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## **What is the Cost of Privatization for Workers?**

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## ABSTRACT

The privatization of state-owned enterprises is on the agenda across the globe. Using Swedish data covering two decades, we show that productivity gains and headcount reductions are coupled with economic costs for incumbent workers. Workers experience income losses and higher unemployment, but half of the losses are covered by the social safety net. We also find small positive effects on entrepreneurship and cash holdings but no meaningful effects on other labor market, family, health, or household finance outcomes. Productivity improves when the CEO is replaced, and the gains outweigh workers' income declines by a factor of between two and six.

*Keywords:* Employment, entrepreneurship, family, health, household finance, 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 in many advanced economies across the globe.<sup>1</sup> Creating effective, evidence-based policies for privatization requires solid facts on the related costs and benefits for all stakeholders. Currently, the benefits of privatization in terms of improved firm governance, productivity, and performance are well documented (Megginson and Netter, 2001; Estrin, Hanousek, Kočenda and Svejnar, 2009; Megginson, 2017a). The impact on workers in SOEs that are privatized is less understood, although this is a major concern for politicians when deciding whether or not to sell (we discuss related studies on workers below). As 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." The effects on workers are *ex-ante* ambiguous. One hypothesis is that better governance and higher productivity increase demand for labor, which, in turn, benefits workers. Another hypothesis is that privatization triggers a workforce reorganization that leaves some or all workers worse off (Boycko, Shleifer and Vishny, 1996; Dessaint, Golubov and Volpin, 2017).<sup>2</sup>

In this paper, we ask: what are the costs of privatization for workers? Using Sweden as a laboratory, we show that firm-level improvements in productivity are coupled with economic, but not social, costs for workers, that the social safety net in Sweden compensates for half of these costs, and that productivity gains outweigh the pre-transfer income declines for workers by a factor between two and six. A first policy implication is thus that a well-functioning social safety net is important to protect workers against income losses. A second implication is that governments should consider implementing research-based policies to help combat unemployment, such as training programs and private-sector employment programs.

Detailed administrative data permits us to employ a stacked difference-in-differences design to identify the parameters of interest and avoid biases that arise in two-way fixed effects models (Baker, Larcker and Wang, 2022; Roth, Sant'Anna, Bilinski and Poe, 2023). The stacked design compares workers and firms that are privatized to a benchmark of workers and firms that remain state-owned. At the *worker-level*, we find that an initial reallocation of human capital takes place, consistent with the effects of new owners reorganizing the workforce. This reallocation has substantial costs for workers. Workers' wage income

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<sup>1</sup>The total value of privatization of SOEs hit USD 320 billion in 2015 compared to only USD 24 billion in 1990. Both developing and advanced countries are privatizing assets. The ten countries with the highest privatization revenues are China, the United Kingdom, Italy, Japan, India, Sweden, Australia, the United States, the Netherlands, and Ireland (Megginson, 2017b).

<sup>2</sup>There are several reasons why 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 to raise productivity by reallocating human capital. The ownership change itself can also lead to value capture by shareholders through the breach of implicit contracts with workers (Shleifer and Summers, 1988).

declines substantially in the two years following privatization (-5.8%), and even more during the years that follow (-9.3% in years three to four and by -8.4% in years five to eight). Part of these wage income declines come from the fact that privatization increases unemployment (12.6%). The effect on unemployment is immediate, then slightly monotonically increasing over time. Part of these costs to workers, however, are mitigated through higher government transfers in the form of unemployment benefits, activity support, and development allowances. These payouts compensate for roughly half of the wage income losses and, as a result, the effect of privatization on income after transfers is transitory.

We also analyze a multitude of other economic and social worker-level outcomes. First, regarding labor market outcomes, we find a doubling of the business ownership rate by former SOE workers, but the absolute number of firms that are formed after privatization is small, and the firms themselves have low productivity and do not hire many other workers. We do not find that privatization tends to increase transitions to retirement, and the effects of leaving the labor force are small and transitory.

Second, a novel feature of our paper is that we study non-labor market outcomes related to family, health, and household finance. Existing research has found that unemployment is coupled with increases in the probability of divorce and with higher mortality and that labor income risk affects the financial decisions individuals take (Gerdtham and Johannesson, 2003; Eliason, 2012; Angerer and Lam, 2009; Fagereng, Guiso and Pistaferri, 2018). We find the divorce rate does increase after privatization in a statistical sense, but the economic magnitude is small (0.63 extra divorces per thousand workers per year). We find no effect on health as measured by mortality. On the whole, workers' financial behavior seems unaffected after privatization regarding stock market participation, the risk level of their portfolios, and their personal leverage. However, their cash holdings increase by a tenth, consistent with workers taking into account the higher labor income risk from unemployment. Thus, overall, spillovers on non-labor market outcomes from privatization exist but they are close to negligible economically.

At the *firm-level*, we document a decrease in firms' employee count that stems from a surge in the job destruction rate while job creation remains unaltered. The decline in the number of employees is accompanied by a decline in firms' total payroll. These effects materialize in the short term immediately after privatization and persist into the medium term. We also find an increase in profitability (by 2.1 percentage points) and a large increase in productivity (35.7%). Quantile regressions show that the increase in productivity is driven by the top 75th and 90th percentiles, and regressions using logged productivity to account for the non-linearity of the productivity distribution yield a more modest productivity gain (11.5%). The observed reductions in firm-level employment and payroll combined with improvements in performance and productivity align with the interpretation that privatization is followed by a cost-saving strategy of reducing the firm's workforce.

While cutting labor costs appears to be a consistent explanation for increased productivity, we also study the importance of upgrading labor quality, financial engineering, and governance engineering. First, one

potential source of productivity improvements following privatization is that the new owners replace less productive workers with more productive ones. Here, we investigate whether hired or separated workers differ in terms of human capital after privatization. To this end, we rely on measures of cognitive and non-cognitive skills from the Swedish military draft test. While we do not find any statistically significant effects on the quality of hires and separations overall, we do document a small increase in the quality of hires in the medium term (4.8%). Second, we delve into the ways privatized firms modify their financial strategies. Significant shifts in capital investments could suggest that firms enhance their capital quality, which might be a reason for improved productivity after privatization. Capital investments might also potentially account for workforce reductions if capital replaces labor in a company's production function. Moreover, changes in leverage post-privatization could occur if the firm's risk profile alters under new ownership or if it aims to boost leverage to gain increased bargaining power over employees. We do not, however, find any economically meaningful effects on the investment or leverage ratio. Finally, we examine whether productivity gains following privatization can be linked to governance changes in privatized firms. Prior research suggests that improving governance is key for better performance (Djankov and Murrell, 2002; Gupta, 2005), and indeed, we find that productivity increases when the CEO is replaced but remains unchanged when the CEO stays on.

Thus, overall our firm-level results suggest that operational and governance engineering following privatizations are key factors contributing to increased productivity and to the costs of privatization for workers that arise due to the reallocation of human capital. A rough calculation using the firm and worker level estimates suggests that, depending on the preferred point estimate, the productivity gains of privatization outweigh the costs to workers before government transfers by a factor between two and six. Government transfers to workers are about ten to 30% of the productivity gains per worker for privatized firms. This implies that workers could be compensated to the full extent of their losses, yet there would still be a positive surplus left to share between the new owners of the firm and the government through the transaction price.

Our paper contributes to the privatization literature.<sup>3</sup> 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 (Megginson and Netter, 2001; Estrin et al., 2009; Earle, 2014). Most studies focus on firm-level employment (La Porta and Lopez-de Silanes, 1999; D'souza and Megginson, 1999; Brown, Earle and Vakhitov, 2006a; Dinc and Gupta, 2011; Chong, Guillen and López-de Silanes, 2011). Notable exceptions are Brown, Earle and Telegdy (2006b), Melly and Puhani (2013), Bastos,

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<sup>3</sup>Our paper also contributes to the broader labor and finance literature (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 et al., 2017; Mueller, Ouimet and Simintzi, 2017; Subramanian and Megginson, 2018). This literature has used matched employer-employee data to study the worker effects of other ownership changes such as M&As (Lagaras, 2023), corporate diversification (Tate and Yang, 2015), and private equity buyouts (Agrawal and Tambe, 2016; Olsson and Tåg, 2017; Antoni, Maug and Oberberger, 2019).

Monteiro and Straume (2014), and Arnold (2022).

We provide three novel contributions to this literature. First, we expand the literature beyond relatively short-run effects on workers' wages by studying the potential long-run scarring effects of privatizations for several labor and *non-labor worker outcomes*. Earlier work on the effects of privatization at the worker level has focused almost exclusively on the effect on wages. The literature is silent on other labor market outcomes, such as retirement or entering entrepreneurship, or on non-labor worker outcomes related to family, health, and household financial outcomes. Our data allows us to show that worker costs in Sweden are primarily paid in wage income reductions and higher unemployment in the long run and that spillovers on non-labor worker outcomes are small. Knowing to what extent privatizations can harm workers in other dimensions than wage losses is an important input for policy makers. Moreover, the fact that we document that long-run unemployment emerges after privatization suggests that governments can mitigate the effects of privatization on workers by implementing research-based policies on how to combat unemployment (such as training programs and private-sector employment programs).

Second, we provide empirical evidence on how the *social safety net cushions* workers from the effects of privatization. This aspect of privatization has not been studied before. The economic insight we provide is that policy makers in countries with extensive social safety nets can partially rely on existing structures to mitigate the costs of privatization for workers and that privatizations can have *fiscal externalities* on the government budget through increasing unemployment payouts.

Finally, the detailed firm-level data allows us to investigate operational, financial, and governance engineering efforts that take place after privatization. In particular, we are the first to investigate whether *labor quality upgrades* take place after privatization, and to obtain an estimate of *productivity gains relative to worker losses and government transfers to workers*. Thus our paper suggests that from an economic point of view, privatizations have paid off for Sweden in terms of higher privatization revenues, despite worker income losses.

Our findings have implications for active privatization policies in many parts of the world because we provide detailed firm and worker-level evidence from an institutional environment that is similar to those of several countries that are currently privatizing or considering privatizing assets in an *ad hoc* way rather than through large privatization programs. (OECD (2003) label privatization as *ad hoc* if it is carried out on a case-by-case basis.) Figure 1 shows that there are several developed economies that have extensive social safety nets and many workers in SOEs. Countries with both a high proportion of the labor force in SOEs and a similar level of social expenditure as Sweden include the other Scandinavian countries and large European countries such as France, Germany, Portugal and Italy. These are also countries that have very similar functioning labor markets to Sweden.<sup>4</sup>

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<sup>4</sup>Lazear and Shaw (2009) report strong similarities between labor markets in Scandinavian countries and Belgium, France, Germany, Italy, the Netherlands, and the United States.

A key feature of the costs to workers from privatization that we document here is that they partly manifest through increased unemployment. A first policy implication is thus that existing social safety nets set up to protect workers from unemployment help buffer workers against income losses. A second policy implication is that governments should consider implementing research-based policies to help combat unemployment as a way to mitigate the costs to workers from privatization. For instance, Card, Kluve and Weber (2018) summarize over 200 studies of active labor market programs and find that the long-term unemployed benefit from training programs and private sector employment programs, but they get relatively less help from job search programs.

The rest of the paper is structured as follows. The next section delves into the institutional background, while Section 3 outlines our data sources, defines privatization, and specifies our sample. In Section 4, we detail the empirical design, assess selection into privatization, compare treated and control groups, and discuss external validity. Worker-level findings appear in Section 5, and Section 6 explores firm-level outcomes and underlying mechanisms. Section 7 addresses anticipation effects, alternative definitions of privatization, policy implications, and additional heterogeneity analyses related to industry and macroeconomic conditions, and to who gets laid off from the privatized firm. We conclude in Section 8.

## **2 Institutional background**

### **2.1 Privatization in Sweden**

The main reasons for state ownership of firms in Sweden and other countries have been to control natural monopolies (telecommunication, railroads, and utilities) and natural resources (mining and forestry). The government has also intervened in industries in crisis to save jobs and prevent macroeconomic deterioration (ship-making, steel, banking) (Munkhammar, 2007). During our sample period, privatizations followed the European trend and aimed to increase operating efficiency and raise funding for SOEs. In the early 1990s, there was a widespread assumption among Swedish policymakers and practitioners that state ownership hindered the growth and development of SOEs and that many no longer needed state involvement. The government was free to choose when to privatize to maximize revenues and imposed few restrictions on potential buyers. It instructed that only companies in competitive markets should be privatized and that monopolistic markets should become competitive so that SOEs in these markets could be privatized later (Regeringens proposition 1991/92:69). Like in other countries, a key concern about privatization in Sweden was jobs, especially the fear of job losses if foreign corporations or financial investors bought SOEs (Dansbo and Wallner, 2007).

Sweden did not have formal rules for privatization and adopted a rather *ad hoc* approach, deciding what firms and when to privatize on a case-by-case basis. This *ad hoc* approach is more common in countries

with fewer privatization candidates and where privatization targets are small compared to the entire corporate sector. In contrast, explicit privatization programs are more common when the scale and scope of privatization is large such as in former transition countries. A key feature of privatizations in Sweden is that partial or sequential sales have been relatively rare, with most privatizations being trade sales of 100% stakes (OECD, 2003, 2018).

## **2.2 Employment legislation around privatization**

Sweden has no specific rules regarding employment conditions after privatization. This is due to the government's ownership policy, which requires SOEs to operate under similar conditions as private firms and prioritize long-term firm value, while also, in some cases, meeting social goals (Heyman, Norbäck and Persson, 2012; OECD, 2018). In addition, Swedish employment law mandates that after a change in ownership, the rights and obligations of workers transfer from the old owner to the new owner, unless the worker chooses to renegotiate the contract. Moreover, the change in ownership itself is not a valid reason for terminating the employment contract unless there are significant reorganization needs related to economic, technical, or organizational reasons (LAS 1994:1685, paragraphs 6b and 7). The law does not stipulate that workers are entitled to severance pay if they lose their jobs. Instead, such agreements are individually negotiated between the worker and the employer, and if an agreement is in place, the compensation is paid out as regular labor income (and thus any buyouts of workers will be captured in our measure of wages). If state-owned entities are incorporated in the years leading up to privatization, the government negotiates with unions concerning employment contracts with public employees and reserves the right to terminate the employment of workers who refuse to become employed by the newly incorporated entity (OECD, 2009).

## **2.3 The social safety net**

The social safety net for workers who lose their jobs in Sweden consists of three parts, which are available to all workers equally across all regions in Sweden (Sianesi, 2004). Firstly, Sweden has a relatively generous unemployment insurance system with a replacement rate of around 80% of lost wage income up to a ceiling that is provided for up to 60 weeks. To be eligible, an unemployed person needs to be registered at the unemployment office, actively searching for work, and must accept a job offer if offered, or risk losing the benefits. Secondly, active labor market programs are an essential feature of the Swedish social safety net, and they provide additional compensation to workers for participating in activities that ease the transition to a new job, such as returning to school, entering subsidized jobs, internship programs, relocating, or entering entrepreneurship. The purpose of these programs is to prevent long-term unemployment and to keep people in the labor force. Thirdly, families that have fallen on particularly hard times can apply for means-tested social allowances to cover the costs of basic expenses such as food, housing, and transportation (Sianesi,

2004).

## **3 Data**

### **3.1 Sources**

We use administrative data obtained from multiple databases maintained by Statistics Sweden, who are responsible for Sweden's official statistics. At the individual level, we use the LISA database, which integrates government registers such as tax records, population registers, and surveys. The database contains yearly information on all individuals aged 15 and older registered in Sweden. Our data span the period from 1990 to 2017, and we extract individual-level information on variables such as age, gender, civil status, family size, education, university student status, immigrant status, residence municipality, employer, wage income, capital income, unemployment benefit payouts, active labor market program payouts, social welfare payouts, retirement income, unemployment days, entrepreneurship status, and date of death. To supplement our analysis, we also use data from Statistics Sweden's Wealth Register, covering the period from 1999 to 2007, on total wealth, risky assets, cash, and debt.

At the firm level, we combine data on ownership status from LISA with data on firm performance from the Structural Business Statistics (FEK) database, which covers the entire population of operating firms with employees in Sweden. The FEK database provides information on the industry, ownership status, productivity (measured as value added per employee), and operating return on assets (OROA). This data covers the period from 1997 to 2017. We link the firms and workers in November of each year, allowing us to calculate firm tenure and labor market experience by observing a person's employment history from 1990 to 2017. Table IA1 provides a detailed overview of the variables we use in our analysis.

### **3.2 Defining privatization**

To identify privatizations in the registry data, we use records from the LISA database, categorizing a firm as state-owned if the state, regional government, or municipality holds controlling stakes in an incorporated firm. As with much of the earlier literature, we focus on the privatization of SOEs rather than on selling government-controlled establishments to private firms (as in Orelund (2010)) because we do not have data on establishment-level performance outcomes. We define privatization as a change in ownership classification from state to private between two consecutive years between 1997 and 2017 and focus on firms with at least five employees. This ensures that we have access to both financial accounts and individual information from LISA.<sup>5</sup> In Section 7.2, we present results that demonstrate the robustness of our main findings to alternative

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<sup>5</sup>We exclude a small fraction of firms that alternate between private and state ownership during this period. These fluctuations may occur because the share of state ownership in these firms varied around 50%, but we cannot verify this statement because data

ways of defining privatization, such as partial privatization, share issue privatization, or privatization through sales to specific private actors.

### 3.3 The sample

Figure 2 illustrates the distribution of privatizations in Sweden over time and by industry. Our sample includes firms privatized between 1997 and 2017, with most occurring during the first part of the period. Privatizations are most prevalent in the business activities and financial intermediaries industries, although these firms are relatively small on average. The transport and telecommunications sectors have the largest number of workers affected by privatization.

This industry distribution aligns with the historical motivations for state ownership in Sweden, including control over natural monopolies and natural resources and rescuing failing firms and industries in crisis to save jobs and prevent high regional unemployment (Munkhammar, 2007). Additionally, the industry distribution is broadly consistent with the overall distribution of privatization by sector in other OECD countries, which has been reported to be approximately 40% telecommunications, 11% manufacturing, 14% financial, 12% other, 14% public utilities, and 10% transportation by the value of sales (OECD, 2003).

## 4 Empirical design

### 4.1 Stacked difference-in-differences

To estimate the effects of privatization on workers and to avoid biases that can appear in two-way fixed effects models, we adopt a stacked difference-in-differences model combined with matching (Baker et al., 2022; Roth et al., 2023).<sup>6</sup> This is a common approach in the literature on privatizations and other ownership changes (Brown et al., 2006a; Bastos et al., 2014; Agrawal and Tambe, 2016; Olsson and Tåg, 2017; Antoni et al., 2019; Arnold, 2022). The stacked difference-in-differences estimation method works as follows. For each year of privatizations, denoted  $k + 1$ , we match treated workers with similar workers in SOEs that are not privatized at  $k + 0$  (the matching procedure is discussed in length in Section 4.3). Next, we create event-specific datasets with information on treated and matched control workers in a window from  $k - 3$  up to  $k + 8$  irrespective of workers' labor market status. With privatizations in all years but one during the sample

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on exact ownership shares are not available in our anonymous data. We also exclude firms owned by the Church of Sweden, which were registered as state-owned before 2000 but as private afterward.

<sup>6</sup>A two-way fixed effects model is not appropriate in a setting with heterogeneous treatment effects. As Goodman-Bacon (2021) shows, the estimate in this model is a weighted average of all possible 2x2 DiD-estimates created by the staggered timing of treatment. When the treatment effect varies over time, comparing later treated with early treated can even produce a wrong signed estimate of the treatment effect. The reason is that the weights of each of the 2x2 DiD-estimates depend on the sample size of each subsample and the treatment variance, so treated towards the middle of the sample period will have more weight than units treated earlier or later.

period, we end up with 20 event-specific datasets that we stack and align using normalized time relative to the privatization year  $k + 1$ . We can then apply a standard difference-in-difference model running in event time:

$$Y_{i,f,k,t} = \alpha + \pi \text{After}_k + \gamma D_i + \beta \text{After}_k \times D_i + \omega_t + X_i + X_f + \varepsilon_{i,f,k,t}, \quad (1)$$

for the outcome  $Y$  of worker  $i$  employed in SOE  $f$  at event year  $k - 0$  at calendar year  $t$ .  $\text{After}_k$  takes the value one in the year of the privatization and all years after.  $D_i$  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). Consequently, the interaction term  $\text{After}_k \times D_i$  takes a value of one for treated workers in the year of the privatization and all years after and zero otherwise. So the coefficient  $\beta$  captures the average intention-to-treat effect.<sup>7</sup>  $\omega_t$  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  $k - 0$ , i.e., one year before privatization.

To estimate the dynamic effects around privatizations, we replace  $\text{After}_k$  with event time-specific dummies  $\tau_k$  ranging from  $k - 3$  to  $k + 8$  setting  $k - 0$  as the baseline year:

$$Y_{i,f,k,t} = \alpha + \tau_k + \gamma D_i + \sum_{k=-3}^{k=8} \beta_k \tau_k \times D_i + \omega_t + X_i + X_f + \varepsilon_{i,f,k,t} \quad (2)$$

In this model,  $\beta_k$  captures the average intention-to-treat effect during event time  $k$ . In our figures, we plot  $\beta_k$  to assess the dynamics before and after privatization, and in our tables, we distinguish between short-, medium- and long-run effects by grouping event times  $k + 1$  to  $k + 2$ , and  $k + 3$  to  $k + 4$  and  $k + 5$  to  $k + 8$  using the pre-years as the baseline period. In all regressions, standard errors are clustered at the local labor market level (municipality of residence) to account for common local labor market shocks.

At the firm level, we model the outcome  $Y$  of firm  $f$  at event time  $k$  at calendar year  $t$  as

$$Y_{f,k,t} = \alpha + \pi \text{After}_k + \gamma D_f + \beta \text{After}_k \times D_f + \omega_t + X_f + \varepsilon_{f,k,t}, \quad (3)$$

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

An important feature of the stacked difference-in-differences model—aside from the fact that it deals with the heterogeneous treatment effect problem in two-way fixed effects estimation—is that it deals with

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<sup>7</sup>The reason is that treated and control workers in our sample are only required to work in an SOE at  $k + 0$  but in no other year. As a consequence,  $\beta$  can be thought of as the career effect following privatization, taking into account direct and indirect effects.

the mean reversion pattern created by conditioning workers in our sample to be employed at event time  $k - 0$ , but not at any other point in time. For wages, such conditioning creates an upward trend before treatment and a downward trend after.<sup>8</sup> A staggered two-way fixed effects model will not account for a mean reversion pattern, so comparing treated with early treated or even never treated produces a biased estimate of the treatment effect. Note also that many other adjustment methods to account for the biases in staggered two-way fixed effects models, such as Callaway and Sant’Anna (2021) or Sun and Abraham (2021), do *not* account for this mean reversion pattern while the stacked difference-in-differences model does.

## 4.2 Selection into privatization

A key feature in creating the matched control group is a good understanding of the selection for privatization. First, as discussed in Section 2, Sweden, during our sample period, had a strategy of *ad hoc* privatization. Because the government could choose the timing to optimize revenues, we observe selection across years in privatization intensity. The focus on privatizing firms already operating in competitive markets, and the fact that state ownership was motivated only in certain industries (natural monopolies, natural resources, and industries in crisis), also means there is selection across industries. The selection pattern is visible in Figure 2. Key observables to consider when creating the control groups are thus industry and year of privatization. Industry and year are also key observables that are correlated with workers’ future career outcomes.

Second, it is important to note that our main analysis is focused on worker-level outcomes. Conditional on year, industry, and firm-level observables, it is thus important to account for selection on individual characteristics. This is a concern if unobservables driving the choice to privatize are correlated with workforce characteristics. One concern may be that the productivity of individual workers is related to firm-level productivity and thus, to trends in workers’ career outcomes. Thus, it makes sense to account for individual characteristics related to productivity (for instance, age, labor market experience, education, and geographical location).

Third, because the timing of privatization was *ad hoc*, there could be selection on firm performance into privatization. It is not clear, however, which way this selection goes. On the one hand, since one goal of privatization was increasing efficiency, it is plausible that the worst-performing firms were chosen for privatization. On the other, a second goal of privatization was raising revenues by selling firms already operating in competitive markets. The government may thus want to time the privatization such that maximal revenues can be raised when firm performance is good. Table IA2 in the Internet Appendix runs a selection regression in the sample of SOEs in Sweden. This table shows that firms with fewer employees, higher

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<sup>8</sup>The problem is similar to the so-called Ashenfelter’s-dip where enrolment in active labor market programs is more likely if a person has a temporary dip in earnings just before the program. Such an endogenous treatment assignment creates a mean reversion pattern with increased earnings growth after the program, even in the absence of it (Ashenfelter, 1978).

payroll, and lower productivity were more likely to be selected for privatization (conditional on year and industry fixed effects). The economic magnitudes of the estimates are, however, low, and the adjusted  $R^2$  is only 6.4%. This is consistent with an *ad hoc* process of selecting which firms to privatize rather than the idea that the government selects firms to privatize based on their economic performance.

### 4.3 Constructing the control groups

With the above in mind, we proceed in three steps to create the control groups. At the *worker level*, we first identify all workers employed in the firms that go private in the year before privatization. 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 are at least five years until a person reaches the retirement age of 65 years in the Swedish guaranteed pension system.

Second, for each cohort (privatization year in our sample), we perform exact cell matching at event time  $k - 0$  to find similar workers employed *in SOEs* that are not privatized during our sample period. We randomly match each treated worker with a control worker within cells of age, gender, industry, region, and wage (as measured by labor income).<sup>9</sup> The matching is done without replacement so that the control workers (or treated workers) are matched no more than once. An alternative would be to use all control workers in a cell and weight the estimates in the regressions. However, matching without replacement would leave some cells with only a few control observations because workers tend to stay in the same sector and industry for many years. One could also consider matching at the firm level rather than at the individual level or use propensity score matching rather than exact cell matching. However, matching at the worker level is better when outcomes are at the worker level as it creates a more comparable control group than matching at the firm level only (which omits individual covariates). A comparable control group is essential for the credibility of our identification strategy that substitutes the trend the treated would have had in the absence of treatment with the trend of the control group.<sup>10</sup> Finally, we collect information for treated and control workers in a window from  $k - 3$  up to  $k + 8$  and create event-specific data sets.

For the *firm level* analysis, we implement the same matching procedure as for workers, but with the matching done at the firm level. First, we identify 368 privatized firms that have information on all outcomes for at least three years before privatization. We do this to be able to analyze pre-trends in firm outcomes. Second, we match each firm a year before the privatization to *another SOE* in the same industry, region, year,

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<sup>9</sup>For age and wage, we match respective quartiles of their distributions in the year before privatization. For the region, we use the NUTS1 standard, *Nomenclature of territorial units for statistics*, that divides EU and the UK into major socio-economic regions. For industry, we use four broad industry classifications: Mining, manufacturing, and others; Transport, communication, and storage; Business activities and financial intermediaries; Other.

<sup>10</sup>Nevertheless, in the Internet Appendix, we replicate our main worker-level results to show that they hold when using propensity score matching at the firm level to find controls (Table IA9 and Figure IA6). We also show that our firm-level results continued to hold when using this matching strategy (Tables IA10 and IA11 and Figures IA7 and IA8). This robustness check also verifies that our worker and firm-level results are consistent when using workers from treated and control firms matched at the firm level.

and of similar size in terms of number of employees and whether the net profit is positive or not (randomly and without replacement) that is not privatized. Finally, we gather time-series information for each treated and control firm and combine them into cohort-specific panels, which we then stack. In contrast to our worker-level analysis, our firm-level analysis could suffer from attrition bias because ownership changes 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.<sup>11</sup>

#### 4.4 Comparing the treated and control groups

Panel A of Table 1 displays background, educational, and career characteristics one year before privatization for the treated and control workers we use in the individual-level analysis. 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.<sup>12</sup> A normalized  $t$ -value, in absolute 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. Most 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 16% of the treated workers holding a vocational degree and another 16% holding a university degree. The treated workers have approximately 270,000 SEK in annual wage income on average.<sup>13</sup> Wage income for the control workers is slightly 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 small and not statistically significant).

Panel B of Table 1 displays differences in observable characteristics between the treated and control firms we use in the firm-level analysis. 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 before privatization. For instance, control firms have, on average, around 10% more employees and payroll and slightly higher profitability, productivity, and leverage. These differences, however, are economically small and statistically insignificant with  $t$ -values well below 0.25.

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<sup>11</sup>During the four year post-period, the attrition rate is around 21% for treated firms and 11% for control firms (78 treated firms and 41 control firms). However, in Table IA12 in the Internet Appendix, we confirm that attrition among treated firms is not systematically correlated with pre-privatization observables.

<sup>12</sup>Standard  $t$ -values are a function of the sample size and decrease mechanically with it. 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).

<sup>13</sup>As of September 2023, the exchange rate is 0.09 USD per 1 SEK.

Overall, there are some economically small observable differences in means between the treated and control workers and firms, but normalized  $t$ -tests suggest that these differences are not statistically significant. Nevertheless, we add several of these pre-privatization observables to our regressions as controls to account for any remaining small differences in observables between the treated and control workers and firms. Since the underlying assumption in difference-in-differences analyses is parallel trends in the absence of treatment, these level differences in observables matter for identification only to the extent that they signal important differences in *time-varying unobservables* correlated with both privatizations and with the outcomes of interest.

## 4.5 External validity

Sweden's GDP per capita is above the OECD average, the government enjoys a high level of public trust, corruption is relatively rare, and labor market protections are robust. Privatizations during our sample period are also performed on an *ad hoc* basis. As such, we expect our results to hold the highest external validity in the case of countries that are similar countries to Sweden and with many SOEs that are potential candidates for privatization. Figure 1 plots the relationship between the fraction of employees in SOEs and social expenditures as a percent of GDP. Countries with both a high fraction of the labor force in SOEs and similar level of social expenditures to Sweden include the other Scandinavian countries, and large European countries such as France, Germany, Portugal, and Italy.

Our results are likely to have external validity with respect to these countries for several reasons. First, evidence suggests that labor markets in Sweden behave similarly to those in other developed countries, including other Scandinavian nations, Belgium, France, Germany, Italy, the Netherlands, and the United States (Lazear and Shaw, 2009). Second, the *ad hoc* approach to privatizing companies aligns with privatization efforts in, for instance, Switzerland, Finland, and Denmark. Given that large privatization programs are not as common as now as they were before, the approach Sweden followed with *ad hoc* privatizations is likely to be more common going forward as well.

We expect our results to have relatively lower external validity for less developed countries with lower social expenditures and that privatize firms through large privatization programs rather than in a more *ad hoc* fashion.

## 5 What is the cost of privatization for workers?

### 5.1 Income and unemployment

We first examine how privatization affects income and unemployment. *Ex ante*, the effects on workers are unclear. On the one hand, better governance and higher productivity increase labor demand, leading to

higher income and lower unemployment risk. On the other hand, the ownership change can trigger a workforce reorganization that leaves some or all workers worse off: under state control, soft budget constraints may have let managers avoid firing workers, and the ownership change itself could breach implicit contracts with workers.

Table 2 shows the results of our regressions using Equation 1, and Figure 3 shows the yearly difference-in-differences estimates (relative to event time zero) using Equation 2. In Panels A-D of Figure 3, we see that before privatization, the treated and control workers have almost identical trends in income and unemployment. None of the coefficients in this period significantly differ from zero at the five percent level. Table 2 Panel A reports the results for the entire post-period, while Panel B shows the short, medium, and long-run effects (event times 1-2, 3-4, and 5-8, respectively).

Panel A Column 1 shows that wages drop by 7.9% ( $t = 2.96$ ) on average in the full post-period. Panel B shows that in the short run, mean wage income falls by 5.8% ( $t = 3.47$ ), in the medium run, wages drop by 9.3% ( $t = 4.13$ ), and in the long run, they drop by 8.4% ( $t = 2.23$ ). The long-run wage drop amounts to around half of the drop that Seim (2019) estimates for displaced workers in Sweden.<sup>14</sup> The absence of a positive wage spike right after privatization suggests that labor buyout programs or severance payments to workers, which our wage measure includes, are small or rare (consistent with the discussion in Section 2).

These results can be related to previous studies on the costs of privatization for workers in terms of wages (our other results can not, since the other outcomes we investigate have not been studied before). Our results on temporary wage declines for workers are in line with evidence from two firms in Switzerland in Melly and Puhani (2013), who find that average wages follow a J-curve with an immediate drop and then a recovery. Our findings are also in line with Arnold (2022), who finds wage decreases in Brazil. In Brazil, however, the wage drop is much larger than that in Sweden (25%). A candidate explanation for these differences is that SOEs in Brazil pay a wage premium to workers that erodes after privatization. We are unaware of any studies documenting such wage premia for SOEs in Sweden. Brown et al. (2006b) also document wage declines for Ukraine. 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.

We next turn to the effects on unemployment. The full post-period effect on unemployment in Column 2 of Panel A shows that privatization leads to higher unemployment. The incidence rises by 1.3 percentage points ( $t = 5.16$ ), an 11.9% increase compared to the pre-privatization level among treated workers. Panel B shows that unemployment is *persistently* higher in the post-period with a gradually higher level, with

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<sup>14</sup>Seim (2019) reports that job displacement in Sweden leads, on average, to an earnings drop of 23.5% in the first year and to a 16.4% loss seven years after.

increases of 1.1 percentage points (10.7%,  $t = 4.02$ ) in the short run, 1.2 percentage points (11.5%,  $t = 4.27$ ) in the medium run, and 1.5 percentage points (14.3%,  $t = 5.27$ ) in the long run.

We follow workers over time regardless of their labor market status, so an interesting question is how much of the wage cut is driven by the mechanical effect of higher unemployment. While we can not calculate the exact figure, we can bound the effect. Our estimates show that 822 more treated workers become unemployed yearly after privatization. Assuming that wages for these 822 workers equaled the pre-treated mean for all treated workers, they stay unemployed for a full year and have no wage income during this period; their total wage loss equals 222,827.76 TSEK.<sup>15</sup> So, under these assumptions, the negative wage effect that would come from the 822 persons becoming unemployed would be 3.524 TSEK per treated worker and year.<sup>16</sup> Comparing this figure to the estimated average wage cut for all treated workers suggests that the mechanical effect of unemployment can only explain 16.4% of the actual wage cut we estimate.<sup>17</sup>

Finally, we study government transfers. Columns 3 and 4 show how privatization affects government transfers to workers and their total income, which is the sum of wage income and government transfers. Column 3 shows that government transfers rise by 11.9% ( $t = 5.82$ ), which reduces the change in total income (Column 4) to 3.5% ( $t = 3.5$ ). Panel B shows that government transfers increase steadily from 9.9% in the short run ( $t = 4.15$ ) to 11.2% ( $t = 5.27$ ) in the medium run to 13.5% ( $t = 6.09$ ) in the long run. The dynamic effect on total income in Column 4 of Panel B is roughly half of that on wage income in Column 1 for each sub-post period. To understand the details behind government transfers, we break down the privatization effect into the three key components discussed in Section 2.3—unemployment benefits, activity support, and social benefits—in Table IA3 and Figure IA1 in the Internet Appendix. All three outcomes have parallel pre-trends between the treated and control groups. Unemployment benefits increase immediately after privatization, with a dynamic pattern similar to the one for unemployment incidence. Activity support payments also rise right after privatization, with a slight upward trend over time. We find no evidence that privatization affects social benefit payouts. Overall, the point estimates for the full period show an 11.1% increase in unemployment benefits ( $t = 7.45$ ), a 4.3% ( $t = 6.69$ ) increase in activity support payments, and no effects on social benefit payouts (-0.02%,  $t = -0.23$ ).

## 5.2 Other worker outcomes

### 5.2.1 Labor market status

We complement our analyses on income and unemployment with analyses on unemployment days, leaving the labor force, and retiring. Table 3 presents the results for these outcomes and Figure 4 shows their

<sup>15</sup>The pre-treated mean is 271.08 TSEK, so total wage cut equals  $822 \times 271.08 = 222,827.76$  TSEK.

<sup>16</sup>To calculate the mechanical effect on wage income, we divide the total annual wage drop for the 822 workers by the number of treated workers in privatized firms:  $222,827.76/63,231 = 3.524$ .

<sup>17</sup>The calculation of the mechanical effect on wage income is  $3.524/21.420 = 0.164$

dynamic effects (all have parallel pre-trends). Column 1 in Panel A reveals that privatization increases the mean number of unemployment days by 2.1 days per year ( $t = 5.99$ ) over the full eight-year post-period, which is a 19.5% increase relative to the pre-period mean. Panel B shows that this effect grows over time, up to a 22.8% increase in the long run ( $t = 6.27$ ). Column 2 in Panel B shows a short-run increase of 13.2% ( $t = 2.05$ ) in being outside the labor force, but no effects in other periods (Panel A Column 2:  $t = 1.33$ , Panel B Column 3:  $t = 1.43$ , Panel C Column 3:  $t = 1.03$ ). Column 3 shows no effects of privatization on early retirement, either overall ( $t = -0.34$ ) or in any sub-post period ( $t = -0.63$ ,  $t = 0.72$  and  $t = -0.60$ ).

The estimates for the unemployment incidence and days imply that privatization causes an extra 822 unemployment spells and 133,986 days of unemployment per year, which add up to a total of 6,576 unemployment spells and 1,071,888 unemployment days over the eight-year post-period we analyze. These numbers are too small to impact the whole economy—the average annual number of unemployment days in Sweden was about 100 million from 1997 to 2017—but they reveal that privatization can harm some workers individually: those who become unemployed tend to remain unemployed for an average of 163 days.<sup>18</sup>

### 5.2.2 Entrepreneurship

We next examine whether privatization is associated with increased entrepreneurship among former SOE employees. Entrepreneurship is of particular interest because it has been shown to have positive spillover effects on the economy. Entrepreneurs tend to contribute to new job creation (Haltiwanger, Jarmin and Miranda, 2013), the introduction of new products and services (Acemoglu, Akcigit, Alp, Bloom and Kerr, 2018), the transfer of ideas from incumbent firms to the market (Audretsch, Bönte and Keilbach, 2008), and the creation of most of the new employment following positive demand shocks (Adelino, Ma and Robinson, 2017).

Two hypotheses suggest that privatization may encourage entrepreneurship. First, workers may be forced into entrepreneurship out of necessity due to layoffs and difficulty finding other employment. Indeed, prior research has found that job displacement almost doubles the probability of entering self-employment in Sweden (von Greiff, 2009). Although this form of entrepreneurship may help individuals avoid unemployment and provide a temporary source of income, it is unlikely to have large spillover effects on society regarding innovation or new product development.<sup>19</sup> Second, reorganization and new ownership may moti-

<sup>18</sup>The unemployment incidence increases on average by 1.3 percentage points per year, which results in  $0.013 \times 63,231 = 822$  extra workers becoming unemployed per year during an eight-year post-period. The number of unemployment days increases on average by 2.119 days per year (see Table 3), which results in a total of  $2.119 \times 63,231 = 133,986$  extra days per year and 1,071,888 days during an eight-year post-period. Dividing the total 1,071,888 days of unemployment by the extra 6,576 workers who end up unemployed shows that workers who become unemployed tend to remain unemployed for 163 days.

<sup>19</sup>A selection effect may also come into play: Ozcan 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 non-entrepreneurial people select into the public sector (Tåg, Åstebro and Thompson (2016) find similar patterns

vate entrepreneurial workers to see the change as an opportunity to bring new ideas and innovations to the market. For instance, recent studies have found that corporate R&D spending encourages workers to join founding teams of startups Babina and Howell (2022). This type of entrepreneurship is more likely to have substantial spillover effects on society.

We distinguish two types of entrepreneurship entry: self-employment (sole proprietorship) and business ownership (limited liability firm).<sup>20</sup> Table 3 Columns 4 and 5 and Figure 4 Panels D and E show the results and parallel trends for self-employment and business ownership before privatization. We find no effects on self-employment overall (Column 4,  $t = 0.53$ ) or in any sub-periods after privatization ( $t = 0.13$ ,  $t = 0.85$ , and  $t = 0.49$ ). Business ownership, however, increases by 14.7 basis points (96.8%,  $t = 3.61$ ) after privatization. Panel B Column 5 indicates that the increase is gradual over time, with no increase in the short run ( $t = 1.81$ ), a 14.6 basis-point increase in the medium run (96.3%,  $t = 3.08$ , and a 20 basis-point increase in the long run (131.7%,  $t = 3.52$ ). Because very few workers combined employment with running a business on the side before privatization, these effects correspond to only about one more entrepreneur per privatized SOE or five more entrepreneurs per thousand workers affected by privatization.<sup>21</sup>

We examine the economic importance of the firms run by the entrepreneurs in Table IA4 in the Internet Appendix, which shows the characteristics of the entrepreneurs and their firms after privatization. The evidence suggests that necessity entrepreneurship is the main driver of increased entrepreneurship entry. First, most of the new entrepreneurs (69%) are self-employed and run unlimited liability firms that rarely hire other employees (Astebro and Tåg, 2017). Second, entrepreneurship is not profitable. The total income of those who entered entrepreneurship (170 TSEK) is about half of their income before privatization (300 TSEK), while treated workers earn more than control workers. Third, almost all (95%) of the entrepreneurs run small firms with less than ten employees. Therefore, the spillovers from entrepreneurship entry seem to be limited. The entrepreneurs in our sample create about seven fewer jobs per year per privatized firm or 0.0001 fewer job-years per thousand privatized workers. They also generate additional value added equal to only 0.7% of the value added of the privatized firms.<sup>22</sup>

We explore three alternative mechanisms that could explain the increased entrepreneurship entry. First, for selection into more bureaucratic firms).

<sup>20</sup>The literature often separates necessity and opportunity entrepreneurship, although the terms may vary. See, for example, Schoar (2010), Tåg et al. (2016), Levine and Rubinstein (2017), Rider, Thompson, Kacperczyk and Tåg (2019) and Guzman and Stern (2020). Astebro and Tåg (2017) shows that most of the job growth in employment occurs in new ventures formed as limited liability companies. Starting a sole proprietorship is more suitable for non-growth-oriented firms than growth-oriented ones since it has lower initial capital and reporting requirements than a limited liability company.

<sup>21</sup>With a difference of 336 unique entrepreneurs between the treated and control group (see Table IA4 in the Internet Appendix), 368 unique SOEs in the sample, and 63,231 total workers, we find  $336/368 = 0.9$ , or about one more entrepreneur per privatized SOE, and  $336/63231 = 0.005$ , or about five more entrepreneurs per thousand workers affected by privatization.

<sup>22</sup>Our sample consists of 368 SOEs and 63,231 workers who experience privatization, generating a total value added of  $271.81 \times 63231 = 17,186,818.11$  TSEK before privatization. Column 3 in Table IA4 indicates that we have 714 more firm-year observations, with a mean employment difference of -0.01 workers, resulting in 7 fewer job-years. For value added, using the numbers in Table IA4, we obtain  $((342.05 \times 6.17) - (312.30 \times 6.19)) \times 714 = 126,600.41$  TSEK in extra value added.

the rise in business ownership may result from management buyouts, where the management buys the company from the state and becomes a business owner. This happens in our data, but only in 27 cases for the treated firms during our sample period. Also, if this were the main mechanism, we would see a sudden increase in business ownership at event time one in Figure 4, not a gradual one. Thus, management buyouts are too rare to account for the aggregate pattern. Second, higher entrepreneurship rates may stem from the deregulation of entire industries that create entrepreneurial opportunities for treated workers but not for control workers. However, this is unlikely since we match our control group within industries, so the control workers come from SOEs in the same industry. Furthermore, the deregulated industries during 1997-2017 are the domestic EU/EES airline industry (1997), the railroad and public transport industry (2007-2012), the preschool market (2006), the pharmacy industry (2009), primary health care (2010) and technical testing and analysis of vehicles industry (2010). In our sample, 23 privatizations happened in these industries in or after the year of deregulation, and these firms employ 2,927 workers (i.e., only 4.8% of the treated workers). Third, privatizations may involve firing workers and replacing them with external contractors who may be former SOE employees. Goldschmidt and Schmieler (2017) show that these “domestic outsourcing” practices are significant in at least Germany. However, this does not seem to be a major reason behind the small increases in business ownership we observe. Only 4% of the businesses started by treated entrepreneurs in our sample are in the industries that Goldschmidt and Schmieler (2017) classify as “domestic outsourcing” industries: logistics, cleaning, security, and food services (see Table IA4 in the Internet Appendix).

### **5.2.3 Family and health**

Unemployment increases the probability of divorce and mortality, according to existing research (Gerdtham and Johannesson, 2003; Eliason, 2012). Because privatization increases unemployment, it could also have negative spillovers on the workers’ families and health. We examine how privatization affects the probability of divorce and passing away in Columns 6 and 7 of Table 3. Figure 4 shows the dynamic effects and that there are no pre-trends in the outcomes. We find an increase of 0.63 divorces per thousand workers (8.3%,  $t = 2.22$ ) that shows up statistically significant in the long run (7.6 basis points, 10.0%,  $t = 2.34$ ) but no effects on mortality ( $t = -0.86$ ). In conclusion, privatization does seem to lead to a minuscule increase in divorce rates but does not appear to affect mortality.

### **5.2.4 Household finance**

We finally connect the literature on privatizations with the literature on household finance (Guiso and Sodini, 2013; Gomes, Haliassos and Ramadorai, 2021), which shows that human capital is a key asset for many workers and that income risk affects their financial decisions (Angerer and Lam, 2009; Fagereng et al., 2018). Privatization may alter the risk and return of human capital and thus impact household financial

behavior. Workers may face layoffs, switch to other firms, or stay with the privatized firm that now has a more profit-oriented owner. These changes may increase human capital risk and lead workers to lower their overall risk exposure by reducing financial market participation, decreasing the risky share of their financial portfolio, and paying off debt.

We use the Wealth Register at Statistics Sweden to test these hypotheses. This data source provides detailed information on financial market participation, the risky share of financial portfolios (including risky assets and cash), and total debt (such as mortgages and credit card debt) for Swedish households.<sup>23</sup> We limit our pre- and post-periods to two years and focus only on privatizations between 1999–2007 since this data is available only for this period.

Table 3, Columns 8-10 and Figure 4, Panels D and E, show the results and parallel trends for the three household finance outcomes before privatization. We see no effect of privatization on stock market participation (Column 8,  $t = -0.15$ ), which is consistent with workers not receiving shares in the privatized firms as most Swedish privatizations are trade sales and do not allocate shares to workers (OECD, 2003). We also find no significant effects of privatization on the risky share (Column 9,  $t = -0.88$ ) or the debt ratio (Column 10,  $t = -0.05$ ). The only marginally significant effect is a short-run decrease of -1.0% in the risky share (Column 9,  $t = -1.64$ ), which leads us to examine the components of the risky share and the debt ratio. In the Internet Appendix, Table IA5 and Figure IA2 show that cash holdings increase by 9.8% (3,900 SEK,  $t = 3.14$ ) but that privatization does not affect risky assets, debt, or overall wealth. This suggests that workers may increase their cash holdings slightly as a precautionary measure when their labor market status becomes more uncertain after privatization but that there are no other large effects on household financial outcomes.

## **6 What are the benefits of privatization for firms?**

### **6.1 Cutting labor costs to increase productivity**

Our worker-level analysis documented heightened unemployment and decreased wages after privatization. This pattern supports the notion that, before privatization, employment at state-owned enterprises (SOEs) might have exceeded what would be optimal from a purely profit-maximizing perspective. Such a discrepancy could have resulted from SOEs prioritizing the provision of stable employment and the maintenance of low unemployment rates or from the presence of soft budget constraints that enabled managers to circumvent the unpopular task of dismissing employees.

If this is the case, we would expect to observe a decrease in employment at the firm level, possibly accompanied by increased productivity and performance. To investigate this mechanism as an explanation

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<sup>23</sup>See Calvet, Campbell and Sodini (2007), Calvet and Sodini (2014), or Bach, Calvet and Sodini (2020) for more details on this data.

for the worker-level results, we next investigate how privatization affects firm-level outcomes in terms of the labor force (number of employees, job destruction, job creation, and payroll), performance (OROA), and productivity (value added per employee). Following on Davis, Haltiwanger and Schuh (1998), the employment growth rate from year  $t - 1$  to  $t$  at firm  $f$  is defined as:

$$g_{f,t} = \frac{E_{f,t} - E_{f,t-1}}{0.5 \times (E_{f,t} - E_{f,t-1})}, \quad (4)$$

This growth rate accounts for entries and exits of firms and is bounded between -2 (exits) and 2 (entries). So then, the job destruction and job creation rates are:

$$JDR_{f,t} = |\min\{g_{f,t}, 0\}|, \quad (5)$$

$$JCR_{f,t} = \max\{g_{f,t}, 0\}, \quad (6)$$

Table 4 presents the outcomes of our regression analysis using Equation 3, while Figure 5 illustrates the annual difference-in-differences estimates (in relation to event time zero). In Panels A-F of Figure 5, it is evident that before privatization, the trends in all outcomes for both treated and control firms were nearly identical. During this period, none of the coefficients are significantly distinct from zero at the five percent level. Table 4 contains two panels: Panel A reports the results for the entire post-privatization period, whereas Panel B delineates the short, medium, and long-term effects (corresponding to event times 1-2, 3-4, and 5-8, respectively).

Panel A, Column 1, indicates a 16.3% decrease in the firm's employee count ( $t = 2.82$ ) following privatization. This reduction in employment stems from a 10.9% surge in the job destruction rate (Column 2,  $t = 4.92$ ), while the job creation rate remains unaltered (Column 3). Column 4 displays a 12.2% ( $t = -2.15$ ) decline in the firm's total payroll. Panel B reveals that these effects materialize in the short term immediately after privatization and persist into the medium term.

Regarding performance, Column 5 demonstrates a marginally significant enhancement in profitability by 2.1 percentage points (1.82) from a baseline approximating zero, and Column 6 reveals a 35.7% ( $t = 2.77$ ) augmentation in productivity. Panel B indicates that these effects are relatively consistent in both the short and medium term, albeit with a slightly more pronounced increase in the medium term. The large increase in productivity warrants additional attention. Table IA6 in the Internet Appendix shows that large positive outliers drive the effect. We run both quantile regression and an alternative specification that uses the log of our productivity measure. We find that the results are driven by the top 75th and 90th percentile (16.2% with  $t = 2.52$  and 27.4% with  $t = 2.27$ ) and that logging productivity yields a productivity gain of 11.5% ( $t = 2.03$ ). Thus, productivity gains following privatization seem to be in the range of 11.5-35.7%.

The observed reductions in firm-level employment and payroll combined with improvements in perfor-

mance and productivity align with the interpretation that privatization is followed by a cost-saving strategy of reducing the firm’s workforce. The profitability and productivity increases 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. For instance, our results align well with those of D’souza and Megginson (1999) on profitability and productivity in 28 countries. Our results for profitability and employment are in line with La Porta and Lopez-de Silanes (1999) in Mexico.

## **6.2 Alternative drivers of productivity improvement**

While cutting labor costs can be the primary explanation for increased productivity following privatization, other mechanisms may be at play as well. We next consider three candidate explanations: upgrading labor quality, financial engineering, and governance engineering.

### **6.2.1 Upgrading labor quality**

The first alternative driver of productivity gains we consider is upgrading labor quality. One potential source of productivity improvements following privatization could be that the new owners replace less productive workers with more productive workers. To investigate this potential mechanism, we investigate whether the hired workers are of higher quality after the privatization, and whether those who separate are of relatively lower quality. To this end, we rely on measures of cognitive and non-cognitive skills from the Swedish military draft test. Cognitive skills pertain to a person’s capacity to execute a range of mental tasks closely related to learning and problem-solving. In contrast, non-cognitive skills encompass personality, social, and emotional characteristics, including empathy, sociability, conscientiousness, and determination. These scores are available on stanine scales for most of the males in our sample and have shown to be good measures of worker ability and are closely related to labor market success (Lindqvist and Vestman, 2011; Baghai, Silva, Thell and Vig, 2021). To measure the quality of hires and separations, we average the sum of cognitive and non-cognitive scores for males of all hired workers and all separated workers in a year and relate it to the average quality of workers inside the firm.

Panels A and B in Figure 6 and Columns 1 and 2 in Table 5 display the results. We find no differential pre-trends between treated and control firms. Overall for the full post-period, there are no statistically significant effects on the quality of hires (Column 1,  $t = 0.72$ ) or the quality of separations (Column 2,  $t = 1.15$ ). Panel B, however, shows a slight increase of 4.8% in the quality of hires in the medium run (Column,  $t = 2.12$ ). Thus, firms that privatize appear not to fire workers of lower quality selectively, but they do appear to upgrade the quality of their hires in the medium run. This may be one potential complementary driver of the productivity gains we observe.

### 6.2.2 Financial engineering

Next, we delve into the ways privatized firms modify their financial strategies. Significant shifts in capital investments could suggest that firms enhance their capital quality, which might be a reason for improved productivity after privatization. Capital investments might also potentially account for workforce reductions if capital replaces labor in a company's production function. Moreover, we examine whether there are changes in leverage post-privatization. This could occur if the firm's risk profile alters under new ownership or if businesses aim to boost leverage to gain increased bargaining power against employees. Panels C and D in Figure 6 and Columns 3 and 4 in Table 5 displays the results. The figures show no evidence of pre-trends in the investment ratio (defined as capital investments over total assets) nor in the leverage ratio (total debt to total assets). Panel A Columns 3 and 4 show no overall effects of privatization in the post-period for investment ratio ( $t = -0.93$ ) or leverage ( $t = -1.61$ ). There is, however, a slight 5.7% reduction in the leverage ratio in the medium run ( $t = -2.02$ ), which is statistically but not economically significant. Thus, overall there appears to be little financial engineering going on post-privatization that could explain the productivity gains we observe.

### 6.2.3 Governance engineering

Finally, we examine whether productivity gains following privatization can be linked to governance changes in privatized firms. Prior research provides strong evidence supporting the importance of governance. Djankov and Murrell (2002) reviews the privatization literature and highlights the significant impact of replacing underperforming CEOs. Gupta (2005), for instance, examines the role of management in partial privatizations in India. CEOs in SOEs might have been promoted based on connections rather than merit, leading to substantial improvements in managerial quality after privatization. Furthermore, management changes could disrupt implicit contracts between CEOs and workers, making labor cost reductions more probable (Shleifer and Summers, 1988). To explore whether post-privatization productivity gains are related to CEO changes, we re-run our regressions using productivity as a firm outcome. However, we now differentiate between privatizations where the CEO remains in charge and those where the CEO is replaced.

The results are displayed in Panels E and F of Figure 6 and Columns 5 and 6 in Table 5. Panels E and F in Figure 6 reveal no differential pre-trends in productivity and indicate that productivity increases post-privatization only if the CEO departs the company after privatization. Columns 5 and 6 in Table 5 demonstrate that productivity remains unaffected if the CEO stays ( $t = 0.81$ ) but increases by 31.4% ( $t = 2.00$ ) if the CEO is replaced. Table IA7 in the Internet Appendix replicates the analysis for the other firm-level outcomes we consider and show that privatizations in which the CEO is replaced are also associated with greater reductions in employment and payroll and increased profitability. Thus, consistent with prior literature, governance restructuring appears to be associated with productivity improvements following privatization.

## 7 Discussion

### 7.1 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 before privatization leads to higher selling 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, institutional details suggest that extensive retrenchment and restructuring programs are uncommon. Swedish SOEs tend to be relatively well-governed, which reduces the need for large retrenchment programs (OECD, 2009). There is also the additional complication that EU regulations of state support restrict the ability of countries to invest in substantial restructuring programs before privatization (Munkhammar, 2007). These regulations are more likely to be binding for larger firms that are privatized. And, as noted by the OECD (2003), small and medium-sized SOEs typically are not restructured before being sold because the new owners are likely to anyways restructure the company and so are not willing to pay a premium for a restructured firm. OECD (2003) also notes that restructuring before privatization is not common in trade sales, which is the typical method of privatizing SOEs in Sweden.

Second, in our firm-level analysis, we show that the trends in the number of employees for treated and control firms are stable in the years before privatization, suggesting that large retrenchment programs affecting workers in privatized firms tend not to be implemented.

Finally, in our main analysis we match the treated and control workers one year before a privatization takes place, which mitigates any short-term anticipation effects. But our key results continue to hold when we instead match three years before privatization. However, matching three years before the actual privatization biases the estimates towards zero in our setting, as many workers may leave the firm before the privatization, and are, thus, not affected. Nevertheless, it has the added benefit of accounting for potential separations before privatization due to potential retrenchment programs. Table IA8 in the Internet Appendix provides these results.

### 7.2 Alternative definitions of privatization

In this subsection, we investigate the robustness of our results to alternative definitions of privatization. We consider partial privatizations, share issue privatizations, and privatization through sales to specific actors.

### 7.2.1 Partial privatization

Partial privatizations in which the government retains a substantial stake in the SOE after the privatization are common all across the world (Bortolotti and Faccio, 2009). Partial privatizations can have powerful effects on incentives for managers to act on improving performance, even if the private investors hold less than a controlling majority stake. For instance, partial privatizations of less than 50% stakes in India are enough to improve profitability, productivity, and investment in partial privatized SOEs (Gupta, 2005).

Nevertheless, to investigate whether our results are robust to redefining our privatization dummy to focus on partial privatizations, we identify partial privatizations by looking at SOEs that get listed on a Swedish stock exchange while still remaining controlled by the state (that is, private investors own less than 50%). If the listed SOE is part of a corporate group, all its subsidiaries are considered part of the partial privatization. We then define the treatment group as all workers employed one year before the partial privatization and use the same matching strategy as in the main analysis to create the control group of workers.<sup>24</sup> The final sample contains 29,070 treated workers and an equal number of matched control workers that we track four years before the partial privatizations and up to eight years after.

Table 6 presents the results (figure IA3 in the Internet Appendix displays dynamic effects showing that we have parallel pre-trends in all outcomes). Column 1 shows that wages drop by 15.8% ( $t = 4.10$ ) on average in the full post-period. The effect on wages is almost double that in our main specification. The same doubling of the effect is apparent for unemployment: column 2 shows that privatization again leads to persistent unemployment with the incidence rising by 2.6 percentage points ( $t = 5.84$ ) or 42.6%. Columns 3 and 4 show that government transfers increase by 16.9% ( $t = 5.69$ ), reducing the impact of privatization on workers income to -10.1% ( $t = 3.24$ ). Thus, partial privatizations have similar effects on workers, but with larger magnitudes than full privatizations.<sup>25</sup>

### 7.2.2 Share issue privatization

Next, we investigate whether privatization through the private capital market (asset sales) or through the public market (share issue privatizations) affects worker outcomes in different ways. Issuing shares that are traded on a stock exchange can give managers useful signals on their performance and allows their compensation contracts to be tied to the stock price (Gupta, 2005). Thus, share issue privatizations could lead to stronger reallocation of labor relative to asset sales as managers may have stronger incentives to improve performance. On the other hand, share issue privatizations tend to be more common among more profitable SOEs as governments want share issue privatizations to succeed well to build support for the

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<sup>24</sup>From the pool of potential controls, we exclude workers that are part of a "full" privatization to ensure that the control group is not capturing any privatization effect.

<sup>25</sup>In the worker sample used in the main analysis, only 27 out of 553 privatizations have been partially privatized before we define the treatment. So partial sales play little role in the overall effects in our main analysis.

privatization program among domestic shareholders (Megginson, Nash, Netter and Poulsen, 2004). If higher profitability correlates with a lower need for labor reallocation, workers in share issue privatizations could do better than workers affected by privatizations that happen through asset sales in the private capital market. Moreover, dispersed ownership through share issue privatizations tend to make agency problems worse and thus give more power to managers in the firm who may not be so keen on implementing unpopular reorganization efforts.

To investigate whether workers do better or worse under share issue privatizations, we split our sample of privatizations into share issue privatizations and asset sales. Columns 1-2 in Table 7 and Panels A-B in Figure IA4 (in the Internet Appendix) display the results. In all sub-samples, we have parallel pre-trends in outcomes. Column A Table 7 shows that share issue privatizations tend to be associated with an increase in unemployment by two percentage points (22.1%,  $t = 6.58$ ), while workers' total income remains unchanged (5%,  $t = 1.89$ ). Column B reveals that the effect on unemployment is half of that (1.1 percentage points) for non-share issue privatizations (11.6%,  $t = 3.36$ ). (The p-value for the difference is 0.018.) That workers do worse under share issue privatizations is consistent with the results of the partial privatization in the previous subsection.

### 7.2.3 Privatization through sales to specific actors

Finally, we investigate whether the cost of privatization for workers differs depending on ownership type post-privatization. Existing literature has found larger effects on reorganization and performance if the new owners are financial or foreign owners, whereas insider ownership (by management or workers) is associated with weaker effects on firm outcomes (Megginson and Netter, 2001; Djankov and Murrell, 2002). A rationale is that foreign and financial owners have better access to technology, market knowledge, financing, and management skills. They may thus be better positioned to undertake substantial reorganization, which might bring with it higher costs to workers.<sup>26</sup>

To investigate this dimension of heterogeneity, we perform three sample splits: domestic versus foreign buyer, financial versus non-financial buyer, and management buyouts versus non-management buyouts. Columns 3-8 in Table 7 display the full post-period results. Panels C-H in Figure IA4 (in the Internet Appendix) display dynamic effects, showing that we have good pre-trends for all outcomes.

Column 3 shows that foreign buyers lead to an increase in unemployment by 2.4 percentage points (23.5%,  $t = 4.83$ ), accompanied by a decrease in the total income of 13.8% ( $t = 4.16$ ). When the buyer is domestic, column 4 shows that unemployment increases by only 0.7 percentage points (7.1%,  $t = 2.51$ )

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<sup>26</sup>As noted by Dansbo and Wallner (2007): "Another common argument is that these Swedish assets with a long history as integral parts of Sweden's economic life, that have been important not only for the income they bring but also for the people they employ, would leave the country if bought by a foreign owner. The emotional content in these arguments is exasperated by the traditionally "hot potato" of local employment figures. Opponents argue that if the companies are acquired by foreign corporations or venture capitalists, people will lose their jobs and all the profits will leave the country (without being re-invested in Sweden)."

and wages remain unchanged (2.1%,  $t = 1.04$ ). The difference between the two groups is statistically and economically significant (p-values of 0.000-0.004) and in line with existing evidence on firm-level employment changes outlined above.

Financial buyers, however, appear not to be worse for workers. Column 5 shows that unemployment incidence (10.7%,  $t = 1.49$ ) and total income (2.4%,  $t = 0.61$ ) remain unchanged if the buyer is a financial buyer, whereas column 6 shows that unemployment increases (13.5%,  $t = 4.92$ ) and total income remains unchanged (-3.9%,  $t = 1.73$ ) for non-financial buyers. The difference between the two is not statistically significant for unemployment (p-value of 0.711, but there is a relative statistically significant difference for total income (p-value of 0.047).

Finally, Columns 7 and 8 show that management buyouts (MBOs) appear to be accompanied by no statistically significant effects on unemployment (-20.7%,  $t = -0.96$ ) or wages (7.3%,  $t = 0.54$ ). All the negative effects on workers come from non-MBO privatizations, where unemployment incidence increases (13.9%,  $t = 5.21$ ) and wages remain unchanged (-3.6%,  $t = 1.54$ ). This is consistent with the results in our firm-level analysis, showing that productivity gains occur only when the CEO is replaced. The differences between the two are not, however, statistically significant (p-values of 0.110-0.435).

### **7.3 Additional heterogeneity analyses**

In this subsection, we investigate additional dimensions of heterogeneity. First, a large part of our sample consists of privatizations in the Transport and Business activity/financial intermediation industry (TB) that took place between 1997 and 2004, so we investigate whether our results could be entirely driven by what happened in these industries. To do this, we run a triple difference regression that compares before-after treated-control and non-TB versus TB industries. The results are presented in Column 1 (for unemployment) and Column 4 (for total income) of Table 8. The triple difference estimate (DDD) shows no differential effects on unemployment and total income across the two categories of industries, suggesting that the costs of privatization for workers are similar in both industries.

Second, the costs of privatization for workers could differ depending on the strength of the local labor market or on the macroeconomic conditions. So we also run triple difference regressions distinguishing between privatizations in regions with above-below median unemployment rates and between years with at least one quarter of negative GDP-growth and all other quarters. The results are displayed in Columns 2, 3, 5, and 6 of Table 8. The DDD estimates show no clear differential effects across regions or years in which local labor markets and macroeconomic conditions were weaker.

Third, in our main analysis, we examined labor income and unemployment over the entire individual's career. We here examine which workers are more likely to be laid off by the firm after privatization. We divide workers into subgroups based on age, skill, tenure, and gender. 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. Finally, a privatized firm may be under pressure to operate more efficiently, which might reduce the scope for gender discrimination within the firm and thus layoffs may affect women differently from men.

Because we want to know which workers are more likely to be laid off by the firm after privatization we perform a post-privatization analysis on how the composition of departing workers varies between treated and control groups. Thus the sample only includes employees that exit the firm they are employed at time  $t - 0$ . We take this approach because the difference-in-differences framework doesn't allow for conditioning on post-treatment outcomes. Table IA13 shows negligible differences in who exits the firm in the treated and control groups in terms of age, skill level, tenure, and gender.

## **7.4 Policy**

### **7.4.1 Costs and benefits**

When considering the potential benefits and costs of privatization, it is important to take into account all stakeholders. Some of the key factors in such cost-benefit analysis include the productivity gains resulting from increased efficiency, the impact of privatization on employment and wages for workers, potential changes in consumer surplus due to changes in prices or quality of goods and services, and the long-term dynamic effects on the economy. Additionally, it is important to consider the potential costs of government support for workers who are affected by privatization, as well as the revenues generated by the sale of state-owned assets. By taking a comprehensive approach to cost-benefit analysis, policymakers can better understand the potential impacts of privatization and make more informed decisions about whether and how to pursue such policy change. But capturing all these dimensions is challenging, even with the detailed data at our disposal. Our paper does, however, provide estimates of the direct costs to workers in terms of income losses and the benefits to firms in terms of productivity gains (Tables 2 and 4). Moreover, our results on other worker-level outcomes (Table 3) ensure us that the costs for workers are primarily paid in lost income and not in other non-labor outcomes.

Table IA14 in the Internet Appendix summarizes per worker costs and per worker productivity gains and calculates a few key ratios that we summarize here. First, workers lose out in terms of lost income, resulting

mainly from increased unemployment. Table 2 shows that wage income per worker and year decreases by 7.9% on average. Because we track incumbent workers over time irrespective of their labor market status, the wage effect comprises any direct wage effect for those who stay in the privatized firm and any indirect effect for those who leave. Accounting for government transfers, total income accounting drops by 3.5% on average, showing that government transfers offset half of the costs of privatization for workers in terms of lost wages.

Second, productivity measured by value added per employee increases by 35.7% in the mean (Table 4) and by 11.5% using the log mean that puts less weight on outliers, see Table IA6 (the other quantile estimates of productivity lie somewhere between these numbers). Thus, depending on the preferred point estimate, the productivity gains of privatization outweigh the costs to workers before government transfers by a factor between two and six. These productivity gains are shared between the government and the new owners of the firm through the revenues generated by the privatization (we cannot, however, calculate the exact split, since privatization revenues are not observable in our anonymous registry data).

Finally, government transfers to workers are about ten to 30% of the productivity gains per worker to firms. This implies that workers could be compensated to the full extent of their losses, yet there would still be a positive surplus left to share between the new owners of the firm and the government.

#### **7.4.2 Policy interventions**

To mitigate the possible adverse effects of privatization, governments have historically implemented a variety of responses. According to the OECD (2003), these responses have varied across countries and depend on factors such as the size and significance of the state-owned enterprise (SOE) sector as a source of employment, the macroeconomic environment's capacity to absorb displaced workers, the availability and scope of unemployment benefits and social welfare programs, and the degree to which social benefits and amenities are integrated into SOE activities. For example, in transition economies, SOEs often provide housing and social benefits. Additionally, various schemes for employee participation in privatization have been introduced in many OECD countries. As noted in section 2, privatization in Sweden typically are not combined with any specific policy measures to mitigate the potential costs to workers.

A key feature of the costs to workers from privatization that we document here is that the costs appear through increased (long-term) unemployment. This suggests that governments should consider implementing research-based policies that are helpful in combating unemployment as a way to mitigate the costs to workers from unemployment. For instance, Card et al. (2018) summarize the estimates from over 200 studies of active labor markets programs and find that these programs are particularly helpful for the long-term unemployed. These workers tend to benefit most from training programs and private sector employment programs, whereas they get relatively less help from job search programs.

## **8 Concluding summary**

This paper presents new evidence on the costs and benefits of privatization, focusing on the Swedish context and thus providing novel insights into its impact on workers and firms in advanced economies. Our key economic insight is that firm-level improvements after privatization are primarily coupled with income losses and unemployment and that the social safety net plays an important role in mitigating these costs. The government partially compensates for the wage income losses through increased transfers. At the firm level, privatization leads to improvements with increased productivity and profitability, and the evidence is consistent with the interpretation that operational and governance processes improve. The productivity gains from privatization roughly outweigh the associated economic costs to workers by a factor between two and six.

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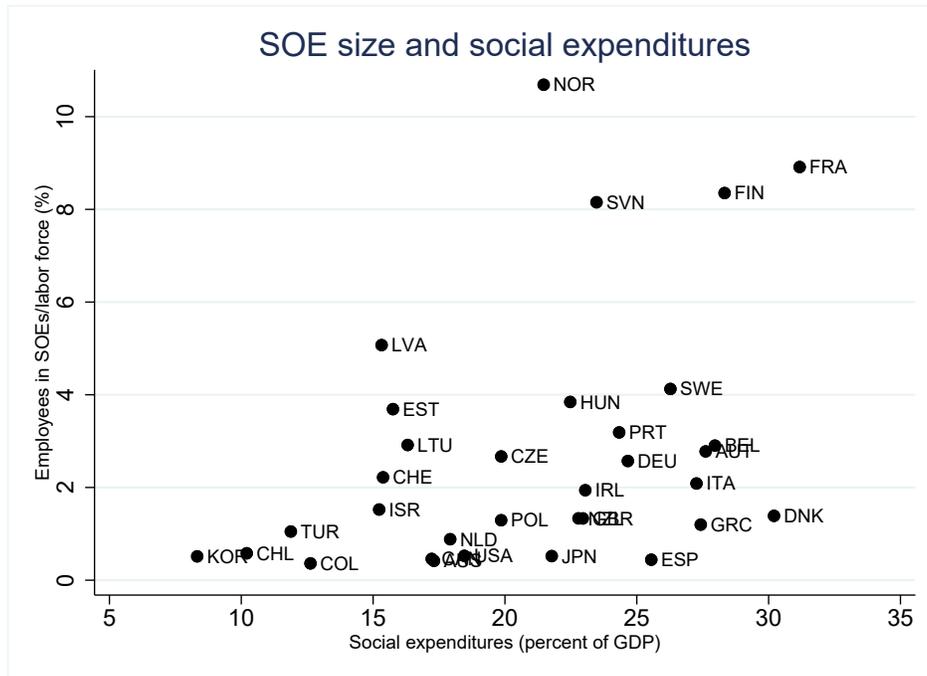
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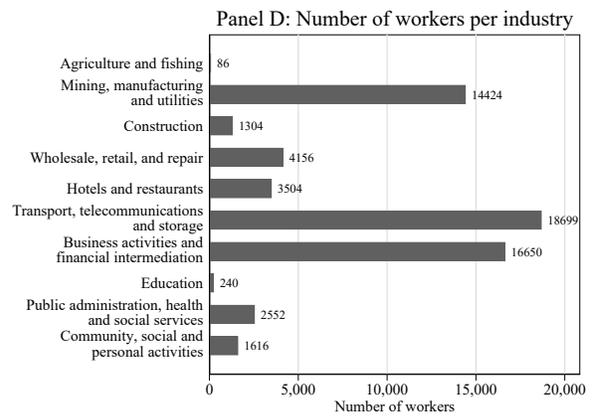
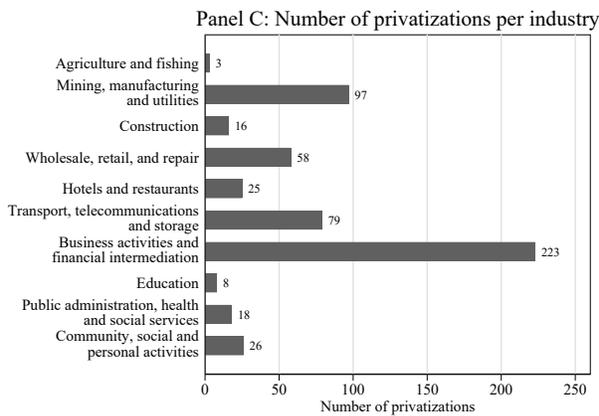
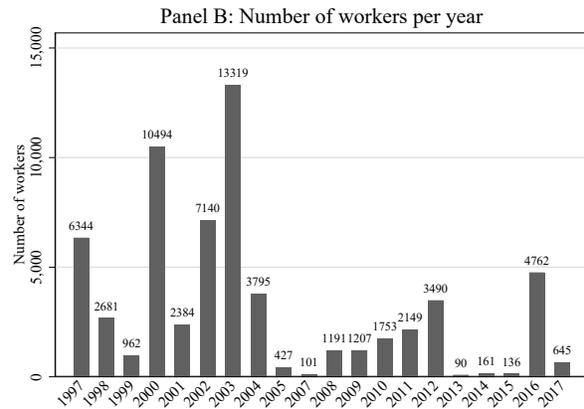
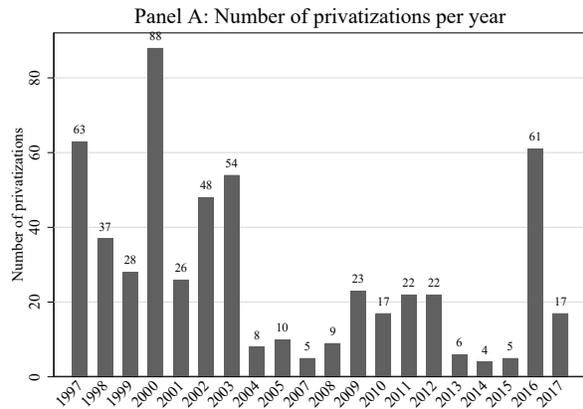
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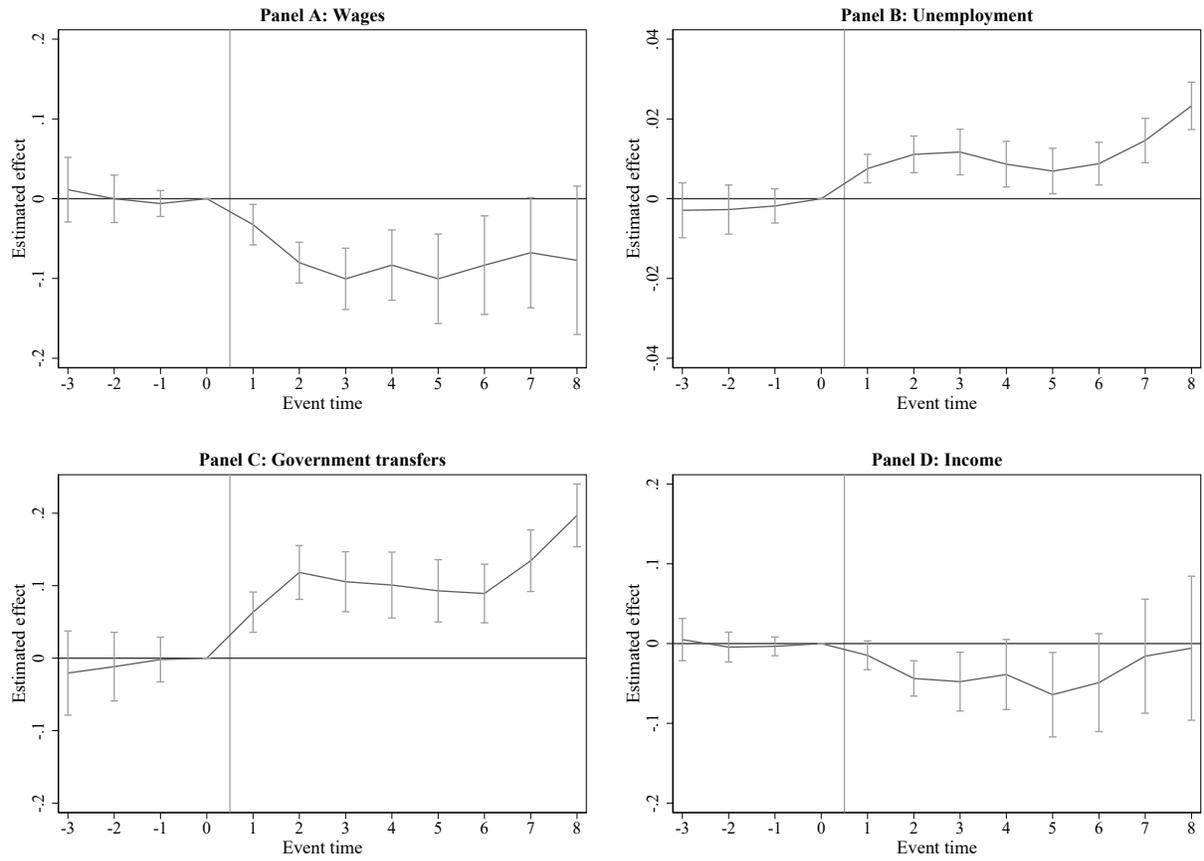


**Figure 1: SOE size and government social expenditures** This figure plots the relationship between the fraction of employees in SOEs and social expenditures as a percent of GDP using OECD data from 2012 (the latest year for which OECD has information on employees in SOEs). *Employees in SOEs* is based on a 2012 survey of government delegates to the OECD Working Party on State Ownership and Privatisation Practices. The remaining data are from the standard OECD reference series.

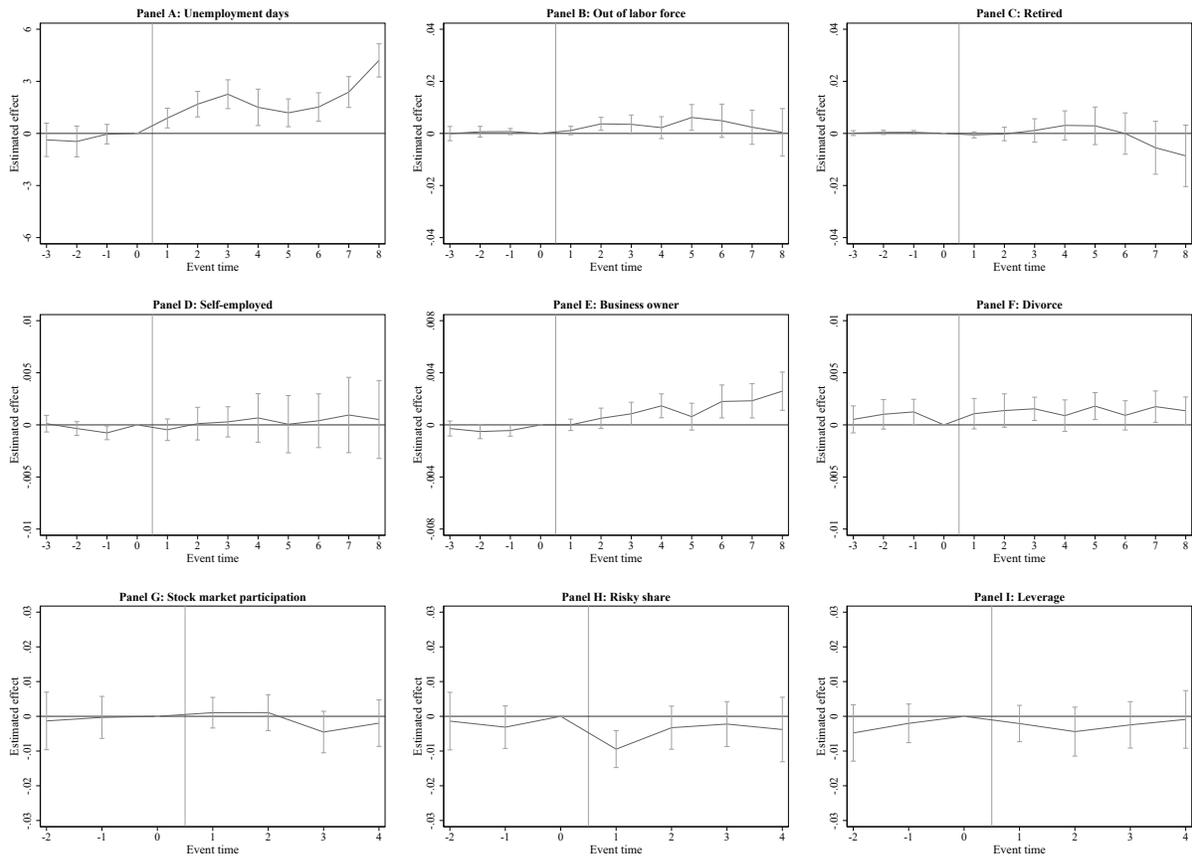


**Figure 2: Privatizations vary over time and across industries**

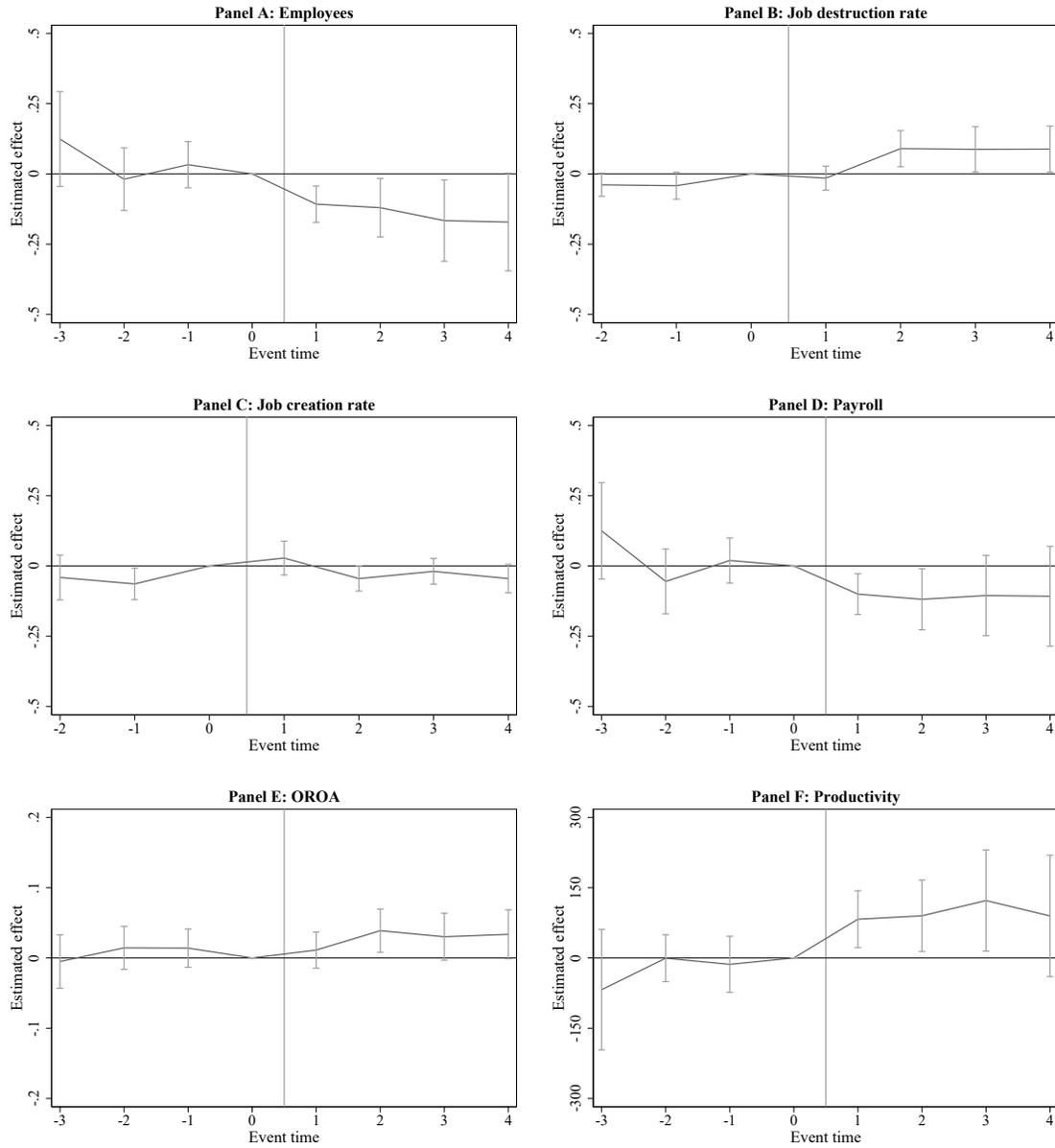
The sample consists of privatized firms in Sweden with more than five employees that took place between 1997 and 2017. The number of workers refers to the number of employees in the privatized firms one year before the privatization. The industry classification is based on NACE Rev 1.1.



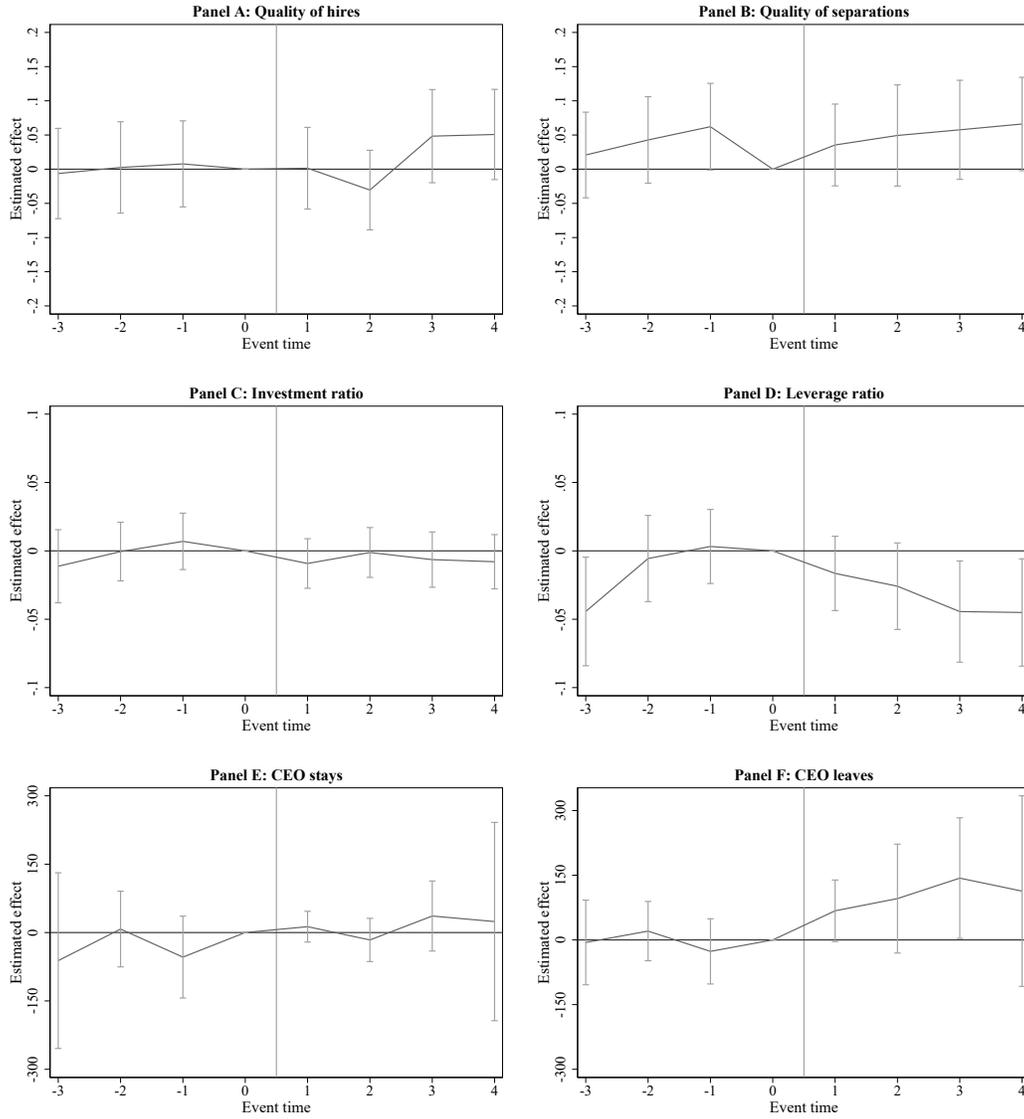
**Figure 3: Effects of privatization on wages, unemployment, government transfers, and income** The figures display dynamic difference-in-differences estimates relative to the year prior to the privatization (event time 0) using the model in Equation 2. The vertical bars display 95% confidence intervals using robust standard errors clustered at the municipality level. Variable and sample descriptions are available in Table 2.



**Figure 4: Effects of privatization on other worker outcomes** The figures display dynamic difference-in-differences estimates relative to the year prior to the privatization (event time 0) using the model in Equation 2. The vertical bars display 95% confidence intervals using robust standard errors clustered at the municipality level. Variable descriptions are available in Table 3.



**Figure 5: Effects of privatization on firm-level outcomes** The figures display dynamic difference-in-differences estimates relative to the year prior to the privatization (event time 0) using the model in Equation 3 adjusted to account for dynamic effects. The vertical bars display 95% confidence intervals using robust standard errors clustered at the firm level. Variable descriptions are available in Table 4.



**Figure 6: Effects on additional firm-level outcomes.** The figures display dynamic difference-in-differences estimates relative to the year prior to the privatization (event time 0) using the model in Equation 3 adjusted to account for dynamic effects. The vertical bars display 95% confidence intervals using robust standard errors clustered at the firm level. Variable descriptions are available in Table 5.

**Table 1: Comparison of treated and control groups**

The table displays mean characteristics of treated and control workers and firms 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. *Employees* is the number of employees, *Payroll* is the sum of the annual wage income of workers employed in the firm in hundreds of SEK, *OROA* is operating return on assets, and *Productivity* is the value added per employee in thousands of SEK. *Investment ratio* is the capital expenditure divided by total assets. *Leverage* is the sum of short- and long-term liabilities divided by total assets.

	Treated 1	Control 2	Difference 3	Norm. T-value 4
<b>Panel A: Treated and control (worker-level analysis)</b>				
Female	36%	36%	0%	0.00
Immigrant	15%	13%	2%	0.05
Age				
20-33	30%	30%	0%	0.00
34-43	26%	26%	0%	0.00
44-52	24%	24%	0%	0.00
53-60	20%	20%	0%	0.00
Education				
Basic	15%	12%	3%	0.06
High School	53%	53%	0%	0.00
Vocational	16%	16%	0%	-0.01
University	16%	19%	-2%	-0.05
Wage (thousands SEK)	271.08	265.72	5.36	0.02
Transfers (thousands SEK)	4.07	3.77	0.31	0.01
Total income (thousands SEK)	275.15	269.49	5.69	0.02
Labor market experience				
0-5	18%	18%	-1%	-0.01
6-10	11%	11%	0%	0.00
11-20	25%	25%	0%	0.00
21-30	23%	23%	0%	-0.01
30+	23%	22%	1%	0.02
Tenure				
0-2	59%	56%	4%	0.05
3-5	21%	21%	0%	0.00
6-10	15%	19%	-4%	-0.08
11-15	3%	3%	0%	0.00
16+	2%	2%	0%	0.02
Observations	63231	63231		
<b>Panel B: Treated and control (firm-level analysis)</b>				
Employees	107.00	118.90	-11.90	-0.02
Payroll	279.58	302.37	-22.79	-0.02
OROA	0.00	0.02	-0.02	-0.06
Productivity	271.81	309.00	-37.19	-0.08
Investment ratio	0.06	0.08	-0.01	-0.08
Leverage	0.63	0.68	-0.05	-0.14
Observations	368	368		

**Table 2: Wages, unemployment, and government transfers**

The table reports difference-in-differences estimates from regressions explaining worker outcomes after privatization using the models in Equations 1 and 2 (t-statistics in parenthesis). The sample consists of treated workers employed one year prior to the privatization and matched control workers. All models control for the number of employees and fixed effects for age, gender, immigrant status, labor market experience, tenure, education, municipality, industry, calendar year and the privatization year. All controls, except calendar and the privatization year, are measured one year before the privatization. The standard errors are clustered at the municipality level. The %-change uses the mean for workers in privatized firms in the years before the privatization as the baseline. *Wage* is the log of annual gross salary income (using inverse hyperbolic sine transformation), *Unemployment* takes the value one if an individual was unemployed at any time during the year and zero otherwise, *Transfers* is the log of annual gross government transfers to workers comprised of unemployment benefits, active support payments, and social benefits (using inverse hyperbolic sine transformation), and *Income* is the log of the sum of wages and transfers (using inverse hyperbolic sine transformation).

Dependent variable Specification	Wages 1	Unemployment 2	Transfers 3	Income 4
<b>Panel A: Average effect</b>				
Full period	-0.079 (-2.96)	0.013 (5.16)	0.119 (5.82)	-0.035 (-1.52)
%-change	-7.9%	12.6%	11.9%	-3.5%
Adjusted $R^2$	0.121	0.072	0.064	0.129
<b>Panel B: Dynamic effect</b>				
Short run (1-2 years)	-0.058 (-3.47)	0.011 (4.02)	0.099 (4.15)	-0.029 (-2.61)
Medium run (3-4 years)	-0.093 (-4.13)	0.012 (4.27)	0.112 (5.27)	-0.043 (-2.15)
Long run (5-8 years)	-0.084 (-2.23)	0.015 (5.27)	0.135 (6.09)	-0.034 (-1.00)
%-change				
Short run	-5.8%	10.7%	9.9%	-2.9%
Medium run	-9.3%	11.5%	11.2%	-4.3%
Long run	-8.4%	14.3%	13.5%	-3.4%
Adjusted $R^2$	0.123	0.072	0.065	0.131
Mean dependent variable	7.995	0.105	0.723	8.142
Number of observations	1414270	1414270	1414270	1414270

**Table 3: Other outcomes**

The table reports difference-in-differences estimates from regressions explaining worker outcomes after privatization using the models in Equations 1 and 2 (t-statistics in parenthesis). The sample consists of treated workers employed one year before the privatization and matched control workers. All models control for the number of employees and fixed effects for age, gender, immigrant status, labor market experience, tenure, education, municipality, industry, calendar year, and the privatization year. All controls, except calendar and the privatization year, are measured one year before the privatization. The standard errors are clustered at the municipality level. The %-change uses the mean for workers in privatized firms in the years before the privatization as the baseline. *Unemployment days* is the yearly total number of unemployment days, *Out of labor force* 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. *Self-employed* takes the value one if an individual runs an unlimited liability sole-proprietorship and zero otherwise. *Business owner* takes the value one if an individual is employed in a limited liability company (aktiebolag) he or she owns 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, *Stock market participation* takes the value one if an individual owns risky stocks, *Mortality* measures if the individual passed away during a given year and is defined as deaths per thousand. The model for mortality uses only post-years with the dynamic estimates estimated by a separate model for each period, *Risky share* measures the share of risky assets to total assets of an individuals portfolio, and *Leverage* measures total personal debt to total personal assets.

Dependent variable Specification	Unem. Days 1	Out of LF 2	Retirement 3	Self-employed 4	Business owner 5	Divorce 6	Mortality 7	Stock market Part. 8	Risky share 9	Debt ratio 10
<b>Panel A: Average effect</b>										
Full period	2.119 (5.99)	0.003 (1.33)	-0.001 (-0.34)	0.0005 (0.53)	0.0015 (3.61)	0.001 (2.22)	-0.080 (-0.86)	-0.0005 (-0.15)	-0.0032 (-0.88)	-0.0002 (-0.05)
%-change	19.5%	17.6%	-39.5%	19.1%	96.8%	8.3%	-	-0.1%	-0.7%	0.0%
Adjusted $R^2$	0.035	0.084	0.221	0.011	0.005	0.002	0.002	0.045	0.032	0.108
<b>Panel B: Dynamic effect</b>										
Short run	1.501 (3.85)	0.002 (2.05)	-0.001 (-0.63)	0.00008 (0.13)	0.00056 (1.81)	0.0006 (1.11)	0.091 (0.54)	0.0016 (0.48)	-0.0049 (-1.64)	-0.0010 (-0.24)
Medium run	2.100 (4.94)	0.003 (1.43)	0.002 (0.72)	0.00073 (0.85)	0.00146 (3.08)	0.0015 (1.14)	-0.071 (-0.23)	-0.0027 (-0.67)	-0.0015 (-0.32)	0.0006 (0.12)
Long run	2.487 (6.27)	0.003 (1.03)	-0.003 (-0.60)	0.00073 (0.49)	0.00200 (3.52)	0.0020 (2.34)	-0.320 (-1.58)			
%-change	13.8%	13.2%	-23.5%	2.7%	36.9%	6.9%	-	0.3%	-1.0%	-0.2%
Short run	19.3%	16.3%	71.7%	25.4%	96.3%	6.8%	-	-0.4%	-0.3%	0.1%
Medium run	22.8%	20.8%	-108.1%	25.2%	131.7%	10.0%	-			
Long run	0.035	0.085	0.221	0.011	0.005	0.002	0.005	0.045	0.032	0.108
Adjusted $R^2$										
Mean dependent variable	10.894	0.016	0.003	0.0029	0.0015	0.0076	-	0.639	0.470	0.560
Number of observations	1414270	1414270	1414270	1414270	1414270	1414270	910276	342554	342554	342554

**Table 4: Firm level outcomes**

The table reports difference-in-differences estimates from regressions explaining worker outcomes after privatization using the models in Equation 3 adjusted to account for dynamic effects (t-statistics in parenthesis). The sample consists of treated firms and matched control firms, includes information for three years before and four years after the privatization, and covers 1997 to 2017. All models include fixed effects for the privatization year, calendar year, industry, region, and firms' age one year before the privatization. All controls, except calendar and the privatization year, are measured one year before the privatization. The %-change uses the mean for privatized firms over the years before the privatization as the baseline. *Employees* is the number of employees, *Job destruction (creation) rate* measures the job destruction (creation) rate defined in Equation 6, *Payroll* is the sum of the annual wage income of workers employed in the firm in hundreds of SEK, *OROA* is operating return on assets, and *Productivity* is the value added per employee in thousands of SEK.

Dependent variable Specification	Employees 1	Job destruction 2	Job creation 3	Payroll 4	OROA 5	Productivity 6
<b>Panel A: Average effect</b>						
Full period	-0.163 (-2.82)	0.109 (4.92)	-0.025 (-1.40)	-0.122 (-2.15)	0.021 (1.82)	109.755 (2.77)
%-change	-16.3%	10.9%	-2.5%	-12.2%	321.3%	35.7%
Adjusted $R^2$	0.186	0.448	0.721	0.224	0.074	0.072
<b>Panel B: Dynamic effect</b>						
Short run (1-2 years)	-0.141 (-2.81)	0.118 (4.40)	0.018 (1.46)	-0.124 (-2.45)	0.018 (1.46)	102.247 (2.91)
Medium run (3-4 years)	-0.196 (-2.32)	0.098 (3.23)	0.026 (1.69)	-0.122 (-1.45)	0.026 (1.69)	122.811 (2.12)
%-change						
Short run	-14.1%	11.8%	-2.7%	-12.4%	273.8%	33.3%
Medium run	-19.6%	9.8%	-2.2%	-12.2%	392.1%	39.9%
Adjusted $R^2$	0.186	0.448	0.721	0.224	0.074	0.073
Mean dependent variable	0.186	0.072	0.375	11.392	0.007	307.497
Number of observations	4804	4611	4611	4804	4804	4804

**Table 5: Additional firm level outcomes**

The table reports difference-in-differences estimates from regressions explaining firm outcomes after privatization using the model in Equation 3 adjusted to account for dynamic effects (t-statistics in parenthesis). The sample consists of treated firms and matched control firms, includes information for three years before and four years after the privatization, and covers 1997 to 2017. All models include fixed effects for the privatization year, calendar year, industry, region, and firms' age one year before the privatization. All controls, except calendar and the privatization year, are measured one year before the privatization. The %-change uses the mean for privatized firms over the years before the privatization as the baseline. *Quality Hir* is the mean talent of hired persons relative to the mean talent of the incumbent workforce, where talent is defined as the sum of cognitive and non-cognitive ability measures at military enlistment for males. *Quality Sep* is the mean talent of persons leaving the firm relative to the mean talent of the incumbent workforce. *Investment ratio* is the capital expenditure divided by total assets. *Leverage* is the sum of short- and long-term liabilities divided by total assets. *Productivity* is the value added per employee in thousands of SEK, and *CEO remains* refers to the sub-sample of privatizations in which the CEO is not replaced in the post-period. Specifications 1 to 4 use the full sample while the sample in specification 5 includes all control firms but only treated firms where the CEO remained the same during the post-period. The sample in specification 6 includes all control firms but only treated firms where the CEO was replaced during the post-period. Standard errors are clustered at the firm level.

Dependent variable	Quality Hir	Quality Sep	Investment ratio	Leverage	Productivity	Productivity
Sample	Full	Full	Full	Full	CEO remains	CEO replaced
Specification	1	2	3	4	5	6
<b>Panel A: Average effect</b>						
Full period	0.012 (0.72)	0.020 (1.15)	-0.006 (-0.93)	-0.022 (-1.61)	40.009 (0.81)	98.024 (2.00)
%-change	1.2%	1.9%	-8.4%	-3.5%	12.8%	31.4%
Adjusted $R^2$	0.004	0.025	0.063	0.108	0.116	0.077
<b>Panel B: Dynamic effect</b>						
Short run (1-2 years)	-0.015 (-0.78)	0.011 (0.52)	-0.005 (-0.76)	-0.012 (-0.91)	26.725 (0.64)	81.907 (1.82)
Medium run (3-4 years)	0.048 (2.12)	0.031 (1.41)	-0.007 (-0.91)	-0.036 (-2.02)	58.086 (0.77)	130.291 (1.65)
%-change						
Short run	-1.5%	1.1%	-7.2%	-1.9%	8.5%	26.2%
Medium run	4.8%	3.0%	-9.9%	-5.7%	18.6%	41.7%
Adjusted $R^2$	0.006	0.026	0.063	0.108	0.116	0.077
Mean dependent variable	1.011	1.012	0.069	0.630	312.662	312.662
Number of observations	2957	2757	4804	4804	2433	2278

**Table 6: Wages, unemployment and government transfers for partial privatizations**

The table reports difference-in-differences estimates from regressions explaining worker outcomes after privatization using the models in Equations 1 and 2 (t-statistics in parenthesis). The sample consists of treated workers employed one year prior to the partial privatization and matched control workers. All models control for the number of employees and fixed effects for age, gender, immigrant status, labor market experience, tenure, education, municipality, industry, calendar year, and the privatization year. All controls, except calendar and the privatization year, are measured one year before the privatization. The standard errors are clustered at the municipality level. The %-change uses the mean for workers in privatized firms in the years before the privatization as the baseline. Variable descriptions are available in Table 2.

Dependent variable Specification	Wage 1	Unemployment 2	Transfers 3	Total income 4
<b>Panel A: Average effect</b>				
Full period	-0.158 (-4.10)	0.026 (5.84)	0.169 (5.69)	-0.101 (-3.24)
%-change	-15.8%	42.6%	16.9%	-10.1%
Adjusted $R^2$	0.136	0.061	0.049	0.149
<b>Panel B: Dynamic effect</b>				
Short run (1-2 years)	-0.061 (-3.10)	0.002 (0.55)	0.016 (0.70)	-0.039 (-2.47)
Medium run (3-4 years)	-0.089 (-2.90)	0.026 (6.87)	0.134 (5.44)	-0.070 (-2.64)
Long run (5-8 years)	-0.241 (-4.46)	0.038 (6.57)	0.264 (6.82)	-0.148 (-3.37)
%-change				
Short run	-6.1%	3.0%	1.6%	-3.9%
Medium run	-8.9%	42.4%	13.4%	-7.0%
Long run	-24.1%	62.9%	26.4%	-14.8%
Adjusted $R^2$	0.137	0.062	0.050	0.150
Mean dependent variable	8.084	0.061	0.398	8.166
Number of observations	690027	690027	690027	690027

**Table 7: Heterogeneity across privatization type**

The table reports difference-in-differences estimates from regressions explaining worker outcomes after privatization using the model in Equation 1 (t-statistics in parenthesis). Each sub sample consists of treated workers employed one year prior to the privatization and matched control workers. All models control for the number of employees and fixed effects for age, gender, immigrant status, labor market experience, tenure, education, municipality, industry, calendar year, and the privatization year. All controls, except calendar and the privatization year, are measured one year before the privatization. The standard errors are clustered at the municipality level. The %-change uses the mean for workers in privatized firms in the years before the privatization as the baseline. *Share issue* is the sample of treated workers where the privatized firm (or its new owner) is listed on the stock market, *Foreign buyer* refers to the sample of treated workers where the new owner of the privatized firm is a foreign firm, *Financial buyer* refers to new owners with industry codes corresponding to "Investment trust activities", "Security brokering and fund management", "Activities auxiliary to financial intermediation", or "Activities of investment companies and venture capital companies", and *MBO* refers to privatizations in which at least one of the management/employees at  $t - 1$  is the new owner, i.e., the firm is privatized through a management buyout. *Unemployment* and *Total income* are defined in Table 2.

Type	Share issue		Foreign buyer		Financial buyer		MBO	
	Yes	No	Yes	No	Yes	No	Yes	No
Specification	1	2	3	4	5	6	7	8
<b>Panel A: Unemployment</b>								
Full period	0.020 (6.58)	0.011 (3.36)	0.024 (4.83)	0.007 (2.51)	0.010 (1.49)	0.013 (4.92)	-0.020 (-0.96)	0.013 (5.21)
%-change	22.1%	11.6%	23.5%	7.1%	10.7%	13.5%	-20.7%	13.9%
P-value for difference		0.018		0.004		0.711		0.110
Mean of dep. var.	0.092	0.092	0.100	0.100	0.098	0.098	0.097	0.097
Adjusted $R^2$	0.075	0.075	0.076	0.075	0.082	0.071	0.081	0.072
<b>Panel B: Total income</b>								
Full period	0.050 (1.89)	-0.063 (-2.69)	-0.138 (-4.16)	0.021 (1.04)	0.024 (0.61)	-0.039 (-1.73)	0.073 (0.54)	-0.036 (-1.54)
%-change	5.0%	-6.3%	-13.8%	2.1%	2.4%	-3.9%	7.3%	-3.6%
P-value for difference		0.000		0.000		0.047		0.435
Adjusted $R^2$	0.136	0.129	0.134	0.130	0.135	0.129	0.134	0.129
Number of observations	884844	1237123	949451	1172516	761384	1360583	713262	1408705

**Table 8: Heterogeneity across industries, labor markets, and macroeconomic conditions**

The table reports the triple-difference coefficient from regressions explaining worker outcomes after privatization (t-statistics in parenthesis). Each subsample consists of treated workers employed one year prior to the privatization and matched control workers. All models control for the number of employees and fixed effects for age, gender, immigrant status, labor market experience, tenure, education, municipality, industry, calendar year and the privatization year. All controls, except calendar and the privatization year, are measured one year before the privatization. The standard errors are clustered at the municipality level. The %-change uses the mean for workers in privatized firms in the years before the privatization as the baseline. *Industry* takes the value one if treated and control workers were employed at time  $t - 1$  in the Transport industry or the Business activity/intermediary industry and zero for all other industries. *High unemp.* takes the value one if treated and control workers were employed at time  $t - 1$  in a region with an above median unemployment rate. *Recession* takes the value one if there was at least one quarter of negative GDP-growth during year  $t - 1$  and zero otherwise. *Unemployment* and *Total income* are defined in Table 2.

Dependent variable Split Specification	Unemployment			Total income		
	TB 1	High unemp. 2	Recession 3	TB 4	High unemp. 5	Recession 6
DDD	-0.003 (-0.30)	0.011 (1.83)	0.000 (-0.05)	-0.045 (-1.23)	-0.010 (-0.26)	-0.015 (-0.45)
%-change	-2.8%	8.6%	-0.3%	-4.5%	-1.0%	-1.5%
Adjusted $R^2$	0.072	0.072	0.072	0.129	0.129	0.129
Mean dep. variable	0.091	0.134	0.091	8.227	8.107	8.211
Number of obs.	1414270	1414270	1414270	1414270	1414270	1414270

# Internet Appendix

## What Is the Cost of Privatization for Workers?

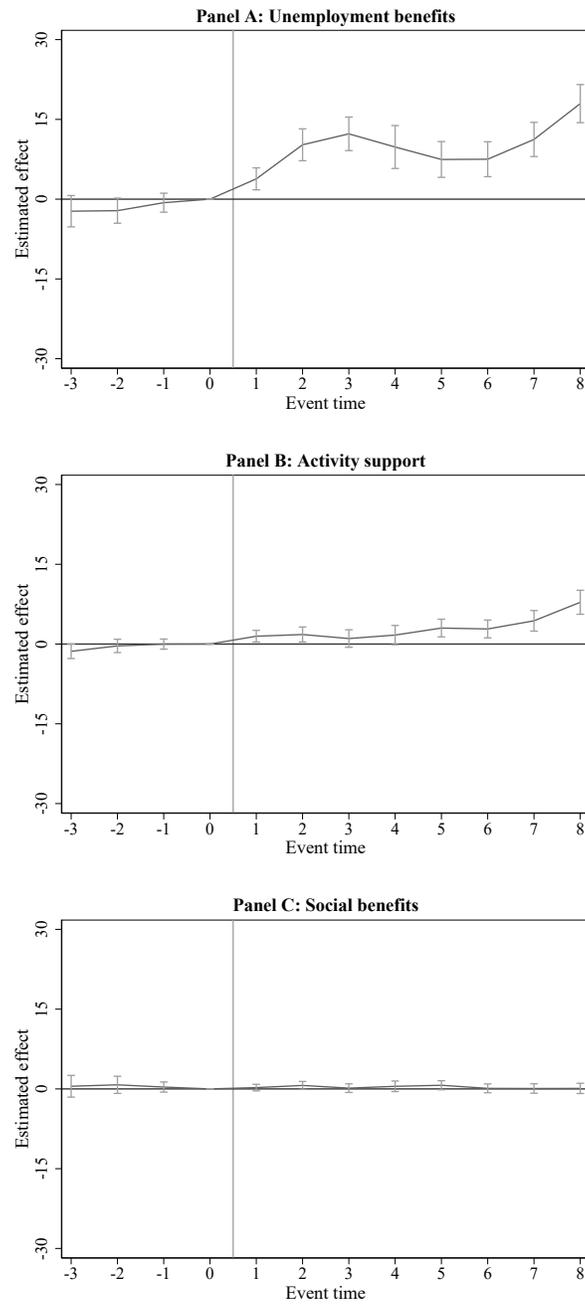
Martin Olsson

IFN and IFAU

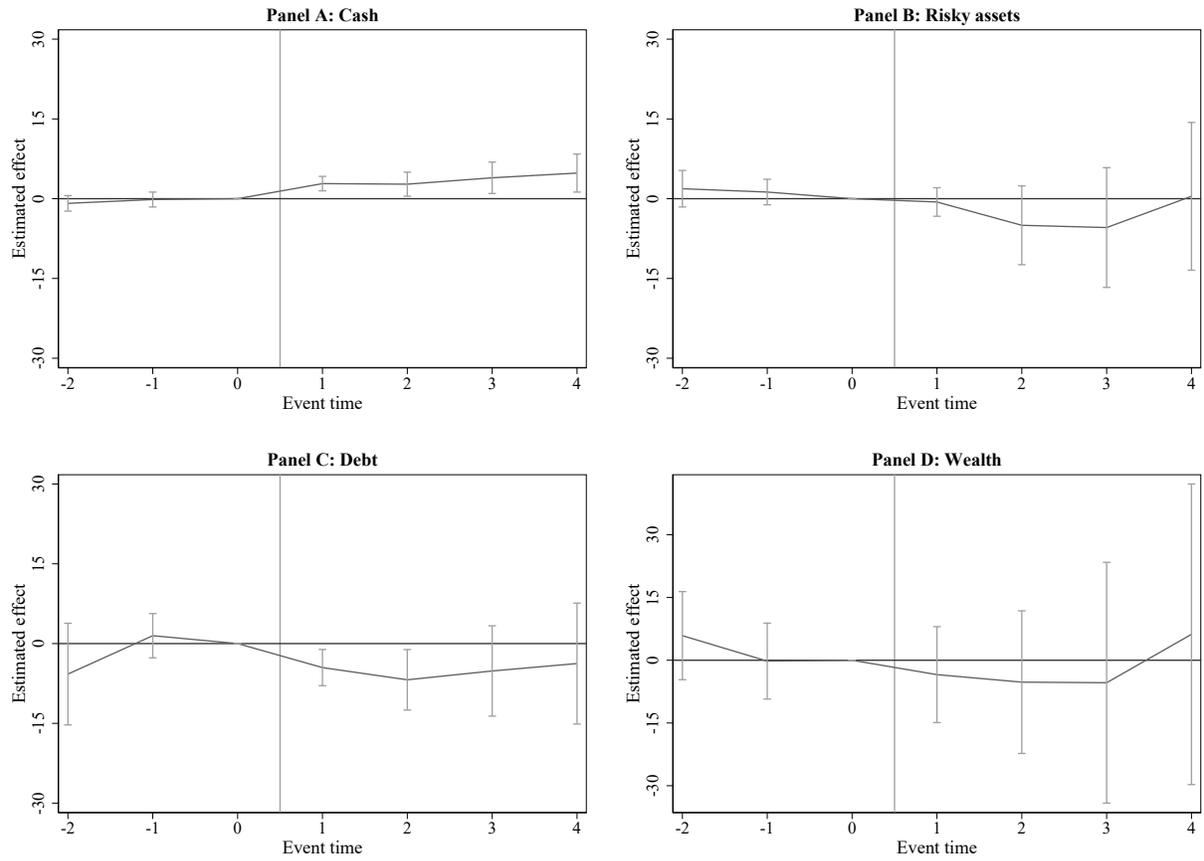
Joacim Tåg

IFN and Hanken School of Economics

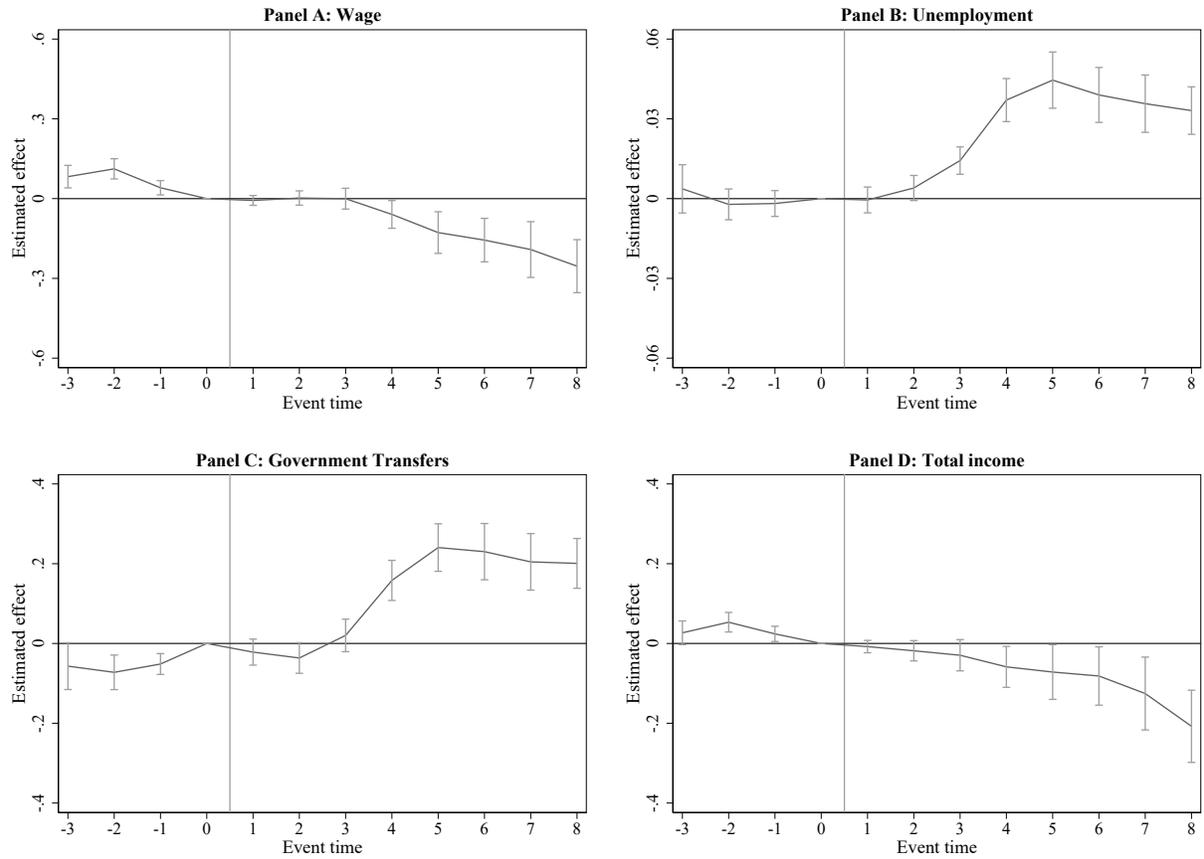
September 2023



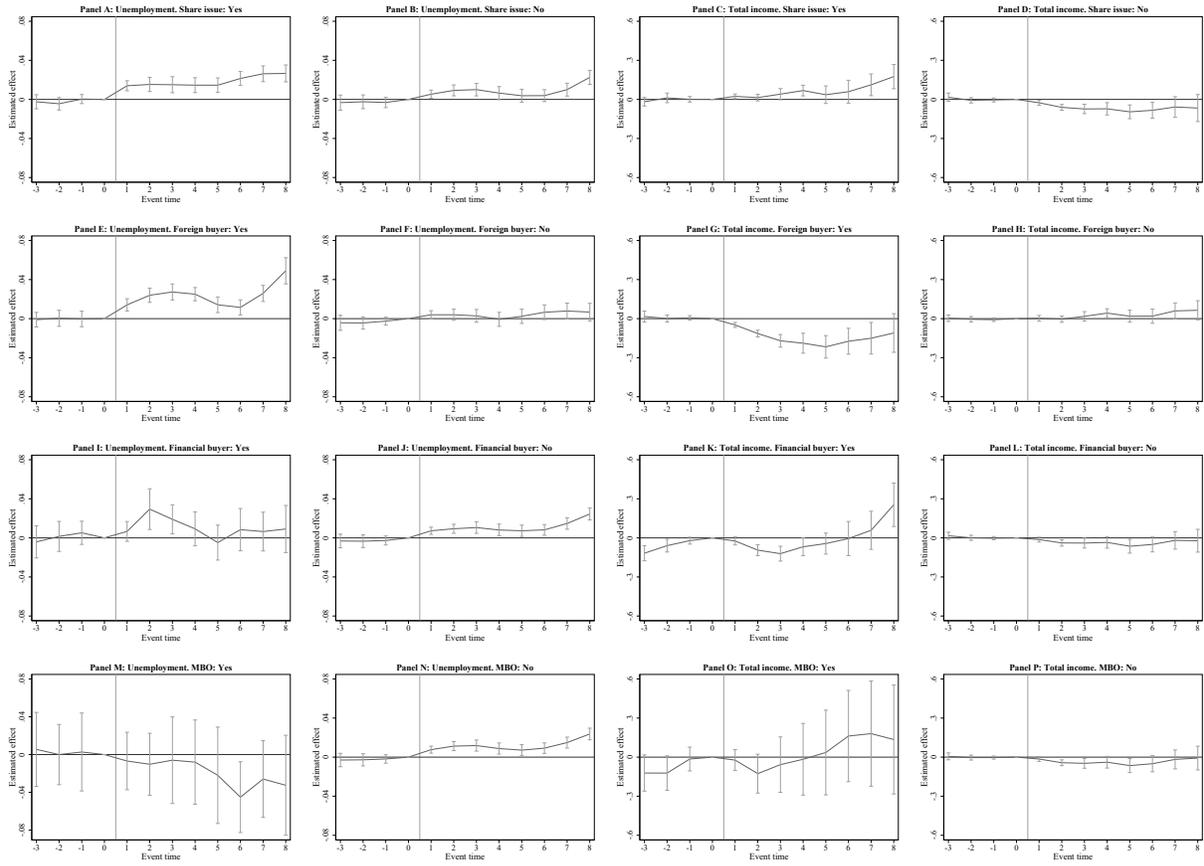
**Figure IA1: Effects of privatization on components of government transfers.** The figures display dynamic differences-in-difference estimates relative to the year prior to the privatization (event time 0) using the model in Equation 2. The vertical bars display 95% confidence intervals using robust standard errors clustered at the municipality level. Variable descriptions are available in Table IA1.



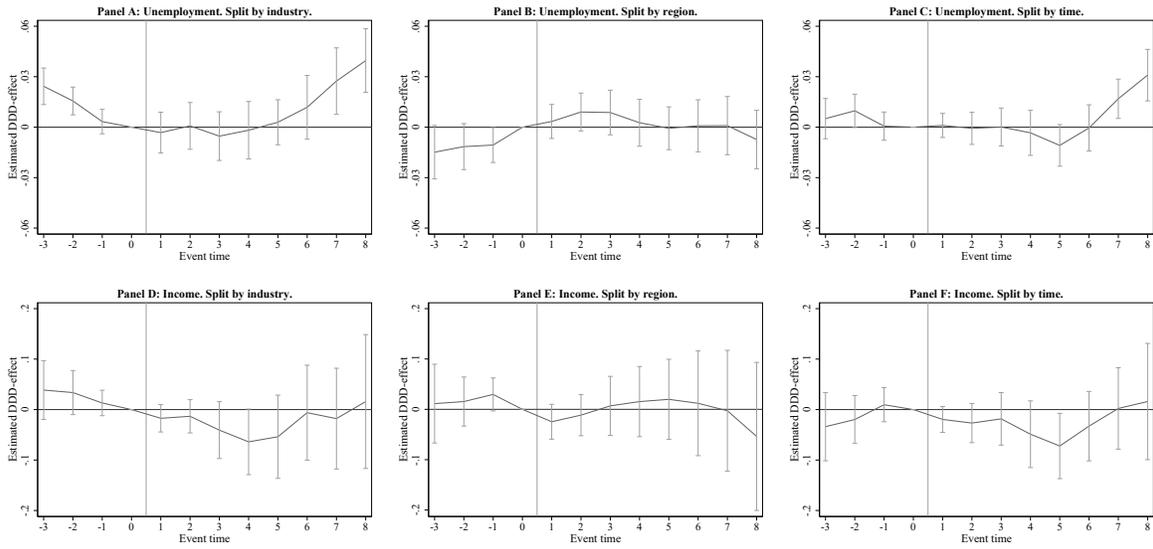
**Figure IA2: Effects of privatization on household finance outcomes in levels.** The figures display dynamic differences-in-difference estimates relative to the year prior to the privatization (event time 0) using the model in Equation 2. The vertical bars display 95% confidence intervals using robust standard errors clustered at the municipality level. Variable descriptions are available in Table IA1.



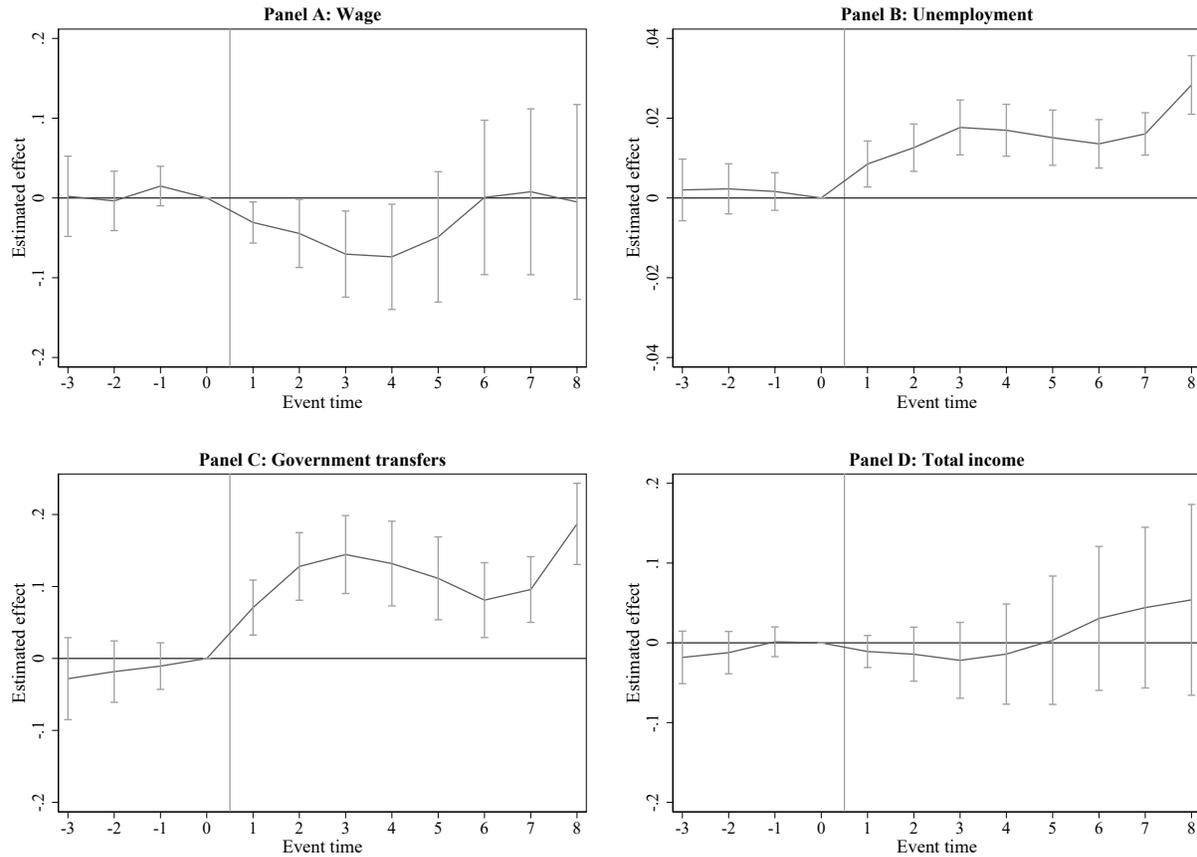
**Figure IA3: Effects on worker outcomes from partial privatization** The figures display dynamic differences-in-difference estimates relative to the year prior to the privatization (event time 0) using the model in Equation 2 using a sample of partial privatisations. The vertical bars display 95% confidence intervals using robust standard errors clustered at the municipality level. Variable descriptions are available in Table IA1.



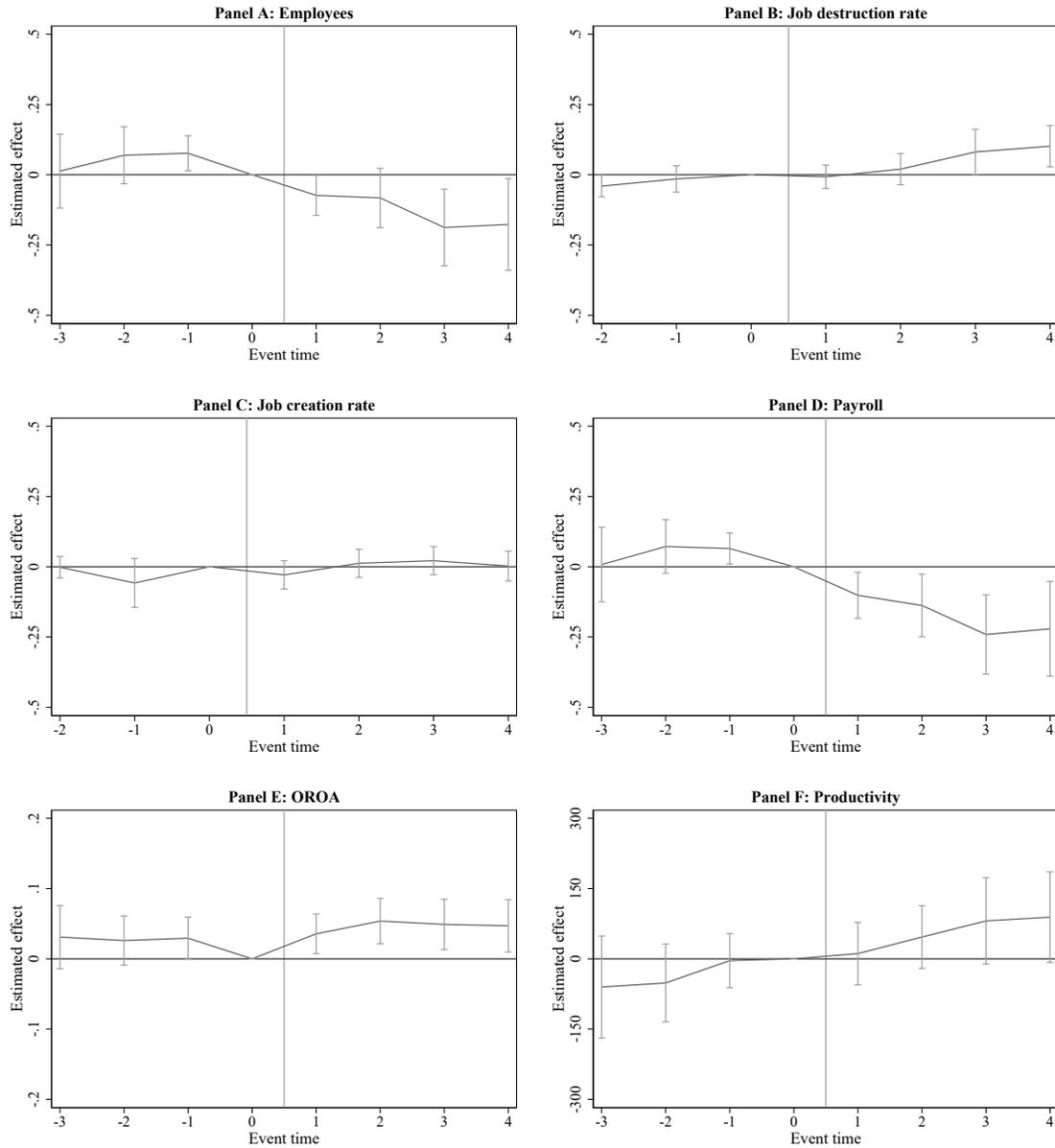
**Figure IA4: Heterogeneity across privatization type** The figures display dynamic differences-in-difference estimates relative to the year prior to the privatization (event time 0) using the model in Equation 2 for different sub-samples of privatization types. The vertical bars display 95% confidence intervals using robust standard errors clustered at the municipality level. Variable descriptions are available in Table IA1 and the sample cuts are described in the notes to Table 7.



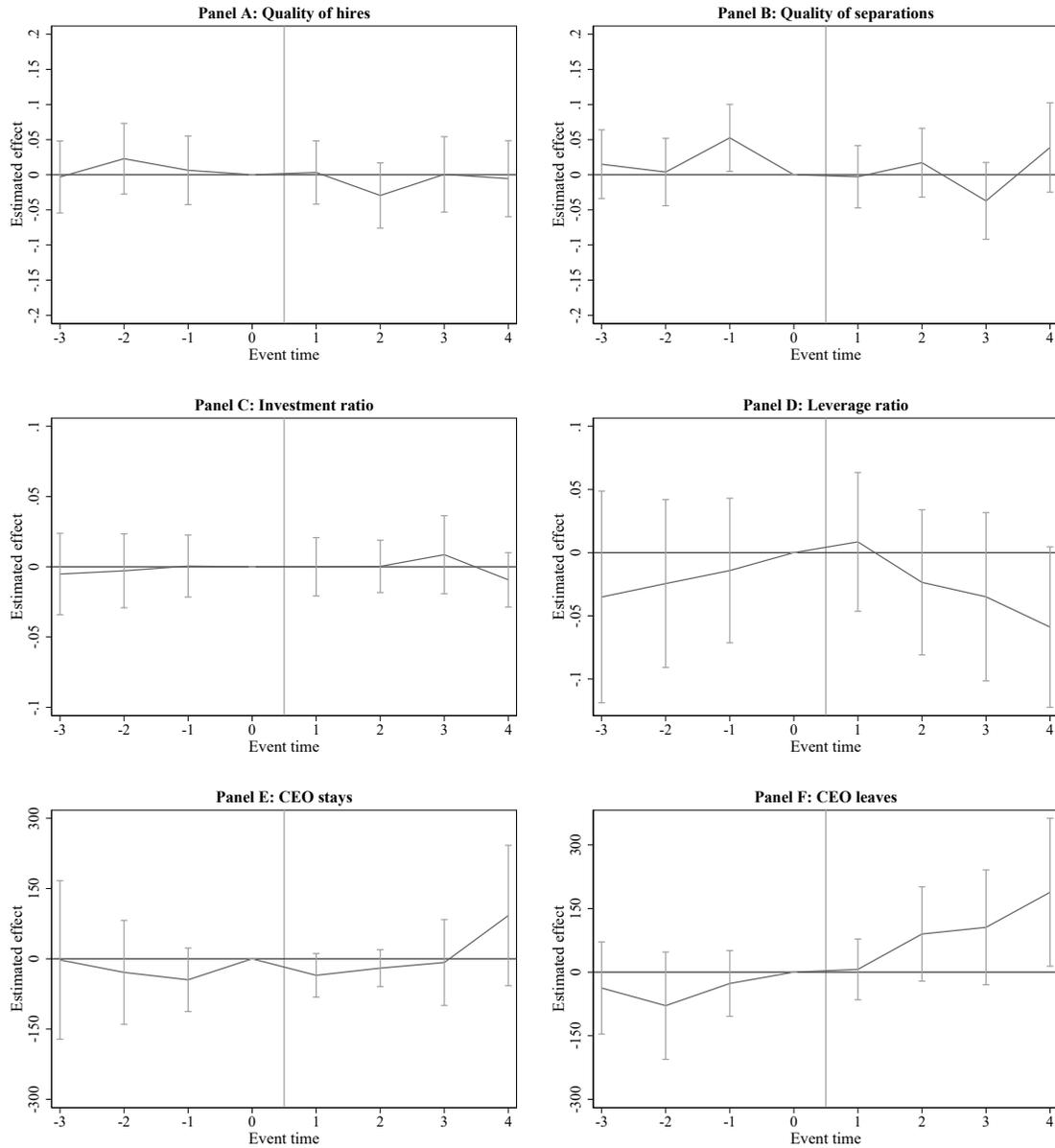
**Figure IA5: Heterogeneity across industry, region and time.** The figures display dynamic triple-difference estimates relative to the year prior to the privatization (event time 0) using the model in Equation 2 extended to incorporate triple difference interactions. The vertical bars display 95% confidence intervals using robust standard errors clustered at the municipality level. Variable descriptions are available in Table IA1 and the sample cuts are described in the notes to Table 8.



**Figure IA6: Effects of privatization on worker outcomes using propensity score matching** The figures display dynamic differences-in-difference estimates relative to the year prior to the privatization (event time 0) using the model in Equation 2 and using propensity score matching to construct the control group. The matching is done at the firm level in several steps. First, for each year  $t$ , we run a probit model where the dependent variable is a dummy taking the value one if an SOE is privatized at year  $t + 1$ , zero otherwise, controlling for industry (10 classes), firm age, size, and the mean earnings, age, and educational level among workers. Second, we use the predicted propensity score to match treated and control firms within a year and broad industries (4 classes) using one-to-one nearest neighbor matching. Third, we identify all workers in the treated and matched control firms in year  $t$ . Fourth, we track the workers three years before year  $t$  and eight years after. The vertical bars display 95% confidence intervals using robust standard errors clustered at the municipality level. Variable descriptions are available in Table IA1.



**Figure IA7: Effects on firm outcomes using propensity score matching** The figures display dynamic differences-in-difference estimates relative to the year prior to the privatization (event time 0) using the model in Equation 3 adjusted to account for dynamic effects. The matching is done at the firm level and in several steps. First, for each year  $t$ , we run a probit model where the dependent variable is a dummy taking the value one if an SOE is privatized at year  $t + 1$ , zero otherwise, controlling for industry (10 classes), firm age, size, and the mean earnings, age, and educational level among workers. Second, we use the predicted propensity score to match treated and control firms within year and broad industries (4 classes) using one-to-one nearest neighbor matching. Third, track the firms three years before year  $t$  and four years after. The vertical bars display 95% confidence intervals using robust standard errors clustered at the firm level. Variable descriptions are available in Table IA1.



**Figure IA8: Effects on additional firm outcomes using propensity score matching** The figures display dynamic differences-in-difference estimates relative to the year prior to the privatization (event time 0) using the model in Equation 3 adjusted to account for dynamic effects. The matching is done at the firm level and in several steps. First, for each year  $t$ , we run a probit model where the dependent variable is a dummy taking the value one if an SOE is privatized at year  $t + 1$ , zero otherwise, controlling for industry (10 classes), firm age, size, and the mean earnings, age, and educational level among workers. Second, we use the predicted propensity score to match treated and control firms within year and broad industries (4 classes) using one-to-one nearest neighbor matching. Third, track the firms three years before year  $t$  and four years after. The vertical bars display 95% confidence intervals using robust standard errors clustered at the firm level. Variable descriptions are available in Table IA1.

**Table IA1: Detailed variable descriptions**

The table displays descriptions of the variables that we use. Unless separately specified, the variables are sourced from the LISA database at Statistics Sweden. This database gathers information from various registers at Statistics Sweden. 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
<b>Panel A: Individual level variables</b>	
Age	Calculated using birth year information from the population registration.
Business owner	Indicator variable created by Statistics Sweden ( <i>yrkstalln=5</i> ). It is based on the occupational status of the main source of labor income during a given year. Business owners refer to owners of Limited Liability Corporations ( <i>aktiebolag</i> ).
Debt ratio	Defined as debt divided by total wealth and is provided by the Swedish Wealth Register. When the debt is equal of higher than the total wealth, the debt ratio is defined as one.
Divorce	A person is defined as divorced if switching the civil status from married to single between two consecutive years.
Education	Information on the highest completed education level comes from the Education Register at Statistics Sweden ( <i>Utbildningsregistret</i> ).
Gender	Based on information from the population registration.
Immigrant	Indicator variable for if the individual has immigrated to Sweden.
Income concepts	Wage income is gross labor income, capital income is gross capital income, total income is gross wage income plus government transfers. Original source for wage and capital income is the Swedish Tax Office records. The data covers all payments above 100 SEK (about 10 USD).
Labor market experience	Based on a person's age or year of academic degree. If the highest educational level is primary 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 is less than two years, age minus 20. If post-secondary education two years or longer, age minus 23. If higher educational level, as age minus year of academic degree.
Mortality	Information from the RTB register ( <i>Mikrodata för Registret över totalbefolkningen</i> ).
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.
Risky share	Calculated as risky assets divided by total assets. Total assets are defined as risky assets plus cash. Risky assets are defined as financial assets minus cash. Data comes from the Swedish Wealth register ( <i>Förmögenhetsregistret</i> ) available from 1997 to 2007.
Self-employed	Indicator variable created by Statistics Sweden ( <i>yrkstalln=4</i> ). Is based on the occupational status from the main source of labor income and refers to the self-employed ( <i>enskild firma</i> )
Stock market participation	Information from the Swedish Wealth register ( <i>Förmögenhetsregistret</i> ).
Tenure	Calculated based on observing worker-firm links between the years 1990 and 2017. A worker can thus have a maximum tenure of 27 years.
Transfers	The sum of unemployment benefits, activity support and social benefits. Unemployment benefits is the annual amount of benefit caused by unemployment. Activity support is given to persons who participates in an active labor market program provided by the Swedish Public Employment Service ( <i>Arbetsförmedlingen</i> ) and is measured as the annual amount of benefits. Social benefits is provided by the National Board of Social Affairs and Health and is measured at the family level.
Unemployment	The yearly number of days in unemployment based on registry data from the Swedish Public Employment Service ( <i>Arbetsförmedlingen</i> ). Registering at the Swedish Public Employment Service is mandatory for receiving unemployment benefits. An individual is designated as unemployed 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 of 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**

Age	Calculated as the number of years since the firm identifier emerged in the data for the first time. Because the LISA database at Statistics Sweden starts in 1990, age of a company is censored.
Domestic outsourcing industries	Refers to the fraction of firms started in the logistics, cleaning, security, and food service industries, with the following codes according to the Swedish Standard Industrial Classification (SNI): 5551, 5552, 6024, 6311, 6312, 6321, 6322, 6323, 6340, 7450, 7460, 7470, and 9003.
Employees	Total number of workers in the firm as of November each year (when Statistics Sweden defines the employer-employee link).
Financial buyer	When the new owner of the privatized firm has an industry code corresponding to "Investment trust activities," "Security brokering and fund management," "Activities auxiliary to financial intermediation," or "Activities of investment companies and venture capital companies"
Industry and location	Information on the industry and geographical location of the firm comes from Statistics Sweden, which assigns identifiers, industry, and location codes to physical places of work (the underlying databases at Statistics Sweden are the RAMS and the <i>Företagsdatabasen</i> ). The industry classification underwent a minor change in 2002 and 2007 but changes were minor at the 10-category classification level we use.
Investment ratio	Capital expenditures divided by total assets.
Job creation rate	Based on Davis et al. (1998). See Equation (6) for the definition.
Job destruction rate	Based on Davis et al. (1998). See Equation (5) for the definition.
Leverage	Total long-term and short-term debt divided by total assets (all information from the financial statements of the firm). The variable has been winsorized so that values above ten are set to ten.
Management buyouts (MBO)	Privatizations in which at least one of the managers and or employees at $t - 1$ is the new owner.
OROA	Operating return on assets are defined as profit before taxes divided by total assets. The variable has been winsorized so that values above one are set to one, and values below negative one are set to negative one.
Partial privatization	SOEs that are listed on a Swedish stock exchange while still remaining partially controlled by the state, that is, less than 50% are owned by private investors. We obtain information on listing status from Statistics Sweden's FRIDA and FinBas databases. If the listed firm is part of a corporate group, we define all subsidiaries of the listed firm as listed.
Payroll	Total payroll of the firm as reported in the financial statements.
Productivity	Value added divided by number of employees, winsorized at the 95th/5th percentile.
Quality of hires	The mean talent of hired persons relative to the mean talent of the firm's incumbent workforce, where talent is defined as the sum of cognitive and non-cognitive ability measures at military enlistment for males. Data from Swedish War Archive, see Lindqvist and Vestman (2011) for details.
Quality of separated	The mean talent of persons that leave the firm relative to the mean talent of the firm's workforce that does not leave in a given year, where talent is defined as the sum of cognitive and non-cognitive ability measures at military enlistment for males. Data from Swedish War Archive, see Lindqvist and Vestman (2011) for details.
Share issue privatization	When the privatized firm (or its new owner) is listed on one of the Swedish stock markets.

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**Table IA2: Selection into privatization**

The table reports selected coefficients from a selection regression explaining the probability of an SOE being privatized the next year (t-statistic in parenthesis). The sample consists of a pooled cross-section of all SOEs. Variable descriptions are available in Table IA1.

Dependent variable Specification	Privatized 1
Employment	-0.0436 (-7.44)
Payroll	0.0229 (4.67)
Productivity	-0.0000 (-12.66)
OROA	0.0217 (1.73)
Investment Ratio	-0.0013 (-1.37)
Leverage	-0.0077 (-1.15)
Year FE	Yes
Industry FE	Yes
Adjusted $R^2$	0.064
Number of observations	18312

**Table IA3: Components of government transfers**

The table reports difference-in-differences estimates from regressions explaining worker outcomes after privatization using the models in Equation 1 and 2 (t-statistics in parenthesis). The sample consists of treated workers employed one year prior to the privatization and matched control workers. All models control for the number of employees and fixed effects for age, gender, immigrant status, labor market experience, tenure, education, municipality, industry, calendar year, and the privatization year. All controls, except calendar and the privatization year, are measured one year before the privatization. The standard errors are clustered at the municipality level. The %-change uses the mean for workers in privatized firms in the years before the privatization as the baseline. *Unem. benefits* is the log of annual gross unemployment benefit payments, *Activity support* is the log of annual gross activity support benefits, and *Social benefits* is the log of annual gross social benefit payments. When taking log, we use the inverse hyperbolic sine transformation. Variable descriptions are available in Table IA1.

Dependent variable Specification	Unem. benefits 1	Activity support 2	Social benefits 3
<b>Panel A: Average effect</b>			
Full period	0.111 (7.45)	0.043 (6.69)	-0.002 (-0.23)
%-change	11.1%	24.8%	-0.2%
Adjusted $R^2$	0.055	0.018	0.022
<b>Panel B: Dynamic effect</b>			
Short run (1-2 years)	0.092 (5.14)	0.034 (4.74)	0.001 (0.07)
Medium run (3-4 years)	0.123 (7.61)	0.028 (3.59)	-0.001 (-0.15)
Long run (5-8 years)	0.116 (6.66)	0.056 (7.57)	-0.004 (-0.46)
%-change			
Short run	9.2%	19.5%	0.1%
Medium run	12.3%	16.1%	-0.1%
Long run	11.6%	32.6%	-0.4%
Adjusted $R^2$	0.055	0.018	0.022
Mean dependent variable	0.498	0.173	0.207
Number of observations	1414270	1414270	1414270

**Table IA4: Comparison of treated and control entrepreneurs**

The table compares treated (column 1) and control (column 2) entrepreneurs over the full eight-year post-period. Column 3 reports differences in means, and column 4 t-values on the difference. Firm size bins are based on the EU classification of micro, small, medium, and large firms. Variable descriptions are available in Table IA1. *Total income* refers to wage and capital income, as entrepreneurs in Sweden can choose to earn income from their businesses either as wage or capital income. Capital income is negative when an individual reports a tax-deductible capital loss. *Domestic outsourcing industries* refers to fraction firms started in the logistics, cleaning, security, and food service industries (SNI-codes: 5551, 5552, 6024, 6311, 6312, 6321, 6322, 6323, 6340, 7450, 7460, 7470, and 9003).

	Treated 1	Control 2	Difference 3	T-value 4
<b>Panel A: Entrepreneurs</b>				
Business Owner	31%	25%	6%	4.36
Post income (TSEK)				
Wage income	136.99	112.72	24.27	4.70
Capital income	33.71	29.61	4.10	0.84
Wage + capital income	170.70	142.34	28.37	3.64
Pre income (TSEK)				
Wage income	303.29	277.44	25.84	3.72
Capital income	-2.63	-1.14	-1.49	-0.40
Total income	300.66	276.31	24.35	3.18
Entrepreneur-year observations	9866	8819	1047	
Unique entrepreneurs	3477	3141	336	
<b>Panel B: Firms</b>				
Employment				
Number of employees	6.17	6.19	-0.01	-0.00
1-10 employees	95%	96%	-2%	-3.01
10-50 employees	5%	3%	2%	3.06
50-250 employees	0%	0%	0%	2.26
250+ employees	0%	0%	0%	-1.25
Productivity (TSEK)	342.05	312.30	29.75	2.11
In "domestic outsourcing" industries	4%	4%	0%	0.26
Firm-year observations	9400	8686	714	
Unique firms	3630	3326	304	

**Table IA5: Components of household finance outcomes**

The table reports difference-in-differences estimates from regressions explaining worker outcomes after privatization using the models in Equation 1 and 2 (t-statistics in parenthesis). The sample consists of treated workers employed one year prior to the privatization and matched control workers. All models control for the number of employees and fixed effects for age, gender, immigrant status, labor market experience, tenure, education, municipality, industry, calendar year, and the privatization year. All controls, except calendar and the privatization year, are measured one year before the privatization. The standard errors are clustered at the municipality level. The %-change uses the mean for workers in privatized firms in the years before the privatization as the baseline. Variable descriptions are available in Table IA1.

Dependent variable Specification	Risky assets 1	Cash 2	Debt 3	Wealth 4
<b>Panel A: Average effect</b>				
Full period	-3.705 (-0.90)	3.912 (3.14)	-3.655 (-0.81)	-3.898 (-0.33)
%-change	-4.2%	9.8%	-1.6%	-0.7%
Adjusted $R^2$	0.016	0.033	0.059	0.067
<b>Panel B: Dynamic effect</b>				
Short run (1-2 years)	-3.847 (-1.76)	3.126 (3.20)	-4.251 (-1.18)	-6.223 (-0.88)
Medium run (3-4 years)	-3.559 (-0.57)	4.711 (2.73)	-3.049 (-0.52)	-1.534 (-0.09)
%-change				
Short run	-4.3%	7.8%	-1.9%	-1.1%
Medium run	-4.0%	11.8%	-1.4%	-0.3%
Adjusted $R^2$	0.016	0.033	0.059	0.067
Mean dependent variable	89.120	39.892	225.207	561.555
Number of observations	342554	342554	342554	342554

**Table IA6: Additional productivity results**

The table reports difference-in-differences estimates from quantile regressions explaining productivity after privatization at different parts of the productivity distribution (t-statistics in parenthesis). The sample consists of treated firms and matched control firms, includes information for three years before and four years after the privatization, and covers 1997 to 2017. All models include fixed effects for the privatization year, calendar year, industry, region, and firms' age one year before the privatization. All controls, except calendar and privatization year, are measured one year before the privatization. The %-change uses the mean for privatized firms over the years before the privatization as the baseline. *Productivity* is the value added per employee in thousands of SEK. Standard errors are clustered at the firm level.

Part of the distribution Specification	Mean 1	p25 2	p50 3	p75 4	p90 5	Log 6
Full period	109.755 (2.772)	1.263 (0.125)	25.124 (1.538)	69.513 (2.523)	206.037 (2.267)	0.115 (2.030)
%-change	31.9%	1.8%	12.2%	16.2%	27.4%	11.5%
Adjusted $R^2$	0.089	0.159	0.182	0.153	0.091	0.190
Mean dependent variable	341.666	67.402	206.569	428.374	752.079	5.185
Number of observations	4,804	4,804	4,804	4,804	4,804	4,536

**Table IA7: Firm level outcomes by CEO turnover**

The table reports difference-in-differences estimates from regressions explaining worker outcomes after privatization using the models in Equation 3 adjusted to account for dynamic effects (t-statistics in parenthesis). The sample consists of treated firms and matched control firms, includes information for three years before and four years after the privatization, and covers 1997 to 2017. All models include fixed effects for the privatization year, calendar year, industry, region, and firms' age one year before the privatization. All controls, except calendar and privatization year, are measured one year before the privatization. The %change uses the mean for privatized firms over the years before the privatization as the baseline. *Employees* is the logarithm of the number of employees, *Payroll* is the logarithm of the sum of the annual wage income of workers employed in the firm in hundreds of SEK, *OROA* is the operating return on assets, and *Productivity* is the value added per employee in thousands of SEK. *Quality Hir* is the mean talent of hired persons relative to the mean talent of the incumbent workforce, where talent is defined as the sum of cognitive and non-cognitive ability measures at military enlistment for males. *Quality Sep* is the mean talent of persons leaving the firm relative to the mean talent of the incumbent workforce. *Investment ratio* is the capital expenditure divided by total assets. *Leverage* is the sum of short- and long-term liabilities divided by total assets.

Dependent variable Specification	1 Employees	2 Payroll	3 OROA	4 Productivity	5 Quality Hir	6 Quality Sep	7 Investment ratio	8 Leverage
<b>Panel A: Overall</b>								
Full period	-0.200 (-3.152)	-0.166 (-2.607)	0.022 (1.380)	68.773 (1.694)	-0.007 (-0.349)	0.019 (0.885)	-0.007 (-0.868)	-0.023 (-1.367)
%-change	-20%	-16.6%	25.0%	22.0%	0.7%	1.9%	-11.1%	-3.7%
Adjusted $R^2$	0.234	0.250	0.088	0.085	0.002	0.022	0.083	0.139
Mean dependent variable	3,589	11,514	-0.002	312.667	1.012	1.009	0.063	0.625
Number of observations	3,129	3,129	3,129	3,129	1,877	1,748	3,129	3,129
<b>Panel B: No CEO turnover</b>								
Full period	-0.123 (-1.811)	-0.068 (-0.973)	-0.003 (-0.209)	40.009 (0.806)	-0.006 (-0.239)	0.027 (0.991)	-0.002 (-0.304)	-0.013 (-0.745)
%-change	-12.3%	-6.8%	-11.5%	10.7%	-0.6%	2.7%	3.2%	2.1%
Adjusted $R^2$	0.238	0.240	0.091	0.116	-0.007	0.015	0.084	0.175
Mean dependent variable	11,198	0,026	375,249	1,005	1,008	0,062	0,629	0,629
Number of observations	2,433	2,433	2,433	2,433	1,402	1,274	2,433	2,433
<b>Panel C: CEO turnover</b>								
Full period	-0.258 (-2.659)	-0.243 (-2.483)	0.047 (1.927)	98.024 (1.998)	-0.002 (-0.076)	0.011 (0.459)	-0.011 (-1.044)	-0.036 (-1.418)
%-change	-25.8%	-24.3%	234.3%	41.4%	-0.2%	1.1%	-17.2%	-5.8%
Adjusted $R^2$	0.221	0.245	0.113	0.077	0.007	0.040	0.079	0.163
Mean dependent variable	3,943	11,898	-0.035	236.663	1.019	1.010	0.064	0.620
Number of observations	2,278	2,278	2,278	2,278	1,413	1,316	2,278	2,278

**Table IA8: Matching three years prior to the privatization**

The table reports difference-in-differences estimates from regressions explaining worker outcomes after privatization using the models in Equation 1 and 2 (t-statistics in parenthesis). The sample consists of treated workers employed three years prior to the privatization and matched control workers. All models control for the number of employees and fixed effects for age, gender, immigrant status, labor market experience, tenure, education, municipality, industry, calendar year and the privatization year. All controls, except calendar and the privatization year, are measured one year before the privatization. The standard errors are clustered at the municipality level. The %-change uses the mean for workers in privatized firms in the years before the privatization as the baseline. Variable descriptions are available in Table IA1.

Dependent variable Specification	Wage 1	Unemployment 2	Transfers 3	Total income 4
<b>Panel A: Average effect</b>				
Full period	-0.095 (-4.30)	0.009 (3.14)	0.082 (4.59)	-0.057 (-2.73)
%-change	-9.5%	7.9%	8.2%	-5.7%
Adjusted $R^2$	0.128	0.079	0.069	0.136
<b>Panel B: Dynamic effect</b>				
Short run (1-2 years)	-0.034 (-2.13)	0.000 (0.09)	0.000 (-0.01)	-0.021 (-1.38)
Medium run (3-4 years)	-0.079 (-3.89)	0.013 (4.36)	0.094 (4.26)	-0.042 (-2.09)
Long run (5-8 years)	-0.144 (-4.26)	0.013 (3.36)	0.126 (5.93)	-0.089 (-2.84)
%-change				
Short run	-3.4%	0.2%	0.0%	-2.1%
Medium run	-7.9%	11.1%	9.4%	-4.2%
Long run	-14.4%	10.7%	12.6%	-8.9%
Adjusted $R^2$	0.129	0.079	0.069	0.137
Mean dependent variable	7.957	0.118	0.804	8.109
Number of observations	633935	633935	633935	633935

**Table IA9: Wage, unemployment and government transfers with propensity score matching**

The table reports difference-in-differences estimates from regressions explaining worker outcomes after privatization using the models in Equation 1 and 2 (t-statistics in parenthesis). The sample consists of treated workers employed one year prior to the privatization and propensity-scored matched control workers. The matching is done at the firm level and in several steps. First, for each year  $t$ , we run a probit model where the dependent variable is a dummy taking the value one if an SOE is privatized at year  $t + 1$ , zero otherwise, controlling for industry (10 classes), firm age, size, and the mean earnings, age, and educational level among workers. Second, we use the predicted propensity score to match treated and control firms within a year and broad industries (4 classes) using one-to-one nearest-neighbor matching. Third, we identify all workers in the treated and matched control firms in year  $t$ . Forth, we track the workers three years before year  $t$  and eight years after. All models control for the number of employees and fixed effects for age, gender, immigrant status, labor market experience, tenure, education, municipality, industry, calendar year, and the privatization year. All controls, except calendar and the privatization year, are measured one year before the privatization. The standard errors are clustered at the municipality level. The %-change uses the mean for workers in privatized firms in the years before the privatization as the baseline. Variable descriptions are available in Table IA1.

Dependent variable Specification	Wage 1	Unemployment 2	Transfers 3	Total income 4
<b>Panel A: Average effect</b>				
Full period	-0.037 (-0.84)	0.014 (4.61)	0.132 (5.21)	0.015 (0.38)
%-change	-3.7%	14.2%	13.2%	1.5%
Adjusted $R^2$	0.188	0.069	0.062	0.215
<b>Panel B: Dynamic effect</b>				
Short run (1-2 years)	-0.032 (-1.30)	0.009 (2.46)	0.111 (3.95)	0.001 (0.06)
Medium run (3-4 years)	-0.076 (-2.05)	0.016 (4.60)	0.153 (5.38)	-0.012 (-0.35)
Long run (5-8 years)	-0.012 (-0.21)	0.017 (5.14)	0.131 (4.98)	0.042 (0.76)
%-change				
Short run	-3.2%	8.8%	11.1%	0.1%
Medium run	-7.6%	15.8%	15.3%	-1.2%
Long run	-1.2%	16.4%	13.1%	4.2%
Adjusted $R^2$	0.190	0.070	0.062	0.216
Mean dependent variable	7.871	0.101	0.731	8.019
Number of observations	1458808	1458808	1458808	1458808

**Table IA10: Firm level outcomes with propensity score matching**

The table reports difference-in-differences estimates from regressions explaining firm outcomes after privatization using the model in Equation 3 adjusted to account for dynamic effects (t-statistics in parenthesis). The sample consists of treated firms and propensity score matched control firms, includes information for three years before and four years after the privatization, and covers 1997 to 2017. The matching is done at the firm level and in several steps. First, for each year  $t$ , we run a probit model where the dependent variable is a dummy taking the value one if an SOE is privatized at year  $t + 1$ , zero otherwise, controlling for industry (10 classes), firm age, size, and the mean earnings, age, and educational level among workers. Second, we use the predicted propensity score to match treated and control firms within a year and broad industries (4 classes) using one-to-one nearest-neighbor matching. Third, track the firms three years before year  $t$  and four years after. All models include fixed effects for the privatization year, calendar year, industry, region, and firms' age one year before the privatization. All controls, except calendar and the privatization year, are measured one year before the privatization. The %-change uses the mean for privatized firms over the years before the privatization as the baseline. Variable descriptions are available in Table IA1.

Dependent variable Specification	Employment 1	Job Destruction 2	Job Creation 3	Payroll 4	OROA 5	Productivity 6
<b>Panel A: Average effect</b>						
Full period	-0.162 (-3.00)	0.069 (3.11)	0.056 (2.39)	-0.203 (-3.78)	0.026 (2.22)	74.616 (2.08)
%-change	-16.2%	6.9%	5.6%	-1.8%	-1341.6%	25.8%
Adjusted $R^2$	0.137	0.292	0.183	0.184	0.083	0.075
<b>Panel B: Dynamic effect</b>						
Short run (1-2 years)	-0.118 (-2.45)	0.063 (2.35)	0.025 (2.04)	-0.155 (-3.19)	0.025 (2.04)	50.725 (1.55)
Medium run (3-4 years)	-0.223 (-2.90)	0.077 (2.51)	0.029 (1.94)	-0.268 (-3.46)	0.029 (1.94)	107.578 (2.19)
%-change						
Short run	-11.8%	6.3%	5.6%	-15.5%	-1257.7%	17.5%
Medium run	-22.3%	7.7%	5.9%	-26.8%	-1454.2%	37.2%
Adjusted $R^2$	0.137	0.292	0.192	0.184	0.083	0.076
Mean dependent variable	0.137	0.089	0.243	11.448	-0.002	289.545
Number of observations	6992	6514	6514	6992	5663	5623

**Table IA11: Additional firm level outcomes with propensity score matching**

The table reports difference-in-differences estimates from regressions explaining firm outcomes after privatization using the model in Equation 3 adjusted to account for dynamic effects (t-statistics in parenthesis). The sample consists of treated firms and propensity score matched control firms, includes information for three years before and four years after the privatization, and covers 1997 to 2017. The matching is done at the firm level and in several steps. First, for each year  $t$ , we run a probit model where the dependent variable is a dummy taking the value one if an SOE is privatized at year  $t + 1$ , zero otherwise, controlling for industry (10 classes), firm age, size, and the mean earnings, age, and educational level among workers. Second, we use the predicted propensity score to match treated and control firms within a year and broad industries (4 classes) using one-to-one nearest neighbor matching. Third, track the firms three years before year  $t$  and four years after. All models include fixed effects for the privatization year, calendar year, industry, region, and firms' age one year before the privatization. All controls, except calendar and the privatization year, are measured one year before the privatization. The %-change uses the mean for privatized firms over the years before the privatization as the baseline. Variable descriptions are available in Table IA1.

Dependent variable Specification	Quality Hir 1	Quality Sep 2	Investment ratio 3	Leverage 4	CEO Remains 5	CEO Replaced 6
<b>Panel A: Average effect</b>						
Full period	-0.013 (-0.96)	-0.015 (-1.06)	0.001 (0.20)	-0.008 (-0.44)	21.180 (0.49)	113.455 (2.35)
%-change	-1.3%	-1.5%	2.0%	-1.2%	7.4%	39.7%
Adjusted $R^2$	0.011	0.014	0.049	0.249	0.144	0.124
<b>Panel B: Dynamic effect</b>						
Short run (1-2 years)	-0.018 (-1.16)	-0.012 (-0.78)	0.002 (0.21)	0.009 (0.50)	-8.801 (-0.25)	78.489 (1.84)
Medium run (3-4 years)	-0.009 (-0.49)	-0.019 (-0.95)	0.002 (0.16)	-0.031 (-1.36)	59.966 (0.97)	178.092 (2.38)
%-change						
Short run	-1.8%	-1.1%	2.2%	1.3%	-3.1%	27.5%
Medium run	-0.9%	-1.9%	2.1%	-4.6%	21.0%	62.4%
Adjusted $R^2$	0.011	0.014	0.049	0.250	0.145	0.126
Mean dependent variable	1.017	1.021	0.072	0.665	285.551	285.551
Number of observations	4384	4270	5622	5663	2595	2479

**Table IA12: Firm attrition and selection on observables**

The table reports selected coefficients from the following difference-in-differences model:  $Y_f = \alpha + \gamma D_f + \omega \text{Leavingpanel}_f + \beta \text{Leavingpanel}_f \times D_f + X_f + \varepsilon_f$ , that explains whether there are systematic pre-privatization differences between treated and control firms that leave and stay in the firm panel (t-statistics in parenthesis). The variable  $\text{Leavingpanel}_f$  takes the value one if firm  $f$  leaves the panel before the maximum number of years it can stay in the panel, zero otherwise.  $D_f$  takes the value one for all privatized firms, zero otherwise. The sample consists of treated and matched control firms in year  $t - 0$ . All models include fixed effects for the privatization year, industry, region, and firms' age one year before the privatization. Consequently, the coefficient  $\gamma$  represents the conditional mean difference in variable  $y$  in year  $t - 0$  between treated and control firms that are not leaving the panel,  $\omega$  represents the conditional mean difference in variable  $y$  between control firms that leave the panel and control firms that stay in the panel, and  $\beta$  represents the conditional mean difference in variable  $y$  between treated and control firms that leave the panel relative to the mean difference between treated and controls that do not leave the panel. The dependent variable  $\text{Employees}$  is the number of employees,  $\text{Payroll}$  is the sum of the annual wage income of workers employed in the firm in hundreds of SEK,  $\text{OROA}$  is operating return on assets, and  $\text{Productivity}$  is the value added per employee in thousands of SEK.

Dependent variable Specification	Employees 1	Payroll 2	OROA 3	Productivity 4
Treated	0.057 (0.531)	0.110 (0.991)	-0.004 (0.267)	-63.424 (2.475)
Leaving panel	-0.096 (0.419)	-0.052 (0.217)	0.008 (0.190)	-0.287 (0.005)
Treated $\times$ Leaving panel	-0.133 (0.502)	-0.173 (0.635)	-0.069 (1.324)	53.189 (0.775)
Adjusted $R^2$	0.125	0.172	0.049	0.101
Number of observations	736	736	736	736

**Table IA13: Comparison of treated and control workers who are separated**

The table displays mean characteristics of treated and control workers who, during the eight-year post-period are separated from the firm they were employed in at time  $t - 0$ . *Old age* is the share of the separated workers who were above 40 years of age, *Low skilled* is the share of the separated workers who had at most upper secondary education, *Low skilled* is the share of the separated workers who had less than two years of tenure, and *Female* is share of the separated workers who were females. A worker is defined as separated from the firm they were employed in at time  $t - 0$  if the firm identifier changes while the firm is still in business. A normalized t-test above 0.25 indicates substantial differences in means (Imbens and Wooldridge, 2009).

	Treated	Control	Difference	Norm. T-value
	1	2	3	4
<b>Treated and control (worker-level analysis)</b>				
Old age	47.4%	46.34%	1.1%	0.02
Low skilled	63.8%	61.9%	1.9%	0.03
Non-tenured	86.2%	84.3%	1.9%	0.04
Female	38.2%	38.3%	-0.2%	-0.00
Observations	40429	35362		

**Table IA14: Estimates of costs and benefits per worker**

	Estimate 1	Source 2
Annual costs of privatization per worker (SEK)		
Labor income losses (wage)	18,719 SEK	Table 2 column 1 and the pre-mean in Table 1.
Labor income losses after transfers (total income)	9,271 SEK	Table 2 column 4 and the pre-mean in Table 1.
Annual productivity gains in firms per worker (SEK)		
Upper bound	109,755 SEK	Table 4 column 6.
Lower bound	35,362 SEK	Table IA6 column 6.
Ratios based on above estimates		
Fraction of labor income losses covered by transfers	50%	
Productivity gains as multiple of labor income losses		
Upper bound	5.9	
Lower bound	1.9	
Transfers as fraction of productivity gains		
Upper bound	9%	
Lower bound	27%	