

# RESTRUCTURING OF STATE-OWNED ENTERPRISES AND WORKERS: EVIDENCE FROM EAST GERMANY

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## Abstract

I study the impact on workers of restructuring communist firms during the transition to capitalism. Drawing on close to the universe of communist state-owned enterprises under the authority of the German privatization agency in the 1990ies, I am able to link firms across industries and districts to workers and their labor market outcomes before and after exposure to firm restructuring. I study effects of two treatments: First, I find that the advent of market competition through firm closures by the privatization agency increases unemployment. Second, I estimate that the genesis of private ownership in the form of privatizations lowers household income and well-being of workers. To explore the role of ownership, I conduct event studies at the firm level and find that privatized firms downsize compared to firms remaining state-owned. Building a conceptual framework on the idea that the transition implied large temporary uncertainties, the shrinking of privatized firms might have followed from a higher desire by private owners to reduce exposure to risk as opposed to the state.

**Keywords:** Privatization, Firm Closure, Liquidation, State-Owned Enterprise, Unemployment, Life, Satisfaction, Democratization, Communism, Socialism

**JEL codes:** E24, I31, J23, L33, P31, P16

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# 1 Introduction

State ownership is seen by many economists as one of the most direct interventions in the economy. Governments around the world have in particular resorted to state ownership in times of crisis and during large transformations. Many economies during the great recession saw several bailouts in banks and non-financial firms in the form of direct state control, airline carriers or carmakers were taken over during the Covid-19 pandemic while some countries consider state equity in key industries that can support the green transition (OECD, 2020; EURACTIV, 2022). Albeit the pervasiveness of state ownership during times of upheaval, we still lack a fundamental understanding of whether it can achieve the desired goals set by policy makers.

In this paper, I contribute to filling this gap in our knowledge by studying the impact of East Germany's mass privatization program on workers. East Germany stands out from the transition countries as it adopted West Germany's institutions and ceased to exist as a country with the German reunification. The West German model based on principles of liberal democracy, market competition and the dominance of private ownership over firms was essentially extended to the East. To facilitate the transfer of corporate governance and management practices, the newly founded privatization agency was headed by West German top managers. Moreover, public expenditures in the new East German federal states depended on the West German majority in reunified Germany's political institutions. As a consequence, East German interest groups had arguably less influence on the scope of economic reform as opposed to other transition countries. East Germany is therefore uniquely suited to identify effects from the reorganization of state-ownership on workers.

I make use of nearly the universe of East Germany's former communist firms under the authority of the privatization agency, covering close to all of medium to large enterprises of East Germany's state-owned economy. I construct two measures of exposure of local labor markets to the reorganization of firms undertaken by Germany's privatization agency: First, the number of firms that were liquidated. Second, the number of firms that were privatized. I define local labor markets as two digit industries within districts and match both treatments to individuals working in these local labor markets. To identify the effects of firm closures and privatizations on workers, I pursue a difference-in-differences strategy using fixed-effects exploiting variation within year and local labor market. I employ a difference-in-differences estimator developed by Dube et al. (2022) using local projections to estimate effects robust to treatment heterogeneity. Treated workers are exposed to either privatizations, liquidations or both. Control workers are not affected in their local labor market as they either work in firms continuing in state-ownership, in self-employment, small formerly informal firms, newly founded firms, in public institutions or nonprofit organizations. I find that liquidations and privatizations increase unemployment over the period from 1990 until 2006. Moreover, privatizations depress household incomes and lead to lower self-reported health and satisfaction with life. To understand the effects of privatizations within local labor markets on privatized firms, I construct balanced panels around the year of privatization of firms. Matching privatized firms to firms remaining in state-ownership on observables, I conduct event studies with staggered adoption of the privatization treatment. I find that privatized firms reduce their workforce and incur lower revenues. The results hold across all firms and only firms inside public utility industries. Results are robust to heterogeneity in effects over time and treatment groups. Using total

factor productivity as a simple measure of productivity, I do not find any effect of privatization within an event window of 7 years. To rationalize these findings, I use a stylized bargaining model that suggests two explanations why privatized firms downsize: First, constraints on the budget to cover labor costs are harder for privatized firms. Second, when private owners are assumed to be more risk averse than the state in the context of political and macro-economic uncertainties of transition, they would find it optimal to reduce firm size. Taken together, these findings suggest that even with the entry of new firms, self-employment and migration opportunities, adverse effects from the restructuring of former communist firms on affected workers persist over close to a decade. In ongoing work, I estimate the marginal value of public funds from restructuring, comparing the costs of affected workers to the benefits of tax payers based on the costs of continued state-ownership.

This study contributes to four broad strands in the literature. First, it informs our understanding of the impact of democratization on economic welfare. Using a binary variable of democratization constructed by [Papaioannou and Siourounis \(2008\)](#), [Acemoglu et al. \(2019\)](#) estimate the effect of democratization on economic growth in a large panel of countries. Since authoritarian countries experience a decline in growth before democratization, the authors employ dynamic lag models, propensity score re-weighting and regional waves of democratization as instruments. The positive effect seems to be driven by investment into capital, schooling and health. [Dorsch and Maarek \(2019\)](#) study how democratization impacts distribution. They argue that the political coalition in power in autocracies is narrower than in democracies such that distributional outcomes are more extreme as well. They show that formerly egalitarian autocracies such as the transition countries become more unequal and unequal autocracies become more egalitarian after their transition to democracy. East Germany is not included in the samples of these cross-country studies due to the reunification, but regional development in Germany's new federal states fits these patterns in the medium run. Inequality increased in East Germany and even though output per capita most likely dipped with the reunification, East German GDP per capita recovered fast ([Krueger and Pischke, 1992](#); [Carlin, 2010](#)). [Giuliano et al. \(2013\)](#) show that privatization is a popular economic reform during democratization. The fact that East Germany adopted a pre-determined model of institutions, permits this paper to conduct a clean test of how reorganization of former state-owned enterprises impacts welfare during democratization.

Second, I contribute to the vast economic literature on communism and transition. The fall of communism transformed the lives of a third of the world's population. The ensuing transition period brought about a drop in output, income and employment in Eastern Europe and the former Soviet Union akin to the great depression in Western Europe and the US ([Milanovic, 1998](#)). The hardship experienced across the transition countries puzzled many observers: The arrival of market competition was thought to lift inefficient distortions ([Blanchard, 1997](#)). Private ownership of firms was meant to increase productivity ([Nellis and Shirley, 1992](#)). In short, capitalism was thought to bring about prosperity in the near-term. Measured against the severity of this economic downturn, empirical evidence from microdata on how the reorganization of production impacted welfare seems scarce. [Milanovic and Ersado \(2012\)](#) make use of household surveys from 26 post-communist countries over the period 1990-2005 to regress shares of income deciles on EBRD economic reform indices using country fixed effects. The authors find that privatization of small firms tends to be pro-poor, whereas privatization of public utilities seems to be negatively cor-

related with the income share of bottom deciles. The increase in unemployment from the sale and closure of former state-owned enterprises likely contributed to growing inequality in East Germany. Orbán (2019) revisits theories around the output dynamics during the transition to capitalism (see e.g. (Blanchard, 1997; Blanchard and Kremer, 1997; Roland and Verdier, 1999; Roland, 2000; Castanheira and Roland, 2000)) and decomposes output with firm-level data from 1986-1999 in Hungary. The author finds that a reduction in labor input drove most of the initial output drop and within-industry reallocation of capital and labor the recovery. My firm-level event studies show that former state-owned enterprises downsize after their sale to private owners, suggesting that privatization could be a partial explanation for why output dropped across the transition countries.

Third, this paper contributes to the literature of effects of privatizations on workers. The main objective of theoretical and empirical work has sought to understand efficiency and performance differences between corporate ownership structures. In influential theoretical arguments, privatization might increase efficiency due to delegation of authority (Sappington and Stiglitz, 1987), less political influence over employment decisions (Boycko et al., 1996), due to incomplete contracts (Hart et al., 1997) or hardening of budget constraints (Berglof and Roland, 1998). In a survey, Megginson and Netter (2001) conclude that "research now supports the proposition that privately owned firms are more efficient". Iwasaki and Mizobata (2018) conduct a meta analysis of privatizations in Central and Eastern Europe and the former Soviet Union. Private compared with state-owned domestic owners seem to increase firm performance on average across studies. I address this question in the context of East Germany, employing recent advances in difference-in-difference estimators. As Mergele et al. (2020) show for the German privatization program, more productive firms were likely to be privatized earlier. I therefore use a matched control group of firms remaining in state ownership as a control. In contrast to most of the literature, I am not able to reject zero effects on total factor productivity. Given that East Germany adopted West Germany's institutions, the quality of institutions cannot explain this finding as put forward in the literature (e.g. (Borghesi et al., 2016)) A potential explanation could be Demougin and Sinn (1994), who argue that a higher aversion to risk on the side of private investors compared to the government might undermine investment into restructuring. The papers closest to mine are by Arnold (2019) and Olsson and Tåg (2021) on effects of privatization on workers in Brazil and Sweden respectively using rich employer-employee data. Arnold (2019) finds large wage effects decreasing the formal sector wage by 3.1% due to large indirect effects. Olsson and Tåg (2021) estimate negative effects on employment and wages due to worker churn, but conclude that productivity gains compensate for lost wage incomes. East Germany differs from these case studies due its departure from a fully state-owned and centrally planned economy and sheer scale of the privatization program. Nevertheless, I also find persistent negative effects on employment of affected workers. Unaddressed in the literature is the fact that affected workers might benefit from privatizations if they create consumption gains due to price reductions or quality improvements. The estimated effects in this study could be interpreted in the way that if positive consumption gains exist, they are outweighed by negative components of life satisfaction such as unemployment.

Fourth and lastly, I contribute to the literature on the German reunification. Germany seems to contradict institutional theories of economic development as the East's adoption of West Germany's basic law approved under the auspices of allied forces in 1949, failed

to lead to convergence between both regions until now (see e.g. [Bachmann et al. \(2021\)](#) for descriptive evidence on non-convergence). This paper points to a potential explanation by documenting the labor market scarring from initial restructuring policies. Empirical studies on East Germany based on the the same individual data used in this paper show that absolute and relative income are strong determinants of life satisfaction ([Frijters et al., 2004](#); [Easterlin and Plagnol, 2008](#)). In line with these studies I estimate privatization effects on income and life satisfaction. Drawing on matched employer-employee data starting in 1992, [Findeisen et al. \(2021\)](#) show that most East German workers relocated for new jobs within the East at high rates, in particular older workers. Decomposing the sharp wage growth in the East, they estimate that a third of the positive East-West difference in wage growth can be explained by reallocation. Relative to this work, I study multiple labor market outcomes of workers.

This paper is structured as follows. Section 2 discusses the background of East Germany's privatization program. Section 3 introduces data of East German SOEs. Section 4 shows results on restructuring effects on workers. Section 5 discusses results of firm-level event studies of privatization. Section 6 rationalizes the findings with a bargaining model of workers and private vs state-owned firms. Section 7 concludes.

## 2 Background on East Germany's transformation

### 2.1 Firms, work and well-being in East Germany

Several waves of expropriations paved East Germany's way into a centrally-planned state-owned economy. The first wave was led by Soviet occupying forces that expropriated 40% of firms in manufacturing by 1948 ([Fetscher, 2000](#)). With the foundation of the German Democratic Republic in 1949, the transformation of privately owned firms into state-owned combines continued. The second wave was decided by the politburo in 1972 and mandated the full takeover of the remaining privately owned firms in forced sales to integrated them into the central planning system ([Fetscher, 2000](#)). In 1987 there were 126 combines (*Kombinate*) led by the central government and 93 combines under the planning of one of 14 East German provincial governments ([Jahrbuch, 1988](#)). The central authorities gathered information from combines and set prices of input and output goods centrally. Enterprises were set output goals by plans and were awarded bonuses when they fulfilled them. [Steiner \(2020\)](#) describes how firms often concealed their production capacities and hoarded inputs to react to changes in centrally set production quotas.

The GDR's constitution granted the right to work to every citizen with free choice of their place of work and equal pay for the same work. Employees could therefore not be fired, which meant that firms were not able to forcefully adjust their labor force ([Zuhlke and Goedicke, 2000](#)). Compared to West Germany, there were no labor market institutions such as employment centers for job seekers or unemployment insurance schemes. Using a retrospective survey of 2,323 workers, [Zuhlke and Goedicke \(2000\)](#) estimate that only 50% of workers entering the labor market in 1960 switched jobs between firms in their first ten years compared to 65% of West German men and 75% of West German women. A particularity of many work places was that certain public goods such as child care or gyms were integrated into them. The wage distribution was relatively compressed in East

Germany and never officially published. [Stephan and Wiedemann \(1990\)](#) use 1988 data collected by the East German ministries and find that tertiary educated workers earned only 15% more than workers with a lower level of education compared to 70% in the West. The authors describe that the wage gap unconditional on task and education between women and men was at around 16% in the East and 30% in the West.

Food, housing and basic consumption goods were heavily subsidized in the GDR. As residential housing was provided by the government, [Mohr \(2019\)](#) demonstrates that the regime politicized housing to use it as a carrot in municipalities home to oppositional forces in the uprising of 1953. Towards the end of the regime it became increasingly clear that new construction and renovation of the stock lagged behind the needs. Concerning purchasing power, an intense debate raged around the exchange rate between Ostmark and Westmark that was effectively applied with the reunification. Even though black markets rates were much higher than 1 Ostmark for 1 Westmark that was applied to cash and 2:1 that was applied to savings, many economists and central bankers argued that the rate would need to compensate for the phase-out of subsidies ([Sinn et al., 1992](#)). There was little scope to voice dissatisfaction with the government, as elections were uncontested and the rule of the Socialist Unity Party was cemented by the constitution. Nevertheless, the regime showed a certain level of responsiveness to formal grievances that could be filed by citizens during election cycles ([Lueders, 2021](#)). The main vehicle besides the military ([Mohr, 2019](#)) to quell the opposition was the state police *Stasi* with its large network of unofficial informants ([Lichter et al., 2021](#)). From 1945-1961, one fifth of East Germany's population voted with their feet and moved to West Germany ([Becker et al., 2020](#)). The erection of the wall brought the migration down to rare successful cases of border crossings. The foundation of the Berlin wall estimates that at least 140 people were killed during attempted border crossings ([Mauer, 2022](#)).

## 2.2 The mass privatization program

The fall of the Berlin Wall on the night of the 9th of November 1989 marked the dramatic end of the German Democratic Republic (GDR) and paved the way to the reunification of West and East Germany. With the reunification, East Berlin joined West Berlin to form a single city that became Germany's capital in 1999. The five states of Brandenburg, Mecklenburg-Western Pomerania, Saxony, Saxony-Anhalt and Thuringia abolished in the GDR in 1952 were reconstituted and joined the Federal Republic of Germany as new states. With the reunification treaty, East Germany adopted West Germany's basic law (Grundgesetz) the constitutional basis of West Germany's political system.

Initiated by the transition government in 1989-1990, the new federal government in reunited Germany faced the challenge of orchestrating the transition of the centrally-planned economy of East Germany into a market economy and integrating it into a unified economy exposed to international competition. A privatization authority called the Treuhand agency was created in the Summer of 1990 to accomplish this monumental task. In its form similar to a state holding it took on the responsibility over almost all manufacturing firms and to some extent services firms. The agency oversaw initially more than 8,200 state-owned enterprises and 4.1 Mio employees ([Kühl et al., 1991](#)). About every second employee in East Germany worked at a firm in the portfolio of the Treuhand. By restructuring these industrial combines, the firm count increased even further over the years. Dubbed by the

OECD as the ‘big bang approach’, the Treuhand’s self-stated objective was to “privatize quickly, restructure resolutely, and liquidate carefully” ([Bundesanstalt für vereinigungsbedingte Sonderaufgaben, 1994](#)). Privatizations were conducted mostly through direct sales and bids were evaluated by sales price, guarantees for maintaining employment and investing into the business, such as management concepts. The investors who bought former state-owned enterprises were largely West German corporations ([Mergele et al., 2020](#)). Participating in these sales was not a viable strategy for most East Germans external to the firm, such that essentially the only two ways to take over firms were through restitutions in court decisions or management buy out. The agency closed its operations at the end of the year in 1994.

### 3 Data and summary statistics

This section presents the data used to estimate the effect of restructuring of communist firms on workers. I rely on unique administrative firm data of East Germany’s former communist firms during the privatization program. To estimate the effects on workers, I match firm data with a representative panel of individuals.

#### 3.1 East German state-owned enterprises

I use a combination of administrative sources and surveys to follow the communist firms from their entry into a market economy in 1990 until their eventual privatization or liquidation. I build on the most recent **firm registry** with the universe of firms under the privatization program obtained through a Freedom of Information request from the successor organization of the privatization agency ([Bundesanstalt für vereinigungsbedingte Sonderaufgaben, 2016](#)). It records for each firm one of four different privatization statuses after the end of the privatization program in December 1994. First, the sale of the firm to a private owner. Second, restitution to formerly expropriated owners. I group the sale of the firm and restitution to owners into simply privatization. Third, the liquidation which entailed the eventual cessation of economic activity. Fourth, public ownership as defined by full ownership by sub-national federal or communal governments, sometimes also called remunicipalization.

I enrich this list with firm characteristics from the **ISUD database**, the privatization agency’s information system that was used internally ([Treuhandanstalt, 2022](#)). Annual information on employment, revenue and capital can be extracted from the database in particular for earlier years. The agency recorded as well for which company a request for continued state-ownership was filed by mainly local governments.

Since the database received only irregular updates after 1994, I rely on the **SOESTRA Treuhand firm surveys** carried out by a private survey company and commissioned by the Federal Agency for Employment ([SÖSTRA, 2003](#)). The biannual survey submitted questionnaires to the universe of East German state-owned enterprises. Surveys were conducted with firms that were still under the authority of the Treuhand privatization agency and already under the control of their new owners post-privatization. The first survey was conducted in April 1991 and the last one in May 2003. The surveys contained questions about the employment composition, balance sheet information and the privatization

process. (Mergel [et al., 2020](#)) show that these surveys are representative for states and privatization outcomes.

I use the firm data in two ways: First, I aggregate privatizations and liquidations at the labor market level that I define by 2 digits sector and district cells. Second, I construct a balanced annual firm panel of firm characteristics. To fill annual gaps in the panel, I impute the average of the earliest and latest non-missing year.

### 3.2 Employment, income and well-being

The SOEP is a panel data set of individuals and households administered by the DIW Berlin and uniquely suited to study outcomes over the entire transition period due to its long periodical coverage (Socio-Economic Panel, 2019). It contains detailed information on employment, income and well-being. My main sample consists of all working-age (18-64) individuals who lived in the GDR before the fall of the Wall in 1989 and live in one of the six East German federal States at the time of the survey. The first surveys in East Germany were conducted in June 1990. The DIW complies with German data protection laws by providing access to district codes through an email server.

Figure 1 shows aggregate trends in my main outcomes measuring welfare. The first two plots from the left show puzzling dynamics. Whereas joint income at the household level grew dramatically in the first years following the reunification converging towards the average West German level, life satisfaction dipped in 1991 and 1992. The third plot on the right suggests a potential answer to this puzzle, showing how unemployment surged in 1992 and 1993 and remained at about 15% until 2005 - three times as high as for West Germans.

Figure 1: Average household income, life satisfaction and unemployment status between East and West Germans



Note: The plot shows average the average level of household income, life satisfaction and unemployment for East Germans who lived East of the Wall in 1989 and continued living in the East and for West Germans who lived in the West and continued living in the West.

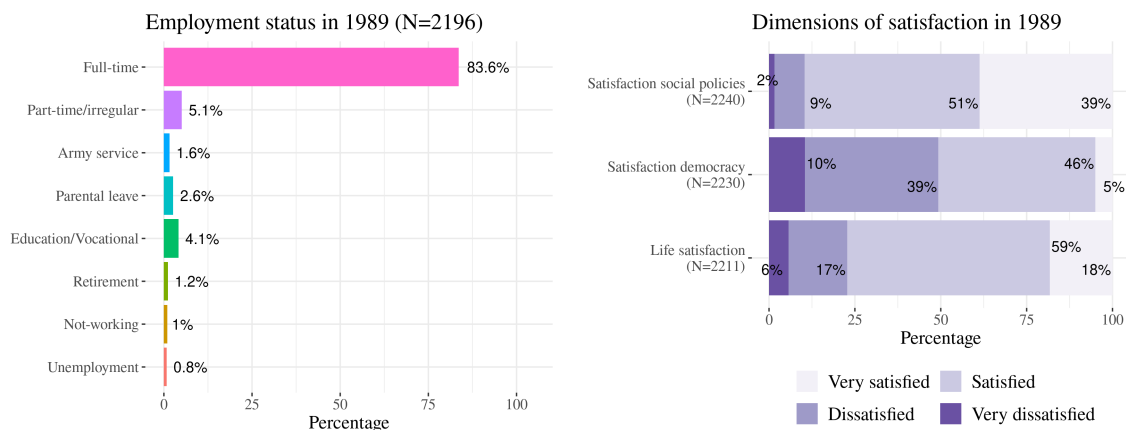
Source: Socio-Economic Panel (2019)

Survey respondents in SOEP were posed questions about life in the GDR in retrospective in 2017. The answers might be subject to recall bias and sample selection bias and



should therefore be interpreted carefully. Figure 2 shows answers to questions concerning employment and life satisfaction. The left panel breaks down responses obtained on the employment status before the fall of the wall in 1989. Only 0.8% respondents indicated to be in unemployment and one additional percent reported to be not working. Using these survey responses as a benchmark, joblessness was likely to be very rare in East Germany. The right plot of Figure 2 breaks down responses to survey questions on life satisfaction. 77% of respondents were satisfied with their lives in the GDR. In the original scaling of the question comparable to the question posed on life satisfaction from 1990 onward the average was 6.95. Interestingly, if East German respondents are able to recall their life satisfaction correctly, the average level of satisfaction is similar in 1989 before the fall wall as well as after the fall in 1990. Two further opinions on the satisfaction with social polices and democracy in the GDR were solicited. There is relatively strong satisfaction with social policies, but comparably less with democracy.

Figure 2: Retrospective survey questions about employment and life satisfaction in the German Democratic Republic (GDR)



Note: The plot shows answers to questions posed retrospective questions to SOEP survey participants in 2017 about their lives in the German Democratic Republic. The sample contains individuals who were at least 18 old in 1989. Responses to overall life satisfaction were rescaled from a scale of 0 to 10 to a scale of 1 to 4.

Source: Socio-Economic Panel (2019)

I am able to match individuals with firms by linking them to individuals based on their district of residence and sector of industry. Working with district codes is complicated by the fact that the administrative boundaries of districts were reformed in several waves since the reunification. The number of districts was reduced from 217 at the end of 1989 to 78 at the end of 2017. I therefore harmonize the territorial status of districts to the end of 2017, modeling splits, aggregations and code changes of districts using a cross walk with populations weights provided by the federal Institute for Research on Building, Urban Affairs and Spatial Development (*Bundesinstitut für Bau-, Stadt- und Raumforschung*, 2020). To match the NACE 2 digits industry codes used in SOEP to the internal 4 digits industry codes used by the Treuhand, I build a cross-walk manually.

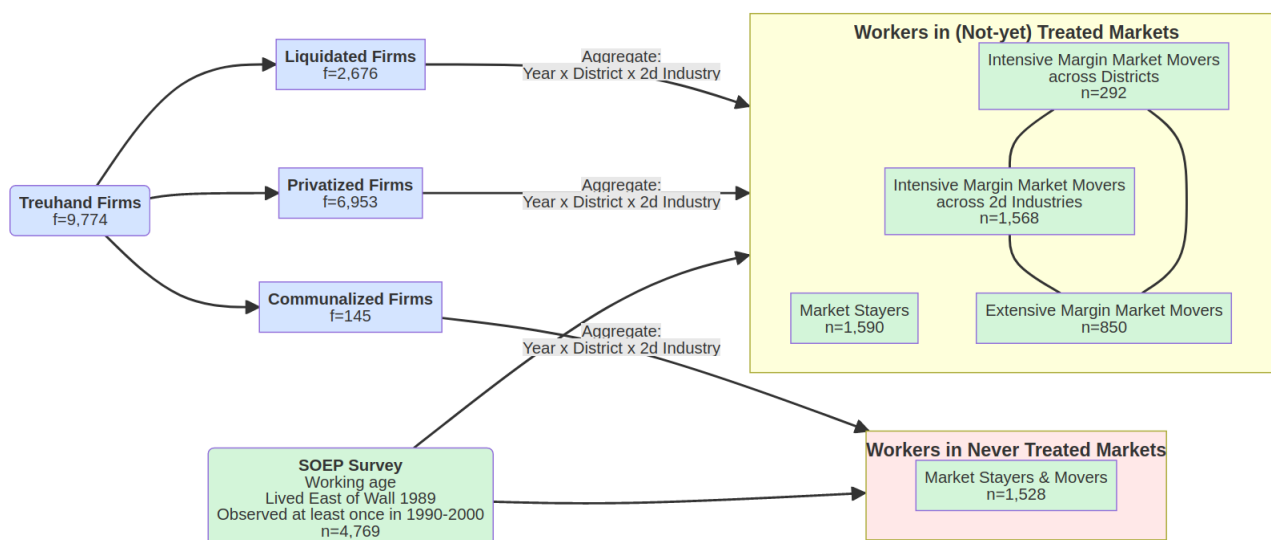
## 4 Consequences of firm restructuring for exposed workers in local labor markets

How did restructuring in the form of privatizations and liquidations impact workers in local labor markets? The goal of this section is to assess the impact using a difference-in-differences strategy.

### 4.1 A control group for workers in labor markets impacted by privatizations and liquidations

I follow the strategy of using workers who live and work in local labor markets that were not exposed and workers who have not been exposed yet as a control group. Making use of annual information on the location and 2 digit industry of employment of workers, I match privatizations and liquidations with worker outcomes to construct a panel data set. Figure 3 illustrates the matching of firms with workers across data sources.

Figure 3: Construction of worker sample in treatment and control labor markets



*Note:* The plots stylizes the construction of the worker sample. The number of different workers per group is indicated by  $n$ . The number of firms is indicated by  $f$ . The black lines connecting the three types of movers symbolize that these sets partially overlap.

Summary statistics in table 6 in the appendix show that 60.8% of workers in the sample were exposed to either privatizations and/or liquidations. Since all larger firms in East Germany were state-owned in 1989 and subsequently came under the authority of the privatization agency, most workers in treated local labor markets worked at state-owned enterprises prior to treatment. Nevertheless, there is the possibility that workers in the treatment group were only treated indirectly in their local labor market by already working at new firms that had entered after the reunification. Workers in never treated markets could be employed as well by never privatized nor liquidated state-owned enterprises, by private new entrants, the public and nonprofit sector or be self-employed. Table 1 shows the balance of pre-treatment covariates between treatment and control group. There is some

imbalance in the sense that control group workers are more likely to be women, have a college degree and to be employed in some specific industries. These imbalances are not necessarily an issue for my identification strategy, unless they have a time-varying impact on my outcomes correlated with treatment. Nevertheless, I am able to eliminate these differences following a matching strategy in the appendix. Moreover, I am able to leverage individual panel data to track down workers who moved across treatment. Figure 3 shows that some workers moved across the intensive and extensive margin of treatment. In robustness checks I am able to split the treatment group into movers and stayers.

Table 1: Balance of covariates of workers in markets affected and unaffected by restructuring

	Treated	Never or Not-Yet Treated	
	Mean	Mean	ASMD
<b>Characteristics</b>			
Binary gender (M=0/F=1)	0.394	0.606	0.307
Age (years)	41.842	40.84	0.054
College (0/1)	0.166	0.356	0.315
State:			
Berlin (0/1)	0.007	0.004	0.023
Brandenburg (0/1)	0.168	0.2	0.057
Mecklenburg-Western Pomerania (0/1)	0.116	0.137	0.043
Saxony (0/1)	0.313	0.248	0.102
Saxony-Anhalt (0/1)	0.199	0.203	0.006
Thuringia (0/1)	0.197	0.209	0.021
Industry (1d NACE rev1.1):			
Mining, food, textiles (0/1)	0.145	0.08	0.146
Wood, paper, petroleum, chemicals, metals (0/1)	0.199	0.064	0.289
Machinery, vehicles, furniture, recycling (0/1)	0.053	0.035	0.063
Energy, water, construction (0/1)	0.214	0.025	0.431
Retail, wholesale, hotels (0/1)	0.228	0.045	0.391
Transport, communication (0/1)	0.076	0.116	0.097
Real estate, research, public admin (0/1)	0.06	0.227	0.347
Education, health, social (0/1)	0	0.358	0.747
Sewage, other services (0/1)	0.024	0.05	0.097
Observations	14938	13208	

*Note:* The table shows the balance of characteristics of workers in local labor markets impacted by liquidations or privatizations compared to unaffected markets. The column SMD shows standardized mean differences in covariates. SMD values close to 0 indicate balance, values larger than 0.1 imply imbalance.

## 4.2 Event-study of privatization and liquidation impacts on workers in local labor markets

I estimate differences between workers in labor markets affected by the privatization agency's decisions and markets that were not or not-yet affected over time using difference-in-differences with local projections (Dube et al., 2022). Even though individual labor market data is only available from 1990 onward, I am able to exploit the staggered treatment at the local labor market level to test for differential pre-trends. I aggregate the number of liquidations

and privatizations within a district and two-digits industry from the first year of treatment. Treatment is therefore absorbing in the sense that a dosage is given once which does not change subsequently. I include working-age individuals in all industries who lived in East Germany from before the fall of the wall until 2006. Individuals who never worked or where industry information is missing are dropped from the sample. As soon as individuals become unemployed, I carry forward the last industry they worked in. I drop markets with gaps, such that the baseline sample is unbalanced with no gaps at the market level and with gaps at the individual level. To employ [Dube et al. \(2022\)](#)'s estimator that is robust to heterogenous treatment effects, I aggregate the sample at the year-market level and weigh observations by initial size of the labor market. Formally, I estimate each dynamic event study coefficient  $h$  by

$$\underbrace{y_{j,t+h} - y_{j,t-1}}_{\text{Long difference}} = \beta_{LP-DID}^h \underbrace{\Delta Exposure_{jt}}_{\text{First-differenced treatment}} + \underbrace{\tau_t^h}_{\text{Time FE}} + \epsilon_{jt}^h \quad (1)$$

where  $y_{jt}$  are aggregate labor market outcomes in local labor market  $j$  defined as 2-digits NACE industries and district and year  $t$ . Treatment is either the natural logarithm of the number of total privatizations ( $P_{jt}$ ) or liquidations ( $L_{jt}$ ) weighted by each firm's employment size. To disentangle the effect of privatizations from liquidations and to prevent the contamination of the estimates from "negative weights", I follow [Dube et al. \(2022\)](#) and [de Chaisemartin and D'Haultfœuille \(2020\)](#) and include either A. newly treated markets with privatizations that have not been treated with liquidations (i.e.  $\Delta P_{jt} > 0$  &  $\Delta L_{jt} = 0$ ) or B. "clean control" units that have not been treated in the past (i.e.  $P_{j,t+h} = 0$  &  $L_{j,t+h} = 0$ ). To estimate the effect of liquidations on labor market outcomes I proceed analogously.

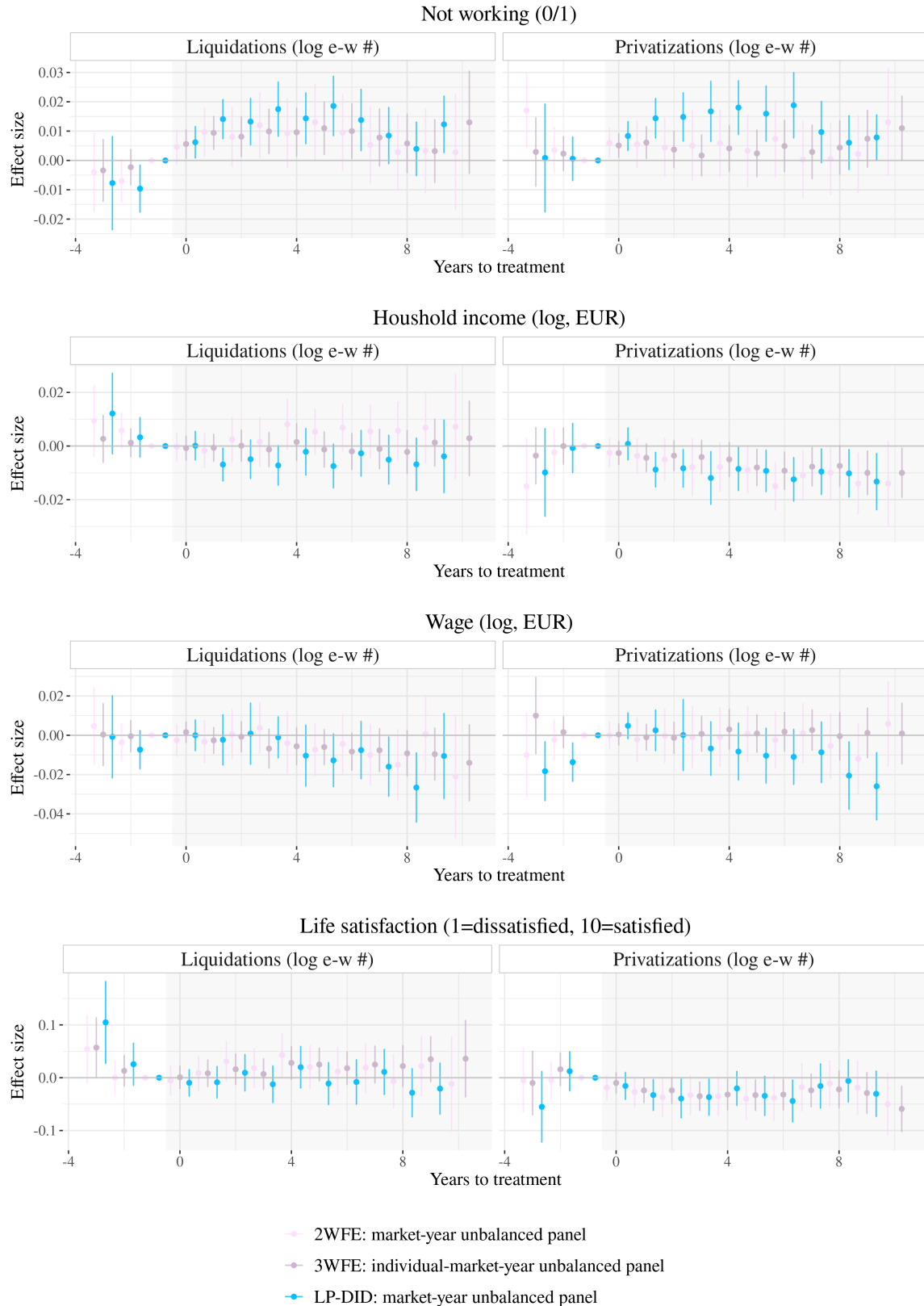
### 4.3 Estimating restructuring effects on workers from labor market treatments

Event study plots based on estimating the effects of privatizations and liquidations on workers through difference-in-differences using local projections are presented in [Figure 4](#) and [5](#). Flat estimates prior to period  $t = 0$  in the plots are supportive to the assumption made in difference-in-differences studies that pre-trends might indeed be parallel and that there is no anticipation of treatments. [Table 2](#) exhibits the point estimates of the 3rd and the 6th coefficient following treatment in  $t = 0$ .

I find support for predominantly adverse effects on workers. The upper panel in [Figure ??](#) shows the effect of the log number of privatizations and liquidations weighted by firm employment on **not working** respectively. The third line in [Table 2](#) for privatizations and liquidations respectively computes the effect for a standard deviation increase in treatment. One standard deviation increases the likelihood of dropping out the labor force three years after treatment by 6.15pp for privatizations and by 5.66pp for liquidations. **Wages** and **household income** seem also to be impacted negatively in particular by privatizations. Wages drop by 4.57% six years following an increase in privatized jobs in the labor market by a standard deviation from 0 to 3913. Furthermore, the panel data allows me to assess self-rated **health** on a scale from 1 (bad) to 5 (very good). I find significant effects for workers affected by privatizations. Lastly, I can estimate effects on reported **well-being** on a scale from 1 (dissatisfied) to 10 (satisfied), assessed in the surveys from 1990 onward. Privatiza-

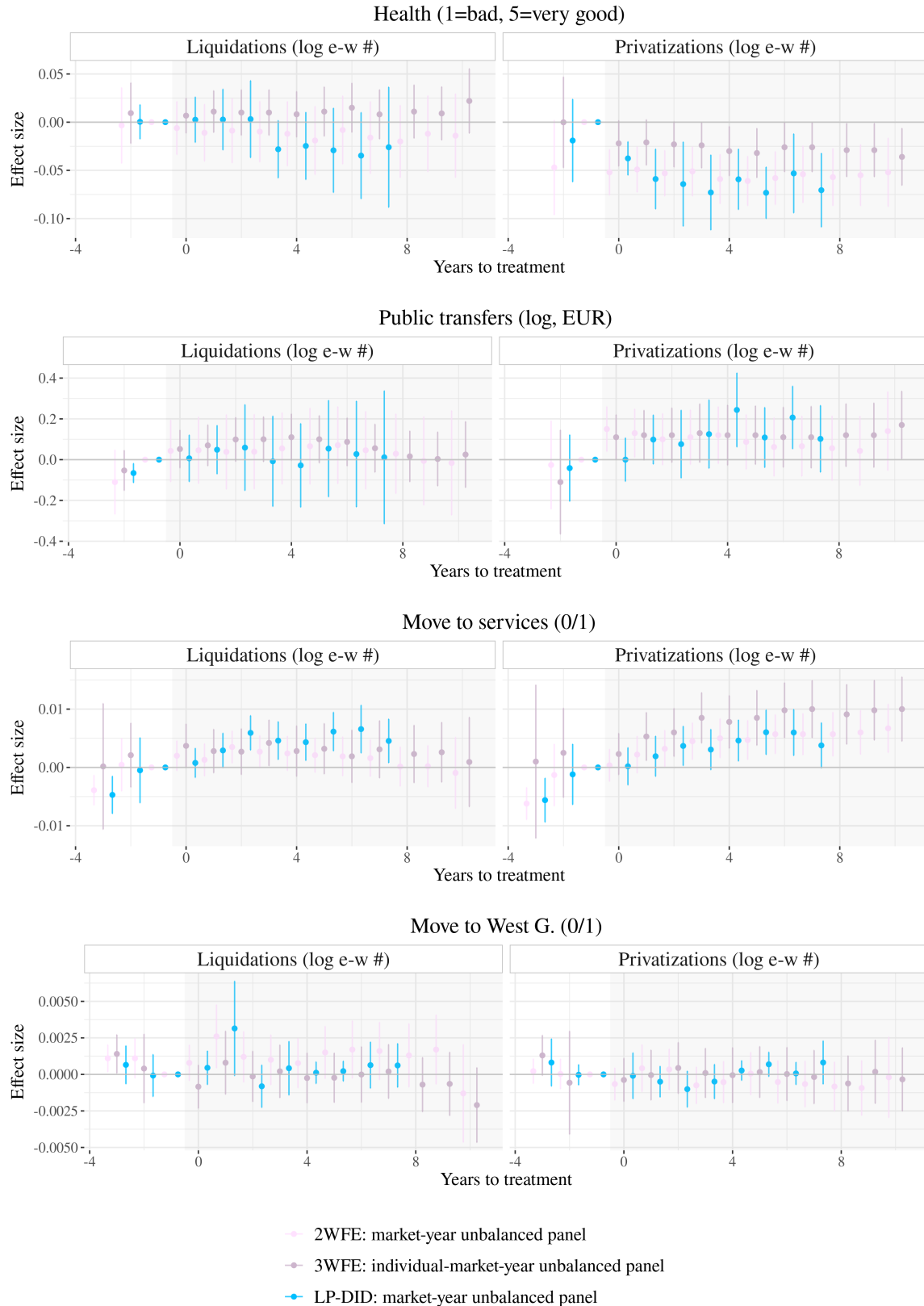
tions seem to depress life satisfaction in a way that workers in affected markets report to be less satisfied by one fifth of a category. All in all, the results point to a detrimental impact of privatizations on welfare outcomes of workers.

Figure 4: Impact of restructuring on workers: Main outcomes



Note: The plots show coefficients of the impact of privatizations and liquidations from estimating Equation 1. 95% confidence intervals around the point estimate are shown.

Figure 5: Impact of restructuring on workers: Additional outcomes



Note: The plots show coefficients of the impact of privatizations and liquidations from estimating Equation 1. 95% confidence intervals around the point estimate are shown.

Table 2: Impact of restructuring on workers

		Not work.	HH inc.	Wage	Life satisf.	Health	Pub. transf.	Move serv.	Move West
		(0/1)	ln(EUR)	ln(EUR)	[1,10=satisf.]	[1,5=good]	ln(EUR)	(0/1)	(0/1)
<b>Unbalanced</b>									
Privatizations (log e-w #)	$\beta_3^{lp-did}$	0.017**	-0.012*	-0.007	-0.037*	-0.073***	0.125	0.003	0.000
	$se(\beta_3^{lp-did})$	(0.005)	(0.005)	(0.007)	(0.018)	(0.02)	(0.085)	(0.002)	(0.001)
	$f(sd(P) * \beta_3^{lp-did})$	6.15pp	-4.37%	-2.47%	-0.14	-0.27	45.98%	1.12pp	-0.18pp
	$n_3$	1976	1976	1549	1976	1262	1274	1564	1567
	$\beta_6^{lp-did}$	0.019**	-0.012**	-0.011	-0.044*	-0.053*	0.207**	0.006**	0.000
	$se(\beta_6^{lp-did})$	(0.006)	(0.004)	(0.007)	(0.02)	(0.021)	(0.078)	(0.002)	(0)
Liquidations (log e-w #)	$\delta_3^{lp-did}$	0.018***	-0.007	-0.001	-0.012	-0.028	-0.008	0.005**	0.000
	$se(\delta_3^{lp-did})$	(0.005)	(0.004)	(0.005)	(0.018)	(0.015)	(0.113)	(0.002)	(0.001)
	$f(sd(L) * \delta_3^{lp-did})$	5.66pp	-2.33%	-0.33%	-0.04	-0.09	-2.58%	1.49pp	0.14pp
	$n_3$	1941	1941	1515	1941	1274	1286	1593	1596
	$\delta_6^{lp-did}$	0.014*	-0.003	-0.008	-0.008	-0.035	0.027	0.007**	0.001
	$se(\delta_6^{lp-did})$	(0.005)	(0.004)	(0.007)	(0.022)	(0.023)	(0.132)	(0.002)	(0.001)
Privatizations (log e-w #)	$\beta_3^{lp-did}$	0.016**	-0.01*	-0.007	-0.049**	-0.09***	0.137	-	-
	$se(\beta_3^{lp-did})$	(0.005)	(0.005)	(0.008)	(0.018)	(0.02)	(0.097)	-	-
	$f(sd(P) * \beta_3^{lp-did})$	5.73pp	-3.63%	-2.72%	-0.18	-0.33	50.43%	-	-
	$n_3$	1529	1529	1247	1529	962	964	-	-
	$\beta_6^{lp-did}$	0.019**	-0.012**	-0.012	-0.039	-0.054*	0.205*	-	-
	$se(\beta_6^{lp-did})$	(0.006)	(0.004)	(0.007)	(0.021)	(0.023)	(0.084)	-	-
Liquidations (log e-w #)	$\delta_3^{lp-did}$	0.019***	-0.008*	-0.001	-0.014	-0.022	-0.007	-	-
	$se(\delta_3^{lp-did})$	(0.005)	(0.004)	(0.006)	(0.019)	(0.016)	(0.124)	-	-
	$f(sd(L) * \delta_3^{lp-did})$	6.18pp	-2.72%	-0.39%	-0.04	-0.07	-2.21%	-	-
	$n_3$	1510	1510	1226	1510	973	975	-	-
	$\delta_6^{lp-did}$	0.014*	-0.004	-0.008	-0.003	-0.03	0.019	-	-
	$se(\delta_6^{lp-did})$	(0.005)	(0.005)	(0.008)	(0.022)	(0.023)	(0.136)	-	-
Market movers	$\beta_3^{lp-did}$	0.012*	-0.004	-0.003	-0.017	-0.009	0.081	0.002	-0.001
	$se(\beta_3^{lp-did})$	(0.006)	(0.006)	(0.006)	(0.023)	(0.024)	(0.084)	(0.002)	(0.001)
	$f(sd(P) * \beta_3^{lp-did})$	4.57pp	-1.6%	-1%	-0.06	-0.03	29.88%	0.88pp	-0.19pp
	$n_3$	1872	1872	1480	1872	1247	1260	1526	1526
	$\beta_6^{lp-did}$	0.018*	-0.013	-0.014	-0.065*	-0.053*	0.171*	0.005	0.000
	$se(\beta_6^{lp-did})$	(0.008)	(0.007)	(0.009)	(0.029)	(0.024)	(0.085)	(0.003)	(0)
Liquidations (log e-w #)	$\delta_3^{lp-did}$	0.01	0.000	-0.002	-0.018	-0.024	0.205*	0.004	-0.001
	$se(\delta_3^{lp-did})$	(0.006)	(0.007)	(0.007)	(0.022)	(0.021)	(0.095)	(0.003)	(0.001)
	$f(sd(L) * \delta_3^{lp-did})$	3.22pp	-0.01%	-0.76%	-0.06	-0.08	66.32%	1.27pp	-0.34pp
	$n_3$	1861	1861	1465	1861	1260	1272	1552	1556
	$\delta_6^{lp-did}$	0.018*	-0.003	-0.003	-0.047	-0.089**	-0.018	0.003	-0.001
	$se(\delta_6^{lp-did})$	(0.008)	(0.007)	(0.01)	(0.025)	(0.029)	(0.229)	(0.003)	(0.001)
Privatizations (log e-w #)	$\beta_3^{lp-did}$	5.81pp	-0.88%	-1.11%	-0.15	-0.29	-5.93%	0.86pp	-0.17pp
	$se(\beta_3^{lp-did})$	(0.008)	(0.007)	(0.01)	(0.025)	(0.029)	(0.229)	(0.003)	(0.001)
	$f(sd(L) * \delta_6^{lp-did})$	5.81pp	-0.88%	-1.11%	-0.15	-0.29	-5.93%	0.86pp	-0.17pp
	$n_6$	983	983	786	983	456	465	714	716

Note: The table shows coefficients of the impact of privatizations and liquidations from estimating Equation 1. Significance levels are \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

## 4.4 Heterogeneity

I explore the heterogeneity of these effects with respect to binary gender, age and education. Figure 9 in the Appendix shows the estimates splitting the sample into **women** and **men** where the grey line shows the difference of both coefficients including confidence intervals. Apart from potentially small effects on wages, there are no significant differences between women and men. Interestingly, binary gender differences in labor market outcomes cannot be explained by the closure and sale of state-owned enterprises. The picture for **old** and **young** workers in Figure 10 looks like that there are moderate differences in labor force participation with older workers being more likely to drop out. There are no differences in both



treatments for wages, household income and life satisfaction. Figure 11 shows differences between college-educated and non college-educated. Interestingly, the effect on the propensity of labor force drop-out is higher for college-educated for both treatments. Even though firm closures lead to a stronger downward pressure on wages for only secondary-schooled workers, there does not seem to be a negative effect on household income combining wages and labor force participation for workers without college education.

## 4.5 Robustness

Table 2 and Table 7 in the Appendix demonstrate the robustness of these findings across alternative samples and identification strategies. Table 2 compares estimates of the unbalanced baseline sample, to a sample based on a panel balanced on labor markets and to the baseline sample restricting the treatment group to workers that moved their local labor market at least once. Table 7 in the Appendix includes estimates based on a sample restricting the treatment group to workers that never move their labor market, and estimates using matching weights based on 1 digit industries and industries and gender, age and college.

# 5 Consequences of privatization for firm outcomes

How do private owners restructure former state-owned enterprises? To answer this question, this section makes use of rich firm data exploiting the staggered privatization over time.

## 5.1 A control group for privatized firms

What constitutes a good control group for privatized firms? A natural approach would be to compare privatized to not-yet privatized firms over time. However, Mergele et al. (2020) document that the privatization agency used an indicator of competitiveness to prioritize firms, one of the likely reasons why more productive firms were sold earlier. If firms privatized earlier during the program are more competitive, they would likely fare better prior to privatization such that parallel trends are violated. A more suitable control group are companies that remain in state ownership. The reunification treaty specified that ownership over companies serving "communal tasks and services" was to be handed over to municipalities, cities and districts (Treuhandanstalt, 1994). It included public utilities in energy, water, waste, health and environment such as companies in education, research and cultural production. Local governments had to submit a request for continued state-ownership to the privatization agency for each company. To alleviate concerns over structural differences between treatment and control group, I follow a complementary strategy of coarsened exact matching and scrutinizing robustness in subsamples (Iacus et al., 2009). In subsamples, I restrict the treatment group to firstly, firms for which communalization requests had been filed and secondly, to firms in utility sectors.

Table 3 presents the matching strategy on federal state, 1 digit industry and whether the company was handled through the agency's headquarter or a local branch. The baseline sample is balanced for all firms with controls and the main outcomes employment and revenue. Firms under continued state-ownership are more frequent in the state of

Table 3: Balance of covariates between privatized and state-owned firms: Baseline sample

	Pre-matching			Post-matching		
	Privatized	State-owned	SMD	Privatized	State-owned	SMD
<b>Covariates</b>						
State:						
East Berlin	0.046	0.022	0.115	0.024	0.024	0
Brandenburg	0.125	0.111	0.043	0.131	0.131	0
Mecklenburg-West. Pom.	0.116	0.178	-0.194	0.042	0.042	0
Saxony-Anhalt	0.181	0.156	0.065	0.202	0.202	0
Saxony	0.333	0.333	0	0.383	0.383	0
Thuringia	0.198	0.2	-0.004	0.218	0.218	0
Industry (1 digit):						
Agriculture	0.014	0	0.119	0	0	0
Mining, energy and water	0.025	0.133	-0.685	0.031	0.108	-0.489
Manufacturing	0.632	0	1.309	0.713	0	1.479
Construction	0.11	0	0.351	0.103	0	0.331
Retail and trade	0.069	0	0.273	0	0	0
Transport	0.019	0.533	-3.737	0.02	0.392	-2.701
Other services	0.131	0.333	-0.601	0.133	0.501	-1.092
Local branch	0.625	0.4	0.464	0.587	0.587	0
<b>Observations</b>						
Firms (#)	1140	45		949	45	
Observations (#)	7980	315		6643	315	
Period	1991-1997	1991-1997		1991-1997	1991-1997	

*Note:* The table shows descriptive statistics of privatized and state-owned enterprises before and after matching on federal state, whether company was privatized through a local branch of the privatization agency and coarsened 1 digit industries. The column SMD indicates the standardized mean difference between treatment and control. SMD values close to 0 indicate perfect balance and values larger than 0.1 show imbalance.

Mecklenburg-West Pomerania, and less frequent in former East Berlin. State-owned firms are concentrated in three 1 digit industries. I match within state, local branch and 1 digit industries with state-owned firms exactly and pool 1 digit industries with no state-owned firms in the baseline sample.

## 5.2 Event-study of privatization impacts on firms

I use a balanced firm panel of East German state-owned enterprises to estimate the impact of privatization on firm outcomes. I employ an event-study strategy around the year of privatization utilizing remaining public companies as never-treated control group matched on firm characteristics. Formally, I start from the following dynamic difference-in-differences (DiD) specification with staggered rollout of treatment and two-way fixed effects (TWFE)

$$y_{jt} = \sum_{e=-K}^{-2} \gamma_e P_{jt}^e + \sum_{e=0}^M \gamma_e P_{jt}^e + \zeta_j + \zeta_t + \epsilon_{jt} \quad (2)$$

where  $P_{jt}^e$  indicates that firm  $j$  is being  $e$  periods away from its year of privatization. The model includes full leads and lags of the treatment indicator from period  $-K$  until  $M$ ,

except for the relative year before treatment  $e \neq 1$ . Moreover,  $\xi_j$  are firm fixed effects,  $\xi_t$  are annual fixed effects and  $\varepsilon_{jt}$  an error term. This popular design has recently been subject to intense criticism evolving around the causal interpretation of the estimated parameters (de Chaisemartin and D’Haultfœuille, 2020; Borusyak et al., 2021; Sun and Abraham, 2021; Callaway and Sant’Anna, 2021; Goodman-Bacon, 2021; Athey and Imbens, 2022). In fact, two-way fixed effect regressions can only recover a weighted average of treatment effects. Crucially, if treatment effects are heterogenous over time, TWFE may lead to arbitrary weighting to the extent that the average of treatment effect estimates can obtain a different sign. As Goodman-Bacon (2021) shows implicit weights on treatment effects by TWFE are sensitive to the size of each treatment group treated in a specific year, the total number of periods and variation in treatment timing. Intuitively, one issue lies in the case where treatment effects increase over time and already treated groups serve as a control group for later treated groups. Various solutions have been proposed in the literature to circumvent potential weighting issues. The first step in Sun and Abraham (2021)’s estimator is based on interacting the treatment indicator with indicators for the treatment initiation period. The effects at each treatment initiation period are weighted with the relative group size. Callaway and Sant’Anna (2021) proceed in a similar fashion, dissecting the DiD estimator into possible subgroups receiving treatment at the same time. Both methods have in common that they are able to circumvent negative weights of the DiD estimator and allow for heterogenous effects across units and time. The identification assumptions are similar as one needs to rule out anticipatory reactions and manipulation into treatment and different forms of non-parallel trends. I therefore report TWFE along both methods proposed by Sun and Abraham (2021) and Callaway and Sant’Anna (2021).

Average treatment effects on the treated can be identified in DiD even in the presence of non-random selection into treatment. For instance, if treated companies are not necessarily public utilities, that does not pose a problem as long as their trend is parallel to the trend of privatized firms had they not been privatized. In other words, should firms producing for instance tradable consumption goods be subject to different competitive pressures than firms in transportation, I would expect to detect differences in outcomes prior to privatization. To assess the parallel trends assumption, I follow the common practice in testing for differential pre-trends between firms in the treated and control group. The difference between Sun and Abraham (2021) and Callaway and Sant’Anna (2021) is whereas the former estimates coefficients with respect to period  $-1$ , the latter estimates coefficients for all treatment periods. To lend further credibility to the parallel trends assumption, I follow the matching procedure described in the previous section. Moreover, I subset the main sample to firstly firms, for which a successful or unsuccessful request for continued state-ownership had been filed with the privatization agency by government bureaucrats and secondly only to public utilities. Firms might likely be more comparable across unobservables in these subsets such as political clout of governments in their jurisdiction, local externalities firms generate etc.

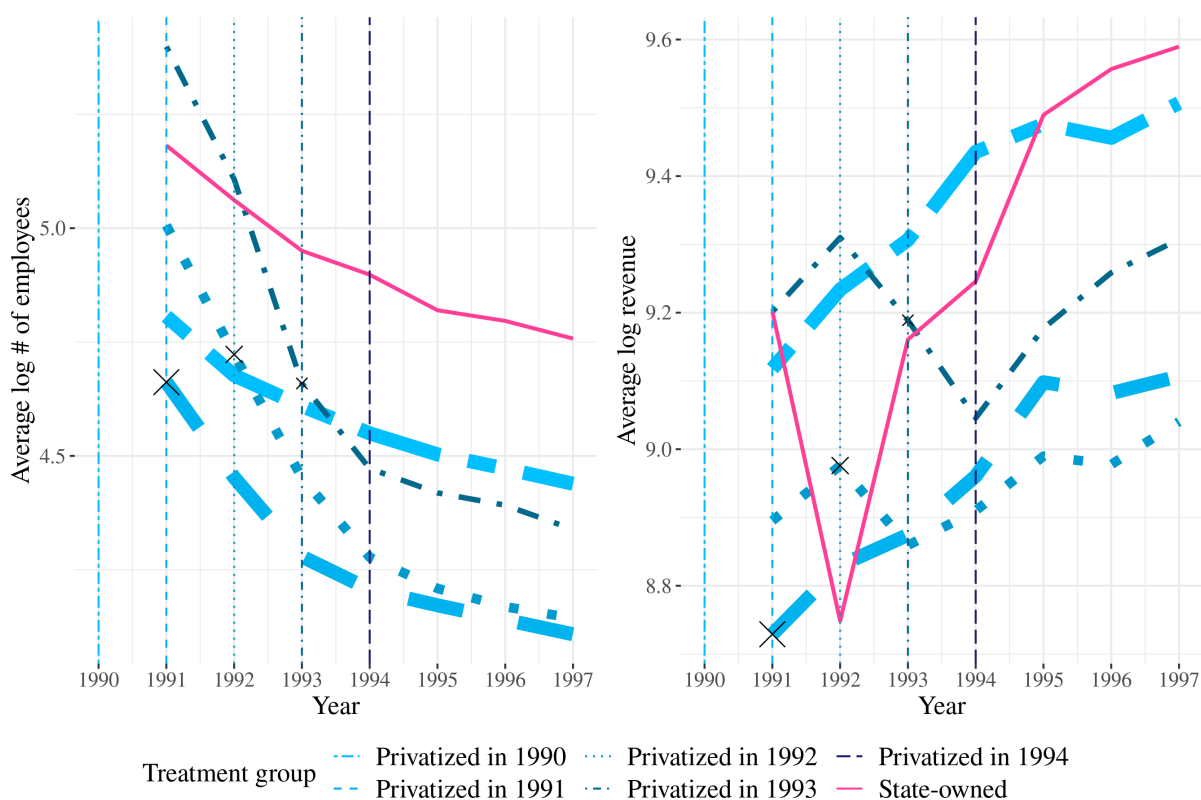
A second crucial assumption in difference-in-differences is that treatment is not anticipated. The assumption can be relaxed by using leads of the treatment indicator in the estimation (Borusyak et al., 2021). Since the privatization agency was able to appoint the management of the companies under its ownership, it is possible that restructuring efforts might have been influenced in anticipation of future privatization. I therefore use one lead

of the treatment indicator, unless stated otherwise.

### 5.3 Estimated effects of privatization on firm outcomes

What are the impacts of privatizations on firm outcomes? I first present the evolution of main firm outcomes in levels over time for companies after privatization and in continuous state-ownership. Figure 6 plots the average number for the baseline sample with log employees and log revenue. Each plot shows the average outcomes of companies that remain state-owned in a violet solid line. These companies serve as a control group for privatized firms. The averages of privatized firms are shown in blue dotted lines. Darker blue means that these companies were privatized later during the begin of the privatization program in June 1990 and its end December 1994. The thickness of the lines is adjusted linearly to the size of the group. The upper left panel of Figure 6 suggests that employment declined less rapidly over time than in groups of privatized firms. In the lower left panel of Figure 6, public companies experience faster growth in revenues compared to most privatized companies.

Figure 6: Average levels of firm outcomes by treatment timing



*Note:* The plots show average levels of total employment and revenue for privatized firms in different shades of blue and public firms in violet. Privatized firms are split into groups along the year of treatment where light blue denotes that they were privatized earlier. The thickness of the line represents relative group size. Crosses on the blue lines indicate treatment timing.

Moving beyond levels, Figure 7 shows estimates of three different estimation methods, TWFE from Equation 2, Sun and Abraham (2021)'s estimator and Callaway and Sant'Anna (2021)'s estimator employed across three different samples with two different outcomes:

employment and revenue. Table 4 shows the aggregate effect over the entire event window. The left panels in Figure 7 exhibit the privatization effects on **employment**. All estimators move qualitatively downwards in unison following privatization in period 0 and exhibit significant employment losses due to privatization. The effects are large in magnitude with point estimates for the ATT in the baseline sample shown in table 4 of  $100(e^{-0.336} - 1) = -28.5\%$  and  $100(e^{-0.401} - 1) = -33.0\%$  respectively. The negative employment effects seem to exist despite employment guarantees that had to be given by investors to the privatization agency. However, only 52% of the number of jobs pledged by acquirers were actually contractually binding for a short period ([Bundesanstalt für vereinigungsbedingte Sonderaufgaben, 1994](#)). The employment effects holds for all privatized firms with non-missing observations for employment and revenue in the baseline sample such as both samples that use only subsets of treated firms. Privatized firms with unsuccessful requests for continued state-ownership and privatized firms within public utility sectors show these negative employment effects. There seems to be an impact of privatization on **revenue** as can be seen in the right panels of Figure 7. All estimation methods show a decline in revenue by firms for the baseline and the state-ownership request sample. Table 4 shows that the aggregate effects are negative and significant for [Sun and Abraham \(2021\)](#)'s estimator on the baseline sample and for all methods on the sample using only privatized firms with unsuccessful state-ownership requests by local governments. The main assumption for the causal interpretation of these estimates is that outcomes of privatized firms would have evolved parallel to firms remaining state-owned had they not been privatized. It is common practice that insignificant differences prior to treatment are interpreted in support of this untestable assumption. The failure of the rejection of placebo tests at the 95% confidence level shows that there is some support for this assumption. Figure 12 in the Appendix shows the robustness to various matching specifications.

I am able to test privatization effects on other outcomes using balanced subsamples of the baseline sample due to missing observations in the outcomes. Figure 8 shows event study plots of privatization effects on further outcomes. The upper left panel plots coefficients from regressions using **capital** as outcome. The value of capital used in the firm seems to decline for privatized firms. The aggregate effect is only significant for [Callaway and Sant'Anna \(2021\)](#)'s estimator as presented in table 5. I test privatization effects on **productivity**, using total factor productivity obtained as the residual from regressing revenue on employment and capital controlling for 2-digits fixed effects for each year. Evidence from China ([Chen et al., 2021](#)) and Eastern European countries ([Brown et al., 2006](#)) suggests that privatization may increase productivity. Surprisingly, I find no effects on productivity over the event window in contrast with many earlier findings from the literature. The stylized model in section 6 can rationalize these findings. When privatization does not increase productivity, declines in employment could be due to two reasons. First, government-owned firms might derive additional benefits from labor beyond their contribution to output. Politicians might express their preference for jobs directly through position on the management board or indirectly through subsidies. Second, state-ownership might be less averse to the risks of investing into communist firms than compared to private firms.

Turning to **wages**, figure 8 shows that the average wage in privatized firms seem to experience increases that are not significantly different from 0 in line with the conceptual

Table 4: Aggregate effects of privatization on firm outcomes: Main samples

	Baseline		SO request		Utilities	
	Log empl.	Log rev.	Log empl.	Log rev.	Log empl.	Log rev.
TWFE	-0.252*** (0.028)	-0.132* (0.064)	-0.346*** (0.042)	-0.306** (0.096)	-0.205*** (0.058)	-0.003 (0.166)
ATT (Abraham & Sun, 2021)	-0.215*** (0.031)	-0.063 (0.071)	-0.318*** (0.047)	-0.225+ (0.115)	-0.167** (0.062)	0.082 (0.200)
ATT (Callaway & Sant'Anna, 2021)	-0.201*** (0.050)	0.019 (0.221)	-0.371*** (0.068)	-0.283 (0.175)	-0.157* (0.073)	0.189 (0.526)
N privatized	1140	1140	333	333	200	200
N state-owned	45	45	45	45	45	45
N obs	8295	8295	2646	2646	1715	1715
Matching	CEM	CEM	CEM	CEM	CEM	CEM

+  $p < 0.1$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

*Note:* The table shows estimated aggregate effects from event studies based on equation 2 using three different samples. Regressions are weighted with matching weights obtained from coarsened exact matching presented in table 3 for the baseline sample. The second sample comprises of privatized firms for which a request for continued state-ownership had been submitted to the privatization agency by local governments. The third sample reduces the treatment group to firms in the 1 digit industries mining, energy and water, transportation and other services.

framework in section 6. Figure 8 provides little evidence for effects on the **share of women** in the firm's workforce. In light of recent work on the persistence of the East German cultural norm for women to work, it seems that West German ownership as the dominant form of ownership in privatized firms did not have an impact on female employment in these firms (Boelmann et al., 2021). Both lower plots in Figure 8 scrutinize changes in the composition of the workforce. The lower left plot shows some evidence of negative effects on the **share of employees in R&D**. As most privatized firms were bought by West German companies (Mergele et al., 2020), vertical integration may have led to a specialization of R&D, management, marketing and final goods in West European headquarters and production in the East. Interestingly, more than two decades after the privatization program, private R&D is still lower in East Germany today (Eickelpasch, 2015; Mertens and Müller, 2022). Lastly, the lower right plot in Figure 8 shows no effect on the **share of trainees**. To conclude, I find large effects of privatization on firm outcomes. The main finding is that privatized public utilities, firms in jurisdictions that sought their continued state-ownership and all other privatized firms experience declines in employment and to some extent in revenue. Estimates reveal negative effects on capital and potentially the share of employees in R&D. There is no evidence for improvements in simple total factor productivity.

## 6 Rationalizing the restructuring of East German state-owned enterprises

To rationalize previous findings, I present a simple model of wage bargaining between workers and the management of privately or state-owned firms to explain potential av-

Table 5: Aggregate effects of privatization on firm outcomes: Subsamples

	Log capital	TFP	Log wage	Sh. fem. empl.	Sh. R&D empl.	Sh. trainees
TWFE	-0.156+ (0.082)	0.082 (0.093)	0.051** (0.019)	-2.216** (0.719)	-1.846 (1.410)	-2.030 (1.795)
ATT (Abraham & Sun, 2021)	-0.182* (0.081)	0.077 (0.099)	0.053* (0.021)	-2.601** (0.904)	-3.515+ (1.907)	-1.322 (2.109)
ATT (Callaway & Sant'Anna, 2021)	-0.318*** (0.075)	-0.151 (0.268)	0.033 (0.048)	-2.911+ (1.648)	-6.835 (4.276)	-2.896 (2.538)
N privatized	530	527	473	529	480	288
N state-owned	17	17	18	29	24	21
N obs	3829	3808	2946	2790	2520	2163
Matching	CEM	CEM	CEM	CEM	CEM	CEM

+  $p < 0.1$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

*Note:* The table shows estimated aggregate effects from event studies based on equation 2. Regressions are weighted with matching weights obtained from coarsened exact matching presented in table 3 for the baseline sample. Since missing observations differ across outcomes, the number of observations drop relative to the baseline sample.

enues how privatization of communist firms could impact workers. I infuse the argument made by [Demougin and Sinn \(1994\)](#) that private owners were more risk averse than the state during the transition of the East German economy into the model of privatization by [Arnold \(2019\)](#). There were many risks involved in investing in communist firms for example due to changes in consumer preferences and expenditures, outward migration of East German workers, or the instability of Eastern European export markets. [Demougin and Sinn \(1994\)](#) argue that state-owned enterprises are able to load off risk on a large number of taxpayers as opposed to private firms that were often closely held in West Germany by few ultimate owners. Since local credit and insurance markets only slowly developed in former Communist economies, private firms in East Germany likely had fewer risk-sharing options than when operating in Western markets.

## 6.1 Model setup

Consider a group of workers  $\bar{L}_j$  bargaining over the employment level  $L_j$  and wages  $w_j$  in a state-owned (*soe*) or privately-owned (*p*) firm  $j$ . Employed workers earn together  $L_j w_j$ , whereas workers of the group who do not obtain employment earn  $(\bar{L}_j - L_j)v$  with the value of the outside option  $v$ . The value of the outside option is equal to the expected wage in a new job or in unemployment with benefits minus search costs. If bargaining breaks down the entire group is left with  $\bar{L}_j v$ . I rule out cases where the bargained wage is below the outside option.

The firm has a concave production function  $F_j(K_j, L_j)$ . It utilizes capital  $K_j$  in its production at cost  $r$ . Firms captures benefits  $\tau_j L_j$  beyond a worker's contribution to production that can encapsulate differences in preferences of private vs government ownership (similar to [Azmat et al. \(2012\)](#)). This term could capture the argument that politicians on the board of state-owned enterprises might attempt to use public funds to maximize employ-

ment directly (Boycko et al., 1996). Regional firm subsidies aimed at job creation were also commonplace during East Germany's transition period (Siegloch et al., 2021). Taking both arguments together, I assume that  $\tau_j$  is on average larger for SOEs than for private enterprises. The main difference with Arnold (2019)'s model is that there is an additive random shock to profits following a normal distribution  $\theta \sim \mathcal{N}(0, \sigma^2)$ . The shock propagates through labor input  $\theta\sqrt{L_j}$  decreasing in marginal severity through  $L_j$ . The firm's owners therefore maximize expected utility from profits  $\mathbb{E}[U(\pi(L_j, K_j, w_j))] = \mathbb{E}[U(F_j(K_j, L_j) + \tau_j L_j - rK_j - w_j L_j - \theta\sqrt{L_j})]$  assuming zero profits when negotiations break down. To take the expectation, I assume that CARA utility equal to  $U(\pi(K_j, L_j, w_j)) = -e^{-\lambda_j(\pi(K_j, L_j, w_j))}$ , where risk aversion increases with  $\lambda_j$ . Log-transformation of utility simplifies the firm's objective using  $-\log(-\mathbb{E}[U(\pi(K_j, L_j, w_j))])$ . To take the expectation I rely on the fact that  $\log(\mathbb{E}[e^{-\lambda_j\theta\sqrt{L_j}}]) = \mathbb{E}[-\lambda_j\theta\sqrt{L_j}] + \frac{1}{2}\text{Var}[-\lambda_j\theta\sqrt{L_j}]$  when shocks are normally distributed. The expectation on the right side is zero assuming a mean of zero. Lastly, The bargaining weights  $\gamma_j$  can be different across firms, capturing the idea that unions might be stronger in SOEs than in private sector firms. For the firm side, I rule out cases where profits are negative. The bargaining problem can be written as:

$$\max_{L_j, K_j, w_j} (L_j w_j + (\bar{L}_j - L_j)v - \bar{L}_j v)^{\gamma_j} (\lambda_j (F_j(K_j, L_j) + \tau_j L_j - rK_j - w_j L_j - \lambda_j \frac{\sigma^2}{2} L_j))^{1-\gamma_j} \quad (3)$$

To solve this expression for optimal choices of  $K_j^*$ ,  $L_j^*$  and  $w_j^*$ , I assume a Cobb Douglas production function  $F_j(L_j, K_j) = A_j K_j^\alpha L_j^\beta$ . Taking the first order conditions of the bargaining problem yields the following results:

$$L_j^* = A_j^{\frac{1}{1-\alpha-\beta}} \left(\frac{\alpha}{r}\right)^{\frac{\alpha}{1-\alpha-\beta}} \left(\frac{\beta}{v - \tau_j + \lambda_j \frac{\sigma^2}{2}}\right)^{\frac{1-\alpha}{1-\alpha-\beta}} \quad (4)$$

$$K_j^* = A_j^{\frac{1}{1-\alpha-\beta}} \left(\frac{\alpha}{r}\right)^{\frac{1-\beta}{1-\alpha-\beta}} \left(\frac{\beta}{v - \tau_j + \lambda_j \frac{\sigma^2}{2}}\right)^{\frac{\beta}{1-\alpha-\beta}} \quad (5)$$

$$w_j^* = \frac{v\lambda_j + \tau_j}{\lambda_j + \frac{\gamma_j}{1-\gamma_j}} + \frac{\gamma_j A_j}{\gamma + (1-\gamma)\lambda} K_j^{*\alpha} L_j^{*\beta} - \frac{v}{\lambda_j + \frac{\gamma_j}{1-\gamma_j}} \frac{K_j^*}{L_j^*} - \frac{\lambda_j \sigma^2}{2\lambda_j + 2\frac{\gamma_j}{1-\gamma_j}} \frac{1}{L_j^*} \quad (6)$$

## 6.2 Theoretical effects from privatization on workers in firms

The model has several interesting predictions when firm ownership changes from state-owned to private. I derive predictions under two reasonable assumptions: First, decreasing returns to scale ( $\alpha + \beta < 1$ ). Second, the outside option of workers  $v$  is higher than the political incentives per-worker that firms receive  $\tau_j$ . Within the framework, state-owned enterprises are bigger and utilize more labor and capital than private enterprises out of three reasons: First, if state-owned enterprises receive more subsidies per worker than private enterprises  $\tau_{soe} > \tau_p$ . Second, when state-owned firms are more productive  $A_{soe} > A_p$ . Third, due to introduction of risk and when state-owned enterprises are less risk averse than private ones  $\lambda_{soe} < \lambda_p$ . When state-ownership facilitates the spread of risk, state-owned enterprises employ more labor even though it increases the volatile component of



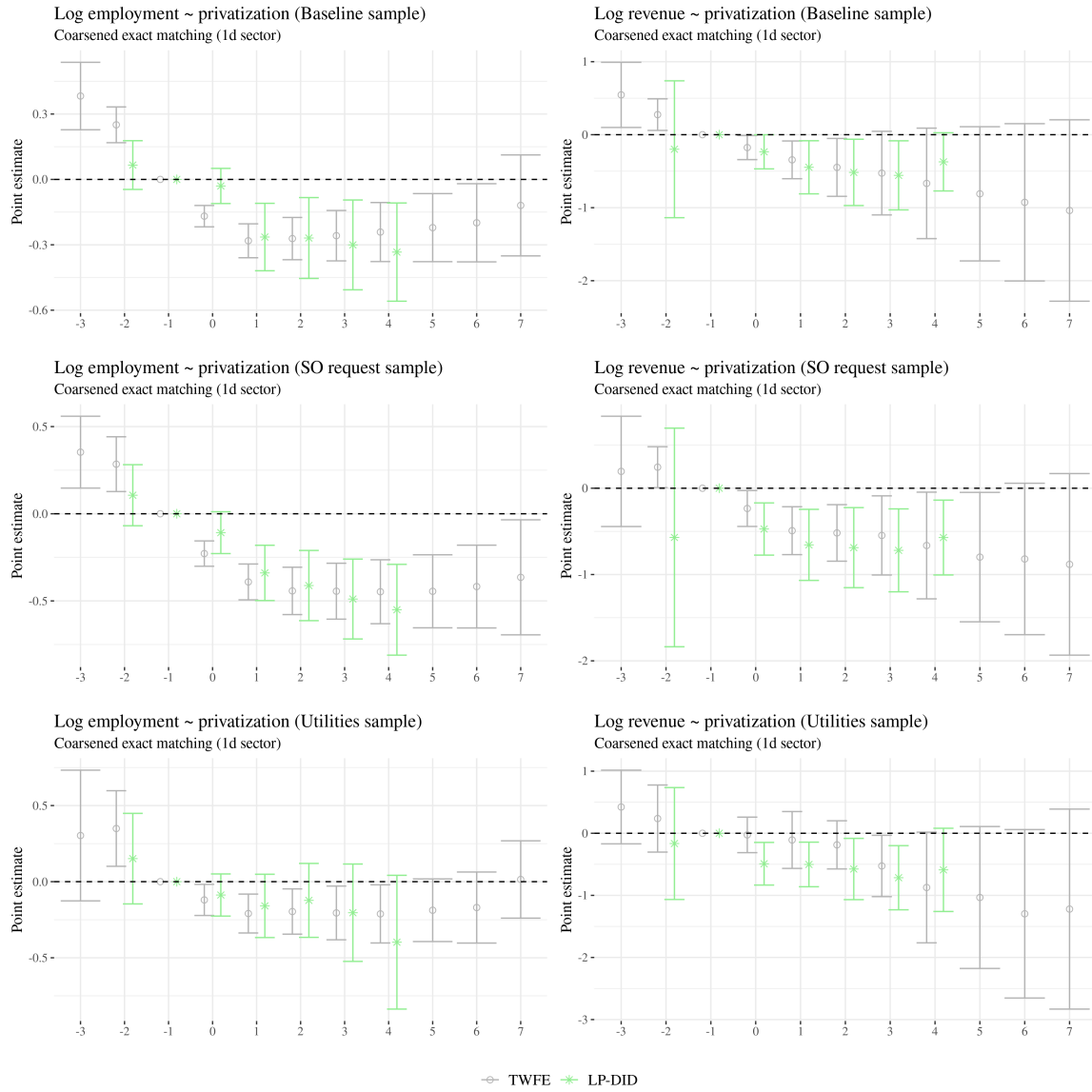
profits. Interestingly, the level of employment does not depend on the bargaining power of workers. Lastly, there are no clear predictions for wages across private and state-owned enterprises.

## 7 Conclusion

The social choice of private versus public ownership of capital was the decisive ideological battle of the 20th century. The fall of the wall in 1989 was not only a peaceful revolution against an authoritarian regime, it also epitomized the victory of capitalism over the "collective ownership of the means of production". Revisiting East Germany's transition can contribute to our understanding of how economic restructuring of state-owned enterprises impacts workers during democratization. More broadly it can inform the debate about the role of the state in structural economic transformation.

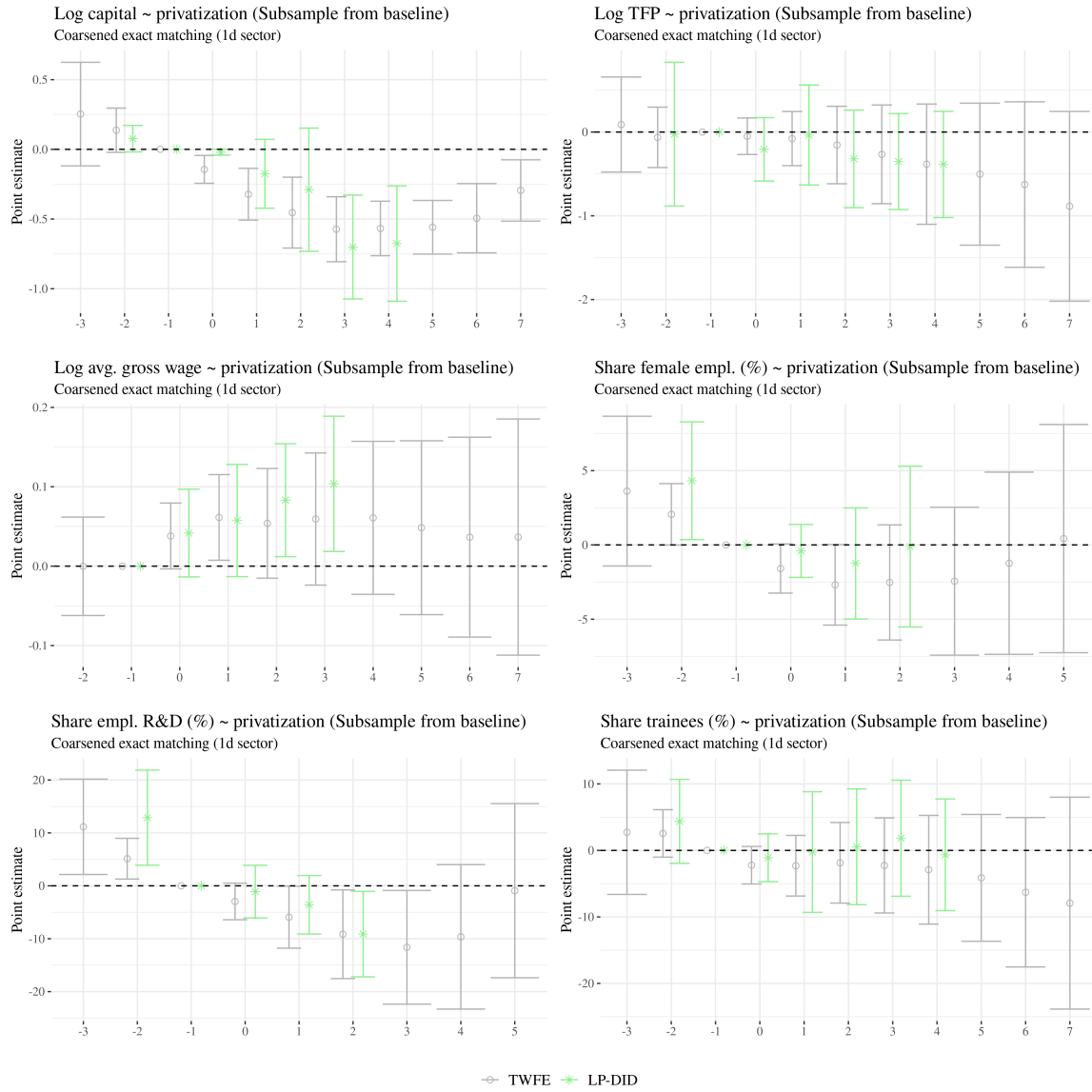
Studying privatization of East Germany's formerly state-owned enterprises through the lens of workers, I find that in spite of employment guarantees granted to the privatization agency, privatized firms separate themselves from workers. Linking privatizations to labor markets of regional industries, I find that privatizations lead to a persistent increase in unemployment and a decrease in income and life satisfaction. Even though effects are predominantly negative on workers, it remains an open question whether it would have been financially viable to extend state-ownership for longer transition periods. In ongoing work, I am gathering data on the government's budget constraint to study the implications of counterfactual restructuring policies on well-being.

Figure 7: Event study of effects of privatization on firm outcomes: Main samples



Note: The plots show estimated coefficients in grey from TWFE regression with staggered timing of privatization  $y_{jt} = \sum_{e=-K}^{-2} \gamma_e P_{jt}^e + \sum_{e=0}^L \gamma_e P_{jt}^e + \xi_j + \zeta_t + \epsilon_{jt}$  and methods to deal with effect heterogeneity developed by Callaway and Sant'Anna (2021) and Sun and Abraham (2021). The coefficient of the year before privatization is omitted for Sun and Abraham (2021)'s method and TWFE. 95% confidence intervals around the point estimate are shown.

Figure 8: Event study of effects of privatization on firm outcomes: Subsamples



Note: The plots show estimated coefficients in grey from TWFE regression with staggered timing of privatization  $y_{jt} = \sum_{e=-K}^{-2} \gamma_e P_{jt}^e + \sum_{e=0}^L \gamma_e P_{jt}^e + \xi_j + \zeta_t + \epsilon_{jt}$  and methods to deal with effect heterogeneity developed by Callaway and Sant'Anna (2021) and Sun and Abraham (2021). The coefficient of the year before privatization is omitted for Sun and Abraham (2021)'s method and TWFE. 95% confidence intervals around the point estimate are shown.

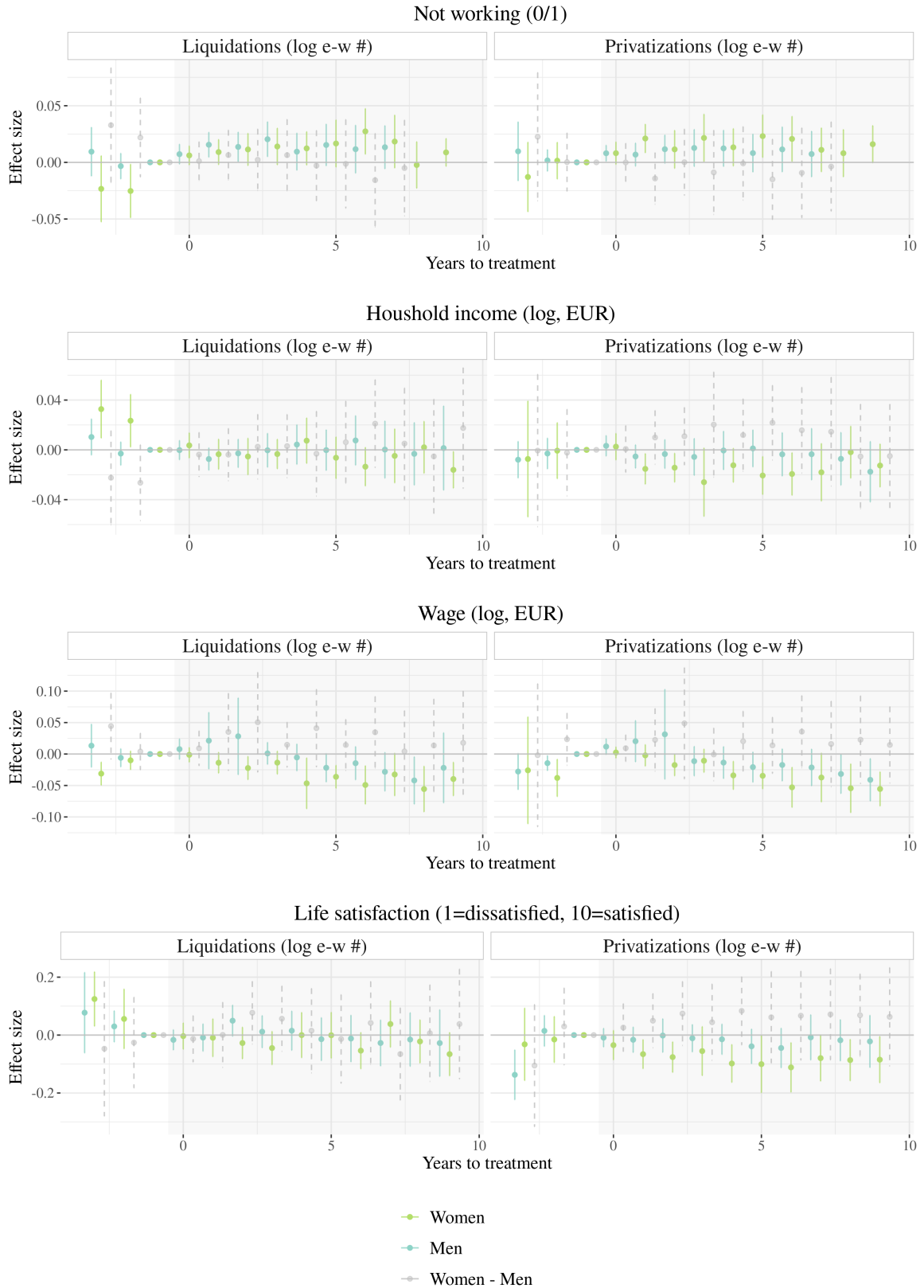
## 8 Appendix

Table 6: Summary statistics of worker sample

	mean	sd	min	max	n
<b>Outcomes</b>					
Not-working (0/1)	0.174	0.377	0	1	27978
Life satisfaction (1=dissatisfied, 10=satisfied)	6.379	1.757	0	10	27978
Household income (Log, current EUR)	7.343	0.479	0	9.696	27978
Wage (log, EUR)	6.616	0.633	0	9.569	21780
Unemployment (0/1)	0.101	0.299	0	1	27978
Public Transfers (Log, EUR)	5.734	3.603	0	10.647	22516
Health (1=bad, 5=very good)	3.485	0.843	1	5	22188
Move to services (0/1)	0.04	0.196	0	1	23263
Move to West G. (0/1)	0.008	0.088	0	1	23512
<b>Treatments</b>					
Privatizations (0/1)	0.497	0.5	0	1	28160
Liquidations (0/1)	0.36	0.48	0	1	28160
Privatizations (empl-weigh.)	1521.377	3913.664	0	44795	28160
Liquidations (empl-weigh.)	623.062	1950.602	0	35191	28160
Privatizations (log, empl-weigh.)	3.469	3.673	0	10.71	28160
Liquidations (log, empl-weigh.)	2.318	3.231	0	10.469	28160
Ever treated (0/1)	0.608	0.488	0	1	28160
<b>Characteristics</b>					
Binary gender (M=0/F=1)	0.493	0.5	0	1	28160
Age (years)	41.376	13.039	17	88	28160
College (0/1)	0.255	0.436	0	1	28146
State:					
Berlin (0/1)	0.005	0.074	0	1	28160
Brandenburg (0/1)	0.183	0.387	0	1	28160
Mecklenburg-Western Pomerania (0/1)	0.126	0.332	0	1	28160
Saxony (0/1)	0.282	0.45	0	1	28160
Saxony-Anhalt (0/1)	0.201	0.401	0	1	28160
Thuringia (0/1)	0.202	0.402	0	1	28160
Industry (1d NACE rev1.1):					
Mining, food, textiles (0/1)	0.114	0.318	0	1	28160
Wood, paper, petroleum, chemicals, metals (0/1)	0.136	0.343	0	1	28160
Machinery, vehicles, furniture, recycling (0/1)	0.045	0.207	0	1	28160
Energy, water, construction (0/1)	0.126	0.331	0	1	28160
Retail, wholesale, hotels (0/1)	0.142	0.35	0	1	28160
Transport, communication (0/1)	0.094	0.292	0	1	28160
Real estate, research, public admin (0/1)	0.138	0.345	0	1	28160
Education, health, social (0/1)	0.168	0.374	0	1	28160
Sewage, other services (0/1)	0.036	0.187	0	1	28160

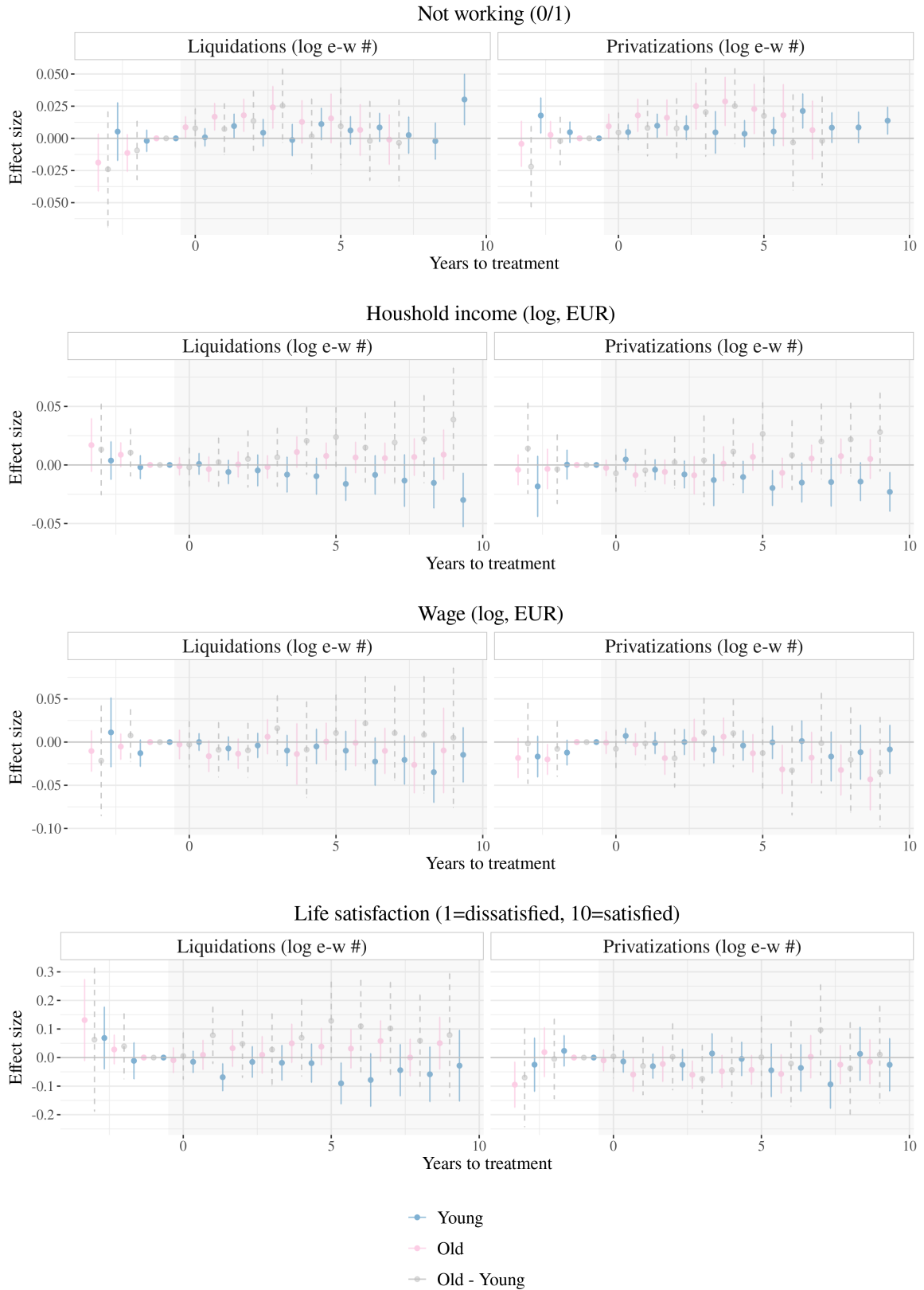
*Note:* The table summarizes the baseline sample used to estimate the impact of restructuring of state-owned enterprises on workers.

Figure 9: Impact of restructuring on workers: Women and men



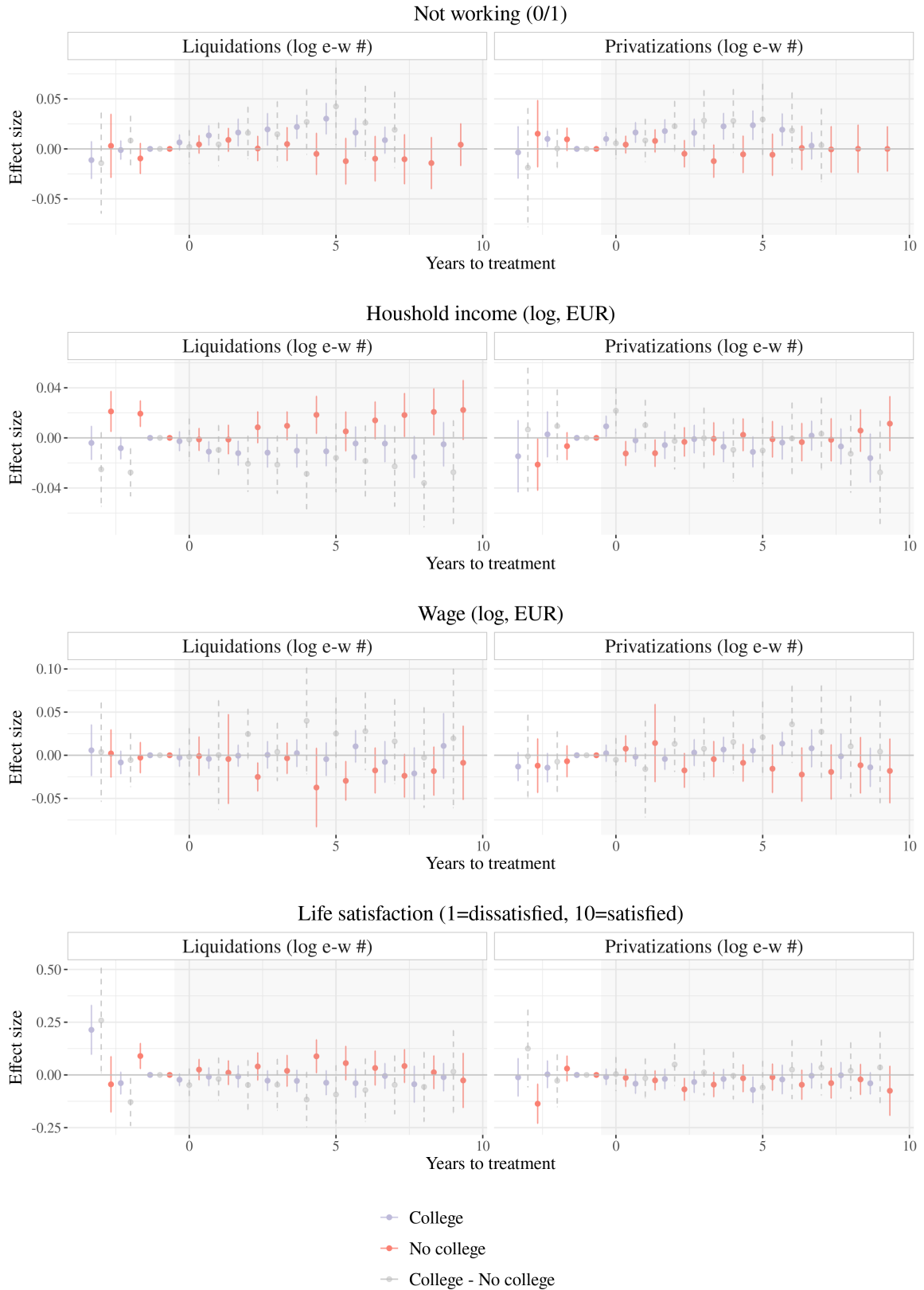
Note: The plots show coefficients of the impact of privatizations and liquidations from estimating Equation 1. 95% confidence intervals around the point estimate are shown.

Figure 10: Impact of restructuring on workers: Old and young



Note: The plots show coefficients of the impact of privatizations and liquidations from estimating Equation 1. 95% confidence intervals around the point estimate are shown.

Figure 11: Impact of restructuring on workers: Tertiary and secondary education



Note: The plots show coefficients of the impact of privatizations and liquidations from estimating Equation 1. 95% confidence intervals around the point estimate are shown.

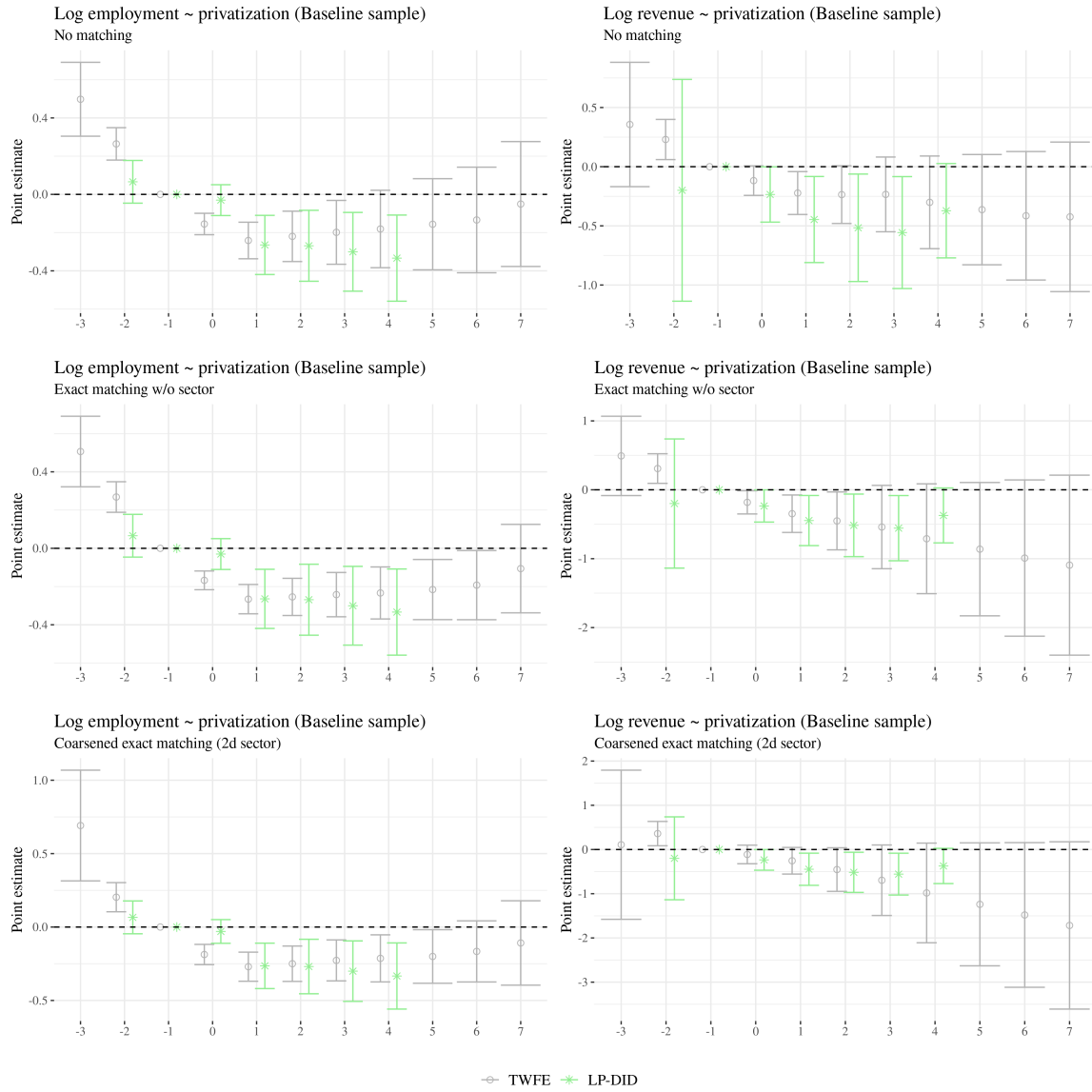
Table 7: Impact of restructuring on workers: Robustness II

		Not work.	HH inc.	Wage	Life satisf.	Health	Pub. transf.	Move serv.	Move West
		(0/1)	ln(EUR)	ln(EUR)	[1,10=satisf.]	[1,5=good]	ln(EUR)	(0/1)	(0/1)
<b>Market stayers</b>									
Privatizations (log e-w #)	$\beta_3^{lp-did}$	0.009	-0.002	-0.002	-0.005	-0.01	0.125	-	-
	$se(\beta_3^{lp-did})$	(0.006)	(0.006)	(0.007)	(0.024)	(0.026)	(0.094)	-	-
	$f(sd(P) * \beta_3^{lp-did})$	3.36pp	-0.87%	-0.9%	-0.2	-0.4	45.88%	-	-
	$n_3$	1860	1860	1469	1860	1246	1258	-	-
	$\beta_6^{lp-did}$	0.015	-0.011	-0.012	-0.053	-0.016	0.197*	-	-
	$se(\beta_6^{lp-did})$	(0.008)	(0.007)	(0.009)	(0.031)	(0.058)	(0.084)	-	-
Liquidations (log e-w #)	$\delta_3^{lp-did}$	0.007	0.000	-0.005	-0.013	-0.039	0.176	-	-
	$se(\delta_3^{lp-did})$	(0.006)	(0.006)	(0.008)	(0.023)	(0.027)	(0.127)	-	-
	$f(sd(L) * \delta_3^{lp-did})$	2.42pp	-0.09%	-1.64%	-0.4	-0.13	56.74%	-	-
	$n_3$	1850	1850	1458	1850	1257	1269	-	-
	$\delta_6^{lp-did}$	0.021*	-0.003	-0.006	-0.047	-0.056*	0.152	-	-
	$se(\delta_6^{lp-did})$	(0.008)	(0.007)	(0.01)	(0.029)	(0.024)	(0.196)	-	-
	$f(sd(L) * \delta_6^{lp-did})$	6.75pp	-1.01%	-2.05%	-0.15	-0.18	49.27%	-	-
	$n_6$	978	978	782	978	455	464	-	-
<b>Matching on 1d industry</b>									
Privatizations (log e-w #)	$\beta_3^{lp-did}$	0.011	-0.01	-0.011	-0.011	-0.075**	0.19	0.006*	0.000
	$se(\beta_3^{lp-did})$	(0.006)	(0.007)	(0.009)	(0.022)	(0.023)	(0.097)	(0.002)	(0)
	$f(sd(P) * \beta_3^{lp-did})$	4.13pp	-3.76%	-4.02%	-0.4	-0.27	69.78%	2.26pp	0.01pp
	$n_3$	1233	1233	882	1233	761	772	949	952
	$\beta_6^{lp-did}$	0.009	-0.009	-0.023**	-0.039	-0.026	0.327***	0.01***	0.000
	$se(\beta_6^{lp-did})$	(0.007)	(0.005)	(0.009)	(0.025)	(0.025)	(0.069)	(0.003)	(0)
Liquidations (log e-w #)	$\delta_3^{lp-did}$	0.008	-0.003	-0.002	0.023	-0.009	0.006	0.007**	0.001
	$se(\delta_3^{lp-did})$	(0.006)	(0.005)	(0.006)	(0.021)	(0.017)	(0.149)	(0.002)	(0.001)
	$f(sd(L) * \delta_3^{lp-did})$	2.67pp	-0.88%	-0.76%	0.07	-0.03	1.92%	2.2pp	0.37pp
	$n_3$	1223	1223	871	1223	772	783	981	984
	$\delta_6^{lp-did}$	0.003	0.000	-0.018*	0.000	-0.006	0.029	0.01***	0.000
	$se(\delta_6^{lp-did})$	(0.006)	(0.005)	(0.009)	(0.027)	(0.028)	(0.166)	(0.003)	(0)
	$f(sd(L) * \delta_6^{lp-did})$	1.01pp	-0.05%	-5.8%	0.000	-0.02	9.28%	3.35pp	-0.15pp
	$n_6$	656	656	473	656	269	277	450	450
<b>Matching on 1d industry, gender, age &amp; college</b>									
Privatizations (log e-w #)	$\beta_3^{lp-did}$	0.011	-0.01	-0.012	-0.01	-0.078**	0.166	0.006*	0.000
	$se(\beta_3^{lp-did})$	(0.007)	(0.007)	(0.009)	(0.021)	(0.025)	(0.111)	(0.002)	(0)
	$f(sd(P) * \beta_3^{lp-did})$	4.22pp	-3.82%	-4.33%	-0.4	-0.29	60.92%	2.25pp	0.01pp
	$n_3$	1236	1236	883	1236	763	774	953	956
	$\beta_6^{lp-did}$	0.009	-0.01*	-0.023**	-0.031	-0.051*	0.239*	0.01***	0.000
	$se(\beta_6^{lp-did})$	(0.007)	(0.005)	(0.009)	(0.024)	(0.025)	(0.096)	(0.003)	(0)
Liquidations (log e-w #)	$\delta_3^{lp-did}$	0.009	-0.004	-0.002	0.026	-0.009	0.006	0.007**	0.001
	$se(\delta_3^{lp-did})$	(0.006)	(0.005)	(0.006)	(0.021)	(0.017)	(0.149)	(0.002)	(0.001)
	$f(sd(L) * \delta_3^{lp-did})$	2.99pp	-1.19%	-0.79%	0.08	-0.03	1.92%	2.19pp	0.37pp
	$n_3$	1226	1226	872	1226	772	783	983	986
	$\delta_6^{lp-did}$	0.003	-0.001	-0.018*	0.01	-0.006	0.029	0.01***	0.000
	$se(\delta_6^{lp-did})$	(0.006)	(0.005)	(0.009)	(0.026)	(0.028)	(0.166)	(0.003)	(0)
	$f(sd(L) * \delta_6^{lp-did})$	1.03pp	-0.43%	-5.84%	0.03	-0.02	9.28%	3.34pp	-0.15pp
	$n_6$	659	659	473	659	269	277	452	452

Note: The table shows coefficients of the impact of privatizations and liquidations from estimating Equation 1. Significance levels are \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .



Figure 12: Event study of effects of privatization on main firm outcomes: Matching robustness



Note: The plots show estimated coefficients in grey from TWFE regression with staggered timing of privatization  $y_{jt} = \sum_{e=-K}^{-2} \gamma_e P_{jt}^e + \sum_{e=0}^L \gamma_e P_{jt}^e + \xi_j + \zeta_t + \epsilon_{jt}$  and methods to deal with effect heterogeneity developed by Callaway and Sant'Anna (2021) and Sun and Abraham (2021). The coefficient of the year before privatization is omitted for Sun and Abraham (2021)'s method and TWFE. 95% confidence intervals around the point estimate are shown.

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