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Committing to grow: Employment targets and firm dynamics

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Committing to Grow: Employment Targets and Firm Dynamics

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Committing to Grow: Employment Targets and Firm Dynamics*

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Abstract

We study the firm-level and aggregate effects of government-imposed employment targets. We develop a dynamic general equilibrium model with heterogeneous firms and endogenous productivity growth in which penalties for below-target hiring generate a polarization mechanism: low-productivity firms exit, while others expand employment beyond efficient levels, and firms invest in productivity to avoid future penalties. We test and confirm the model's firm level predictions using unique contractual data on more than 18,000 employment commitments from the East German privatization, exploiting quasi-random variation in the assignment of privatizers to firms. Quantitatively, employment targets reduce unemployment in the short run, but these gains reverse over time as distorted labor allocations and weakened investment incentives slow aggregate productivity growth and reduce welfare. We also evaluate how alternative designs for employment-protection (e.g., the choice between mandates and subsidies, the structure of targets) impact misallocation and the resulting short- and long-run outcomes.

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

JEL classification: D22, D24, J08, L25

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1 Introduction

Governments routinely condition public support to firms on employment guarantees. In privatizations, ownership transfers require buyers to maintain or expand workforces; in bailouts and crisis interventions, forgivable loans depend on payroll preservation; in mergers and acquisitions, regulatory approval hinges on job-creation pledges. These *employment commitments*—contractual requirements that firms maintain specified employment levels—are among the most widely used yet least studied instruments of government intervention in firm decisions. Systematic data on how such commitments are designed, monitored, and enforced are rarely available. As a result, little is known about whether employment commitments achieve their intended objectives, and at what cost to firm productivity and long-run welfare.

This paper studies how employment commitments reshape firm behavior and evaluates their aggregate consequences. Our analysis combines three elements. First, we develop and quantify a dynamic general equilibrium model with heterogeneous firms and endogenous productivity growth in which firms are penalized for hiring below mandated employment levels. Second, we test the model’s firm-level predictions using uniquely rich contractual data on more than 18,000 employment commitments covering over 900,000 workers from the East German privatization, exploiting quasi-random variation in the assignment of privatizers to firms. Third, we use the estimated model to quantify the aggregate effects of employment targets and to compare alternative policy designs. We find that employment targets generate rich firm responses and give rise to a sharp short-run versus long-run trade-off in aggregate outcomes.

In the model, employment targets distort firms’ hiring and investment decisions through multiple channels. First, a static “polarization” effect arises because, for firms below the target, the penalty acts as a fixed cost while lowering the marginal cost of labor: low-productivity firms exit, while surviving firms expand employment beyond efficient levels. Second, a dynamic channel emerges because each productivity improvement narrows the gap between a firm’s efficient scale and its mandated employment level, reducing the distortion cost and inducing constrained firms to invest more in productivity-enhancing activities. These firm-level responses feed back into equilibrium prices and wages. Crucially, the model features endogenous unemployment arising from wage anchoring motivated by the German reunification episode: due to political and institutional pressures for rapid wage equalization, East German wages were anchored above market-clearing levels, generating persistent unemployment. In this environment, employment policies can reduce unemployment by stimulating labor demand, but they also distort firm behavior, creating misallocation and dynamic inefficiencies.

To build confidence in the model that we later quantify, we take its firm-level predictions to the data. Focusing on these predictions, which hold for any wage path, allows us to isolate the core mechanisms, verify that the model captures salient features of firm behavior, and discipline the mapping between theory and measurement. We use data from the privatization of East German firms by the Treuhandanstalt (THA) in the early 1990s. Privatization contracts explicitly recorded employment targets, deadlines, and penalties, and rich administrative data allow us to track firm outcomes over

the commitment period. We develop an instrumental variable strategy, inspired by the judge-leniency literature, that exploits heterogeneous preferences of individual privatizers and their quasi-random assignment to firms within THA offices. Balancing tests confirm that the instrument is uncorrelated with a comprehensive set of pre-privatization firm, investor, and privatizer characteristics.

We find that, over the average contract duration of approximately three years, firms with binding employment targets experience 25 percentage points higher annual employment growth, 11 percentage points higher annual productivity growth, and a 1.25 percentage point higher annual exit probability. These results are robust to controls for other contract dimensions and alternative measures of firm restructuring. Consistent with the model, productivity effects are stronger for longer commitment horizons. We document several mechanisms contributing to the productivity gains: increased patenting activity among firms with binding targets and changes in management practices and workforce composition, including active recruitment of workers with prior West German experience.

Next, to quantify the aggregate effects of employment targets, we structurally estimate the model by jointly matching the firm-level responses from the empirical analysis and the twenty-year evolution of East German unemployment and East–West relative wages. Our results suggest that, in the short run, employment targets raise labor demand, reducing unemployment and generating welfare gains. However, employment targets distort the allocation of labor across firms, and the resulting wage pressure compresses profitability for unconstrained firms, reducing their incentives for productivity-enhancing investment. Over time, these distortions accumulate, slowing aggregate productivity growth and reversing the initial welfare gains.

We also compare the employment commitment policy as implemented in East Germany to four alternative policy designs: a more aggressive target scenario that varies policy intensity, a firing moratorium, an employment subsidy commonly adopted in similar restructuring episodes, and a productivity investment subsidy that supports employment through productivity growth rather than requiring firms to hire directly. Two findings stand out. First, both the intensity and the distribution of the mandate matter: milder constraints distributed broadly across firms limit exit and misallocation, while setting targets too aggressively generates more exit and severe misallocation, whose costs quickly overwhelm the short-run employment gains. Second, the choice between forced employment mandates and more organic approaches to employment growth involves a fundamental tradeoff: mandating employment directly can deliver short-run welfare gains but creates misallocation and slows productivity growth over time, while subsidizing productivity growth imposes upfront costs but generates compounding benefits and stronger employment gains in the long run.

Broader policy settings. To situate the East German experience in broader perspective, we document more than 50 distinct institutional settings across six policy domains in which employment commitments have been deployed as a policy instrument.¹ Appendix Table A.1 reports illustrative cases drawn from the full inventory documented in the *Online Policy Repository*, covering advanced

¹We define employment commitments as labor-related conditions attached to public–private transactions involving asset transfers, regulatory approvals, or subsidies, designed to make firms internalize employment objectives. The inventory excludes commitments arising from general labor regulations or attached to state-owned enterprises that were never transferred to private owners.

and emerging economies over several decades. These range from privatizations and crisis interventions to ownership-change regulations, place-based development programs, and public procurement. Employment commitments are thus not confined to any single policy domain but arise wherever governments seek to make firms internalize employment objectives as part of a public-private transaction. Our approach to both the modeling and the empirical analysis is designed to make the findings informative for these settings as well: the model builds on a standard framework of firm dynamics, and the firm-level predictions do not depend on how wages are determined or why targets were imposed—they follow from the firm’s own optimization problem. Our empirical strategy similarly isolates firm-level responses to employment targets. In this sense, the East German case provides a unique laboratory for studying these policies, owing to the scale of the privatization program, the richness of its administrative records, and the quasi-random assignment of privatizers to firms. The theoretical predictions and empirical estimates can therefore shed light on how firms would respond to employment commitments in the diverse settings we document. The aggregate implications, by contrast, depend on the specific frictions present in each context, and would require adapting the general equilibrium framework accordingly.

Related Literature. This paper contributes to four strands of literature. First, it adds to the growing literature on industrial policy and government intervention in firm decisions, comprehensively surveyed by [Juhász et al. \(2023\)](#).² Several recent contributions exploit historical settings to identify the dynamic effects of industrial policy. [Lane \(2022\)](#) and [Choi & Levchenko \(2021\)](#) study South Korea’s heavy and chemical industry drive, while [Giorcelli & Li \(2021\)](#) estimates the long-run effects of technology and know-how transfers during China’s structural transformation. Methodologically, our paper also relates to quantitative frameworks that evaluate industrial policy in general equilibrium ([Kalouptsidei 2018](#), [Barwick et al. 2021](#), [Acemoglu et al. 2018](#), [Liu 2019](#), [Bustos et al. 2016, 2020](#), [Crisciuolo et al. 2019](#)). We study a policy instrument that is common but much less understood: employment commitments, or contractual requirements that firms maintain or expand employment as a condition for public-private transactions. Unlike subsidies, tariffs, or procurement policies, these commitments operate through enforceable firm-level constraints on labor choices. Using contract-level administrative data from the East German privatization, we observe the targets, deadlines, monitoring, and penalties that are typically unavailable to researchers, and combine this evidence with a quantitative general equilibrium model to evaluate both firm-level responses and aggregate welfare effects.

Second, the paper relates to the literature on labor-market regulation, size-dependent policies, and misallocation. A large body of work shows that distortions to firms’ labor choices can lower aggregate productivity by shifting resources away from their most productive uses ([Restuccia & Rogerson 2008](#),

²The objectives and tools of industrial policy are wide-ranging. Policies aimed at structural change include financial liberalization and privatization programs that facilitate foreign capital inflows ([Papaioannou 2009](#), [Kalemli-Ozcan](#), [Papaioannou & Perri 2013](#), [Kalemli-Ozcan](#), [Papaioannou & Peydro 2013](#)). Productivity-enhancing policies use taxes, subsidies, and regulations to redirect technological change or support particular sectors ([Aghion et al. 2016](#), [Acemoglu et al. 2016](#)). Persistent regional disparities have also motivated place-based development policies ([Greenstone et al. 2010](#), [Kline & Moretti 2014](#)).

Hsieh & Klenow 2009, Syverson 2011, Bartelsman et al. 2013). Garicano et al. (2016) study the equilibrium and welfare effects of size-contingent labor laws in France, Braguinsky et al. (2011) attribute changes in Portugal’s firm-size distribution to strong worker protections, and Martin et al. (2017) show that removing small-scale industry promotion in India raises entry and output growth. More recent work embeds size-dependent distortions in endogenous growth models to study how regulations create barriers to firm expansion (Aghion et al. 2023, Akcigit et al. 2023). Employment commitments differ from these policies in two important ways: they are firm-specific and time-limited, and they push firms to grow rather than directly constraining expansion. Our analysis therefore extends the misallocation literature by studying a “push-to-grow” distortion whose aggregate implications depend critically on the labor-market environment.

Third, the paper contributes to research on firm dynamics, endogenous productivity growth, and the idea that constraints can stimulate upgrading. In canonical models of firm dynamics and endogenous growth, firm-level investments in innovation and productivity improvement are central to aggregate productivity dynamics (Klette & Kortum 2004, Akcigit & Kerr 2018, Atkeson & Burstein 2019). Our model introduces a new incentive for such investment: necessity-driven productivity improvement. Firms that are forced to operate above their efficient employment level have stronger incentives to raise productivity because productivity gains reduce the cost of carrying excess labor. This mechanism connects to Aghion et al. (2005), where firms invest to escape competitive pressure, and to Mukoyama & Osotimehin (2019), where firms above their optimal size invest more to avoid downsizing costs under firing taxes. It also relates to evidence on directed technical change, in which constraints on inputs or changes in relative prices redirect innovation: Hanlon (2015) shows that input-supply shocks induced new technologies, and Aghion et al. (2016) show that higher fuel taxes redirect innovation toward clean technologies.

Fourth, the paper contributes to the literature on the long-run economic consequences of historical episodes and institutional transformations (Michalopoulos & Papaioannou 2017), and in particular to research on privatization, transition economies, and German reunification. A large privatization literature studies how ownership transfers affect firm performance and restructuring (Megginson & Netter 2001, Djankov & Murrell 2002, Brown et al. 2006). The East German transition has been studied through the lenses of labor reallocation (Dauth et al. 2021, Fuchs-Schündeln & Schündeln 2005), migration (Uhlig 2008, Hunt 2006, Peters 2022, Redding & Sturm 2008), capital investment (Sinn 2002), collective bargaining and wage convergence (Burda & Hunt 2001, Burda 2010), and persistent differences in social and cultural attitudes (Alesina & Fuchs-Schündeln 2007, Burchardi & Hassan 2013, Laudenbach et al. forthcoming).³ Mergele et al. (2025) study the Treuhand privatization process and show that firms with higher baseline productivity were more likely to command higher sales prices. Our analysis emphasizes the comprehensive nature of THA ownership transfers by linking them to contractual obligations governing post-privatization outcomes. We provide, to our knowledge, the first quantitative evaluation of the employment commitment policy that accompanied the

³Persistent East–West economic differences are studied by Snower & Merkl (2006) and Burda (2006). More recently, Heise & Porzio (2021) provide evidence for low labor mobility between East and West Germany, while Bachmann et al. (2022) relate monopsony power to limited productivity convergence.

East German privatization, showing how it reshaped firms' labor, productivity, and exit decisions and assessing its welfare consequences over the full transition.

Overall, our findings speak to a broader and timely policy debate. With renewed attention to job creation and industrial policy, our approach—combining rich micro-level evidence with a quantitative general equilibrium framework—delivers both new empirical insights from the East German experience and a tractable structure to evaluate policy design. Beyond the specific policies studied here, the framework yields important predictions on the static and dynamic consequences of a range of commonly used interventions. We view it as a flexible foundation that can be extended to analyze other dimensions of industrial policy, and serve as a useful input for future work.

The rest of the paper is structured as follows. Section 2 documents the institutional environment using THA archival records, internal manuals, and a systematic analysis of parliamentary debates. Section 3 develops the model: the firm problem characterizes how employment targets generate a polarization effect on labor choices and a necessity-driven incentive for productivity improvement; the general equilibrium framework introduces wage anchoring and involuntary unemployment. Section 4 describes the data and links privatization contracts to firm outcomes. Section 5 presents the identification strategy based on privatizer preferences and the main empirical results. Section 6 calibrates and estimates the model. Section 7 evaluates the aggregate and welfare consequences of employment targets and compares the observed policy to alternative designs. Section 8 concludes.

2 Institutional Background

This section documents the institutional environment in which employment commitments in East Germany were conceived, negotiated, and enforced. Understanding this environment serves three purposes. First, it describes the economic crisis that followed reunification and the broad set of policy responses it triggered, situating employment commitments within the larger landscape of government intervention in East Germany. Second, it grounds our identification strategy, which exploits variation in privatizer-assigned employment targets to estimate the causal effects of binding labor commitments on firm behavior. Third, it provides the structural information—including penalty rates, contractual horizons, and labor market frictions—needed to calibrate the model and assess the aggregate employment and productivity implications for the East German economy. We begin by describing the macroeconomic context of reunification, the broader policy response, and the THA's privatization program, then turn to the design and enforcement of employment commitments. Our account draws on interviews with former THA officials, internal manuals, archival records, and a systematic analysis of parliamentary debates.

2.1 Reunification and the East German Economic Transition

2.1.1 The East German Economic Crisis and the Policy Response

Following the fall of the Berlin Wall in November 1989, German reunification proceeded rapidly through economic, monetary, and political integration. The transition confronted the East German

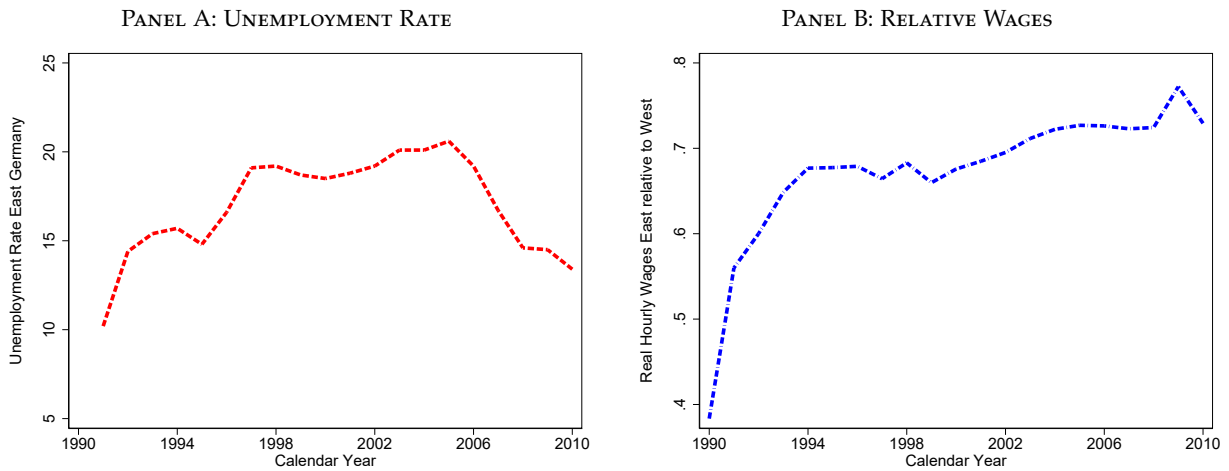
economy with several overlapping shocks whose combined impact was without peacetime precedent (Akerlof et al. 1991; Sinn 2002). Industrial production fell by two-thirds within eighteen months of reunification, affecting virtually every sector. Demand contracted sharply as East German consumers shifted spending toward Western products and traditional export markets within the Council for Mutual Economic Assistance (CMEA) disintegrated. The currency conversion of July 1, 1990—which exchanged East German Marks for Deutsche Marks at par—immediately exposed firms to international competition at cost levels far above world market prices. Using input-output data, Akerlof et al. (1991) estimated that firms employing only about 8 percent of the industrial workforce were viable in the sense of covering short-run variable costs at prevailing wages and world market prices. Large-scale westward migration compounded the adjustment, with more than one million East Germans—roughly 6 percent of the population—leaving between 1989 and 1991 (Burda & Hunt 2001).

Among these shocks, much of the literature identifies the failure of wages to adjust as the most consequential factor behind the persistence of East German unemployment (Akerlof et al. 1991; Sinn 2002). The currency conversion translated existing wage contracts into Deutsche Marks at par, immediately pushing nominal wages well above market-clearing levels. Rather than falling back to correct this overvaluation, nominal wages continued to rise as pressure from West German unions seeking to prevent low-wage competition, combined with a broad political commitment to equalize living conditions and obtain wage convergence (*Lohnangleichung*) between East and West, drove wages upward despite the large productivity gap (Akerlof et al. 1991; Burda & Hunt 2001; Hunt 2001; Sinn 2002; Snower & Merkl 2006). The collective bargaining institutions that took shape during reunification entrenched this misalignment: wage negotiations were conducted between West German employers' associations and newly founded East German unions advised by their western counterparts, and the negotiating parties agreed to equalize wages within as few as five years (Sinn 2002).⁴

The economic crisis prompted a large-scale policy response that extended well beyond privatization. Massive fiscal transfers from West to East Germany—totaling more than DM 1.5 trillion over the first decade—financed social insurance payments, infrastructure modernization, and investment subsidies (Sinn 2002; Burda & Hunt 2001). On the labor market side, the employment collapse was cushioned by short-time work (*Kurzarbeit*), publicly funded job creation schemes (*Arbeitsbeschaffungsmaßnahmen*), early retirement programs (*Vorruhestandsregelungen*), and retraining measures (Akerlof et al. 1991; Burda & Hunt 2001). Despite this extensive policy response, unemployment rose sharply and remained persistently elevated for over a decade. Figure 1 documents the resulting labor market dynamics: the unemployment rate climbed from already around 10% in 1991 (officially zero unemployment before reunification) to nearly 20% by 1996, while East German wages as a share of West German levels rose from roughly 45% to over 70% over the same period before converging more gradually thereafter.

⁴As Sinn (2002) documents, the dominant motive in these negotiations was to protect West German jobs from low-wage competition, and the resulting wage schedule implicitly respected the West German social assistance level as a floor. By 1996, 85 percent of East German firms had left the employers' associations to escape the collectively bargained wage agreements.

FIGURE 1: UNEMPLOYMENT AND WAGES



Notes: The figure shows the unemployment rate (left panel) in East Germany and relative wages (right panel) in East Germany relative to West Germany. Unemployment numbers are based on official statistics provided by the German Statistical Office. Wages are based on the German socio-economic panel (SOEP 36long) and refers to hourly wages of full-time employees. Wage data for 1990 are based on retrospective questions. Wages are deflated based on separated deflators for East and West Germany until 2000. Prices changes and 1990 in East Germany are assumed to be proportional to West Germany.

2.1.2 The Privatization Process

It was in this environment of rising unemployment and above-market wages that the transition from a centrally planned to a market economy took shape through the privatization of the state-owned sector. The Treuhandanstalt (THA), established in March 1990 under the last East German government and granted expanded authority through the *Treuhandgesetz* in July 1990, was charged with restructuring and privatizing former state-owned enterprises.⁵ The agency became the largest holding company in the world, overseeing approximately 12,000 enterprises employing around 4.5 million workers, roughly 50% of the East German workforce. It inherited a diverse and often disjointed portfolio of activities structured within large centrally planned conglomerates (*Kombinate*).

The initial step toward restructuring involved splitting these conglomerates into firms organized under private law (*Entflechtung*). Each enterprise was then required to submit an opening balance sheet in Deutsche Mark (*Eröffnungsbilanz*) and a business plan for review. The privatization process further streamlined operations as firms divested non-core activities through asset sales.⁶ In this sense, the THA did not act as a passive owner but played an active role in restructuring East German firms and, where feasible, restoring their viability, addressing a broad set of constraints ranging from legacy

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

⁶The THA itself built up rapidly from an initial staff of about 200 mostly East German employees to an institution of around 4,000 employees plus 800 full-time consultants. These people were divided approximately equally between the central office in Berlin and the 15 branch offices distributed among the major cities of the new federal states. Appendix Figure D.1 represents the former German Democratic Republic (GDR) districts and the location of THA branch offices. Smaller firms with fewer than 1,500 employees were assigned to local branch offices, while larger firms were assigned to industry-based divisions in the Berlin headquarters. In particular, the THA headquarters organized firms with more than 1,500 employees or with revenue or balance sheet values above 1.5 million Deutsche Mark (DM).

debt and unresolved property rights to firms' investment and employment needs.

The standard procedure for privatization was direct sales to interested acquirers; structured auctions were the exception (Fischer et al. 1993b). Sales contracts specified the purchase price alongside guarantees by the acquirer regarding minimum levels of employment and investment. In this respect, the Treuhandanstalt's approach mirrors privatization practices observed across other transition economies. While programs differed in pace, sequencing, and institutional design, most—including those in Poland, Hungary, and the Czech Republic—relied predominantly on direct sales to strategic investors, management-employee buyouts, and negotiated asset transfers (Megginson & Netter 2001). As in the German case, authorities typically retained discretion over contractual terms beyond price, using investment and employment provisions to pursue political and social objectives, although the THA's extensive involvement in pre-sale restructuring was more distinctive. Poland, by contrast, pursued a more gradual and decentralized privatization strategy that spread restructuring over time, thereby attenuating immediate social dislocation and enabling a more incremental adjustment and negotiation of employment outcomes (Aghion & Blanchard 1994; Roland 2000). Russia's mass voucher privatization stands out as a contrasting model that emphasized broad ownership dispersion; even there, however, voucher schemes were complemented by negotiated sales of larger enterprises.

2.2 Employment Commitments

2.2.1 Parliamentary Debates and Social Pressure

The rapid increase in unemployment following reunification generated substantial social unrest, placing the privatization process under intense political scrutiny and increasing pressure on the THA to internalize employment dynamics. As Dodds & Wächter (1993) noted, "the urgency of the Treuhand's need to maintain and create jobs cannot be overestimated." High-profile events, such as the 81-day hunger strike by workers of the Kaliwerk Bischofferode after the Treuhandanstalt (THA) announced its closure, illustrate the salience of employment losses in the public debate (Bernhard 2011). Political pressure further intensified after the assassination of THA president Detlev K. Rohwedder in April 1991, reinforcing the perception that privatization decisions carried significant social costs.

The Treuhandanstalt quickly became a central subject of political debate with references to the agency appearing in more than 80% of parliamentary debates in a given quarter. To understand what policy objectives and frictions were associated by parliamentarians to the use of employment commitments by Treuhand in East Germany, we analyze the full corpus of daily Bundestag session transcripts from 1990 to 1995. The results, reported in Table B.1 of the Appendix, point to a clear mandate. Employment commitments were discussed in parliament almost exclusively in conjunction with the Treuhandanstalt and East German unemployment—not alongside broader industrial policy objectives such as productivity, technology, or agglomeration spillovers in the former GDR. This pattern confirms that labor commitments were conceived as a targeted political response to the unemployment crisis in East Germany.

2.2.2 Contractual Features of Employment Commitments

As a consequence of this social and political pressure, the goal of privatization was not necessarily to maximize the sale price but to secure employment at the privatized firm. Employment commitments were written directly into privatization contracts as obligations to maintain a specified number of full-time equivalent jobs. While such commitments could result in discounts on the sales price, the valuation process was negotiated on a case-by-case basis and did not follow a fixed formula within the THA. Within this process, the discretion assigned to individual privatizers was considerable. Internal THA handbooks stated explicitly that the weighting of bid components was not subject to a rigid formula but was left “to the acumen of the privatizer.”⁷ This delegation reflected the speed of the process and the organizational constraints of a nascent bureaucracy.⁸

The commitment applied to the specific establishment being privatized and could not be fulfilled by employing workers in other establishments of the acquiring firm (Siebert 1991; Fischer et al. 1993a). Moreover, these provisions were embedded in the corporate contract and therefore remained binding even if the firm was subsequently sold.

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

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

3 The Model

To understand how employment commitments reshape firm behavior and to evaluate their aggregate consequences, we develop a dynamic model of heterogeneous firms with endogenous productivity growth, where a subset of firms operates under employment targets that penalize hiring below a mandated level. We first characterize how employment targets affect firm decisions, then embed the

⁷More specifically the “Generally binding principles that prescribe how each individual case should be assessed are not intended to be specified. In this core area, your (the privatizer) knowledge and negotiating skills are primarily required.”

⁸As Dodds & Wächter (1993) described it, the THA was “an adolescent bureaucracy, born of chaos and destined to be phased out without ever functioning normally.” 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).

firm problem into a general equilibrium framework, which enables us to quantify aggregate tradeoffs.

At the firm level, we keep the environment deliberately standard. A firm takes wages and aggregate conditions as given and makes the same types of decisions as in canonical models of firm dynamics. The channels through which employment targets affect these decisions operate entirely through the firm’s own optimization problem, not through features of the aggregate environment. The firm-level predictions therefore do not depend on *why* targets were imposed or on the specific economy in which they operate, and one would expect the same effects to arise wherever firms face employment targets, whether in privatizations, bailouts, merger approvals, or crisis interventions.⁹ The aggregate implications, by contrast, depend on the specific labor market frictions that motivate the use of targets; Section 3.2 therefore develops a general equilibrium framework tailored to the East German transition.

The model economy operates in continuous time. A continuum of firms, heterogeneous in productivity, produce a homogeneous good under perfect competition, taking the wage as given. At each instant, a firm chooses how many workers to hire, how much to invest in improving its productivity, and whether to exit; exiting firms are replaced by new entrants. Because productivity growth is endogenous, the model allows us to study the dynamic consequences of employment targets beyond their static effects on labor choices.

3.1 The Firm Problem

3.1.1 Static Environment

Firms are endowed with a constant-returns-to-scale Cobb–Douglas production function of productivity and labor:¹⁰

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

where $z_{t,j}$ denotes the idiosyncratic productivity level of firm j at time t and $l_{t,j}$ is the amount of labor hired. Firms take the wage rate w_t as given and operate under perfect competition; the price of the homogeneous good is normalized to one.¹¹ We drop the time and firm subscripts t, j whenever this does not cause confusion.

A subset of firms operates under employment targets l_* , which are heterogeneous across firms and exogenously set by the policymaker at the time of privatization. Consistent with the institutional evidence in Section 2, firms that operate below their target level of employment pay a penalty

⁹Consistent with this approach, the empirical strategy in Section 5 identifies the causal effects of employment targets on firm-level outcomes while controlling for aspects of the economic environment that are specific to East Germany. The firm-level estimates therefore isolate the same objects that the firm problem characterizes.

¹⁰For expositional clarity, the baseline model uses labor as the sole factor of production. Appendix C.5 extends the production function to include capital and shows that the main predictions carry through.

¹¹Assuming monopolistic competition with a standard CES demand system would not change the main predictions of the model, as markups would be constant and the firm’s optimization problem would be rescaled but not qualitatively altered. Under demand systems with variable markups, such as the oligopolistic competition framework with nested CES demand in Atkeson & Burstein (2008), markups depend on a firm’s cost position relative to its closest competitor. In such a setting, employment targets could interact with product market structure: by altering firm scale and productivity, targets would change firms’ competitive positions and therefore their markups, creating an additional channel of misallocation. We view this interaction as an interesting direction for future work.

proportional to the shortfall in employment and to prevailing wages:

$$\gamma(l_* - l)^+ w, \quad (2)$$

where $(x)^+ = \max\{x, 0\}$ and γ controls the penalty per missing employee as a fraction of the wage.

At a given wage w and for given (z, l_*) , a firm chooses employment to maximize static profits:

$$\Pi(z, l_*) = \max_{l \geq 0} \left\{ z^{1-\alpha} l^\alpha - wl - \gamma(l_* - l)^+ w \right\}. \quad (3)$$

It is convenient to normalize productivity by the wage, defining

$$\tilde{z} \equiv \frac{z}{w^{1/(1-\alpha)}}, \quad (4)$$

so that profits take the form $\Pi(z, l_*) = \pi(\tilde{z}, l_*) w$, where π depends on z and w only through the ratio \tilde{z} . The following lemma summarizes the solution.

Lemma 1. *The normalized profits and the optimal labor choice for a firm with normalized productivity \tilde{z} and employment target l_* are given by*

$$\pi(\tilde{z}, l_*) = \begin{cases} \alpha^{\frac{\alpha}{1-\alpha}} (1-\alpha) \tilde{z} > 0 & \text{if } \tilde{z} \in [\tilde{z}_1, \infty) \quad (\text{Undistorted}) \\ \tilde{z}^{1-\alpha} l_*^\alpha - l_* > 0 & \text{if } \tilde{z} \in [\tilde{z}_2, \tilde{z}_1) \quad (\text{Distorted, Bunching}) \\ \left(\frac{\alpha}{1-\gamma}\right)^{\frac{\alpha}{1-\alpha}} (1-\alpha) \tilde{z} - \gamma l_* > 0 & \text{if } \tilde{z} \in [\tilde{z}_3, \tilde{z}_2) \quad (\text{Distorted, No Bunching}) \\ \left(\frac{\alpha}{1-\gamma}\right)^{\frac{\alpha}{1-\alpha}} (1-\alpha) \tilde{z} - \gamma l_* \leq 0 & \text{if } \tilde{z} \in [\tilde{z}_4, \tilde{z}_3) \quad (\text{Distorted, No Bunching}) \\ \text{exit} & \text{if } \tilde{z} \in (0, \tilde{z}_4) \quad (\text{No Production}) \end{cases} \quad (5)$$

and

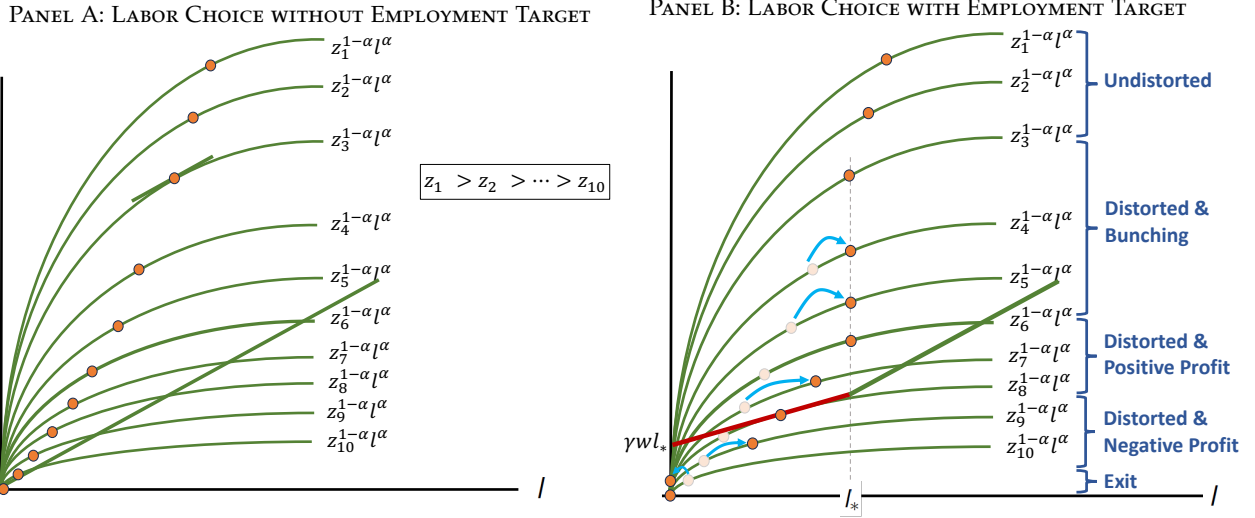
$$l(\tilde{z}, l_*) = \begin{cases} \alpha^{\frac{1}{1-\alpha}} \tilde{z} & \text{if } \tilde{z} \in [\tilde{z}_1, \infty) \quad (\text{Undistorted}) \\ l_* & \text{if } \tilde{z} \in [\tilde{z}_2, \tilde{z}_1) \quad (\text{Distorted, Bunching}) \\ \left(\frac{\alpha}{1-\gamma}\right)^{\frac{1}{1-\alpha}} \tilde{z} & \text{if } \tilde{z} \in [\tilde{z}_4, \tilde{z}_2) \quad (\text{Distorted, No Bunching}) \\ 0 & \text{if } \tilde{z} \in (0, \tilde{z}_4) \quad (\text{No Production}) \end{cases} \quad (6)$$

where the threshold levels $\tilde{z}_1 > \tilde{z}_2 > \tilde{z}_3 > \tilde{z}_4$ are ordered by productivity, with $(\tilde{z}_1, \tilde{z}_2, \tilde{z}_3)$ depending on the target l_* and the penalty parameter γ , while the exit threshold \tilde{z}_4 additionally depends on the continuation value from the dynamic problem described in Section 3.1.2. When $l_* = 0$ (no employment target), only the undistorted region applies: $\pi(\tilde{z}) = \alpha^{\alpha/(1-\alpha)} (1-\alpha) \tilde{z}$ and $l(\tilde{z}) = \alpha^{1/(1-\alpha)} \tilde{z}$ for all $\tilde{z} > 0$.

Lemma 1 implies that optimal employment and profits are non-decreasing in normalized productivity \tilde{z} and can be characterized by five productivity regions, as illustrated in Figure 2. Panel A shows the undistorted case: the orange circles mark the point where the marginal product of labor equals the wage, and more productive firms hire more workers. Panel B shows the distorted case under employment targets. The mechanism through which targets distort labor decisions becomes transparent by rewriting the profit function as

$$\Pi(z, l_*) = \max_l \begin{cases} z^{1-\alpha} l^\alpha - \underbrace{wl}_{\text{variable cost}} & \text{for } l \geq l_* \\ z^{1-\alpha} l^\alpha - \underbrace{(1-\gamma)wl}_{\text{variable cost}} + \underbrace{\gamma wl_*}_{\text{fixed cost}} & \text{for } l_* \geq l \geq 0 \end{cases} \quad (7)$$

FIGURE 2: THE IMPACT OF EMPLOYMENT TARGETS ON LABOR CHOICES



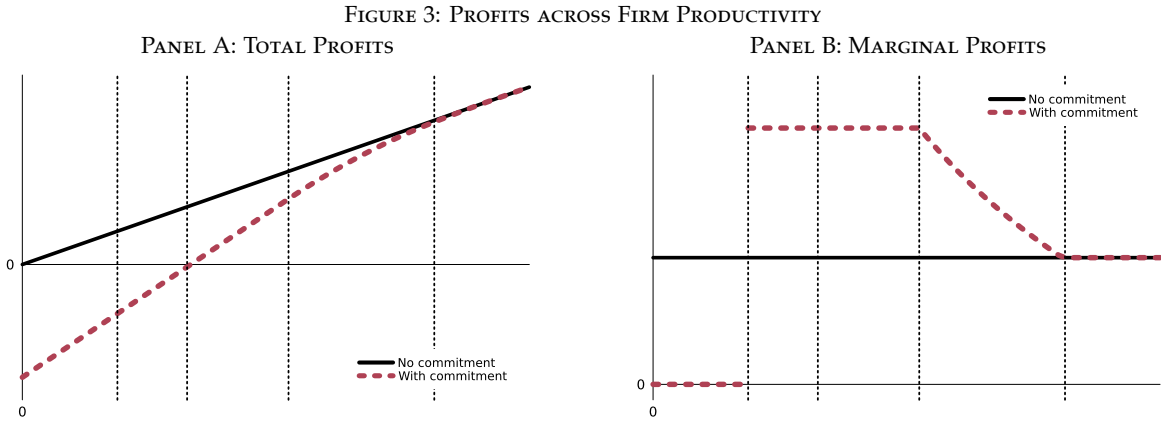
Comparing the two lines of (7) reveals two effects. First, employment targets add a fixed cost $\gamma w l_*$ to the cost function: the firm must pay this amount regardless of the number of workers it hires. Second, for firms operating below the target, the marginal cost of labor falls from w to $(1 - \gamma)w$. The government has already charged a γ fraction of the wage bill as part of the fixed cost, so the residual variable cost is lower. Employment targets therefore generate heterogeneity in the effective marginal cost of labor across firms, creating misallocation: constrained firms face a lower marginal cost than unconstrained ones, so labor is drawn toward less productive firms at the expense of more productive ones.

This change in the cost structure is represented by the red cost line in Figure 2B. The linear cost function becomes V-shaped with a kink at l_* , sorting firms into five groups. The highest-productivity firms ($\tilde{z} \geq \tilde{z}_1$) remain undistorted because the target is not binding. Firms with $\tilde{z} \in [\tilde{z}_2, \tilde{z}_1)$ increase labor and bunch at the target l_* , since lower variable costs make it profitable to hire up to the target but variable costs revert beyond it. Firms with $\tilde{z} \in [\tilde{z}_3, \tilde{z}_2)$ increase labor due to the lower variable cost, but their new optimum falls short of l_* . Firms with $\tilde{z} \in [\tilde{z}_4, \tilde{z}_3)$ cannot achieve positive profits even at the distorted optimum. Note that this region and the one above it ($\tilde{z} \in [\tilde{z}_3, \tilde{z}_2)$) share the same profit expression and the same labor choice; they differ only in the sign of flow profits. In a purely static framework, a firm with negative profits would exit. These firms remain active because the dynamic problem in Section 3.1.2 endows them with a continuation value: a successful improvement in productivity can move the firm into a region with positive profits, and as long as this continuation value exceeds the value of exiting, the firm finds it optimal to absorb current losses. The exit threshold \tilde{z}_4 is therefore determined jointly by the static profit structure and the dynamic continuation value, not by the static environment alone. The lowest-productivity firms ($\tilde{z} < \tilde{z}_4$) find it optimal to hire zero workers and exit.

Taken together, employment targets create a “polarization” effect. The highest-productivity firms remain undistorted. Among the rest, surviving firms increase their employment, with some operating

at negative profits, while the lowest-productivity firms cannot cover the implied fixed cost and exit. Employment targets thus push firms either up or out.

The static implications of employment targets also shape the returns to productivity improvement, which will drive the dynamic analysis. Figure 3 (left panel) plots total profits against firm productivity. The dashed red line shows that distorted firms have lower total profits due to the fixed cost $\gamma w l_{*t}$, which grows with the target level. The right panel plots *marginal* profits and shows that distorted firms have a higher marginal profit with respect to productivity. The lower variable cost of labor means that a marginal increase in productivity is more valuable for a constrained firm: it reduces the gap between the firm's optimal scale and its mandated employment, lowering the cost of the distortion. In a dynamic setting, this difference in marginal returns will induce constrained firms to invest more in productivity improvement.



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

3.1.2 Dynamic Problem of the Firm

A firm can improve its productivity by choosing a Poisson arrival rate $x \geq 0$ of successful productivity improvements. A successful improvement raises normalized productivity from \tilde{z} to $(1 + \lambda_t)\tilde{z}$, where $\lambda_t > 0$ is the step size, which may vary over time to capture catch-up dynamics during the transition. Choosing intensity x entails a flow cost

$$\hat{c}(x, \tilde{z}, t) = \frac{\phi}{2} x^2 \frac{\tilde{z}}{\bar{\tilde{z}}(t)}, \quad (8)$$

where $\phi > 0$ scales the cost of investment and $\bar{\tilde{z}}(t) \equiv \mathbb{E}[\tilde{z}]$ is the mean normalized productivity across all firms, determined in general equilibrium (Section 3.2). The cost is convex in x and proportional to the firm's normalized productivity \tilde{z} , as is standard in models of firm dynamics with endogenous growth (Klette & Kortum 2004, Akcigit & Kerr 2018). The proportionality to \tilde{z} implies that firm growth is consistent with Gibrat's law in the absence of employment targets: the growth rate of

sufficiently productive firms is independent of their size, as in Klette & Kortum (2004). The division by $\bar{z}(t)$ captures intertemporal knowledge spillovers (Atkeson & Burstein 2019): as the aggregate level of technology improves, the accumulated knowledge base reduces the cost of further advances for each firm.

Employment targets expire exogenously at Poisson rate $\mu > 0$: a firm with state (\tilde{z}, l_*) transitions to $(\tilde{z}, 0)$ when the target expires. Each firm also faces an exogenous arrival rate $\delta > 0$ of destruction. Destroyed firms exit the economy and are replaced by new entrants; the entry process is described in Section 3.2. A firm with a binding employment target ($l_* > 0$) can also choose to exit endogenously, paying a normalized cost $C_e(t)$. This cost reflects not only the penalties from failing to meet the target (since employment drops to zero upon exit) but also implicit costs from impaired relations between the acquirer and the government.¹²

Value function. The normalized value function satisfies

$$\rho \hat{V}(\tilde{z}, l_*, t) - \dot{\hat{V}}(\tilde{z}, l_*, t) = \max \left\{ -C_e(t), \max_{x \geq 0} \left[\begin{array}{l} \pi(\tilde{z}, l_*; \gamma) - \frac{\phi}{2} x^2 \frac{\bar{z}}{\bar{z}(t)} \\ + x [\hat{V}((1 + \lambda_t)\tilde{z}, l_*, t) - \hat{V}(\tilde{z}, l_*, t)] \\ + \mu [\hat{V}(\tilde{z}, 0, t) - \hat{V}(\tilde{z}, l_*, t)] \\ - \delta \hat{V}(\tilde{z}, l_*, t) \end{array} \right] \right\} \quad (9)$$

where ρ is the household discount rate. The left-hand side is the normalized flow value of holding the firm, net of capital gains. The outer maximization is an optimal stopping problem: at each instant, the firm compares the value of exiting—paying normalized cost $C_e(t)$ —against the value of continuing to operate. Since the continuation value is monotonically increasing in productivity while the exit payoff is independent of \tilde{z} , the exit decision follows a cutoff rule: a firm exits when its productivity falls below a threshold. Since the associated fixed cost γl_* depresses the continuation value, the threshold is higher for firms with larger employment targets. These firms are therefore more likely to exit, conditional on initial productivity.

A firm that continues earns normalized flow profits $\pi(\tilde{z}, l_*; \gamma)$ from Lemma 1 and pays the flow cost of productivity investment. It also faces three Poisson events. At rate x , chosen optimally by the firm, a successful improvement raises normalized productivity from \tilde{z} to $(1 + \lambda_t)\tilde{z}$, producing a capital gain $\hat{V}((1 + \lambda_t)\tilde{z}, l_*, t) - \hat{V}(\tilde{z}, l_*, t)$. At rate μ , the employment target expires, transitioning the firm to the unconstrained state $l_* = 0$ and changing its value by $\hat{V}(\tilde{z}, 0, t) - \hat{V}(\tilde{z}, l_*, t)$. At rate δ , the firm is exogenously destroyed and its value drops to zero.

Optimal investment. For firms that choose to stay, the first-order condition for x yields the optimal arrival rate of productivity improvement:

$$x(\tilde{z}, l_*, t) = \frac{\bar{z}(t)}{\phi \tilde{z}} [\hat{V}((1 + \lambda_t)\tilde{z}, l_*, t) - \hat{V}(\tilde{z}, l_*, t)], \quad (10)$$

¹²Firms without targets ($l_* = 0$) face no exit cost.

which equates the marginal cost of investment to the expected gain from a successful productivity improvement. Since the value function inherits the properties of the profit function, the investment rate mimics the pattern of marginal profits illustrated in Panel B of Figure 3: it is higher for firms with binding employment targets. This is the “necessity-driven” effect. A firm forced to employ more than its unconstrained optimum has excess labor whose marginal product is below the wage. An increase in productivity raises the marginal product of all workers, including the excess ones, so the constrained firm gains more from a given productivity improvement than an unconstrained firm at the same productivity level. The longer the firm operates under the employment target (lower μ), the steeper the value function becomes with respect to productivity, and the stronger the incentive to invest.

3.1.3 Predictions for firm-level analysis

The analysis identifies two channels through which employment targets affect firm decisions, each generating testable predictions for the firm-level analysis in Section 5.

The first is a “polarization” effect operating through static labor choices. Employment targets add a fixed cost to the firm’s problem and reduce the marginal cost of labor for firms below the target (Equation 6). The fixed cost component pushes low-productivity firms to exit, while the reduction in the effective marginal cost of labor induces surviving constrained firms to expand employment. Together, these effects yield two empirical predictions:

Prediction 1: Firms with binding employment targets exhibit higher employment growth during the commitment period than firms with non-binding targets.

Prediction 2: Firms with binding employment targets exhibit higher exit rates during the commitment period than firms with non-binding targets.

The second is a “necessity-driven” improvement in productivity, induced by the higher marginal return to productivity investment for constrained firms (Equation 10). Each increment in productivity narrows the gap between a firm’s efficient scale and its mandated employment level, reducing the distortion cost. Constrained firms therefore invest more in productivity than otherwise identical unconstrained ones. This yields a third prediction:

Prediction 3: Firms with binding employment targets exhibit higher productivity growth during the commitment period than firms with non-binding targets.

These firm-level predictions hold for any given wage path and are therefore independent of assumptions about the wage-setting environment. Beyond their implications for individual firms, the polarization channel generates misallocation of labor whose aggregate costs we quantify in Section 6.1: constrained firms face a lower effective marginal cost of labor than unconstrained ones, drawing employment toward less productive firms.

3.2 General Equilibrium: The East German Transition

The firm-level analysis characterizes how employment targets affect individual decisions. To evaluate the aggregate consequences, we embed the firm problem into a general equilibrium framework in which household labor supply, firm entry and exit, and wage setting jointly determine unemployment and the distribution of firms. The key institutional feature that motivates the general equilibrium analysis is the political pressure for wage equalization documented in Section 2: during the transition, East German wages were persistently pushed above market-clearing levels, generating involuntary unemployment. We capture this through a wage determination rule that anchors East German wages toward West German levels, nesting competitive wage setting as a special case. We keep the rest of the general equilibrium structure deliberately simple to isolate the role of employment targets in an economy where wage anchoring generates involuntary unemployment.

3.2.1 Households and Labor Supply

The economy is populated by a continuum of workers of total mass L , who supply labor inelastically. Workers are hand-to-mouth and consume their current income each period. An employed worker earns the wage w_t ; an unemployed worker receives unemployment benefits $\bar{b} w_t$, where $\bar{b} \in (0, 1)$ is the replacement rate. Instantaneous utility is $u(c)$, where u is CRRA with risk aversion σ . All workers hold equal ownership shares in the firm sector, so that profits net of productivity investment costs accrue to them as dividends; Appendix C.1 details the resulting consumption and welfare expressions.

Because labor supply is perfectly inelastic, unemployment is determined entirely by the demand side of the labor market. When the prevailing wage exceeds the level at which firms' aggregate labor demand equals the labor force, some workers are involuntarily unemployed. This demand-determined structure is central to the analysis: wage anchoring to West German levels pushes the East German wage above market clearing, and the resulting unemployment is the first-order welfare cost that employment targets are designed to offset.

3.2.2 Firms, Entry, and the Aggregate Distribution

At each point in time, the economy contains a continuum of firms of total mass M , each characterized by its normalized productivity \tilde{z} and employment target l_* (equal to zero for firms without a target). The joint distribution $m(\tilde{z}, l_*, t)$ summarizes firm heterogeneity at time t and determines aggregate labor demand, output, and productivity, and through market clearing, the equilibrium wage. The general equilibrium is a fixed point in which the wage path shapes firm decisions, firm decisions determine the evolution of the distribution, and the distribution pins down the wage.

Several forces drive the evolution of the distribution. Productivity-enhancing investment shifts mass toward higher productivity: a firm at \tilde{z} that succeeds at its chosen rate x (Equation 10) jumps to $(1 + \lambda_t)\tilde{z}$. Firms exit through two channels. Every firm faces an exogenous destruction hazard δ . In addition, firms with an employment target may exit endogenously because of the fixed-cost structure

created by the penalty (Section 3.1), with the rate depending on how binding the target is. Exiting firms are immediately replaced by new entrants with no employment target, keeping total firm mass at M . Entrants arrive with productivity z_0 at the mean of the productivity distribution, which grows at the balanced growth path (BGP) productivity growth rate. Employment targets also expire at Poisson rate μ , transitioning firms from (\tilde{z}, l_*) to $(\tilde{z}, 0)$. Together, new entry and target expiration gradually unwind labor market distortions over time. Finally, wage growth during the transition erodes normalized productivity \tilde{z} for any firm whose physical productivity does not grow commensurately, intensifying exit pressure on low-productivity firms.

The distribution $m(\tilde{z}, l_*, t)$ evolves according to the Kolmogorov forward equation implied by these forces; Appendix C.4 provides the full expression and describes the numerical solution.

3.2.3 Wage Determination and Unemployment

The wage w_t that firms face reflects both the endogenous response of the labor market to firm dynamics and the political pressures for wage equalization documented in Section 2. We capture this through a parsimonious wage rule in which the East German wage is a convex combination of an endogenous competitive wage w_t^C and the exogenous West German wage w_t^W :

$$w_t = (1 - \kappa_t) w_t^C + \kappa_t w_t^W, \quad 0 \leq \kappa_t \leq 1, \quad (11)$$

where $\kappa_t \in [0, 1]$ governs the strength of wage anchoring at time t . The competitive wage w_t^C is the wage at which unemployment equals the structural rate u_{BGP} , given the total labor force L :

$$L^D(w_t^C) = (1 - u_{\text{BGP}}) L, \quad (12)$$

where aggregate labor demand at any wage w is obtained by integrating the firm-level employment choice from Lemma 1 over the distribution:

$$L^D(w) = \int l(\tilde{z}, l_*; w) m(\tilde{z}, l_*, t) d\tilde{z} dl_*, \quad (13)$$

and $l(\tilde{z}, l_*; w)$ is the optimal employment from Equation (6). The structural unemployment rate captures institutional and labor market factors outside the model, such as skill mismatches, search costs, and regulatory barriers.

Through the competitive component w_t^C , the prevailing wage w_t responds to the evolving state of the labor market: as firms invest in productivity and the distribution of firms shifts, aggregate labor demand changes and w_t^C adjusts accordingly. This channel also allows employment target policies to feed back into wages: when targets increase aggregate labor demand, the competitive wage rises, generating a general equilibrium response that would be absent without the endogenous w_t^C component. Through the West German component w_t^W , the rule captures the above-market-clearing pressures from wage anchoring.¹³ When $\kappa_t = 0$, the wage is fully competitive and unemployment

¹³The wage rule generates above-market-clearing wages whenever $w_t^W > w_t^C$, so that anchoring to the West German wage pulls the prevailing wage above the competitive level. This condition holds throughout the East German transition, as West German wages substantially exceeded East German wages over the entire period.

equals the structural rate. When $\kappa_t > 0$, the wage exceeds the competitive level and unemployment rises above the structural rate:

$$N_t = L^D(w_t), \quad U_t = L - N_t. \quad (14)$$

3.2.4 Equilibrium

Definition 1 (Equilibrium). *Given exogenous policy paths $\{\gamma(t), \lambda(t), C_e(t), \kappa_t\}_{t=0}^T$, the West German wage path $\{w_t^W\}_{t=0}^T$, and an initial firm distribution $m_0(\bar{z}, l_*)$, an equilibrium of the economy described above is a collection of a normalized value function $\hat{V}(\bar{z}, l_*, t)$, firm policies $\{x, l, \text{exit}\}$, a competitive wage path $\{w_t^C\}_{t=0}^T$, a prevailing wage path $\{w_t\}_{t=0}^T$, and a firm distribution $\{m(\bar{z}, l_*, t)\}_{t=0}^T$ such that:*

- (i) **Firm optimality.** *The normalized value function \hat{V} solves the HJB equation (9), employment follows Lemma 1, the investment rate follows (10), and a firm with a binding employment target ($l_* > 0$) exits when its continuation value falls below $-C_e(t)$.*
- (ii) **Wage determination.** *The competitive wage w_t^C satisfies (12) and the prevailing wage w_t satisfies the wage rule (11). Aggregate employment is $N_t = L^D(w_t)$ and unemployment is $U_t = L - N_t$.*
- (iii) **Distribution consistency.** *The distribution $m(\bar{z}, l_*, t)$ evolves according to the Kolmogorov forward equation (given in Appendix C.4) given the firm policies in (i) and the wage path in (ii).*

The equilibrium is computed by iterating jointly on the wage path and the firm distribution until convergence. Appendix C.4 describes the computational procedure and Appendix C.1 derives the aggregate accounting identities.

General equilibrium feedback. The equilibrium embeds a feedback loop through which employment targets affect the broader economy. Targets raise labor demand at constrained firms (Lemma 1), increasing aggregate demand L^D in (13). Through the wage rule (11), the competitive component w_t^C rises, pushing up the prevailing wage. The higher wage reduces flow profits for all firms (Lemma 1), lowering the returns to productivity improvement and reducing the optimal investment rate (10).¹⁴ Over time, slower aggregate productivity growth reduces labor demand, partially unwinding the initial employment gains. Employment targets thus create a tension between short-run unemployment reduction and long-run productivity costs.

4 Data and Descriptive Statistics

To test the model predictions regarding the impact of employment commitments on firm dynamics, we rely on a unique dataset from the German Federal Archives (*Bundesarchiv*) containing all contracts and documentation produced by the THA. These data are confidential, and, thanks to an institutional cooperation with the IWH Institute, we are among the first to gain access to them. Importantly, the

¹⁴The higher wage reduces the returns to productivity improvement for all firms, including constrained ones. For constrained firms, however, this effect is offset by the necessity-driven incentive described in Section 3.1.2, and whether the general equilibrium wage effect or the necessity-driven incentive dominates depends on the parameterization of the model.

agency digitally recorded all contracts for monitoring and enforcement purposes in more than 500 data tables (*ISUD System*). These tables provide comprehensive contract-level information on the privatization of assets, including all the employment commitments that have been agreed upon, as well as dates and audits associated with each commitment. To be able to measure post-privatization dynamics, we combine this contract-level data with information from the Mannheim Enterprise Panel (MUP) and a firm survey called SOESTRA.¹⁵

4.1 Contract Data

In total, the ISUD dataset contains 18,235 contracts with labor commitments that amount to more than 900,000 committed workers and represent about 20% of the initial workforce population of THA firms. Table 1 provides descriptive statistics for 14,726 privatization contracts featuring an employment commitment with at least two audits.¹⁶

Panel A of Table 1 shows contract-level employment information at the start date of the contract, the final level of employment, and the target level of employment. With, on average, 66 employees firms had been relatively sizable at the onset of privatization. Over the course of the commitment period of around three years, firms decreased in size. Panel B relates the initial size of the firm to the final target. The fraction of firms initially below their target is 22%, while about 20% of the firms had to remain at their initial size.¹⁷

In Panel C, we observe for a subset of 1,272 firms the total amount of penalties claimed by the THA due to violations of labor commitments as well as the total number of violations. Conditional on having at least one labor violation, the average firm deviates 2.2 times. The cumulative number of missed employment over multiple violations corresponds, on average, to 111 workers.¹⁸ Finally, consistent with documentation on THA policy (Küpper & Mayr 1993), our calculations suggest that the average stipulated penalty per missed employee amounts to 10,768 EUR (in current prices).

Figure 4 empirically shows the importance of employment targets in distorting firm's labor choices. The bunching patterns constitute the first evidence in line with the predictions of the model. The horizontal axis measures the difference between the realized employment measured at the last audit of a commitment and the final employment target. Firms below 0 are smaller in terms of their realized employment relative to the committed level, whereas firms above 0 have a larger employment with respect to their committed level. The figure plots the bin counts around the normalized target shown by the red vertical line at zero, with each bin representing a unit of employment deviation. A

¹⁵Supplementary Data Appendix E provides a detailed description of the explored ISUD data tables, and detailed description of the data merges with the MUP and SOESTRA data.

¹⁶Figure D.2 in Appendix D provides additional statistics on contracts and audits. Panel A shows that 90% of employment commitments were signed between 1991 and 1994. Panel B documents that these contracts underwent regular audits conducted by contract managers employed by the THA: on average, each contract was audited 6.3 times, with a minimum of 1 and a maximum of 84 audits, and about 82% of all contracts were audited at least twice.

¹⁷In principle, the contract could stipulate multiple employment targets for different deadlines over the course of the commitment period. However, in more than 80% of cases the employment target is constant.

¹⁸The maximum cumulative missed employment amounts to 8,567 workers. This is above the maximum of the final employment target in Panel A, as there can be multiple violations. In addition, the maximum number in Panel A corresponds to the final commitment level.

TABLE 1: SUMMARY STATISTICS

	N (1)	Mean (2)	SD (3)	Minimum (4)	Maximum (5)
A: Average firm size					
Initial employment	14,726	66.20	319.57	0.00	23,691
Final employment	14,726	60.67	194.26	0.00	8540
Final employment target	14,726	52.98	183.25	1.00	6906
B: Initial size relative to target					
Fraction initially below target	14,726	0.22	0.42	0.00	1.00
Fraction initially at target	14,726	0.20	0.40	0.00	1.00
C: Penalties					
Number of observed violation	1,272	2.24	1.29	1.00	12.00
Total number of violated labor	1,272	111.58	393.22	0.24	8,567.47
Penalty per missed employee (in 1000 EUR)	1,272	10.77	10.67	0.10	58.52
D: Productivity					
Initial productivity	3422	10.60	1.48	4.31	17.04
Final productivity	3194	11.92	0.75	10.33	14.39
Initial TFP	3006	6.74	1.23	2.64	9.67
Final TFP	2137	7.28	1.09	3.47	10.32
E: Market exit					
Exit until final commitment year	4622	0.06	0.23	0.00	1.00

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

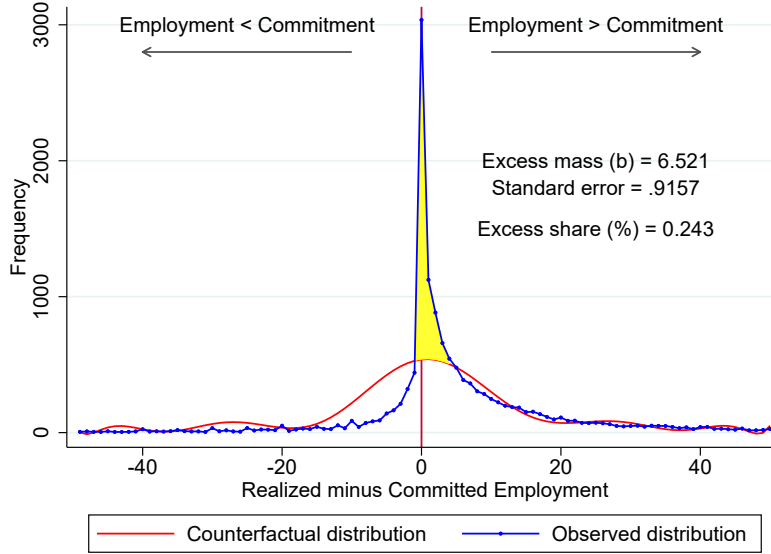
striking feature of the data is the large spike exactly at the committed level of employment, suggesting the importance of these constraints for firms' labor choices. Following Chetty et al. (2011), we estimate an excess mass around the threshold of 652% relative to the average height of the counterfactual distribution.¹⁹

4.2 Firm Data on Exit and Productivity

To construct market exit and productivity measures for privatized firms we rely on data from the Mannheim Enterprise Panel (MUP). The data stems from *Creditreform e.V.*, the largest credit rating agency in Germany, and provides firm-level information for the population of East German com-

¹⁹The red line in Appendix Figure 4 plots the estimated counterfactual density based on a twelve-degree polynomial ($p = 12$) and an asymmetric window around the threshold $R = [3, -1]$. $R = [3, -1]$ denotes the omitted bunching range including firms having up to three more employees than their committed target. The yellow shaded region depicts the estimated excess mass around the threshold. Appendix Figure D.9 provides robustness checks with respect to the degree of the polynomial, the bunching window, and the bin definition.

FIGURE 4: EMPLOYMENT DISTRIBUTION AROUND THE COMMITMENT LEVEL



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

panies since 1993 (Bersch et al. 2014).²⁰ To connect THA contracts to post-privatization outcomes, we merge the contract partners' names with the ownership information in the MUP. For a detailed description of the data merge between the two datasets, please refer to Supplementary Appendix E.

We compute three measures of productivity growth for firms under employment commitments. First, we consider a model-consistent productivity measure given by sales per worker adjusted by the labor share in the production function. Specifically, following the production function in our model, we define

$$\text{Prod}_{it} \equiv \frac{y_{it}}{l_{it}^{\alpha}},$$

where l_{it} is employment, y_{it} is revenue and α is the labor share. This object is monotone in the firm's underlying efficiency in the model: with $y = z^{1-\alpha}l^{\alpha}$, we have $y/l^{\alpha} = z^{1-\alpha}$.²¹ To assess initial productivity, we use information from the opening balance sheets of THA firms with contract data regarding employment at the time of privatization. To measure productivity at the end of the employment commitment, we merge the sales information from MUP with the final employment audit. Second, we construct a firm-level TFP measure as the residual from a Cobb-Douglas production func-

²⁰Bersch et al. (2014) provide detailed information on data collection, processing and the definition of variables. We use wave 56 of the data with the latest available year being 2019.

²¹In the baseline analysis, we set $\alpha = 0.8$, consistent with the aggregate labor share during this period. For robustness we provide our results with different values of α in the Appendix.

tion with capital and labor as inputs. We use the SOESTRA survey, which provides information on revenues, employment, and investment; we build a capital stock measure and estimate the production function estimated separately by sector (more details are provided in Appendix E). Finally, we also employ the semi-parametric approach proposed by [Ackerberg et al. \(2015\)](#) to estimate labor and capital elasticities. Following ACF, we construct a *proxy function* for productivity based on a polynomial in observable inputs, and then estimate labor and capital elasticities using a GMM procedure that exploits the AR(1) structure of productivity.²² The estimation is performed again separately for each 1-digit sector.

Panel D of Table 1 reveals a substantial increase in productivity during the commitment period. This productivity improvement aligns with the documented convergence process observed in the years following reunification. Within the first decade after reunification, approximately half of the measured labor productivity gap and over one-third of the GDP per capita gap between East and West have been closed ([Burda 2006](#)).²³ Starting in 1990, our calculation suggests an increase in productivity of 1.5 log points. The calculated improvements in TFP between the initial contract year and the final commitment period amounts to 0.54 log points. Finally, Panel E of Table 1 provides information on market exit for the matched contracts with the MUP. The sample size for this analysis is larger compared to the productivity assessment, because of missing data in terms of the sales variable. Over the commitment period, which spans an average of 3.3 years, we observe an exit share of 6%.

5 Empirical Analysis of Labor Commitments and Firm Dynamics

We now turn to the empirical framework for testing the predictions derived from the dynamic firm model in Section 3. We outline how we leverage the unique contract data presented in Section 4 to construct an instrumental variable based on the exogenous allocation process of privatizers with heterogeneous labor preferences. Sections 5.2, 5.3, and 5.4 provide the empirical results for employment growth, exit, and productivity growth, respectively.

5.1 Identification Strategy

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

²²Due to limitations in our historical data, we lack systematic information on intermediate inputs. Consequently, we estimate the production function using labor as the flexible input.

²³Based on aggregate statistics, [Bachmann et al. \(2022\)](#) show that GDP per worker increased by about 0.7 log points between 1991 and 2000.

control. By combining this estimate of the fixed trait with the quasi-random allocation of decision makers, we obtain an exogenous shifter that helps mitigate potential biases.

The proposed empirical framework for the analysis is well-suited to our institutional setting for several reasons. The number of privatizations in those years meant that THA agents typically worked on multiple cases. Importantly, the breakneck speed of privatizations generated, within offices, randomness in the assignment of these cases. A consultant with the THA in those years described the process as “an exceptional situation where there was a lot of improvisation.” Finally, at the moment of privatization, each THA agent possessed significant discretion in establishing the conditions for the firm to be privatized, thus leaving room for privatizer traits to matter in the process.²⁴ In our data, we observe the name of the privatizer for 11,194 signed contracts. These contracts are handled by 1,659 different individuals with an average of 6.7 cases per privatizer. Figure D.4 in Appendix D shows the distribution of cases per privatizer. We condition the analysis on privatizers with at least five transactions, resulting in a final sample of 9,363 privatizations and an average of 15.8 cases per privatizer.²⁵

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

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

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

²⁴We provide novel descriptive evidence on privatizer characteristics by linking 702 individuals from the Treuhand archives to IAB employment biographies. The typical privatizer is highly educated (79% hold a university degree), male (84%), German (98%), and around 40 years old, entering the Treuhand as a manager (76%) or administrator (19%). Prior to joining, 38% held managerial roles and 38% administrative positions, and nearly two-thirds had West German work experience before 1990. Prior industry backgrounds include real estate (29%), manufacturing (21%), finance (15%), and the public sector (13%). Appendix Table D.1 reports full descriptive statistics.

²⁵We also provide additional evidence on the sensitivity of our estimates to alternative restrictions on the minimum number of cases per privatizer. Appendix Table D.14 reports the main IV estimates when conditioning on privatizers with more than 10 cases (Panel A) and more than 20 cases (Panel B). In Panel A, restricting the sample to privatizers handling more than 10 cases increases the average number of cases per privatizer to about 25. In Panel B, when limiting the sample to those with more than 20 privatizations, the average rises further to above 36. Across both specifications, the estimated impact of binding labor commitments remains quantitatively very similar to the baseline results. As expected, when conditioning on privatizers with more than 20 cases, the smaller sample size leads to somewhat less precise estimates, although point estimates remain stable.

where i denotes the firm, o the THA office, and j the assigned privatizer.²⁶ $Binding_i$ is an indicator variable equal to 1 if initial employment is smaller than the final employment target, i.e., the firm is constrained to grow. n_{oj} is the number of cases handled by the privatizer in THA office o , and n_o is the number of cases handled by the local THA office. Note that the second term in the formula normalizes the measure by taking into account the average office propensity of writing out binding commitments. Z_{ioj}^{empl} , therefore, measures the leave-one-out measure of binding labor requirements of privatizer j assigned to firm i .

Instrument Validity. Figure D.7 in the Appendix documents a strong and monotone first stage: moving from the bottom to the top decile of the privatizer stringency distribution raises the probability of a binding contract by 21 percentage points. Privatizer preferences are also highly stable across consecutive assignments—the correlation between the leave-one-out stringency measure on successive cases within the same privatizer is 0.91—indicating that the instrument captures persistent individual traits rather than idiosyncratic responses to case characteristics.

Appendix Table D.2 presents balancing tests against pre-privatization observables. We examine a rich set of covariates: firm-level employment, revenue, and productivity measures (Panel A); investor size, creditworthiness, and location (Panel B); and privatizer gender, doctoral degree, and caseload (Panel C). Across all these dimensions, the stringency measure is uncorrelated with every covariate, and after Romano-Wolf correction for multiple testing, not a single coefficient reaches statistical significance. The instrument is likewise uncorrelated with the probability of subsequent contract renegotiation, consistent with the organizational separation between the privatization and contract management units within the THA. Appendix Tables D.3 and D.4 extend this analysis to the sector decomposition and a richer set of privatizer characteristics drawn from IAB social security records, yielding the same conclusion.

Finally, the monotonicity condition is satisfied: first-stage coefficients are uniformly positive and statistically significant across firm-size and sector subgroups, and we cannot reject the joint null of pairwise monotonicity and exclusion following Frandsen et al. (2023). Appendix D.2 discusses the instrument strength, random assignment, monotonicity condition and exclusion restriction in full details.

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

$$y_i = \beta \mathbb{1}(Binding_i) + X_i' \theta + \epsilon_i, \quad (15)$$

²⁶We 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.

with y_i , for example, indicating the growth rate of employment between the first and the final audits.²⁷ 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 and the length of the contract measured in months. The parameter of interest is β , which measures the effect of assigning a binding contract on firm dynamics.

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

$$Binding_i = \gamma Z_{i(j)}^{empl} + X_i' \lambda + \kappa_i \quad (16)$$

One empirical challenge to our identification strategy relates to the fact that privatization decisions are multidimensional as privatizers also decide on the sales price, investment commitments, and financial support. A similar situation arises in the literature on judge decisions as they may not only decide on incarceration but also fines, community service, probation, or guilt. We examine this threat to the exclusion restriction, by following Bhuller et al. (2020), and augmenting our baseline model by including privatizer preferences in these other contract dimensions. That is, if $Z_{i(j)}^{empl}$ denotes the privatizer stringency instrument for the assignment of a binding labor commitment, we construct and include preference measures $Z_{i(j)}^{price}$, $Z_{i(j)}^{investment}$, $Z_{i(j)}^{financial}$ respectively for sales prices, investment commitments, and financial support. This approach allows us to address potential bias arising from the correlation between the privatizer's stringency on labor commitments and their preferences in other contractual decisions, which directly impact subsequent firm dynamics. By controlling for privatizer stringency in these additional contract dimensions within our two-stage least squares estimation, we effectively eliminate this source of bias. Again, we disentangle the specific conditions of a case from the intrinsic privatizer preferences by estimating them as leave-one-out measures.²⁸

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

²⁷More specifically, we follow Davis & Haltiwanger (1999) and construct the firm-level growth rate between the first audit and the final audit of the commitment as $(y_{i,Final} - y_{i,Initial}) / 0.5(y_{i,Final} + y_{i,Initial})$, where y_i , for example, denotes the level of employment of firm i . Subscript *Final* refers to the date of the final commitment audit and, *Initial* refers to the date of the first commitment audit.

²⁸A potential concern is that using a continuous price variable may yield a noisy leave-one-out measure and fail to capture potential nonlinearities in the price schedule. We address both issues jointly in Appendix Table D.15. The table reports IV estimates from our main specification augmented with two alternative leave-one-out price measures. First, we include a dummy equal to one for "high-price" transactions, defined as those above the sample median. Second, we include a dummy equal to one for transactions with prices below DM 100 (EUR 50), which likely reflect cases of negative prices. In our baseline sample, such transactions account for 5.2% of all contracts. Reassuringly, the estimated effects of binding labor commitments remain stable across these alternative specifications.

5.2 Labor Commitments and Firm Employment Growth

Table 2 presents results on the labor distortions induced by employment commitments. The specifications analyze labor growth dynamics throughout the commitment period. Columns (1) to (3) provide OLS estimates starting from the baseline specification and sequentially adding control variables for industry and privatizer characteristics. Columns (4) to (9) provide IV estimates. Standard errors are two-way clustered at the privatizer and office level.

Columns (1) to (3) confirm model predictions suggesting that firms under binding labor commitments upwardly distort their employment decisions. Conditional on the set of baseline controls, industry dummies, and privatizer characteristics, the association between binding contracts and employment growth is on average 52 percentage points until the final commitment date. At a yearly level, the point estimate corresponds to about 13 percentage points differential growth.²⁹

TABLE 2: REGRESSION RESULTS, EMPLOYMENT GROWTH

	OLS Model			2SLS-Model					
				Survivor		Heckman	-2 Growth	Continuous treatment	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	Survivor	-2 Growth
<i>Panel A: Second-stage results</i>									
Binding contract (0/1)	0.530*** (0.025)	0.527*** (0.025)	0.528*** (0.025)	1.083*** (0.269)	1.090*** (0.282)	1.170*** (0.279)	0.587** (0.256)		
Contract leniency						[.72;3.02]		-0.820*** (0.180)	-0.544** (0.212)
<i>Panel B: First-stage results</i>									
Stringency				0.1877*** (0.044)	0.1944*** (0.047)	0.2208*** (0.065)	0.1870*** (0.050)	-0.238*** (0.067)	-0.186*** (0.075)
Observations	9,051	9,051	9,051	9,050	9,051	9,051	9,363	9,051	9,363
Initial size	60.608	60.608	60.608	60.608	60.608	60.612	60.068	60.608	60.068
Mean growth (non-binding)	-0.002	-0.002	-0.002	-0.002	-0.002	-0.002	-0.064	-0.002	-0.064
F-Statistic				20.08	12.58	9.548	10.75	14.77	7.006
<i>Control variables</i>									
Baseline	✓	✓	✓	✓	✓	✓	✓	✓	✓
Industry FE		✓	✓	✓	✓	✓	✓	✓	✓
Privatizer			✓	✓	✓	✓	✓	✓	✓
<i>Leave-out dimensions</i>									
Price					✓	✓	✓	✓	✓
Investment					✓	✓	✓	✓	✓
Financial					✓	✓	✓	✓	✓
IMR						✓			✓

Notes: The table shows OLS and IV regression results of employment growth on binding contracts. Panel A shows the second-stage results. Panel B shows the reduced form regressing the binding contract indicator on the stringency measure. All specifications control for fully interacted THA agency and year fixed effects and are conditional on having at least five privatizations per privatizer. Binding contracts are defined as initial firm size below the committed target level. Contract leniency is defined as initial firm size over the committed target level (winsorized at 4). Instrument refers to the leave-one-out measure of assigning binding contracts. F-Statistic refers to the Kleibergen-Paap F-Statistic. Baseline controls include the contract length (in months) and log initial employment level measured at the first audit. Industry FE are 2-digit industry dummies. Individual controls refer to the gender of the privatizer and a dummy for a PhD degree. Standard errors are two-way clustered at privatizer and THA office level. Squared brackets indicate 90% bootstrap CI clustered at the privatizer and THA office level using 2,500 replications. *p<0.1, **p<0.05, ***p<0.01.

IV estimates starting in Column (4) support the causal interpretation of the impact of the policy

²⁹We approximate annual growth rates as: $[(1 + \hat{\beta})^{(1/avg. \text{ maturity})}] - 1$, with average contract maturity of 3.3 years.

on firm employment growth. In these specifications, following the random assignment of a binding labor contract, firms are estimated to grow their workforce by approximately 25 percentage points annually during the years under commitment. The effect is not only economically large but also precisely estimated at the 1% significance level. The direction of the OLS bias suggests that, consistent with the institutional mandate of the THA, binding labor targets were allocated more frequently to low-growth firms, thereby worsening the static misallocation resulting from such commitments. In line with the previous evidence, the first-stage statistics for weak instruments is large (Panel B). The Kleibergen-Paap F -tests reject the hypothesis of weak instruments with statistics ranging between 7 and 20. Economically, the first-stage estimates imply that a 10 percentage points increase in labor preferences of privatizers result in a 2 percentage points higher likelihood of a binding labor contract. In line with the quasi-random assignment mechanism, the inclusion of additional covariates does not affect the first-stage coefficients.

The results are robust to a series of alternative specifications. Starting in Column (5) we consider whether our IV estimates are affected by the inclusion of privatizer preferences on other contract dimensions such as prices, investment commitments and financial support. Consistent with the exclusion restriction, our estimates are unaffected. Columns (6) and (7) consider whether survivorship bias could account for the impact of employment commitments on firm employment. Column (6) specifies a Heckman selection equation and includes the inverse mills ratio into the 2SLS estimation.³⁰ In Column (7), we assign a -2 growth rate for firms that exit by the end of the commitment period. Estimates are again robust to both specifications. Finally, Columns (8) and (9) present estimates from specifications where the labor commitment policy is measured as a continuous treatment variable, defined as the ratio of initial firm size to the final target. In terms of magnitude, a one standard deviation stricter labor target, corresponding to a change of -0.52 in the continuous measure, is associated with a 42.6 percentage points increase in employment growth over the commitment period ($-0.52 \times -0.82 \times 100 = 42.6$).

5.3 Labor Commitments and Firm Exit

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

Table 3 provides estimates for the exit specification controlling for the same variables as in the employment growth regressions. The first three columns use the ISUD zero employment indicator,

³⁰Along with X_i as in equation (16), the selection equation specification includes the exogenous preferences $Z_{i(j)}^{empl}$ and $Z_{i(j)}^{other} = \{Z_{i(j)}^{price}, Z_{i(j)}^{investment}, Z_{i(j)}^{financial}\}$. To mitigate possible collinearity across the system of equations, we use additional information about firms determined before the reunification to predict selection (see Appendix Table D.7).

TABLE 3: REGRESSION RESULTS, EXIT PROBABILITY AT FINAL COMMITMENT

	ISUD zero employment			MUP exit indicator		
	OLS	2S2SLS		OLS	2S2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Binding contract (0/1)	0.021* (0.012)	0.042* (0.022) [.003;.09]		0.007 (0.010)	0.126* (0.073) [-.05;.30]	
Contract leniency			-0.022 (0.013) [-.04;.002]			-0.033 (0.077) [-.14;.10]
Observations	4,514	3,061	3,061	4,514	3,061	3,061
Exit share (non-binding)	.006	.002	.002	.048	.044	.044
Share with binding contracts	0.196	0.169	0.169	0.196	0.169	0.169
<i>Control variables</i>						
Baseline controls	✓	✓	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓	✓	✓
Privatizer	✓	✓	✓	✓	✓	✓
Leave-out price		✓	✓		✓	✓
Leave-out investment		✓	✓		✓	✓
Leave-out financial		✓	✓		✓	✓

Notes: The table shows OLS and 2S2SLS regression results of exiting probabilities at the end of the commitment period. The outcome variable takes the value of 1 if the firm is exiting by the end of the commitment period and 0 otherwise. All specifications control for fully interacted THA agency and year fixed effects. Binding contracts are defined as initial firm size below the committed target level. Contract leniency is defined as initial firm size over the committed target level (winsorized at 4). Instrument refers to the leave-one-out measure of assigning binding contracts. Controls are as in the baseline specification. Additional control variable is the exporting status to the Eastern block before 1990. Standard errors are two-way clustered at privatizer and THA office level. Squared brackets indicate 90% bootstrap CI clustered at the privatizer and THA office level using 2,500 replications. *p<0.1, **p<0.05, ***p<0.01.

whereas the last three columns are based on the MUP exit indicator.³¹

The results confirm the model's prediction that labor commitments reduce firm profits and raise exit rates. The OLS estimate in Column (1) implies that binding contracts increase market exit by 2.1 percentage points over the commitment period, or about 0.6 percentage points annually. Columns (2) and (3) report two-sample 2SLS estimates that instrument the labor commitment with privatizer preferences; standard errors in parentheses are two-way clustered at privatizer and THA office level along with clustered bootstrap 90% CIs using 2,500 replications reported in square brackets. In Column (2), binding labor commitments raise firm exit by 4.2 percentage points by the end of the commitment period, corresponding to 1.25 percentage points annually. Column (3) uses the continuous treatment variable—the ratio of initial firm size to final target—and confirms that more stringent labor targets increase the probability of exit. Columns (4) to (6) replicate the analysis using the MUP exit indicator and, despite noisier estimates, yield the same conclusion.

³¹Appendix Figure D.5 shows that firms with non-binding contracts have a relatively constant exit rate of about 4.8%, whereas firms with binding contracts exhibit a higher exit rate of 8.4%, which further increases with the tightness of labor commitments.

5.4 Labor Commitments and Firm-Level Productivity Growth

A contribution of our theoretical framework on labor commitments was to reveal a "necessity-driven" channel for improvements in firm productivity. This dynamic channel is induced by higher marginal profits of distorted firms and arises as they have higher incentives to align their productivity with the new employment target. To provide evidence on this mechanism, we now extend the empirical analysis to productivity dynamics. The analysis uses three productivity measures introduced in Section 4.2: a model-consistent measure, a TFP measure that accounts for capital investment, and a semi-parametric estimate following [Akerberg et al. \(2015\)](#).

Table 4 provides OLS and IV estimates for the "necessity-driven" productivity growth effect. In Panel A, we provide the results using the model-implied productivity measure, whereas Panel B shows the results using the TFP measure that accounts for capital. Column (1) provides estimates for the OLS specification, while Columns (2) to (9) provide estimates for the two-sample 2SLS specification. The bootstrapped 90% confidence intervals are shown in square brackets. Note also that all specifications control for initial productivity.³²

Column (1) of Table 4 provides OLS evidence that firms with binding labor contracts experience higher productivity growth of around 8 to 10 percentage points between the opening balance sheet to the end of the commitment period, corresponding to approximately 2 percentage points annually. As with employment and exit, the OLS estimates display a downward bias relative to the IV estimates. Column (2) shows a point estimate of about 0.7, corresponding to approximately 11 percentage points higher annual productivity growth, with consistent magnitudes across both productivity measures. Column (3) confirms robustness to the inclusion of other privatizer preferences. Column (4) shows that the results are not driven by differences in capital accumulation.³³ Columns (5) and (6) account for survival bias. Columns (7) and (8) use the continuous treatment variable, the ratio of initial firm size to final target, and confirm that more stringent targets generate larger productivity gains: a one standard deviation stricter labor target is associated with approximately 7 percentage points higher annual productivity growth.³⁴

Appendix Table D.8 reports results based on the semi-parametric TFP measure of [Akerberg et al. \(2015\)](#), with very similar point estimates. Finally, Table D.11 in the Appendix shows that necessity-driven productivity improvements were significantly larger for firms facing longer commitment horizons.

5.5 Mechanism and Further Robustness Checks

We now turn to an examination of the potential drivers behind the observed relationship between labor commitments and endogenous productivity improvements. Identifying these mechanisms is

³²Appendix Figure D.6 shows that productivity growth averages 86.8% for firms with non-binding contracts over the commitment period and remains relatively constant above the threshold, while firms with binding contracts exhibit significantly higher productivity growth. Plots based on TFP measures are similar.

³³Appendix Table D.10 provides additional robustness checks for alternative measures of capital.

³⁴We provide further robustness checks for our productivity growth results in Appendix Table D.9 by varying the labor share coefficient for the model-implied productivity measure.

TABLE 4: REGRESSION RESULTS, PRODUCTIVITY GROWTH

	OLS Model		2S2SLS-Model						
	Survivor			Control for capital		Heckman	-2 Growth	Continuous treatment	
	(1)	(2)	(3)	(4)	(7)			(8)	
<i>Panel A: Model-implied ($\alpha = 0.8$)</i>									
Binding contract (0/1)	0.083** (0.030)	0.677* (0.330)	0.779** (0.351)	0.740* (0.372)	0.955** (0.425)	0.672* (0.382)			
Contract leniency		[-.08;1.34]	[.02;1.45]	[-.11;1.46]	[.09;1.68]	[-.33;1.52]		-0.691*** (0.229)	-0.954*** (0.333)
Observations	2,290	1,623	1,623	1,623	1,623	1,669		1,623	1,669
Mean growth (non-binding)	0.852	0.854	0.854	0.783	0.854	0.776		0.854	0.776
<i>Panel B: TFP growth</i>									
Binding contract (0/1)	0.098** (0.043)	0.671** (0.244)	0.719*** (0.245)	0.793*** (0.252)	0.733*** (0.237)	0.837*** (0.211)			
Contract leniency		[-.17;1.10]	[.22;1.16]	[.29;1.24]	[.19;1.21]	[.31;1.36]		-0.310** (0.112)	-0.344*** (0.096)
Observations	2,097	1,824	1,824	1,824	1,824	1,860		1,824	1,860
Mean growth (non-binding)	.352	.366	.366	.366	.366	.328		.366	.328
<i>Control variables</i>									
Baseline	✓	✓	✓	✓	✓	✓		✓	✓
Industry FE	✓	✓	✓	✓	✓	✓		✓	✓
Privatizer	✓	✓	✓	✓	✓	✓		✓	✓
<i>Leave-out dimensions</i>									
Price			✓	✓	✓	✓		✓	✓
Investment			✓	✓	✓	✓		✓	✓
Financial			✓	✓	✓	✓		✓	✓
IMR					✓				

Notes: The table shows OLS and 2S2SLS regression results of measures of productivity growth on binding contracts. All specifications control for fully interacted THA agency and year fixed effects. Binding contracts are defined as initial firm size below the committed target level. Contract leniency is defined as initial firm size over the committed target level (winsorized at 4). Instrument refers to the leave-one-out measure of assigning binding contracts. Controls are as in the baseline specification. Additional controls are log initial productivity. Standard errors are two-way clustered at privatizer and THA office level. Squared brackets indicate 90% bootstrap CI clustered at the privatizer and THA office level using 2,500 replications. *p<0.1, **p<0.05, ***p<0.01.

important not only for internal validity but also for assessing whether the documented effects might generalize beyond our setting and inform broader debates about industrial policies. Table 5 presents the instrumental variables estimates of the relationship between labor commitments and firm-level outcomes in technological innovation, managerial organization, and workforce composition.

Columns (1) and (2) explore innovation activity, proxied by the number of (citation-weighted) patents filed during the commitment period. To account for the skewed distribution of patent counts, the outcome variable is transformed using the inverse hyperbolic sine function. The estimates for the full sample in Column (1) are positive and statistically significant. Column (2) shows that binding labor commitments lead to a slightly larger increase in patenting activity for firms with below-median initial productivity. This finding suggests that firms facing binding labor targets responded by increasing investments in innovation.³⁵

³⁵Appendix Figure D.8 plots mean differences in patent characteristics between firms with binding and non-binding contracts. Patents from firms under binding commitments draw on a broader range of 4-digit IPC technology classes, are more likely to cite prior art from the United States and Japan and less likely to cite East German inventors, and—applying

Columns (3) through (6) examine changes in firm organization and workforce composition. Leveraging a novel linkage between our contract data and the IAB matched employer-employee dataset, we measure within-firm adjustments in managerial intensity, workforce skills, and remuneration practices.³⁶ Column (3) shows that firms subject to binding labor commitments substantially reorganized their managerial structure, increasing the share of managers by an average of 11 percentage points, even after conditioning on their initial managerial intensity. This finding suggests that firms under binding labor targets not only increased their workforce but also substantially changed their internal hierarchy, adding managerial layers to possibly facilitate more complex production processes and monitor more effectively. Column (6) documents a second notable adjustment in workforce composition. Firms under binding labor commitments became 6.7 percentage points more likely to hire workers with prior experience in West Germany. This recruitment pattern suggests that firms actively sought to acquire market knowledge and managerial practices from the more advanced Western economy. The role of managerial improvements in driving productivity growth aligns with evidence from the existing literature (Giorcelli 2019, Giorcelli & Li 2021, Bianchi & Giorcelli 2022).

Leveraging the matched employer-employee data allows us to measure other possible changes in firm organization and employment. The estimates in Columns (4) and (5) indicate neither the wages of new hires nor the share of blue-collar workers show a systematic relationship with binding labor commitments. These results suggest that employment dynamics reflected qualitative changes in firm organization and workforce composition, rather than cost-cutting or labor downgrading.

Robustness Checks. The complexity of the privatization process means that both pre- and post-sale factors could in principle interact with the allocation of labor commitments and contaminate our estimates. To address these potential confounders, we exploit the detailed records from the Treuhand Archive, which systematically documented firm-specific interventions throughout the privatization process. Appendix Table D.12 addresses the three most salient concerns for the full set of outcomes—employment growth, market exit, and TFP growth. First, firms with binding targets may have disproportionately renegotiated their commitments downward following adverse shocks; controlling for a binary indicator of any renegotiation attempt leaves the estimates unchanged, consistent with the strict institutional separation between THA sales and monitoring units. Second, systematic pre-sale restructuring by the THA could simultaneously improve firm viability and influence contractual targets; after including an indicator for collective redundancy agreements made under THA ownership, the coefficients are unaffected. Third, we incorporate the viability ratings assigned by the *Leitungsausschuss*—a special supervisory board of external consultants that assessed larger firms prior to sale—which confirm that our instrument is not proxying for private information on firm quality embedded in privatization decisions.

Appendix Table D.13 examines further potential confounders drawn from the THA archive: restitution claims by former owners, environmental liabilities inherited from GDR-era production, and the natural language processing classifier of Heinrich et al. (2022)—are approximately 20 percentage points more likely to span both product and process innovation dimensions.

³⁶For further details on the data linkage see Diegmann et al. (2024).

TABLE 5: SECOND STAGE REGRESSION RESULTS, MECHANISM

	Patenting (cit. weighted)		Workforce			
	Full sample	< Median Productivity	Manager share	Blue-collar share	Log Wages new hire	West GE hire
	(1)	(2)	(3)	(4)	(5)	(6)
Binding (0/1)	0.173** (0.077) [.00;.34]	0.186** (0.084) [-.19;.65]	0.115** (0.044) [-.008.;.21]	0.036 (0.054) [-.14.;.17]	0.174 (0.247) [-.34;.14]	0.067* (0.037) [-.03.;.14]
Observations	3,057	652	2,138	2,138	1,547	2,250
Mean outcome	.008	.008	.052	.800	4.688	.006
Share binding	.204	.164	.152	.152	.152	.16
<i>Control variables</i>						
Baseline	✓	✓	✓	✓	✓	✓
Industry	✓	✓	✓	✓	✓	✓
Privatizer	✓	✓	✓	✓	✓	✓
Leave-out other	✓	✓	✓	✓	✓	✓
IMR	✓	✓	✓	✓	✓	✓

Notes: The table shows 2S2SLS regression results of different outcome variables on binding contracts. The outcome variable in Columns (1) and (2) is the (citation-weighted) number of patents produced (inverse hyperbolic sign transformed) during the commitment period based on the MUP data. The number of citations is based on a 5-year forward-looking window. Column (2) selects firms with initial (model-implied) productivity levels below the median in the sample. Both specifications control for initial patenting activity. Outcome variables in Columns (3) to (6) are based on employer-employee social security data from the Institute of Employment Research (IAB). Column (3) measures the share of managers at the end of the commitment period controlling for the initial manager share. Column (4) measures the share of blue-collar workers at the end of the commitment period controlling for the initial blue-collar share. We define blue-collar at the occupational level with skilled and unskilled manual, service and administrative workers. Column (5) measures average wages of new hires during the commitment period controlling for the initial wage level. Column (6) is a dummy variable equal to 1 if the firm hires an employee from West Germany and zero otherwise. All specifications control for fully interacted THA agency and year fixed effects. Binding contracts are defined as initial firm size below the committed target level. Instrument refers to the leave-one-out measure of assigning binding contracts. Standard errors are two-way clustered at privatizer and THA office level. Squared brackets indicate 90% bootstrap CI clustered at the privatizer and THA office level using 2,500 replications. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

legacy financial obligations (*Altkredite*). Across all specifications, the estimated effects of binding labor commitments remain stable. Two additional checks concern the construction of the instrument itself. Appendix Table D.14 confirms that restricting the sample to privatizers handling at least 10 or 20 cases leaves the estimates quantitatively unchanged, though the larger threshold comes at the cost of some precision. Appendix Table D.15 shows that replacing the continuous price leave-one-out measure with binary indicators—one for effectively negative prices (below DM 100) and one for above-median transactions—does not affect the results. Taken together, these checks corroborate the earlier balancing tests and reinforce the credibility of our identification strategy.

6 Quantitative Analysis

The empirical analysis established that firms with binding employment targets experience higher employment growth, higher exit rates, and faster productivity growth. These firm-level effects, however, do not by themselves reveal the aggregate consequences of the policy. Did employment targets reduce overall unemployment in East Germany, or did they merely redistribute employment across firms? How large were the productivity costs of misallocating labor? And could alternative policy

designs have achieved similar employment gains at lower cost? To answer these questions, we take the general equilibrium model to data on the East German transition (1991–2010).

We first calibrate parameters that can be pinned down from external sources or from properties of the balanced growth path (Section 6.1). We then estimate the remaining structural parameters by matching the firm-level effects of binding employment targets from the empirical analysis, together with the full 20-year paths of East German unemployment and East-West relative wages (Section 6.2). A key feature of the estimation is that the firm-level regression coefficients from Section 5 serve as targeted moments. We document the estimated parameters and model fit in Sections 6.3 and 6.4. Section 7 then uses the estimated model to evaluate the aggregate and welfare consequences of employment targets and to compare the observed policy to alternative designs.

6.1 Calibration

Table 6 summarizes the externally calibrated parameters. The labor share $\alpha = 0.8$ is set to match the labor earnings share. The pure discount rate is $\rho = 0.05$, consistent with a 5% annual return on capital in steady state. The exogenous exit rate $\delta = 0.05$ is in line with Eurostat business demography statistics for employment-weighted exit rates. The unemployment benefit replacement rate $\bar{b} = 0.67$ reflects average replacement rates in the German unemployment insurance system during the 1990s, and we assume CRRA utility with risk aversion $\sigma = 2$. The target expiration rate $\mu = 12/37.7 \approx 0.318$ per year is calibrated to the mean Treuhand contract duration of 37.7 months in the data. The fraction of firms subject to employment targets, 20%, matches the share of Treuhand-privatized firms in the estimation sample. West German wage growth g_W is estimated from the East-West relative wage data (1991–2000) and fixed at its sample average of 1.3% per year. Because productivity-enhancing investment and growth on the balanced growth path depend on λ and ϕ only through λ^2/ϕ , we normalize the initial step size to $\lambda_0 = 0.25$ and estimate ϕ .

TABLE 6: EXTERNALLY CALIBRATED PARAMETERS

Parameter	Description	Value	Source / Target
α	Labor share	0.800	Labor earnings share
ρ	Discount rate	0.050	Standard
δ	Exogenous exit rate	0.050	Eurostat / Fackler et al. (2013)
\bar{b}	Replacement rate	0.670	German UI system
σ	Risk aversion (CRRA)	2.00	Standard
μ	Target expiration rate	0.318	Mean contract duration: 37.7 months
Frac. with targets	Employment share of targeted firms	0.200	Treuhand privatization share
λ_0	Initial productivity step size	0.250	Normalized

Because we estimate the model through the East German transition toward the Western frontier, we allow for a catch-up process in productivity. Firms far from the frontier can achieve large productivity improvements by adopting existing Western technologies, management practices, and organi-

zational know-how. As the productivity gap narrows, the scope for such improvements diminishes (the classic “advantage of backwardness”; see [Acemoglu et al. 2006](#)). We implement this by allowing the productivity step size $\lambda(t)$ to decline over the transition, parameterized as a piecewise function with estimated values $\lambda(3), \lambda(10), \lambda(15), \lambda(20)$ at knot times $t = 3, 10, 15, 20$ years, bounded below by the long-run BGP step size λ_{BGP} . The latter is calibrated so that the balanced growth path delivers productivity growth consistent with the long-run West German economy. The long-run structural unemployment rate is calibrated to 5%, matching the West German average.

6.2 Estimation

The remaining parameters are estimated jointly by matching the model to empirical moments. These are: the employment target penalty γ , the cost of productivity-enhancing investment ϕ , the exit cost C_e , and four productivity step size values $\lambda(3), \lambda(10), \lambda(15), \lambda(20)$. We estimate these jointly with 20 yearly values of the wage anchoring parameter κ_t (one for each year from mid-1991 to mid-2010), giving a total of 27 estimated parameters. Let M^E denote the vector of empirical moments and $M(\Omega)$ the vector of model-simulated moments, where Ω is the set of parameters to be estimated. We search over Ω to minimize the distance between the model and data:

$$\min_{\Omega} \sum_m \frac{|M_m^E - M_m(\Omega)|}{|M_m^E|}. \quad (17)$$

For given parameter values, evaluating the model proceeds in three stages. We first construct the initial firm distribution by fitting log-normal densities to the observed firm-size distribution. We then solve the general equilibrium transition by iterating jointly on the wage path and aggregate productivity: at each iteration, we solve the firm’s dynamic problem backward in time, evolve the firm distribution forward, and update the wage and aggregate productivity paths, repeating until convergence. On the converged path, we simulate Monte Carlo firm panels and run the same regressions as in the empirical analysis to obtain model-implied coefficients. [Appendix C.4](#) describes the full procedure. We minimize the objective using a combination of simulated annealing and Nelder-Mead simplex, with parallelization across starting values.

Moments. The estimation targets 43 moment conditions matched against 27 parameters, organized in three groups. The first group consists of three firm-level regression coefficients estimated in the empirical analysis: the effect of binding employment targets on employment growth, productivity growth, and exit rates. For these moments, we follow an indirect inference approach: we run the same regressions on model-simulated firm panels, using the same sample restrictions and regression specifications as in the empirical analysis, and match the resulting coefficients to their empirical counterparts. The second group consists of 20 yearly unemployment rates (1991–2010), capturing the full dynamic path of the East German labor market over the transition. The third group consists of 20 yearly East-West relative wage ratios (1991–2010), capturing the pace of wage anchoring.

Identification. All parameters are estimated jointly to match the full set of moment conditions. The firm-level regression coefficients on employment growth and productivity growth discipline the penalty parameter γ , while the coefficient on exit rates informs the exit cost C_e . The investment cost ϕ , the productivity catch-up path λ_t , and the wage anchoring path κ_t are jointly identified by the 20-year paths of East German unemployment and East-West relative wages. With the initial step size normalized, the catch-up path and ϕ jointly determine the intensity of productivity-enhancing investment and therefore the pace at which aggregate labor demand recovers; the unemployment path disciplines these quantities. The wage anchoring path κ_t governs how far wages are pushed above competitive levels. Because West German wage growth g_W is taken as given, targeting the East-West wage ratio is effectively equivalent to targeting East German wage growth. These two time series must be used jointly because the parameters interact through general equilibrium: faster catch-up raises labor demand, pushing down unemployment and pushing up wages, while higher κ_t raises wages directly and depresses employment.

6.3 Estimated Parameters

Table 7 reports the estimated parameter values.

TABLE 7: ESTIMATED PARAMETER VALUES

Parameter	Description	Value
γ	Employment target penalty	0.721
ϕ	Productivity investment cost	0.017
C_e	Exit cost	51.6
$\lambda(3)$	Productivity step size at $t = 3$	0.163
$\lambda(10)$	Productivity step size at $t = 10$	0.043
$\lambda(15)$	Productivity step size at $t = 15$	0.043
$\lambda(20)$	Productivity step size at $t = 20$	0.043

The estimated penalty rate implies that for every worker below the mandated level, the firm pays a fine equal to 72% of the wage. At this rate, the penalty is large enough to push most firms with employment targets to hire up to their mandated level, generating the employment growth differential documented in the empirical analysis. The penalty also acts as a fixed cost proportional to the gap between the target and optimal employment, which is largest for low-productivity firms. This produces the model's polarization effect: firms near the bottom of the productivity distribution find it cheaper to exit than to bear the penalty, while surviving firms invest more in productivity to close the gap between their committed labor force and their optimal size. The estimated γ generates employment, productivity, and exit effects that are quantitatively consistent with the empirical regression coefficients.

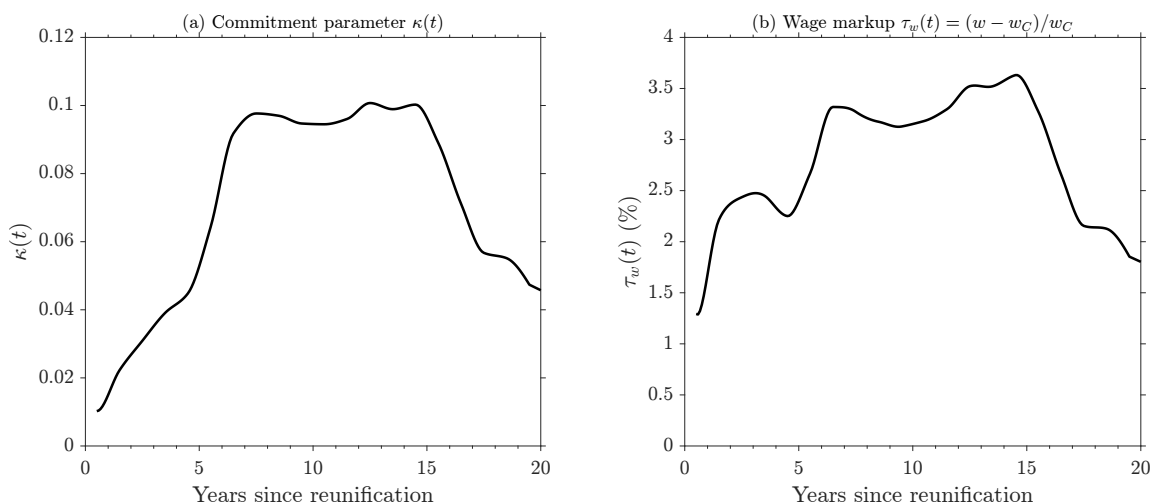
The estimated productivity step size $\lambda(t)$ falls from 0.25 at the start of the transition to 0.163 by year 3, then decreases further to its long-run balanced growth value of 0.043 by year 10, where it

remains for the rest of the horizon. This pattern is consistent with rapid productivity growth in the early post-reunification years followed by a sharp slowdown as the scope for rapid catch-up narrowed (Burda & Hunt 2001).

Estimated $\kappa(t)$ path and wage markup. Figure 5(a) plots the estimated path of $\kappa(t)$, which governs how strongly East German wages are anchored toward West German levels. At $\kappa = 0$ wages are set entirely by local labor market conditions; at $\kappa = 1$ they are fully pegged to the West German wage. The estimated path starts near zero ($\kappa = 0.01$ in mid-1991) and rises gradually, reaching approximately 0.10 by the late 1990s, before declining toward the end of the sample. The gradual rise suggests that wage anchoring pressure built up cumulatively through expanding collective bargaining coverage rather than arriving as a single shock at reunification.

The parameter κ measures the intensity of wage anchoring, but it does not by itself determine the size of the labor market distortion. Because the East German wage is a weighted average of the competitive and West German levels, the distortion also depends on the gap between them. For a given κ , the wider the gap between West and competitive Eastern wages, the further the actual wage is pushed above the market-clearing level. A more direct measure of the resulting distortion is therefore the *wage markup* $\tau_w(t) = (w(t) - w_C(t))/w_C(t)$, the percentage by which the actual East German wage exceeds the competitive level. This measure is analogous to an implicit tax on hiring: firms face an effective labor cost of $(1 + \tau_w)w_C$, and the resulting labor demand shortfall generates involuntary unemployment. With a labor share of $\alpha = 0.8$, the elasticity of labor demand is $-1/(1 - \alpha) = -5$, so a wage markup of $\tau_w = 3\%$ reduces labor demand by approximately 15 percent.

FIGURE 5: WAGE ANCHORING: COMMITMENT INTENSITY AND LABOR MARKET DISTORTION



Notes: Panel (a): $\kappa = 1$ means full convergence to West wages; $\kappa = 0$ means competitive wage. 20 yearly knot values estimated jointly with structural parameters. Panel (b): $\tau_w = (w - w_C)/w_C$ measures the percentage by which the equilibrium wage exceeds the market-clearing level.

Figure 5 (b) plots $\tau_w(t)$ over the transition. The wage markup rises from about 1% in 1991 to a peak of roughly 3.5% in the early 2000s, before gradually declining. The two panels together illustrate how κ and τ_w can move differently. In the early 1990s, the competitive East German wage was far below the West German level (the gap $w_W - w_C$ exceeded 100% of w_C), so even a small commitment ($\kappa \approx 0.01$) generated a meaningful wage markup. As Eastern productivity grew and the West–East gap narrowed, larger values of κ were needed to sustain the distortion. By the late 1990s, κ had risen to about 0.10, but the gap had compressed to roughly 35% of w_C , yielding a wage markup of approximately 3.5%. At its peak, this wage wedge priced roughly 15 to 18 percent of East German workers out of employment.

6.4 Model Fit

Table 8 reports the fit of the estimated model to the targeted moments. The model captures the key patterns documented in the empirical analysis. The three firm-level regression coefficients, which measure the differential effect of binding targets on productivity growth, employment growth, and exit rates, are quantitatively close to their empirical counterparts.

TABLE 8: MODEL VS. DATA MOMENTS

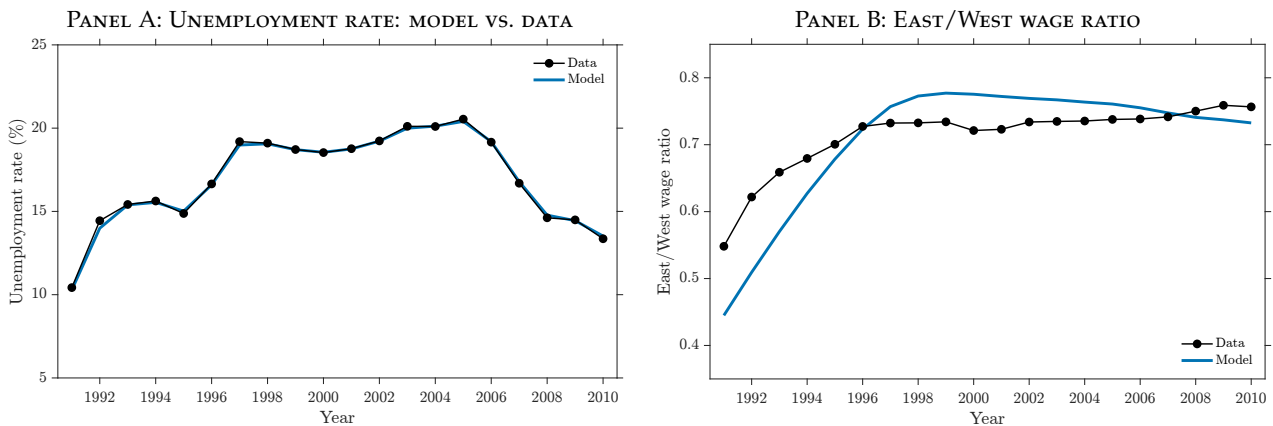
Moment	Data	Model
Emp. growth (binding targets)	0.530	0.411
Lab. prod. growth	0.083	0.068
Exit rate (binding targets)	0.021	0.021
Unemployment rate (1991–2010)	see Figure 6	
East-West wage ratio (1991–2010)	see Figure 6	

Notes: Monte Carlo simulation average over 10 replications. Each firm-level moment is a regression coefficient on a binding-target indicator (initial employment below committed target), controlling for firm size and commitment duration, matching the specification used in the empirical analysis. Unemployment and wage ratio targets consist of 20 yearly observations each.

Figure 6 shows the model’s fit to the 20-year unemployment and relative wage paths, reported as yearly averages. The estimated initial conditions (unemployment 4.0%, wage ratio 0.41 at $t=0$) correspond to the start of the transition; the yearly averages for 1991 incorporate the rapid deterioration during the first year and therefore begin around 10% unemployment and a wage ratio near 0.55. The model fits unemployment closely, tracking the rise to nearly 20% by the late 1990s and the subsequent stabilization. The wage-ratio fit captures the broad convergence pattern well, though the largest deviations occur in the early transition years. The estimated $\kappa(t)$ path provides the flexibility needed to capture the nonlinear dynamics of the transition.

Non-targeted moment: GDP per capita growth. Table 9 compares annualized GDP per capita growth in the data to model-implied output per capita growth over four horizons beginning in 1991.

FIGURE 6: MODEL VS. DATA



Notes: Yearly averages over the 20-year estimation horizon (1991–2010).

Neither aggregate output nor its growth rate is targeted in the estimation. The model captures the broad pattern of rapid early catch-up followed by a sustained slowdown: annualized growth falls from 11.3% over the first four years to 3.9% over the full twenty-year horizon, tracking the data (14.7% and 4.1%, respectively) without matching it by construction. The fit is closest at longer horizons, where the annualized rates are within half a percentage point.

TABLE 9: GDP PER CAPITA GROWTH: DATA VS. MODEL (NON-TARGETED)

	1991–1995	1991–2000	1991–2006	1991–2010
Data	14.7%	7.1%	4.8%	4.1%
Model	11.3%	6.7%	4.3%	3.9%

Note: Annualized growth rates computed from endpoint levels. Data: East German GDP per capita from national accounts. Model: output per capita (Y/L) from the estimated transition. Neither output levels nor growth rates are targeted in the estimation.

In addition, Figure C.1 in the appendix shows that firm-level employment growth closely matches the empirical patterns, even though it is not targeted in the estimation. Both the data and the model exhibit faster growth among firms that are further below their employment targets.

7 Aggregate Consequences of Employment Commitments

We now use the estimated model to evaluate the aggregate implications of employment targets for firm productivity, labor market outcomes, and welfare. The central question is whether employment targets, designed to protect jobs during the transition, achieve this objective, and at what cost to productivity growth and the allocation of resources across heterogeneous firms. We begin by evaluating employment targets in an environment where wages are anchored above market-clearing levels. We then turn to alternative policy designs and isolate how specific design choices (the structure of incentives, the intensity of constraints, the choice between mandates and subsidies) shape the short-run

and long-run consequences of employment commitment policies.

7.1 Employment Targets under Wage Anchoring

Following German reunification, wages in East Germany were rapidly aligned with West German levels through institutional harmonization and political commitments to equal living standards. Because this convergence far outpaced productivity, wages were effectively anchored above market-clearing levels, generating high unit labor costs, widespread restructuring and exit, and persistent unemployment. In this environment, labor commitments have first-order welfare effects, which we quantify using the model. With wage anchoring, employment targets partially offset the resulting unem-

TABLE 10: EFFECTS OF EMPLOYMENT TARGETS RELATIVE TO NO-TARGET ECONOMY

	$T = 3$		$T = 35$	
	Static	Full	Static	Full
Welfare				
Consumption-equivalent variation (%)	+0.41	+0.17	-0.21	-0.04
Aggregate outcomes (period averages)				
Avg. unemployment (%)	-3.16	-3.17	+0.46	-0.13
Output p.c. (%)	-0.14	+0.07	-0.53	-0.17
Consumption p.c. (%)	+0.31	+0.12	-0.46	-0.15
Wage level (%)	+0.18	+0.25	-0.36	-0.11
Growth & investment (period averages)				
TFP growth (%)	-1.58	-0.77	-0.38	-0.13
Invest. rate, untargeted (%)	-0.83	-1.09	-0.48	-0.34
Invest. rate, targeted (%)	-0.86	+2.74	-0.49	+0.41
Firm size				
Avg size, untargeted (%)	-2.24	-2.66	-0.44	-0.44
Avg size, targeted (%)	+11.30	+14.28	+0.23	+9.06
Misallocation				
Labor alloc. eff., all firms (%)	-0.27	-0.20	-0.04	-0.03
Labor alloc. eff., targeted firms (%)	-1.66	-1.15	-1.45	-0.69

Notes: All entries report percentage changes relative to the no-target economy, which retains the estimated wage anchoring path ($\kappa > 0$) but no firm receives an employment target. *Static* isolates the pure labor reallocation channel by holding firms' productivity investment at the no-target level; *Full* allows firms to re-optimize their investment in response to employment targets. Consumption-equivalent variation is the uniform percentage change in consumption, applied to the benchmark, that would make the household indifferent between benchmark and counterfactual; positive values indicate welfare gains. Unemployment, output per capita, consumption per capita, wage level, firm size, and allocative efficiency are period averages over $[0, T]$. TFP growth is annualized from initial to terminal values. Targeted firms are those assigned a contractual employment commitment at privatization; statistics for targeted firms track the cohort of initially targeted firms over the full horizon. Labor allocative efficiency is the ratio of realized aggregate output to the first-best output attainable by reallocating labor across firms, holding the productivity distribution fixed; it equals one when no firm's employment is distorted. The formal definition of the allocative efficiency measure is provided in Appendix C.1.

ployment distortion but do so by distorting firms' decisions, creating a trade-off between short-run

employment gains and long-run productivity losses. Table 10 reports the quantitative effects as percentage changes relative to a benchmark without employment targets ($\gamma = 0$, $C_e = 0$) but with wage anchoring ($\kappa > 0$). We report results at two horizons, three years and thirty-five years, to quantify how the balance between employment gains and productivity costs shifts over time. At each horizon, we decompose the effects into a static equilibrium, in which the necessity-driven investment incentive created by the penalty is shut down, and a full equilibrium that incorporates it.³⁷ Comparing the two isolates the role of necessity-driven investment: effects in the static column reflect labor reallocation and the general equilibrium response, while the difference between the two captures the additional productivity response of constrained firms to the penalty.

Short run (3 years). Employment targets operate largely as intended in the short run, through both direct and incentive effects. The static equilibrium column shows that, even with the necessity-driven investment channel shut down, the policy reduces unemployment by 3.16%, its primary objective, and raises average consumption by 0.31%, reflecting improved income distribution. These gains, however, come at a distortionary cost. The mandate compels constrained firms to expand employment, increasing their workforce by 11.3% on average. The resulting increase in labor demand bids up wages by 0.18%, which reduces employment among untargeted firms by 2.2%. This reallocation, driven solely by the mandate, generates a sizable labor misallocation, amounting to 0.27%.³⁸

The full equilibrium column shows that the penalty structure raises the marginal return to productivity, inducing necessity-driven investment (+2.74%). This further increases aggregate labor demand and tightens the labor market. In equilibrium, wages rise (+0.25%), compressing profits for unconstrained firms and leading them to reduce both employment and investment. Despite these general equilibrium effects, the expansion of constrained firms dominates, lowering unemployment by 3.17% and raising output modestly (0.07%). The same wage pressure, however, distorts resource allocation and reduces aggregate productivity growth (−0.77%). In the short run, level effects from higher employment and wages outweigh the growth losses, yielding a welfare gain of 0.17% in consumption-equivalent terms.

Long run (35 years). Over longer horizons, the cumulative cost of slower productivity growth outweighs the direct employment gains from the mandate. Persistent profit compression reduces investment by unconstrained firms (−0.34%). Originally targeted firms remain larger (+9.1%) and still exhibit labor misallocation (−0.69%), while unconstrained firms remain too small relative to the no-policy benchmark (−0.44%). Lower investment among unconstrained firms slows productivity growth (−0.13%), gradually reducing labor demand. As productivity declines, the initial unemployment reduction largely dissipates: unemployment falls by only 0.13% relative to the no-policy bench-

³⁷All other responses remain active in the static equilibrium: firms' productivity investment responds to general equilibrium conditions (wages, profits), exit is optimally determined, and labor adjusts to the mandate. See Section 3.1 for the characterization of the necessity-driven channel.

³⁸Labor allocative efficiency measures the output loss from misallocated labor: a value of −0.27% means that, holding the productivity distribution fixed, aggregate output would be 0.27% higher if labor were allocated to its first-best level across firms. Appendix C.1 provides the formal definition.

mark. The productivity decline also pulls output per capita below the benchmark (-0.17%) and wages down (-0.11%). Consequently, the initial welfare gains are fully eroded, ultimately leading to a welfare decline of 0.04% .

The static-full decomposition underscores the importance of necessity-driven investment over longer horizons. Without it, employment targets become counterproductive even for their primary objective: unemployment rises 0.46% above the no-policy benchmark, output falls by 0.53% , and welfare declines by 0.21% . Necessity-driven investment prevents this reversal, cutting the welfare loss from 0.21% to 0.04% .

7.2 Alternative Policy Designs

The preceding analysis shows that employment targets generate short-run employment gains but at a significant cost of misallocation and reduced productivity growth that becomes increasingly welfare-reducing over longer horizons. A natural question is whether alternative policy instruments can achieve the employment objective at lower cost. We evaluate four alternatives that share the broad objective of protecting employment but differ in their implementation. Several of these alternatives have been adopted across countries during the restructuring episodes discussed in Section 2 to support employment during economic transitions. Table 11 reports the results, with the first column reporting the equilibrium under employment targets as a benchmark. The exercise does not seek to identify the optimal policy. Rather, by evaluating policies of comparable magnitude that operate through distinct mechanisms, we isolate how specific design choices (the structure of incentives, the intensity of constraints, the choice between mandates and subsidies) shape the short-run and long-run consequences of employment commitment policies. We believe this decomposition offers useful insights for the ongoing industrial policy debate.

Stricter targets. To explore how the short-run/long-run tradeoff scales with policy intensity, we set the target-to-employment ratio of all targeted firms to the 95th percentile of its observed distribution. Stricter targets force firms to expand employment further, reducing unemployment by about 9% in the short run and raising welfare by about 0.3% , despite inducing additional exit among constrained firms. However, tighter targets exacerbate misallocation of labor and further slow productivity growth (-0.44% compared to -0.13% under the baseline policy). As a result, long-run welfare declines by 0.27% , more than six times the baseline loss.

Firing moratorium. Under a firing moratorium, each firm's employment target is set equal to its initial size while the penalty rate remains at its estimated value, making the constraint just binding at the onset. Firms experiencing productivity declines are prevented from downsizing, while expanding firms remain unconstrained. This generates a more uniform distribution of milder constraints across firms, in contrast to employment targets, where many firms face binding constraints that require them to expand well above their efficient scale.

TABLE 11: ALTERNATIVE POLICIES UNDER WAGE ANCHORING ($\kappa > 0$)

	Empl. Targets	Extreme Targets	Firing Moratorium	Empl. Subsidy	Prod. Invest. Subsidy
<i>Panel A: T = 3</i>					
Consumption-equivalent variation (%)	+0.17	+0.30	+0.47	+0.12	-2.13
Avg. unemployment (%)	-3.17	-9.00	-4.96	-1.96	-0.78
Output p.c. (%)	+0.07	+0.16	+0.32	+0.10	+0.68
Consumption p.c. (%)	+0.12	+0.16	+0.36	+0.10	-1.92
Wage level (%)	+0.25	+0.77	+0.41	+0.23	+0.55
TFP growth (%)	-0.77	-2.18	-0.79	-0.35	+2.66
Labor alloc. eff. (%)	-0.20	-0.54	-0.22	-0.09	-0.00
Labor alloc. eff., targeted (%)	-1.15	-1.67	-1.14	-0.51	-0.00
<i>Panel B: T = 35</i>					
Consumption-equivalent variation (%)	-0.04	-0.27	+0.09	+0.01	+0.14
Avg. unemployment (%)	-0.13	-0.08	-0.45	-0.14	-2.84
Output p.c. (%)	-0.17	-0.60	-0.11	-0.07	+1.64
Consumption p.c. (%)	-0.15	-0.54	-0.10	-0.06	+1.29
Wage level (%)	-0.11	-0.39	-0.06	-0.04	+1.17
TFP growth (%)	-0.13	-0.44	-0.09	-0.05	+1.11
Labor alloc. eff. (%)	-0.03	-0.07	-0.03	-0.01	+0.00
Labor alloc. eff., targeted (%)	-0.69	-1.04	-0.83	-0.38	+0.00

Notes: All entries report percentage changes relative to the no-target economy, which retains the estimated wage anchoring path ($\kappa > 0$) but no firm receives an employment target. Each column corresponds to a different policy instrument: *Employment targets* is the estimated policy; *Extreme targets* assigns more stringent commitments (P95 of the target-to-employment ratio); *Firing moratorium* prohibits layoffs for targeted firms. Both the *Employment subsidy* and the *Productivity investment subsidy* are calibrated to match one percent of output over three years. Consumption-equivalent variation is the uniform percentage change in consumption, applied to the benchmark, that would make the household indifferent between benchmark and counterfactual; positive values indicate welfare gains. Unemployment, output per capita, consumption per capita, wage level, and allocative efficiency are period averages over $[0, T]$. TFP growth is annualized from initial to terminal values. Targeted firms are those assigned a contractual employment commitment at privatization. Labor allocative efficiency is the ratio of realized aggregate output to the first-best output attainable by reallocating labor across firms, holding the productivity distribution fixed. The formal definition of the allocative efficiency measure is provided in Appendix C.1.

This difference matters for exit. As shown in the theoretical analysis, the penalty generates an implicit fixed cost that is more likely to trigger exit the further the target exceeds the firm's efficient scale. Employment targets impose this burden on many constrained firms; the moratorium, where the target equals initial size, imposes a lighter burden and induces less exit. Over three years, aggregate labor misallocation ends up being similar under both policies (-0.22% versus -0.20%),³⁹ and TFP growth declines by a comparable magnitude (-0.79% versus -0.77%). By preserving firms that would exit under employment targets, however, the moratorium delivers larger reductions in unemployment at both horizons (4.96% versus 3.17% in the short run, 0.45% versus 0.13% in the long run) and

³⁹The similar aggregate misallocation reflects two offsetting forces. Employment targets create large firm-level distortions but induce exit among the most distorted firms, partially cleaning up the misallocation they generate. The moratorium creates smaller firm-level distortions but induces less exit, so these milder distortions persist across a broader set of firms. In aggregate, the two effects roughly cancel.

substantially larger output gains (+0.32% versus +0.07%). As a result, welfare under the moratorium is higher by 0.47% in the short run and remains positive in the long run (+0.09%), in contrast to the negative welfare outcome under employment targets (−0.04%).

Employment subsidy. The preceding policies did not require direct fiscal expenditures. We now turn to another set of common job-creation policies that operate through subsidies, either to the wage bill of production labor or to productivity-enhancing investment. The next two exercises focus on these instruments. To ensure comparability across the two subsidy instruments, we calibrate the fiscal cost of each to 1% of output.⁴⁰

Under the employment subsidy, the penalty-based mandate is replaced with a per-worker transfer. While the baseline employment targets generate outcomes similar to a labor subsidy, a key difference lies in the structure of incentives: the penalty introduces a fixed-cost component (Equation 7), whereas the subsidy does not. As a result, labor commitments induce firm exit, while a pure subsidy policy largely preserves firms.

This difference along the exit margin is central. With lower exit, the subsidy policy spreads production across a broader set of firms. Although it reduces unemployment by less (1.96% versus 3.17%), it generates a larger increase in aggregate output (about 0.10%, compared to 0.07% under labor commitments). Moreover, labor subsidies generate less severe misallocation, leading to a smaller decline in productivity in both the short and long run. Consequently, labor commitments yield higher welfare gains in the short run (0.17% versus 0.12%), but their long-run effects are inferior: welfare declines under labor commitments, while it increases under the subsidy policy.

Productivity investment subsidy. The productivity investment subsidy takes a different approach: instead of mandating employment, it replaces employment targets with a subsidy to productivity-enhancing investment for the same set of firms. This policy encourages technology adoption and growth, which in turn raises labor demand. Unlike employment targets, it reallocates resources away from consumption toward investment.

The short-run effects reflect this reallocation. Higher investment increases employment and reduces unemployment by 0.78%. Aggregate output rises by 0.68%, but a substantial share is devoted to technology investment, reducing consumption by 1.9%. As a result, short-run welfare declines relative to the no-policy benchmark. In the long run, however, the policy yields substantial gains. By raising productivity growth by 1.11%, it generates additional job creation and reduces unemployment by 2.8%. Importantly, the associated distortions are quantitatively negligible, so long-run welfare increases by 0.14%.

Taken together, the alternative policy exercises yield two main lessons. First, both the intensity and the distribution of the employment mandate matter. Milder constraints distributed broadly across firms, as illustrated by the firing moratorium, limit exit and misallocation while still achieving substantial reductions in unemployment. Aggressive targets deliver larger short-run employment

⁴⁰We have also studied different levels of fiscal cost; those results are available upon request.

gains but generate severe misallocation and more exit, whose cumulative costs quickly overwhelm those gains.

Second, the comparison between employment mandates and the productivity investment subsidy reveals a more fundamental tradeoff between forced and organic approaches to employment growth. All employment mandate policies mandate hiring directly and deliver short-run welfare gains by reducing unemployment, but they create misallocation of labor and, over time, slow aggregate productivity growth. The productivity investment subsidy takes a different approach: rather than forcing employment, it subsidizes productivity growth, which raises labor demand organically as firms become more productive. The compounding nature of productivity gains means that the employment benefits build over time rather than dissipating as they do under employment mandates. Over the full transition, the investment subsidy generates the strongest productivity growth, the largest unemployment reduction, and the highest output gains among all policies evaluated. We note, however, that a pure productivity investment subsidy may not have been a realistic policy option in the East German transition. For firms entering a new market economy, productivity upgrading likely requires more deliberate and targeted interventions than a reduction in investment costs alone can deliver. More broadly, policymakers facing an acute unemployment crisis prioritize policies that deliver immediate employment relief over those with gradual benefits, helping explain why employment commitments were favored over productivity subsidies.

8 Conclusion

Employment commitments are a pervasive tool of government intervention, yet their consequences for firm behavior and aggregate welfare have remained poorly understood. This paper combines a dynamic model of heterogeneous firms, causal evidence from the East German privatization, and a quantitative general equilibrium framework to fill this gap.

The prevalence of employment commitments across more than fifty documented settings suggests that the tradeoffs we identify extend well beyond the East German transition. Our model builds on a standard framework of firm dynamics, and the empirical strategy isolates firm-level responses from the aggregate environment; both the polarization and necessity-driven productivity channels therefore apply, in principle, wherever firms face enforceable employment targets. The general equilibrium analysis, however, is tailored to the East German transition. The post-reunification economy faced many challenges simultaneously, including low initial productivity, the collapse of traditional export markets, and large-scale migration. Among these, our framework focuses on the one that most directly motivated the adoption of employment targets: the sharp rise in unemployment. Several factors contributed to this rise, but to keep the aggregate analysis tractable we focus on the most salient one, emphasized by the literature: the political pressure for rapid wage equalization between East and West Germany, which we capture through a simple wage determination rule.

This focus comes with deliberate simplifications that point toward several promising extensions. Richer wage-setting mechanisms, such as firm-level bargaining or endogenous union formation,

could interact with employment targets in ways that a reduced-form wage rule does not fully capture. Local demand externalities, input-output linkages, and agglomeration spillovers could amplify the social costs of unemployment and provide additional rationale for employment-preserving policies. More broadly, employment commitments can be deployed in bundles: both the East German transition and the Greek banking restructuring of 2013 paired them with early retirement and voluntary separation schemes, and understanding the joint welfare consequences of such policy bundles is an important open question. Adapting the general equilibrium framework to incorporate these forces, and calibrating the welfare assessment to the specific institutional features of the many other settings documented in our policy inventory, are natural directions for future research. Our structural framework and the growing availability of micro-data on industrial policies are well positioned to support these important directions for research.

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A Employment Commitments Across the World

To situate the East German experience within a broader comparative perspective, we assembled and verified an inventory of more than 50 distinct institutional settings in which employment commitments have been deployed as a policy instrument.⁴¹ Appendix Table A.1 reports illustrative cases drawn from the full inventory documented in the *Online Data Repository*, covering six distinct policy domains and spanning advanced and emerging economies over several decades.

Two main patterns emerge. First, employment commitments are not confined to privatization programs but constitute a recurrent feature of a broad range of public–private transactions. Panel A illustrates the privatization case: the 2021 sale of Air India to the Tata Group required a two-year layoff moratorium, enforced through financial penalties, as a condition of the transaction. As shown in Panels B through F, similar requirements arise in a broad set of policy domains, including regulations governing ownership changes, place-based development policies, public procurement, and frameworks designed to manage firm distress or liquidation. Although the economic objectives and policy trade-offs differ across these settings, they share a common feature: governments use firm-level

⁴¹We define employment commitments as labor-related conditions attached to public–private transactions involving asset transfers, regulatory approvals, or subsidies, designed to make firms internalize employment objectives. The inventory excludes commitments arising from general labor regulations or attached to state-owned enterprises that were never transferred to private owners.

policies to transmit and pursue employment objectives. Importantly, such policies appear across both advanced and emerging economies and span several decades, with many remaining in force today.

Second, the form of employment commitments varies with institutional context. In privatization and ownership-change transactions, commitments typically specify layoff moratoria or minimum employment thresholds enforced through financial penalties. Place-based and procurement programs instead rely on explicit job-creation targets tied to conditional subsidies—the agreement between Foxconn and the State of Wisconsin conditioned \$ billion in public support on the creation of 13,000 jobs (Panel C). Crisis-management frameworks employ a wider toolkit, ranging from time-limited moratoria backed by loan forgiveness (U.S. Paycheck Protection Program, 2020–21) to voluntary retirement schemes under tripartite oversight (Greek Restructuring Framework Agreement, 2013; Panel F).

The *Online Policy Repository* provides full documentation, with each case linked to primary sources. Taken together, these cases establish that the core mechanism we study—enforceable firm-level employment conditions attached to a public transaction—is neither institution-specific nor country-specific, while the East German privatization offers a uniquely tractable environment for causal identification owing to the scale of the program and the richness of its administrative records.

This systematic comparison highlights two key advantages of our empirical setting. First, it underscores the unique empirical strength of the East German case as a laboratory for studying employment commitments. In the vast majority of environments where similar labor requirements arise, systematic micro-level data on targets, monitoring, and enforcement are unavailable, making it difficult to evaluate how such commitments shape firm dynamics. By contrast, the large-scale deployment of employment commitments in East Germany provides substantial heterogeneity in target intensity and enables a credible identification strategy based on the quasi-random assignment of privatizers to firms. Second, the international comparison clarifies the broader applicability of our framework: alternative forms of employment-conditioned policies—such as layoff moratoria, job-creation requirements, or conditional subsidy schemes—can be interpreted through the lens of our structural model, demonstrating that the core mechanism we study is neither institution-specific nor policy-specific.

B Analysis of Parliamentary Debates

To examine the context in which employment commitments were discussed, we analyze the full corpus of daily Bundestag session transcripts between 1990 and 1995, the main period of Treuhand operations. The unit of observation is the daily parliamentary protocol. Within each protocol, we identify references to employment commitments and to a range of economic policy objectives, and determine whether these topics are discussed specifically in the context of East Germany or within a broader national frame.⁴²

⁴²Employment commitments are identified by searching for variants of the German term *Arbeitsplatzzusagen*, including *Beschäftigungsklausel* and *Beschäftigungszusagen*. We construct broad categories capturing economic objectives potentially relevant for policymakers, including unemployment, agglomeration economies, productivity, technology, and knowledge accumulation. To identify the East German context, we restrict counts to cases in which a topic term appears within a 600-word window of a reference to East Germany. Results are robust to alternative window sizes of 200 and 400 words.

TABLE A.1: POLICY AREAS: COMMITMENT REQUIREMENTS AND ENFORCEMENT MECHANISMS

Example	Country	Period	Policy / Case	Commitment	Enforcement
Panel A. Privatizations & Government Asset Transfers					
Example 1	India	2021–22	Air India	Layoff moratoria	Financial Penalties
Example 2	Italy	2000	Leonardo	Layoff moratoria	Financial Penalties
Example 3	Hungary	2008	MAV / RCA	Layoff moratoria	Financial Penalties
Panel B. M&A and Ownership Change Regulation					
Example 1	UK	2016	SoftBank / ARM	Job creation	Financial Penalties
Example 2	Canada	1985–...	Investment Canada Act	Min. employment	Financial Penalties
Example 3	France	2014 ...	GE / Alcatel	Job creation	Financial Penalties
Panel C. Place-Based Policies					
Example 1	US	2017–2025	Foxconn Wisconsin	Job creation	Conditional subsidies
Example 2	Türkiye	2016–...	CMP Act	Job creation	Conditional subsidies
Example 3	Germany	1969–...	GRW Act	Min. employment	Conditional subsidies
Panel D. Public Procurement & Concessions					
Example 1	UK	2021–25	A465 Section 5&6	Job creation	Conditional subsidies
Example 2	South Africa	2001	–	Job creation	Financial penalties
Example 3	Italy	2023	Social Clause	Job creation	Financial penalties
Panel E. Crisis, Restructuring & Bankruptcy					
Example 1	France	2014–	Loi Florange	Min. employment	Financial Penalties
Example 2	France	2006–	Go-Sport (L642-5)	Min. employment	Financial Penalties
Panel F. Crisis Response					
Example 1	Germany	N/A	Kurzarbeitergeld	Layoff moratoria	Conditional subsidies
Example 2	Greece	2013	RF Agreements	Layoff moratoria	Voluntary Retirement
Example 3	USA	2020–2021	Paycheck Protection Program	Min. employment	Conditional subsidies

To link references to employment commitments with references to economic policy objectives, we estimate:

$$1(\text{empl. commitment}_p) = \alpha + \beta \times 1(X_p) + \text{Words}_p + \lambda_t + \epsilon_p, \quad (18)$$

where the dependent variable is an indicator equal to one if protocol p contains a reference to employment commitments, and $1(X_p)$ is a dummy equal to one if the protocol references economic objectives such as agglomeration economies, productivity, technology, unemployment, or knowledge spillovers. We control for the log number of words in the protocol, Words_p , to account for the mechanical relationship between protocol length and the probability of observing any given term.

Appendix Table B.1 reports the results. Column (1) shows that references to labor commitments are strongly associated with discussions of the Treuhandanstalt: a protocol mentioning the agency is 6.4 percentage points more likely to also reference employment commitments. Column (2) reveals that this association with unemployment is specific to the East German context—we find no analogous relationship for unemployment references pertaining to Germany as a whole or to West Germany. Column (3) confirms that labor commitments are jointly discussed when both East German unemployment and the Treuhandanstalt are mentioned; the interaction between these two terms is the only coefficient that is statistically and economically significant, raising the probability of an employment commitment reference by 12.4 percentage points. Columns (4) through (7) report analogous specifications for technology, productivity, agglomeration economies, and knowledge accumulation in East Germany: none of these objectives exhibits a significant association with employment commitments or with the Treuhandanstalt. The final column, which includes the full set of interaction terms, confirms that the only robust association arises from the conjunction of East German unemployment concerns and the Treuhandanstalt's privatization mandate.

TABLE B.1: REGRESSION RESULTS: PROBABILITY OF EMPLOYMENT COMMITMENTS DAILY LEVEL

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1(Treuhand)	0.064*** (0.018)		0.058 (0.052)	0.030 (0.019)	0.030 (0.033)	0.040** (0.020)	0.054** (0.024)	0.028 (0.055)
1(Unempl. East)		0.061** (0.027)	-0.028 (0.019)					-0.023 (0.020)
× 1(Unempl. East)			0.124*** (0.042)					0.104** (0.045)
1(Unempl. All)		-0.011 (0.018)	0.004 (0.013)					0.005 (0.010)
× 1(Unempl. All)			-0.051 (0.055)					-0.056 (0.058)
1(Technology East)				-0.029 (0.025)				-0.019 (0.021)
× 1(Technology East)				0.106 (0.076)				0.088 (0.088)
1(Technology All)				0.019 (0.013)				0.014 (0.011)
× 1(Technology All)				0.038 (0.039)				0.027 (0.037)
1(Prod. East)					-0.042* (0.025)			-0.022 (0.017)
× 1(Prod. East)					0.066 (0.048)			-0.013 (0.055)
1(Prod. All)					0.016 (0.013)			0.017* (0.010)
× 1(Prod. All)					0.022 (0.044)			-0.002 (0.043)
1(Aggl. East)						-0.035 (0.031)		-0.013 (0.031)
× 1(Aggl. East)						0.067 (0.114)		0.023 (0.113)
1(Aggl. All)						0.003 (0.011)		-0.002 (0.014)
× 1(Aggl. All)						0.079 (0.053)		0.070 (0.049)
1(Knowledge East)							-0.024 (0.025)	-0.008 (0.027)
× 1(Knowledge East)							0.051 (0.070)	-0.002 (0.072)
1(Knowledge All)							0.009 (0.017)	0.011 (0.016)
× 1(Knowledge All)							0.010 (0.040)	0.005 (0.038)
Log # words	0.009 (0.011)	0.007 (0.012)	0.003 (0.012)	-0.003 (0.011)	0.001 (0.012)	0.004 (0.012)	0.004 (0.012)	-0.010 (0.015)
Observations	439	439	439	439	439	439	439	439
R-squared	0.072	0.068	0.096	0.099	0.079	0.092	0.095	0.160
Mean outcome	.038	.038	.038	.038	.038	.038	.038	.038

Notes: The table shows regression results of the probability that the word labor commitments appears in official protocols of the German Bundestag on indicator variables equal to 1 if the terms appear in the same protocols of the German Bundestag at the daily level. The terms include Treuhand, unemployment; agglomeration economies (e.g., *Agglomerationsvorteile, Wissensspillover, Technologietransfer, Netzwerkeffekte, Skalenerträge, Innovationsökosystem, Unternehmensansiedlung, Regionalförderung*); productivity (e.g., *Produktivitätsfortschritt, Produktivität, Effizienzgewinne, Lohnstückkosten, Wettbewerbsfähigkeit, Unternehmensführung*); technology (e.g., *Digitalisierung, Automatisierung, Technologie, Forschung und Entwicklung, Patent, Innovation*); and knowledge accumulation (e.g., *Weiterbildung, learning-by-doing, Wissenstransfer, Prozesswissen, Lernkurve*). All regressions include an interaction term with the Treuhand indicator. All regressions control for year fixed effects and the log number of all words in the protocols. Robust standard errors clustered at the weekly level in parenthesis. Significance level: *p<0.1, **p<0.05, ***p<0.01.

C Model Appendix

C.1 Aggregation and Misallocation

Each firm produces output $y = z^{1-\alpha} l^\alpha$. In the absence of employment targets ($\gamma = 0$), each firm's profit-maximization first-order condition yields the efficient employment level

$$l^{\text{eff}}(z) = \left(\frac{\alpha}{w_t} \right)^{\frac{1}{1-\alpha}} z, \quad (19)$$

which is proportional to productivity. Define the distortion ratio

$$\tau(z, l^*) \equiv \frac{l(z, l^*)}{l^{\text{eff}}(z)}, \quad (20)$$

so that $\tau = 1$ when a firm hires at its unconstrained optimum, $\tau > 1$ when it over-hires, and $\tau < 1$ when it under-hires.

Substituting $l = \tau \cdot l^{\text{eff}} = \tau \cdot (\alpha/w_t)^{1/(1-\alpha)} z$ into firm output gives

$$z^{1-\alpha} l^\alpha = z^{1-\alpha} \left[\tau \left(\frac{\alpha}{w_t} \right)^{\frac{1}{1-\alpha}} z \right]^\alpha = \left(\frac{\alpha}{w_t} \right)^{\frac{\alpha}{1-\alpha}} z \tau^\alpha.$$

Aggregating over the joint distribution $m(z, l^*, t)$ of productivity and employment targets,

$$Y_t = \left(\frac{\alpha}{w_t} \right)^{\frac{\alpha}{1-\alpha}} \int z \tau(z, l^*)^\alpha m(z, l^*, t) dz dl^*. \quad (21)$$

Aggregate employment is

$$N_t = \int l m dz dl^* = \left(\frac{\alpha}{w_t} \right)^{\frac{1}{1-\alpha}} \int z \tau(z, l^*) m dz dl^*. \quad (22)$$

Both expressions involve integrals of the form $\int z \cdot f(\tau) m dz dl^*$. Define aggregate productivity

$$S_t \equiv \int z m(z, l^*, t) dz dl^*, \quad (23)$$

and the productivity-weighted expectation $\mathbb{E}_z[\cdot]$, which averages using weights $z \cdot m/S_t$. Then (21) and (22) become

$$Y_t = \left(\frac{\alpha}{w_t} \right)^{\frac{\alpha}{1-\alpha}} S_t \mathbb{E}_z[\tau^\alpha], \quad N_t = \left(\frac{\alpha}{w_t} \right)^{\frac{1}{1-\alpha}} S_t \mathbb{E}_z[\tau].$$

Solving the second equation for $(\alpha/w_t)^{1/(1-\alpha)} = N_t/(S_t \mathbb{E}_z[\tau])$ and substituting into the first eliminates the wage:

$$Y_t = \frac{N_t^\alpha}{S_t^\alpha \mathbb{E}_z[\tau]^\alpha} S_t \mathbb{E}_z[\tau^\alpha] = S_t^{1-\alpha} N_t^\alpha \cdot \frac{\mathbb{E}_z[\tau^\alpha]}{\mathbb{E}_z[\tau]^\alpha}.$$

By Jensen's inequality applied to the concave function $\tau \mapsto \tau^\alpha$ (with $0 < \alpha < 1$),

$$\mathbb{E}_z[\tau^\alpha] \leq \mathbb{E}_z[\tau]^\alpha,$$

with equality if and only if τ is constant across firms, i.e., $l \propto z$. Define the misallocation wedge

$$\Omega_t \equiv \frac{\mathbb{E}_z[\tau^\alpha]}{\mathbb{E}_z[\tau]^\alpha} = \frac{Y_t/N_t^\alpha}{S_t^{1-\alpha}} \leq 1. \quad (24)$$

This yields the aggregate production function

$$Y_t = \underbrace{S_t^{1-\alpha}}_{\text{efficient TFP}} \cdot \underbrace{\Omega_t}_{\text{misallocation wedge} \leq 1} \cdot N_t^\alpha, \quad (25)$$

with $S_t = M\bar{Z}_t$. Efficient TFP reflects the productivity distribution shaped by innovation and entry, while the misallocation wedge captures how efficiently labor is allocated across firms given that distribution.

Labor allocative efficiency. The wedge Ω_t measures how much output is lost to the misallocation of labor across firms. In the undistorted competitive equilibrium ($\gamma = 0$), $\tau = 1$ for every firm and $\Omega = 1$. Employment targets create dispersion in τ —constrained firms over-hire ($\tau > 1$), increasing aggregate labor demand and pushing up the equilibrium wage, which causes unconstrained firms to under-hire ($\tau < 1$)—driving Ω_t below unity.

C.2 Aggregate Productivity Growth

Because productivity jumps are multiplicative ($z \rightarrow (1+\lambda)z$), the aggregate productivity growth rate decomposes as

$$g(t) = \lambda(t) \mathbb{E}_z[x(t)] + \delta \cdot \frac{\tilde{z}_0(t) - \bar{z}(t)}{\bar{z}(t)} + \frac{R_{\text{exit}}(t)}{M} \cdot \frac{\tilde{z}_0(t) - \bar{z}_{\text{exit}}(t)}{\bar{z}(t)}, \quad (26)$$

where $\mathbb{E}_z[x(t)] \equiv \int z x m(z, l_*, t) dz dl_* / \int z m(z, l_*, t) dz dl_*$ is the productivity-weighted investment rate, $\bar{z}(t) \equiv \int z m dz dl_* / M$ is mean productivity, $\tilde{z}_0(t)$ is the entry productivity, $R_{\text{exit}}(t)$ is the flow rate of endogenous exit, and $\bar{z}_{\text{exit}}(t)$ is the mean productivity of exiting firms. The first term captures incumbent productivity-enhancing investment; the second, the composition effect of exogenous exit and entry; the third, the composition effect of endogenous exit.

C.3 Welfare

Employment targets affect unemployment, productivity growth, and the allocation of resources across firms, with potentially opposing consequences for welfare. We define a consumption-based welfare measure that captures these tradeoffs.

All L workers hold equal ownership shares in the firm sector, so that profits net of productivity investment costs accrue to workers as dividends. The government funds unemployment benefits through a lump-sum tax on all workers and runs a balanced budget each period. Under this budget rule, penalties paid by firms that fall short of their targets are rebated to workers and cancel in aggregate: they redistribute between firms and the government but do not represent a net resource cost.

Individual consumption is

$$c_t^E = w_t + d_t, \quad (27)$$

$$c_t^U = \bar{b} w_t + d_t, \quad (28)$$

for employed and unemployed workers respectively, where

$$d_t = \frac{Y_t - w_t N_t - C_t^R - \bar{b} w_t (L - N_t)}{L} \quad (29)$$

is the per-worker dividend net of the tax that finances unemployment benefits. Here Y_t is aggregate output and C_t^R is aggregate productivity investment cost.

Flow welfare at time t is

$$\mathcal{U}_t = N_t u(c_t^E) + (L - N_t) u(c_t^U), \quad (30)$$

where $u(c) = c^{1-\sigma}/(1-\sigma)$ is CRRA utility. Discounted welfare over the transition is

$$W = \int_0^T e^{-\rho t} \mathcal{U}_t dt. \quad (31)$$

We report welfare effects as consumption equivalents. The consumption-equivalent measure λ_{CE} is the uniform proportional change in baseline consumption that equates baseline welfare to welfare under the policy:

$$\int_0^T e^{-\rho t} \left[N_t^{\text{base}} u\left((1 + \lambda_{\text{CE}}) c_t^{E,\text{base}}\right) + (L - N_t^{\text{base}}) u\left((1 + \lambda_{\text{CE}}) c_t^{U,\text{base}}\right) \right] dt = W^{\text{policy}}. \quad (32)$$

A positive λ_{CE} indicates that the policy economy delivers higher welfare than the baseline.

C.4 Solution Algorithm

This subsection describes the computational procedure for evaluating the model at a given parameter vector Ω . The algorithm proceeds in four stages: calibration of the balanced growth path, construction of the initial firm distribution, solution of the general equilibrium transition, and simulation of firm panels for moment matching.

C.4.1 Balanced Growth Path and Normalization

The computational algorithm begins by solving for the balanced growth path (BGP) of the undistorted economy (no employment targets, no wage convergence). The BGP serves two purposes: it provides the terminal condition for the backward value function recursion, and it pins down the initial wage level.

Productivity normalization. The model is scale-free in productivity: multiplying all productivities by a common constant leaves employment, investment decisions, and growth unchanged (wages adjust proportionally). We exploit this degree of freedom by normalizing mean normalized productivity to unity on the BGP:

$$\bar{z} \equiv \frac{1}{M} \int \bar{z} m(\bar{z}) d\bar{z} = 1. \quad (33)$$

This is a normalization without loss of generality. The entire BGP distribution, value function, and growth rate can then be expressed in terms of the structural parameters $(\rho, \delta, \lambda, \phi, \alpha)$ alone, without reference to wage levels or the size of the economy.

Value function, growth rate, and initial wage. On the balanced growth path, employment targets are absent ($\gamma = 0, C_e = 0$), the commitment parameter is zero ($\kappa = 0$), and wages grow at constant rate g_w . The normalized value function is linear, $\hat{V}(\tilde{z}) = v \tilde{z}$, where $\hat{V} \equiv V/w$ and v is the constant slope. Since the interest rate satisfies $r = \rho + g_w$ and wages grow at rate g_w , the effective discount rate in the normalized problem equals the pure time preference rate ρ .

The slope v solves the quadratic

$$\frac{\lambda^2}{2\phi} v^2 + (\rho + \delta) v - \bar{\pi} = 0, \quad (34)$$

where $\bar{\pi} = (1 - \alpha)\alpha^{\alpha/(1-\alpha)}$ is the normalized flow profit per unit of \tilde{z} . The unique positive root delivers the BGP investment rate $x^* = (\lambda/\phi) v$ and the growth rate

$$g_w = (1 - \alpha) \lambda x^*. \quad (35)$$

The normalization $\bar{z} = 1$ pins down the initial wage through labor market clearing on the BGP:

$$w_{E,0} = \alpha \left(\frac{M \bar{z}}{(1 - u_{\text{BGP}}) L} \right)^{1-\alpha}, \quad (36)$$

where u_{BGP} is the long-run structural unemployment rate. The West German wage is $w_{W,0} = w_{E,0}/w_{\text{Gap}_0}$, where w_{Gap_0} is the initial East-West wage ratio. We calibrate λ_{BGP} by solving $g(\lambda) = g_w$ via root-finding. The BGP also determines the upper and lower tail exponents of the stationary productivity distribution, which set the boundaries of the computational grid.

C.4.2 Initial Firm Distribution

The initial joint distribution $m_0(\tilde{z}, l^*)$ is constructed from the Treuhand firm panel. For each firm i with observed initial employment l_i , we recover normalized productivity using the static labor demand relation for unconstrained firms:

$$\tilde{z}_i = \frac{l_i}{\alpha^{1/(1-\alpha)}}. \quad (37)$$

The computational grid discretizes the employment target dimension into K levels $l_1^* < l_2^* < \dots < l_K^*$, plus a mass point at $l^* = 0$ for firms without targets. We assign each data firm to its nearest target grid point and fit a separate log-normal distribution to $\log \tilde{z}$ within each group, obtaining location and scale parameters (μ_k, σ_k) for target level k . The initial distribution is thus a mixture of $K + 1$ conditionals: for each target level, the productivity distribution is log-normal with its own mean and variance, weighted by the share of firms at that target level in the data.

Firms in the data represent the fraction $f = 20\%$ of firms subject to employment targets. We scale to the full economy by setting the total mass of firms to $M = N_{\text{data}}/f$ and the labor force to $L = E_{\text{data}}/(f \cdot (1 - u_0))$, where E_{data} is total employment in the sample and u_0 is initial unemployment. This scaling preserves average firm size.

Adjusting for initial unemployment The BGP is calibrated at the long-run structural unemployment rate $u_{\text{BGP}} = 5\%$, but the East German economy in 1991 begins with a higher unemployment rate u_0 . We shift the initial productivity distribution to deliver the target u_0 . Specifically, we apply a common shift $\Delta\mu$ to the log-normal location parameters: $\mu_k \rightarrow \mu_k + \Delta\mu$ for all target groups k . This shifts the mean of the distribution to

$$\bar{z}(0) = \frac{1 - u_0}{1 - u_{\text{BGP}}} \bar{z}_{\text{BGP}}, \quad (38)$$

which, given the wage $w_{E,0}$ from (36), generates labor demand equal to $(1 - u_0)L$. When $u_0 > u_{\text{BGP}}$, the shift is negative ($\Delta\mu < 0$): the initial distribution has lower mean productivity than the BGP, reducing labor demand and raising unemployment to the target level. The value of $\Delta\mu$ is found by bisection. The continuous log-normals are then discretized onto the productivity grid via CDF-based binning, with bin edges at geometric midpoints between adjacent grid points.

C.4.3 General Equilibrium Transition

We solve for the transition path by iterating jointly on the wage path $\{w(t)\}_{t=0}^T$ and the path of mean normalized productivity $\{\bar{z}(t)\}_{t=0}^T$. The two paths are coupled because the cost of productivity investment depends on aggregate productivity: the flow cost is $(\phi/2)x^2\bar{z}/\bar{z}(t)$, so the firm's investment decision depends on the distribution, which in turn depends on investment decisions. Starting from BGP values, each iteration performs the following steps.

Backward solve Given the wage path and the mean productivity path, we solve the Hamilton-Jacobi-Bellman equation backward from $t = T$ to $t = 0$ on the grid (\bar{z}, l^*, t) . The terminal condition is the BGP value function. At each grid point and time step, the firm chooses employment to maximize static profits, chooses a productivity investment rate, and decides whether to exit (shutting down if the continuation value falls below $-C_e$).

Forward solve Given the policy functions from the backward step, we evolve the firm distribution $m(\bar{z}, l^*, t)$ forward from m_0 . The law of motion is the Kolmogorov forward equation:

$$\begin{aligned} \frac{\partial}{\partial t} m(\bar{z}, l^*, t) = & \frac{\partial}{\partial \bar{z}} \left[\psi_t \bar{z} m(\bar{z}, l^*, t) \right] \\ & + x\left(\frac{\bar{z}}{1+\lambda}, l^*, t\right) \frac{1}{1+\lambda} m\left(\frac{\bar{z}}{1+\lambda}, l^*, t\right) - x(\bar{z}, l^*, t) m(\bar{z}, l^*, t) \\ & - \mu \mathbf{1}\{l^* > 0\} m(\bar{z}, l^*, t) + \mathbf{1}\{l^* = 0\} \mu \int_{\bar{l} > 0} m(\bar{z}, \bar{l}, t) d\bar{l} \\ & - q(\bar{z}, l^*, t) m(\bar{z}, l^*, t) + e_t(\bar{z}, l^*), \end{aligned} \quad (39)$$

where $\psi_t = g_w(t)/(1-\alpha)$ is the drift rate from wage growth, $q(\bar{z}, l^*, t) = \delta + \mathbf{1}\{\text{endogenous exit}\}$ is the total exit rate, and $e_t(\bar{z}, l^*)$ is the entry flow. The first line captures the downward drift in normalized productivity as wages grow. The second line captures productivity jumps: a firm at $\bar{z}/(1+\lambda)$ that invests successfully arrives at \bar{z} , while firms at \bar{z} that invest successfully leave. The third line captures

target expiration at rate μ , which moves mass from $l^* > 0$ to $l^* = 0$. The fourth line captures exit and replacement entry. Exiting firms are replaced by entrants at $\tilde{z}_0 = \tilde{z}(t)$ with $l^* = 0$, so the entry flow e_t is concentrated at this point. We discretize this equation using an explicit finite-difference scheme on the grid (\tilde{z}, l^*, t) . From the resulting distribution we extract the updated mean productivity path: $\tilde{z}^{\text{new}}(t) = \sum_{\tilde{z}} \tilde{z} \sum_{l^*} m(\tilde{z}, l^*, t) / \sum_{\tilde{z}} \sum_{l^*} m(\tilde{z}, l^*, t)$.

Wage clearing At each point in time, we solve for the competitive wage $w_C(t)$ that equates aggregate labor demand to $(1 - u_{\text{BGP}})L$. Labor demand is evaluated at the initial wage $w_{E,0}$ rather than the current $w(t)$, because the distribution already incorporates cumulative wage dynamics through the drift term; using $w(t)$ would double-count. The equilibrium wage is

$$w(t) = (1 - \kappa_t) w_C(t) + \kappa_t w_W(t), \quad (40)$$

where $w_W(t)$ is the West German wage path.

Update and convergence Both paths are updated via damping: $w^{(n+1)} = \omega_w w^{(n)} + (1 - \omega_w) w^{\text{new}}$ and $\tilde{z}^{(n+1)} = \omega_z \tilde{z}^{(n)} + (1 - \omega_z) \tilde{z}^{\text{new}}$, where $\omega_w, \omega_z \in (0, 1)$ are damping parameters. The algorithm terminates when the maximum relative change in both the wage path and the mean productivity path falls below a tolerance ε .

C.4.4 Simulation and Moment Construction

On the converged transition path, we construct the model-implied firm-level moments by simulating firm panels that mirror the structure of the Treuhand data. Each simulation initializes every firm i in the Treuhand panel at its observed employment level l_i (converted to productivity $z_i = l_i / \alpha^{1/(1-\alpha)}$) and its observed employment target l_i^* , then evolves the firm month by month over its observed contract duration. In each period, the firm's investment rate $x(z_i, l_i^*, t)$ is obtained by interpolating the policy function from the backward solve over both productivity and employment target dimensions. Three stochastic events can occur:

1. With probability $x_i \Delta t$, the firm receives a productivity improvement: $z_i \rightarrow z_i (1 + \lambda_t)$.
2. With probability $\delta \Delta t$, the firm exits exogenously.
3. If the firm's productivity falls below the endogenous exit threshold from the backward solve, the firm exits.

Surviving firms choose employment to maximize static profits each period. When a firm's contract duration expires, its employment target is set to zero.

At each firm's commitment end date, we record employment growth, productivity growth, and a survival indicator. We then run the same regressions as in the empirical analysis on the simulated panel, using binding employment targets as the treatment variable and controlling for firm size

bins and contract duration. The regression coefficients on the treatment indicator are the model-implied firm-level moments. We average across 10 independent simulation draws (with different random seeds) to reduce Monte Carlo noise. The remaining moments (unemployment rates and East-West wage ratios) are computed directly from the general equilibrium solution by averaging the continuous-time paths to yearly frequency.

C.5 Model with Capital

In this section, we extend our baseline model to include capital in the production function and show that the main predictions of the model presented in Section 3 remain unchanged. For this extension, we assume firms are endowed with constant returns to scale Cobb-Douglas production function of productivity, labor, and capital:

$$y_{t,j} = z_{t,j}^{1-\alpha-\beta} k_{t,j}^\beta l_{t,j}^\alpha$$

where $k_{t,j}$ is the amount of capital hired from the rental market at rate r which is taken as given by the firm. We assume capital fully depreciates after it is used in production at time t for simplicity. With this modification, the static profit maximization problem of a firm with productivity z and employment target l_* is given by

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

The solution to this profit maximization problem follows a pattern similar to that of the baseline model without capital. In particular, optimal labor decision and the resulting profits depend on the the region of productivities:

$$\Pi(z, l_*) = \begin{cases} \left(\frac{A}{w^{\frac{\alpha}{1-\alpha-\beta}} r^{\frac{\beta}{1-\alpha-\beta}}} \right) z > 0 & \text{if } z \in [z_1, \infty) \quad (\text{Undistorted}) \\ z^{\frac{(1-\alpha-\beta)}{1-\beta}} \left[\left(\frac{l_*^\alpha \beta}{r} \right)^{\frac{\beta}{1-\beta}} [(1-\beta)l_*^\alpha] \right] - wl_* > 0 & \text{if } z \in [z_2, z_1) \quad (\text{Distorted, Bunching}) \\ \frac{1}{(1-\gamma)^{\frac{\alpha}{1-\alpha-\beta}}} \left(\frac{A}{w^{\frac{\alpha}{1-\alpha-\beta}} r^{\frac{\beta}{1-\alpha-\beta}}} \right) z - \gamma l_* w > 0 & \text{if } z \in [z_3, z_2) \quad (\text{Distorted, No Bunching}) \\ \frac{1}{(1-\gamma)^{\frac{\alpha}{1-\alpha-\beta}}} \left(\frac{A}{w^{\frac{\alpha}{1-\alpha-\beta}} r^{\frac{\beta}{1-\alpha-\beta}}} \right) z - \gamma l_* w \leq 0 & \text{if } z \in [z_4, z_3) \quad (\text{Distorted, No Bunching}) \\ \text{exit} & \text{if } z \in (0, z_4) \quad (\text{No Production}) \end{cases} \quad (41)$$

and

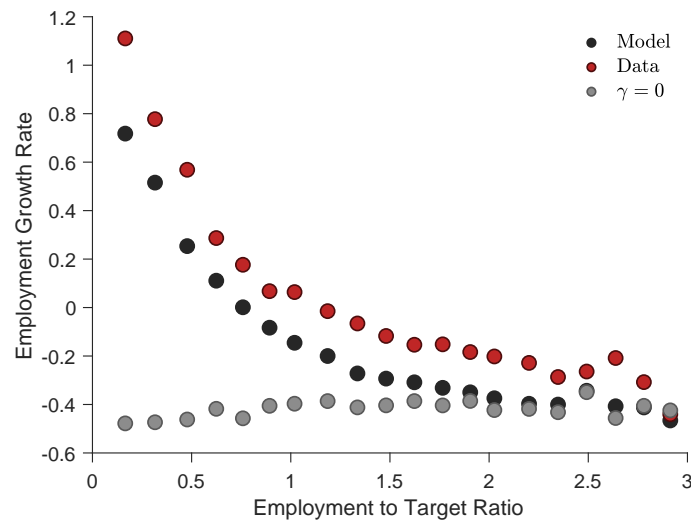
$$l(\tilde{z}, l_*) = \begin{cases} \frac{B}{w^{\frac{1-\beta}{1-\alpha-\beta}} r^{\frac{\beta}{1-\alpha-\beta}}} z & \text{if } z \in [z_1, \infty) \quad (\text{Undistorted}) \\ l_* & \text{if } z \in [z_2, z_1) \quad (\text{Distorted, Bunching}) \\ \frac{1}{(1-\gamma)^{\frac{1-\beta}{1-\alpha-\beta}}} \left(\frac{B}{w^{\frac{1-\beta}{1-\alpha-\beta}} r^{\frac{\beta}{1-\alpha-\beta}}} \right) z & \text{if } z \in [z_4, z_2) \quad (\text{Distorted, No Bunching}) \\ 0 & \text{if } z \in (0, z_4) \quad (\text{No Production}) \end{cases} \quad (42)$$

where $A \equiv \left(\alpha^{\frac{\alpha}{1-\alpha-\beta}} \beta^{\frac{\beta}{1-\alpha-\beta}} \right) (1 - \alpha - \beta)$, $B \equiv \alpha^{\frac{1}{1-\alpha-\beta}} \left(\frac{\beta}{\alpha} \right)^{\frac{\beta}{1-\alpha-\beta}}$ and threshold levels for productivities depend on the employment target l_* . These results suggest that in the model with capital, highly productive firms continue to exhibit no distortions in input choice, while lower-productivity firms distort their labor choice toward the employment target due to the ‘wedge’ in the marginal cost of labor created by employment targets. As a result, distorted firms continue to have a higher marginal profit with respect to productivity, relative to the case of no employment target, due to the lower variable cost of labor induced by the penalty for missing employment target. 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 will have a stronger incentive to invest in productivity improvements even when capital is included in the production function.

C.6 Non-targeted moment: employment growth by target ratio

As an external validation, Figure C.1 plots employment growth against the initial employment-to-target ratio. Firms far below their target (low ratio) exhibit the fastest employment growth, both in

FIGURE C.1: EMPLOYMENT GROWTH RATE BY EMPLOYMENT-TO-TARGET RATIO



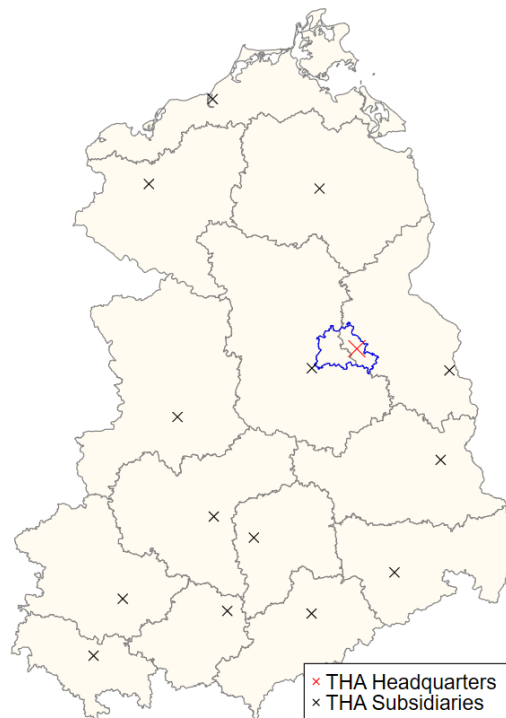
Notes: Symmetric growth rate $2(l_T - l_0)/(l_T + l_0)$ averaged within 20 equal-width bins of the employment-to-target ratio l_0/l_* . Model and $\gamma = 0$ are averages over 10 Monte Carlo seeds.

the data and in the model. Removing targets ($\gamma = 0$) eliminates this relationship entirely: absent the mandate, there is no systematic link between a firm’s proximity to its (now-irrelevant) target and its employment dynamics. The model’s ability to replicate this cross-sectional gradient, which is not targeted in the estimation, provides independent support for the necessity-driven mechanism.

D Additional Empirical Results

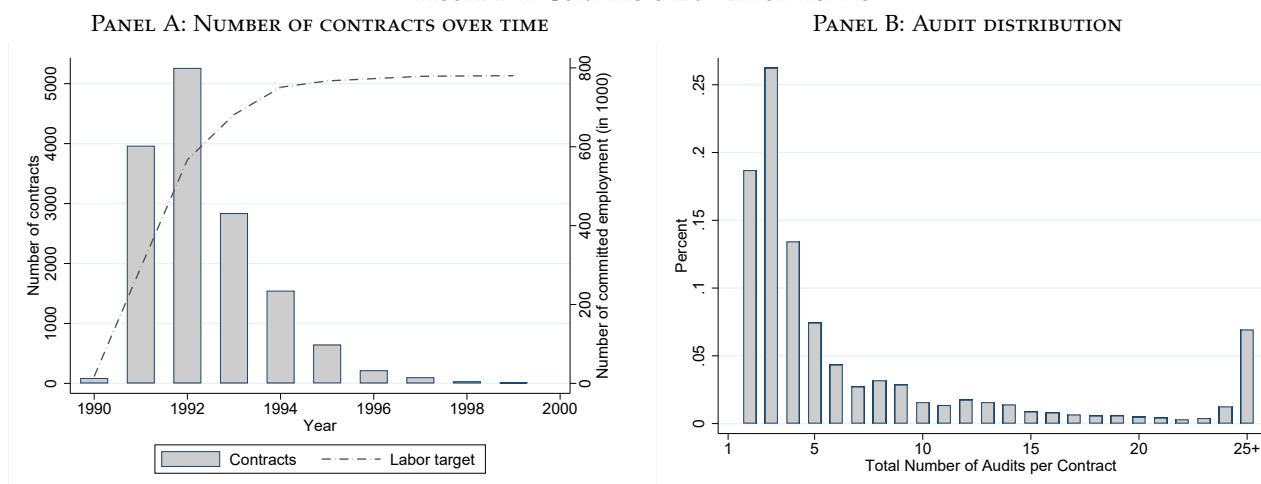
D.1 Additional Descriptive Statistics & Empirical Results

FIGURE D.1: THA HEADQUARTERS AND SUBSIDIARIES



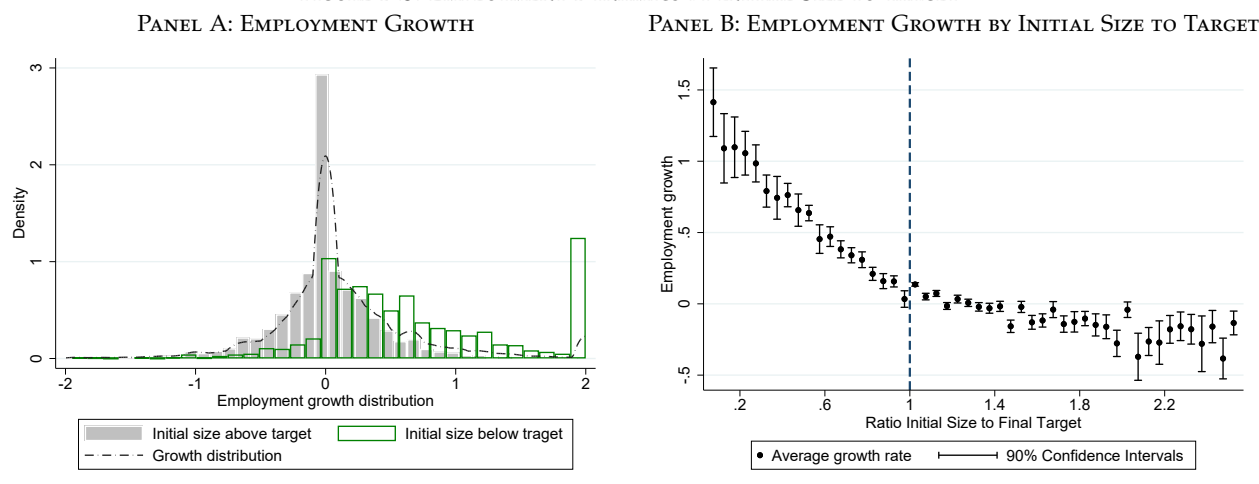
Notes: The figure shows the location of the THA headquarters and local subsidiaries across East Germany. Including East Berlin, the former GDR consisted of 15 district. Each district possesses a local THA office. The headquarters is located in East Berlin, which is indicated by the red cross.

FIGURE D.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.

FIGURE D.3: EMPLOYMENT DYNAMICS BY INITIAL SIZE TO TARGET



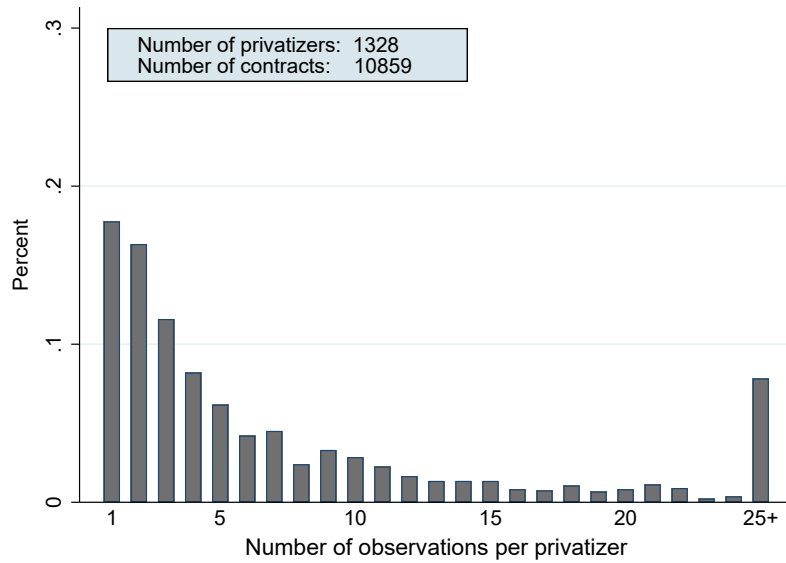
Notes: Panel A shows the overall employment growth distribution as well as the employment growth distribution distinguishing by firms initially below or above (including firms initially at their target) their commitment employment level. Panel B shows average growth rates by the distance of the initial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

Privatizer Characteristics Because our identification strategy exploits variation across privatizers in the employment targets they assigned, it is important to characterize who these agents were. The privatization contracts record each privatizer’s gender and doctoral degree status. To obtain richer measures of human capital and career background, we link 702 individual privatizers to their employment biographies in the Institute for Employment Research (IAB).⁴³ Appendix Table D.1 reports descriptive statistics.

Panel A describes privatizers at the start of their Treuhand spell. The typical privatizer is highly

⁴³We identify establishments belonging to the Treuhand and perform record linkage with workers observed in those establishment IDs (see Diegmann et al. 2025 for the linkage of Treuhand firms to IAB establishments). For data protection reasons, the IAB’s Data and IT Management (DIM) department conducted the privatizer-level linkage, providing all candidate matches together with match-quality indicators.

FIGURE D.4: PRIVATIZATIONS PER PRIVATIZER



Notes: The figure plots the number of privatizations handled per individual privatizer (winsorized at 25). The total number of privatizations is 10,859, handled by 1,328 individuals. 2.17% of all cases are organized by privatizers who appear only once in the sample.

educated (79% hold a university degree), male (84%), German (98%), and approximately 40 years old. Most enter the Treuhand as managers (76%) or administrators (19%). Panel B characterizes their last pre-Treuhand employment spell.⁴⁴ Prior to joining the Treuhand, 38% held managerial roles and 38% administrative ones, suggesting that Treuhand employment was often associated with upward occupational mobility. Nearly two-thirds had West German work experience before 1990, supporting their classification as West Germans. In terms of industry background, 29% previously worked in real estate, 21% in manufacturing, 15% in finance, and 13% came from public-sector employers.

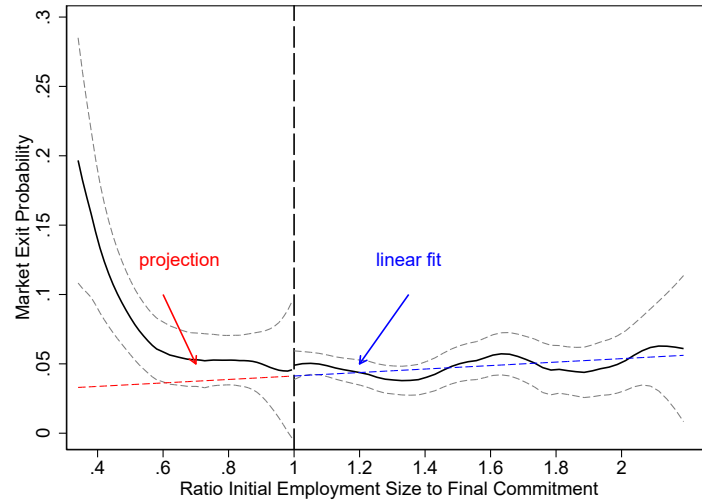
⁴⁴In 240 cases, the first recorded spell in the IAB data corresponds to the Treuhandanstalt itself, likely reflecting East German employees whose records begin only after reunification, or individuals entering the labor market directly from university or another public institution.

TABLE D.1: TREUHAND PRIVATIZER SUMMARY STATISTICS

	Mean (1)	Standard Deviation (2)	Minimum (3)	Maximum (4)
<i>Panel A: Characteristics at THA entry (N=702)</i>				
Age	41.772	10.96	21.00	64.00
Female	0.161	0.37	0.00	1.00
German nationality	0.984	0.12	0.00	1.00
Low-skilled	0.006	0.08	0.00	1.00
Medium skilled	0.152	0.36	0.00	1.00
High-skilled	0.786	0.41	0.00	1.00
Manual jobs	0.009	0.09	0.00	1.00
Engineers/Technicians	0.013	0.11	0.00	1.00
Service	0.009	0.09	0.00	1.00
Experts	0.023	0.15	0.00	1.00
Administrator	0.187	0.39	0.00	1.00
Manager	0.760	0.43	0.00	1.00
Real daily wages	148.149	77.96	0.02	686.64
<i>Panel B: Characteristics before THA entry (N=462)</i>				
Manual jobs	0.029	0.17	0.00	1.00
Engineers/Technicians	0.055	0.23	0.00	1.00
Service	0.044	0.21	0.00	1.00
Experts	0.108	0.31	0.00	1.00
Administrator	0.382	0.49	0.00	1.00
Manager	0.382	0.49	0.00	1.00
Work experience before 1990	0.626	0.48	0.00	1.00
Agricultural	0.005	0.07	0.00	1.00
Mining	0.008	0.09	0.00	1.00
Manufacturing	0.207	0.41	0.00	1.00
Energy and water	0.008	0.09	0.00	1.00
Construction	0.021	0.14	0.00	1.00
Wholesale and retail trade	0.084	0.28	0.00	1.00
Hospitality	0.008	0.09	0.00	1.00
Transportation	0.018	0.13	0.00	1.00
Finance and insurance	0.154	0.36	0.00	1.00
Real estate	0.293	0.46	0.00	1.00
Public sector	0.128	0.33	0.00	1.00
Education	0.024	0.15	0.00	1.00
Healthcare	0.010	0.10	0.00	1.00
<i>Panel C: Number of Observation</i>				
Unique individuals	702			

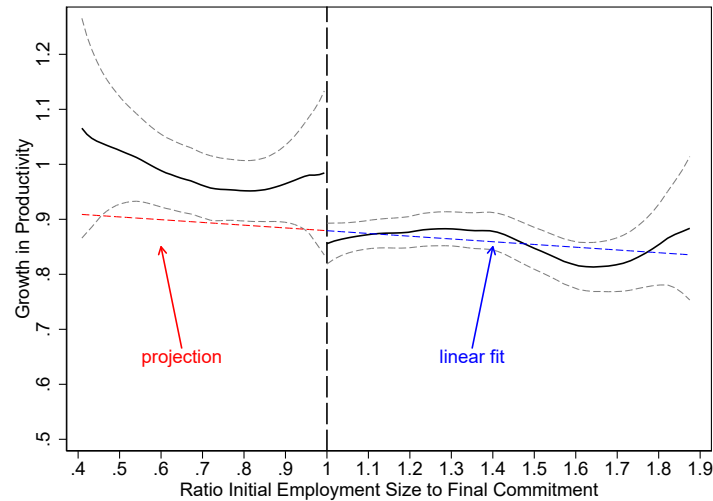
Notes: The table shows summary statistics of individuals employed at the Treuhand. Wages are measured in 2015 prices.

FIGURE D.5: 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.

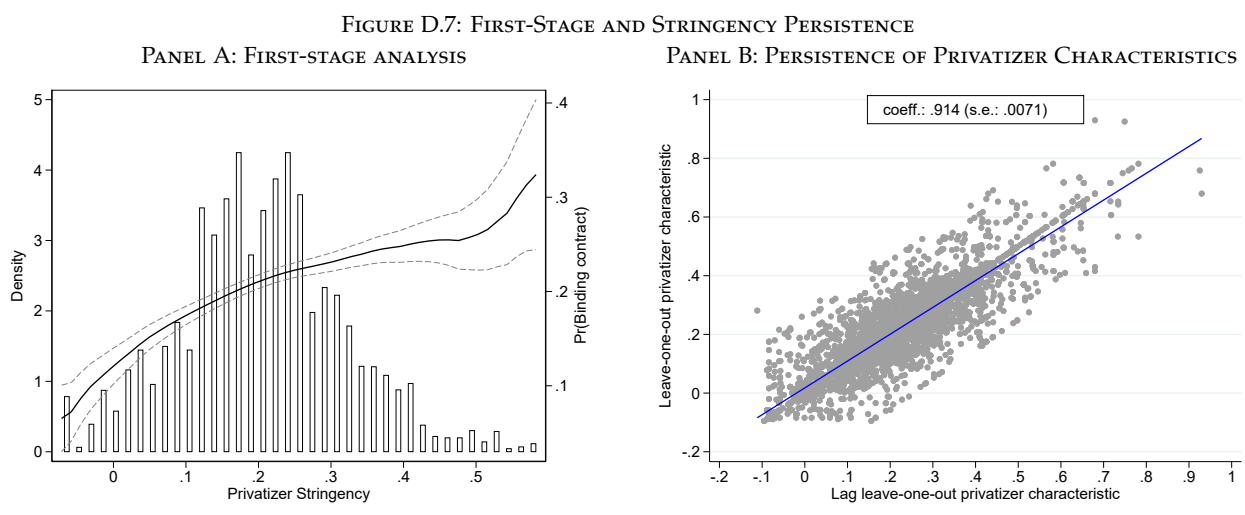
FIGURE D.6: PRODUCTIVITY GROWTH AND THE DEGREE OF BINDING CONTRACTS



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

D.2 Additional Instrumental Variable Results

Instrument Strength. Panel A of Appendix Figure D.7 plots the relationship between binding labor commitments and the estimated privatizer preferences. The density plot is accompanied by a local linear regression highlighting considerable variation in how privatizers impose labor commitments. The probability of receiving a binding contract continuously increases along the stringency measure, e.g. moving from the lowest decile to the highest decile increases the probability of assigning a binding contract by 21 percentage points. Panel B of Appendix Figure D.7 shows that preferences for binding labor commitments are consistent within the individual privatizer, i.e., the correlation coefficient between the leave-one-out measure in the previous case and the leave-one-out measure of the current case (the order of the cases is defined by the date the contract is signed) is 0.91.



Notes: Panel A plots the probability of having a binding contract (initial firm size < final committed size) against the leave-one-out mean privatizer stringency on the right y-axis. The plotted solid line corresponds to a local linear regression of binding contracts on the privatizer stringency. The two dashed lines show the corresponding 95% CI. All plotted values in the local linear regression are mean-standardized residuals from regressions on THA subsidiary times year of privatization fixed effects. The histogram shows the density of privatizer stringency (left y-axis). The figure excludes top and bottom 1% of the stringency measure. Panel B plots the leave-one-out rate of a tight contract (initial firm size < final committed size) in the previous case against the leave-one-out rate of a binding contract in the current case. All plotted values are mean-standardized residuals from regressions on fully interacted THA office and year of privatization fixed effects. The blue line corresponds to a linear regression. Both panels are constructed by conditioning of having handled at least five privatization contracts.

Random Assignment. In a second step we show that labor preferences are uncorrelated to ex-ante characteristics of the assets to be sold. The test exploits pre-assignment information on 12,500 firms that submitted their opening balance sheets in July 1990. In these balancing tests, if the instrument is valid, it should not correlate with these characteristics. We therefore link the labor contracts to these firms and test whether pre-privatization firm-level characteristics correlate with our measure of

privatizer stringency. In Column (1) of Appendix Table D.2 each coefficient represents a regression, with the independent variable being our measure of labor preferences (conditional on fully interacted year and office fixed effects). Column (2) provides p -values with two-way clustered standard errors at the privatizer and local office level. Finally, we provide adjusted p -values for multiple testing using the procedure proposed by Romano & Wolf (2005a) and Romano & Wolf (2005b) with 1,000 bootstrap replications.

Estimates in Table D.2 provide strong evidence that cases are randomly assigned to privatizers in our sample. Consistent with the institutional evidence, the assignment of privatizers with preferences for stringent labor targets is uncorrelated to i) firm characteristics, ii) privatizer characteristics, iii) buyer characteristics. 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.

Panel B investigates whether labor preferences of privatizers can influence buyer selection. To check whether this is the case, we link investor names from the contract data with external MUP data via record linkage. This allows us to obtain investor level information relative to size, credit rating, location (East/West), and industry. After the record linkage, we observe investor characteristics for 4,993 privatization contracts. Similar to Panel A, we fail to observe any systematic evidence for buyer selection. Appendix Table D.3 provides balancing tests in terms of sectoral affiliation. At the contract-level, only 2 out of 16 coefficients are individually significant at the 5% level. Adjusting for multiple testing, however, shows no statistically significant relationship between the privatizer stringency measure and sector affiliation.

The data also includes individual-level characteristics of the privatizer. We leverage this information to examine whether the stringency measure predicts the number of cases, the privatizer's gender, and whether they hold a PhD degree. If there is systematic variation in the stringency measure based on academic qualifications or the ability to handle privatizations, as reflected in the number of cases, it could suggest that decisions regarding labor commitments are influenced by heterogeneity in skills rather than preferences (Chan et al. 2022). However, our findings, presented in Panel C, indicate that privatizer characteristics do not have any predictive power over our instrument. This suggests that heterogeneity in decision-making stems from preferences for strict labor contracts. Appendix Table D.4 replicates this analysis using the richer set of privatizer characteristics available for those matched to IAB social security records, yielding the same conclusions.

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

In summary, the balancing tests indicate that our instrument is as good as randomly assigned with respect to ex-ante characteristics of privatization cases. This finding aligns with institutional

TABLE D.2: TEST OF RANDOM ASSIGNMENT

	Indep. variable: Stringency			Dep. variables	
	Coefficient (1)	<i>p</i> -value (2)	Adj. <i>p</i> -value (3)	Mean (4)	Standard deviation (5)
<u>A: Firms/Contracts characteristics</u>					
<i>Employment</i>					
Accounting	-0.386	0.277	0.983	2.254	1.464
Purchasing	0.191	0.675	1.000	1.638	1.560
HR	-0.029	0.941	1.000	1.886	1.768
Production	-0.103	0.874	1.000	4.436	2.460
R&D	0.041	0.889	1.000	1.250	1.830
Sales	-0.119	0.833	1.000	2.252	1.872
Administration	-0.387	0.379	0.999	3.268	1.872
Firm size above 2000	0.048	0.495	1.000	0.106	0.310
<i>Revenue</i>					
Revenue	-1.099	0.191	0.937	8.084	3.382
Revenue upper 80p	-0.068	0.424	1.000	0.192	0.394
Share of revenue West Europe	0.067	0.162	1.000	0.256	0.436
<i>Productivity</i>					
Labor productivity	-0.130	0.553	1.000	10.252	1.264
Productivity upper 80p	-0.064	0.393	1.000	0.196	0.396
<i>Renegotiation attempt</i>					
Labor renegotiation	-0.018	0.421	1.000	0.064	0.244
<u>B: Investors characteristics</u>					
<i>Employment</i>					
Log investor size	-0.300	0.197	0.974	2.470	1.824
Investor size > 100 employees	-0.078	0.134	0.858	0.140	0.348
<i>Credit rating</i>					
Creditworthiness investor	15.237	0.048	0.974	283.708	101.110
High creditworthiness	-0.041	0.101	0.974	0.066	0.246
<i>Location</i>					
West German investor	0.029	0.597	0.992	0.678	0.468
<u>C: Privatizer characteristics</u>					
Number of cases	12.698	0.269	0.485	30.282	25.128
Gender	-0.013	0.924	1.000	0.870	0.338
PhD degree	0.098	0.395	0.995	0.272	0.446

Notes: The contract-level sample is based on 7,152 contracts with employment, revenue and sector information at the year of reunification 1990. The investor-level sample is based on 4,993 contracts matched to investor characteristics. Employment, revenue, and productivity is measure in logs. All explanatory variables in Panel A refer to the THA initial firm. Explanatory variables at the investor-level are based on the MUP sample and represent the earliest available year 1993. Each line represents a single regression of the explanatory variable on the stringency measure that takes values between 0 (minimum) and 1 (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 & Wolf 2005a; Romano & Wolf 2005b) with 2,500 bootstrap replications. **p*<0.1, ***p*<0.05, ****p*<0.01.

evidence on the allocation mechanism of privatizers within offices. Together, these results strengthen the credibility of the exclusion restriction and help explain the robustness of our estimates to a wide range of case-level characteristics that could potentially correlate with the assigned labor target.

TABLE D.3: TEST OF RANDOM ASSIGNMENT – SECTOR AFFILIATION

	Indep. variable: Stringency			Dep. variables	
	Coefficient (1)	<i>p</i> -value (2)	Adj. <i>p</i> -value (3)	Mean (4)	Standard deviation (5)
<i>Firm/contract characteristics</i>					
Agriculture, forestry, fishing	-0.009	0.681	1.000	0.014	0.114
Energy and water	0.004	0.898	1.000	0.016	0.128
Mining and quarrying	0.014	0.506	1.000	0.008	0.084
Chemical industry and petroleum	0.046	0.137	0.999	0.048	0.216
Plastics and rubber	0.006	0.801	1.000	0.010	0.104
Extraction of cut-stone and sand	0.006	0.734	1.000	0.024	0.156
Iron, casting, steel forming	-0.002	0.956	1.000	0.024	0.152
Steel construction, mechanical engineering	0.209	0.021	0.485	0.166	0.372
Electrical engineering, optics	0.081	0.221	0.972	0.068	0.252
Wood, paper, print industry	0.002	0.945	1.000	0.044	0.204
Textile and clothing	0.010	0.790	1.000	0.058	0.234
Food and beverage industry	-0.062	0.109	0.978	0.046	0.210
Construction and buildings trades	-0.080	0.216	0.941	0.058	0.232
Wholesale and foreign trade	-0.026	0.564	1.000	0.058	0.232
Retail trade	-0.067	0.215	0.885	0.034	0.178
Service	-0.133	0.024	0.868	0.150	0.356
<i>Investor characteristics</i>					
Agriculture, forestry, fishing	0.008	0.708	0.992	0.012	0.112
Mining and quarrying	-0.006	0.449	0.992	0.004	0.070
Manufacturing	-0.062	0.209	0.974	0.176	0.382
Energy and water	-0.033	0.449	0.974	0.044	0.206
Construction	-0.049	0.328	0.974	0.126	0.332
Retail trade	0.107	0.019	0.543	0.200	0.400
Transportation	-0.029	0.216	0.974	0.054	0.228
Hospitality	0.021	0.140	0.991	0.038	0.194
ICT	0.000	0.991	0.992	0.042	0.198
Banking and Insurance	-0.003	0.865	0.992	0.032	0.174
Real Estate	0.021	0.396	0.992	0.058	0.234
Technical services	-0.031	0.517	0.992	0.104	0.304
Economic services	0.034	0.058	0.931	0.034	0.180
Other	0.022	0.733	0.992	0.076	0.264

Notes: Each line represents a single regression of the explanatory variable on the stringency measure that takes values between 0 (minimum) and 1 (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 & Wolf 2005a; Romano & Wolf 2005b) with 1,000 bootstrap replications. **p*<0.1, ***p*<0.05, ****p*<0.01.

TABLE D.4: TEST OF RANDOM ASSIGNMENT - PRIVATIZER CHARACTERISTICS

	Indep. variable: Stringency			Dep. variables	
	Coefficient (1)	<i>p</i> -value (2)	Adj. <i>p</i> -value (3)	Mean (4)	Standard deviation (5)
<u>A: At Treuhand entry</u>					
Age	0.009	0.928	0.999	3.744	0.270
Female	-0.113	0.384	0.993	0.118	0.322
German nationality	0.068	0.012	0.888	0.990	0.100
Low-skilled	-0.004	0.748	0.999	0.004	0.062
Medium skilled	-0.092	0.333	0.993	0.186	0.388
High-skilled	-0.006	0.963	0.999	0.742	0.438
Manual jobs	0.008	0.641	0.998	0.006	0.078
Engineers/Technicians	-0.006	0.639	0.998	0.006	0.080
Service	0.022	0.144	0.903	0.006	0.082
Experts	0.064	0.470	0.995	0.034	0.180
Administrator	-0.049	0.764	0.999	0.224	0.416
Manager	-0.039	0.840	0.999	0.724	0.448
Real daily wages	-0.195	0.191	0.973	4.868	0.464
<u>B: Before Treuhand entry</u>					
Manual jobs	-0.048	0.242	1.000	0.020	0.140
Engineers/Technicians	0.009	0.841	1.000	0.040	0.194
Service	0.143	0.059	0.928	0.034	0.182
Experts	0.007	0.974	1.000	0.122	0.328
Administrator	-0.089	0.638	1.000	0.360	0.480
Manager	-0.022	0.837	1.000	0.424	0.494
Work experience before 1990	0.041	0.743	1.000	0.558	0.496
Agricultural	-0.007	0.687	1.000	0.004	0.062
Mining	-0.028	0.555	1.000	0.006	0.082
Manufacturing	-0.283	0.017	0.694	0.156	0.364
Energy and water	0.035	0.129	0.997	0.006	0.074
Construction	-0.034	0.258	0.997	0.010	0.102
Wholesale and retail trade	-0.062	0.228	1.000	0.090	0.286
Hospitality	0.013	0.367	0.998	0.004	0.058
Transportation	-0.031	0.719	1.000	0.026	0.158
Finance and insurance	0.159	0.356	1.000	0.148	0.354
Real estate	-0.110	0.752	1.000	0.324	0.468
Public sector	0.045	0.619	1.000	0.160	0.366
Education	0.228	0.068	0.981	0.030	0.172
Healthcare	-0.011	0.537	1.000	0.008	0.092
Other services	0.086	0.027	0.966	0.028	0.162

Notes: The contract-level sample is based on 6,744 observations in Panel A and 4,267 contracts in Panel B. Each line represents a single regression of the explanatory variable on the stringency measure that takes values between 0 (minimum) and 1 (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 & Wolf 2005a; Romano & Wolf 2005b) with 2,500 bootstrap replications. **p*<0.1, ***p*<0.05, ****p*<0.01.

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

To test this condition, we first test whether the first stage regression of binding commitments on privatizer stringency is non-negative across various observable subgroups (Bhuller et al. 2018; Dobbie et al. 2018). In Appendix Table D.5, we present the results of first-stage regressions for different firm size groups based on the 1990 measurements and also for sub-samples grouped by sector affiliation. We construct our instrument using the entire sample and perform the first-stage regressions on the sub-samples.⁴⁵ As expected under the assumption of average monotonicity, all first-stage coefficients are positive and statistically significant. Finally, following Frandsen et al. (2023), we implement and fail to reject in Appendix Table D.6 the joint null hypothesis that pairwise monotonicity and exclusion hold. The test for the joint null hypothesis is performed for different numbers of knots and Bonferroni weights using the suggested quadratic spline (controlling for THA office times year fixed effects).

TABLE D.5: FIRST-STAGE REGRESSION RESULTS BY SUB-SAMPLES

	Baseline	Employment in 1990		Revenue in 1990		Sector affiliation	
	(1)	< p(75) (2)	< p(50) (3)	< p(75) (4)	< p(50) (5)	Tradeable (6)	Non-tradeable (7)
Privatizer stringency	0.1488*** (0.041)	0.1123** (0.052)	0.1252* (0.060)	0.1228** (0.046)	0.1173* (0.060)	0.1238** (0.049)	0.0656 (0.049)
Observations	10,250	5,916	3,932	5,826	3,877	5,080	2,927
Average employment at contract date	63.75	57.958	46.668	55.886	49.54	69.17	70.88
Average growth rate	.136	.084	.07	.088	.086	.108	.058
Share with binding contracts	.206	.182	.168	.19	.182	.218	.144
<i>Set of control variables</i>							
Baseline controls	✓	✓	✓	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓	✓	✓	✓
Industry controls	✓	✓	✓	✓	✓	✓	✓

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

⁴⁵To ensure an adequate sample size, these regressions are conducted on sub-samples with a minimum of two privatizations per privatizer.

TABLE D.6: TEST OF JOINT NULL OF MONOTONICITY AND EXCLUSION

	15 knots				20 knots			
	$\omega = 1$ (1)	$\omega = 0.8$ (2)	$\omega = 0.5$ (3)	$\omega = 0.3$ (4)	$\omega = 1$ (5)	$\omega = 0.8$ (6)	$\omega = 0.5$ (7)	$\omega = 0.3$ (8)
Test statistic	638	638	638	638	547	547	547	547
d.f.	(585)	(585)	(585)	(585)	(580)	(580)	(580)	(580)
P-value	[0.063]	[0.079]	[0.126]	[0.210]	[0.835]	[1.000]	[1.000]	[1.000]

Notes: The table presents results from the test proposed in Frandsen et al. (2023) for the joint null hypothesis that the monotonicity and exclusion restrictions hold. We test this null using THA office times year-of-privatization effects conditional on having handled at least 5 privatizations. Columns (1) to (4) provide the results imposing 15 knots in the quadratic spline function. Columns (5) to (8) provide the results imposing 20 knots in the quadratic spline function. Each column is associated with different weighting schemes between the fit and slope components of the test. A failure to reject the null implies that we cannot reject the hypothesis that the monotonicity and exclusion restrictions jointly hold. The test was implemented in Stata via the package `testjfe`.

D.3 Additional Robustness Checks

TABLE D.7: HECKMAN SELECTION EQUATIONS: PROBIT REGRESSION RESULTS

	P(Zero employment at contract end)	
	(1)	(2)
1(Soviet Revenue)	-0.254*** (0.078)	-0.241*** (0.078)
Observations	9,369	8,687
Mean outcome	.034	.036
THA office FE	✓	
Year FE	✓	
THA office times year FE		✓

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

TABLE D.8: REGRESSION RESULTS, TFP GROWTH BASED ON ACKERBERG ET AL. (2015)

	OLS Model				2S2SLS-Model			
	Survivor		Control for capital		Heckman		Continuous treatment	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Binding contract	0.068 (0.045)	0.552** (0.224)	0.582** (0.218)	0.633** (0.227)	0.606** (0.216)	0.733*** (0.175)		
Contract leniency		[.07;1.01]	[.07;1.06]	[.12;1.08]	[.07;1.03]	[.17;1.16]	-0.244** (0.096)	-0.298*** (0.077)
							[-.49;-.11]	[-.50;-.09]
Observations	2,097	1,824	1,824	1,824	1,824	1,860	1,824	1,860
TFP growth	.318	.322	.322	.322	.322	.285	.322	.285
<i>Control variables</i>								
Baseline	✓	✓	✓	✓	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓	✓	✓	✓	✓
Privatizer	✓	✓	✓	✓	✓	✓	✓	✓
<i>Leave-out dimensions</i>								
Price		✓	✓	✓	✓	✓	✓	✓
Investment		✓	✓	✓	✓	✓	✓	✓
Financial		✓	✓	✓	✓	✓	✓	✓
IMR					✓			

Notes: The table shows OLS and 2S2SLS regression results of TFP growth following Akerberg et al. (2015). All specifications control for fully interacted THA agency and year fixed effects. Binding contracts are defined as initial firm size below the committed target level. Contract leniency is defined as initial firm size over the committed target level (winsorized at 4). Instrument refers to the leave-one-out measure of assigning binding contracts. Controls are as in the baseline specification. Additional controls are log initial productivity. Standard errors are two-way clustered at privatizer and THA office level. Squared brackets indicate 90% bootstrap CI clustered at the privatizer and THA office level using 2,500 replications. *p<0.1, **p<0.05, ***p<0.01.

TABLE D.9: ROBUSTNESS TESTS, PRODUCTIVITY GROWTH WITH DIFFERENT α

	$\alpha = 0.7$		$\alpha = 1.0$	
	OLS (1)	2S2SLS (2)	OLS (3)	2S2SLS (4)
Binding contract	0.094*** (0.029)	0.931** (0.412) [.10;1.69]	0.064* (0.033)	0.893** (0.391) [.04;1.59]
Observations	2,290	1,623	2,290	1,623
Mean outcome (non-binding)	.836	.844	.862	.866
<i>Set of control variables</i>				
Baseline controls	✓	✓	✓	✓
Industry controls	✓	✓	✓	✓
Privatizer controls	✓	✓	✓	✓
Leave-out other dimension		✓		✓
IMR		✓		✓

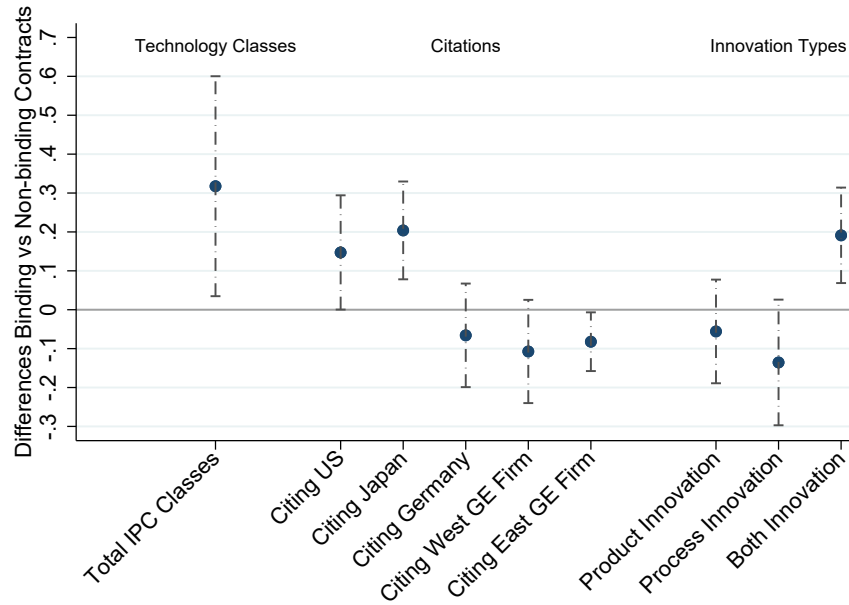
Notes: The table shows OLS and 2S2SLS regression results of measures of productivity growth on binding contracts. All specifications control for fully interacted THA agency and year fixed effects. Binding contracts are defined as initial firm size below the committed target level. Standard errors are two-way clustered at privatizer and THA office level. Squared brackets indicate 90% bootstrap CI clustered at the privatizer and THA office level using 2,500 replications. *p<0.1, **p<0.05, ***p<0.01.

TABLE D.10: SECOND STAGE PRODUCTIVITY GROWTH REGRESSION RESULTS: CONTROLLING FOR CAPITAL

	TFP Growth		Model-consistent Productivity Growth		
	(SOESTRA Data)		(SOESTRA Data)	(MUP Data)	
	(1)	(2)	(3)	(4)	(5)
Binding contract (0/1)	0.758*** (0.233) [.21;1.23]	0.814*** (0.245) [.28;1.26]	0.996*** (0.203) [.47;1.43]	1.182*** (0.218) [.63;1.66]	0.895** (0.369) [-.03;1.58]
Log initial capital	0.030 (0.022)		0.092*** (0.021)		
Capital growth		0.148** (0.053)		0.307*** (0.053)	0.030** (0.011)
Observations	1,824	1,824	1,824	1,824	1,416
<i>Control variables</i>					
Baseline	✓	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓	✓
Privatizer	✓	✓	✓	✓	✓
Other preferences	✓	✓	✓	✓	✓
IMR	✓	✓	✓	✓	✓

Notes: The table shows 2S2SLS regression results of measures of productivity growth on binding contracts. Columns (1) to (4) are based on SOESTRA data, with TFP growth Columns (1) and (2) and model-consistent productivity growth in Columns (3) and (4). Column (5) uses model-consistent productivity growth based on the MUP data. Columns (1) and (3) control for log initial capital. Columns (2) and (4) control for capital growth. Column (5) controls for log final investment as a measure of capital growth. All specifications control for fully interacted THA agency and year fixed effects. Binding contracts are defined as initial firm size below the committed target level. Instrument refers to the leave-one-out measure of assigning binding contracts. Controls are as in the baseline specification. Additional controls are log initial productivity. Standard errors are two-way clustered at privatizer and THA office level. Squared brackets indicate 90% bootstrap CI clustered at the privatizer and THA office level using 2,500 replications. *p<0.1, **p<0.05, ***p<0.01.

FIGURE D.8: PATENT-LEVEL CHARACTERISTICS



Notes: The figure plots differences of patent-level characteristics between firms with binding vs. non-binding contracts. Each point represents a single regression on the binding labor commitment indicator along with 90% CI.

TABLE D.11: REGRESSION RESULTS, PRODUCTIVITY GROWTH BY CONTRACT LENGTH

	Model-implied ($\alpha = 0.8$)				TFP growth			
	OLS		2S2SLS		OLS		2S2SLS	
	Short (1)	Long (2)	Short (3)	Long (4)	Short (5)	Long (6)	Short (7)	Long (8)
Binding contract	0.077 (0.087)	0.094*** (0.028)	-0.106 (1.418)	1.049** (0.380)	0.020 (0.114)	0.098* (0.053)	0.247 (0.560)	0.857** (0.331)
Observations	242	2,012	161	1,433	396	1,685	350	1,457
<i>Set of control variables</i>								
Baseline controls	✓	✓	✓	✓	✓	✓	✓	✓
Privatizer controls	✓	✓	✓	✓	✓	✓	✓	✓
Industry controls	✓	✓	✓	✓	✓	✓	✓	✓
Leave-out other dimension			✓	✓			✓	✓
IMR			✓	✓			✓	✓

Notes: The table shows OLS and 2S2SLS regression results on measures of productivity growth on binding contracts by contract length. Short contracts are defined with a duration between the first and the last audit below or at 12 months. Long contracts have a length between the first and the last audit above 12 months. All specifications control for fully interacted THA agency and year fixed effects. Binding contracts are defined as initial firm size below the committed target level. Leave-one-out variables in other dimensions and the inverse mills ratio enter as discussed in the text. Standard errors are two-way clustered at privatizer and THA office level. Squared brackets indicate 90% bootstrap CI clustered at the privatizer and THA office level using 2,500 replications. *p<0.1, **p<0.05, ***p<0.01.

TABLE D.12: ALTERNATIVE INTERPRETATIONS: EMPLOYMENT, EXIT, TFP

	Employment growth (2SLS)			Market exit (2S2SLS)			TFP growth (2S2SLS)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Binding	1.037*** (0.255)	0.992*** (0.241)	1.041*** (0.262)	0.044* (0.023) [.004;.09]	0.043* (0.022) [.004;.08]	0.036 (0.022) [-.003;.08]	0.688** (0.256) [.15;1.13]	0.667** (0.240) [.17;1.11]	0.737** (0.261) [.21;1.17]
Renegot.	-0.102*** (0.023)			-0.001 (0.002)			-0.022 (0.025)		
Restruct.		-0.020 (0.013)			-0.001 (0.002)			-0.051 (0.037)	
<i>LA rating (ref: privatize)</i>									
Restructure			-0.034 (0.026)			0.005 (0.003)			-0.227* (0.113)
Liquidate			-0.079** (0.031)			0.016 (0.013)			-0.181 (0.154)
Rating missing			-0.027 (0.034)			0.003 (0.002)			-0.207 (0.128)
Observations	9,051	9,051	9,051	3,061	3,061	3,061	1,824	1,824	1,824
Baseline	✓	✓	✓	✓	✓	✓	✓	✓	✓
Privatizer	✓	✓	✓	✓	✓	✓	✓	✓	✓
Industry	✓	✓	✓	✓	✓	✓	✓	✓	✓
Other pref.	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The table shows IV regression results on the three main outcome variables employment growth, market exit, TFP growth. Each column within each outcome variable adds different further control variables. Columns (1), (4), and (7) control for renegotiation attempts. Columns (2), (5), and (8) control for pre-contract restructuring dynamics measured by an indicator variable equal to 1 if the contract contains redundancy agreements and 0 otherwise. Columns (3), (6), and (9) control for initial firm viability rating (reference category refers to privatization recommendation). All specifications control for fully interacted THA agency and year fixed effects. Binding contracts are defined as initial firm size below the committed target level. Leave-one-out variables in other dimensions enter as discussed in the text. Standard errors are two-way clustered at privatizer and THA office level. Squared brackets indicate 90% bootstrap CI clustered at the privatizer and THA office level using 2,500 replications. *p<0.1, **p<0.05, ***p<0.01.

TABLE D.13: ALTERNATIVE INTERPRETATIONS II: EMPLOYMENT, EXIT, TFP

	Employment growth (2SLS)		Market exit (2S2SLS)		TFP growth (2S2SLS)	
	(1)	(2)	(3)	(4)	(5)	(6)
Binding	1.028*** (0.262)	0.979*** (0.248)	0.043* (0.024)	0.043* (0.021)	0.743*** (0.240)	0.738*** (0.238)
Restruct.	0.020** (0.009)		-0.000 (0.003)		-0.051 (0.043)	
Restitution		-0.021 (0.014)		-0.008* (0.004)		-0.069 (0.052)
Legacy debt		-0.020 (0.029)		-0.002 (0.002)		-0.063 (0.072)
Sep. real est.		0.013 (0.054)		-0.001 (0.002)		-0.070 (0.132)
Environment		0.003 (0.032)		-0.001 (0.004)		-0.040 (0.043)
Observations	9,051	9,051	3,061	3,061	1,824	1,824
Baseline	✓	✓	✓	✓	✓	✓
Privatizer	✓	✓	✓	✓	✓	✓
Industry	✓	✓	✓	✓	✓	✓
Other pref.	✓	✓	✓	✓	✓	✓

Notes: The table shows IV regression results on the three main outcome variables employment growth, market exit, TFP growth. Each column within each outcome variable adds different further control variables. Columns (1), (3), and (5) control for pre-contract restructuring dynamics measured as the growth rate in revenues between the initial observation and the year before the contract. Columns (2), (4), and (6) control for contract-related variables outside the control of the privatizer such as restitution claims of the asset, legacy debt, real estate separations and environmental. All specifications control for fully interacted THA agency and year fixed effects. Binding contracts are defined as initial firm size below the committed target level. Leave-one-out variables in other dimensions enter as discussed in the text. Standard errors are two-way clustered at privatizer and THA office level. Squared brackets indicate 90% bootstrap CI clustered at the privatizer and THA office level using 2,500 replications. *p<0.1, **p<0.05, ***p<0.01.

TABLE D.14: DIFFERENT NUMBER OF CASES PER PRIVATIZER

	Employment (1)	Model-implied Prod. (2)	TFP (3)	Exit (4)
Panel A: >10 cases				
Binding contract	1.2861*** (0.324)	0.720** (0.316)	0.584** (0.273)	0.076** (0.033)
		[-.24;1.58]	[-.06;1.12]	[-.13;.33]
Observations	7,033	1,267	1,420	2,413
Average cases	24.866	24.866	24.866	24.866
F-Statistic	11.03			
Panel B: >20 cases				
Binding contract	1.2911** (0.570)	0.794 (0.491)	0.665* (0.384)	0.015 (0.016)
		[-.55;1.62]	[-.20;1.28]	[-.14;.39]
Observations	4,887	878	974	1,708
Average cases	36.842	36.842	36.842	36.842
F-Statistic	3.433			

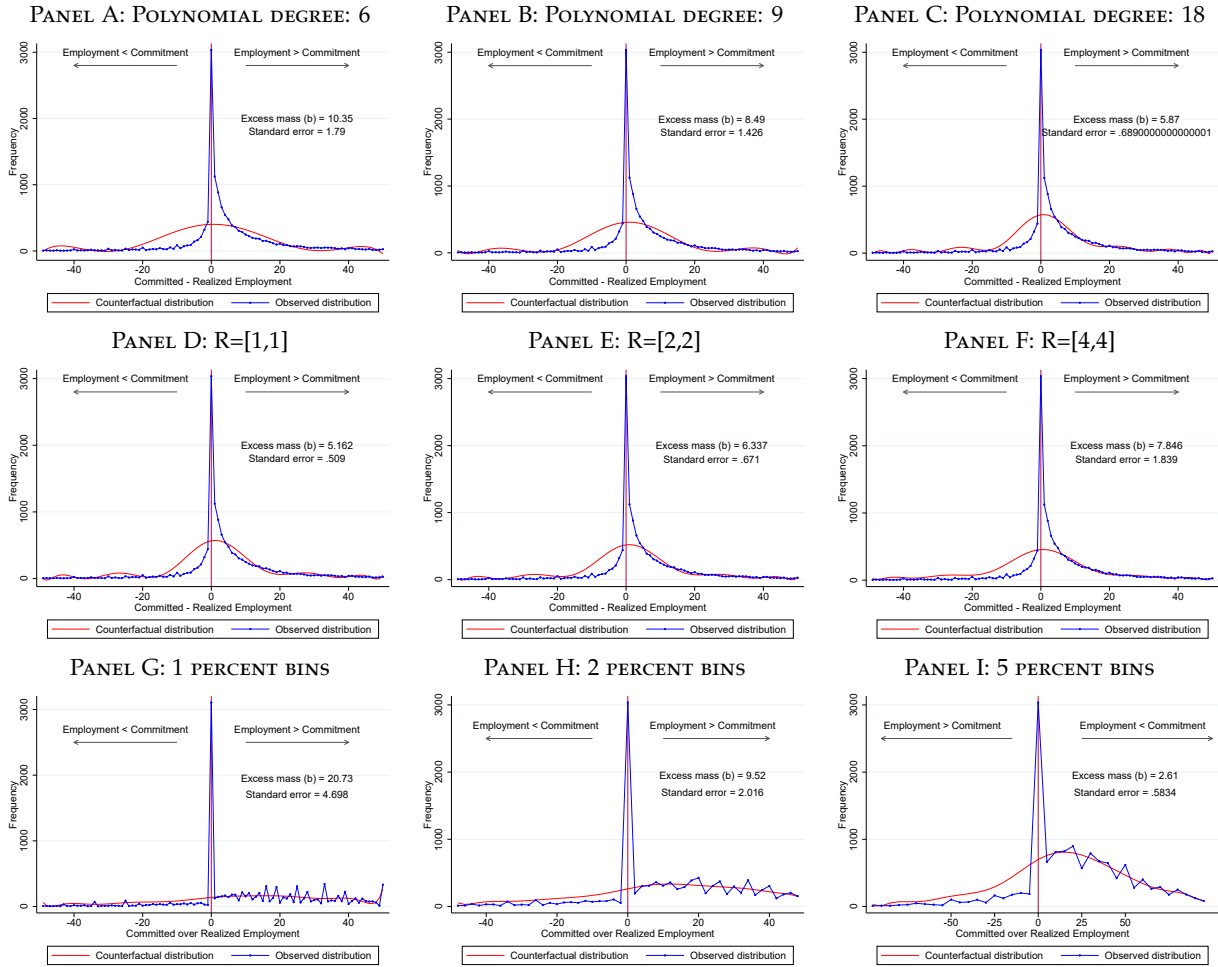
Notes: The table shows IV in Column (1) and 2S2SLS in Columns (2)-(4) regression results of the main outcome variables for different number of cases per privatizer. All specifications control for fully interacted THA agency and year fixed effects. Binding contracts are defined as initial firm size below the committed target level. Instrument refers to the leave-one-out measure of assigning binding contracts. Controls are as in the baseline specification including privatizer preferences in other dimensions. Standard errors are two-way clustered at privatizer and THA office level. Squared brackets indicate 90% bootstrap CI clustered at the privatizer and THA office level using 2,500 replications. *p<0.1, **p<0.05, ***p<0.01.

TABLE D.15: LEAVE-ONE-OUT PRICE DIMENSION AS DUMMIES

	Employment (1)	Model-implied Prod. (2)	TFP (3)	Exit (4)
Binding contract	1.1040*** (0.308)	0.797** (0.361)	0.707*** (0.248)	0.041* (0.022)
		[.02;1.49]	[.21;1.15]	[.003;.08]
Leave-out "high" price	-0.008 (0.064)	0.000 (0.000)	-0.038 (0.185)	0.003 (0.007)
Leave-out "negative" price	-0.053 (0.081)	-0.173 (0.151)	-0.151 (0.185)	0.006 (0.012)
Observations	9,051	1,623	1,824	3,061

Notes: The table shows IV in Column (1) and 2S2SLS in Columns (2)-(4) regression results of the main outcome variables. All specifications control for fully interacted THA agency and year fixed effects. Binding contracts are defined as initial firm size below the committed target level. Instrument refers to the leave-one-out measure of assigning binding contracts. Controls are as in the baseline specification including privatizer preferences in other dimensions. Price dimension is specified as high prices relative to the median price level and low prices defined as below 100 DM. Standard errors are two-way clustered at privatizer and THA office level. Squared brackets indicate 90% bootstrap CI clustered at the privatizer and THA office level using 2,500 replications. *p<0.1, **p<0.05, ***p<0.01.

FIGURE D.9: BUNCHING WITH DIFFERENT POLYNOMIALS, WINDOWS, AND BIN DEFINITIONS



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 polynomial fitted to the empirical distribution. Standard error is calculated using a parametric bootstrap procedure. Estimation based on [Chetty et al. \(2011\)](#). Panels A to C show the results using a different polynomial orders. Panel D to F show the results excluding -1 and 1, -2 and 2, and -4 and 4, respectively. Panels G to I show the results by constructing different percentage bin deviations.

E Online Supplementary Data Addendum

Committing to Grow: Employment Targets and Firm Dynamics

– Not for Publication Unless Requested –

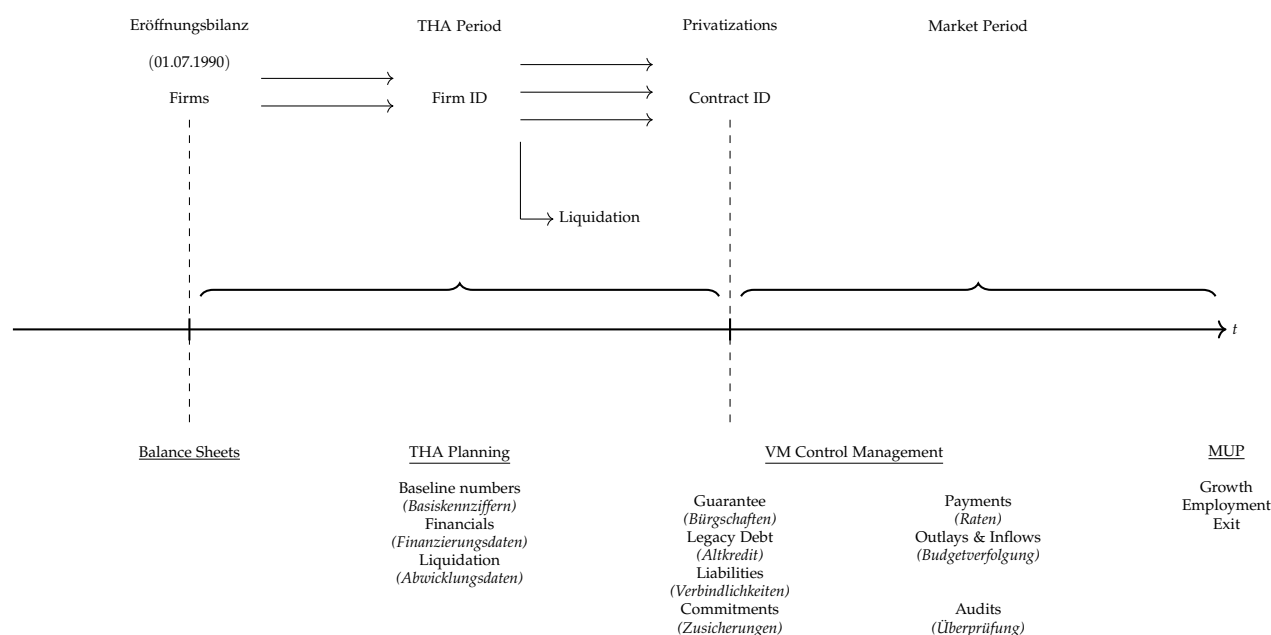
Ufuk Akcigit, Harun Alp, André Diegmann, and Nicolas Serrano-Velarde

Data Addendum - ISUD Data Environment

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

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

FIGURE E.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 1990.⁴⁶ The information relates to employment (including a breakdown into production workers, HR, and administration),

⁴⁶The information can be combined to construct a yearly panel with information at the firm level between 1989 and 1994. This dataset cannot be used to study the evolution of firms over time because the firm disappears from the dataset once the firm transitions out of the THA portfolio either because of a privatization or liquidation.

revenues (including a breakdown of revenues in East and West Europe), and the assignment of firms to THA offices (headquarters or local subsidiary). The data contains a total of 13,552 legal firm entities, out of which 93.3% are observed for the first time in 1990.⁴⁷ We complement the data with additional industry information from the SOESTRA survey (see [Mergele et al. 2025](#)). The final data set is used in the analysis to study random assignment of firms to privatizers in Appendix Table D.2, to calculate productivity growth between 1990 and the final commitment year in Table 4 and market exit effects in Table 3.

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

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

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

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

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

⁴⁷THA 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 E.1: CONTRACT-LEVEL DATA TABLES

Table names	Description
<i>A: Baseline tables</i>	
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.
<i>B: Labor Commitments & Audits</i>	
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.
<i>C: Investment Commitments & Audits</i>	
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.

information on audits (see Panel B of Data Appendix Table E.1). These two tables can be seen as the original tables as suggested from the delivered pdf documentation by the German Federal Archives. The pdf file for labor commitments is shown in Data Appendix Figure E.2. It shows the template how the data was collected in the first place by THA employees. The top right corner corresponds to

the tables **VAPST** and **VAPIT**, respectively. In these two data tables we observe 17,753 unique contracts with labor commitments and 16,583 contracts with at least one audit. As presented in Panel B, the total number of observations in both tables is higher because there can be multiple commitments for different years of the commitment period as well as several audits per commitment.

FIGURE E.2: PAPER FILE: LABOR COMMITMENT

VAPIT VAPST
Vertragsabwickl. Seite 20

VAE1M11 Arbeitsplatzzusagen Vertragspartner

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

2

3 Anzahl

4 (SOLL) bis Wiedervorlage

5 AP SANZ ⁽³⁶⁰⁴⁾ AP SDAT ⁽³⁶⁰²⁾ _____ ⁽³⁶⁰⁹⁾

6 Vorjahr: AP SANZV AP SDATV _____ _____

7 _____

8

9 Vgl. § ⁽³⁶⁰³⁾ APMGRS des Vertrages

10 ⁽³⁶⁰⁷⁾ Konventionalstrafe (J/N)

1 AP KONV

2

3 Abschließend geprüft /

4 erledigt: AP PRDAT ⁽³⁶⁰⁶⁾

5

6 Bemerkungen AP TXT ⁽³⁶⁰⁵⁾

7 _____

8

00114440050500

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

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

The ISUD environment further contains a table called **ASVA12T** with labor commitment contracts. The original table has 322,829 observations. The majority of these observations are labeled as having no labor commitments. We compare this data table with the original **VAPST** table. Conditional on observing one contract ID in both tables (**VAPST** and **ASVA12T**) shows that the information is identical. However, **ASVA12T** has 5,125 additional contracts with labor commitments that are not included in **VAPST**. These additional contracts are, on average, later written out and are entered into the ISUD data system mainly in 2003 and 2004. After following the same data cleaning steps, we end up with 3,385 additional contracts. In terms of labor audits, however, these contracts are not observed in **VAPIT**. There exists another data table that is a natural suspect and is called **ASVA13T**. But again, this table does not contain audit information for the additional contracts with observed labor commitments.⁴⁸ After searching for possible contracts with additional audit information, we found that the history version of **VAPIT**, called **VAPITH**, is suitable to fill parts of the missing audits from **ASVA12T**. Among the 3,385 additional contracts after basic data cleaning steps, we are able to merge the audit information for 2,702 contracts. Together, these data tables generate our final sample of 18,235 contracts with labor commitments.

For the empirical specifications accounting for privatizer preferences in other dimensions, we make further use of investment commitment contracts. The logic and steps in the data cleaning process apply similarly to investment commitment contracts. Figure E.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 Data Appendix Table E.1 provides a list and short description of the investment commitment related data tables.

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

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

⁴⁸Out of the 5,125 additional contracts with labor commitments **ASVA12T**, 17 contracts are found in **VAPIT** and 22 contracts are found in **ASVA13T**.

FIGURE E.3: PAPER FILE: INVESTMENT COMMITMENT

VA ≥ ST
VA ≥ IT

Treuhandanstalt Vertragsabwicklung
Seite 1

VAEZM11 Investitionszusagen Vertragspartner (25 ¹⁵
21 ¹⁶)

1 System-Nr.: _____ TV-Nr.: _____ Lfd.Nr. Zusage/Soll: _____ Vertrags-Nr.: _____
1st Nummer des Vertrages

2

3 Betrag in DM (SOLL) bis Wiedervorlage

4 IVUPBET (3503) IVUPDAT (3503) (3505)

5 _____

6 _____

7

8 vgl. § IVUPG des Vertrages ZISTBET ZISTDAT ZISTMELD
(3504) (3524) (3525) (3526)

9

10 IVKONV Konventionalstrafe (J/N)
(3506)

1 Abschließend geprüft/erledigt: IVPROAT (3518)

2

3

4 Bemerkungen IVTAT (3523)

5

6

7

8

001/0000000000

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

Data Addendum - Merging Contracts to Mannheim Enterprise Panel Data

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

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

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

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

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

MUP data at the median. The 99th percentile corresponds to 51 potential matches, which are rather common names that are matched several times in the MUP data. We therefore exclude the upper decile (more than nine different IDs in the MUP data) of the matches as a further pre-selection step.

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

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

Data Appendix Table E.2 provides an overview on the selection criteria. It states that 38% of our matches are based on the exact county, date (date of incorporation and contract date) and audit information. Another 15.5% of the matches are selected based on the Federal State information, the date and audit information. This indicates that slightly more than 50% are based on region, date and employment information available in both dataset. Then, there are some few matches of around 10% that are only based on region and date or region and audit information. About a quarter of the matches are based only on the information of the contract date and the date of incorporation, whereas 2.6% are only based on audit information. Finally, 8.1% of the selected MUP firms are selected because there is only one possible match, i.e., the matched owner has only one firm ID attached.

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

Based on the firm-year observation, we are able to merge employment audits from the contract management system of the ISUD environment. Note that in the selection procedure, we have used the match with the smallest deviation. We will now be able to justify the match by studying employment number differences between the two datasets. For 3,609 firms, we observe at least one audit (with positive employment information) which allows us to calculate employment differences. The

TABLE E.2: SOURCES OF SELECTED MATCHES

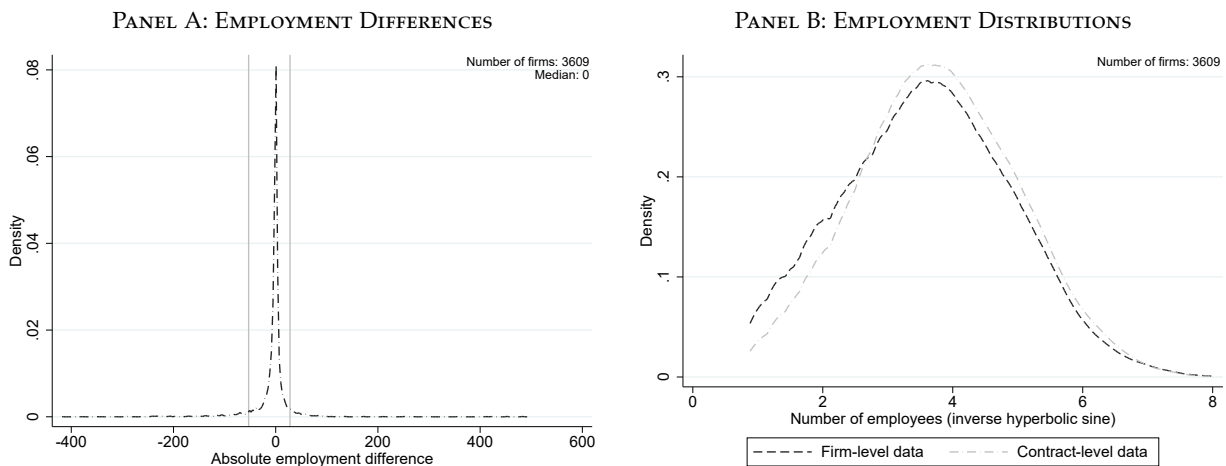
	Share
Selected based on county, date & audit information	0.378
Selected based on state, date & audit information	0.155
Selected based on county & date	0.016
Selected based on state & date	0.009
Selected based on county & audit information	0.061
Selected based on state & audit information	0.024
Selected 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.

median (mean) estimated difference in employment is 0 (2.67). However, we observe large tails in the distribution of employment differences. For this reason, we further drop matches with absolute employment differences above 500. After this adjustment, our sample consists of 4,735 firms. At this stage, we do not drop firms if precise calculations of employment differences are not possible, which means that we rely on date and regional information for the merge.

Data Appendix Figure E.4 provides a comparison between the contract-level employment information and the MUP data. Panel A provides a visualization of count differences with a median of zero (maximum of 500 by construction). Panel B shows the inverse hyperbolic sine transformed employment numbers between the MUP firm-level data in black and the contract-level data in grey. These results suggests that the MUP firm-level data shows slightly more mass among smaller firms.

FIGURE E.4: COMPARISON OF EMPLOYMENT FIGURES BETWEEN CONTRACTS AND MUP



Notes: Panel A shows employment differences between matched contracts and firms in the MUP data that is centered around 0. Panel B shows the log employment distribution of matched contracts and the employment distribution in the MUP dataset. 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:

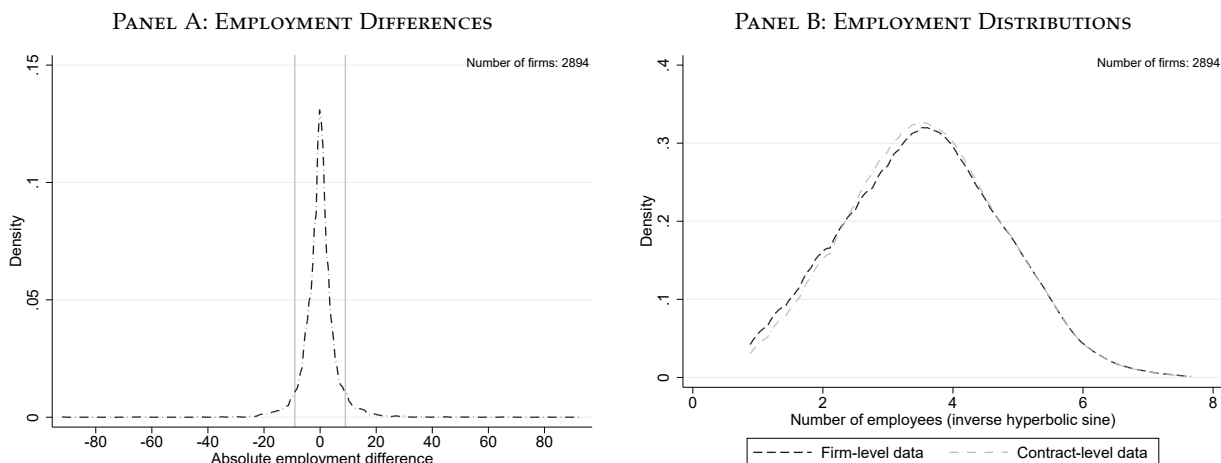
$$employment_{diff} = \frac{(empl_{MUP} - empl_{ISUD})}{(empl_{MUP} + empl_{ISUD})}$$

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 E.5 shows the same distributions among firms that are considered to be close i.e., have employment differences below the defined threshold value. At the firm level, 2,894 out of 3,609 firms are below the defined threshold value, which corresponds

FIGURE E.5: CLOSE MATCHES BETWEEN CONTRACTS AND MUP

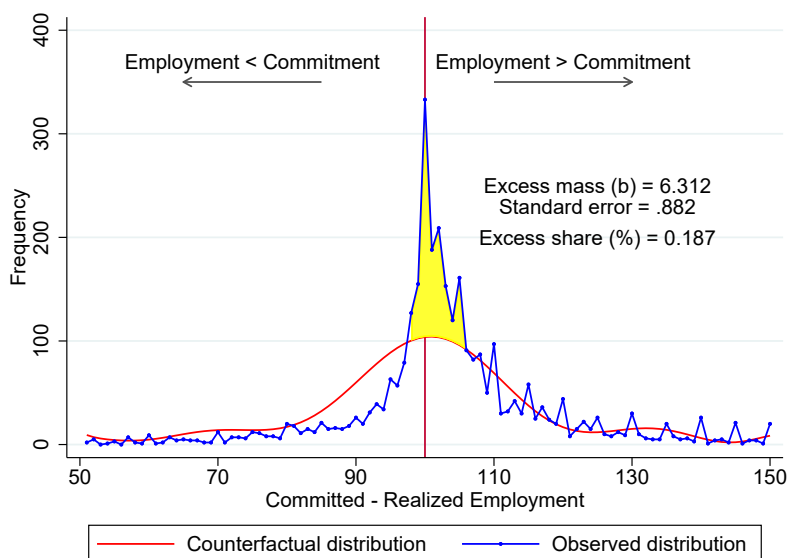


Notes: Panel A shows employment differences between matched contracts and firms in the MUP data that is centered around 0. Panel B shows the log employment distribution of matched contracts and the employment distribution in the MUP dataset. The sample is conditional on fulfilling the threshold rule.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

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

Based on this sample, we can re-calculate the employment distribution around the commitment level as shown in Figure 4. Data Appendix Figure E.6 shows the bunching estimate using the employment information in the MUP for the year of the final commitment. We adjust the bunching window slightly by excluding the area of 6 and less above the committed level as well as 1 and 2 employment below the committed level.

FIGURE E.6: EMPLOYMENT DISTRIBUTION AROUND THE COMMITMENT LEVEL USING FIRM-LEVEL DATA



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

Similar to the baseline bunching estimates, bunching with the matched sample between the contracts and the MUP is estimated to be 6.312. The estimated standard error is 0.88, indicating a significance level of 1%. This value is, furthermore, relatively close to 6.52 presented in Figure 4. Overall, these results suggest that the merge between the two datasets can be considered highly reliable.

Data Addendum - Treuhand Firm Survey Data

This section describes how we construct firm-level capital stock and TFP estimates and the merge between our baseline contract-level data and the THA firm survey data. The bi-annual survey was conducted by the SOESTRA institute with its first wave in April 1991. The survey data has been used and analyzed, among others, by [Wahse et al. \(1996\)](#) and [Mergel et al. \(2025\)](#).

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

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

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

In order to calculate the yearly capital stock at the firm level, we start with the initial capital stock measured in 1990, add investments, and assume a 10% depreciation rate. All Deutsch Mark (DM) values are deflated by the CPI measured in 2016 prices. The capital stock can only be estimated if investment information is available. Data Appendix Table E.3 shows in Column (4) that the question

on investment primarily exists for the years between 1992 and 1995. Coverage is particularly low towards the end of the sample period and in 1991. For example, there are only 560 firms with full investment information between 1991 and 1994, and only 160 always have investment numbers. Likewise, but to a lower extent, Column (3) shows the number of firms with revenue information. In the first two years, around 98% of all firms do have information on revenue, whereas this share decreases afterwards.

TABLE E.3: ACTUAL AND IMPUTED INVESTMENT INFORMATION

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

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

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

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

Data Appendix Figure E.7 shows actual vs predicted investment numbers pooled over the whole time period. On average, actual and predicted numbers line up at the 45 degree line. Based on these

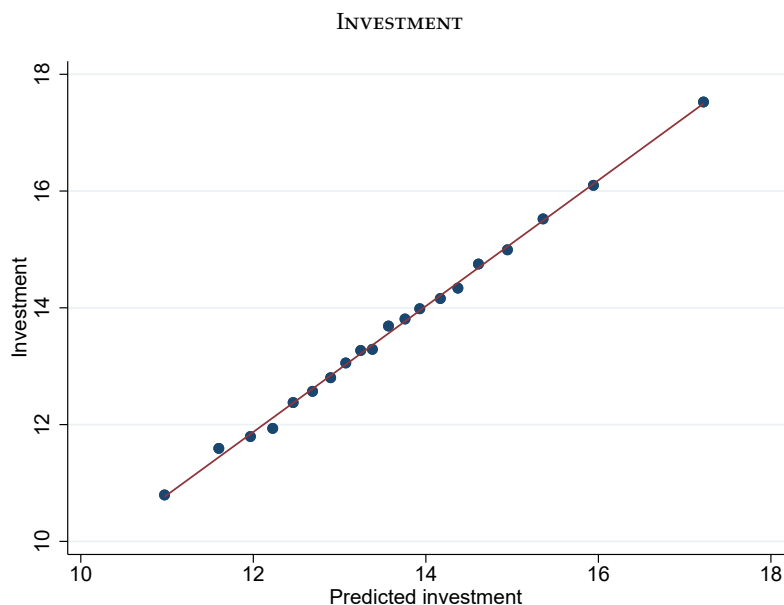
TABLE E.4: LASSO RESULTS: LN(INVESTMENT)

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

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

predictions, we impute investment information in case actual investment information is missing and the selected covariates are not missing. Column (4) of Table E.3 shows the number of imputed observations.

FIGURE E.7: CORRELATION ACTUAL AND PREDICTED VALUES

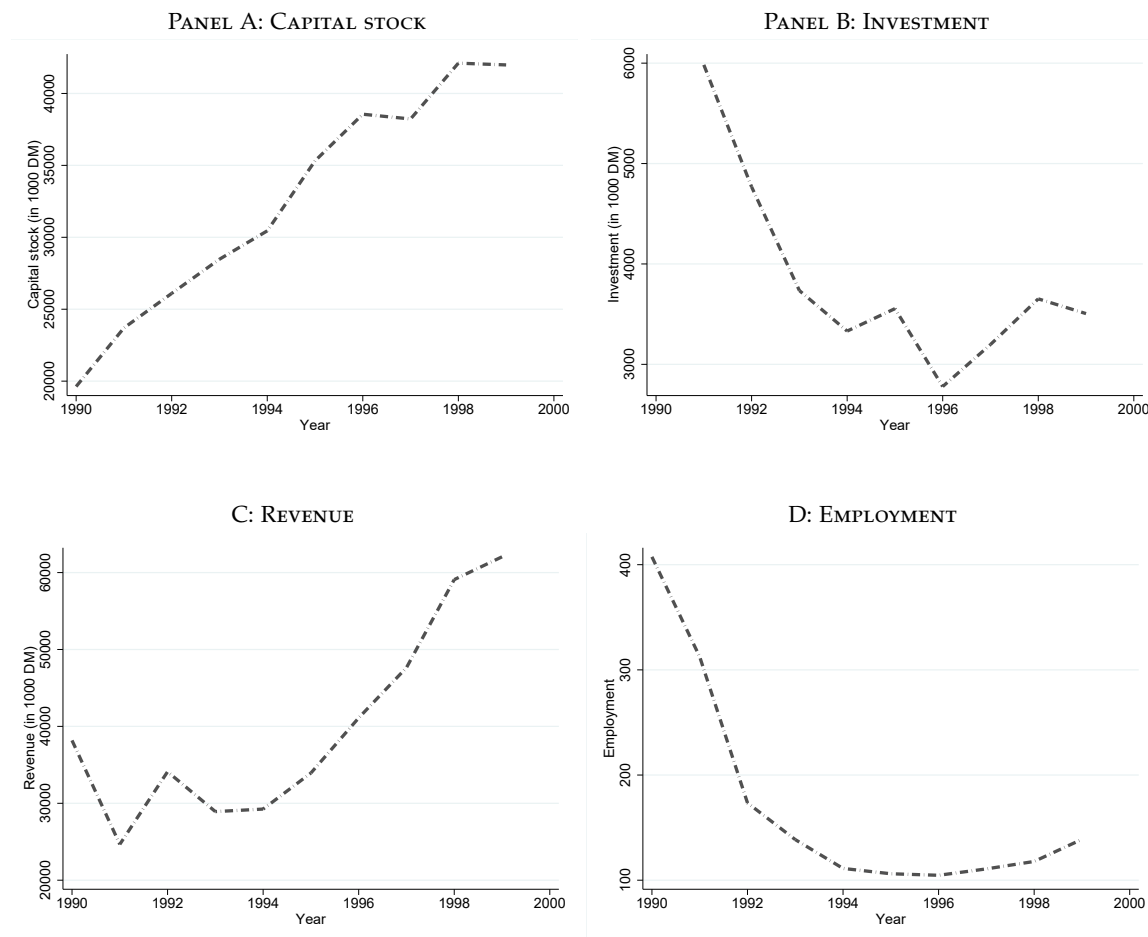


Notes: The figure plots actual vs. predicted investment numbers pooling all years between 1991 and 1999 with the cross-validated lambda.

In a next step, we construct the capital stock at the firm level starting with the initial capital stock in 1990 and add these (actual and imputed) investment numbers and subtract a 10% depreciation rate. Figure E.8 provide firm-level averages over the period between 1990 and 1999. Although these numbers might not be representative for the East Germany economy due to selectivity and panel attrition, the panels A and C of the figure show an increasing trend in the constructed capital stock

measure and firm revenue. Average investment amounts decrease over time, indicating a disproportional high investment need. Average firm-level employment decreases over time. The drop in firm level employment is consistent with total employment in the economy, with the largest decrease happening between 1990 and 1991.

FIGURE E.8: MAIN VARIABLES USED FROM SOEASTRA FIRM SURVEY



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

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

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

where y_i is the logarithm of the firm's output, in our case, revenue. l_i and k_i are the logarithm of

the firm inputs, in our case, the number of employees and the capital stock. We construct TFP as $\omega_i = \exp(y_i - \hat{\beta}_l l_i - \hat{\beta}_k k_i)$. Table E.5 provides the regression results separately for each year between 1991 and 1999. In Panel A, we provide the results using the baseline imputation approach of firm-

TABLE E.5: REGRESSION RESULTS: LN(REVENUE)

	1991	1992	1993	1994	1995	1996	1997	1998	1999
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: baseline, covariates: employment & revenue dummies									
Ln(Empl.)	0.4899*** (0.020)	0.6679*** (0.015)	0.6937*** (0.013)	0.6482*** (0.012)	0.6683*** (0.014)	0.5895*** (0.019)	0.6070*** (0.019)	0.6887*** (0.023)	0.7290*** (0.025)
Ln(Capital)	0.4582*** (0.018)	0.3497*** (0.015)	0.3454*** (0.014)	0.3859*** (0.014)	0.3654*** (0.014)	0.4019*** (0.018)	0.4407*** (0.019)	0.4093*** (0.021)	0.4002*** (0.023)
N	6,448	6,449	5,823	5,251	3,847	2,677	2,296	1,771	1,502
R ²	0.560	0.538	0.577	0.583	0.623	0.597	0.634	0.672	0.699
Panel B: covariates: ln(employment) & ln(revenue)									
Ln(Empl.)	0.4413*** (0.020)	0.6505*** (0.016)	0.6832*** (0.013)	0.6378*** (0.012)	0.6622*** (0.014)	0.5794*** (0.019)	0.5977*** (0.020)	0.6796*** (0.023)	0.7251*** (0.026)
Ln(Capital)	0.4927*** (0.018)	0.3613*** (0.015)	0.3502*** (0.014)	0.3897*** (0.013)	0.3634*** (0.014)	0.3988*** (0.018)	0.4347*** (0.018)	0.4056*** (0.021)	0.3927*** (0.022)
N	6,448	6,449	5,823	5,251	3,847	2,677	2,296	1,771	1,503
R ²	0.566	0.542	0.580	0.587	0.625	0.599	0.635	0.673	0.699
Panel C: covariates: ln(employment) & ln(revenue) with second polynomial order									
Ln(Empl.)	0.4293*** (0.020)	0.6509*** (0.016)	0.6843*** (0.013)	0.6383*** (0.012)	0.6623*** (0.014)	0.5805*** (0.019)	0.5996*** (0.019)	0.6763*** (0.023)	0.7214*** (0.026)
Ln(Capital)	0.5007*** (0.018)	0.3603*** (0.015)	0.3499*** (0.014)	0.3909*** (0.013)	0.3633*** (0.014)	0.3947*** (0.018)	0.4300*** (0.018)	0.4092*** (0.021)	0.3984*** (0.023)
N	6,439	6,440	5,816	5,244	3,840	2,669	2,290	1,766	1,499
R ²	0.563	0.538	0.577	0.584	0.621	0.592	0.629	0.671	0.699
Panel D: baseline with proportional initial capital stock									
Ln(Empl.)	0.3012*** (0.022)	0.5456*** (0.020)	0.6002*** (0.017)	0.5460*** (0.015)	0.5764*** (0.017)	0.4876*** (0.024)	0.4872*** (0.024)	0.5961*** (0.029)	0.6467*** (0.033)
Ln(Capital)	0.6537*** (0.020)	0.4454*** (0.021)	0.4109*** (0.018)	0.4728*** (0.018)	0.4569*** (0.018)	0.4836*** (0.023)	0.5351*** (0.024)	0.4759*** (0.027)	0.4508*** (0.029)
N	4,279	4,280	4,004	3,683	2,748	1,919	1,649	1,283	1,063
R ²	0.600	0.528	0.569	0.572	0.622	0.579	0.618	0.653	0.671
Mean revenue	15.33	15.474	15.434	15.55	15.66	15.744	15.684	15.602	15.556
Mean employment	4.636	3.966	3.676	3.484	3.394	3.338	3.436	3.526	3.714
Mean capital	15.674	15.748	15.81	15.878	15.95	15.948	15.922	15.906	15.85

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

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

Panels B and C provide the estimation results for the different lasso specifications. Panel D provides the results with the baseline imputation procedure using the proportionality approximation of the initial capital stock. While Panels B and C show rather similar results, Panel D shows that β_k is higher by a magnitude of around 0.1, whereas β_l is lower by about the same magnitude. The reason

might be that the imputed investment numbers are relatively large, relative to the approximated initial capital stock, which increases the elasticity of capital in the production function estimation.

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

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

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

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

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

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

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

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

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

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