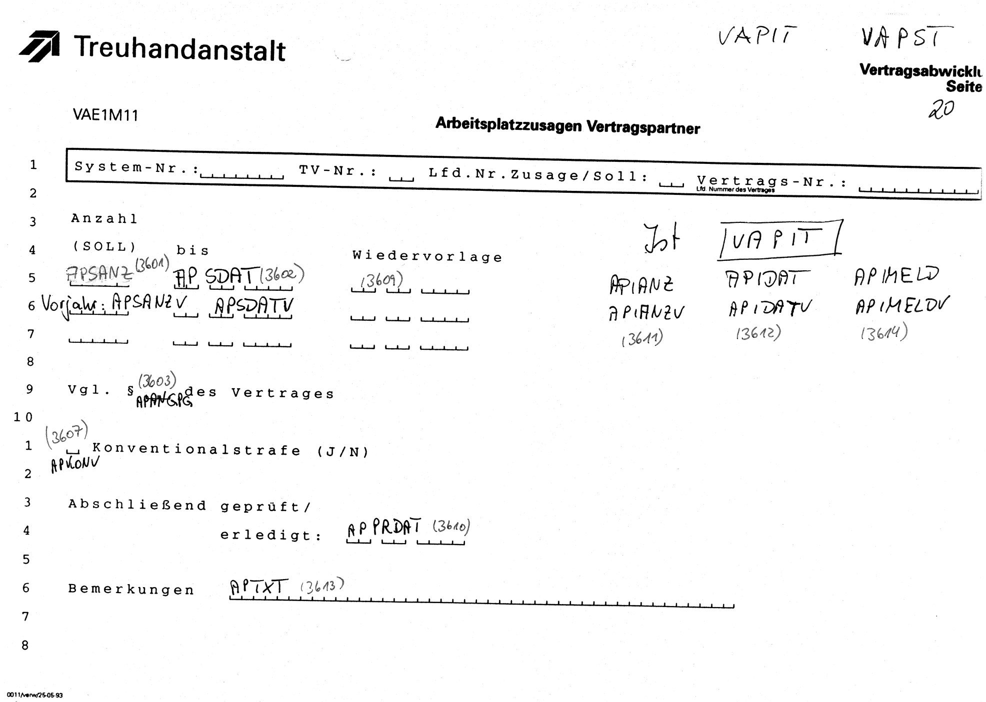
[30] THA created legal entities over time, and, as a result, 5.1% of firms are observed for the first time in 1991, and 1.2% in 1992, and 0.48% in 1993.

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.

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 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

Vertragsabwickl
Seite
20

VAE1M11

Arbeitsplatzzusagen Vertragspartner

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

Anzahl
(SOLL) bis
APSANZ (3601) AP SDAT (3602)
Vorjahr: APSANZV APSDATV

Wiedervorlage
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Ist
VAPIT

APIANZ APIANZV (3611)
APIDAT APIDATV (3612)
APIMELD APIMELDV (3614)

Vgl. § (3603) APAN GRG des Vertrages

(3607) APKONV Konventionalstrafe (J/N)

Abschließend geprüft/
erledigt: APPRDAT (3610)

Bemerkungen APTXT (3613)

0011/verw/25-05-93

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.[31] 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.

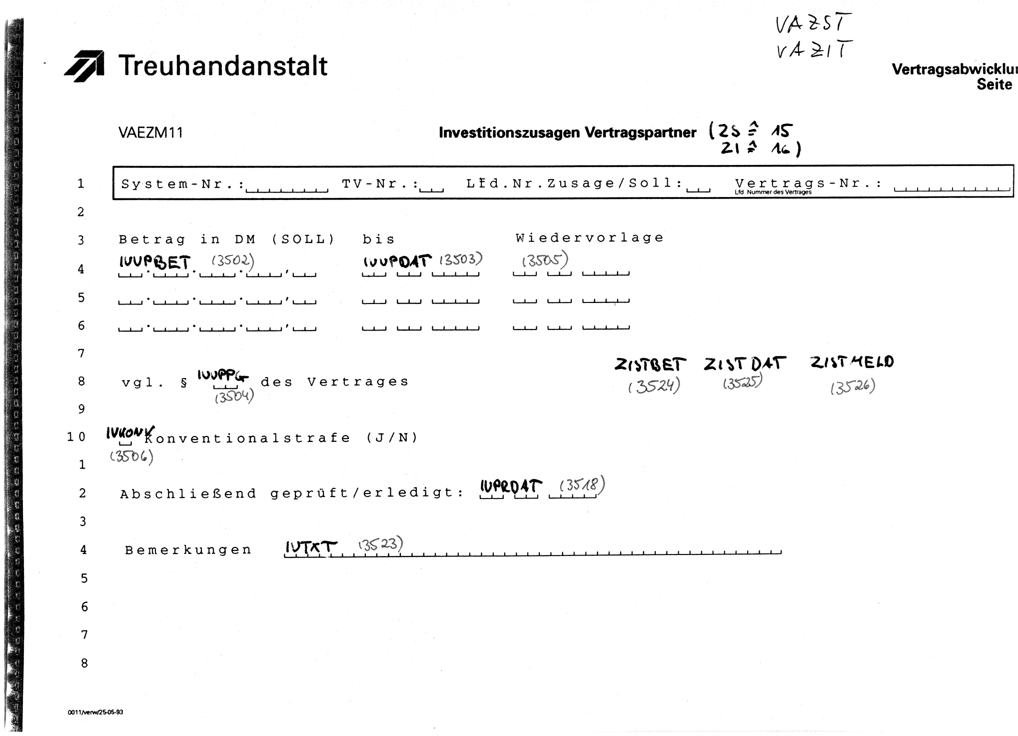
For the empirical specifications accounting for extensive/intensive margin privatizer preferences presented in Table A.4, we make further use of investment commitment contracts. The logic and steps in the data cleaning process apply similarly to investment commitment contracts. Figure B.3 shows the template 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

[31] 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

IUVPBET (3502) IUVPDAT (3503) (3505)

vgl. § IUVPPG (3504) des Vertrages

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

IUKONV Konventionalstrafe (J/N)
(3506)

Abschließend geprüft/erledigt: IUPRDAT (3518)

Bemerkungen IUTXT (3523)

0011/verw/25-05-93

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

any labor commitment condition we merge this combined dataset with the 58,544 contracts with cleaned privatizer names.

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 owner. 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 4,805 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 employment number differences between the two datasets. For 3,609 firms, we observe at least one audit

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.

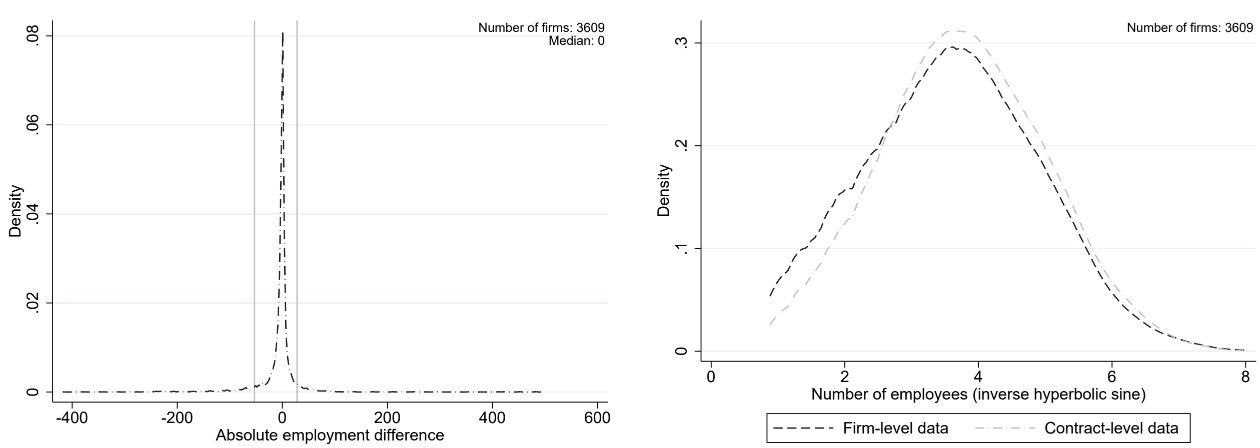
(with positive employment information) which allows us to calculate employment differences. The median (mean) estimated difference in employment is 0 (2.67). However, we observe large tails in the distribution of employment differences. For this reason, we further drop matches with absolute employment 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.

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.

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:

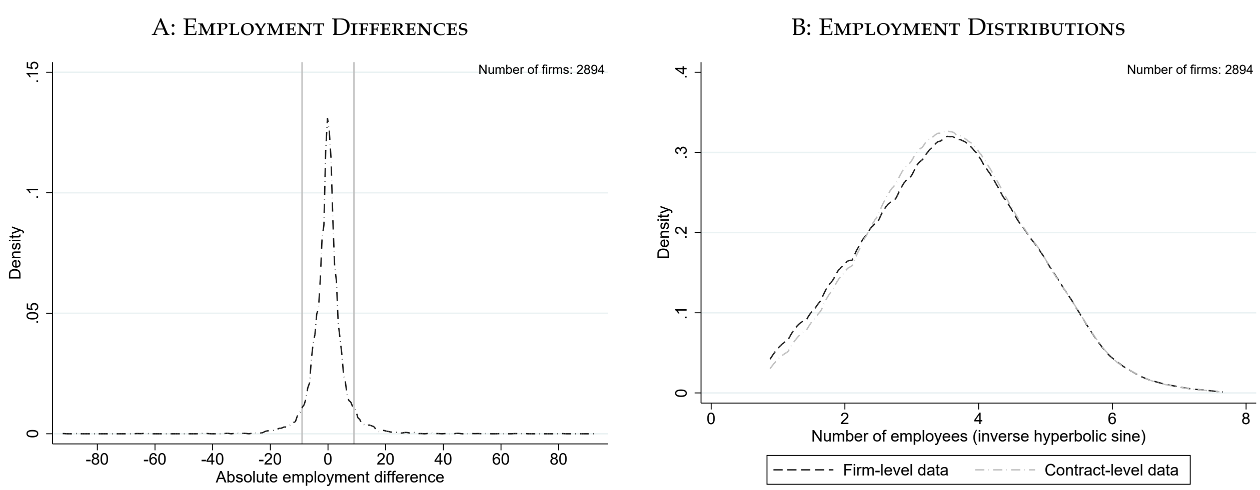
$$employment_{diff} = \frac{(empl_{MUP} - empl_{ISUD})}{(empl_{MUP} + empl_{ISUD})},$$

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. 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

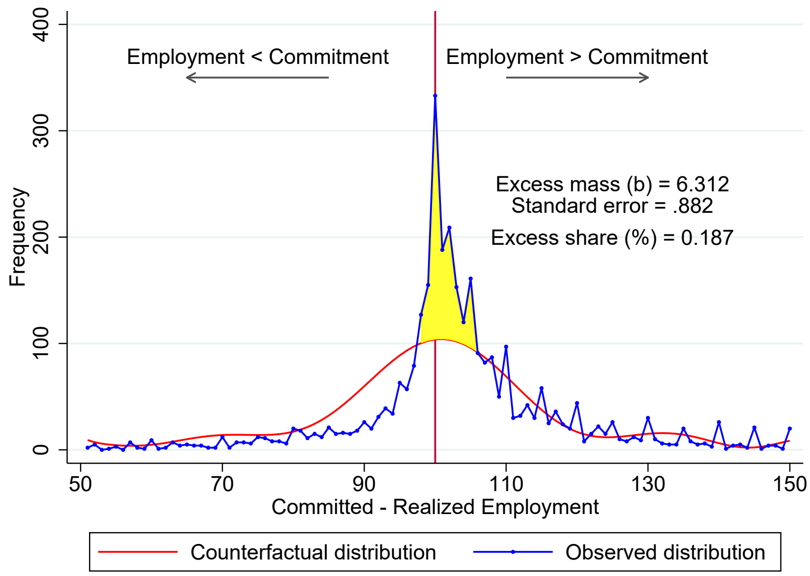


Notes: Panel A shows employment differences between matched contracts and firms in the MUP data that is centered around 0. Panel B shows the log employment distribution of matched contracts and the employment distribution in the MUP dataset. The sample is conditional on fulfilling the threshold rule. 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 4. 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 4. 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 and belonging to 9,596, 69% of the information belongs to an end-of-year question. Thus, more 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 measuredin 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	N	N with investment	N with imputed investment
(1)	(2)	(3)	(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

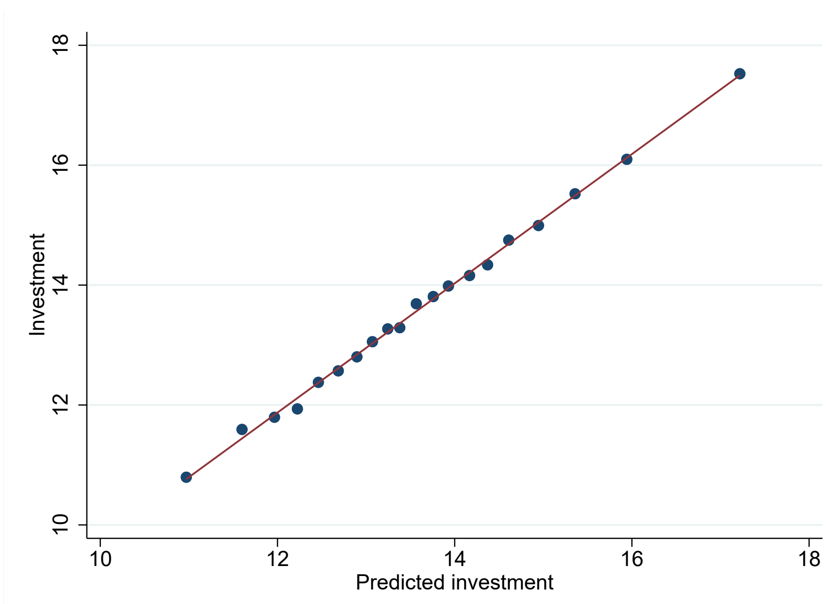
Notes: The table shows summary results from yearly lasso regressions with ln(investment) as the outcome variable.

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

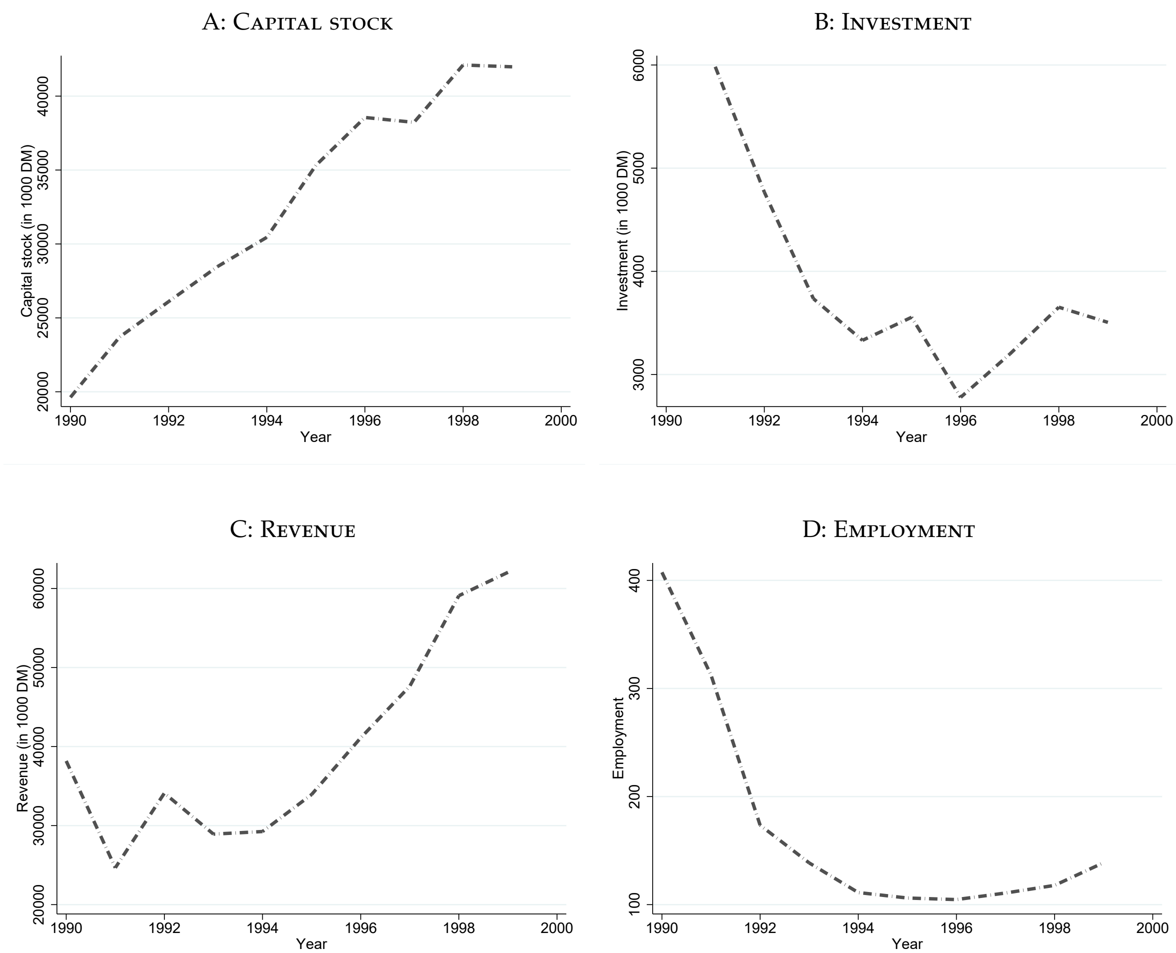
Investment



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.

We follow the empirical specification from the baseline model – including THA office times year fixed effects, initial firm size, time between the first and last audits, and industry and privatizer-level contracts – and show results of binding contracts on TFP growth with and without including purchasing price and investment targets as control variables. We deviate from the baseline model by conditioning the sample on observing three or more privatizations per privatizer (instead of five) for sample size reasons. This defines the final sample of 1,962 firm-contract observations.

Panel A of Table A.10 shows the results of binding labor contracts on firm-level TFP growth following the capital stock imputation of Panel A in Table D.3. OLS results show that binding labor commitments are associated with and increase in TFP growth of about 15% points. This point estimate is rather stable across different empirical specifications and close to the ϵ transformed labor productivity results of 0.145 presented in Tables 4 and A.9. The point estimates decrease by about 6% points to 10% points when including firm exits in Panel B of Table A.10. Column (4) provides the 2S2SLS results with a highly significant (bootstrapped) point estimate of around 0.93 (Panel A). Compared to Tables 4 and A.9 results are highly in line with each other. When including firm exits, 2S2SLS estimates decrease to 0.58 (significant at the 1% level). Again, compared to Panel C of Table A.9 with documented point estimates of around 0.55, these results are very consistent.

Table D.4: OLS Robustness Results: TFP Growth

	Covariates: ln(rev) & ln(empl)			Covariates: ln(rev) & ln(empl), poly 2			Covariates: Baseline/proportional initial capital			No imputation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Binding contracts	0.097***	0.112***	0.114***	0.092***	0.107***	0.109***	0.063	0.066	0.068*	0.1019
	(0.034)	(0.035)	(0.036)	(0.032)	(0.034)	(0.034)	(0.037)	(0.038)	(0.039)	(0.251)
Observations	1,962	1,962	1,962	1,961	1,961	1,961	1,962	1,962	1,962	91
TFP growth	.474	.474	.474	.486	.486	.486	.556	.556	.556	.386
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Industry controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Purchasing price	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Investment target	No	No	Yes	No	No	Yes	No	No	Yes	No

Notes: The table shows OLS regression results of TFP growth on binding contracts for different lasso specifications to impute investment for constructing firm-level capital stock. Columns (1)-(3) use as covariates ln(revenue) and ln(employment). Columns (4)-(5) use as covariates ln(revenue) and ln(employment) with a polynomial degree of order 2. Columns (7)-(8) use as covariates the baseline revenue and employment introduced with size dummies. All lasso specification include 259 4-digit sector dummies. Column (10) provides the results without investment imputation. All regression specifications control for fully interacted THA agency and year fixed effects. Binding contracts are defined as initial firm size below the committed target level. Baseline controls are as in the baseline specification. The purchasing price is flexibly introduced using decile dummies. Investment targets is a dummy is the contract contains investment commitments. Standard errors are two-way clustered at privatizer and THA office level. *p<0.1, **p<0.05, ***p<0.01.

Table D.4 shows OLS estimation results with different specifications of the imputation of the capital stock variables (conditional on survival). The first six columns use revenue and employment in different combinations, whereas columns (7) to (9) approximate the initial capital stock measured in 1990 proportional to the employment share in the year of the contract. All specifications provide positive point estimates between 0.07 and 0.12% points. The final column (10) provides the results without the imputation of the capital stock. Following the description in Section D.1, this results in a sample size of 91 observations. Although insignificant due to the sample size, the point estimate is with 0.102, rather close to the specifications with imputed capital stock.