

# How Costly are Privatizations for Workers?\*

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## Abstract

The costs and benefits of privatizations have received substantial attention in the literature, yet evidence on how individual workers are affected is scarce. We use matched employer-employee data to track workers' careers in terms of unemployment around privatizations. Relative to state-employed workers, privatizations increase unemployment incidence by about a fifth and duration by almost a third. Consistent with technological upgrading following privatizations, lower skilled workers, older workers, and worker performing routine-intensive job tasks take greater hits to their careers. Women and workers with weaker labor market protections are also hurt more than men and more protected workers.

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## **1. Introduction**

The costs and benefits of privatizations have received substantial attention in the literature. Yet, we still know little about how they affect individual workers employed in privatized firms. This paper characterizes the consequences of privatizations for individual workers' careers in terms unemployment. We track workers involved privatizations of state owned enterprises (SOEs) and similar workers employed by the state using extraordinary rich data on the entire population of Swedes. Figure 1 shows that over an eight year period, privatizations increase unemployment incidence by about a fifth and duration by almost a third relative to that of similar workers remaining employed by the state. This increase leads to a convergence in unemployment incidence and duration to that of a random sample of workers in the private sector.

These average effects masks considerable heterogeneity. First, we find evidence of technological upgrades following privatizations consistent with delayed such upgrades in state owned firms relative private sector firms. Lower skilled workers, older workers, and worker performing routine-intense job tasks take greater hits to their careers. Second, we find evidence of a greater weight put on gender equality in state owned firms relative private sector firms. Although both men and women experience increases in unemployment incidence and duration, the effects are relatively larger for women compared to men. Finally, we highlight heterogeneity in terms of effects depending on employment protections suggesting that at least part of the effects we find are due to involuntary separations. Workers with longer tenure are due to labor laws in Sweden harder to fire, and we also find that these are the workers that are at a relatively higher risk of unemployment after privatizations.

The evidence we present here helps cast light on the costs and benefits of privatizations, and is helpful in evaluating theories of how SOEs differ from private firms.<sup>1</sup> The reason is that ex-ante, the overall career consequences of privatizations are not obvious. First, there is ample empirical evidence that privatizations tends to improve firm performance (Megginson and Netter 2001). Such improvements can come about due to cost cutting through layoffs or due to productivity enhancing investments that are complements to labor inputs (Boycko, Shleifer, and Vishny 1996; Aghion and Blanchard 1998). These can triggered by privatization events (Roland and Sekkat 2000). In the former case, unemployment risk is likely amplified by privatizations whereas in the latter case unemployment risk can decrease if expansion takes place to new markets and more labor inputs are needed (Frydman et al. 1999).

Second, SOEs and private firms are likely to differ substantially in their human resource management practices due to different objective functions. Whereas private firms most often operate to primarily maximize shareholder returns, SOEs often have other objectives as well. These include a preference for politicians to keep unemployment levels low, a preference for letting government owned firms signal government policy to private firms on matter such as gender equality, and the possibility of nepotism in which lower ability workers are given jobs they otherwise would not quality for. All of these are perks for some types of workers, and these perks may disappear in privatizations leading to an increased risk of unemployment. Simultaneously, employed workers not receiving these perks may see a decreased risk of unemployment following privatizations.

Finally, privatization events in themselves can help make or break careers. They can present an opportunity for management to advance their careers and take over the firm and they may yield entrepreneurial opportunities for workers as the state goes from providing

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<sup>1</sup> The privatization literature is today extensive. Megginson and Netter (2001), Djankov and Murrell (2002), and Estrin et al. (2009) provide detailed surveys of this literature.

services to purchasing them. These mechanism can decrease the risk of unemployment. On the other hand, privatization events may be coupled with changes in management which causes breach of implicit contracts and and thus an increase in unemployment risk (Shleifer and Summers 1988).

The net effects of these potential effects of privatizations is thus an empirical question of which there is so far limited evidence on. To evaluate the career consequences of privatizations, we study privatizations of SOEs in Sweden. Sweden is a great test bed since privatisations were common in the 1990s and 2000s and registry data on the working age population is readily available for the period 1990 to 2011. Our data also allows us to observe the ultimate ownership of firms so we observe all privatizations of SOEs taking place in Sweden during this time span. Our focus is primarily on career outcomes in terms of unemployment incidence and duration. This is because unemployment is coupled with substantial personal and societal costs. These costs include wage cuts after accepting a new job offer and costs incurred during job search such as consumption and income losses (Katz et al. 1990; Jacobson, LaLonde, and Sullivan 1993; Gruber 1997; Farber 2005). There is also evidence that the unemployed are also less happy than the employed (Di Tella, MacCulloch, and Oswald 2001). We do, however, also perform a complementary analysis on career outcome in terms of wages.

Our empirical strategy is to combine propensity score matching with a difference-in-difference analysis.<sup>2</sup> Privatization events are not random, but as Smith and Todd (2005) show, propensity score matching in combination with difference-in-differences regressions helps in overcoming selection biases. Our specifications also include a multitude of individual level control variables, and we show that our main effects are robust to the inclusion of firm fixed effects, to aggregating our data to the group level as advocated by Donald and Lang (2007),

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<sup>2</sup> Heckman et al. (1997) note that propensity score matching performs well when the set of observables and the set of potential controls is large.

and to randomly selecting controls from state-employed workers instead of using propensity score matching.

Our paper contributes to the literature on privatizations by taking a career perspective on privatizations. While this literature on privatizations is extensive, there is a dearth of hard large sample empirical evidence on the costs and benefits of privatizations for *individual* workers. Most evidence on the effects on workers is based on changes in net employment at privatized firms.<sup>3</sup> While influential and important, these papers potentially miss effects on workers employed in the firm at the time of the privatization as net changes in employment can mask substantial churn. They are also not able to provide evidence of heterogeneity in employment effects across workers, and cannot study worker welfare effects due to changes in transitions to unemployment. Closest in spirit to our work is Brown, Earle, and Vakhitov (2006) that studies data from a 2003 survey of 4000 Ukranian households, and Bastos, Monteiro, and Straume (2014) who use Portuguese data to study worker wages around privatizations. Both of these use large samples to study individual workers effects of privatizations and also take care to account for selection effects of workers into firms that privatize.<sup>4</sup> Related is also Melly and Puhani (2013), who study the personell records of a large state owned company that was privatized and compares it to the personell records of a firm that remained state owned. Our paper complements and goes beyond these papers in that we are better able to assess the negative welfare consequences to individuals and society due to detailed data on unemployment. Moreover, since we use registry data our dataset is considerably larger and our analysis do not suffer from attrition problems.

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<sup>3</sup> This literature is now extensive and is surveyed in Megginson and Netter (2001), Djankov and Murrell (2002), and Estrin et al. (2009). Key contributions on firm level employmen include La Porta and Lopez-de-Silanes (1999), Jones and Simon (2005), Lizal and Svejnar (2002), and Brown, Earle, and Telegdy (2006), but none of these observe churn. The exception is Brown and Earle (2003) that studies hirings and separations in a Russian firm survey.

<sup>4</sup> There are also several papers on worker effects of privatizations using smaller samples, see for example Haskel and Szymanski (1993), Kikeri (1998) and Peoples and Talley (2001).

Another important difference is that we provide evidence from a non-transition country. While there is ample evidence on the effects of privatization in Eastern European countries during times in which the institutional environment was in flux, there is much less evidence on the effects of privatizations on workers in developed countries in which a large amount of privatizations has recently taken place. As such, our paper offers a complementary analysis to most other papers on the topic as we are in a position to compare our findings to the findings on privatizations in Eastern European countries.

## **2. Data and empirical strategy**

### *2.1. Data source*

Our main source of data is Statistics Sweden's LISA database, for which we have available individual level data on unemployment from 1992 to 2011. LISA is a longitudinal matched employer-employee database that includes every person older than 15 that is registered as living in Sweden. The LISA database merges data from different government registers, including population registers, tax records, and statistical surveys. An individual exits the database only by dying or moving to another country. The database contains individual identifiers that enable us to track people over time, independently of labor market status. From LISA, we extract annual information on individuals' age, gender, education, occupation (available for 2001-2011), labor income, firm affiliation, residence municipality, and the yearly number of days registered as unemployed. To obtain information about privatizations, we use the firm level ownership status variables and code transitions from state owned to privately owned as a privatization.

## 2.2. Empirical strategy

Our main econometric concern is selection because privatizations do not happen at random. Selection bias can occur if privatizations are correlated with worker characteristics that are correlated with unemployment risk. Our main strategy to mitigate selection bias is to leverage the registry data with complete working age population data to construct a control group of workers similar to the ones part of privatizations, but that are not affected by privatizations. We then use a difference-in-differences estimator that compares the career outcomes of the treated and control group controlling for a large set of observable ex-ante characteristics. Specifically, we model career outcomes of worker  $i$  at year  $t$  as:

$$U_{it} = \alpha + \delta Post_t + \gamma Privatized_i + \beta Post_t \times Privatized_i + \theta X_i + \lambda_t + \varepsilon_{it} \quad (1)$$

The dependent variable is the unemployment incidence or duration  $U_{it}$ , the  $Post_t$  dummy is one for the year of the privatization and all years after, and the  $Privatized_i$  dummy is one for workers affected by privatizations and zero for workers in the control group. We measure unemployment incidence as at least one day of registered unemployment in a year, and unemployment duration is measured as the number of days of unemployment in a year. The coefficient  $\beta$  captures the average treatment effect, which in our case is an intention-to-treat effect which is smaller than the average treatment effect on the treated. This is because we mark workers as treated the year before the privatization takes place. Some of them might have left the firm before the privatization happened. We also include a vector of individual level controls,  $X_i$ , and calendar year dummies,  $\lambda_t$ . We measure all individual level controls in the year before the privatization event (control workers have an imputed privatization year).

The estimated treatment effect has a causal interpretation under the parallel trend assumption and the stable unit value treatment assumption (SUTVA). The parallel trend

assumption requires that the treated and control groups would have had parallel trends in the absence of treatment. Because the counterfactual outcomes are unobservable, it is impossible to test this assumption. However, we can assess the plausibility of the assumption by comparing trends in unemployment incidence before treatment. Historical parallel trends indicate that shocks, at least in the past, have affected the two groups in a similar way. The SUTVA assumption is likely to hold in our setting on account of that we select controls out of the entire population of state-employed workers in Sweden. It is unlikely that a privatization event in one part of Sweden affects control workers in another part of Sweden. Note also that we analyze these pre-existing workers before and after privatizations regardless of whether they remain employed at the same firm. Because workers leave the sample only by moving abroad or dying, we do not need to worry about compositional changes in the treatment and control groups.

### *2.3. Creating the control group and details of the sample*

To obtain treated workers, we restrict attention to limited liability firms (“aktiebolag”) with more than 10 employees that are privatized 1995 and 2007. This gives us three pre-periods of worker level data and at least four post-periods of data (since the LISA unemployment data is available to us for 1992 to 2011). Table 1 report summary statistics on the sample of privatizations over time and across industries. Sweden had a privatization wave in the 1990s, which is clearly visible in the time series in Panel A. As Panel B shows, most of these privatizations took place in the mining, manufacturing, transport, mail, telecommunications, and real estate, renting and business activities sector. In total, we capture 614 firms that are privatized and these privatization events affect 145 952 individual workers.

We create our primary control group by using propensity score matching within strata. The set of potential control workers is all workers in limited liability SOEs in Sweden with

more than 10 employees that are not privatized during the sample period. We take the potential controls and the treated workers in the last year the SOE was state owned and group them in year times industry bins (using 17 industry bins) and drop bins with no treated workers in them. For each year, we generate a propensity score on each worker through a probit regression explaining treatment status with worker age, six education dummies, worker tenure and the log wage. We do this procedure separately for males and females, and then select for each treated worker the nearest neighbor control using the estimated propensity score. This procedure ensures that we have an exact match on industry and gender (say, female daycare workers) and then a weighted match on age, tenure, education, and pay. Control workers also get an imputed privatization year since they are randomly assigned to a year bin. As a secondary simpler alternative, we provide a robustness check on the matching procedure in Section 4 in which we randomly select a control for each treated worker within gender times industry times year bins. As pointed out by Angrist and Pischke (2009), a straight regression with controls can be considered a particular sort of weighted matching and provides better transparency than propensity score matching on observable characteristics.

Table 2 reports means and standard deviations on worker characteristics in privatized firms and matched workers in state owned firms that are not privatized. Means are from the final year the firm was state owned. Workers in the sample are on average 43 years old, earn an annual salary of 262 250 SEK (about 31 470 USD in 2005), and has a tenure of 3.2 years. About a third are female and 16% have at least two years of post-graduate studies. The final column report a normalized t-test statistic based on Imbens and Wooldridge (2009). A value above 0.25 indicates substantial differences in means. Though there are slight differences in means between treated and control workers, the normalized t-test suggests that the differences are not substantial. This suggests that the matching procedure works well in ironing out differences between treated and control workers.

### **3. The career consequences of privatizations**

#### *3.1. Privatizations increase unemployment incidence and duration*

We start with a visual inspection of the pre-trends in unemployment incidence and duration for treated and control workers in our sample. Figure 1 displays the trends in normalized unemployment incidence and duration measured in days against event time in years around privatizations. Unemployment incidence and duration declines up until event time minus one, because minus one measures the last year the firm was state owned. Since we pick up workers in this year through their employment, their unemployment incidence and duration is the lowest in this year. Statistics Sweden measures employment primarily in November every year so unemployment incidence and duration can be above zero even in this year. Before event time minus one, treated and control workers have fairly similar trends in unemployment incidence but we can observe markedly higher increase in unemployment incidence for workers in privatized firms relative to workers in state owned firms that are not privatized.

Table 3 reports selected selected coefficients from estimating the model in Equation 1 using OLS and clustering the standard errors at the local labor market level (proxied by residence municipality). The estimates show that privatizations increase unemployment incidence by about 1.5 percentage points and unemployment duration by about two days. Relative to means before the privatization event, this converts to a 22% increase in unemployment incidence and a 29% increase in unemployment duration. Adding a battery of individual level controls to the regressions leave the estimates close to unchanged. The average effect---a relative increase of two days per year over an eight year post period---means that privatizations have accounted for about  $145000 \times 2 \times 8 = 2.32$  millions days of additional unemployment in Sweden during these years or 0.25 days per inhabitant on average. A economically significant sum.

These findings can be put into contrast to the findings at the firm level by, for example, Brown, Earlie, and Telegdy (2010) who consistently do not find evidence of job losses in the manufacturing sector of Hungary, Russia, Ukraine, and Romania following privatizations. Moreover, Brown, Earle, and Vakhitov (2006) finds that privatizations cut the layoff probability in half using retrospective survey data from Ukraine. This comparison suggests that privatizations of previous state owned manufacturing firms in eastern Europe had markedly different effects on workers than privatizations in Sweden during the 1990s and 2000s. In what follows, we next investigate potential mechanisms behind increases in unemployment incidence and duration following privatization events.

### *3.2 Heterogeneous effects consistent with technological upgrading*

As mentioned in the introduction, there is ample empirical evidence that privatizations tends to improve firm performance (Megginson and Netter 2001). Such improvements can come about due to cost cutting through layoffs, which would explain the increased unemployment incidence for workers, or due to productivity enhancing investments. Such investments may affect workers differently depending on what types of jobs they perform. The recent literature on job polarization has suggested that technological development have led to certain occupations disappearing and that workers with outdated skills are more likely to be affected by such upgrades (Acemoglu and Autor 2011; Goos, Manning, and Salomons 2014). Olsson and Tåg (forthcoming) also point out and show that ownership changes can be associated with greater moves to unemployment for workers performing tasks that are substitutes to technological upgrades due to a lack of incentives of previous management to invest in such upgrades.

To investigate if there are heterogeneous effects consistent with technological upgrading following privatizations, Table 4 reports estimates by subgroups based on skill,

age, and routine-intensity of the job task performed. Our hypothesis is that unskilled workers, older workers, and workers performing routine-intense job tasks would be more likely to take adverse hits to their careers due to privatizations.

Panel A shows that low skilled workers experience a statistically significant increase in incidence of about 1.7 percentage points (25%), while the same for high skilled workers is only 0.4 percentage points (8%). Consistent with this, the triple difference estimate in the third column shows a relative difference of 1.3 percentage points. For duration, the corresponding numbers are 2.4 days (32%), 0.6 days (11%) and a triple difference estimate of 1.8 days in favor of skilled workers.

Panel B report results based on separating workers into young and old workers based on the median age for workers in privatized firms (44 years). Young workers face an increase in unemployment incidence of 8 percentage points (8%), whereas the same number for old workers is 2.3 percentage points (87%). The triple-difference estimate in the final column confirms the relatively greater impact of privatizations on older workers: a statistically significant point estimate of 1.4 percentage points in incidence and 2.7 days in duration.

Finally, Panel C reports results by the routine intensity of the job tasks the worker performs. These regressions are run only for privatizations in 2002 or later since occupation data is available in our data only from 2001 and onwards. Workers performing routine job tasks see a 3.0 percentage point (48%) increase in unemployment incidence, compared to a 1.5 percentage point increase for workers performing non-routine job tasks (55%) resulting in a triple difference estimate of 1.5 percentage points in column 3. We find a similar pattern for employment duration: an increase in duration for workers performing routine job tasks by 4.5 days (82%) and for workers performing non-routine job tasks the increase is 1.9 days (74%) resulting in a triple difference estimate of 2.6 days.

Thus, all of these subsample analyses paints a consistent story of relatively greater effects on workers we expect are more likely to be affected by technological upgrades that privatization events may trigger. However, we also consistently find that both types of workers experience increases in unemployment incidence and duration. It is also worthwhile noting that the heterogeneous effects in terms of skill are consistent with Brown, Earle, and Vakhitov (2006) and Melly and Puhani (2013). Melly and Puhani (2013) also find evidence of greater adverse effects on older workers.

### 3.3 *Women are more affected than men*

Next, we turn to gender differences in the effects of privatizations on careers. 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. Additionally, there is evidence of greater a public-private sector wage premium for females compare to males which suggests privatizations could affect women to a greater extent than men.

Table 5 breaks down our sample by gender and we find clear evidence of a greater effect of privatizations on women relative to men, although again both subgroups experience increases in both unemployment incidence and duration. The point estimate suggests in increase in duration for women of 2.4 percentage points (32%) and for men a 1 percentage point increase (17%), resulting in a triple-difference estimate of 1.4 percentage points. The same number for unemployment duration are 2.9 days (40%), 1.7 days (24%), and a triple difference estimate of 1.6 days. These results are consistent with Melly and Puhani (2013) that also find greater effects on women compared to men, but they are not consistent with Brown, Earle, and Vakhitov (2006) who does not find any gender differences in effects.

### 3.4 *Labor laws protect workers*

Finally, we consider to what extent labor laws protect workers from unemployment in the wake of privatizations. This is relevant since Sweden had in 2004 the seventh-strongest labor market protection among 30 countries (OECD 2004) so the potential for negative career consequences of privatizations is likely greater in other countries than in Sweden that have weaker labor market protections if it is the case that labor protections help workers. To investigate this heterogeneity, we proxy employment protection status with tenure, such that workers with high protection are workers with more than two years of tenure. This is based on the Swedish employment protection law LAS (“Lagen om anställningsskydd”) which has been in force since 1982. This law 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.

Table 6 reports the results. Workers with low employment protection experience increases in incidence by 1.4 percentage points (12%) or 1.9 days of unemployment (15%) whereas workers with high employment protections experience increases in incidence by 0.5 (35%) percentage points and 0.9 days of unemployment (54%). The triple-difference estimates show statistically significant differences between the subsamples. Thus, we observe increases in unemployment incidence for both groups and in percentage point and unemployment days the increase is relatively larger for the workers with low employment protections. This is consistent with the interpretation that labor laws protect workers to some extent, but not fully.

## 4. Extensions and robustness

### 4.1 Privatizations and wages

Throughout we have found consistently negative effects of privatizations on workers relative to workers that continue being employed by the state. But if workers accept a higher unemployment risk in the private sector we might expect them to be compensated for taking on that higher risk. Thus, we would expect wages to increase following privatizations. This would be consistent with the wage increases observed in Bastos, Monteiro, and Straume (2014) for Portugal. On the other hand, previous evidence in Brown, Earle, and Vakhitov (2006) have found evidence of wage decreases consistent with cost cutting following privatizations.

To investigate this issue, Table 7 results from our main regression by replacing unemployment incidence and duration with wages. We measure wages by total annual labor income in thousands of SEK in 2005, where total annual income include all income taxed as labor income, such as salaries, stock options, bonus payments, and other benefits from employment. The regressions suggest that wages increase statistically following privatizations by about 5320 SEK or 2%. Taking logs increases the effect to a 5.5% log percent effect.

These effects are economically relatively small, but it is worth noting that any effects on wages on average will be a combined effect from the wages of workers entering unemployment, the workers transitioning to another employer, and the workers that remain with the privatized firm. Since we observe increases in moves to unemployment, which are correlated with lower wages, the wage effects on workers remaining with the firm or transition to another firm are likely greater than 2-5.5%. However, from an econometric perspective it is difficult for us to separate between these effects since we cannot condition on regressions on where workers end up without introducing biases in the estimated coefficients.

#### 4.2 *Alternative estimation strategies*

This subsection reports a set of robustness checks on our main results. Panel A of Table 8 reports the results from an OLS regression at the group-event time level as suggested by Donald and Lang (2007). Mean incidence or duration is regressed on a constant and a Post dummy taking the value one for the means of workers' in privatized firms at event time zero and higher. We also include a specification with normalized means using individual level controls as in Figure 1. The point estimate of a 1.6 percentage point increase in incidence and a 2.2 days increase in unemployment duration is close to identical to the estimates from our main specification in Table 3.

The same is true for the estimates in Panel B in Table 8, which reports results from a firm fixed effects specification. The Post dummy takes the value one for the workers' in privatized firms at event time zero and higher. We find a slightly lower estimate of the impact of privatizations on unemployment incidence of 1.23 percentage points (19%), but a slightly higher effect on unemployment duration of 2.6 days (36%). That the point estimate does not change markedly with firm fixed effects suggests that little selection of privatizations of SOEs goes on on the basis of observed or unobserved firm characteristics.

Finally, Panel C reports results from estimations based on selecting controls randomly for each treated worker within gender times industry times year bins instead of using propensity score matching. As pointed out by Angrist and Pischke (2009), a straight regression with controls can be considered a particular sort of weighted matching and provides better transparency than propensity score matching on observable characteristics. The point estimates for unemployment incidence show a slightly smaller effect of 0.8 percentage points (12.5%) increase in incidence and a 1.5 days (22%) increase in unemployment duration, but both estimates remain highly statistically significant. Note also that adding controls does not change the point estimate markedly.

## **5. Concluding remarks**

Using propensity score matching combined with a difference-in-difference analysis, we have in this paper characterized the consequences of privatizations for individual workers' careers in terms unemployment. We have shown that over an eight year period, privatizations increase unemployment incidence and duration relative to that of similar workers remaining employed by the state. The increase leads to a convergence in unemployment incidence and duration to that of a random sample of workers in the private sector. These average effects masks considerable heterogeneity. Consistent with technological upgrading following privatizations, lower skilled workers, older workers, and worker performing routine-intense job tasks take greater hits to their careers. Women and workers with weaker labor market protections are also hurt more than men and more protected workers.

To what extent does our results generalize to other countries? While external validity is difficult to assess, there are at least three reasons believe that our results have external validity. First, we have throughout our analysis compared our findings in this paper to findings from other countries and institutional environments and found broadly consistent results. Second, there is some evidence that labor markets behave similar in Sweden as in other developed countries such as the other Scandinavian countries and Belgium, France, Germany, Italy, the Netherlands, and the United States (Lazear and Shaw 2009). Finally, Sweden had in 2004 the seventh-strongest labor market protection among 30 countries (OECD, 2004) and as we have shown we observe greater effects on less protected workers. As such, the potential for negative career consequences of privatizations is likely greater in other countries than in Sweden that have weaker labor market protections so our estimates could represent a lower bound on the negative career effects of privatizations.

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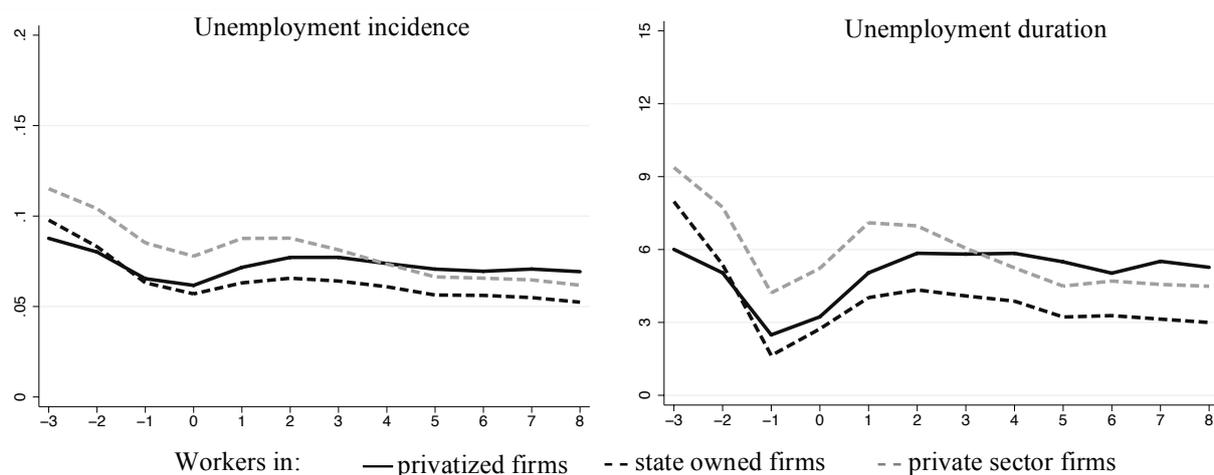
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**Figure 1. Privatizations increase unemployment incidence and duration.** These figures plot the trends in normalized unemployment incidence and duration measured in days (vertical axis) against event time in years around privatizations (horizontal axis). Normalized incidence/duration is obtained from a regression of incidence/duration on event time dummies and controls for age, gender, education, tenure, region, industry, firm size and firm age. We draw lines for workers in privatized firms, for similar workers in state owned firms that are not privatized, and for a random sample of workers in private firms. Section 2.2 provide details on these samples.

**Table 1****Privatizations vary over time and across industries**

The sample consists of all privatizations of limited liability firms (“*aktiebolag*”) with more than 10 employees undertaken between 1995 and 2007. Privatizations are identified through changes in the Statistics Sweden institutional sector codes for firms. Industry classification is based on NACE Rev 1.1.

<b>Panel A: Variation over time</b>		
<b>Year</b>	<b>Firms</b>	<b>Workers</b>
1995	157	37649
1996	88	15784
1997	70	12655
1998	41	5900
1999	20	1027
2000	68	12703
2001	27	6928
2002	59	13208
2003	63	32614
2004	3	4121
2005	12	2273
2006	0	0
2007	6	1090
<b>Total</b>	<b>614</b>	<b>145952</b>

<b>Panel B: Variation across industries</b>		
<b>Industry</b>	<b>Firms</b>	<b>Workers</b>
Mining and manufacturing	128	35213
Electricity, gas, and water	58	8163
Construction	17	1790
Hotels and restaurants	19	3007
Transport, mail, and telecommunications	90	44492
Real estate, renting, and business activities	214	29660
Education, health and social work	32	5028
Other	56	18599
<b>Total</b>	<b>614</b>	<b>145952</b>

**Table 2****The treated and control workers resemble each other ex-ante**

This table report means and standard deviations (reported below means) on workers in privatized firms and matched workers in state owned firms that are not privatized. Section 2.2 describes the matching procedure and how event time is assigned to state employed controls. Means are from the year before the firm was privatized. The final two columns report differences in means and a normalized t-test statistic based on Imbens and Wooldridge (2009). A value above 0.25 indicates substantial differences in means. *Wage* is total annual labor income in thousands of SEK in 2005 (1 SEK  $\approx$  0.12 USD), *Skilled* is a dummy for having at least two years of post secondary education, and *Tenure* measures the number of years a workers has been employed in the firm (truncated at 16 years) based on Statistics Sweden's dynamic worker flow identifiers (FAD-codes). Total annual income include all income taxed as labor income, such as salaries, stock options, bonus payments, and other benefits. The sample consists of 145952 worker observations from firms that are privatized and an equal number of control workers observations from state owned firms that are not privatized.

Variable	Sample of workers		Difference	
	Privatized	State controls	Privatized vs state	Normalized t-test
Age (years)	43.03	43.34	-0.32	-0.02
	11.49	11.62		
Wage (SEK 2005)	262.25	242.01	20.24	0.09
	165.41	170.60		
Female	0.32	0.32	0.00	0.00
	0.47	0.47		
Skilled	0.16	0.15	0.01	0.08
	0.37	0.35		
Tenure (years)	3.16	3.01	0.15	0.03
	2.97	3.29		

**Table 3****Privatizations increase unemployment incidence and duration**

This table reports selected coefficients from an OLS regression explaining worker-year unemployment incidence or duration for 1992 to 2011 with a constant, difference-in-difference dummies, and a set of control variables. The standard errors are clustered at the local labor market level (proxied by residence municipality). The sample includes worker level time series information for three years before and up to eight years after the privatization event. *Post* is a dummy variable that turns on after a privatization event (at event time zero) and *Privatized* is a dummy taking the value one for workers employed in privatized firms in event time minus one, and zero for matched control workers in state owned firms. The set of control variables include 67 age dummies, a gender dummy, five education level dummies, 16 tenure dummies, 19 calendar year dummies, 51 industry dummies, 24 county dummies, firm size, and 20 firm age dummies. Firm age is calculated using Statistics Sweden's FAD-codes and is truncated at 20 years. The coefficients for unemployment incidence are reported in percentage points and for duration in days. The percent change uses means for workers in privatized firms over three years before the privatization event as the baseline. 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 \*.

Dependent variable Specification	Incidence		Duration	
	No controls	Controls	No controls	Controls
Post*privatized	1.44*** (0.21)	1.47*** (0.22)	2.07*** (0.25)	2.09*** (0.30)
Privatized	-1.19*** (0.22)	-0.32* (0.18)	-1.46*** (0.22)	-0.43* (0.23)
Post	-1.56*** (0.13)	-0.65*** (0.15)	-0.77*** (0.15)	0.70*** (0.17)
R <sup>2</sup>	0.000	0.056	0.000	0.025
Percent change	22.3%	22.7%	29.1%	29.4%
Workers	291904	291904	291904	291904
Observations	3426740	3426740	3426740	3426740

**Table 4****Heterogeneous effects consistent with technological upgrades**

This table reports selected regression coefficients from running the regression specification with controls in Table 3 separately for different subsamples. We also report selected coefficients from triple-difference specifications that compares the subsamples to each other. Panel A reports results by a worker's skill level. High skill workers are workers with two or more years of post-secondary education. Panel B report results based on separating workers into young and old workers based on the median age for workers in privatized firms (44 years). Panel C reports results by the routine intensity of the job tasks the worker performs. Workers are classified into performing routine or non-routine job tasks based on their occupation and the RTI index of occupations in Goos, Manning, and Salomons (2014). A worker perform routine job tasks if his or her occupation has a positive RTI index value. The regressions in Panel C are run only for privatizations in 2002 or later since occupation data is available from 2001. 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 \*.

<b>Panel A: Skill level</b>						
Dependent variable Specification	Incidence			Duration		
	Low	High	Triple-diff	Low	High	Triple-diff
Post*privatization	1.68*** (0.23)	0.43** (0.20)	1.68*** (0.23)	2.39*** (0.32)	0.63** (0.26)	2.38*** (0.31)
Privatization	-0.24 (0.20)	-0.32* (0.18)	-0.23 (0.19)	-0.35 (0.26)	-0.31 (0.27)	-0.32 (0.24)
Post	-0.63*** (0.15)	-1.33*** (0.25)	-0.66*** (0.14)	0.77*** (0.18)	-0.51* (0.30)	0.63*** (0.17)
Post*privatization *high			-1.31*** (0.23)			-1.86*** (0.33)
R <sup>2</sup>	0.060	0.031	0.055	0.028	0.016	0.025
Percent change	25.16%	7.97%	-20.24%	32.35%	11.31%	-26.17%
Workers	247264	44640	291904	247264	44640	291904
Observations	2901904	524836	3426740	2901904	524836	3426740

<b>Panel B: Age</b>						
Dependent variable Specification	Incidence			Duration		
	Young	Old	Triple-diff	Young	Old	Triple-diff
Post*privatization	0.83*** (0.22)	2.27*** (0.23)	0.81*** (0.21)	0.86*** (0.28)	3.58*** (0.36)	0.83*** (0.26)
Privatization	-0.14 (0.23)	-0.54*** (0.16)	-0.48** (0.22)	-0.22 (0.28)	-0.73*** (0.24)	-0.54* (0.28)
Post	-1.82*** (0.24)	0.45*** (0.11)	-2.60*** (0.24)	-0.31 (0.25)	1.53*** (0.18)	-1.57*** (0.25)
Post*privatization *old			1.44*** (0.21)			2.72*** (0.39)
R <sup>2</sup>	0.058	0.025	0.051	0.034	0.015	0.025
Percent change	8.12%	87.52%	22.25%	8.08%	105.5%	38.30%
Workers	147762	144142	291904	147762	144142	291904
Observations	1745555	1681185	3426740	1745555	1681185	3426740

<b>Panel C: Routine intensity</b>						
Dependent variable	Incidence			Duration		
	Routine	Non-routine	Triple-diff	Routine	Non-routine	Triple-diff
Post*privatization	2.98*** (0.49)	1.49*** (0.35)	2.99*** (0.48)	4.52*** (0.69)	1.91*** (0.46)	4.56*** (0.70)
Privatization	-1.50*** (0.57)	-0.18 (0.27)	-0.90** (0.40)	-2.84*** (0.71)	-0.14 (0.34)	-1.62*** (0.46)
Post	0.81** (0.39)	-0.16 (0.31)	-1.19*** (0.27)	2.03*** (0.70)	0.26 (0.47)	-0.54 (0.41)
Post*privatization *non-routine			-1.48*** (0.51)			-2.64*** (0.77)
R <sup>2</sup>	0.056	0.025	0.037	0.029	0.014	0.018
Percent change	47.99%	55.27%	-41.88%	81.66%	74.13%	-80.39%
Workers	31770	56876	88646	31770	56876	88646
Observations	365033	663167	1028200	365033	663167	1028200

**Table 5**  
**Women are more affected than men**

This table reports selected regression coefficients from running the regression specification with controls in Table 3 separately by gender. We also report selected coefficients from triple-difference specifications that compares males to females. 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 \*.

Dependent variable Specification	Incidence			Duration		
	Women	Men	Triple-diff	Women	Men	Triple-diff
Post*privatization	2.39*** (0.31)	1.03*** (0.23)	1.03*** (0.23)	2.92*** (0.40)	1.69*** (0.32)	1.68*** (0.32)
Privatization	-0.79*** (0.20)	-0.12 (0.22)	-0.11 (0.21)	-0.78*** (0.25)	-0.33 (0.29)	-0.32 (0.28)
Post	-0.51** (0.23)	-0.70*** (0.15)	-0.51*** (0.14)	1.26*** (0.25)	0.39** (0.19)	0.69*** (0.18)
Post*privatization *woman			1.36*** (0.24)			1.25*** (0.33)
R <sup>2</sup>	0.060	0.054	0.056	0.028	0.026	0.025
Percent change	32.55%	16.98%	21.03%	39.50%	24.27%	17.60%
Workers	94558	197346	291904	94558	197346	291904
Observations	1112422	2314318	3426740	1112422	2314318	3426740

**Table 6**  
**Labor laws protect workers**

This table reports selected regression coefficients from running the regression specification with controls in Table 3 separately by employment protection status. We also report selected coefficients from triple-difference specifications that compares workers with low and high protection to each other. We proxy employment protection status with tenure, such that workers with high protection are workers with more than two years of tenure. This is based on the Swedish employment protection law LAS (*“Lagen om anställningskydd”*) which has been in force since 1982. This law 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. 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 \*.

Dependent variable Specification	Incidence			Duration		
	Low	High	Triple-diff	Low	High	Triple-diff
Post*privatization	1.37*** (0.29)	0.52*** (0.18)	1.49*** (0.27)	1.93*** (0.40)	0.93*** (0.28)	2.14*** (0.36)
Privatization	-0.55** (0.28)	1.01*** (0.19)	-0.73*** (0.27)	-0.66* (0.37)	1.23*** (0.30)	-1.01*** (0.37)
Post	-3.16*** (0.26)	2.26*** (0.15)	-4.16*** (0.27)	-1.85*** (0.30)	3.31*** (0.25)	-3.35*** (0.30)
Post*privatization *high			-0.89*** (0.29)			-1.09** (0.48)
R <sup>2</sup>	0.059	0.019	0.057	0.028	0.012	0.025
Percent change	11.83%	35.44%	-13.75%	15.26%	54.60%	-15.30%
Workers	152185	139719	291904	152185	139719	291904
Observations	1790520	1636220	3426740	1790520	1636220	3426740

**Table 7**  
**Privatizations increase wages**

This table reports selected regression coefficients from running the regression specifications in Table 3 but replacing unemployment incidence and duration with wages or the log of wages. Wages are measured by total annual labor income in thousands of SEK in 2005 (1 SEK  $\approx$  0.12 USD), where total annual income include all income taxed as labor income, such as salaries, stock options, bonus payments, and other benefits from employment. When taking logs, we code workers with zero income as earning 1000 SEK to keep them in the sample. 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 \*.

Dependent variable Specification	Wage		Log(wage)	
	Controls	No controls	Controls	No controls
Post*privatization	5.32*** (1.24)	5.56*** (1.64)	5.48*** (1.63)	5.74*** (1.92)
Privatization	21.97*** (2.48)	-16.14*** (1.72)	14.8*** (1.61)	-9.21*** (1.27)
Post	4.20*** (1.56)	-15.40*** (1.48)	-59.4*** (1.62)	-48.9*** (1.58)
R <sup>2</sup>	0.004	0.260	0.023	0.245
Percent change	2.09%	2.18%		
Workers	291904	291904	291904	291904
Observations	3426740	3426740	3426740	3426740

**Table 8****The effects are robust to alternative estimation strategies**

This table reports selected regression coefficients from alternative regression specifications and control groups. Panel A reports the results from an OLS regression at the group-event time level as suggested by Donald and Lang (2007). Mean incidence or duration is regressed on a constant and a *Post* dummy taking the value one for the means of workers' in privatized firms at event time zero and higher. We also include a specification with normalized means using individual level controls as in Figure 1. Panel B reports selected coefficients from an OLS regression at the individual-time level explaining incidence or duration with a constant, a *Post* dummy, and firm fixed effects. The *Post* dummy takes the value one for the workers' in privatized firms at event time zero and higher. Panel C reports selected coefficients from the same regression specification as in Table 3, but using a control group of randomly selected state employed workers in firms that are not privatized. Section 4.4 details the construction of this group.

<b>Panel A: Collapsed data</b>				
Dependent variable	Incidence		Duration	
	Controls	No controls	Controls	No controls
Post	1.56*** (0.34)	1.47*** (0.32)	2.17** (0.76)	2.09*** (0.62)
R <sup>2</sup>	0.758	0.772	0.604	0.654
Percent change	24.11	22.72	30.61	29.47
Workers	291904	291904	291904	291904
Observations	12	12	12	12
<b>Panel B: Firm fixed effects</b>				
Dependent variable	Incidence		Duration	
Post		1.23*** (0.19)		2.57*** (0.27)
R <sup>2</sup>		0.0376		0.023
Percent change		19.01		36.24
Observations		3426740		3426740
<b>Panel C: Randomly selecting controls</b>				
Dependent variable	Incidence		Duration	
	Controls	No controls	Controls	No controls
Post*privatization	0.81*** (0.18)	0.83*** (0.18)	1.54*** (0.24)	1.55*** (0.26)
Privatization	-0.27 (0.19)	0.01 (0.19)	-0.37 (0.24)	-0.20 (0.24)
Post	-0.93*** (0.10)	0.08 (0.13)	-0.24* (0.13)	1.39*** (0.17)
R <sup>2</sup>	0.000	0.058	0.000	0.027
Percent change	12.53	12.76	21.69	21.81
Workers	291904	291904	291904	291904
Observations	3429308	3429308	3429308	3429308