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Committing to Grow: Size-Dependent Regulations and Firm Dynamics in East Germany*

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Abstract

We study the implications of employment targets on firm dynamics during the privatization of the East German economy. Exploiting novel contract-level data, we document three stylized facts. First, the policy distorted firm size choices and generated bunching of firms around their committed employment target. Second, exploiting heterogeneous labor preferences of privatizers, we show that assigning tight commitments to firms causes an increase in employment growth and leads to higher productivity growth. Finally, tighter commitments also result in significant costs by leading to increased firm exit. We interpret these results through the lens of a dynamic model with endogenous productivity growth at the firm level. The model highlights that while tight commitments distort the employment decision statically and lead to a higher exit probability, they also induce a "catch-up" increase in productivity growth. This is because although firm profits are lower under tight commitments, marginal profits with respect to productivity are higher. We calibrate the model to our data and find that the policy lead to a 3 percentage points higher aggregate TFP growth thanks to the productivity improvements of firms with tight contracts.

Keywords: size-dependent regulations, industrial policy, productivity, endogenous growth.

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JEL classification: D22, D24, J08, L25.

1 Introduction

The past decade has witnessed a revival of industrial policies across the world. Such policies directly affect resource allocation by stimulating specific economic activities and promoting structural change. The case for industrial policy lies in the existence of market failures induced by externalities that create scope for welfare-improving regulation and taxation. In this view static efficiency costs due to temporary distortions of industrial policies are far outweighed by their dynamic benefits (Gerschenkron 1962). An alternative view is that the effectiveness of these policies is limited in environments in which information, incentives, and selection are important (Acemoglu, Aghion, and Zilibotti 2006; Shleifer 1998; Rodrik 2004).

The German reunification represents a unique setting in which to study industrial policies and reallocation dynamics. The German Democratic Republic (GDR) created the *Treuhandanstalt* (THA) in June 1990 to implement the rapid transformation of a state-controlled economy into a modern market economy.[1] The agency was entrusted with the mission of privatising more than 12000 firms and conglomerates employing a workforce of about 4.5 million East German workers. A key concern during the sales process centered around the social costs of unemployment and downsizing of activities (Siebert 1991). To address these issues, THA asked buyers to commit to employment targets. These targets were firm-specific, lasted on average for 3 years, and were enforced through audits and penalties proportional to missing employment (Küpper and Mayr 1993). Quantitatively, labor commitments covered about 18,000 contracts representing more than 900,000 East German workers.

In this paper, we study the implications of this policy in terms for firm dynamics and aggregate productivity. The type of policy intervention we analyse is unique, insofar as it primarily puts constraints on firms labor choices as opposed to using subsidies. Our objective will therefore be to quantify the welfare implications of this policy by considering not only the costs arising from these constraints on firm optimisation, but also their potential dynamic benefits in terms of incentivizing productivity growth.

The analysis relies on a unique dataset from the German Archives (*Bundesarchiv*) that contains all contracts and documentation produced by the *Treuhandanstalt* (THA). These data are confidential, and, thanks to an institutional cooperation, we are 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 contain detailed contract-level information not only about the privatisation of assets, but also about all agreed-upon economic and financial commitments. In terms of employment commitments, we will be able to not only rely on detailed information about agreed-upon targets and deadlines included in privatisation contracts, but we will also be able to measure the dates and results of each on-site audit.

The first step in the analysis is to empirically assess the importance of employment targets in affecting firm's labor choices. To do so, we estimate the size of the implied labor distortions through

[1] The GDR created a first version of the *Treuhandanstalt* (UrTHA) in March 1990 to organize public property. However, the conservative party CDU won the last GDR elections (*Volkskammerwahl*) and the new coalition passed legislation in June 1990 that restructured the THA (Fischer, Hax, and Schneider 1993).

a bunching analysis. Two features of our empirical design naturally lend themselves to this exercise (Kleven 2016). First, bunching designs consider the case in which the assignment variable is a direct choice, and we are interested in the endogenous response to the kinked incentives set by policy or regulation. Second, we can use the detailed contract- and audit-level data to precisely measure the ex-ante committed level of employment and the results of on-site audits determining compliance. We estimate an excess mass around the threshold of 652% relative to the average height of the counterfactual distribution.

In the second step of the analysis we address the endogeneity of the assigned employment target. We leverage our contract-level data to estimate the propensity of a privatizer to require tight labor commitments, i.e., labor targets that force the firm to grow. To address the own-observation problem, we follow the literature by estimating a leave-one-out measure of a privatizer's labor preferences. The 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. The break-neck speed of privatisations also 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 privatisation, each THA agent had a lot of leeway in order to determine conditions for the firm to be privatized.

We find that the probability of receiving a tight contract increases continuously along the labor preference measure, e.g., moving from the lowest decile to the highest decile increases the probability of assigning a tight contract by 21% points. These preferences are not only heterogeneous across privatizers but also persistent across time. We also provide balancing tests of the assignment mechanism of firms to privatizers. The test exploits pre-assignment information on the initial 12,500 firms that submitted their opening balance sheets in July 1990. We are able to link the sales contracts to these initial units and test whether firm-level characteristics correlate with our measure of privatizer stringency. We find no evidence of economically or statistically significant correlation of our instrument with a wide range of sectoral characteristics, employment and revenue measures, as well as other individual characteristics of the privatizers.

We first embed our instrument into a 2SLS equation relating THA's employment targets to employment growth. OLS estimates suggest the effect of tight contracts on firm growth to be 49% points until the final commitment date. Once the assignment of a tight labor contract is instrumented by privatizer preferences, the estimate increases in size. The causal effect of tight contracts on employment growth is estimated to be 67% points. The effect is not only economically large but also precisely estimated at the 1% significance level. The first stage statistics for weak instruments is large, indicating that weak identification issues do not apply in our setting. In addition, we provide a wide range of robustness checks relating to the construction of the instrument, control variables, and measurements of the dependent variables.

To disentangle the sources of employment growth we extend the empirical analysis to productivity and exit dynamics of these firms. In terms of productivity, we find that firms with tight labor commitments experience a 8 to 14% points higher overall growth with respect to firms with non-tight

commitments. Once we instrument for the assignment of these targets, the implied yearly productivity growth is substantially larger. Over the course of, on average, five years measured between July 1990 and the end of commitment period, firms with tight labor contracts experience a yearly productivity growth of roughly 14% points. Again, the estimates are robust and supported by additional patent-based evidence. In terms of selection, firms in our sample have on average a 5.5% probability of exit until the end of the commitment period. OLS estimates suggest that tight contracts create financial fragility leading to an increase in the exit probability. The IV estimation again suggests an downward bias of the OLS point estimate.

To interpret our empirical results we introduce a dynamic model of firms with heterogeneous productivity, endogenous productivity growth at the firm level, and employment targets. The model highlights two key implications of the existence of employment commitments on firm dynamics. First, the model indicates a direct effect of tight commitments on firms profits and exit probability. The combination of upward distorted labor choices and the penalty lowers firm profits and generate higher exit rates. Second, distorted firms have a higher marginal profit with respect to increases in productivity and will dynamically be more willing to invest in productivity improvements. This "catch-up" effect is intuitive as increasing productivity not only increases profits but also reduces the amount of distortions and penalties.

We estimate the model parameters by matching model-simulated moments and empirical moments from our data. The most important parameter in the model is the cost of not hitting the employment target. To discipline this parameter we target several moments from the bunching analysis, estimates of employment and productivity growth for firms with tight commitments, and exit rates for tight commitments. We also pin down initial distortions in the economy as well as investment and exit cost parameters by matching average growth and exit patterns of our overall sample.

Our estimates allow us to not only decompose the direct and indirect channels of the policy, but also to quantify its aggregate implications for productivity. To do so we simulate an economy where we kill the "catch-up" effect by keeping marginal profits across firms constant. In other words, this counterfactual only includes the direct effect of employment targets. The decomposition suggest that 12% of of the observed employment growth can be attributed to the higher productivity growth induced by tight contracts. Consistent with the catch-up effect on productivity growth, we find a large contribution of the policy to aggregate productivity growth. During the three years of commitment policy, the economy experiences a 3 percentage points higher aggregate TFP growth thank to productivity improvements of firms with tight contracts.

The paper contributes to the recent literature revisiting the merits and costs of industrial policies. Lane (2019) and Choi and Levchenko (2021) use unique historical data to study the dynamic impact of the South Korean heavy and chemical industry drive from 1973 to 1979. Lane (2019) 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) disentangles the various subsidies during the intervention and estimates their impact. Finally, Liu (2019) embeds industrial policy in a production network setting and applies it to interventions in South Korea in the 1970's and modern-day China. 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 analyse 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 also contributes to the recent literature on the welfare consequences of size-dependent regulations that frequently favor smaller firms. These policies can potentially create distortions in the economy that affect aggregate productivity by misallocating resources toward less-productive firms (Restuccia and Rogerson 2008; Hsieh and Klenow 2009; Bartelsman, Haltiwanger, and Scarpetta 2013; Syverson 2011). Garicano, Lelarge, and Van Reenen (2016) leverage size-contingent laws in France to identify the equilibrium and welfare effects of labor regulation. The structural estimates suggest that the welfare cost amount to 3.4% of GDP and mainly fall on workers. 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 lead not only to increased entry but also higher output growth in more exposed districts. Similar to these studies we use firm-dependent employment targets to identify their impact on firm dynamics. However, in our setting, the policy actively pushes firms to grow or keep a larger size. Consequently the trade-offs generated by the policy are distinct from those associated to those that put barriers to growth.

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).[3] 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 practises (Dyck 1997), to labor reallocation (Dauth, Lee, Findeisen, and Porzio 2021) and migration (Uhlig

[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).

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

2008; Hunt 2006), capital investments from West Germany (Sinn 2002) as well as collective bargaining agreements (Burda and Hunt 2001; Burda 2010).[4] 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 innovation activity of firms as well as by imposing the least productive firms, through a selection effect, to exit the market.

The remainder of this paper is organized as follows. Section 2 provides an overview of the German institutional framework after reunification. In Section 3, we describe the data and provide descriptive statistics. Sections 4 and 5 reports our empirical results. The structural model and the quantitative estimation is shown in Section 6. Section 7 concludes.

2 Institutional Background

The government agency Treuhandanstalt (THA) was first established in March 1990 under the last Communist regime in East Germany. It was empowered in July 1990 under the terms of the *Treuhandgesetz*.[5] The THA became the largest holding-type company in the world as it took legal title to all the companies that had previously belonged to the state of East Germany. The THA began its task on July 1, 1990, with a portfolio containing about 12,000 companies and employing a total of about 4.5 million employees representing approximately 50% of the total workforce population.

The portfolio of firms under Treuhand management were inherited from the centrally planned structures of organized conglomerates. This resulted in 4000 *Kombinate* that included diverse and often unrelated activities. The first step in the transformation of these companies was to split these large conglomerates into firms organized under private law (*Entflechtung*). In a second step, these THA enterprises were required to submit an opening balance sheet in Deutsche Mark (*Eröffnungsbilanz*) and a business plan to the Treuhand. The privatization process further streamlined business activities as firms divested through assets sales.

To privatize assets, the THA relied on direct sales in cash. These assets not only represented wholesale privatizations of firms but also included divestitures and spin-offs from firm restructuring and liquidation. Contracts for sales included the sales price, and potential guarantees by purchasers of minimum levels of employment and investment. Such commitments by the buyer could result in discounts on the sales price, but THA had no set formula for valuing competing bids and each case was decided "on its own merits" (Dodds and Wächter 1993). To make commitments enforceable, penalty clauses were written into contracts that specified payments to the THA if the targets were not fulfilled.

The imposition of employment targets reflected the obligation placed on the THA by the govern-

[4]The convergence process started to level-off and stagnate at the end of 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.

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

ment to take account of the social costs of unemployment. Based on official numbers by the Federal Statistical Office, the unemployment rate in East Germany surged after reunification to 10.2% by 1991 and further increased to 15.7% in 1994. Economic activity within THA firms decreased strongly. Our estimation suggest that within the first three years after reunification, employment within THA-organized firms decreased by almost 70%. The massive increase in unemployment and THA decisions to systematically shut down economically distressed firms created social upheavals. In April 1991, the first president of the THA, Detlev K. Rohwedder, was assassinated. The murder is still unsolved but has been linked to the Red Army Faction, a West German far-left terrorist group. Similarly, employees in the former VEB Kaliwerk Bischofferode (part of the VEB Kombinat Kali in Sondershausen) entered a 81 days long hunger strike in summer 1993 after the THA decided in December 1992 to close the company (Bernhard 2011).

Given the institutional and social context, contracts typically included an agreed number of full-time equivalent jobs that should be maintained for an agreed period of time. This contract based policy instrument was negotiated with acquirers and aimed at preserving economic activity in East Germany (Siebert 1991; Fischer, Hax, and Schneider 1993). If the target number of employees was not reached, the investor had to pay a penalty. The Treuhand negotiated to have penalties set at a level that ensured that the agreed upon employment commitments were credible. Frequently, the agreed penalty negotiated in the contract approached the cost of keeping an employee on staff. The commitment to employment targets was subsequently monitored through multiple audits organized within the contract management system of the THA organization.

Treuhand 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 fifteen branch offices distributed among the major cities of the new federal states. Figure 1 represents the former 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).[6]

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 hiring and retention of qualified employees was especially challenging due to the temporary nature of THA's mission.[7] This also meant that the organization of the sales and restructuring process depended to a large extent on privatizers and consultants assigned to each firm. The intense workload,

[6]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.

[7]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)

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 indicated by the red cross.

within offices, resulted an allocation of privatizers to firms on a first come, first served principle. A former employee of the THA in those years described the allocation and organization process as "an exceptional situation where there was a lot of improvisation."

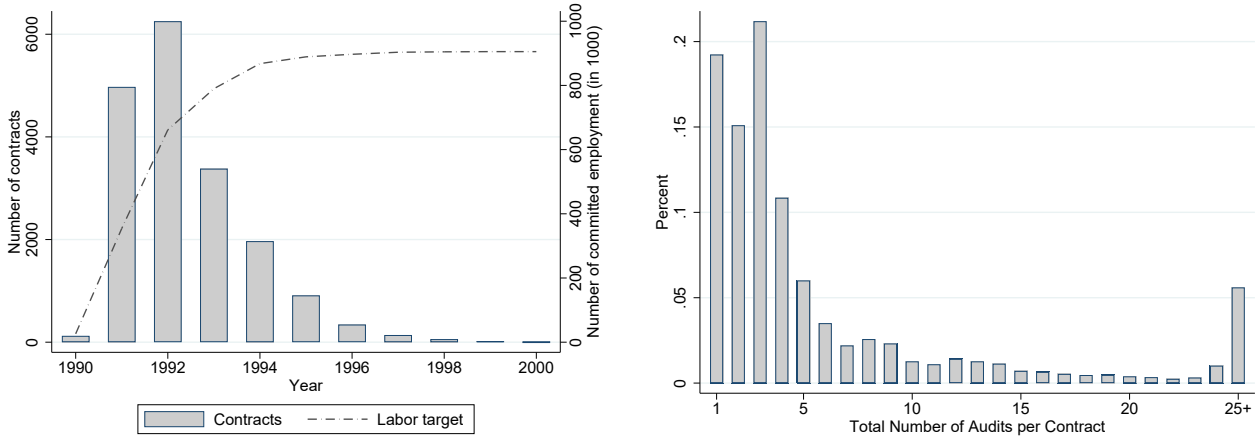
Based on our data for 13,051 Treuhand firm entities with ownership information, our calculation shows that 70.8% were privatized and 29.2% liquidated.[8] Reasons for the relatively high share of liquidated firms are discussed in the literature and mainly correspond to low competitiveness due to an inefficient cost structure and outdated production technologies, the collapse of the Soviet Union with an associated collapse of the main export market of the firms, low management skills, and the currency reform on July 1, 1990 that increased labor costs significantly (Akerlof, Rose, Yellen, Hessenius, Dornbusch, and Guitian 1991; Dornbusch and Wolf 1994; Dyck 1997). Moreover, Western German trade unions succeeded in rapidly transferring the system of collective bargaining to the East.

[8] Mergele, Hennicke, and Lubczyk (2020) show, based on survey data, that about 60% where privatized, 30% liquidated, and the remaining 10% restituted or municipalized.

3 Data and Descriptive Statistics

The analysis relies on a unique dataset from the German Federal Archive (*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 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 contain detailed contract-level information not only about the privatisation of assets, but also about all agreed-upon economic and financial commitments. We are able to not only rely on detailed information about agreed-upon targets and deadlines included in privatisation contracts, but also to measure the dates and results of each audit. Appendix Section B provides a detailed description of the explored ISUD data tables.

Quantitatively, 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,[9] and the committed level of employment along with the date of the final commitment. As shown in Panel A of Figure 2 the majority of all contract are signed between 1991 and 1993 (90% are signed between 1991 and 1994).[10] In total, these labor contracts amount to more than 900,000 committed workers, representing about 20% of the initial workforce population of THA firms.[11]



A: Number of contracts over time

B: Audit distribution

Figure 2: 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.

These contracts were audited on a regular basis by the THA via contract managers. The audits represent the source of realized employment levels we exploit in this paper. The contract managers

[9] For around 200 contracts, the date when the contract was signed by the notary is missing.

[10] Less then 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.

[11] Note that the aggregate committed workforce is based on the final labor commitments by each contract partner.

approach the contracting party and audit the commitments either physically by visiting the firm or via phone. As shown in Panel D of Table 1, the number of audits vary between 1 and 84. On average, each contract is audited 6.3 times. Panel B of Figure 2 shows that about 18% of all contracts are only audited once.

In order to analyze employment dynamics after privatization, we rely on at least 2 audits per contract. While we always observed the employment level at the final commitment date, we approximate the initial employment with the first labor audit. This audit is typically between six to seven 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, as well as the target level. 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 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 audit.

Table 1: Summary Statistics

	N	Mean	SD	Minimum	Maximum
	(1)	(2)	(3)	(4)	(5)
A: Average firm size					
Initial employment	14,726	66.20	319.57	0.00	23,691
Final employment	18,235	56.22	182.22	0.00	8,540
Final employment target	18,235	49.75	172.09	1.00	6,906
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: Accumulated labor numbers					
Total employment realizations (in 1000)	18,235	1025.12	0.00	1,025.12	1,025.12
Total labor commitments (in 1000)	18,235	907.26	0.00	907.26	907.26
D: Maturity and audits					
Contract length (in months)	18,046	37.24	18.76	1.00	185.00
Number of audits	18,235	6.33	8.58	1.00	84.00
E: 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)	1272	10.77	10.67	0.10	58.52
Fraction without payments	1,272	0.28	0.45	0.00	1.00
Final penalty per missed employee (in 1000 EUR)	1,272	3.42	8.02	0.00	56.73

Notes: The table shows summary statistics of privatization contracts. In Panel A, initial employment level is calculated for contracts with at least two observation. This corresponds to 14,726 contracts. In Panel E, we observe 1,272 contracts with at least one observed labor commitment violation.

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 *pro rata temporis* employee. Conditional on having at least one labor violation, the average firm deviates 2.2 times. Accumulating the number of violated commitments within each firm shows that, on average, 111 employees are missing. The maximum amounts to 8,567 which is above the maximum of the final employment target in Panel A. The reason is that within firms, there can be several violations, whereas the maximum number in Panel A corresponds to the final commitment level. Our calculation suggest that the average penalty per missed employee amounts to 10,768 EUR.

The data also contains information on actual payments which allows us to calculate the effective penalty per missed employee. The effective penalty per missed employee amounts to 3,424 EUR or 28% of the average wage of a full-time equivalent worker (Küpper and Mayr 1993). The effective penalty is lower because we observe firms that do not pay a positive amount, or, conditional on paying a positive amount, the payment is lower than the penalty. Around a third of infringements are observed to not pay anything. The discrepancy between legal and enforced penalty can be traced to clauses on "conditions beyond the purchaser's control" (Dodds and Wächter 1993), renegotiation, minor violations of targets (*Bagatellfall*), judicial decisions, and bankruptcy.

We next assess the impact of the employment targets by documenting endogenous labor responses around the employment target. We then provide causal estimates of how binding labor commitments affected firm employment growth, labor productivity, and market exit. To do so, we will exploit information privatizers and their preferences for labor targets.

4 Employment Bunching around Committed Targets

We first assess how employment targets affected firm employment policies by a bunching analysis allowing us to estimate the size of the distortions. The approach was initially developed to estimate behavioural responses to taxes (Saez 2010; Chetty, Friedman, Olsen, and Pistaferri 2011), but it is increasingly used to estimate the impact of regulatory distortions on firm productivity (Garicano, Lelarge, and Van Reenen 2016).

We argue that two features of our empirical design naturally lend themselves to this exercise (Kleven 2016). First, bunching designs consider the case in which the assignment variable is a direct choice, and we are interested in the endogenous response to the kinked incentives set by policy or regulation. Second, we can use the detailed contract- and audit-level data to precisely measure the ex-ante committed level of employment and the results of on-site audits determining compliance.

Figure 3 analyzes bunching around the final employment target. The horizontal axis measures the difference between the realized employment measured at the last audit and the final committed employment level. This definition allows us to represent all firms with respect to their normalized target. Firms below 0 are smaller in terms of their realized employment relative to the committed level, whereas firms above 0 have a larger employment with respect to their committed level. The

figure plots the bin counts around the normalized target shown by the red vertical line at zero, with each bin representing a unit of employment 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' optimisation policies.

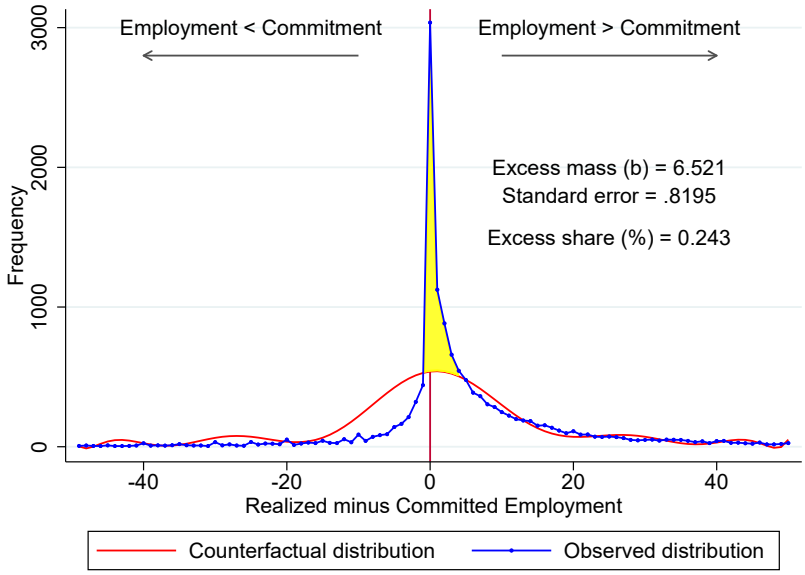


FIGURE 3: EMPLOYMENT DISTRIBUTION AROUND THE COMMITMENT LEVEL

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 have 3 employees more than committed. The shaded region in yellow is the estimated excess mass at, 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.

To accurately quantify the excess mass associated to the observed employment distribution we next estimate the counterfactual density, i.e., the employment distribution if firms where unconstrained in their hiring and firing decision. Consider a setting in which y denotes firm employment, and y^* the committed level of the variable. Total bunching, denoted by B, can be expressed as $B = \int_{y^*}^{y^*+\Delta y^*} f_c(y)dy$, where $f_c(y)$ is the baseline density that is assumed constant on the bunching segment $(y^*, \Delta y^*)$. To recover the counterfactual distribution, we fit a polynomial to the observed distribution and extrapolate the fitted distribution around the omitted threshold range. Empirically:

$$c_j = \sum_{i=0}^{p} \beta_i \cdot (y_j)^i + \sum_{i=y_-}^{y^+} \gamma_i \cdot 1[y_j = i] + v_j,$$

where c_j is the number of units in bin j; $[y_-, y^+]$ is the excluded range; and p is polynomial order. The difference between the observed and estimated distribution will then provide us with an estimate of the excess mass at the committed employment level, $B = \hat{c}_j - c_j$.

The red line in Figure 3 plots the estimated counterfactual density based on a twelve-degree poly-

nomial (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 3 more employees than their committed target. The yellow shaded region depicts the estimated excess mass around the threshold. Given the defined polynomial and the excluded range, we estimate $B = 6.52$, which corresponds to an excess mass around the threshold of 652% relative to the average height of the counterfactual distribution.

Qualitatively, these results do not change when changing p or R. We provide a series of robustness checks in the Appendix. Figure A.6 shows the results for varying the polynomial order p. A higher polynomial order fits the empirical distribution to the left of the excluded range more precisely but only decreases slightly the estimated excess mass to 5.79. Figure A.7 shows bunching estimates with symmetric R, ranging between 1 and 4. Defining the excluded rang to be one employee deviation relative to the target results in sizeable bunching of $b = 5.21$. Increasing the excluded rang increase the estimate of b. We further provide bunching evidence by re-defining the bins. Instead of calculating the deviation from the target in counts, we calculate the percentage deviation and construct percentage deviation bins. This changes the composition of the firms around the threshold towards larger firms because, e.g., missing one employee for a firm with 10 employees already results in a deviation of 10%. Figure A.8 shows the results for 1 percentage bins, 2 percentage bins and 5 percentage bins. In all specifications, we set the excluded rang to be $\pm$ one bin. Intuitively, increasing the bin size results in lower estimated excess mass. Using 5 percentage deviation bins still generates sizeable and significant bunching mass of 2.95, i.e., excess mass around the threshold to be 295% of the average height of the counterfactual distribution. Finally, we provide heterogeneous estimates in the amount of bunching by groups using the baseline specification. Table A.7 shows significant estimated excess mass for various ways of splitting the data (e.g. industry, contract maturity, initial size).

5 The Causal Effect of Tight Labor Commitments

5.1 Identification Strategy

Instrument Construction. An empirical challenge when analyzing firm-level responses to labor commitments arise because commitments are unlikely to be randomly allocated. To address this issue, we first develop a framework for reduced-form identification that draws from the methods developed in the literature 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 a fixed trait or preference of decision makers with respect to outcomes under their control (e.g. leniency or toughness). Combined with a quasi-random allocation of these decision makers, this estimate of the fixed trait can then be used as an exogenous shifter for 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. The break-neck speed of privatisations also 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 privatisation, each THA agent had a lot of leeway in order to determine conditions for the firm to be privatized.

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 shows the distribution of cases per privatizer. In order to calculate meaningful averages, 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. We provide robustness estimates using ten cases per privatizer.

The first step in the analysis is to estimate the propensity of a privatizer to require tight labor commitments, i.e., labor targets that force the firm to grow. To address the own-observation problem, we follow the literature by estimating a leave-one-out measure of commitment tightness:

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

where i denotes the firm, o the THA office, and j the assigned privatizer.[12] $Tight_i$ 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. Z_{ioj}, therefore, measures the leave-one-out tightness of labor requirements of THA privatizer j.

Figure 4 plots the relationship between tight 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 during a sale. Above the median of the leave-one-out distribution, the likelihood of receiving a tight contract is about 24%, while it is only 17% below the median. The probability of receiving a tight contract increases continuously along the stringency measure, e.g., moving from the lowest decile to the highest decile increases the probability of assigning a tight contract by 21% points.[13]

Random assignment. We can use this classification to indirectly test the assignment mechanism of cases to privatizers. The test exploits pre-assignment information on the initial 12,500 firms that submitted their opening balance sheets in July 1990. We are able to link the sales contracts to these initial units and test whether 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

[12] 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.

[13] Figure A.3 shows that preferences for tight 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.

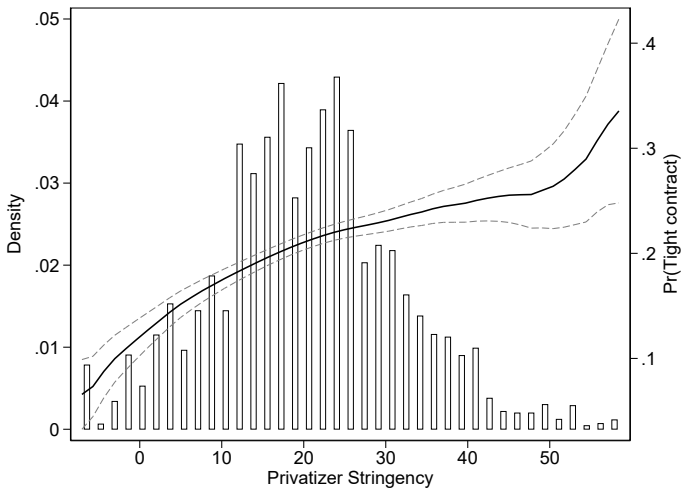


FIGURE 4: FIRST-STAGE ANALYSIS

Notes: The figure plots the probability of having a tight 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 tight 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 9,363.

for multiple testing using the procedure proposed by Romano and Wolf (2005b); Romano and Wolf (2005a) 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 at the 5% level. Adjusting for multiple testing, however, there is no statistically significant relationship between the privatizer stringency measure and sector affiliation. Finally, the data provides information on three individual-level characteristics of the privatizer. Therefore, we exploit this information to test if the stringency measure predicts the number of cases, the gender of the privatizer and whether he or she has a PhD degree. Again, results indicate no predictive power of our instrument. The last two rows show the regression results of our stringency measure on the probability of renegotiating contract conditions. As shown in Table 1, firms where able to renegotiate if committed targets where not achieved, which could indicate that the effective tightness of contracts might be lower. As born out by the table, the instrument is not correlated with future renegotiations. This is in line with the fact that the organization of privatizations and contract management took place in different THA units.

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. The regression model

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 charateristics					
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 2005b; Romano and Wolf 2005a) with 1,000 bootstrap replications. **p<0.1, **p<0.05, ***p<0.01.

can be written as:

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

with y_i, for example, indicating the growth rate of employment between the first and the final audit, ΔL_i.[14] 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.[15] 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 audit and the final audit to capture differences in commitment length across contracts. The parameter of interest is β, which measures the effect of assigning a tight contract (initial size at contract date is smaller than committed size) on the growth rate of the firm.

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

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

where Z_i denotes labor preferences of privatizer j assigned to case i. The model therefore estimates the local average treatment effect of THA labor requirements on firm employment policies. More precisely, we identify (under monotonicity and exogeneity) the causal effect of assigning a tight labor contract on firm performance among the group of compliers who would have received a different labor commitment had their cases been assigned to a less stringent privatizer.

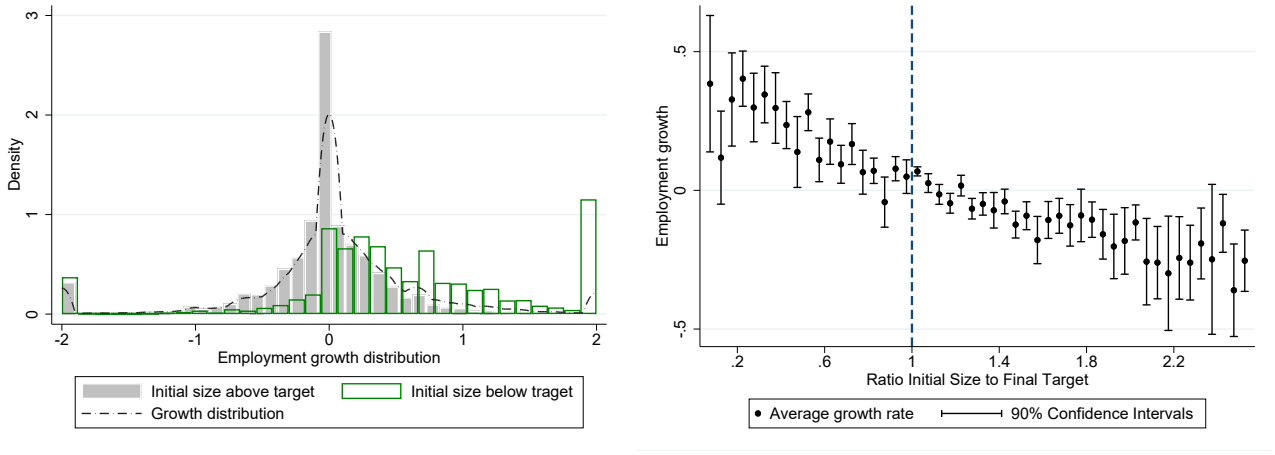
5.2 Tight Labor Commitments and Firm Growth

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 date of the final commitment as $\Delta L_i = (L_{it} - L_{it-1})/0.5(L_{it} + L_{it-1})$, 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.

Figure 5 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%. Weighting the growth rates by initial firm size shows that initially above target firms shrink by -11.2%, whereas initially below target firms grow by 26.7%.

[14] 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.

[15] Figure A.2 shows the initial firm size distribution by contract tightness.



A: Employment Growth B: Employment Growth by Initial Size to Target

Figure 5: 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.

The right panel of Figure 5 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 tightness of contract commitments and 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 figures suggest 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 tight labor commitments. Conditional on the set of baseline controls, industry dummies, and privatizer characteristics, the association between tight contracts employment growth is on 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.

The difference between the OLS and the IV estimates is sizable. IV estimates in columns (4) to (6) suggest that the causal effect of tight 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 tight 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. 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 tight labor contract. Consistent with the quasi-random assignment mechanism, the inclusion of

additional covariates does not affect the first-stage coefficients.[16]

Table 3: Regression results, employment growth

	OLS Model			IV-Model		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Second-stage results						
Tight 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	0.064	0.064	0.064	0.064	0.064	0.064
Share with tight 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 tight contracts. Panel A shows the reduced form regressing the tight 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 5 privatizations per privatizer. *F*-Statistic refers to the Kleibergen-Paap *F*-Statistic. Baseline controls are time between first and last audit 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 tight contracts. *p<0.1, **p<0.05, ***p<0.01.

A key challenge to our identification strategy is that privatizer decisions are multidimensional. THA privatizers not only negotiate 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 privatisation contracts. Table A.1 shows that adding extensive and intensive investment preferences does not affect our baseline results qualitatively.[17] Point estimates increase slightly from 0.68 to 0.7. In Panel A of Table A.3, we also control for subsequent renegotiation attempts initiated by buyers. Our estimated IV parameters remain quantitatively very stable across all specifications. The table also provides additional robustness checks. 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. All results generate a consistent picture that tight contracts cause the firm to grow. The table further shows that random assignment holds for the different sub-samples and alternative measures for the instrument.[18]

[16]Under heterogeneous effects, our model specification requires monotonicity. Monotonicity implies that the first-stage coefficient is non-negative for sub-groups. In Table A.2 we provide first-stage regression results for different firm size groups measured in 1990 using the link to the initial GRD firm as well as sub-samples according to the sector affiliation. The table shows that our results are consistent with the monotonicity assumption.

[17]Figure A.4 shows at the level of the privatizer that preferences for labor commitments at the extensive margin correlate positively with preferences for investment commitments. Likewise, preferences for tight labor commitments are positively correlated with tight investment commitments (measured as investment target over initial employment level).

[18]We argue that the differenes between OLS and IV represents a selection effect. However, it might be possible that

5.3 Tight Labor Commitments and Growth in Labor Productivity

To disentangle the mechanism behind the growth in employment we extend the empirical analysis to productivity measures. The audits of the contract-level data, unfortunately, do not contain direct information about sales after privatization. We exploit two sources to measure sales. The first source exploits investor information in the contracts where we link the names of contract partners to ownership data in the Mannheim Enterprise Panel (MUP). The merge allows us to construct labor productivity numbers starting in the year 1993. Given the average contract maturity of three years for labor commitments, we are able to measure firm output at the end of the commitment period for almost all firms that are linked. Supplementary Appendix C provides a description of the merge between the two datasets.

Our measure of initial labor productivity is directly computed as the ratio of sales per worker reported in the opening balance sheets of the THA firms in July 1990. We use this measure of labor productivity to approximate labor productivity at the time of the privatization. Given that we know the size of the firms in terms of employment at privatization from the contracts (first observed audit), we impute the sales numbers such that the ratio of sales per worker at privatization corresponds to the ratio measured in July of 1990. This adjustment allows for a cleaner computation of productivity growth over the commitment period. We provide robustness checks in the Appendix using the ratio of sales per worker reported in July 1990. In the resulting sample of 2,395 privatization contracts, we measure labor productivity as sales per worker at the year of the final commitment and the initial year at the time of privatization.

Figure 6 describes the relationship between productivity growth and initial tightness of labor commitments. The figure plots local linear regressions with degree 1 on both sides of the threshold, i.e., below the vertical line indicating tight contracts. The fitted blue line represents a linear regression for firms above or at the threshold of tight contracts, while the red line projects this linear fit below the threshold, i.e., for firms with initial employment below their committed level. The figure provides two major insights. First, growth in labor productivity is relatively constant for firms above the threshold for tight contracts. The average growth rate in the data amounts to 86.8% which indicates substantial improvements in labor productivity during the first years after reunification. Second, productivity growth is significantly higher for firms initially below their committed employment.

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

the observed differences are due to effect heterogeneity. In order to test effect heterogeneity, we first perform a principal component analysis (PCA) 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.4 shows that re-weighting based on observed characteristics increases the OLS estimate slight 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.

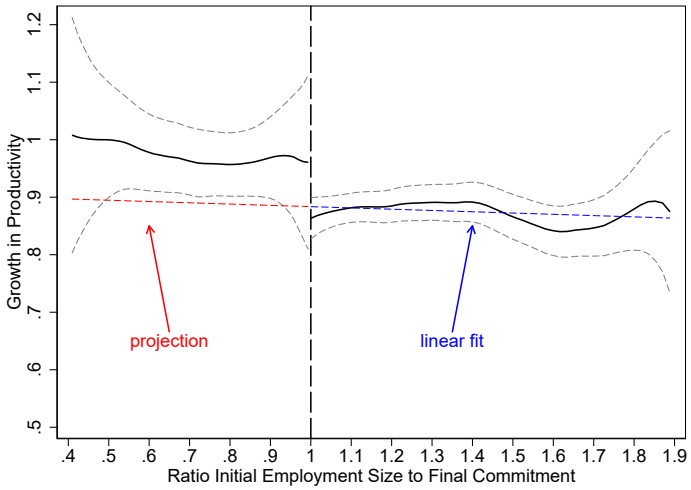


FIGURE 6: LABOR PRODUCTIVITY GROWTH AND THE DEGREE OF BINDING CONTRACTS

Notes: The figure plots the growth in labor 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 2,395.

date and the first audit measured in months.[19] We provide three different versions of the outcome variable. We first measure productivity through the ratio of sales per worker. We then consider a productivity measure that is consistent with the structural model introduced in Section 6. Specifically, based on a decreasing returns to scale production technology with labor, we compute firm level productivity as $sales/employment^{\alpha}$ where α is the labor share. In the same spirit, the table provides results by calculating sales over employment as $sales^{\epsilon/(\epsilon-1)}/employment$. With this transformation, as $\epsilon \to \infty$ the term collapses to the simple ratio of sales over employment. In the baseline empirical analysis, we set $\alpha = 0.8$ and $\epsilon = 2$. Appendix Table A.6 provides results for different values of α and ϵ.

Columns (1)-(3) of Table 4 provide evidence that, depending on the transformation, firms with tight labor commitments experience higher labor productivity growth between 7 and 14% points. This result is consistent with the graphical observation in Figure 6. The last three columns provide IV evidence on productivity growth. To do so, we implement a two-sample 2SLS estimation by using the predicted values from the full sample (conditional on having at least five observations per privatizer) in the second stage regression where we only have information of productivity growth for a subset of contracts.[20] To calculate the standard errors in columns (5)-(7), we perform 2,500 bootstrap replications presented in parenthesis as well as clustered at the level of the privatizer presented in squared brackets. Over the course of five years measured between July 1990 and the end of commitment period, on average, firms with tight labor contracts experience a yearly productivity growth of roughly

[19] We observe 16.2% of the contract without information of the individual privatizer.

[20] Note that the sample would be too small to run the fully nested IV specification.

Table 4: Regression results, labor productivity growth

	OLS			2S2SLS		
	Baseline	$\alpha = 0.8$	$\epsilon = 2$	Baseline	$\alpha = 0.8$	$\epsilon = 2$
	(1)	(2)	(3)	(4)	(5)	(6)
Tight contract	0.0658**	0.0834***	0.1447***	0.6267*	0.7109**	1.1530**
	(0.026)	(0.023)	(0.036)	(0.368)	(0.363)	(0.565)
Observations	2,395	2,395	2,395	1,612	1,612	1,612
Average productivity at contract date	9.992	10.599	23.021	9.989	10.598	23.025
Average productivity growth rate	0.884	0.875	1.148	0.881	0.876	1.165
Share with tight contracts	0.171	0.171	0.171	0.155	0.155	0.155
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	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table shows OLS and 2S2SLS regression results of measures of labor productivity growth on tight contracts. All specifications control for fully interacted THA agency and year fixed effects. Tight 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 (flexibly introduced using decile dummies). 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.

14% points. Reassuringly, the magnitude and direction of biases in the estimates are consistent with those in the employment analysis. At the same time, their interpretation is complicated due to the smaller sample size.[21]

Appendix Table A.6 provides four robustness checks for different values of α and ϵ as well as covariates included in the model. Similar to the baseline specification presented in Table 3, Panel A of Table A.6 shows the results including only baseline and industry controls. Panel B introduces not only the purchasing price but also controls for having investment targets. Panel C shows the results including firms that exit the market until the final commitment date. In Panel D we provide results that differ by ϵ. In Panel E, we shows the estimation results by using actual sales and employment numbers measured in July of 1990. As born out by the table, the effect of tight contracts on labor productivity growth is robust.

5.4 Tight Labor Commitments and Market Exit

Finally, we document the effect of tight labor commitments on market exit. For the exit analysis we first take the merged sample of contracts to MUP firms described in Append C and exploit the exit indicator available in the dataset. In this situation, the sample size is higher compared to the labor productivity analysis, because of missing data points in the sales variable. A second measure of exit is based on the final labor audit reporting 0 workers.

Similar to Figure 6, Figure 7 describes the relationship between the exit probability and initial tightness of labor commitments. Again, the share of firms exiting the market is relatively constant for less tight or non-tight firms (above the vertical line of 1). The exit rate for these firms amounts

[21] Appendix Table A.5 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 tight contracts and patenting. Although imprecisely estimated, the 2S2SLS coefficient shows again a downward bias of the OLS point estimate.

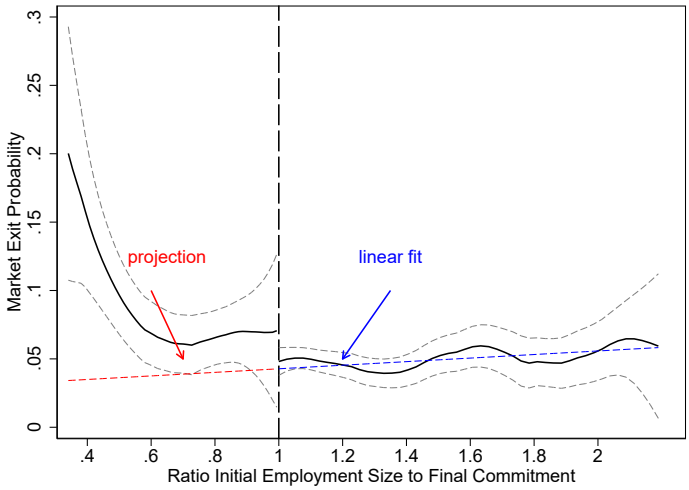


FIGURE 7: MARKET EXIT AND THE DEGREE OF BINDING CONTRACTS

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 4,596.

to 4.8% on average. With an average exit rate of 8.4%, firms with tight contracts show a higher level of market exit that is increasing in the tightness measure. This indicates descriptively a downside of this policy. Although firms react by increasing the level of employment and subsequently the level of innovation intensity, which results in higher labor productivity responses; for some firms, this policy seems to generate financial fragility, forcing firms to exit the market.

Table 5 provides a regression version of Figure 7 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). Interestingly, both regression specifications generate similar OLS results. Tight contracts are associated with an increase in market exit between 2.5 and 2.6% points. The IV implementation provides some evidence for a downward bias of the OLS point estimate. Note that with the second definition of market exit, we are also able to run the analysis with the full sample. The fully nested IV results point to an even larger exit effect of 0.18 (significant at the 10% level).

These empirical results suggest that the policy not only pushes firms to grow in terms of employment and innovative activity, but comes with the side effect of forcing some firms to exit the market.

6 Model and Quantitative Analysis

In this section, we introduce a dynamic model of firms with heterogeneous productivity, endogenous productivity growth at the firm level, and employment targets to interpret our empirical results. We start with describing the model environment, then discuss the calibration of the model to the firm

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)
Tight 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	0.055	0.055	0.055	0.010	0.010	0.010
Share with tight 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. Tight 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 (flexibly introduced using decile dummies). 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.

level data, and lastly provide several counterfactual analysis to quantify the different channels at work.

6.1 The Model

We consider an economy in continuous time, populated by a large number of heterogeneous firms in productivity producing a homogeneous good. At any point in time, firms choose (i) the amount of labor to hire for production, (ii) how much to invest on improving firm productivity, and (ii) whether to exit the economy of not. In the baseline specification, we assume that labor supply is perfectly elastic, i.e. wage growth is exogenous.

6.1.1 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 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 is firm specific. Consistent with the institutional framework, firms pay a penalty if they do not maintain the target level of employment, which 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

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

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

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

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

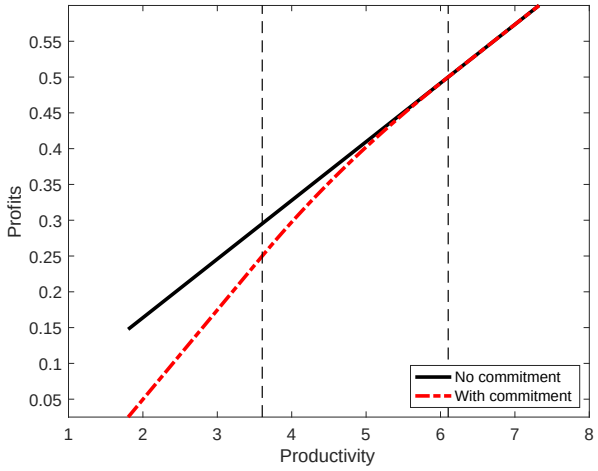
The above lemma suggests that firms' optimal employment and the resulting profits are positively correlated with the level of (relative) productivity, $\tilde{z}$, and can be characterized based on three productivity regions. For high productivity firms, $\tilde{z} > \tilde{z}_*$, the labor choices are not distorted and optimal with respect to their productivity. 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 implied by the productivity levels. 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 tight contracts, i.e. having an employment target bigger than the current employment, experience a higher employment growth for a given level of productivity: their employment is simply biased upward towards the target. We refer to this channel as the "direct" effect of tight labor contracts on employment growth.

Figure 8 illustrates two key implications of the existence of employment commitments on firm dynamics. The left panel plots total profits according to a transformed version of 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 and will therefore be more likely to exit. At the same time, the right panel plots *marginal* profits across firm productivity and shows that distorted firms have a higher marginal profit with respect to increases in productivity. This is intuitive: increasing productivity not only increases profits but also reduces the amount of

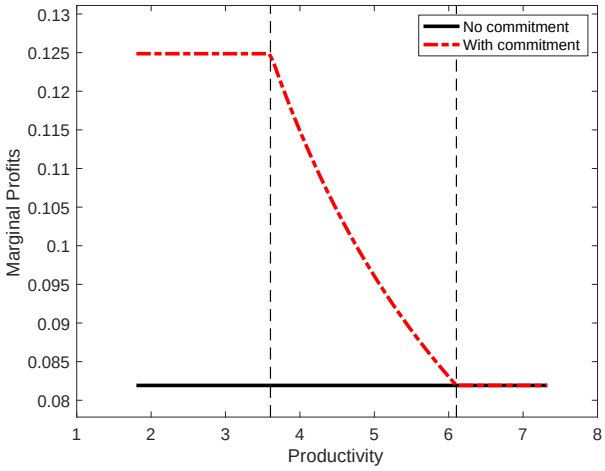
distortion (if the firm is bunching) or penalty paid (if the firm is not bunching) for those firms with tight contracts. In a dynamic setting, this implies that the increase in profits from productivity improvements will be higher for distorted firms relative to undistorted ones, i.e. distorted firms would be more willing to invest in productivity improvements. This implication of the model is consistent with the empirical result that firms with tight labor contracts experience higher labor productivity growth, which we refer to as the "catch-up" effect of tight contracts on productivity. Since the labor choice is positively correlated with the level of productivity, this higher productivity growth by tight-contract-firms constitutes the second channel for higher employment growth through its dynamic implications on productivity growth.

FIGURE 8: PROFITS ACROSS FIRM PRODUCTIVITY

PANEL A: TOTAL PROFITS



PANEL B: MARGINAL PROFITS



6.1.2 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, which we parameterize with C_e. We normalize the outside option to zero, without loss of generality. 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)

$$c(x|\tilde{z}) = \frac{\phi}{2} x^2 \tilde{z}^{\frac{1}{1-\alpha}} w$$

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 is chosen such 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 expires 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

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

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.[22] 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. This problem implies the following expression for the optimal level of investment in productivity (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 the 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 8: it is higher for firms that are distorted by the tight employment targets.

6.2 Calibration and Quantitative Analysis

To perform the analysis and the policy experiments described in the introduction, we first calibrate the parameters of our model. We set the labor share parameter in the production function $\alpha = 0.8$ to match the labor earning share. The arrival rate of employment contract expiration, μ, is set to $1/3$ which is consistent with the average contract length of three years in data. The rest of the parameters are calibrated 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. 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 chose Ω to minimize the absolute relative deviation between the model and data; that is, we solve

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

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

Table 6 and 7 contain the calibrated parameters and the targeted moments, respectively. The most important parameter in the model is the cost of not hitting the employment target, γ. To discipline this parameter, we target several moments: the share of the firms bunching at their target employment levels and those who stay below their target by the end of the commitment period; as well as the point estimates of the effect of tight contracts on employment growth and productivity growth. We also include the total employment growth and exit rate of firms with tight contract to identify the investment cost parameter (ϕ) and exit cost parameter (C_e) in our internal calibration. As seen from Table 7, despite the over-identification of matching 6 moments with 3 parameters, the fit is quite good.

Table 6: Internally Calibrated Parameters

#	Description	Model	Estimate
	Penalty for not hitting target employment	γ	0.379
	Scale for investment cost parameter	ϕ	0.099
	Cost of exit	C_e	48.47

Table 7: Moments Used in Calibration

#	Description	Model	Data
M_1	Share of firms at target	0.391	0.453
M_2	Share of firms below target	0.143	0.141
M_3	Employment growth regression	0.544	0.498
M_4	Productivity growth regression	0.083	0.083
M_5	Exit rate regression	0.031	0.027
M_6	Total employment growth	-0.050	-0.050

In Figure 9, we depict the employment growth by initial size to target employment, analogous to Panel B of Figure 5. Having a initial size to target employment smaller than one implies that the firm has a tight contract. The blue and red dots show the model implied employment growth rates and the data, respectively. Despite the fact that we only target the average excess growth rate of tight-contract firms (employment regression coefficient), the quantified model was able to match employment growth rate for the whole spectrum of employment to target ratios, which is reassuring in terms of model performance.

To quantify the effect of the employment targets on firm dynamics and aggregate economy, 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 value and set the cost of commitment to zero, $\gamma = 0$. Yellow dots in Figure 9 show the employment growth rate in this counterfactual economy. As seen from the figure, the employment growth rate is substantially reduced in the absence of employment targets, especially for those firms with tighter contracts.

Figure 10 decomposes the decrease in the employment growth rate in the counterfactual economy into "direct" and "indirect" effects of employment commitments. For this, we simulate a second

Figure 9: Employment Growth

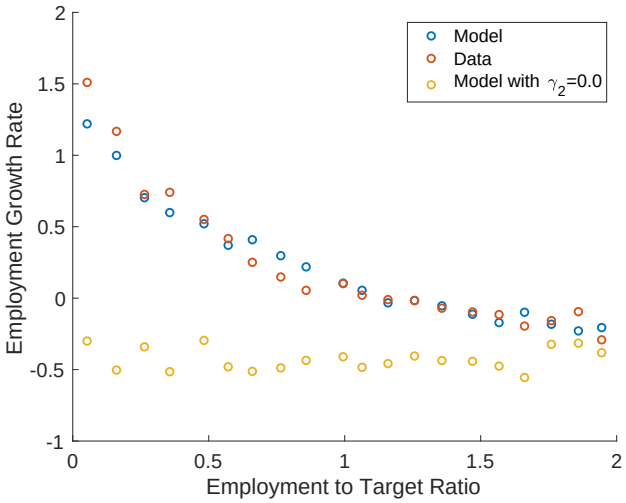
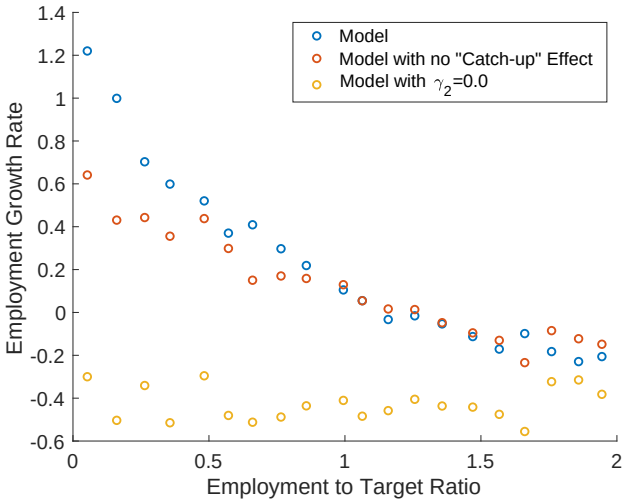


Figure 10: Decomposing Employment Growth



counterfactual economy where firms still operate under employment targets but we kill the "catch-up" productivity growth effect by simply assuming that marginal profits are the same across firms. In other words, the second counterfactual economy only includes the direct effect of employment targets on employment growth which is depicted with the red dots in Figure 10. The decomposition results suggest that catch-up productivity growth contributed to the employment growth by a non-trivial amount, which is especially significant for firm with very tight contracts. Overall, our exercise suggests that 12% percent of the aggregate employment growth can be attributed to the higher productivity growth induced by tight contracts.

Figure 11: Aggregate TFP Growth Rate

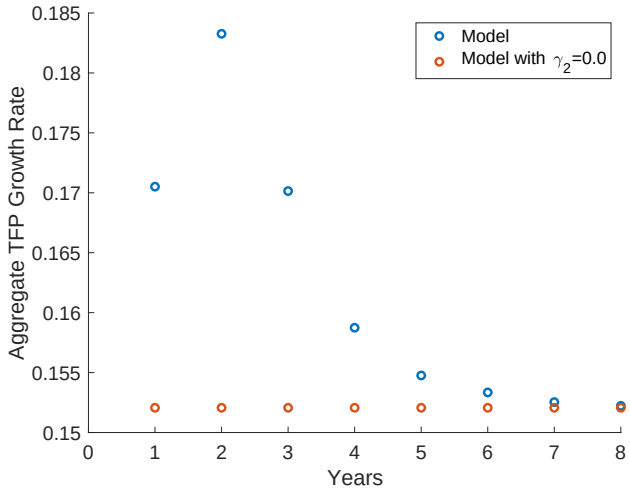


Figure 11 shows the aggregate implications of employment commitments on aggregate TFP growth. Consistent with the catch-up effect on productivity growth, tight contracts contributed to aggregate productivity growth significantly in early years (blue dots), relative to the counterfactual economy of

no employment targets (red dots). For the first three years, our results suggests around a 3 percentage point higher aggregate TFP growth rate thanks to the firms with tight contracts investing more on productivity growth. Later years, the discrepancy between baseline and counterfactual economy closes as the contracts expire. This higher TFP growth, however, comes with a cost of higher exit rate in the economy: in the counterfactual economy where there are no employment targets, the exit rate is lower at 3.3%, as oppose to 5.4% in the baseline calibration.

7 Conclusion

The German reunification represents a unique setting in which to study industrial policies and reallocation dynamics. 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 analyse is unique, insofar as it primarily puts constraints on firms labor choices as opposed to using subsidies.

Exploiting novel contract-level data, we document three stylized facts. First, the policy distorted firm size choices and generated bunching of firms around their committed employment target. Second, exploiting heterogeneous labor preferences of privatizers, we show that assigning tight commitments to firms causes an increase in employment growth and leads to higher productivity growth. Finally, tighter commitments also result in significant costs by leading to increased firm exit.

We interpret these results through the lens of a dynamic model with endogenous productivity growth at the firm level. The model highlights that while tight commitments distort the employment decision statically and lead to a higher exit probability, they also induce a "catch-up" increase in productivity growth. This is because although firm profits are lower under tight commitments, marginal profits with respect to productivity are higher.

Our structural estimates allow us to not only decompose the direct and indirect channels of the policy, but also to quantify its aggregate implications for productivity. The decomposition suggests that 12% of of the observed employment growth can be attributed to the higher productivity growth induced by tight contracts. Consistent with the catch-up effect on firm-level productivity growth, we find a large contribution of the policy to aggregate productivity growth. During the three years of commitment policy, the economy experiences a 3 percentage points higher aggregate TFP growth thank to productivity improvements of firms with tight contracts.

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Supplementary Appendix

A Further Empirical Results

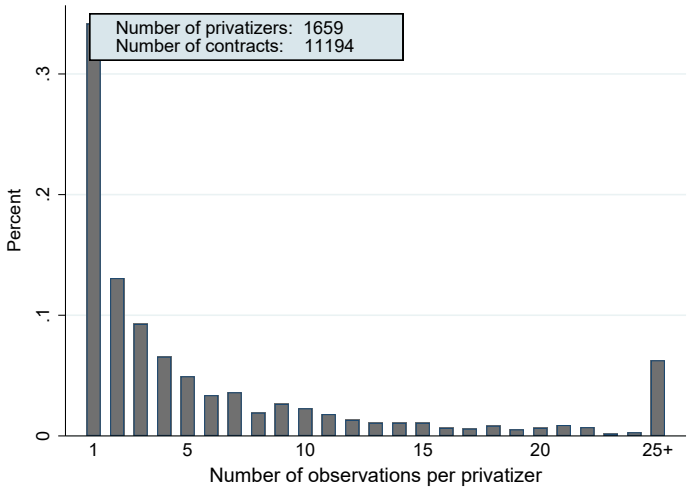


Figure A.1: Privatizations per privatizer

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.

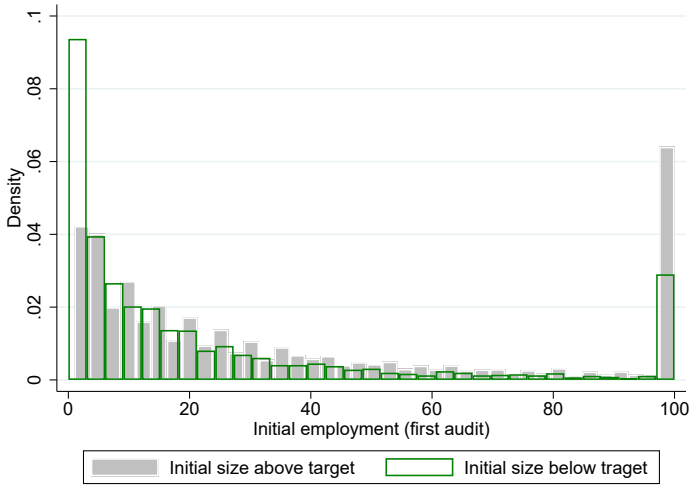


Figure A.2: Initial Firm Size by Contract Type

Notes: The figure plots the distribution of initial firm size by firms initially above their committed labor target (grey bars) and firms initially below their labor target (green bars). Initial employment is measured at the first labor audit. The employment numbers are winsorized at 100 for the shake of visibility.

Table A.1: Regression results accounting for extensive margin preferences

	IV-Model Results			First stage
	(1)	(2)	(3)	(4)
Tight 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 tight 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 log time between first and last audit (+1) measured in day, 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.

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 tight 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 time between first and last audit 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 tight contracts. **p<0.1, **p<0.05, ***p<0.01.

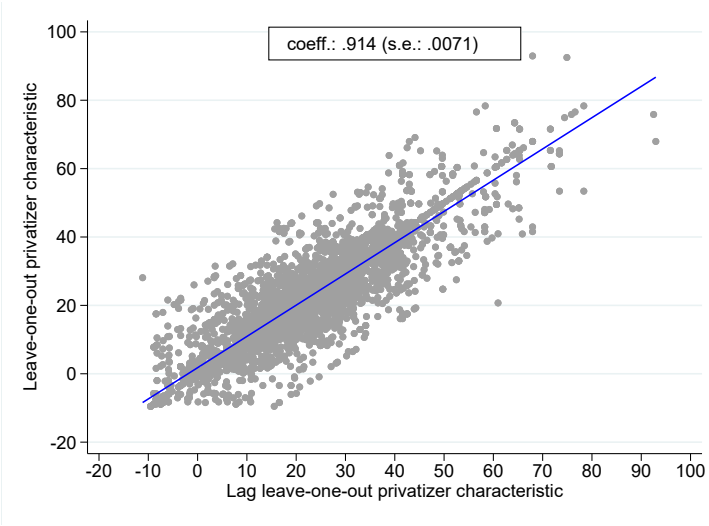
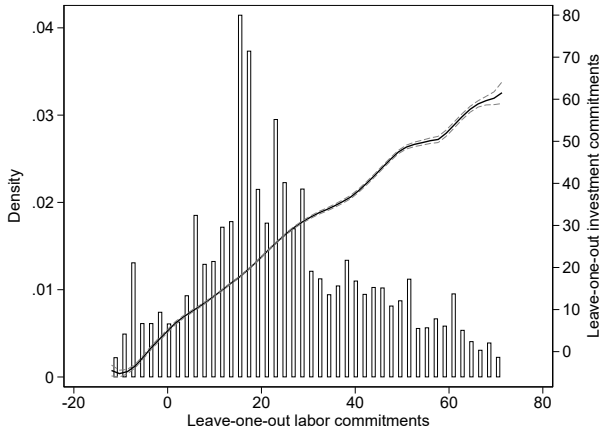
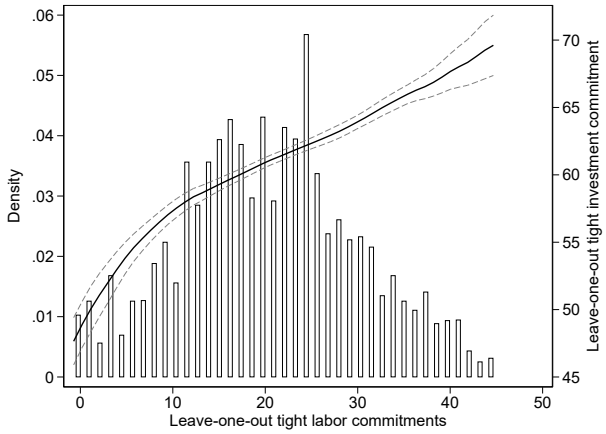


Figure A.3: Persistence of privatizer characteristics

Notes: The figure plots the leave-one-out rate of a tight contract (initial firm size < final committed size) in the previoue case against the leave-one-out rate of a tight 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 8,759.



Extensive margin (*N=52,446*)



Intensive margin (*N=8,987*)

Figure A.4: Correlation of preferences

Notes: The figure plots privatizer preferences at the extensive (left panel) and intensive (right panel). The left panel shows the correlation between the leave-one-out measure of assigning a labor contract against the leave-one-out measure of assigning an investment contract. The right panel shows the correlation between the leave-one-out measure of assigning a tight labor contract against the leave-one-out measure of assigning a tight investment contract (measured as the upper decile of the distribution of investment target over initial employment). The right panel is conditional on having a contract with labor and investment commitment. The plotted solid line corresponds to a local linear regression. 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 office and 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 3% of the labor stringency measure.

Table A.3: Robustness tests, employment growth

	Dep. variable: Firm growth			Random Assignment
	Coefficient	First-stage	*F*-Statistic	Joint *F*-test (*p*-value)
	(1)	(2)	(3)	(4)
Panel 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)		
Panel 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)		
Panel 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 first and last audit (+1) measured in day, 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.4: OLS Regression Results with Different Control Variables & Weighting

	OLS-Model Results		
	(1)	(2)	(3)
Panel A: Baseline			
Tight contract	0.4992***	0.4975***	0.4975***
	(0.031)	(0.030)	(0.030)
Panel B: Complier re-weighting			
Tight 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 tight 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 first and last audit (+1) measured in day, 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.5: Regression results, cumulative patents during commitment period

	OLS			2S2SLS
	(1)	(2)	(3)	(4)
Tight contract	0.0050**	0.0049*	0.0061**	0.0081
	(0.002)	(0.002)	(0.003)	(0.059)
Observations	4,563	4,563	4,563	1,430
Mean of Y of tight contracts	.012	.012	.012	.012
Mean of Y of non-tight 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. Tight 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 and clustered at the privatizer level. *p<0.1, **p<0.05, ***p<0.01.

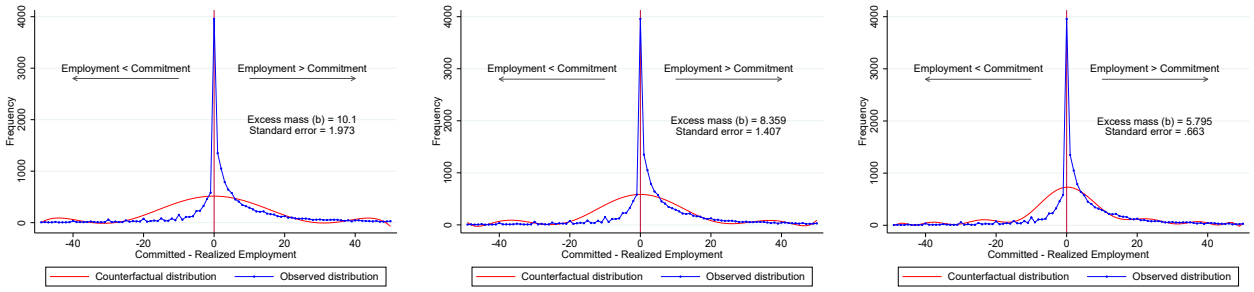
Table A.6: Robustness tests, labor productivity growth

	OLS			2S2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
A: Baseline & industry controls	$\alpha = 1$	$\alpha = 0.7$	$\alpha = 0.8$	$\alpha = 1$	$\alpha = 0.7$	$\alpha = 0.8$
Tight 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	$\alpha = 1$	$\alpha = 0.7$	$\alpha = 0.8$	$\alpha = 1$	$\alpha = 0.7$	$\alpha = 0.8$
Tight 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	$\alpha = 1$	$\alpha = 0.7$	$\alpha = 0.8$	$\alpha = 1$	$\alpha = 0.7$	$\alpha = 0.8$
Tight 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: ϵ-transformation	$\epsilon \to \infty$	$\epsilon = 2$	$\epsilon = 3$	$\epsilon \to \infty$	$\epsilon = 2$	$\alpha = 3$
Tight contracts	0.0658**	0.1447***	0.1239***	0.6267*	1.1530**	1.0001*
	(0.026)	(0.036)	(0.031)	(0.368)	(0.565)	(0.551)
Average productivity growth rate	0.884	1.148	1.053	0.884	1.165	1.062
Observations	2,395	2,395	2,395	1,612	1,612	1,612
E: July 1990 productivity		$\alpha = 1$			$\alpha = 1$	
Tight 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 labor productivity growth on tight contracts. All specifications control for fully interacted THA agency and year fixed effects. Tight 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.

Figure A.5: Labor Productivity across Firm Size

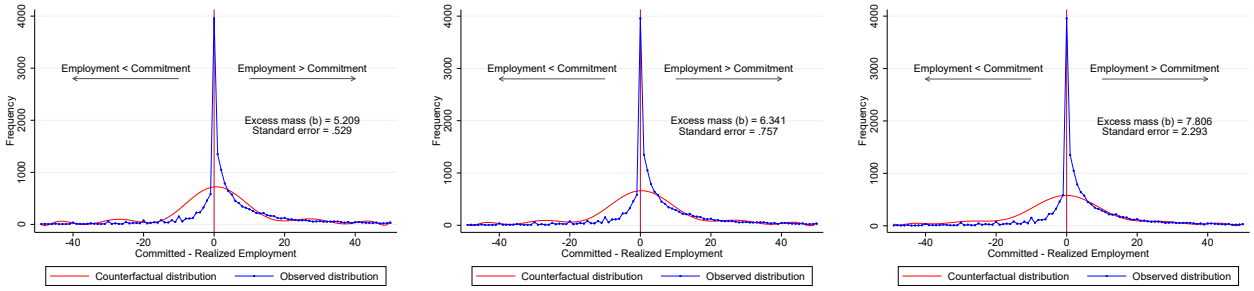
Notes: The figure plots labor productivity across the firm size distribution among 7,620 initial GDR firms with sales and employment information in 1990. The figure exclude the top and the bottom 1% of the productivity measure.



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

Figure A.6: Bunching with different polynomials

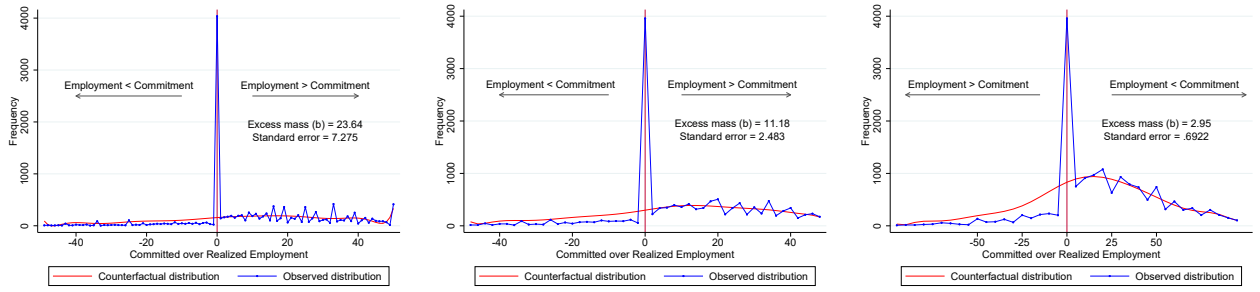
Notes: The figures show the employment distribution around the committed employment (demarcated by the vertical red line at 0) for contracts between 1990-2002. The blue line in dots is a histogram of actual employment relative to the commitment target in the final commitment year. Each point shows the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



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

Figure A.7: Bunching with symmetric R

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



A: 1 percent bins B: 2 percent bins C: 5 percent bins

Figure A.8: 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.7: 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 have 3 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 was provided to the authors on the basis of an agreement between the IWH (Halle) and the German Federal Archiv (*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 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.[23]

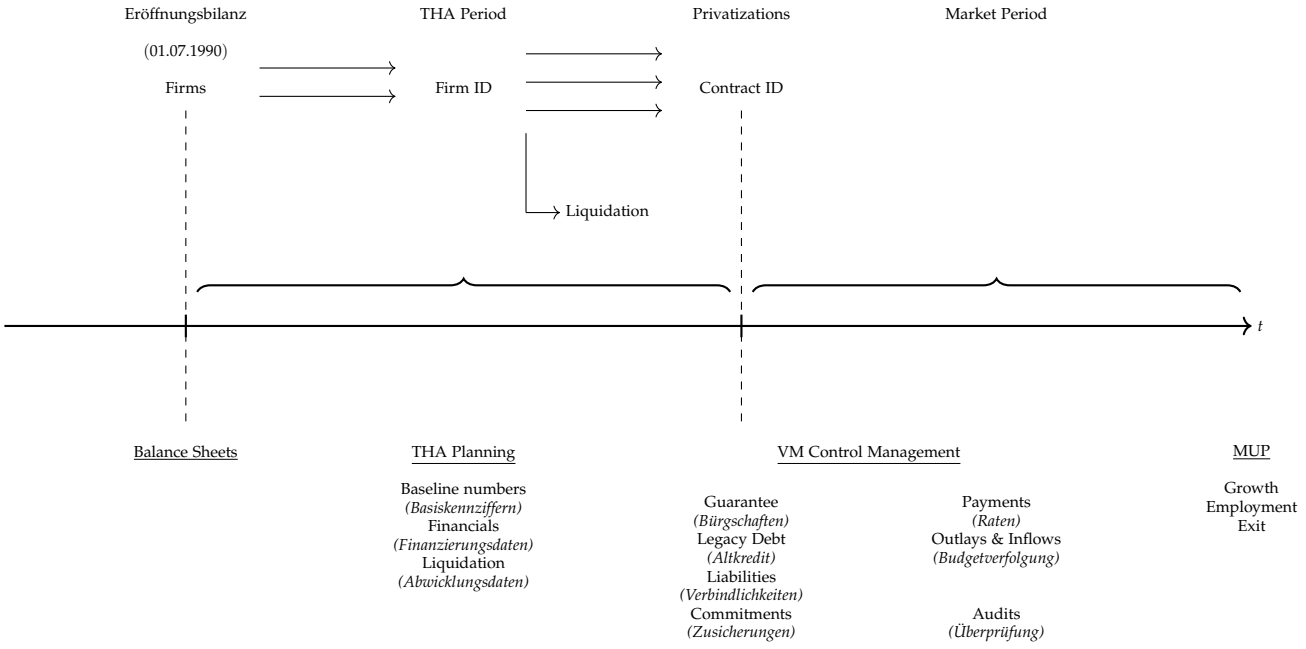


FIGURE B.1: TIMELINE FROM REUNIFICATION TO THE MARKET PERIOD

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.[24] The information relates to employment (including a break down into production workers, HR, and administration),

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

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

revenues (including a break down 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.[25] 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.5.

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. This 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. The following 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 are missing values in this name variable. Out of the 256,842 contracts in the data table, 147,060 do not have information on the name of the privatizer. The reason why most of the contracts do not possess a name of a privatizer is because the contracts are not related to firms but represent estate, machinery or land deals. Therefore, these contracts are not related to firms and consequently do not have a privatizer attached to it. Linking contracts to contain privatizer information, labor commitment contracts, and firm links generates a sample of 11,194 contracts as shown in Section 5.

After this preparation of baseline tables, we obtain information on labor commitments and labor

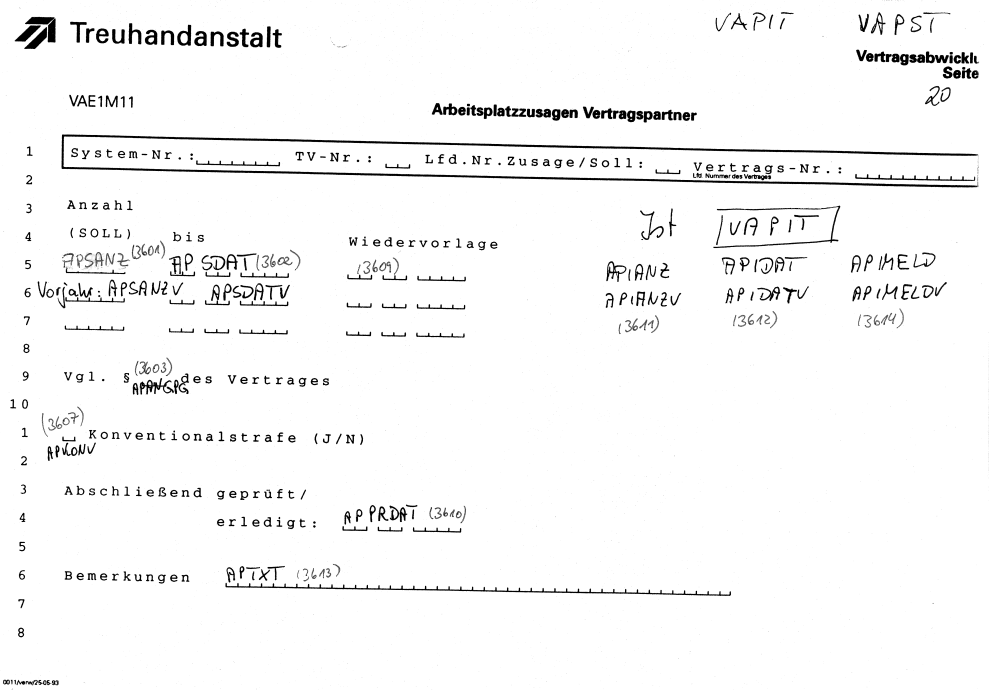
[25] THA created legal entities over time, and, as a result, 5.1% of firms are observed for the first time in 1991, and 1.2% in 1992, and 0.48% in 1993.

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.

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 on the delivered pdf documentation by the Federal Archive. 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 audit per commitment.



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

FIGURE B.2: PAPER FILE: LABOR COMMITMENT

We perform the following steps to clean the data. First, we drop observations without a 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 are 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.

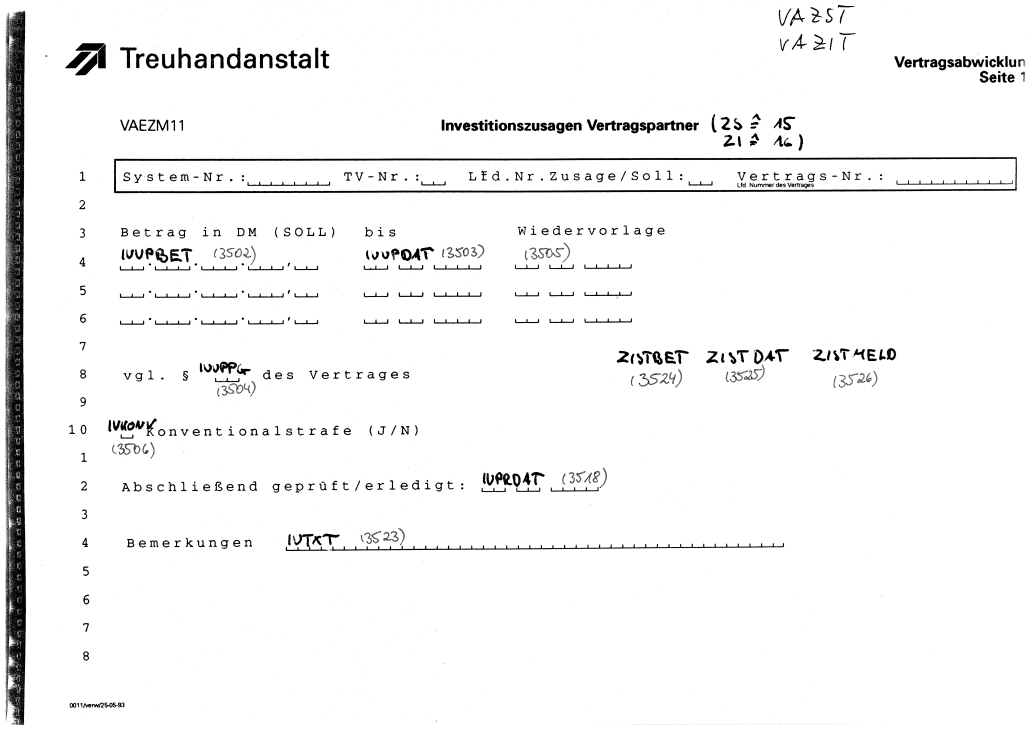
The ISUD environment further contains a table called ASVA12T with labor commitment contracts. The original table has 322,829 observations. The majority of these observations are labeled as having no labor commitments. We compare this data table with the original VAPST table. Conditional on observing one contract ID in both table (VAPST and ASVA12T) shows that the information are 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.[26] 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 for 2,702 contracts audits information. Together, these data tables generate our final sample of 18,235 contracts with labor commitments.

For the empirical specifications accounting for extensive margin privatizer preferences presented in Table A.1 as well as for the correlation analysis of preferences presented in Figure A.4, we make further use of investment commitment contracts. The logic and steps in the data cleaning process applies 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 with 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 (61.9%) have both, labor and investment commitments. 5,427 only have investment commitments, whereas 3,600 contracts only have labor commitments. In order to calculate extensive margin preferences, i.e., writing contracts

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



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

Figure B.3: Paper File: Investment Commitment

with any labor commitment condition, we merge this combined dataset to 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 covers firms in East Germany starting in the year 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 as, for instance, the date of creation and closure of a company, ownership structures, and credit rating scores. Basides 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 (see Bersch, Gottschalk, Müller, and Niefert (2014) for further details).

At the level of the contracts, we do not observe firm names that would allow a string matching based on these names. Instead, we explore the ownership information in both datasets. In the MUP data, we observe for each firm owners. In the contract-elvel 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 of with 9,538 that can be linked via name matching between the owners in the MUP and contract partners in the contract data. These observation 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 missing
- Drop if date of incorporation/ownership start is after 2000
- Drop if contract date 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 observations. 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 and 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 dataset. 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 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 em-

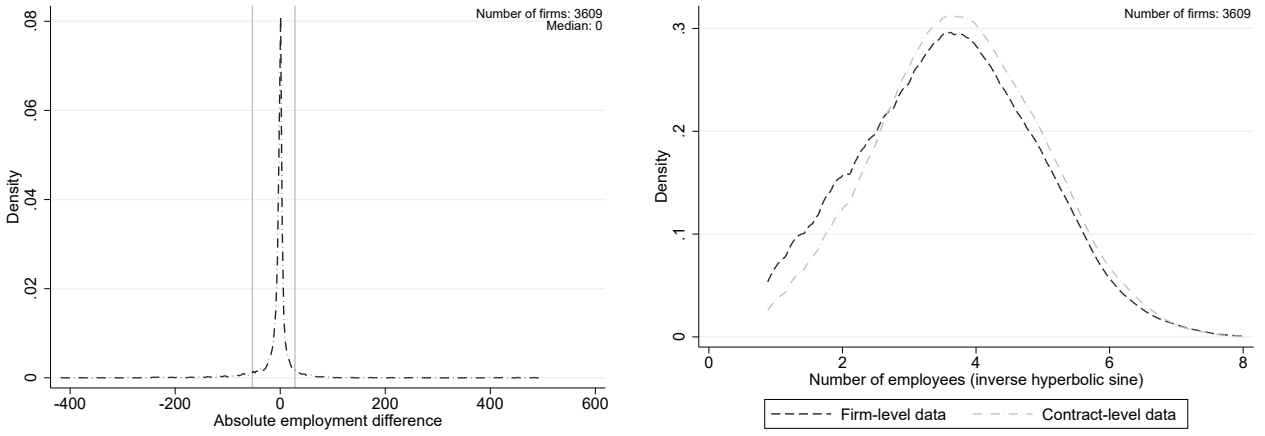
Table C.1: Sources of selected matches

	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.

ployment number differences between the two datasets. For 3,609 firm, we observe at least one audit (with positive employment information) which allows us to calculate employment differences. The median (mean) estimated difference in employment is 0 (2.67). However, we observe large tails in the distribution of employment differences. For this reason, we further drop matches with absolute employment difference 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.



A: Employment Differences

B: Employment Distributions

Figure C.1: Comparison of employment figures between Contracts and MUP

Notes: The figures show in Panel A 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.

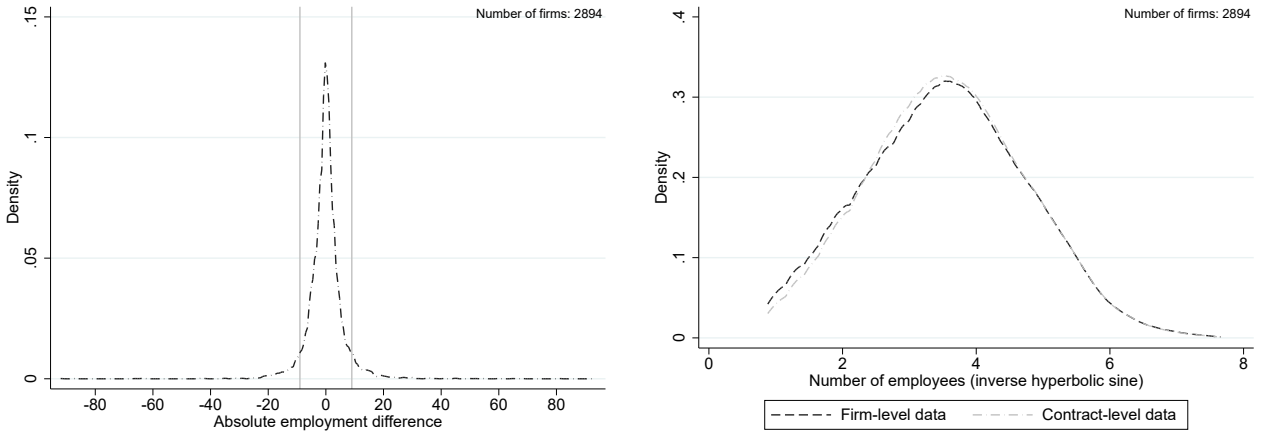
To evaluate the quality of the match, we calculate the share of firms that are "close" to each other in terms of employment figures. To arrive to such a statement, we first calculate the relative employment differences as:

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

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, 2894 out of 3609 firms are below the defined threshold value which corresponds to



A: Employment Differences

B: Employment Distributions

Figure C.2: Close matches between Contracts and MUP

Notes: The figures show in Panel A 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.

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 3. 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 be excluding the area of 6 and less above the committed level as well as 1 and 2 employment below the committed level.

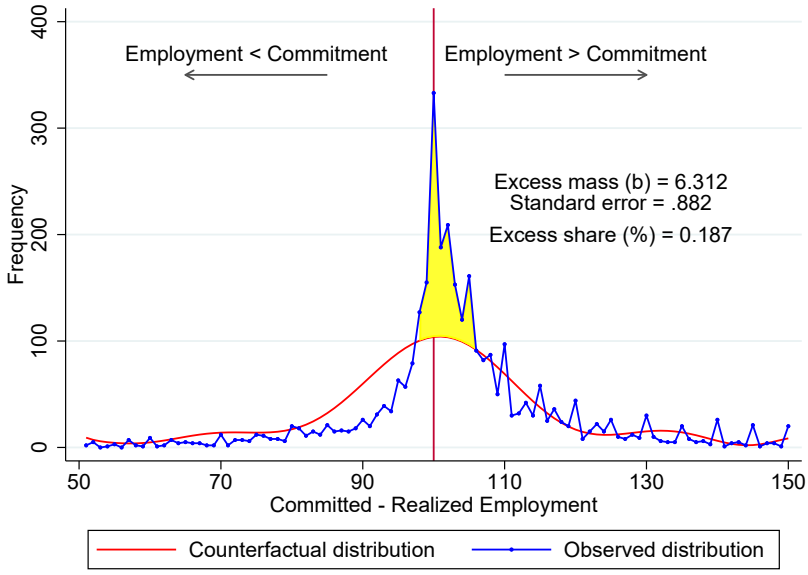


FIGURE C.3: EMPLOYMENT DISTRIBUTION AROUND THE COMMITMENT LEVEL USING FIRM-LEVEL DATA

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 have 6 employees more than committed. The shaded region in yellow is the estimated excess mass at, 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 3. Overall, these results suggest that the merge between the two datasets can be considered highly reliable.