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IFN Working Paper No. 1201, 2018

What is the Cost of Privatization for Workers?

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What is the Cost of Privatization for Workers?*

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ABSTRACT

The world is in the midst of a new wave of privatization, with record dollar amounts raised in both developed and developing countries. Using rich Swedish registry data covering two decades from the mid-1990s, we show that privatizations increased unemployment incidence by almost a fifth, and duration by a quarter, relative to peers who remained employed by a state-owned enterprise. This led to almost one million extra days of unemployment for the workers affected. Wages and labor force participation remain unchanged. Furthermore, we show that privatizations have been costly for workers, and therefore for society, only if they took place during recessions. These results shed new light on the welfare costs of privatization and how they can be mitigated.

Keywords: Employment, privatization, state-owned enterprises (SOEs), unemployment.

JEL Codes: F66; J24; J63; L33; M51.

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1 Introduction

The world is in the midst of a new wave of privatization, with record dollar amounts raised globally. The total value of privatizations of state-owned enterprises (SOEs) peaked at USD 320 billion in 2015 compared to only USD 24 billion in 1990, see Figure 1. Both developing and developed countries are privatizing assets. The top ten nations in privatization revenues in 2015 were China, the United Kingdom, Italy, Japan, India, Sweden, Australia, the United States, Netherlands and Ireland (Megginson, 2017). Privatizations are thus on the agenda of policymakers across the globe.

In this paper, we examine how privatizations affect labor market outcomes for workers. We provide new evidence by analyzing Swedish registry data covering all adults over two decades, and virtually all state and municipal privatizations of SOEs since the mid-1990s. Access to data for the entire population allows us to use a difference-in-difference strategy to compare the labor market outcomes for workers whose workplaces are privatized to the outcomes for comparable peers who remain employed in an SOE.

We show that privatizations in Sweden have been modestly costly for workers on average. Privatizations increased the incidence of unemployment among affected workers by almost a fifth, and the duration by a quarter, relative to peers who remained in public sector employment. Our results suggest that privatizations in Sweden led to almost one million extra days of unemployment for the workers in our sample over an eight-year period after each privatization – an non-negligible cost for workers and society.¹ We do not, however, find any large effects on labor income trajectories, or on workers leaving the labor force.

Not all privatizations are bad news for workers, however. The ability to study workers over two decades allows us to distinguish between privatizations when general economic conditions are good and bad. We show that all of the costs of privatizations on workers come from privatizations during recession years, when labor market conditions are weak. Thus

¹Research shows that unemployment is costly for workers. Workers take wage cuts after accepting a new job offer, they face expenses incurred during job search such as consumption and income loss, and they are less happy (Katz and Mayer, 1990; Jacobson, LaLonde, and Sullivan, 1993; Gruber, 1997; Di Tella, MacCullock, and Oswald, 2001; Farber, 2005).

the timing of privatizations matters a great deal in terms of the impact on workers. We also document that privatizations are more costly for men relative women, for the unskilled relative to the skilled, and for workers with weaker labor market protection.

While the costs and benefits of privatization have received substantial attention in the literature, there are few large-sample studies on how privatization affects labor market outcomes for individual workers, and none on how effects vary across the business cycle (Megginson and Netter, 2001; Estrin, Hanousek, Kočenda, and Svejnar, 2009; Earle, 2014). Most work focuses on firm-level employment. Key contributions include La Porta and de Silanes (1999), Lizal and Svejnar (2002), Jones and Simon (2005), and Brown, Earle, and Álmos Telegdy (2006). Firm-level studies are informative about the net effects of privatization on employment, but most ignore churn (exceptions are Brown and Earle (2003) and Chong, Guillen, and de Silanes (2011)). Churn is important: if we are interested in labor market outcomes for individual workers, then a net effect of zero on overall employment can hide large costs to existing workers if new hires replace them and they end up unemployed. Worker level studies are rarer (Earle, 2014). There are some pioneering small-sample studies, such as Haskel and Szymanski (1993), Kikeri (1998), and Peoples and Talley (2001), and the more recent papers of Brown, Earle, and Vakhitov (2006), Orelan (2010), Melly and Puhani (2013), and Bastos, Monteiro, and Straume (2014).

Relative to this literature, the data we use for analysis are superior in their coverage (population), duration (two decades), and detail (firm links and demographic information). We directly observe three key labor market outcomes (unemployment, labor income, and leaving the labor force), attrition is not an issue, and population data allow us to compare workers who are part of privatizations to their comparable peers who are not. The data give us more precision, and mitigate selection concerns that often arise when relying on survey data. Also, the data allow us to show that privatization is costly for workers only if it takes place during a recession. The panel dimension also allows us to study the effects of privatizations on workers in the long run, over an eight-year period following each event. Finally, evidence from a developed country with a stable institutional environment complements existing studies of

how privatization has affected workers in developing countries and central and eastern Europe.

Our paper proceeds as follows. The next section describes the institutional details, the empirical strategy and data sources. Section 3 studies the labor market effects of privatizations, while Section 4 presents evidence on economic conditions and worker effects. Section 5 provides additional analysis, and Section 6 concludes.

2 Institutional details, empirical strategy, and data sources

2.1 Institutional details

Like many other nations, Sweden experienced an expansion of the public sector after the second world war. In the mid-1980s the public sector accounted for around 37% of employment in Sweden and roughly 63% of GDP according to Statistics Sweden. With such a large public sector, politicians and the public opinion started to be worried about the efficiency and cost of the public sector. As a consequence, many privatizations of SOEs took place during the 1990s and employment in the public sector decreased from 38% in 1987 to 29% in 2016. Yet, despite the push towards privatizations, Sweden still lies above the OECD average of 21%.

2.2 Empirical strategy

Our empirical strategy is to use a difference-in-differences estimator that compares the outcomes of a treated and a control group of workers before and after privatization events. We model outcome Y of worker i at year t as:

$$Y_{it} = \alpha + \lambda_t + \gamma Treat_i + \beta DiD_{it} + \epsilon_{it} \quad (1)$$

where λ_t is time effects and $Treat_i$ is an indicator variable for workers who are employed in an SOE that is privatized one year later (treated), and, zero for workers in the control group that in the same year are employed in a SOE not privatized one year later. Finally, DiD_{it} takes the value one for treated workers in the year of the privatization and all years after, zero otherwise. Hence, a worker is defined as treated if he or she is employed in an SOE

that one year later is privatized, irrespectively of the his or her labor market status before and after this year. As a consequence, the model rules out any compositional bias because the treatment and control groups are kept constant over time. The coefficient β captures an average intention-to-treat effect because workers that we define as treated can leave the firm before the actual privatization takes place, for reasons unrelated to the privatization event. The intention-to-treat effect is smaller than the average treatment effect on the treated.

To capture short, medium and long run effects separately, we estimate the following model:

$$Y_{it} = \alpha + \tau_p + \gamma Treat_i + \beta_p \sum_p \tau_p \times Treat_i + \epsilon_{it} \quad (2)$$

where τ_p is time period effects (years -3-0, years 1-2, years 3-4 and years 5-8). The labor market outcomes, Y_{it} , that we analyze are unemployment incidence (one or more days of registered unemployment in a year), annual number of unemployment days, annual labor income, and leaving the labor force (no annual labor income nor any registered unemployment days). In all regressions, we cluster the standard errors at the local labor market level (residence municipality).

The above models provide causal estimates of the treatment effect (β) under the parallel trend assumption and the stable unit value treatment assumption (SUTVA). The parallel trend assumption requires that the treated and control groups have parallel trends in the absence of privatization. Because the counterfactual outcomes are unobservable, it is impossible to test this assumption. But we can assess the plausibility of the assumption by comparing trends before treatment. Historical parallel trends suggest that shocks in the past have similarly affected the two groups. The SUTVA assumption is likely to hold in our setting since we select controls out of the entire population of SOE employees. It is unlikely that a privatization event in one part of Sweden affects control workers in another part of Sweden.

2.3 Data sources and the control group

The models in Equation 1 and 2 hold the composition of the treatment and control groups constant over time. In practice this requires information on workers irrespectively of their

labor market status. We get this data from Statistics Sweden's LISA database. LISA is a database that includes persons older than 15 that are registered in Sweden. LISA matches workers to firms as of November every year and includes data from several government registers with the consequence that a person exits the database only by dying or moving to another country. From LISA, we extract annual information on age, gender, education, residence municipality, firm affiliation (if any), annual labor income, and the yearly number of days registered as unemployed. We also extract information on the employers' industry and ownership status.

Using firms' ownership status, we define transitions from SOE to non-SOE between two consecutive years as a privatization event.² We restrict the sample to privatizations of limited liability SOEs (aktiebolag) between 1996 and 2010, since unemployment data is available to us in LISA for 1992 to 2011. This gives us at least four pre-periods for evaluation of parallel trends before treatment. A treated worker is defined as a worker employed in a firm that is a SOE this year, and is not one next year. We do not condition on the worker remaining with the firm until next year.

Table 1 displays the distribution of privatizations in Sweden over time (Panel A) and by industry (Panel B). Our sample includes 52,468 treated workers employed in 339 firms privatized during the period 1996 to 2010. Hence, the average treated SOE in our sample employs around 155 persons one year before going private. Our sample includes privatizations in each year of the period 1996 to 2010. But the bulk of deals occur during the first part of the period, which means that we can analyze long-run effects for most of the workers in our sample. Privatizations have been most common in the Business Activities and Financial Intermediaries industry, but since these firms have tended to be small the largest number of workers affected by privatizations have been in the Transport and Telecommunication sector with almost 20 thousand treated workers.

We create the control group by randomly choosing one control worker for each treated worker. For each year, we single out all non-treated workers employed in limited liability

²We exclude a small fraction of firms that jump between private and state ownership during these years because the state ownership share varies around 50%.

SOEs with more than ten employees. We do not want to chose control workers from industries in which privatizations never happen, so we drop all non-treated workers employed in such industries. Finally, we randomly assign one non-treated worker to each treated worker. Since we perform this procedure for each year between 1996 and 2010, we obtain an imputed privatization year for the control workers. Since we cluster the standard errors at a higher aggregation level than the individual level, our estimates are not affected by a worker appearing repeatedly as a control worker.

Table 2 reports descriptive statistics for treated and control workers. The ordinary t -value is a function of the sample size and decreases by the size of the sample, so it is better to rely on the normalized t -value when sample sizes are large (Imbens and Wooldridge, 2009). A normalized t -value above 0.25 indicates a substantial difference in means. Despite our rather simple matching procedure to find control workers, the normalized t -value shows that the treated and control groups resemble each other well on average. This is an indication that firms are not selectively privatized on the basis of worker characteristics.

3 The labor market effects of privatizations

We start our examination of the labor market effects of privatizations by inspecting pre-trends for the treated and control group of workers in all our four main outcome variables: unemployment incidence, unemployment duration, labor income, and being outside of the labor force. Prior to treatment, which occurs at some point between event time zero and one, Figure 2 shows that the treated and control groups behave similar to each other in all four outcomes. Unemployment incidence, unemployment days, and out of labor force incidence continuously decline from minus three to zero while labor income increases. These patterns are a direct consequence of that we match workers at event time zero so the workers are required to have a job at this point.³ The nearly identical pre-trends in all outcome variables

³While we could match workers at time minus three to remove these downward trends, this would give workers a four year time span to leave the firm prior to the firm becoming privately owned. The intention-to-treat effect we estimate would thus likely considerably differ from the average treatment effect on the treated since so many workers would have left the firm before it went private.

combined with the similarity in means of several observable individual characteristics prior to treatment in Table 2 lends support to our identification strategy.

Figure 2 also indicates that privatizations are associated with moves to unemployment. In the post-period, unemployment incidence and unemployment duration increases for treated workers relative the control workers starting from one year after the privatization event. This effect is not transitory, but it persists over the full eight-year post-period. There is, however, no apparent large differences between the treated and control workers in terms of labor income or being outside the labor force.

These differences in means of unemployment incidence and duration are statistically and economically significantly different from each other. Table 3 reports difference-in-differences coefficients from estimating the model in Equation 1 and 2 using OLS. Column 1 in Panel A shows that mean unemployment incidence for treated workers increases by 1.5 percentage points per year during the eight-year post period and that this effect is statistically significantly different from zero at the 1% level. In comparison to the level before the privatizations, this effect corresponds to a 17.8% increase per year. Unemployment duration also increases. Column 2 shows an increase by 2.2 days on average per year which converts to a 24.7% increase in unemployment days. There is a lot of persistence in unemployment incidence and duration. Columns 1 and 2 in Panel B shows that unemployment is higher both in the short run (year one to two), medium run (year three to four), and the long run (year five to eight). The long run point estimates are also more than double the size of the short run estimates and all estimates are statistically significant at the one percent level. The economic magnitudes are quite large. Our estimates suggest about 25% more unemployment days per year for an average worker, which converts to almost one million extra unemployment days generated in the Swedish economy for the 52,468 treated workers in our sample.⁴

Columns 3 and 4 repeat the exercise for the log of annual labor income and incidence of being outside of the labor force. The top cells in Panel A confirm the impression from Figure 2 that privatizations seems to not have had any effect on average labor income or on

⁴2.2 days per year for 52,468 workers during eight years results in 923,436 days of additional unemployment days. The average annual number of unemployment days in Sweden 1992-2010 was a bit less than 100 million.

leaving the labor force. If anything, there is a weak indication (statistically significant at the ten percent level) that the incidence of being outside of the labor force decreases by 0.04 percentage points. Panel B shows that there are no short or medium run effects on either outcome, but that the small decrease in the incidence of being outside of the labor force shows up in years five to eight (still statistically significant only at the ten percent level).

Why do we observe increases in unemployment, yet there are no apparent effects on annual labor income? One possible explanation is that workers that enter unemployment have lower annual labor income, but that their decrease is offset by increases in annual labor income for workers that remain employed. Another is that the impact of increased unemployment incidence and duration is too weak to have a meaningful impact on annual labor income. An annual income loss of 2.2 days of extra unemployment out of a total number of working days of around 220 per year suggests a decrease in annual labor income of about one percent. Which is not far from the statistically insignificant decrease of 1.1 percent in Column 3 of Panel A in Table 3.

To sum up, privatizations in Sweden seems to have be associated with modest costs for workers on average in the sense that they have led to persistent increases in unemployment incidence and duration.

4 Economic conditions and privatizations

One advantage of our data is that it covers multiple upturns and downturns in the economy. This allows us to investigate if the labor market effects of privatizations vary over the cycle. Such variation could come about due to the cyclical nature of job vacancies or to cyclical variation in what types of firms are privatized. First, since privatizations are associated with increased unemployment incidence, the duration of unemployment might be higher when jobs are difficult to find. Conversely, when jobs are easy to find privatizations might not lead to unemployment as workers that lose their jobs quickly find new ones. Second, the politicians that take decide on privatizations might procrastinate in privatizing companies in which they know workers would lose their jobs. Such companies might then only be privatized when

times are bad and tough decisions must be made to raise money. Thus, we might observe worse labor market effects of privatizations when times are bad simply because the worst run companies are privatized in bad times.

Table 4 report the results when we divide the sample into privatizations that occur in upturns and those in downturns of the economy. Figure 3 reports the corresponding figures for inspecting the pre-trends in the outcome variables. We define years with a negative GDP-gap as downturn years (1997, 98, 98, 99, 2003, 05, 08, 09, 10) and the remaining years as upturn years. The government agency called the National Institute of Economic Research reports the GDP-gap, which refers to the difference between potential GDP and actual GDP.

Consistent with the hypotheses above, we do find evidence of considerable cyclicity in the labor market effects of privatizations. Columns 1 and 2 in Panel A and B reveal that unemployment incidence remains unchanged in upturns, but increases by 2.2 percentage points or about 31% in downturns. Again, the effect increases over time so that the long run effect (years five to eight) roughly double the short run effect (years one to two). The triple difference estimate in Column 3 confirms that the differences between upturns and downturns are statistically significant. Columns 4-6 in Panel A and B report the estimates for unemployment duration. We estimate increases in unemployment duration in both upturns and downturns, but the magnitude in terms of percentage points is a lot higher in downturns (2.8 percentage points vs 1.3 percentage points). Since workers that are part of privatizations in upturns have higher pre-unemployment duration the percentage difference goes the other way around (8 percentage increase compares to 10.5 percentage increase). Column 6 shows that the differences between the upturn and downturn years are statistically significantly different from each other.

While we did not find evidence on privatizations affecting labor income on average, Panels C and D Columns 1-3 show that privatizations are associated with decreases in annual labor income of 6.2% when they take place in downturns. There is also some evidence of an increase in annual labor income of about the same magnitude in upturns. The estimate in Panel C is statistically significant at the ten percent level for the full post-period while the effect in

the medium run is statistically significant at the five percent level. In upturns, fewer workers also seems to leave the labor force as the fraction outside of the labor force declines by 0.9 percentage points. For both annual labor income and being outside the labor force, Columns 3 and 6 indicate that the differences between upturn and downturn years are statistically significantly different from each other.

In sum, there is considerable evidence of cyclicalities in the labor market effects of privatizations with privatizations in economic downturns being worse for workers compared to privatizations in economic upturns.

5 Additional heterogeneity analyses

5.1 Are the labor market effects of privatizations gender biased?

Whereas private firms most often operate to primarily maximize shareholder returns, SOEs could be instructed by politicians to signal government policy to private firms on matter such as gender equality. This suggests that privatizations could affect women to a greater extent than men.

Table 5 report the results when we divide the sample into men and women. Figure 4 reports the corresponding figures for inspecting the pre-trends in the outcome variables. The columns in Panel A and B show that men and women tend to enter unemployment and stay there after privatizations to similar degrees. There is, however, effects on labor income and on leaving the labor force that differ between genders. Panels C and D show that women tend to do relatively better than men after privatizations. Their labor income increases by 3.8 percent overall and by 59 percent more than the labor income of men. Women are also less likely to be out of the labor force by 1.4 percentage points. These results are run opposite to Melly and Puhani (2013), who finds a loss of wages of 3 percent for women. They are also not consistent with Brown, Earle, and Vakhitov (2006), who do not find any gender differences in effects.

5.2 Are less skilled workers worse off?

Skilled workers may be better positioned than unskilled workers in finding a new job if they are laid off. Moreover, ownership changes may be related to investments in new technologies, which could be complementary with high skill workers (Olsson and Tåg, 2017). As such, we would expect high skilled workers to do better relative to low skilled workers.

Table 6 report the results when we divide the sample into workers with high and low skill based on their education level. Figure 5 reports the corresponding figures for inspecting the pre-trends in the outcome variables. Although Panel A reveals no statistically significant differences between skilled and unskilled workers in terms of unemployment, Panel B shows that unskilled workers do tend to have greater short and medium run increases in unemployment incidence by about one percentage point. There also clear evidence that skilled workers tend to do better over time relative to unskilled workers in terms of labor income. Panel C shows a 9.1 percent increase in wages for skilled workers which represents a 12.2 percent increase relative low skilled workers. The effect monotonically increases over time since the privatization event. There is also evidence that skilled workers are less likely to be outside of the labor force after privatizations in Columns four to six in Panel C and D. These results are consistent with Melly and Puhani (2013) and Brown, Earle, and Vakhitov (2006), who both find that less skilled workers do worse after privatizations compared to more skilled workers.

5.3 Do employment protection legislations protect workers?

Let us now consider to what extent employment protection legislation (EPL) shield workers from entering unemployment after privatization. The question is relevant because the employment protection vary across countries and as such it is informative about the external validity of our results. Sweden had in 2004 the seventh-strongest employment protection among 30 OECD countries (OECD, 2004), so if employment protection legislations protect workers our estimates could be viewed as a lower bound on the worker costs of privatizations.

We proxy employment protection status with firm-specific tenure: workers with high security are workers with more than two years of tenure. We do this because of the Swedish

employment protection legislation LAS ("Lagen om anställningsskydd") states that temporary employment contracts become permanent after two years. Since workers on permanent contracts are much harder to fire, two years of tenure come with increased labor market protections. Additionally, LAS states that firms that have shortage of work, need to follow a tenured based dismissal order when downsizing their workforce. In practice, this means that the last worker hired, should be the first worker dismissed.

Table 7 report the results when we divide the sample into workers with high and with low tenure. Figure 6 reports the corresponding figures for inspecting the pre-trends in the outcome variables. Employment protections legislations seems to play a role. Columns 1 and 2 in Panel A and B show that workers with short tenure experiences an relative increase in unemployment incidence of 2.6 percentage points whereas the effect for workers with long tenure is only 1.4 percentage points. Both estimates are statistically significant at the one percent level. In percentage terms, however, the increase is much larger for the workers with longer tenure (77% vs 19%) since their pre-mean of unemployment incidence is only 2 percentage points compared to 14 percentage points for workers with short tenure. Column 3 shows that there are statistically significant differences between workers with short and long tenure. Columns 4-6 in Panel A show there are no apparent differences in unemployment duration for workers with short and long tenure. Although workers with short tenure experience an increase in unemployment duration of 3.4 days compared to 2.3 days for workers with long tenure, the estimates are not statistically significantly different from each other. Panel B shows that the effects increase over time, both for unemployment incidence and for duration.

We also find an relative decrease in labor income for workers with shorter tenure of 7.7 percentage points, but no effects on wage for workers with long tenure, see Columns 1 and 2 in Panel C. Column 3 shows that the decrease for short tenured workers relative long term workers is statistically significantly different at the five percent level. As for unemployment, Panel D show an increase in the effect over time, rising from 5.1 percentage points in the short run to 9.3 percentage points in the long run. Columns 4 to 6 in Panels C and D

show that there are no differential effects on moves out of the labor force. In sum, there is some evidence that employment protections legislations shield workers from the negative labor market effects of privatizations.

6 Conclusions

Rich Swedish registry data covering two decades allows us to show that privatizations increased unemployment incidence by almost a fifth, and duration by a quarter, relative to peers who remained employed by a SOE. This led to almost one million extra days of unemployment for the workers affected. Wages and labor force participation remain unchanged. Furthermore, we show that privatizations have been costly for workers, and therefore for society, only if they took place during recessions. We also document that privatizations are more costly for men relative women, for the unskilled relative the skilled, and for workers with weaker labor market protections. These results shed new light on the welfare costs of privatization and how they can be mitigated.

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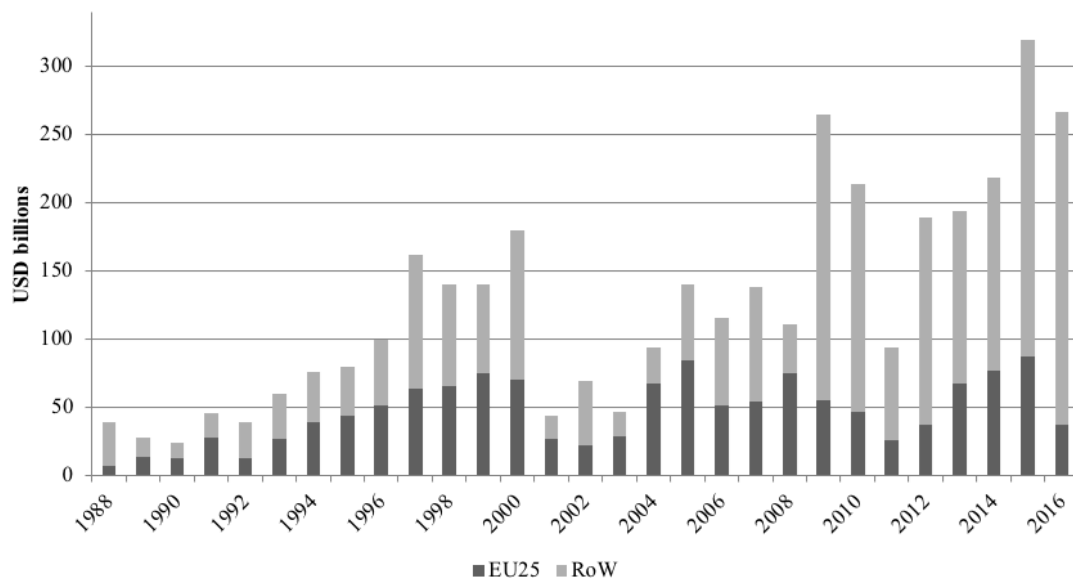


Figure 1: Global privatization revenues 1988-2016. The figure displays the total dollar value of privatizations worldwide with numbers taken from Megginson (2017).

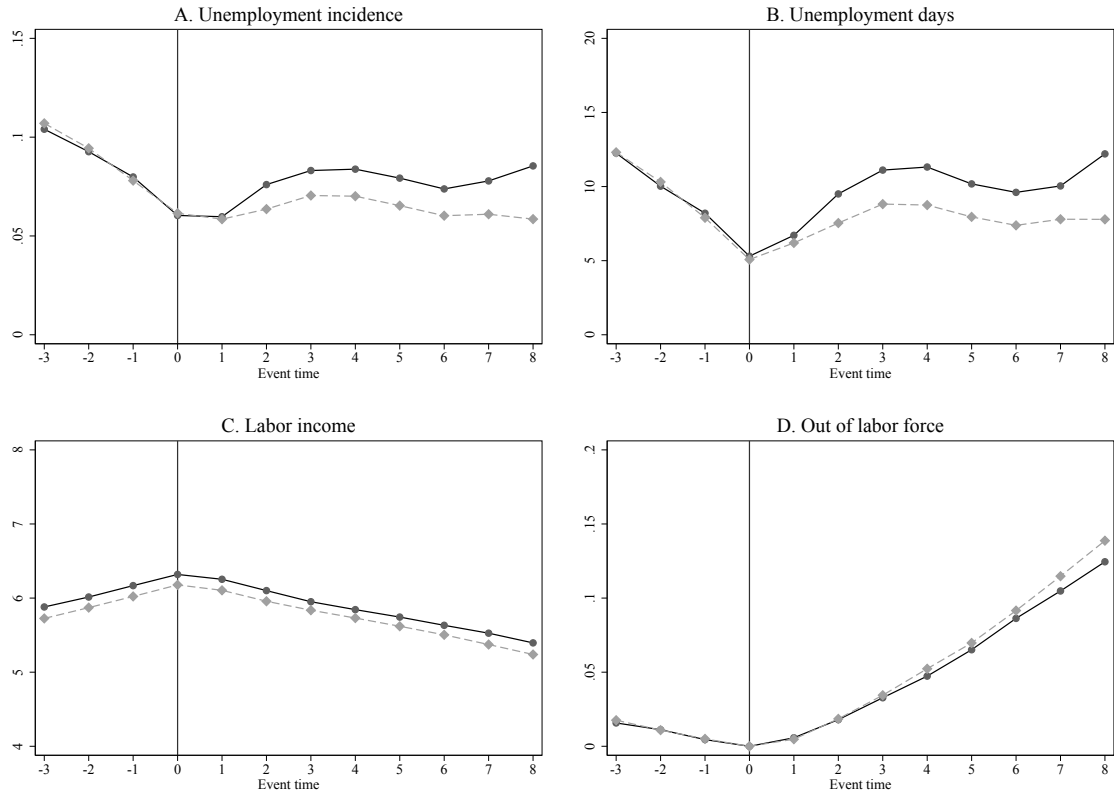


Figure 2: Trends in unemployment incidence. The figures plot mean unemployment incidence (A), mean unemployment days (B), mean log labor income (using the log inverse hyperbolic transformation) (C) and share of workers being out of the labor force (D) against event time in years around privatizations (horizontal axis). Workers in privatized firms marked with continuous lines, and control workers with dotted lines.

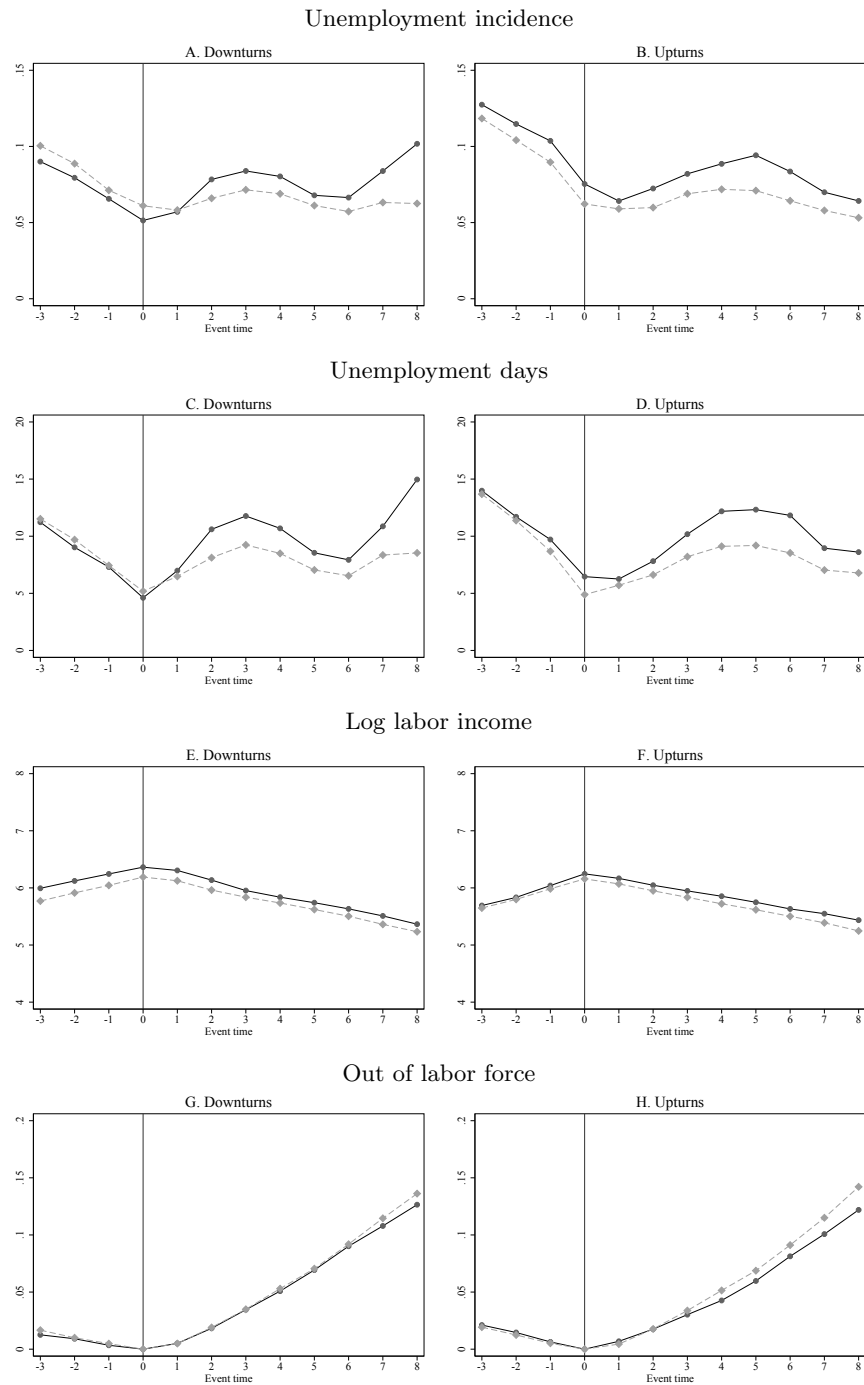


Figure 3: Trends in subgroups by year of privatization. These figures plot mean outcome (vertical axis) against event time in years around privatizations (horizontal axis). The outcome is unemployment incidence in figures A and B, unemployment days in figures C and D, log labor income (using the log inverse hyperbolic transformation) in figures E and F, and share of workers being out of the labor force in figure G and H. All years with a positive GDP-gap are defined as upturn years (2000, 01, 02, 04, 06, 07), and all years with a negative GDP-gap as downturn years (1997, 98, 98, 99, 2003, 05, 08, 09, 10). Workers in privatized firms are marked with continuous lines, and control workers with dotted lines.

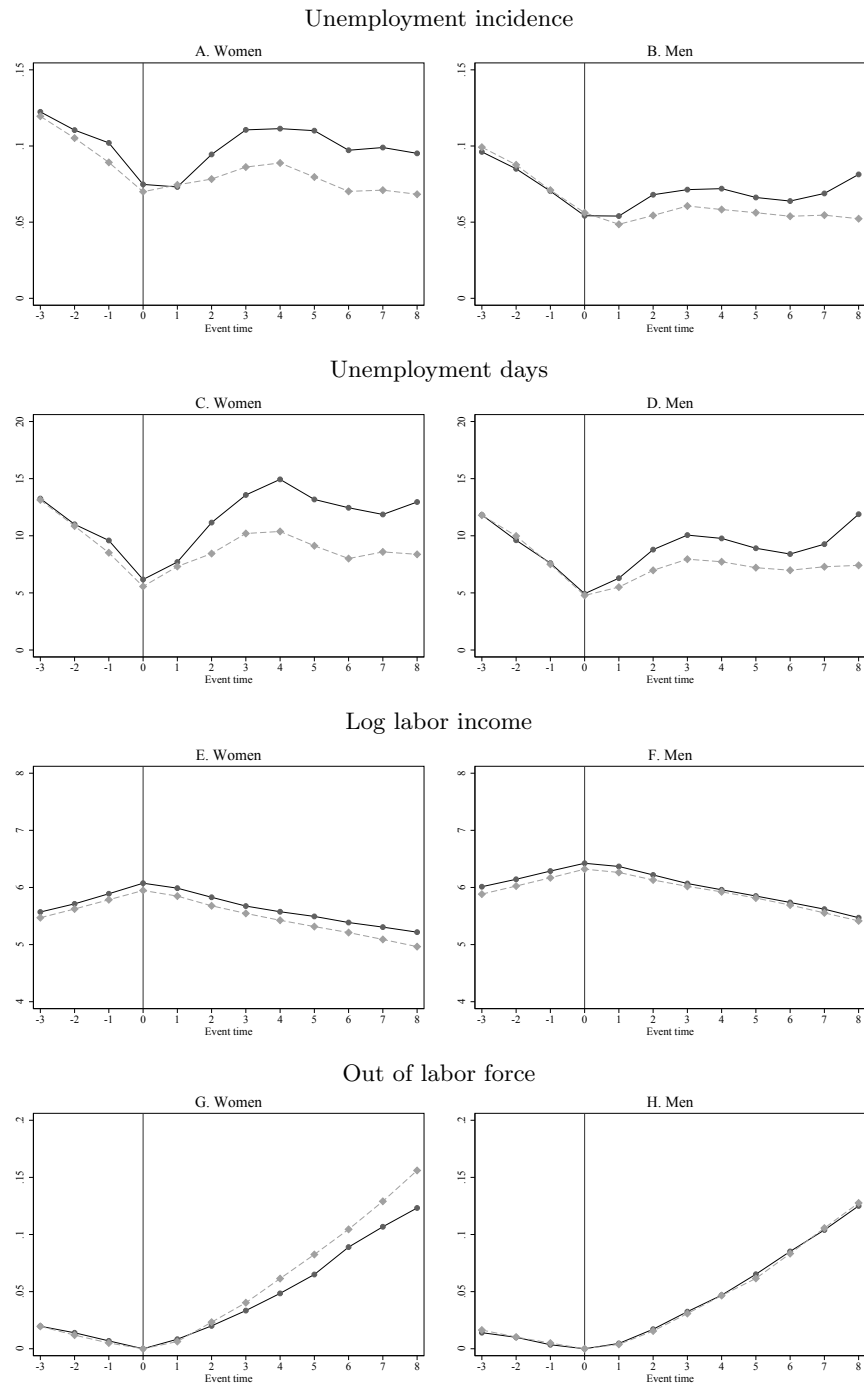


Figure 4: Trends for women and men. These figures plot mean outcome (vertical axis) against event time in years around privatizations (horizontal axis). The outcome is unemployment incidence in figures A and B, unemployment days in figures C and D, log labor income (using the log inverse hyperbolic transformation) in figures E and F, and share of workers being out of the labor force in figure G and H. Workers in privatized firms are marked with continuous lines, and control workers with dotted lines.

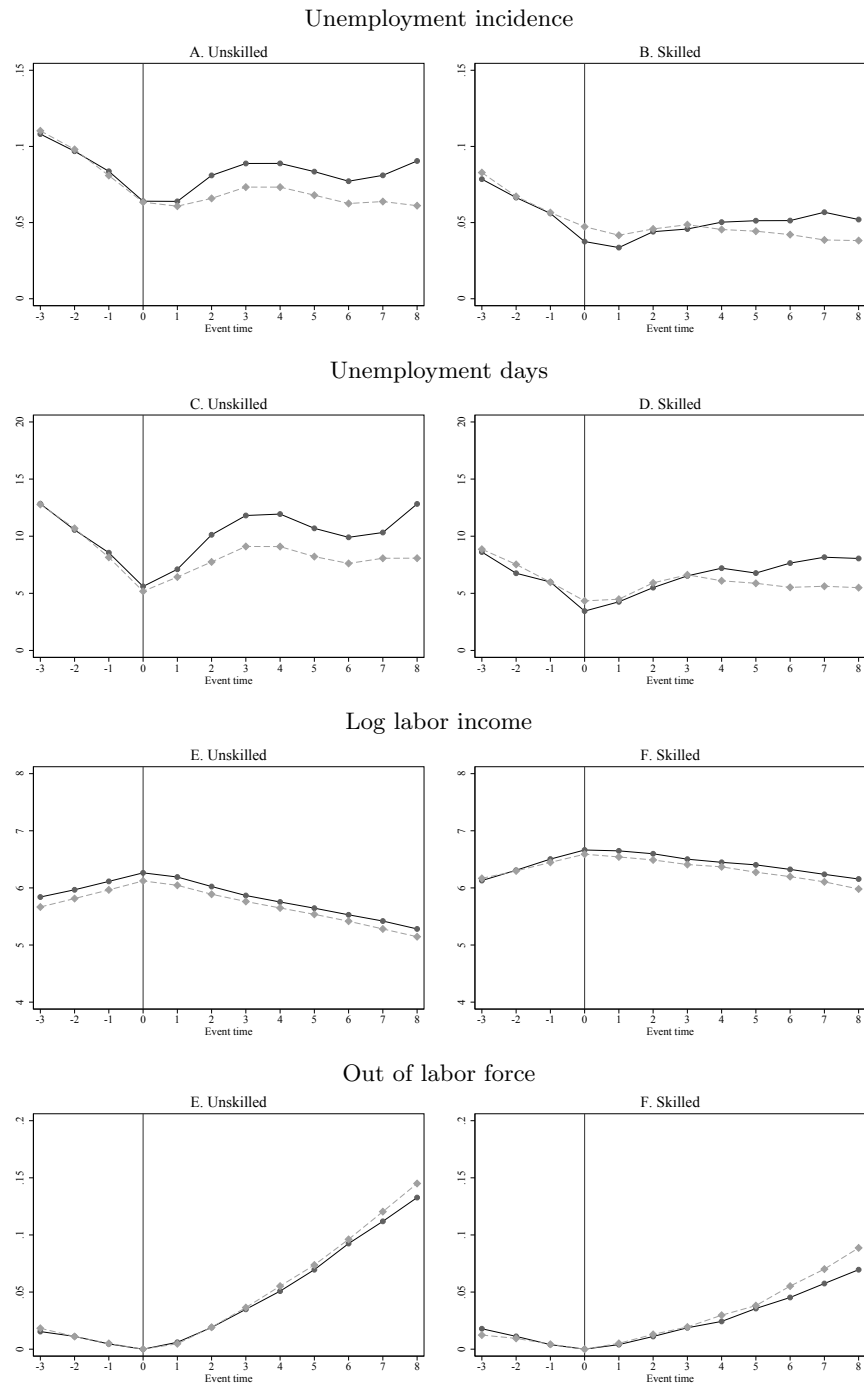


Figure 5: Trends for unskilled and skilled workers. These figures plot mean outcome (vertical axis) against event time in years around privatizations (horizontal axis). The outcome is unemployment incidence in figures A and B, unemployment days in figures C and D, log labor income (using the log inverse hyperbolic transformation) in figures E and F, and share of workers being out of the labor force in figure G and H. Workers are defined as unskilled if they have less than two years of post secondary education, skilled otherwise. Workers in privatized firms are marked with continuous lines, and control workers with dotted lines.

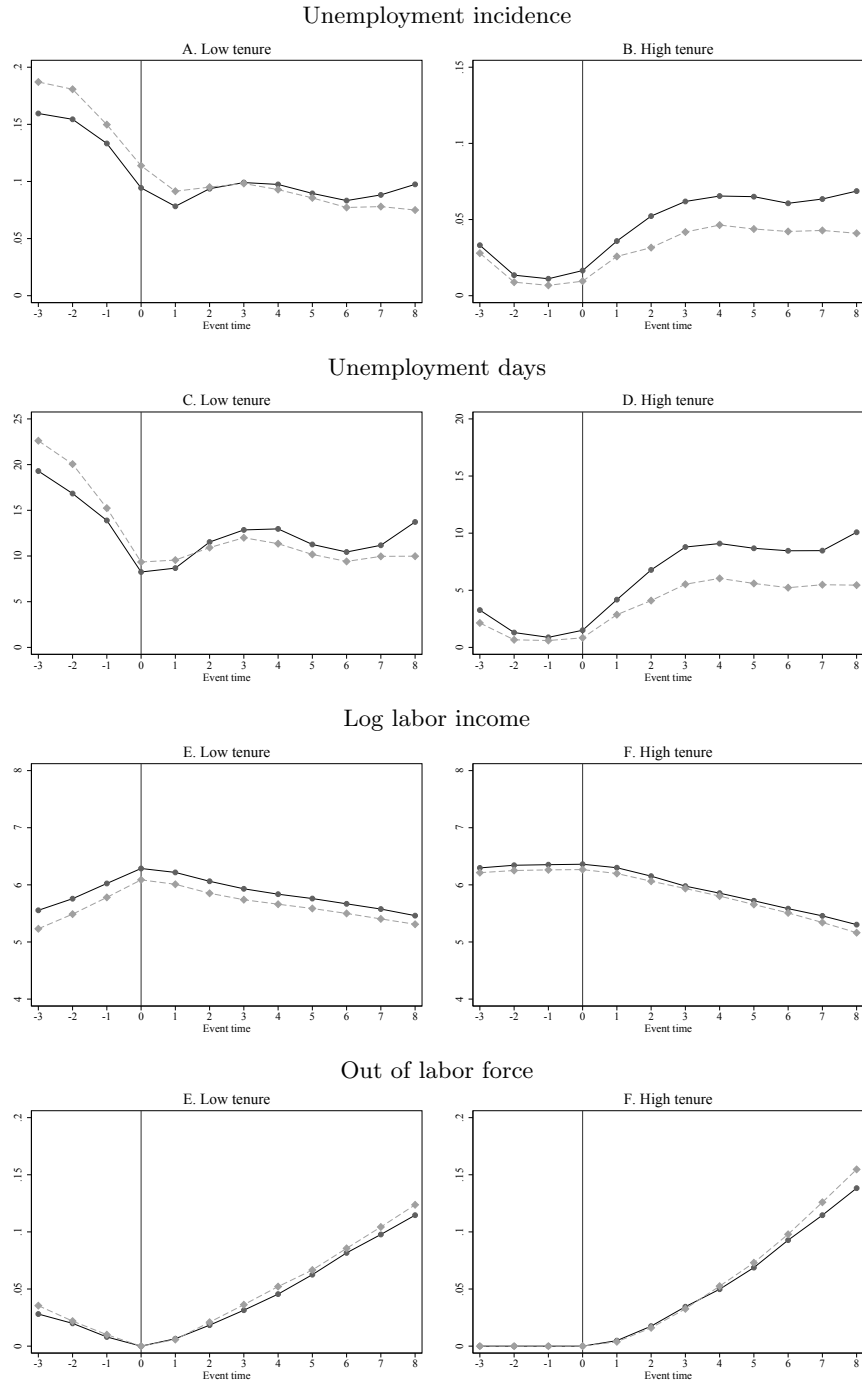


Figure 6: Trends for workers with low and high tenure. These figures plot mean outcome (vertical axis) against event time in years around privatizations (horizontal axis). The outcome is unemployment incidence in figures A and B, unemployment days in figures C and D, log labor income in figures E and F, and share of workers being out of the labor force in figure G and H. A person is defined as having high tenure if having more than two years of tenure, low tenure otherwise. Workers in privatized firms are marked with continuous lines, and control workers with dotted lines.

Table 1: Privatizations vary over time and across industries

The sample consists of all privatizations of limited liability firms (aktiebolag) with more than ten employees undertaken between 1996 and 2010. We identify privatizations through changes in the Statistics Sweden institutional sector codes for firms. Industry classification is based on NACE Rev 1.1. Workers and firms are linked in November each year.

Panel A: Privatizations by year

Year	Firms	Workers	Year	Firms	Workers
1996	57	9,499	2004	9	417
1997	32	2,813	2005	1	32
1998	18	867	2006	4	84
1999	61	8,564	2007	5	1,052
2000	23	2,841	2008	15	1,108
2001	43	7,690	2009	12	2,159
2002	40	12,205	2010	15	2,695
2003	4	442			
				Σ 339	Σ 52,468

Panel B: Privatizations by industry

Industry	Firms	Workers
Agriculture	1	62
Mining, Manufacturing and Utilities	78	12,671
Construction	14	790
Wholesale, Retail and Repair	10	518
Hotels and Restaurants	16	1,666
Transport and Telecommunications	63	19,922
Business Activities and Financial Intermediary	128	14,395
Education	3	78
Public Administration, Health and Social Work	11	1,141
Community Social and Personal Activities	15	1,225
	Σ 339	Σ 52,468

Table 2: Treated and control workers resemble each other

This table compares the mean difference between treated and control workers in various variables. Treated are workers employed one year before a privatization of an SOE and control are workers in SOEs that are not privatized. Means are from the year before the SOE was privatized and displayed in Column 1 and 2. Their difference in Column 3, with the corresponding t-test in Column 4 and normalized t-test in Column 5. A normalized t-test above 0.25 indicates substantial differences in means. See Imbens and Wooldridge (2009) for more information. Labor income refer to total annual labor income in thousands of SEK in 2005 (1 SEK \approx USD 0.12). High education defined as having at least two years of post-secondary education, and, Tenured as having been employed more than two years in the same firm based on Statistics Swedens dynamic worker flow identifiers (FAD-codes).

	Treated (1)	Control (2)	Difference (3)	T-value (4)	Norm. T-value (5)	Obs. treated (6)	Obs. control (7)
Age	42.78	42.40	-0.39	-0.92	-0.025	52,468	52,468
Labor income (k-SEK)	283.15	321.75	38.60	12.63	0.16	52,468	52,468
Share females	0.38	.030	-0.08	-9.57	-0.12	52,468	52,468
Share high skilled	0.12	0.14	0.02	2.75	0.04	52,468	52,468
Share tenured	0.50	0.44	-0.07	-3.99	-0.09	52,468	52,468

Table 3: Privatizations increase unemployment

This table reports selected coefficients from an OLS regression explaining worker-year outcomes around privatizations. The sample includes worker level information for three years before and up to eight years after the privatization event and covers the years 1992 to 2011. Panel A presents results (the β coefficient) using the model in Equation 1 and Panel B presents results using the model in Equation 2. The percent change uses mean for workers in privatized firms over three years before the privatization event as the baseline. The standard errors clustered at the local labor market level (residence municipality). *** denotes statistical significance at the 1% level.

	Unemp. incidence (1)	Unemp.days (2)	Log labor inc. (3)	Out of the labor force (4)
<u>Panel A: Average</u>				
Year 1 to 8	0.015*** (0.001)	2.211*** (0.157)	-0.011 (0.026)	-0.004* (0.003)
R^2	0.003	0.002	0.03	0.044
%-effect	17.8%	24.7%	-1.1%	-50%
<u>Panel B: Dynamics</u>				
Year 1 to 2	0.008*** (0.001)	1.172*** (0.220)	0.000 (0.015)	0.001 (0.001)
Year 3 to 4	0.014*** (0.001)	2.381*** (0.227)	-0.032 (0.020)	-0.003 0.002
Year 5 to 8	0.019*** (0.001)	2.725*** (0.186)	-0.006 (0.038)	-0.008* (0.004)
R^2	0.001	0.001	0.024	0.039
Pre-mean	0.084	8.941	6.096	0.008
Observations	1,162,663	1,162,663	1,162,663	1,162,663

Table 4: Outcomes for privatizations during economic up- and downturns

This table presents results for privatizations occurring in economic up- and downturn years. All years with a positive GDP-gap defined as upturn years (2000, 01, 02, 04, 06, 07), and all years with a negative GDP-gap as downturn years (1997, 98, 98, 99, 2003, 05, 08, 09, 10). The GDP-gap refers to the difference between potential GDP and actual GDP reported by the government agency National Institute of Economic Research (NIER). DiD refers to a difference-in-differences model and DiDiD refers to a triple difference model that is used to compare the two difference-in-differences in the two adjacent columns to the left. *Average* refers to the results (the β coefficient) using the model in Equation 1 and *Dynamics* presents results using the model in Equation 2. *** denotes statistical significance at the 1% level.

Outcome Sample Model	Unemployment incidence			Unemployment days		
	Upturns DiD (1)	Downturns DiD (2)	Both DiDiD (3)	Upturns DiD (4)	Downturns DiD (5)	Both DiDiD (6)
<u>Panel A: Average</u>						
Year 1 to 8	0.002 (0.004)	0.022*** (0.003)	0.020*** (0.005)	1.304** (0.616)	2.787*** (0.382)	1.483** (0.746)
%-effect	2.3%	30.6%		12.5%	34.7%	
<u>Panel B: Dynamics</u>						
Year 1 to 2	-0.003 (0.005)	0.014*** (0.003)	0.017*** (0.006)	0.065 (0.717)	1.869*** (0.424)	1.804** (0.832)
Year 3 to 4	0.003 (0.004)	0.021*** (0.003)	0.017*** (0.005)	1.712*** (0.611)	2.788*** (0.513)	1.076 (0.832)
Year 5 to 8	0.005 (0.005)	0.028*** (0.003)	0.023*** (0.006)	1.736** (0.674)	3.364*** (0.448)	1.627** (0.823)
Pre-mean Observations	0.105 461,312	0.072 701,351		10.5 461,312	8.0 701,351	
	1,162,663					1,162,663
Outcome	Log labor income			Out of labor force		
<u>Panel C: Average</u>						
Year 1 to 8	0.074* (0.043)	-0.062*** (0.020)	-0.136*** (0.038)	-0.009** (0.004)	-0.001 (0.002)	0.008** (0.003)
%-effect	7.4%	-6.2%		-87.1%	-16.4%	
<u>Panel D: Dynamics</u>						
Year 1 to 2	0.041 (0.026)	-0.0233* (0.013)	-0.640** (0.025)	-0.000 (0.002)	0.001* (0.000)	0.001 (0.002)
Year 3 to 4	0.066** (0.030)	-0.093*** (0.020)	-0.159*** (0.032)	-0.007*** (0.003)	0.000 (0.002)	0.008*** (0.003)
Year 5 to 8	0.100 (0.065)	-0.070*** (0.028)	-0.165*** (0.054)	-0.015** (0.006)	-0.003 (0.003)	0.011** (0.005)
Pre-mean Observations	5.953 461,312	6.181 701,351		0.011 461,312	0.006 701,351	
	1,162,663					1,162,663

Table 5: Privatizations are more costly for men compared to women

This table presents results for privatizations separately for men and women. DiD refers to a difference-in-differences model and DiDiD refers to a triple difference model that is used to compare the two difference-in-differences in the two adjacent columns to the left. *Average* refers to results (the β coefficient) using the model in Equation 1 and *Dynamics* presents results using the model in Equation 2. *** denotes statistical significance at the 1% level.

Outcome Sample Model	Unemployment incidence			Unemployment days			
	Men	Women	Both	Men	Women	Both	
	DiD	DiD	DiDiD	DiD	DiD	DiDiD	
	(1)	(2)	(3)	(4)	(5)	(6)	
<u>Panel A: Average</u>							
Year 1 to 8	0.015*** (0.002)	0.015*** (0.004)	-0.000 (0.004)	2.026*** (0.341)	2.864*** (0.578)	0.839 (0.341)	
%-effect	19.9%	14.4%		23.9%	28.6%		
<u>Panel B: Dynamics</u>							
Year 1 to 2	0.012*** (0.002)	0.001 (0.004)	- 0.011** (0.004)	1.305*** (0.380)	1.035* (0.586)	-0.270 (0.551)	
Year 3 to 4	0.014*** (0.003)	0.017*** (0.005)	0.003 (0.004)	2.101*** (0.453)	3.470*** (0.698)	1.369 (0.875)	
Year 5 to 8	0.018*** (0.003)	0.022*** (0.003)	0.004 (0.005)	2.408** (0.382)	3.601*** (0.683)	1.193* (0.677)	
Pre-mean Observations	0.076 764,382	0.102 398,281	1,162,663	10.0 764,382	8.5 398,281	1,162,663	
Outcome	Log labor income			Out of labor force			
	<u>Panel C: Average</u>						
Year 1 to 8	-0.056** (0.027)	0.066** (0.031)	0.122*** (0.026)	0.002 (0.003)	-0.014*** (0.003)	-0.016*** (0.003)	
%-effect	6.6%	-5.6%		-139.2%	25.3%		
<u>Panel D: Dynamics</u>							
Year 1 to 2	-0.021 (0.017)	0.0380** (0.017)	0.587*** (0.019)	0.002** (0.001)	-0.002 (0.000)	-0.004** (0.002)	
Year 3 to 4	-0.074*** (0.022)	0.0332 (0.024)	0.107*** (0.024)	0.002 (0.002)	-0.011*** (0.002)	-0.013*** (0.003)	
Year 5 to 8	-0.067 (0.038)	0.100*** (0.046)	0.166*** (0.036)	0.001 (0.004)	-0.023*** (0.005)	-0.024*** (0.005)	
Pre-mean Observations	6.181 764,382	5.953 398,281	0.006 1,162,663	0.011 764,382	0.011 398,281	1,162,663	

Table 6: High skill workers do better than low skill workers

This table presents results for privatizations separately for high skilled and low skilled workers. High skilled workers are workers with more than two years of post-secondary education. DiD refers to a difference-in-differences model and DiDiD refers to a triple difference model that is used to compare the two difference-in-differences in the two adjacent columns to the left. *Average* refers to results (the β coefficient) using the model in Equation 1 and *Dynamics* presents results using the model in Equation 2. *** denotes statistical significance at the 1% level.

Outcome Sample Model	Unemployment incidence			Unemployment days		
	Unskilled DiD (1)	Skilled DiD (2)	Both DiDiD (3)	Unskilled DiD (4)	Skilled DiD (5)	Both DiDiD (6)
<u>Panel A: Average</u>						
Year 1 to 8	0.015*** (0.003)	0.008** (0.004)	-0.007 (0.005)	2.315*** (0.371)	1.457** (0.575)	-0.858 (0.697)
%-effect	17.5%	13.9%		24.7%	23.5%	
<u>Panel B: Dynamics</u>						
Year 1 to 2	0.009*** (0.003)	-0.001 (0.003)	- 0.010** (0.004)	1.317*** (0.429)	0.167 (0.562)	-1.150 (0.728)
Year 3 to 4	0.016*** (0.003)	0.005 (0.005)	-0.011* (0.004)	2.590*** (0.402)	0.971 (0.855)	-1.619* (0.964)
Year 5 to 8	0.019*** (0.003)	0.016*** (0.005)	-0.003 (0.006)	2.747** (0.436)	2.502*** (0.598)	-0.244 (0.750)
Pre-mean	0.09	0.06		9.4	6.2	
Observations	1,017,251	145,412	1,162,663	1,017,251	145,412	1,162,663
<hr/>						
Outcome	Log labor income			Out of labor force		
<u>Panel C: Average</u>						
Year 1 to 8	-0.031 (0.026)	0.091*** (0.029)	0.122*** (0.027)	-0.003 (0.003)	-0.008*** (0.003)	-0.005* (0.003)
%-effect	-3.1%	9.1%		-40.5%	-97.6%	
<u>Panel D: Dynamics</u>						
Year 1 to 2	-0.015 (0.014)	0.080** (0.016)	0.095*** (0.015)	0.001 (0.001)	-0.003* (0.002)	-0.005*** (0.002)
Year 3 to 4	-0.050*** (0.019)	0.060** (0.029)	0.110*** (0.029)	-0.002 (0.002)	-0.005* (0.003)	-0.003 (0.003)
Year 5 to 8	-0.031 (0.037)	0.114*** (0.043)	0.145*** (0.042)	-0.006 (0.004)	-0.013*** (0.006)	-0.006 (0.005)
Pre-mean	6.181	5.953	0.006		0.011	
Observations	1,017,251	145,412	1,162,663	1,017,251	145,412	1,162,663

Table 7: Labor market protections shield workers from unemployment

This table report results separately for different subsamples based on employment protection status. We proxy employment protection status with tenure. We define a person as having long tenure if having more than two years of tenure, short tenure otherwise. This is based on the Swedish employment protection law which states that temporary employment contracts become permanent automatically after two years of continuous employment. Workers on permanent contracts are much harder to fire than workers on temporary employment contracts. DiD refers to a difference-in-differences model and DiDiD refers to a triple difference model that is used to compare the two difference-in-differences in the two adjacent columns to the left. Statistical significance at the 1% level is denoted by ***, statistical significance at the 5% level is denoted by **, and statistical significance at the 10% level is denoted by *.

Outcome Sample Model	Unemployment incidence			Unemployment days		
	Short DiD (1)	Long DiD (2)	Both DiDiD (3)	Short DiD (4)	Long DiD (5)	Both DiDiD (6)
<u>Panel A: Average</u>						
Year 1 to 8	0.026*** (0.005)	0.014*** (0.003)	-0.012** (0.005)	3.359*** (0.639)	2.301*** (0.377)	-1.062 (0.741)
%-effect	19.4%	76.5%		23.1%	132.5%	
<u>Panel B: Dynamics</u>						
Year 1 to 2	0.015*** (0.003)	0.0010*** (0.003)	- 0.005 (0.005)	2.102*** (0.650)	1.295*** (0.408)	-0.807 (0.728)
Year 3 to 4	0.025*** (0.003)	0.014*** (0.005)	-0.011* (0.006)	3.484*** (0.592)	2.478*** (0.530)	-1.007 (0.964)
Year 5 to 8	0.033*** (0.003)	0.017*** (0.003)	-0.016*** (0.006)	4.015*** (0.739)	2.799*** (0.438)	-1.216 (0.847)
Pre-mean Observations	0.14 625,754	0.02 536,909	1,162,663	14.6 625,754	1.7 536,909	1,162,663
Outcome	Log labor income			Out of labor force		
	<u>Panel C: Average</u>					
Year 1 to 8	-0.077** (0.036)	-0.005 (0.022)	-0.077** (0.036)	-0.002 (0.003)	-0.004* (0.002)	-0.002 (0.003)
%-effect	-7.7%	-0.5%		-12.5%		
<u>Panel D: Dynamics</u>						
Year 1 to 2	-0.051** (0.024)	0.006 (0.011)	0.057** (0.023)	0.002 (0.002)	0.001 (0.001)	-0.001 (0.002)
Year 3 to 4	-0.076*** (0.027)	-0.044* (0.022)	0.33 (0.031)	-0.003 (0.002)	-0.000 (0.002)	0.002 (0.003)
Year 5 to 8	-0.093* (0.047)	0.009 (0.034)	0.102*** (0.037)	-0.003 (0.005)	-0.009*** (0.004)	-0.006 (0.004)
Pre-mean Observations	5.9 625,754	6.3 536,909	0.014 1,162,663	625,754	0 536,909	1,162,663