

What Is the Cost of Privatization for Workers?*

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ABSTRACT

Using rich administrative data on Swedish firms and workers covering two decades, we study how the privatization of state-owned enterprises affects the careers and welfare of workers. Consistent with the effects of an initial reorganization of the workforce, wage income declines and unemployment increases. These effects are stronger for workers protected by state ownership. We do not find negative spillovers of privatization on family and health outcomes. We also demonstrate that privatization encourages workers to enter entrepreneurship. The firm-level productivity gains from privatization are approximately five times larger than the incumbent worker-level costs due to lost wage income.

Keywords: Employment, entrepreneurship, family, health, labor, privatization, state-owned enterprises (SOEs), unemployment, wages.

JEL Codes: G34, J24, J63, L26, L33, M51.

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1 Introduction

The privatization of state-owned enterprises (SOEs) is on the agenda of policymakers across the globe. The total value of privatization of SOEs peaked at USD 320 billion in 2015 compared to only USD 24 billion in 1990. Both developing and developed countries are privatizing assets. The ten nations with the highest privatization revenues are China, the United Kingdom, Italy, Japan, India, Sweden, Australia, the United States, the Netherlands, and Ireland (Megginson, 2017b).

Creating effective evidence-based policy on privatization requires solid facts on the related costs and benefits. Currently, the benefits of privatization in terms of improved governance, productivity, and performance are well documented (Megginson and Netter, 2001; Estrin, Hanousek, Kočenda and Svejnar, 2009; Megginson, 2017a). The impacts on workers employed in the firms that go private are less well studied, although these are a key concern for politicians deciding on privatization.¹ The ex ante effects on workers are ambiguous. One hypothesis is that better governance and higher productivity increase labor demand, which, in turn, benefits workers. Another hypothesis is that such an ownership change triggers a reorganization of the workforce that leaves some or all workers worse off (Boycko, Shleifer and Vishny, 1996). Reorganizing the workforce might make sense for new owners. State ownership can be used to pursue political goals that benefit workers (Shleifer, 1998). For instance, SOEs can be used by politicians to provide stable employment and keep unemployment rates low. Moreover, soft budget constraints mean that managers can avoid the unpopular task of shedding workers. A profit-maximizing private firm may thus seek to replace workers who previously enjoyed state protection in an effort to raise productivity. The ownership change itself can also lead to value capture by shareholders through the breach of implicit contracts with workers (Shleifer and Summers, 1988).

Using Sweden as a laboratory, this paper provides new evidence on the costs and benefits of privatization for workers. Focusing on Sweden allows us to overcome two key challenges of analyzing worker outcomes. First, there are rich administrative matched employer-employee data for Sweden that allow us to follow both

¹As Megginson and Netter (2001) write, "All governments fear that privatization will cause former SOEs to shed workers, and the key question in virtually every case is whether the divested firms' sales will increase enough after privatization to offset the dramatically higher levels of per-worker productivity".

workers and firms over time and study a multitude of worker welfare and firm performance outcomes. Thus, we can paint a comprehensive picture of how benefits to firms and potential costs to workers evolve over the short, medium, and long terms. We can also compare the costs and benefits of privatization at the firm and worker levels within the same set of privatized firms. Second, coverage of the universe of workers and firms permits us to employ a difference-in-differences study design that compares workers in SOEs that go private to matched workers in SOEs that remain state-owned. The ability to select comparable workers and firms from the entire population of SOE firms and workers provides us with a detailed way to construct a suitable control set of workers and firms that are not privatized.

We have three sets of findings. The first main finding is that an initial *reallocation of human capital* takes place, consistent with the effects of new owners reorganizing the workforce. We find transitory wage declines and permanent increases in unemployment. We estimate that workers' yearly wage income declines by 4% on average over a two-year period following privatization and by 9% during the third and fourth years after privatization. After eight years, wage income has recovered fully. Unemployment does not follow the same U-shaped pattern. We find that privatization increases the yearly incidence of unemployment among affected workers by almost one-fifth (18.7%) over the full eight-year follow-up period. The increase in unemployment is immediate and does not show the same transitory pattern as wages do.

These costs primarily affect workers who were more likely to have been protected by state ownership prior to privatization. State-owned firms may offer safer employment than privatized firms and hesitate to introduce modernization efforts that negatively impact workers. Consistent with this, unskilled workers, older workers (more likely to have outdated skills), and workers with weaker employment protection face larger wage declines and unemployment increases. These results hint at the avenues in which policy efforts to mitigate the costs of privatization for workers should be directed.

We also analyze a multitude of other career and welfare outcomes. As further evidence of reorganization, we find small transitory reductions in holding a job, a short-run increase in being outside the labor force and increases in unemployment days of 29%, or 2.7 days per year. We do not find any effects on retirement. A novel feature of our data is that we can study particularly broad measures of worker welfare related to family

and health. Existing research has found that unemployment is coupled with increases in the probability of divorce and with higher mortality (Gerdtham and Johannesson, 2003; Eliason, 2012). The permanent increase in unemployment following privatization could thus be accompanied by negative spillovers on family and health. We do not find evidence of such spillovers. Privatization leaves the probability of divorce unchanged, and effects on mortality are negligible. Thus, it appears that costs of privatization for workers are paid in wage losses and increases in unemployment.

Our second main finding is that privatization is coupled with increased *entrepreneurship entry*. We find a transitory increase in self-employment and a permanent increase in business ownership, translating into approximately 12 additional entrepreneurs per thousand workers affected by privatization. Entrepreneurship is a particularly interesting career outcome, given that empirical evidence has shown that entrepreneurship is associated with significant positive spillovers on the economy. Entrepreneurs tend to contribute to new job creation (Haltiwanger, Jarmin and Miranda, 2013), to introduce new products and services (Acemoglu, Akcigit, Alp, Bloom and Kerr, 2018; Akcigit and Kerr, 2018), to bring ideas from inside incumbent firms to the market (Audretsch, Bönte and Keilbach, 2008), and to create most of the new employment following positive demand shocks (Adelino, Ma and Robinson, 2017).

We investigate two hypotheses about why privatizations could encourage entrepreneurship. First, reorganization in the wake of a privatization could force workers into entrepreneurship out of necessity because they are laid off and cannot find another job. von Greiff (2009) shows that job displacement almost doubles the probability of entering self-employment and that start-up subsidies to the unemployed are an important part of active labor market policies in many OECD countries (Caliendo and Künn, 2011; Hombert, Schoar, Sraer and Thesmar, 2020). Necessity entrepreneurship can keep workers from unemployment and provides a valuable transitory source of income until another job is secured. However, it will have small positive spillovers on society in terms of innovation or bringing new products to the market. Second, reorganization and new owners may encourage innovative entrepreneurship and allow workers within the firm to see the change in ownership or culture as a chance to bring innovations and ideas to the market. For instance, Babina, Ouimet and Zarutskie (2020) finds that ownership changes in the form of IPOs encourage high-skill

workers to form new firms, and Babina and Howell (2020) finds that corporate R&D spending encourages workers to join founding teams of startups. If privatization triggers innovative entrepreneurship, it can have substantial spillovers on society.

Several pieces of evidence point towards necessity entrepreneurship as the main explanation for increased entrepreneurship entry. A full 72.5% of all entrepreneurs in the post-period are self-employed, and the effects on self-employment are transitory; an overwhelming majority (96%) of the entrepreneurs run firms with fewer than 10 employees; and the mean total income of entrepreneurs during the post-entrepreneurship period is half of their income before privatization. As a result, the spillover effects from entrepreneurship entry are modest. The entrepreneurs in our sample generate approximately 1.4 extra jobs per privatized firm or approximately 10 additional jobs per thousand privatized workers. The additional productivity, measured as value added per employee, equals only 2.4% of the productivity improvements of the privatized firms.

Our third finding is that the costs of privatization for incumbent workers are *offset by increases in productivity* at the firm level by a factor of five. We show that firm-level productivity increases by 8.2% over a four-year post-period, implying that privatization in our sample has led to an increase in productivity of approximately 9 billion SEK. In comparison, the wage losses for workers amount to approximately 1.8 billion SEK over this same period. We do not, however, detect any effects on net employment and wages at the firm level despite the worker-level evidence of income losses and higher unemployment. These results point to the importance of worker churn as a mechanism through which profitability and productivity improve after privatization. Moreover, they suggest that firm-level employment effects may not be informative about the social costs for workers, as they ignore the potential adverse effects of churn on workers.

2 Related literature

Our paper contributes to the privatization literature. 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.² Most studies focus on firm-level employment.³ Notable exceptions are Brown, Earle and Vakhitov (2006a), Melly and Puhani (2013), Bastos, Monteiro and Straume (2014), and Arnold (2019). Our paper overcomes limitations of this earlier literature, as our data allow us to provide a much broader picture of the social costs of privatization by going beyond the impact of privatization on wages. It also connects the privatization literature to the family, health and entrepreneurship literatures.⁴ An implication for future empirical work is that effects on wages and unemployment appear to be sufficient statistics for the first-order effects of privatization on incumbent workers since spillovers on family, health, and entrepreneurship are economically small in magnitude. Moreover, we point out that firm-level employment and wage effects may not be informative about social costs to workers, as they ignore potential adverse effects of churn on workers. Finally, with caveats about external validity firmly in mind, our findings have implications for active privatization policies across the globe because we provide evidence on worker-level outcomes from an institutional environment that is rather similar to those of several countries that are currently privatizing or considering privatizing assets. As Megginson (2017a) show, the ten nations with the highest privatization revenues in 2015 were China, the United Kingdom, Italy, Japan, India, Sweden, Australia, the United States, the Netherlands, and Ireland. Several of these countries have institutional environments and labor markets that resemble those of Sweden. For example, Lazear and Shaw (2009) report strong similarities between labor markets in Scandinavian countries and Belgium, France, Germany, Italy, the Netherlands, and the United States.

Our paper also contributes to the labor and finance literature.⁵ The labor and finance literature has used matched employer-employee data to study the worker effects of other ownership changes such as M&As

²See the surveys of Megginson and Netter (2001), Estrin et al. (2009), and Earle (2014).

³See La Porta and Lopez-de-Silanes (1999), D'souza and Megginson (1999), Brown, Earle and Telegdy (2006b), Dinc and Gupta (2011) and Chong, Guillen and López-de Silanes (2011).

⁴Our paper contributes to the literature on health and family economics that has focused on the effects of unemployment or job loss on family and health outcomes (Gerdtham and Johannesson, 2003; Eliason, 2012). We contribute to this literature is by providing evidence of a new shock to job loss: the privatization of SOEs. Our paper contributes to the vast entrepreneurship literature that has studied the determinants of entrepreneurship entry and, in particular, to the literature on how employers' characteristics determine entrepreneurship entry (Özcan and Reichstein, 2009; Babina et al., 2020; Babina and Howell, 2020). We contribute by showing that privatization can encourage workers to enter entrepreneurship.

⁵See Pagano and Volpin (2005), Atanassov and Kim (2009), Cronqvist, Heyman, Nilsson, Svaleryd and Vlachos (2009), Maksimovic, Phillips and Prabhala (2011), Simintzi, Vig and Volpin (2015), Dessaint, Golubov and Volpin (2017), Mueller, Ouimet and Simintzi (2017) and Subramanian and Megginson (2018).

(Ma, Ouimet and Simintzi, 2019; He and le Maire, 2020), corporate diversification (Tate and Yang, 2015), private equity buyouts (Agrawal and Tambe, 2016; Olsson and Tåg, 2017; Antoni, Maug and Obernberger, 2019), and IPOs (Babina et al., 2020). However, there is a lack of work on how ownership changes in the form of the privatization of SOEs influence workers. Overall, our paper extends this literature in three ways. First, we show evidence that other corporate events outside of IPOs (Babina et al., 2020) and R&D expenditures (Babina and Howell, 2020) can lead to higher rates of new firm formation by former employees. Second, we show that there are no spillovers from privatization on workers' health, consistent with Garcia-Gomez, Maug and Obernberger (2020) who finds no health effects after private equity buyouts. Thus, while corporate ownership changes are important for labor market outcomes, they appear not to be of first-order importance for worker health outcomes. Finally, we provide additional evidence to the argument in Subramanian and Megginson (2018) that employment protection legislation has important interactions with privatization.

3 Data and empirical design

3.1 Individual- and firm-level information

We use administrative data from the LISA database maintained by Statistics Sweden. The database is based on government registers such as tax records, population registers and surveys. It holds yearly information on all registered individuals in Sweden older than 15 years of age and the universe of operating firms with employees in Sweden. Data are available to us for most individual variables for the 1990-2011 period and for firm financial accounts for the 1997-2011 period.

From LISA, we extract individual-level information on age, gender, civil status, family size, education, university student status, immigrant status, residence municipality, employer, wage income, capital income, retirement income, unemployment days, entrepreneurship status and date of death for individuals. At the firm level, we have information on industry, ownership status, productivity (measured as value added per employee), operating return on assets (OROA) and number of employees. Firms and workers are linked in

November of each year, so we can calculate firm tenure and labor market experience by observing a person's employment history from 1990 to 2011.⁶

3.2 Privatization in Sweden

Like many other countries, Sweden experienced an expansion of the public sector after World War II. In the mid-1980s, the public sector accounted for approximately 37% of employment in Sweden and approximately 63% of GDP, according to Statistics Sweden. With such a large public sector, concerns regarding the efficiency and cost of the public sector arose. As a consequence, Sweden experienced a wave of privatization during the 1990s, and employment in the public sector decreased from approximately 38% in 1987 to approximately 29% in 2017. During the Swedish privatization wave, SOEs were sold mainly through direct sales or PIPOs.

To identify privatizations, we rely on records in LISA that consider a firm to be state-owned if the state, regional government, or municipality holds majority ownership in that firm. We define privatization to be a shift in the classification of ownership from state to private between two consecutive years. We restrict the sample to privatizations of firms with five or more employees that took place between 1997 and 2011 to ensure that we have data on both financial accounts and individual information from LISA. We exclude firms owned by the Swedish church because they were registered as state-owned before 2000. We also exclude a small fraction of firms that alternate between private and state ownership during these years. These jumps could appear because the share of state ownership in these firms varied by approximately 50%, but we cannot verify this statement since data on ownership shares are not available. Additionally, due to confidentiality reasons, we are unfortunately unable to find more details about privatized SOEs. For instance, we do not know how much control the state exerts after privatization, why exactly a given SOE is privatized, how long in advance the privatization was known, and the exact method of privatizing the firm's assets (e.g., share issues, asset sales, or vouchers).⁷

⁶Table IA1 in the Internet Appendix provides more details on the variables.

⁷We follow the literature and focus on the privatization of SOEs, and not on the sales of government-controlled establishments to private firms as in, e.g., Oreland (2010).

Table 1 displays the distribution of privatizations in Sweden over time (Panel A) and by industry (Panel B). Our sample includes 369 firms privatized during the 1997-2011 period. These firms employed 52,930 workers in the year before privatization, resulting in a mean size of approximately 143 workers. The bulk of the privatizations occurred 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 are most common in the business activities and financial intermediaries industries, but these firms are relatively small on average. The largest number of workers affected by privatization—almost 22,000 workers—are in the transport and telecommunication sectors.

3.3 Empirical design

SOEs are not randomly privatized, so our main econometric concern is selection bias. Selection bias occurs if privatizations are correlated with unobserved worker characteristics that in turn are correlated with our outcomes of interest. To mitigate these concerns, we leverage administrative data with data on the entire working age population to implement an event study design. We first construct a control group of workers similar to those in privatized firms but who work in SOEs that remain state owned. We then use a difference-in-differences estimator that compares the outcomes of the treated and control workers before and after a privatization event. This approach has become standard practice in the literature on privatizations and other ownership changes (Brown et al., 2006b; Bastos et al., 2014; Agrawal and Tambe, 2016; Olsson and Tåg, 2017; Antoni et al., 2019; Arnold, 2019).

We model outcome Y of worker i at event year k at calendar year t as

$$Y_{ikt} = \alpha + \pi AFTER_k + \gamma TREATED_i + \beta DID_{ik} + \omega_t + X_i + X_f + \varepsilon_{ikt}, \quad (1)$$

where $AFTER_k$ takes the value one in the year of the privatization and all years after. $TREATED_i$ takes the value one for workers who are employed in an SOE that is privatized one year later (the treatment group) and zero for workers who, in the same year, are employed in an SOE that is never privatized (the control group). ω_t represents calendar-year fixed effects, and X_i includes controls for individual age, sex, immigrant status,

labor market experience, tenure, educational fixed effects and municipality fixed effects. X_f includes firm size and industry fixed effects. We measure all individual- and firm-level controls at event time zero. Finally, DiD_{ik} is the interaction between $AFTER_k$ and $TREATED_i$ and takes a value of one for treated workers in the year of the privatization and all years after and zero otherwise.

The model keeps workers' treatment status constant over time, irrespective of their labor market status before and after the year of treatment assignment. We thus rule out attrition bias because workers leave the sample only if they die or migrate from Sweden. The coefficient β captures the average intention-to-treat effect under the common 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 not observable, it is impossible to formally test the assumption. However, we assessed its plausibility by comparing trends in outcome variables before treatment. Parallel pretrends suggest that past shocks have affected the two groups in the same way. The SUTVA assumption requires that there is no interference between units, in our case, treated and control firms and workers. With the administrative data at hand, we select controls out of the entire population of SOE employees and do not have to rely on controls from the same narrow geographical area the treated workers are in. Thus, it is unlikely that a privatization event in one part of Sweden affects control workers in another part of Sweden.

To capture dynamic effects, we augment Equation 1 in the following way:

$$Y_{ikt} = \alpha + \tau_k + \gamma TREATED_i + \beta_k \sum_{k=-3}^{k=8} \tau_k \times TREATED_i + \omega_t + X_i + X_f + \varepsilon_{ikt} \quad (2)$$

where τ_k denotes event year fixed effects ranging from $k - 3$ to $k + 8$ and $k - 0$ is set as the baseline event year. β_k captures the average intention-to-treat effect during event time k . Our figures plot β_k , and in our tables, we distinguish between short-, medium- and long-run effects by grouping event times 1-2, 3-4 and 5-8 and using the preperiod as the baseline. In all regressions, standard errors are clustered at the local labor market level (municipality of residence) to account for common local labor market shocks.

To create the group of control workers, we apply exact cell matching.⁸ Starting with the universe of registered individuals in Sweden older than 15 years of age, we identify all persons employed in SOEs in Sweden at any time during the period from 1996 to 2010. We restrict the sample to workers between 20 and 60 years of age to ensure that there is information for at least four years before the matching (the LISA database covers persons older than 15 years of age) and that there is at least five years until a person reaches the retirement age of 65 years in the Swedish guaranteed pension system. Next, we define workers as treated if the SOE in which they are employed in any given year is privatized one year later and potential controls if they are employed in an SOE that is never privatized. We then randomly matched each treated worker with a control worker within cells based on gender, industry and calendar year. Matching within cells ensures that the treated and control workers have the same sex and work in the same industry in the same year. The matching is done without replacement so that the control workers (or treated workers) are matched no more than once.⁹

By matching the treated and control workers in the same industry in the same year, our empirical design accounts for yearly macroeconomic and industry shocks—both negative and positive—that affect labor market outcomes. The procedure also assigns an event year to each control worker, allowing us to follow them relative to treated workers over the same event time: three years before the matching and up to eight years after.

Table 1 displays background, educational and career characteristics one year before privatization for the treated and control workers. Columns 1 and 2 display the mean values for the treated and control workers, respectively, and Column 3 displays the difference. To test for mean differences between the treated and control workers, we use the normalized t -value displayed in Column 4.¹⁰ A normalized t -value, in absolute

⁸A viable alternative is to use propensity score matching to construct the control group. One could also consider matching at the firm level rather than at the individual level. Table IA2 in the Internet Appendix shows that our key results are replicated when using this alternative matching strategy.

⁹An alternative would be to use all control workers in a cell and weight the estimates in the regressions. However, then matching without replacement would leave some cells with only a few control observations since workers tend to stay in the same sector and industry for many years.

¹⁰Standard t -values are a function of the sample size and decrease mechanically in the size of the sample. The normalized t -value divides the difference between the two groups by the square root of the sums of their variances, thereby removing this mechanical relationship, as suggested by Imbens and Wooldridge (2009).

terms, above 0.25 indicates a substantial difference in means. On average, the treated and control workers share similar backgrounds and educational and career characteristics, and the normalized t -values are all well below 0.25. The majority of workers are men and older than 40 years, and few of them are immigrants. Most of the workers have only a high school degree, with 14% of the treated workers holding a vocational degree and another 14% holding a university degree. The treated workers have approximately 270,000 SEK in annual wage income on average. Wage income for the control workers is lower but not significantly different. The treated workers are more likely to have two years or less of tenure than the control workers, while the control workers are more likely to have 3-5 years of tenure and 6-10 years of tenure (but once again, the difference is not statistically significant). Overall, there are differences in means between the treated and control workers, but a normalized t -test suggests that these differences are not statistically significant. Nevertheless, we add several of these preprivatization observables to our regressions as controls to account for any remaining differences in observables between the treated and control workers.

4 Wages and unemployment

4.1 Average effects

We begin our analysis by studying two key career-related outcomes: wage income and unemployment. Wage income is a traditional measure used in most other worker-level studies in the literature (Bastos et al., 2014; Arnold, 2019), and unemployment is a particularly costly outcome for workers in regard to scarring effects such as lower subsequent wages, expenses during job search such as consumption and income losses, and lower happiness (Jacobson, LaLonde and Sullivan, 1993; Gruber, 1997; Di Tella, MacCulloch and Oswald, 2001).

Figure 1 displays the yearly differences-in-differences estimates (relative to event time zero) using specification 2 to analyze the dynamics before and after treatment. Panels A and B show that prior to privatization, the treated and control workers have nearly identical trends in wage income and unemployment, with none of the coefficients being significantly different from zero at the five percent level. Post privatization, Panel

A shows that wage income gradually decreases during the first three years after privatization and then gradually rises after five years towards a zero effect at event time eight. Panel B shows a rapid increase in the share of treated workers who become unemployed relative to the control workers directly after privatization. The effect balances out approximately three years after the privatization event and then gradually starts increasing again at event times seven and eight.

Table 3 summarizes these results in a regression table. It reports the average treatment effect for the whole post-period in Panel A using specification 1 and for the short (event times 1 and 2), medium (event times 3 and 4) and long terms (event times 5 to 8) using a modified version of specification 2 with the pre-years as the baseline period. Column 1 in Panel A shows the mean yearly effect on wages for the treated workers during the post-period. The point estimate is -5.5 percent, but it is not statistically significant at the ten percent level. Panel B shows the dynamic effect. In the short run, mean wage income decreases by 3.7 percent, and in the medium run, it decreases by 9.0 percent. Both effects are statistically significant at the one percent level. The coefficient for the long run is smaller and not statistically significant.

Column 2 in Panel A shows effects on unemployment over the full post-period. The incidence increases by 1.7 percentage points per year for the treated workers, which corresponds to an 18.7% increase compared to the level among treated workers before privatization. Panel B shows that unemployment incidence gradually worsens, with a 1.3-percentage-point (14.1%) increase in the short run, a 1.8-percentage-point increase in the medium run (19.4%), and a 1.9-percentage-point increase in the long run (20.7%).

Because we follow workers over time independently of their labor market status, a higher unemployment incidence can mechanically translate into lower wage income. However, two observations suggest that wage income is affected over and above a mechanical effect of unemployment. First, unemployment increases over the post-period, while the effects on wages are U-shaped. Second, we can bound the mechanical effect. Take the estimates in Table 3 for example. These results show that an extra 900 treated individuals end up in unemployment per year in the post-period and that the mean wage cut among all treated workers is 14,800 SEK per year.¹¹ Suppose that the mean wage income for the 900 extra workers who become unemployed

¹¹We multiply the estimate for unemployment incidence by the number of treated workers to obtain the number of extra treated individuals becoming unemployed per year ($0.017 * 52,930 = 899.9$). Wage income decreases by an average of 5.5% per year for

each year corresponds to the mean wage income for all treated workers and that they all remain unemployed for a full year. That is, the total annual wage income for all 900 treated workers decreases by 269,150 SEK. Assume also that there is no direct effect on wages for workers who do not become unemployed. Under these assumptions, the mechanical effect on wage income from increased unemployment is a decrease of 4,500 SEK per year $((900 * 269.15) / 52,930 = 4.49)$. Thus, the increase in unemployment seems to explain at most approximately one-third of the estimated income loss of 14,800 SEK per year.

4.2 Heterogeneous effects

The career effects of privatizations vary across workers. Table 4 reports selected coefficients from a triple-difference regression for wages (Panel A) and unemployment incidence (Panel B).¹² We divide workers based on whether they are more likely to have been protected by state ownership prior to privatization and hence are more likely to be affected by privatization.

State-owned firms may offer safer employment and hesitate to introduce modernization efforts that negatively impact workers but increase productivity. Privatization can trigger such investments if state-owned firms avoid undertaking them because of widespread agency problems or because of a fear of worker backlash. Technological upgrading should primarily benefit skilled workers over unskilled workers, and younger workers are less likely to have outdated labor market skills than older workers. We also expect that workers with stronger employment protections do better than those with weaker protections. The Swedish employment protection legislation, LAS ("Lagen om anställningsskydd"), states that temporary employment contracts become permanent after two consecutive years of employment. Because workers on permanent contracts are much harder to fire than those on temporary contracts, two years of tenure come with increased labor market protections. Additionally, LAS states that firms that have a shortage of work need to follow a tenure-based dismissal order when downsizing their workforce. In practice, this means that the last worker hired should be the first worker dismissed. Thus, we distinguish between skilled and unskilled workers

all treated workers after privatization. In comparison to the pre-mean for treated workers, the decrease corresponds to 14,800 SEK per treated worker $(-0.055 * 269.15 = -14.80)$.

¹²Table IA3 in the Internet Appendix provides a supplementary subsample analysis separately for each category.

(holding a university degree or not, respectively), young and old workers (below and above the median age of 43 years, respectively), tenured versus non-tenured workers (having two years or more of tenure or less than two years of tenure, respectively) and senior and non-senior workers (determined by when the employee was hired).

Consistent with these hypotheses, Table 4 shows that skilled workers do better than unskilled workers, having 16.8% higher wage income and 12.9% lower unemployment incidence after privatization. Young workers experience a 10.7% higher wage income and 22.6% lower unemployment incidence relative to old workers. Tenured workers with stronger labor market protections see a 7.6% relative improvement in wage income and a 15% decline in unemployment incidence relative to nontenured workers. We find the same pattern when separating workers by seniority, where workers who were hired last fare worse than those with higher seniority. The results for tenure and seniority underscore the importance of accounting for employment protection legislation in the context of privatization (as discussed by Subramanian and Megginson (2018)). It also suggests that our estimates could be viewed as a lower bound on the worker costs of privatization. In 2004, Sweden had the seventh-strongest employment protection among 30 OECD countries (OECD, 2004).

5 Other career outcomes and spillovers on family and health

To further understand how privatizations affect workers, we complement the analyses on income and unemployment with analyses on several other career- and non-career-related outcomes. The additional career-related outcomes we study are unemployment days, being employed, leaving the labor force and retiring. Table 5 presents the results, with accompanying figures on the dynamic effects shown in Figure 2. Column 1 in Panel A shows that over the full eight-year post-period, the mean number of unemployment days increases by 2.7 days per year, which corresponds to a 29% increase relative to the pre-period mean. Panel B shows that this effect monotonically increases over time up to a 32% increase in the long run. These effects are statistically significant at the one percent level. We do not find any overall effects on being employed or remaining outside the labor force. However, we do find small transitory effects of a 0.7%-1.4% decline in

the short and medium terms for being employed (Column 2) and a short-run increase in remaining outside of the labor force of 3.2 basis points, or 21.6% (Column 3). Privatization does not appear to hasten the decision to retire early (Column 4).

The estimates show that privatization leads to an extra 900 unemployment spells and 142,911 days of unemployment per year. Over the eight-year post-period we analyze, these figures aggregate to a total of 7,200 unemployment spells and 1,143,288 unemployment days. While these figures are too small to impact the whole economy—the average annual number of unemployment days in Sweden in 1992-2010 was slightly less than 100 million—they reveal that at a personal level, privatization can be bad news for some workers, as those who become unemployed tend to remain unemployed for an average of 159 days.¹³

We are also able to study the effects on non-career-related outcomes. Existing research has found that unemployment is coupled with increases in the probability of divorce and with higher mortality (Gerdtham and Johannesson, 2003; Eliason, 2012). The permanent increase in unemployment following privatization could thus be accompanied by negative spillovers on family and health. Thus, we also examine how privatizations affect the probability of divorce and passing away. Columns 5 and 6 in Table 5 display these effects. The dynamic effects are show in Figure 2. We find no effects on divorce and a statistically significant, but economically insignificant, reduction in mortality of 0.04%. Thus, there does not appear to be any economically meaningful spillovers from privatization to family and health.

6 Entrepreneurship

We next turn to studying whether privatization is coupled with increased new firm creation by former employees of SOEs. Entrepreneurship is a particularly interesting career outcome, as empirical evidence has shown that entrepreneurship is associated with significant positive spillovers on the economy. Entrepreneurs tend to contribute to new job creation (Haltiwanger et al., 2013), the introduction of new products and ser-

¹³The unemployment incidence increases on average by 1.7 percentage points per year, which results in $0.017 \times 52,930 = 899.8$ extra workers becoming unemployed per year during an eight-year post-period. The number of unemployment days increases on average by 2.7 days per year, which results in a total of $2.7 \times 52,930 = 142,911$ extra days. Dividing the total 142,911 days of unemployment by the extra 900 workers who end up unemployed shows that workers who become unemployed tend to remain unemployed for 159 days.

vices (Acemoglu et al., 2018; Akcigit and Kerr, 2018), the transfer of ideas from incumbent firms to the market (Audretsch et al., 2008), and the creation of most of the new employment following positive demand shocks (Adelino et al., 2017).

Two hypotheses indicate that privatizations could encourage entrepreneurship. First, reorganization in the wake of privatization could force workers into entrepreneurship out of necessity because they are laid off and cannot find another job. In line with this finding, von Greiff (2009) shows that job displacement almost doubles the probability of entering self-employment. This suggests that entrepreneurship can keep workers from unemployment and provides a valuable transitory source of income until another job is secured. This type of entrepreneurship, while valuable, is unlikely to have large spillovers on society in terms of innovation or bringing new products to the market.¹⁴ Second, reorganization and new owners may encourage innovative entrepreneurship and able workers within the firm to see the change in ownership or culture as an opportunity to bring innovations and ideas to the market. For instance, Babina et al. (2020) finds that ownership changes in the form of IPOs encourage high-skill workers to form new firms, and Babina and Howell (2020) finds that corporate R&D spending encourages workers to join founding teams of startups. This form of entrepreneurship could have substantial spillovers on society.

6.1 Average effects

Our analysis separates two types of entrepreneurship entry: self-employment (sole proprietorship) and business ownership (owning a limited liability firm).¹⁵ Figure 3 shows parallel trends in both self-employment and business ownership prior to the privatization event. Panel A shows that self-employment temporarily increases for years two to four but then reverts back with statistically insignificant coefficients for years five

¹⁴A selection effect may also come into play. Özcan and Reichstein (2009) study entrepreneurship entry from the public sector and investigate why the entry rate is so low relative to entry rates from private firms. The authors show that a key reason for the low rate is that nonentrepreneurial people select into the public sector.

¹⁵The separation between necessity and opportunity entrepreneurship is common in the literature, although the naming of the two types of entrepreneurship tends to differ. See, for instance, Schoar (2010), Tåg, Åstebro and Thompson (2016), Levine and Rubinstein (2017), Rider, Thompson, Kacperczyk and Tåg (2019) and Guzman and Stern (2020). Åstebro and Tåg (2017) show that almost all of the job growth in employment occurs in new ventures formed as limited liability companies. Starting a sole proprietorship makes more sense for non-growth-oriented firms than for growth-oriented firms since the initial capital requirements and reporting requirements are lower than those for limited liability companies.

to eight. The pattern is different for business ownership. Panel B shows an increase in business ownership during the entire post-period.

Table 6 summarizes these results in a regression table. It reports the average treatment effect for the whole post-period in Panel A using specification 1 and for the short (event times 1 and 2), medium (event times 3 and 4) and long terms (event times 5 to 8) using a modified version of specification 2 with the pre-years as the baseline period. Panel A, Column 1 displays the average effect on self-employment for the treated workers during the entire post-period. It shows a 13.2-basis-point (44.9%) increase in the rate of self-employment, but it is not statistically significant at the ten percent level. Panel B shows the inverse U-shaped dynamic effect. There was a statistically significant 8-basis-point increase in the short run (27.2%) and a 21.7-basis-point increase in the medium run (73.8%). The coefficient for the long run is smaller and not statistically significant. Column 2 in Panel A shows a statistically significant increase in business ownership of 26.3 basis points (230.7%) over the full post-period. Panel B shows the monotonic increase in business ownership: a 5.6-basis-point (49.1%) increase in the short run, a 20.5-basis-point (179.8%) increase in the medium run and a 41-basis-point (359.6%) increase in the long run. Taken together, these effects translate into approximately two additional entrepreneurs per privatized SOE or 12 additional entrepreneurs per thousand workers affected by privatization.¹⁶

6.2 Mechanisms and spillovers from entrepreneurship entry

Table 7 displays the characteristics of the entrepreneurs and the firms they run over the full eight-year post-period. Several pieces of evidence point towards necessity entrepreneurship as the main explanation for increased entrepreneurship entry. First, Column 1 in Panel A shows that a majority of the new entrepreneurs (72.5%) are self-employed and thus run unlimited liability firms that almost never hire other employees (Astebro and Tåg, 2017). As Table 6 shows, entrepreneurship in the form of self-employment tends to be transitory, which is consistent with necessity entrepreneurship. However, business ownership is more common among workers who have gone through a privatization relative to workers in the control group

¹⁶Our sample includes 369 SOEs and 52,930 workers who go through a privatization, and the difference in unique entrepreneurs in the post-period is 644, as shown in Table 7.

(Columns 2 and 3). Second, although treated workers earn more than control workers, entrepreneurship is not lucrative. The total post-period income of the individuals who entered entrepreneurship (144 TSEK) is approximately half of their pre-period income (291 TSEK).¹⁷ Third, Panel B shows that an overwhelming majority (96%) of the entrepreneurs run firms with fewer than 10 employees. The treated workers, however, tend to start fewer firms with 1-10 employees and slightly more firms with 10-50 employees. Moreover, while the treated workers start firms that have higher productivity on average than the firms of the control workers, productivity measured as value added per employee is only 333 TSEK compared to 503 TSEK for the SOE firms that were privatized. Taken together, the spillovers from entrepreneurship entry appear to be modest. The entrepreneurs in our sample generate approximately 1.4 extra job-years per privatized firm or approximately 10 additional jobs-years per thousand privatized workers. Furthermore, they create additional value added equal to only 2.4% of the value added of the privatized firms.¹⁸

7 Firm-level outcomes

We next investigate whether the worker costs documented above are offset by firm-level performance improvements after privatization. To this end, we model the outcome Y of firm f at time k at calendar year t as

$$Y_{fkt} = \alpha + \omega_t + \pi AFTER_k + \gamma TREATED_f + \beta DID_{fk} + X_f + \varepsilon_{ikt}, \quad (3)$$

where the corresponding elements of the model are defined in a similar fashion as those for the worker-level analysis. We also make similar adjustments to estimate event time effects (displayed in the figures) and dynamic effects (displayed in the tables).

To construct the control group, we implement cell matching one year before the privatization but now at the firm level. The matching is done within year and industry cells. Therefore, for each treated firm (an

¹⁷We focus on total income in this section, as entrepreneurs in Sweden can choose to earn income from their businesses either as wage income or as capital income. Capital income is negative when an individual reports a tax-deductible capital loss.

¹⁸Our sample has 369 SOEs and 52,930 workers who go through a privatization, generating a total of $503.47 \times 52,930 = 26,648,667$ TSEK in value added prior to privatization. Column 3 in Table 7 shows that we have 1,380 additional firm-year observations, with a mean employment difference of 0.38 workers, giving us 524 additional job-years. For value added, using the numbers in Table 7, we obtain $(333.58 \times 6.71 - 281.29 \times 6.33) \times 1,380 = 631,703$ TSEK in additional value added.

SOE that is private one year later), we randomly pick one control firm (an SOE that is not privatized).¹⁹ We then gather information for the treated and control firms before and after the year of the matching. It is important to note, however, that in contrast to our worker-level analysis, our firm-level analysis could suffer from attrition bias because ownership changes such as privatizations are often associated with complex restructurings, such as internal reorganizations, acquisitions, and divestitures, causing the firm identifier to change. As a consequence, we limit the post-period to four years, corresponding to the short- and medium-run periods used in the individual-level analysis.²⁰

7.1 Average effects

Figure 4 displays yearly difference-in-differences estimates relative to the year prior to privatization. The treated and control firms have similar pretrends in mean employment, wages, productivity and operating return on assets (OROA). After privatization, productivity and OROA for the treated firms increase relative to the control firms, while employment and wages remain flat.

Table 8 presents selected coefficients from Equation 3. Columns 1 and 2 show positive but statistically insignificant effects on employment and wages both overall (Panel A) and in the short and medium terms (Panel B).²¹ Column 3 in Panel A shows that after privatization, productivity increases by a statistically significant 8.2% on average over a four-year post-period compared to the pre-period. The dynamic results in Panel B show that productivity increases over time, with a 7.6% increase in the short run and a 9% increase in the medium run relative to the preperiod mean for privatized SOEs. Finally, Column 4 shows that increases in profitability accompany increases in productivity. For the full post-period, profitability increases by 4.5%; in the short run, it increases by 4.1%; and in the medium run, by 4.9%. These effects are all statistically

¹⁹Table IA4 in the Internet Appendix displays differences in observables between the treated and control firms. Finding good controls at the firm level is harder than finding them at the individual level due to the number of SOEs that operate in Sweden; as a result, the treated and control firms differ slightly in observables prior to privatization. What is important for our empirical strategy, however, is parallel trends in outcomes and not perfect balancing in observables. Table IA5 in the Internet Appendix displays the results using propensity score matching instead of cell matching.

²⁰In Table IA6 in the Internet Appendix, we confirm that our results are robust to various ways of accounting for attrition by imputing values for the firms that drop out of the dataset.

²¹Table IA7 in the Internet Appendix shows that the results for employment and wages are also statistically insignificant when we restrict the sample to firms with information on productivity and performance.

significant at the one percent level.

The lack of effects on net employment and wages at the firm level despite the worker-level evidence of income losses and higher unemployment point to the importance of worker churn as a mechanism through which profitability and productivity improve after privatization. The results suggest that firm-level employment effects may not be informative about the social costs for workers, as they ignore potential adverse effects of churn on workers.

7.2 Firm-level productivity gains versus worker-level wage losses

Privatization can trigger several welfare effects. For firms, productivity can change, the turnover of workers can lead to severance payments when terminating employees, and the firm may incur search costs of finding new employees. For workers, wage income can change (either via a direct wage effect or through unemployment), and unemployment can have an adverse effect on health, may induce a search cost, and cause scarring effects when pursuing a career outside of the privatized firm. If entrepreneurial activity increases, it can have positive effects on the entrepreneur and any workers he or she hires, but it can also have a negative effect if it leads to lower income for the entrepreneur. Moreover, the state's coffers may be lined with proceeds from sales and increased corporate tax revenues due to the higher productivity of privatized firms, but they may also be emptied due to higher unemployment benefits it pays to workers who are laid off. Finally, there may be general equilibrium effects such as changes in firms' competitiveness that affect rivals or spill over to other workers in the local labor market if layoffs from privatizations increase. Capturing all these dimensions is challenging, even with the detailed data at our disposal. We can, however, compare productivity gains for firms that are privatized to incumbent worker income losses.

From Table 8, we find that firm-level productivity increases by 8.2% over a four-year post-period, which corresponds to 9,160,204 TSEK. On the worker side, Table 3 shows that wage income decreases by 3.7% on average in the short run and 9% on average in the medium run. Because we track incumbent workers over time irrespective of their labor market status, these estimates comprise any direct wage effect for the privatized firm and any indirect effect for those that leave the privatized firm. Over the four-year post-period,

the estimates translate into a total wage income loss for all treated workers of 1,810,206 TSEK. Thus, the costs of privatization for incumbent workers are offset by increases in productivity at the firm level by a factor of five.²²

8 Discussion

8.1 Anticipation effects

A potential threat to our identification strategy is if retrenchment and restructuring programs are implemented before privatization to increase the selling price or if certain workers choose to leave before privatization because they do not want to work for a private company or dislike the uncertainty that comes with a new owner. If so, our sample of incumbent workers is endogenous to the privatization itself. Lopez-de-Silanes (1997) shows that labor reorganization prior to privatization leads to higher privatization prices. Chong et al. (2011) shows that retrenchment programs before privatizations can lead to skimming and adverse selection, with the highest-ability workers being skimmed out of the privatized firm.

Three pieces of evidence suggest that retrenchment and restructuring programs are not extensive in our sample. First, in our firm-level analysis, we show that the number of employees in SOEs in our sample is stable in the years before privatization, suggesting that large retrenchment programs affecting workers tend not to be implemented. Second, we match the treated and control workers the year before a privatization takes place, which mitigates any short-term anticipation effects. Third, our key results continue to hold when we match three years prior to privatization instead of one year prior. Matching three years prior to the actual privatization event biases the estimates towards zero, as workers may have had time to leave the firm and thus are not affected by the privatization. However, it has the added benefit of accounting for potential separations prior to privatization due to potential retrenchment programs.²³

²²Value added per employee increases by 41.1 TSEK per year. With 369 firms employing 151 workers on average, the total increase in productivity equals $41.1 \times 151 \times 369 \times 4 = 9,160,203.6$ TSEK. Mean wage income for the treated worker is 269.2 TSEK in the pre-period. The average income loss in the short period is then 10 TSEK per year and worker and 24.2 TSEK per year in the medium run. With 52,930 workers, the total wage loss during the four-year post-period is $34.2 \times 52,930 = 1,810,206$ TSEK.

²³Table IA8 in the Internet Appendix provides these results.

8.2 Alternative drivers of entrepreneurship entry

Two alternative mechanisms behind increased entrepreneurship entry merit deeper investigation. First, the increase in business ownership may be driven by management buyouts in which the management of the SOE buys the company from the state and thus becomes a business owner. This phenomenon occurs in our data, but only in a total of 15 cases during our sample period for the treated firms. Moreover, if this were the main mechanism driving the result, we would observe an immediate jump upwards in business ownership at event time one in Figure 3, not a gradual increase. Therefore, while management buyouts do occur, they are too rare to drive the pattern in the aggregate. Second, increased rates of entrepreneurship may be driven by the deregulation of entire industries in which entrepreneurial opportunities open up for treated workers and not for control workers. However, this is not a plausible explanation, given that we create our control group within industries, so the control workers come from SOEs in the same industry. Moreover, the deregulated industries during the period from 1997 to 2010 are domestic EU/EES airline industry (1997), railroad and public transport industry (2007, 2009, 2010 and 2012), the preschool market (2006), the pharmacy industry (2009), primary health care (2010) and the technical testing and analysis of vehicles industry (2010). In our sample, only two privatizations occur in these industries in the year of deregulation or later, and these firms employ only 199 workers.

8.3 Relationship to the existing literature

How do our findings relate to previous studies on the costs of privatization for workers? Table 9 summarizes the key incumbent worker-level studies on privatization and a selection of firm-level studies most relevant to this paper. Our results on temporary wage declines for workers are in line with evidence from Switzerland in Melly and Puhani (2013), who finds that average wages follow a J-curve with an immediate drop and then a recovery. Our findings are also in line with Arnold (2019), who finds wage decreases in Brazil. In Brazil, however, the wage drop is much larger than that in Sweden, and it does not recover over time. A candidate explanation for these differences is that SOEs in Brazil pay a wage premium to workers that erodes after privatization. We are not aware of any studies documenting such wage premia for SOEs in Sweden.

Brown et al. (2006a) also document wage declines for Ukraine. Consistent with our results, Brown et al. (2006a), Melly and Puhani (2013) and Arnold (2019) all document relatively larger wage declines for low-skill workers, and Melly and Puhani (2013) additionally finds larger wage declines for older workers. One study documented increases in wages after privatization, counter to what we find. Bastos et al. (2014) uses data for Portugal to show that wages increase because the wage floor negotiated via collective bargaining increases and because privatized firms pay larger markups on union wage floors. Given the clear mechanisms for wage increases documented in Bastos et al. (2014), a likely candidate for the diverging wage results in Portugal and Sweden are differences in wage-setting institutions. It is worth mentioning that consistent with our findings, Bastos et al. (2014) document relative wage decreases for low-skill workers.

At the firm level, our results of profitability and productivity increases and no effects on net employment are broadly consistent with the existing literature. Megginson and Netter (2001), who survey many firm-level studies on privatization, conclude that productivity and profitability usually increase, while the effects on firm-level employment vary across studies. For instance, our results align well with those of D'souza and Megginson (1999) on profitability, productivity, and employment changes after privatization in 28 countries and with those of Brown, Earle and Telegdy (2010) on employment in the manufacturing sector of Hungary, Russia, Ukraine, and Romania. Our results for profitability are in line with La Porta and Lopez-de-Silanes (1999) in Mexico but not in terms of effects on employment since La Porta and Lopez-de-Silanes (1999) documents employment declines for both blue- and white-collar workers. We also do not find evidence of extensive labor restructuring programs prior to privatization, similar to Chong et al. (2011).

9 Conclusion

In this paper, using rich administrative data on firms and workers covering two decades, we study how the privatization of SOEs affects the careers and welfare of workers. While the benefits of privatization in terms of improved governance, productivity, and performance are well documented, the impacts on workers employed in firms that go private are less well studied.

We show evidence of an initial reallocation of human capital, with transitory wage declines in the short

and medium terms and permanent increases in unemployment. State-owned firms may offer safer employment than private firms and hesitate to introduce modernization efforts that negatively impact workers. Consistent with this notion, unskilled workers, older workers, and workers with weaker employment protection are hit the hardest by privatization. We also study broader measures of worker welfare related to family and health since earlier research has found that unemployment leads to divorce and higher mortality. We do not find evidence of such spillovers from privatization.

We then study whether privatization encourages entrepreneurship and find that it does but that the economic spillovers entrepreneurs create are modest. Reorganization in the wake of a privatization appears to force workers into entrepreneurship out of necessity, as three-quarters of all entrepreneurs are self-employed, the effects on self-employment are transitory, an overwhelming majority of the entrepreneurs run firms with fewer than 10 employees, and the mean total income as entrepreneurs is cut in half during the post-entrepreneurship period relative to their income before privatization. As a result, the spillovers from entrepreneurship entry are small, with the entrepreneurs creating only approximately ten additional jobs per thousand privatized workers.

Finally, we show that at the firm level, profitability and productivity increase, while net employment and the wage bill remain unchanged. The upside for firms in terms of higher productivity is approximately five times larger than the downside for workers in terms of income losses.

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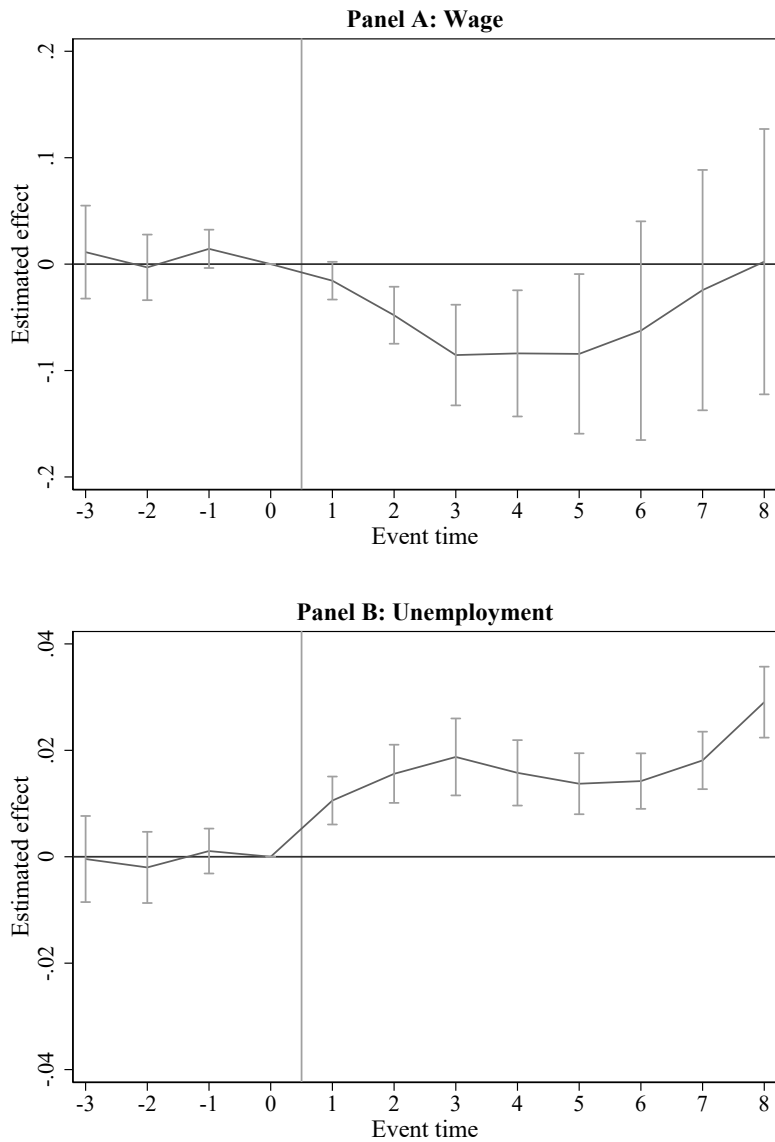


Figure 1: Effects on wage and unemployment. The figures display yearly differences-in-difference estimates relative to the year prior to the privatization (event time 0). The vertical bars display 95% confidence intervals using robust standard errors clustered at the municipality level. The sample consists of treated workers employed one year prior to the privatization and matched control workers. We control for age, gender, immigrant status, labor market experience, tenure, education, firm size, municipality, industry and calendar year. All controls, except the calendar year, are measured one year prior to the privatization. *Wage* is the log of annual wage income (using inverse hyperbolic sine transformation) and *Unemployment* takes the value one if an individual was unemployed at any time during the year and zero otherwise.

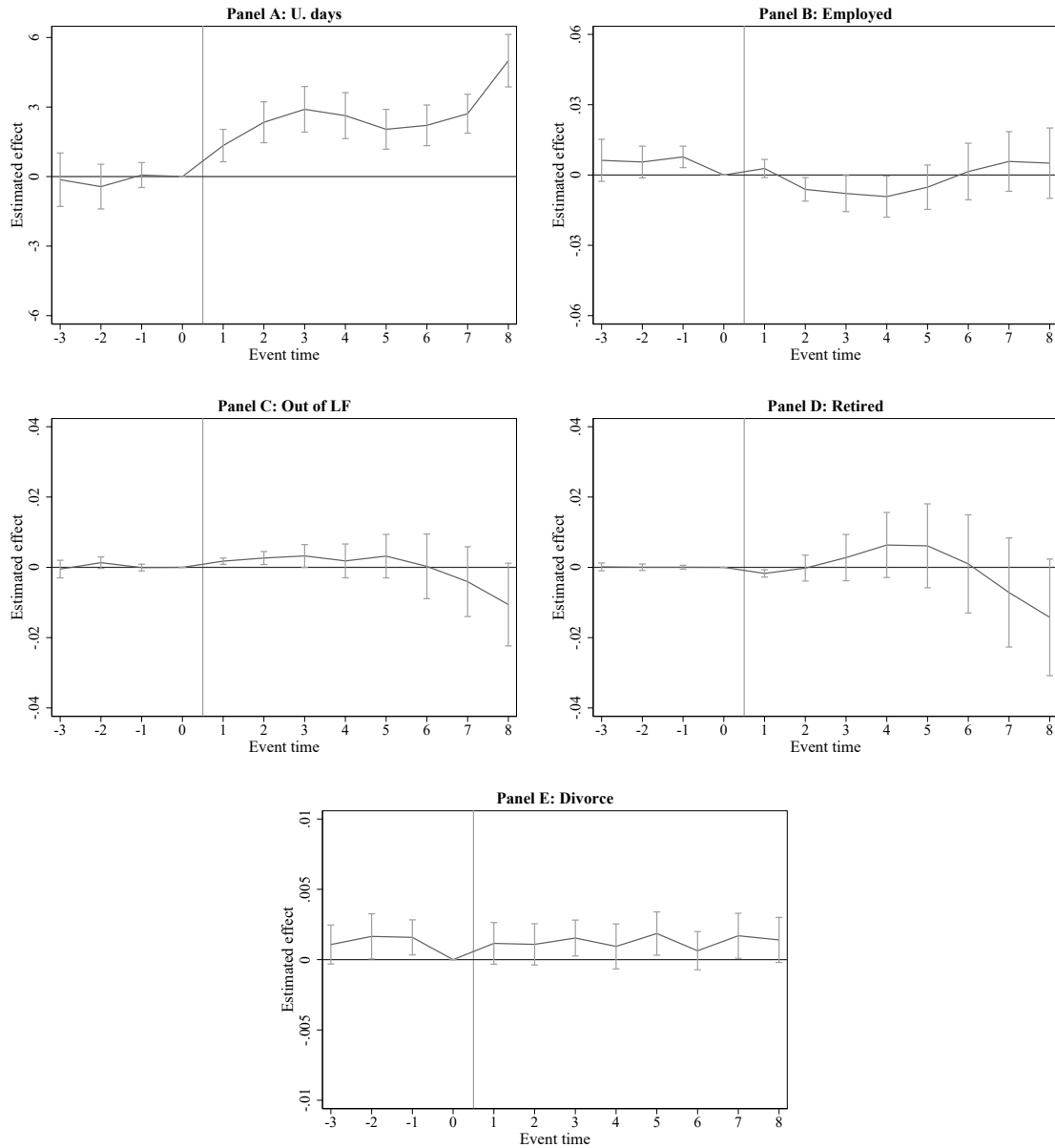


Figure 2: Effects on other outcomes. The figures display yearly difference-in-differences estimates relative to the year prior to the privatization (event time 0). The vertical bars display 95% confidence intervals using robust standard errors clustered at the municipality level. The sample consists of treated workers employed one year prior to the privatization and matched control workers. We control for age, gender, immigrant status, labor market experience, tenure, education, firm size, municipality, industry and calendar year. All controls, except the calendar year, are measured one year prior to the privatization. *U. days* is the yearly total number of unemployment days, *Employed* takes the value one if an individual was employed in November in a given year and zero otherwise, *Out of LF* takes the value one if an individual has no employment, no wage income, and no unemployment days in a given year and zero otherwise, *Retired* takes the value one if an individual receives retirement income in a given year and zero otherwise. *Divorce* takes the value one in a given year if a person who was married one year earlier is registered as unmarried this year and zero otherwise.

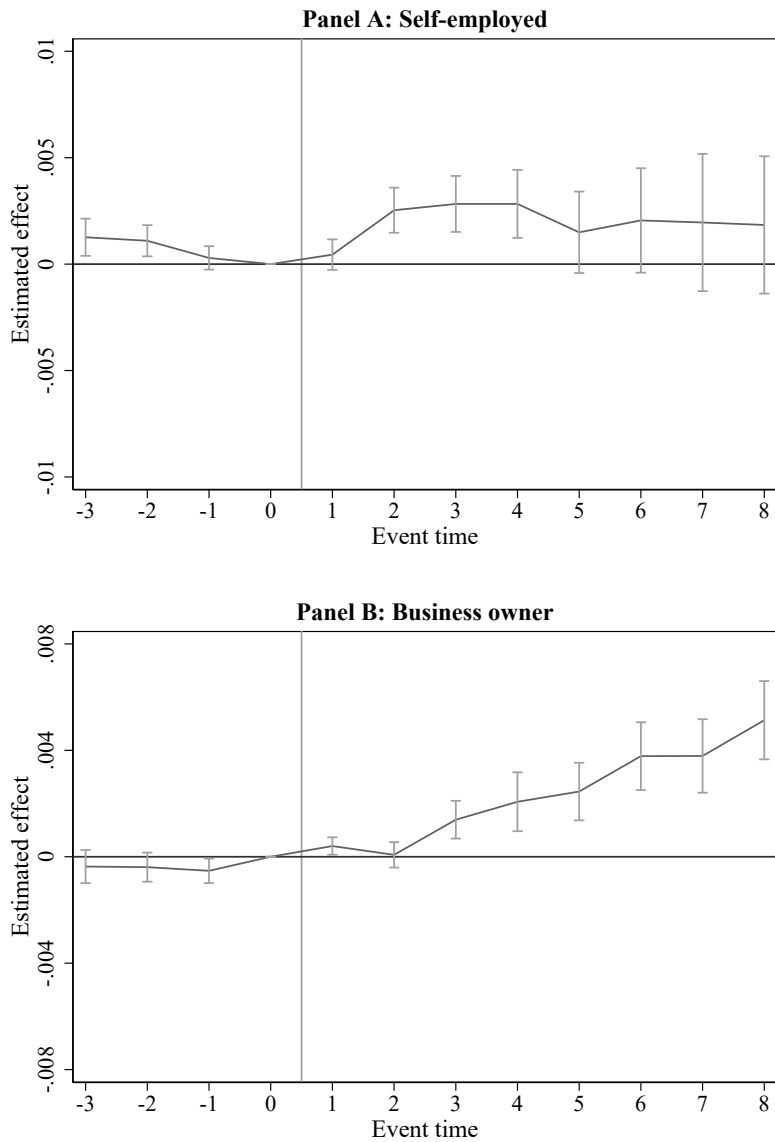


Figure 3: Effects on entrepreneurship. The figures display yearly difference-in-differences estimates relative to the year prior to the privatization (event time 0). The vertical bars display 95% confidence intervals using robust standard errors clustered at the municipality level. The sample consists of treated workers employed one year prior to the privatization and matched control workers. We control for age, gender, immigrant status, labor market experience, tenure, education, firm size, municipality, industry and calendar year. All controls, except calendar year, are measured one year prior to the privatization. *Self-employment* takes the value one if an individual runs an unlimited liability sole-proprietorship and zero otherwise. *Business ownership* takes the value one if an individual is employed in a limited liability company (aktiebolag) he or she owns and zero otherwise.

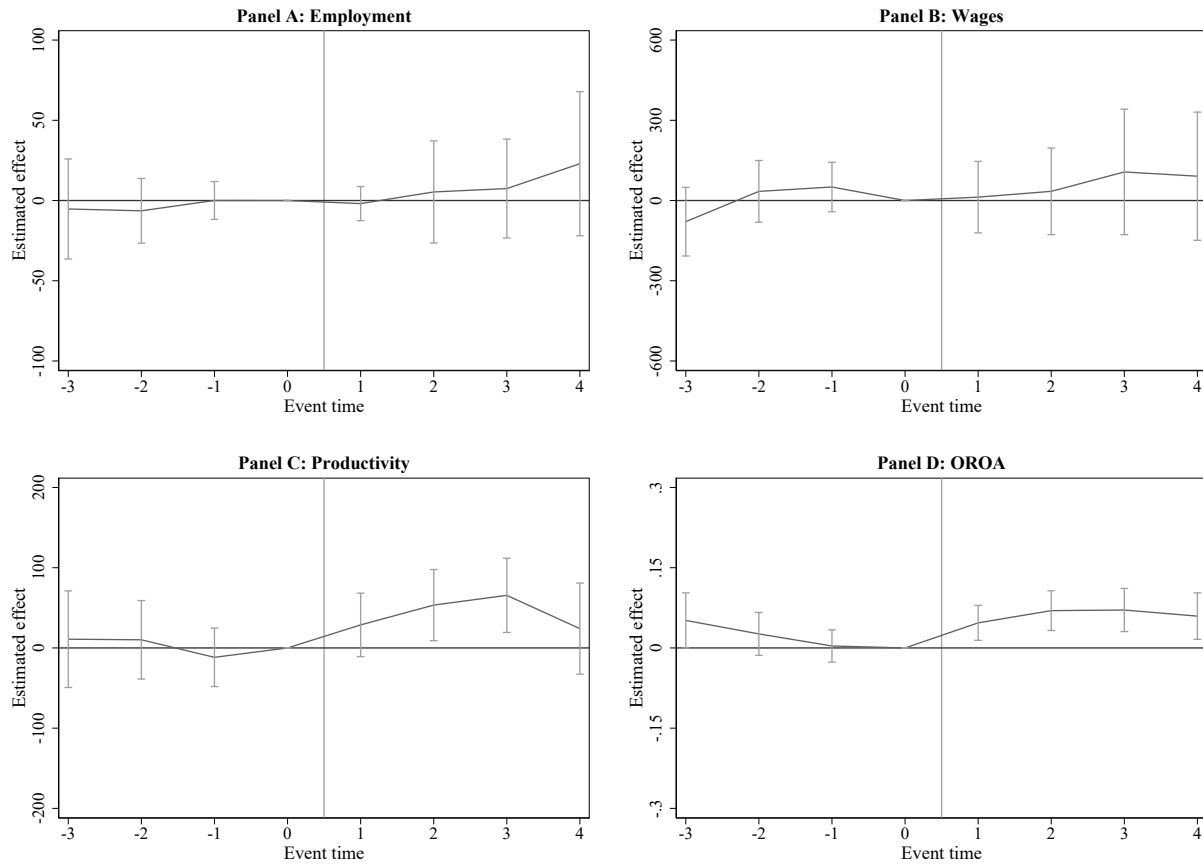


Figure 4: Effects on firm-level outcomes. The figures display yearly differences-in-difference estimates relative to the year prior to the privatization (event time 0). The vertical bars display 95% confidence intervals using robust standard errors clustered at the firm level. The sample consists of treated workers employed one year prior to the privatization and matched control workers. We control for firm age, municipality, industry and calendar year. All controls, except calendar year, are measured one year prior to the privatization. *Employment* measures number of employees, *Wages* sum the annual wage income of workers employed in the firm in hundreds of SEK, *Productivity* is value added per employee in thousands of SEK and *OROA* is operating return on assets.

Table 1: Privatizations vary over time and across industries

The sample consists of privatizations of firms with more than five employees that took place between 1997 and 2011. Workers refers to the number of employees in the privatized firms one year before the privatization. The industry classification is based on NACE Rev 1.1.

Panel A: Privatizations by year

Year	Firms	Workers	Year	Firms	Workers
1997	59	10,112	2005	8	332
1998	33	2,410	2006	0	0
1998	22	823	2007	5	100
2000	53	7,703	2008	8	1,094
2001	18	1,536	2009	23	1,158
2002	45	6,992	2010	17	1,646
2003	54	13,528	2011	21	2,092
2004	3	3,414			
				Σ 369	Σ 52,930

Panel B: Privatizations by industry

Industry	Firms	Workers
Agriculture	2	75
Mining, manufacturing and utilities	60	10,800
Construction	11	1,036
Wholesale, retail and repair	20	684
Hotels and restaurants	20	2,646
Transport and telecommunications	55	21,635
Business activities and financial intermediary	160	12,538
Education	5	115
Public administration, health and social work	15	2,306
Community social and personal activities	21	1,095
	Σ 369	Σ 52,930

Table 2: Comparison of treated and control workers

The table displays mean characteristics of treated and control workers one year before the privatization. A normalized t-test above 0.25 indicates substantial differences in means (Imbens and Wooldridge, 2009). *Wage* is total annual wage income in thousands of SEK, *Labor market experience* is the years since entering the labor market, and *Tenure* is measured as number of years employed in the firm.

	Treated (1)	Control (2)	Difference (3)	Norm. T-value (4)
Background				
Female (%)	31.88	31.88	0	0
Immigrant (%)	14.10	11.35	2.77	0.06
Age (%)				
16–20	0.94	1.26	-0.32	-0.02
21–30	17.74	15.80	1.96	0.04
31–40	25.91	23.69	2.22	0.04
41–50	27.87	29.96	-2.09	-.03
50+	27.54	29.31	-1.78	-0.03
Education (%)				
Basic	0.17	0.17	0.01	0.01
High School	0.54	0.59	-0.04	-.06
Vocational	0.14	0.11	0.03	0.06
University	0.14	0.13	0.01	0.02
Career				
Wage	269.15	241.08	28.07	0.13
Labor market experience (%)				
0–5	13.29	12.42	0.87	0.02
6–10	11.29	9.85	1.43	0.03
11–20	26.10	25.05	1.05	0.02
21–30	22.40	24.63	-2.24	-0.04
30+	26.92	28.04	-1.12	-0.02
Tenure (%)				
0–2	57.74	48.59	9.16	0.13
3–5	25.70	26.84	-1.14	-0.02
6–10	13.37	21.26	-7.89	-0.15
11–15	2.39	2.35	0.04	0.00
16+	0.80	0.96	-0.16	-0.01
Observations	52,930	52,930		

Table 3: Wage and unemployment

This table reports selected coefficients from difference-in-differences regressions explaining worker outcomes after privatization. The sample consists of treated workers employed one year prior to the privatization and matched control workers. We control for age, gender, immigrant status, labor market experience, tenure, education, firm size, municipality, industry and calendar year. All controls, except calendar year, are measured one year prior to the privatization. *Wage* is the log of total wage income (using inverse hyperbolic sine transformation) and *Unemployment* takes the value one if an individual was unemployed at any time during the year, zero otherwise. The % effect uses the mean for workers in privatized firms in the years before the privatization as the baseline. Short run refers to event times 1 and 2, medium run to event times 3 and 4 and long run to event times 5 to 8. The standard errors are clustered at the municipality level and three stars denote statistical significance at the one percent level.

	Wage (1)	Unemployment (2)
Panel A: Average effect		
Full Period	-0.055 (0.040)	0.017*** (0.003)
% effect	-5.5	18.7
R^2	0.05	0.06
Panel B: Dynamic effect		
Short run	-0.037*** (0.014)	0.013*** (0.003)
Medium run	-0.090*** (0.025)	0.018*** (0.498)
Long run	-0.048 (0.056)	0.019*** (0.003)
% effect		
Short run	-3.7	14.1
Medium run	-9.0	19.4
Long run	-4.8	20.7
R^2	0.07	0.06
Preperiod mean	8.125	0.093
Observations	1,184,598	

Table 4: Heterogeneous effects

The table reports selected coefficients from triple-difference regressions explaining worker-year outcomes after privatization. The sample consists of treated workers employed one year prior to the privatization and matched control workers. We control for age, gender, immigrant status, labor market experience, tenure, education, firm size, municipality, industry and calendar year. All splits and controls are defined one year prior to the privatization. *Wage* is the log of total annual wage income (logged using inverse hyperbolic sine transformation) and *Unemployment* takes the value one if an individual was unemployed at any time during the year, zero otherwise. In column 1, workers are split into skilled and unskilled (holding a university degree or not). In column 2, workers are split into young and old (below and above the median age of 43 years). In column 3, workers are split into tenured and non-tenured (having two years or more of tenure or less than two years of tenure) because Swedish Law requires firms to provide a permanent position for workers after two years of uninterrupted employment. In column 4, workers are split into senior and non-senior (whether the employee was the last one hired or not) because the Swedish employment protection legislation stipulates a tenured-based order of dismissal at times of redundancies, a so called last-in-first-out rule. The % effect relates the triple-difference coefficient (DiDiD) to the preperiod mean for treated workers. Standard errors are clustered at the municipality level. Three stars denotes significance at the one percent level, two at the five percent level, and one at the ten percent level.

	Skilled (1)	Young (2)	Tenured (3)	Senior (4)
Panel A: Wage				
DiDiD	0.168*** (0.048)	0.107* (0.056)	0.076** (0.035)	0.091* (0.050)
DiD	-0.082* (0.042)	-0.146*** (0.016)	-0.147*** (0.049)	-0.139** (0.064)
% effect	16.8%	10.7%	7.6%	9.1%
Panel B: Unemployment				
DiDiD	-0.012** (0.006)	-0.021*** (0.004)	-0.014*** (0.005)	-0.013** (0.006)
DiD	0.019*** (0.004)	0.030*** (0.003)	0.033*** (0.006)	0.029*** (0.007)
% effect	-12.9%	22.6%	-15.0%	-14.0%
Observations	1,184,598			

Table 5: Other outcomes

This table reports selected coefficients from difference-in-differences regressions explaining worker-year outcomes after privatization. The sample consists of treated workers employed one year prior to the privatization and matched control workers. We control for age, gender, immigrant status, labor market experience, tenure, education, firm size, municipality, industry and calendar year. All controls, except calendar year, are measured one year prior to the privatization. *Unemp. days* is the yearly total number of unemployment days, *Employed* takes the value one if an individual was employed in November in a given year and zero otherwise, *Out of LF* takes the value one if an individual has no employment, no wage income, and no unemployment days in a given year and zero otherwise, *Retired* takes the value one if an individual receives retirement income in a given year and zero otherwise, *Divorce* takes the value one in a given year if a person who was married one year earlier is registered as unmarried, zero otherwise, and *Mortality* measures if the individual passed away during a given year. The model for mortality uses only post-years with the dynamic estimates estimated by a separate model for each period. The % effect uses the mean for workers in privatized firms over four years before the privatization as the baseline. Short run refers to event times 1 and 2, medium run to event times 3 and 4 and long run to event times 5 to 8. The standard errors are clustered at the municipality level and three stars denote statistical significance at the one percent level, two at the five percent level, and one at the ten percent level. Coefficients, standard errors and preperiod means in columns 2 to 7 are multiplied by 100.

	Labor market outcomes				Non-market outcomes	
	Unemp. days (1)	Employed (2)	Out of LF (3)	Retired (4)	Divorce (5)	Mortality (6)
Panel A: Average effect						
Full Period	2.744*** (0.486)	-0.660 (0.519)	-0.032 (0.306)	-0.090 (0.511)	0.021 (0.031)	-0.043*** (0.015)
% effect	28.5	-0.7	-3.5	-66.2	2.5	-
R ²	0.03	0.04	0.05	0.10	0.00	0.00
Panel B: Dynamic effect						
Short run	1.956*** (0.515)	-0.646** (0.250)	0.199*** (0.068)	-0.105 (0.093)	0.005 (0.045)	-0.038* (0.020)
Medium run	2.895*** (0.498)	-1.350*** (0.371)	0.235 (0.172)	0.447 (0.380)	0.017 (0.047)	0.006 (0.030)
Long run	3.111*** (0.525)	-0.317 (0.703)	-0.296 (0.454)	-0.357 (0.724)	0.032 (0.033)	-0.082*** (0.023)
% effect						
Short run	20.3	-0.7	21.6	-77.2	0.6	-
Medium run	30.1	-1.4	25.5	328.6	2.0	-
Long run	32.3	-0.3	-32.2	-262.5	3.9	-
R ²	0.03	0.04	0.06	0.12	0.00	0.00
Preperiod	9.632	93.900	0.920	0.136	0.826	0
Observations	1,184,598					

Table 6: Effects on entrepreneurship entry

The table reports selected coefficients from a difference-in-differences regression explaining worker-year outcomes after privatization. The sample consists of treated workers employed one year prior to the privatization and matched control workers. We control for age, gender, immigrant status, labor market experience, tenure, education, firm size, municipality, industry and calendar year. All controls, except calendar year, are measured one year prior to the privatization. *Self-employment* takes the value one if an individual runs an unlimited liability sole-proprietorship and zero otherwise. *Business ownership* takes the value one if an individual is employed in a limited liability company (aktiebolag) he or she owns and zero otherwise. The % effect uses the mean for workers in privatized firms over four years before the privatization as the baseline. Short run refers to event times 1 and 2, medium run to event times 3 and 4 and long run to event times 5 to 8. The point estimates, standard errors, and preperiod means are multiplied by 100. The standard errors are clustered at the municipality level and three stars denote statistical significance at the one percent level and two at the five percent level.

	Self-employment (1)	Business ownership (2)
Panel A: Average effect		
Full Period	0.132 (0.087)	0.263*** (0.036)
% effect	44.9	230.7
R^2	0.01	0.01
Panel B: Dynamic effect		
Short run	0.080** (0.038)	0.056** (0.024)
Medium run	0.217*** (0.064)	0.205*** (0.044)
Long run	0.117 (0.131)	0.410*** (0.056)
% effect		
Short run	27.2	49.1
Medium run	73.8	179.8
Long run	39.8	359.6
R^2	0.01	0.01
Preperiod mean	0.294	0.114
Observations	1,184,598	

Table 7: Comparison of treated and control entrepreneurs

The table compares treated and control entrepreneurs over the full eight-year post-period. *Business owner* denotes the fraction of entrepreneurs that are business owners. *Wage* is total annual wage income in thousands of SEK, *Capital income* is net annual capital income in thousands of SEK, and *Total income* is the sum of labor and capital income. Firm size bins are based on EU's classification of micro, small, medium, and large firms. *Productivity* is value added per employee in thousands of SEK and *OROA* is annual operating return on assets. Column 3 reports differences in means. Three stars denotes significant differences at the one percent level, two at the five percent level, and one at the ten percent level.

	Treated (1)	Control (2)	Difference (3)
Panel A: Entrepreneurs			
Business owner (%)	27.48	18.50	8.98***
Post income (TSEK)			
Wage	123.12	88.54	34.58***
Capital income	21.11	19.78	1.32
Total income	144.23	108.32	35.91***
Pre income (TSEK)			
Wage	295.90	247.76	48.14***
Capital income	-4.67	-9.83	5.16
Total income	291.22	237.93	53.30***
Entrepreneur-year observations	8,300	6,442	1,858
Unique entrepreneurs	2,976	2,332	644
Panel B: Firms			
Employment			
Mean	6.71	6.33	0.38
1-10 employees (%)	95.74	96.76	-1.02*
10-50 employees (%)	3.88	2.77	1.11**
50-250 employees (%)	0.28	0.38	-0.11
250+ employees (%)	0.10	0.09	0.01
Productivity	333.58	281.29	52.28***
Firm-year observations	6,874	5,494	1,380
Unique firms	2,770	2,175	595

Table 8: Firm level outcomes

The table reports selected coefficients from difference-in-differences regressions explaining firm outcomes after privatizations. The sample includes firm level information for three years before and four years after the privatization and covers the years 1992 to 2011. We control for calendar year, industry, municipality and firms' age prior to privatization. *Employment* measures number of employees, *Wages* is the mean annual wage income of workers employed in the firm in thousand of SEK, *Productivity* is value added per employee in thousands of SEK and *OROA* is annual operating return on assets. The % effect uses mean for privatized firms over the years before the privatization as the baseline. Short run refers to event time 1 and 2 and medium run to event time 3 and 4. Standard errors are clustered at the firm level. Three stars denotes significance at the one percent level, two at the five percent level, and one at the ten percent level.

	Employment (1)	Wages (2)	Productivity (3)	OROA (4)
Panel A: Average effect				
Full period	8.675 (11.820)	5.061 (6.522)	41.080** (19.450)	0.045*** (0.014)
% effect	5.7	1.8	8.2	-
R^2	0.135	0.372	0.418	0.124
Panel B: Dynamic effects				
Short run	3.458 (9.524)	1.875 (5.897)	38.260* (19.600)	0.041*** (0.015)
Medium run	17.060 (18.890)	9.484 (9.922)	45.140* (23.390)	0.050*** (0.017)
% effect				
Short run	2.3	0.7	7.6	-
Medium run	11.3	3.4	9.0	-
R^2	0.133	0.373	0.418	0.125
Preperiod mean	151.0	281.7	500.4	-0.009
Observations	5,152	5,152	3,826	3,826

Table 9: Previous studies on privatization and workers

The table summarizes the key incumbent worker-level studies and a selection of firm-level studies most relevant to this paper. See Megginson and Netter (2001), Estrin et al. (2009), or Megginson (2017a) for more comprehensive surveys.

Study	Sample and period	Results
Panel A: Worker-level studies		
Brown et al. (2006a)	Retrospective survey of 4,000 households in Ukraine conducted in 2003. Contains data on employment in 1986, 1991, and 1997 to 2003.	Involuntary layoffs are halved, wages are reduced by five percent. High skilled workers do better than low skilled workers. No differences by gender, education or experience.
Melly and Puhani (2013)	Personnel records for 1998-2003 from two Swiss firms: Swisscom privatized in 1998 and Swiss Federal Railways that remained state owned.	Wages decreased for older, high-tenure, low-skilled, part-time workers, permanent residents and women. Average wage follows a J-curve with an immediate drop and then recovery.
Bastos et al. (2014)	Portuguese administrative firm-worker data for 1991-2007.	Wages increase because the wage floor negotiated via collective bargaining increases and because privatized firms pay larger mark-ups on union wage floors. Wage increases are highest for new hires and highly educated workers.
Arnold (2019)	Administrative data on firms and workers in Brazil for 1992-2010.	Wages decrease by 26%. Also finds wage decreases for workers in related private sector firms and that privatizations reduced the aggregate formal sector wage by 3.1%. Low skilled workers experience larger wage declines.
Panel B: Firm-level studies		
La Porta and Lopez-de-Silanes (1999)	Firm level data on 170 privatized Mexican firms during 1983 to 1991.	Increases in operating income to sales of 24 percentage points, out of which 31% are transfers from laid-off workers, 5% from higher product prices, and the rest productivity gains. Both blue-collar and white-collar employment declines.
D'souza and Megginson (1999)	Firm level data on 85 firms from 28 countries that were privatized between 1990 and 1996.	Profitability and operating efficiency increases. Declines in employment that are statistically insignificant.
Brown et al. (2010)	Privatizations of 30,000 manufacturing firms in Hungary, Romania, Russia and Ukraine for the period 1985 to 2006 (but with varying coverage across countries)	No evidence of job losses or substantial wage declines.
Chong et al. (2011)	Survey data on 308 privatizations across 84 countries over the years 1982 to 2002.	Labor restructuring programs carried out by governments before privatization involve hiring out highest ability workers from the firms that are to be privatized. Labor downsizing programs are linked with rehiring of the same workers after the privatization.

Internet Appendix

What Is the Cost of Privatization for Workers?

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February 2021

Table IA1: Detailed variable descriptions

The table displays descriptions of the variables that we use from the Statistics Sweden’s LISA database. The Swedish Secrecy Act protects access to the data from Statistics Sweden, but researchers affiliated with a Swedish research institution can apply for access. A full detailed description of the variables in LISA is available from the Statistics Sweden homepage (scb.se).

Variable	Notes
Panel A: Individual level variables	
Age	Original source is the population registry.
Business owner	Defined in November each year and based on occupational status from the main source of labor income. Business owners refer to owners of Limited Liability Corporations (aktiebolag).
Capital income	Yearly net capital income.
Divorced	A person is defined as divorced if being married one year earlier and not married the year after.
Education	Information on highest completed education level comes from the Education Register at Statistics Sweden (Utbildningsregistret). We define a worker as skilled if holding a university degree, unskilled otherwise.
Employed	Defined in November each year and the variable takes the value one if a person has an employment, zero otherwise.
Female	Original source is the population registry.
Immigrant status	A person is defined as immigrant if LISA holds information on immigrant year, native otherwise.
Individual identifier	Deidentified social security numbers. Original source is the population registry.
Wage	Original source is Swedish Tax Office records and refers to total gross annual wage income. Covers all payments above 100 SEK.
Labor market experience	Based on a person’s age or year of academic degree. If highest educational level is primary education or lower (including missing information), labor market experience is defined as age minus 16. If upper secondary education, it is defined as age minus 19. If post-secondary education less than two years, age minus 20. If post-secondary education two year or loner, age minus 23. If higher educational level, as age minus year of academic degree.
Municipality	The municipality where the person is registered at the time of reference (normally December 31 each year).
Out of labor force	The variable takes the value one if an individual has no employment, no wage income, and no unemployment days in a given year and zero otherwise
Retired	A person is defined as retired if collecting retirement pension payments during a year, not retired otherwise.
Self-employed	Defined in November each year and based on occupational status from the main source of labor income. Self-employment refers to unlimited liability sole-proprietorships.
Seniority Tenure	A worker’s relative tenure to all co-workers within a firm. Constructed using the variable ”tenure”. We calculate the tenure of a worker based on observing worker-firm links between the years 1990 and 2011. A worker can thus have a maximum tenure of 20 years.
Unemployment	Statistics Sweden calculates the yearly number of days in unemployment based on registry data from Arbetsförmedlingen (Swedish Public Employment Service). Registering at Arbetsförmedlingen is mandatory for receiving unemployment benefits. An individual is designated as unemployed by Statistics Sweden if the person does not have a job, is looking for a job but cannot find one, and is not registered as being part of any government labor market policy program. The variable unemployment takes the value one if a person has at least one day of registered unemployment during a given year, zero otherwise. The variable unemployment days is the total number of unemployment days (including zero days) a person has during a given year.

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Panel B: Firm level variables

Firm age	The number of years since the firm identifier emerged in the data for the first time and is therefore censored in 1990.
Firm identifier	Deidentified organisational numbers.
Firm size	Number of employees as of November each year, aggregated from individual level data in LISA.
Industry and location	Information on the industry and geographical location of the firm comes from Statistics Sweden who assigns identifiers, industry, and location codes to physical places of work (the underlying databases at Statistics Sweden are the RAMS and the Företagsdatabasen databases). The industry classification of firms and establishments changed in 2002 but changes were minor at the 10-category classification level we use.
Institutional sector	Information on firms' owner category, sector of society and legal form.
Operating return on assets	Profit before taxes divided by total assets.
Productivity	Value added divided by firm size. WinzORIZED at the 95th/5th percentile.
Wages	Average annual wage income for workers using individual level data in LISA.

Table IA2: Propensity score matching

The table reports selected coefficients from equation 1 and equation 2 using a propensity score matched control group. To identify control workers we start with all SOEs operating in industries where at least one of the firms is privatized one year later. Then for each year during 1996-2010, we run a probit model to estimate a propensity score where the dependent variable takes the value one if the firm is privatized one year later, zero otherwise. The model controls for industry, firm size, firm age, share females among workers, mean wage income among workers and mean tenure among workers. We use the estimated propensity score to identify control firms within industries and year using one to one nearest neighbor matching. The matching is done without replacement. After identifying one control firm for each treated firm, we link all workers employed in the firms at the year of the matching. If a worker is employed in more than one treated or control firm over time, we keep the first observation. Finally, we track the workers three years before the matching year and up to eight years after. Using the treated and control sample we run the differences-in-differences models represented by equation 1 and equation 2. The models control for age, gender, immigrant status, labor market experience, tenure, education, firm size, municipality, industry and calendar year. All controls, except calendar year, are measured one year prior to the privatization. *Wage* is the log annual wage income (using inverse hyperbolic sine transformation) and *Unemployment* takes the value one if an individual was unemployed at any time during the year, zero otherwise. *Self-employment* takes the value one if an individual runs an unlimited liability sole-proprietorship and zero otherwise. *Business ownership* takes the value one if an individual owns a limited liability company (aktiebolag) and zero otherwise. The % effect uses the mean for workers in privatized firms over four years before the privatization as the baseline. Short run refers to event times 1 and 2, medium run to event times 3 and 4 and long run to event times 5 to 8. The standard errors are clustered at the municipality level and three stars denote statistical significance at the one percent level, two at the five percent level, and one at the ten percent level. In Columns 3 and 4 the coefficients, standard errors and preperiod means are multiplied by 100.

	Wage (1)	Unemployment (2)	Self-employment (3)	Business ownership (4)
Panel A: Average effect				
Full period	-0.003 (0.058)	0.020*** (0.0059)	0.168* (0.093)	0.113** (0.048)
% effect	-0.3	21.7	57.9	101.8
R ²	0.08	0.06	0.01	0.01
Panel B: Dynamic effects				
Short run	-0.045** (0.021)	0.014** (0.006)	0.055 (0.050)	0.013 (0.029)
Medium run	-0.049 (0.037)	0.019*** (0.006)	0.199*** (0.077)	0.112** (0.0559)
Long run	0.031 (0.073)	0.024*** (0.006)	0.241* (0.127)	0.183*** (0.068)
% effect				
Short run	-4.5	15.2	19.0	11.7
Medium run	-4.9	20.7	68.6	100.9
Long run	3.1	26.1	83.1	164.9
R ²	0.08	0.06	0.01	0.01
Preperiod mean	8.141	0.092	0.290	0.111
Observations	1,044,541	1,044,541	1,044,541	1,044,541

Table IA3: Heterogeneity analysis sub-sample results

The table reports selected coefficients from difference-in-differences regressions explaining worker-year outcomes around privatization. The sample consists of treated workers employed one year prior to the privatization and matched control workers. We control for age, gender, immigrant status, labor market experience, tenure, education, firm size, municipality, industry and calendar year. All splits and controls are defined one year prior to the privatization. *Wage* is the log of annual wage income (using inverse hyperbolic sine transformation) and *Unemployment* takes the value one if an individual was unemployed at any time during the year and zero otherwise. *Skill* refers the highest attained educational level. Low skill workers do not have a university degree whereas high skilled workers do. *Age* refers to workers' age. Old workers are older than the sample median age of 43. *Tenure* refers to the number of consecutive years a worker has been employed in the same firm. Low tenured workers have less than two years of tenure. *Seniority* refers to workers' relative tenure within a firm. Low seniority refers to the last hired workers. The % effect relates the average full period effect to the preperiod mean for treated workers. Standard errors are clustered at the municipality level. Three stars denotes significance at the one percent level, two at the five percent level, and one at the ten percent level.

	Skill		Age		Tenure		Seniority	
	Low	High	Old	Young	Low	High	Low	High
Panel A: Wage								
Full period	-0.082* (0.084)	0.083*** (0.027)	-0.147** (0.060)	-0.039** (0.015)	-0.146*** (0.050)	-0.077* (0.043)	-0.142** (0.065)	-0.049 (0.060)
R^2	0.05	0.05	0.13	0.10	0.05	0.09	0.06	0.06
% effect	-8.2	8.3	-14.7	-3.9	-14.6	-7.7	-14.2	-4.9
Pre-period mean	5.989	6.343	6.320	5.787	5.851	6.296	7.623	8.297
Panel B: Unemployment								
Full period	0.019*** (0.004)	0.007* (0.004)	0.031*** (0.004)	0.009*** (0.003)	0.033*** (0.006)	0.020*** (0.003)	0.028*** (0.007)	0.017*** (0.003)
R^2	0.06	0.04	0.03	0.08	0.07	0.03	0.08	0.04
% effect	29.3	10.8	79.4	14.2	22.6	95.2	14.8	0.28
Preperiod mean	0.097	0.065	0.039	0.142	0.146	0.021	0.189	0.060
Observations	1,025,427	162,063	579,323	605,275	634,663	549,935	292,833	891,765

Table IA4: Comparison of treated and control firms

The table compares treated and control firms one year before the privatization. *Employment* measures number of employees, *Wages* is the mean annual labor income of workers employed in the firm in thousand of SEK, *Productivity* is value added per employee in thousands of SEK and *OROA* is annual operating return on assets. A absolute normalized t-test above 0.25 indicates substantial differences in means (Imbens and Wooldridge, 2009).

	Treated (1)	Control (2)	Difference (3)	Norm. T-value (4)
Employees	154.45	101.28	53.18	0.10
Mean wages	297.03	257.91	39.12	0.23
Productivity	503.47	665.59	-162.12	0.37
OROA	-0.03	0.02	-0.04	0.14
Observations	369	369		

Table IA5: Firm outcomes with propensity score matching

This table reports selected coefficients from difference-in-differences regression explaining firm outcomes after privatizations. The model controls for calendar year fixed effects, industry fixed effects, municipality fixed effects and firms' age. The sample includes firm level information for three years before and four years after the privatization and covers the years 1992 to 2011. The control firms are the same as in Table IA2. After identifying one control firm for each treated firm, we track them three years before the matching and up to four years after. *Employment* measures number of employees, *Wages* is the mean annual wage income of workers employed in the firm in thousand of SEK, *Productivity* is value added per employee in thousands of SEK and *OROA* is annual operating return on assets. The % effect uses mean for privatized firms over the years before the privatization as the baseline. Short run refers to event times 1 and 2 and medium run to event times 3 and 4. Standard errors are clustered at the firm level. Two stars denotes significance at the five percent level and one at the ten percent level.

	Employment (1)	Wages (2)	Productivity (3)	OROA (4)
Panel A: Average effect				
Full period	12.910 (15.990)	3.331 (6.585)	30.600 (21.310)	0.027* (0.015)
% effect	8.5	3.5	6.1	-
R^2	0.16	0.36	0.39	0.16
Panel B: Dynamic effects				
Short run	6.101 (13.210)	-0.638 (6.378)	41.000* (21.100)	0.031** (0.016)
Medium run	22.630 (24.050)	8.746 (9.927)	18.110 (25.770)	0.022 (0.019)
% effect				
Short run	4.0	-0.2	8.2	-
Medium run	14.9	3.1	3.6	-
R^2	0.16	0.36	0.39	0.16
Preperiod mean	152.3	282.5	504.7	-0.009
Observations	4,767	4,767	3,402	3,402

Table IA6: Firm level outcomes adjusting for attrition

The table reproduces Table 8 adjusting for firm attrition. Panel A displays results with no adjustment. Panel B displays results using dynamic firm identifiers based on worker flows. A firm that changes its identifier between two consecutive years is defined by the dynamic firm identifier as the same if more than 50% of the employees at time $t - 1$ is employed at time t and these employees make up at least 50% of the total number of employees at time t . Panel C assigns the post period mean value for firms that drop out of the data in the post period. Panel D assigns the last available value for firms that drop out of the data set in the post period. Standard errors are clustered at the firm level. Three stars denotes significance at the one percent level, two at the five percent level, and one at the ten percent level.

	Employment (1)	Wages (2)	Productivity (3)	OROA (4)
Panel A: Baseline				
Full period	8.675 (11.820)	5.061 (6.522)	41.080** (19.450)	0.045*** (0.014)
Observations	5,152	5,152	3,826	3,826
Panel B: With dynamic identifiers				
Full period	3.228 (10.740)	81.620 (71.320)	36.280* (19.230)	0.041*** (0.014)
Observations	5,198	5,198	3,866	3,865
Panel C: Imputing using post-period means				
Full period	-2.750 (9.333)	27.700 (85.280)	41.700** (20.570)	0.039*** (0.014)
Observations	5,476	5,476	4,185	4,185
Panel D: Imputing using last observed values				
Full period	-4.174 (9.936)	32.690 (92.890)	39.630* (20.990)	0.032** (0.016)
Observations	5,476	5,476	4,106	4,106

Table IA7: Firm level outcomes for restricted firm sample

The table reports selected coefficients from difference-in-differences regression explaining firm outcomes after privatizations. The sample includes firm level information for three years before and four years after the privatization and covers the years 1992 to 2011. The sample is restricted to firms with information on all four outcomes. The models control for year fixed effects, industry fixed effects, municipality fixed effects and firms' age. *Employment* measures number of employees, *Wages* is the mean annual wage income of workers employed in the firm in thousand of SEK, *Productivity* is value added per employee in thousands of SEK and *OROA* is annual operating return on assets. The % effect uses mean for privatized firms over the years before the privatization as the baseline. Short run refers to event times 1 and 2 and medium run to event times 3 and 4. Standard errors are clustered at the firm level. Three stars denotes significance at the one percent level, two at the five percent level, and one at the ten percent level.

	Employment (1)	Wages (2)	Productivity (3)	OROA (4)
Panel A: Average effect				
Full period	1.425 (29.16)	10.370 (8.160)	41.080** (19.450)	0.045*** (0.014)
% effect	1.0	3.5	8.2	-
R^2	0.152	0.359	0.418	0.124
Panel B: Dynamic effects				
Short run	-7.085 (26.42)	4.643 (7.903)	38.260* (19.600)	0.041*** (0.015)
Medium run	13.28 (35.09)	18.190 (11.520)	45.140* (23.39)	0.050*** (0.017)
% effect				
Short run	-4.9	1.6	7.6	-
Medium run	9.3	6.1	9.0	-
R^2	0.152	0.360	0.418	0.125
Preperiod mean	143.4	296.8	500.4	-0.009
Observations	3,826	3,826	3,826	3,826

Table IA8: Matching three years prior to the privatization

The table reports selected coefficients from a difference-in-differences regression explaining worker-year outcomes after privatizations. The sample consists of treated workers employed three years prior to the privatization and cell matched control workers. We control for age, gender, immigrant status, labor market experience, tenure, education, firm size, municipality, industry and calendar year. *Wage* is the log of annual wage income (using inverse hyperbolic sine transformation) and *Unemployment* takes the value one if an individual was unemployed at any time during the year and zero otherwise. *Self-employment* takes the value one if an individual runs an unlimited liability sole-proprietorship and zero otherwise. *Business ownership* takes the value one if an individual is employed in a limited liability company (aktiebolag) he or she owns and zero otherwise. The % effect uses the mean for workers in privatized firms over four years before the privatization as the baseline. Short run refers to event times 1 and 2, medium run to event times 3 and 4 and long run to event times 5 to 8. The standard errors are clustered at the municipality level and three stars denotes significance at the one percent level, two at the five percent level, and one at the ten percent level. In Columns 3 and 4, coefficients, standard errors and preperiod means are multiplied by 100.

	Wage (1)	Unemployment (2)	Self-employment (3)	Business owner (4)
Panel A: Average effect				
Full period	-0.003 (0.034)	0.019*** (0.003)	0.246** (0.097)	0.175*** (0.046)
% effect	-0.3 0.06	20.4 0.06	135.9 0.01	221.5 0.01 0
Panel B: Dynamic effects				
Short run	0.003 (0.016)	0.012*** (0.004)	0.060 (0.037)	0.053** (0.025)
Medium run	0.003 (0.027)	0.026*** (0.004)	0.223*** (0.081)	0.151*** (0.051)
Long run	-0.014 (0.046)	0.019*** (0.003)	0.370** (0.145)	0.259*** (0.063)
% effect				
Short run	0.3	12.9	33.1	67.1
Medium run	0.3	28.0	123.2	191.1
Long run	-1.4	20.4	204.4	327.8
R ²	0.07	0.06	0.01	0.01
Preperiod mean	8.081	0.093	0.181	0.079
Observations	792,984	792,984	792,984	792,984