What Is the Cost of Privatization for Workers?

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ABSTRACT

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 associated 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.

PRIVATIZATION OF STATE-OWNED ENTERPRISES (SOEs) figures prominently on the policy agendas of numerous advanced economies worldwide.[1] Formulating effective, evidence-based privatization policies requires a thorough understanding of the associated costs and benefits for all stakeholders. To date, the advantages of privatization, such as enhanced firm governance, productivity, and performance, have been extensively documented (Megginson

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and Netter (2001), Estrin et al. (2009), Megginson (2017a)). The impact on workers in privatized SOEs, however, remains less understood, even though it constitutes a significant concern for policymakers.[2]

In this paper, we help fill this gap by exploring the costs of privatization for workers in Sweden. The Swedish setting allows us to exploit the availability of detailed registry data that cover both the entire population of private and public firms and the entire population of persons across multiple decades. These data enable us to quantify effects on workers in terms of wages, income, long-term career prospects, financial characteristics, and well-being. These data also allow us to quantify the magnitude of government transfers required to mitigate negative income shocks and to compare these transfers with the firm-level productivity gains from privatization.

Moreover, the Swedish registry data enable us to address several empirical challenges. The nonrandom nature of privatizations renders direct comparisons between privatized and nonprivatized SOEs vulnerable to selection bias. We address this concern by leveraging the data to adopt an event study difference-in-differences approach, constructing control groups of workers or firms that remain state-owned but share similar characteristics with their privatized counterparts. In addition, while a conventional two-way fixed effects model is unsuitable for units treated at different periods, owing to its limited capacity to accommodate heterogeneous treatment effects over time (de Chaisemartin and D'Haultfœuille (2020), Goodman-Bacon (2021)), we employ a stacked difference-in-differences model, which is commonly used together with matching in the literature on privatizations and other ownership changes (Brown, Earle, and Vakhitov (2006), Bastos, Monteiro, and Straume (2014), Agrawal and Tambe (2016), Olsson and Tåg (2017), Antoni, Maug, and Obernberger (2019), Arnold (2022)).

Our results are as follows. At the *worker level*, we first find an initial reallocation of human capital. This reallocation imposes significant costs on workers as evidenced by substantial declines in wage income of −5.8% in the first two years postprivatization, −9.3% during the third and fourth years, and −8.4%

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[2] 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. On the one hand, better governance and higher productivity increase the demand for labor, which benefits workers. On the other hand, privatization triggers a workforce reorganization that leaves some or all workers worse off (Boycko, Shleifer, and Vishny (1996), Dessaint, Golubov, and Volpin (2017)). 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 of SOEs 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 increase 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)).

in years five through eight. Part of these negative wage income effects can be attributed to privatization-induced unemployment, which increases by 12.6%. However, some of these costs to workers are mitigated by an increase in government transfers, which include unemployment benefits, activity support, and social benefit payouts, compensating for approximately half of the wage income losses. Moreover, we find that the business ownership rate among former SOE workers doubles, but the overall number of new firms established after privatization remains low, with these entities exhibiting low productivity and limited employment capacity. We note that our analysis does not indicate that privatization significantly influences transitions to retirement or any other labor force exits over the long term.

In a novel aspect of our study, we also examine nonlabor market outcomes related to family dynamics, health, and household finance. We find a statistically significant increase in the divorce rate following privatization, though the economic impact amounts to only an additional 0.63 divorces per 1,000 workers annually. We find no significant impact on health, as measured by mortality rates, and negligible effects on workers' financial behavior as measured by stock market participation, portfolio risk levels, and personal leverage. However, workers' cash holdings increase by 10%, which slightly reduces the risky share of their portfolios in the short term. Overall, spillovers of privatization on nonlabor market outcomes exist but are close to negligible economically.

Additional worker-level analyses uncovers novel heterogeneous effects of privatization on workers. Partial privatization and share-issue privatization exhibit more negative impacts on workers than full privatization. Moreover, foreign ownership postprivatization proves more detrimental to workers compared to domestic ownership. In contrast, financial buyers do not exhibit greater harm than nonfinancial buyers and we find no statistically significant differential effects across regions or the business cycle.

At the *firm level*, we observe a decline in the number of employees driven by an increase in the job destruction rate and an unchanged job creation rate. The reduction in the number of employees is accompanied by a decrease in total firm payroll. These effects manifest immediately following privatization and continue into the medium term. In addition, we find that profitability increases by 2.1 percentage points and productivity by 35.7%. Quantile regressions reveal that the productivity gains are driven predominantly by the upper 75th and 90th percentiles, while analysis using logged productivity, which accounts for nonlinearity in the productivity distribution, indicates a more modest gain of 11.5%. The reductions in firm-level employment and payroll, coupled with the increases in profitability and productivity, support the view that privatization leads to cost savings realized, at least in part, through workforce reductions.

We next explore possible mechanisms behind why privatization increases productivity. One way to realize productivity improvement following privatization is to replace less productive workers with more productive workers. To test this channel, we examine differences in human capital between hired and separated workers after privatization, relying on measures of cognitive and

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noncognitive skills from the Swedish military draft test. While we find no statistically significant effects on the overall quality of hires and separations, the results point to a modest improvement in the quality of hires in the medium term of 4.8%. We next investigate whether privatized firms realize productivity gains by adjusting their financial strategies. New owners may boost productivity by increasing capital quality, which would be reflected in changes in capital investments and possibly a replacement of labor with capital in the firm's production function. Alternatively, new owners may change firm leverage in an attempt to increase bargaining power over labor unions. We find no economically meaningful effects on firms' investment or leverage ratios. Finally, we examine whether productivity gains following privatization are driven by governance changes. Prior research suggests that improving governance is key to increase performance (Djankov and Murrell (2002), Gupta (2005)). We find that productivity increase when the CEO is replaced but is unchanged when the CEO stays on. One likely explanation for this effect is that management changes can disrupt implicit contracts between CEOs and workers, making labor cost reductions more likely. This would manifest as a reduction in employment and payroll only when the CEO is replaced, which is precisely what we observe.

Overall, the above results suggest that operational and governance engineering following privatization leads to a reallocation of human capital that contributes to both an increase in firm-level productivity and to losses in income for workers. Rough calculations based on firm- and worker-level data suggest that, depending on the point estimate, the productivity gains from privatization exceed the associated worker costs pregovernment transfers at a ratio that ranges from two to six. Moreover, government transfers to workers total 10% to 30% of the per-worker productivity gains in privatized firms. These results therefore suggest that workers could receive full compensation for their losses, with a residual surplus remaining to be distributed between the firms' new owners and the government through the transaction price.

Our paper makes three significant contributions to the privatization literature.[3] While the costs and benefits of privatization have received substantial attention, few large sample studies consider the effect of privatization on individual workers (Megginson and Netter (2001), Estrin et al. (2009), Earle (2014)). Most studies focus on firm-level employment only (D'souza and Megginson (1999), LaPorta and Lopez-de Silanes (1999), Brown, Earle, and Vakhitov (2006), Chong, Guillen, and López-de Silanes (2011), Dinc and Gupta

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[3] Our paper also contributes to the broader labor and finance literature (Pagano and Volpin (2005), Atanassov and Kim (2009), Cronqvist et al. (2009), Maksimovic, Phillips, and Prabhala (2011), Simintzi, Vig, and Volpin (2015), Dessaint, Golubov, and Volpin (2017), Mueller, Ouimet, and Simintzi (2017), Subramanian and Megginson (2018)). This literature uses matched employer-employee data to study worker-level effects of other ownership changes such as mergers and acquisitions (Lagaras (2025)), corporate diversifications (Tate and Yang (2015)), and private equity buyouts (Agrawal and Tambe (2016), Olsson and Tåg (2017), Antoni, Maug, and Obernberger (2019)).

(2011)). Notable exceptions include Brown, Earle, and Telegdy (2006), Melly and Puhani (2013), Bastos, Monteiro, and Straume (2014), and Arnold (2022).

We first extend the scope of this literature from short-term impacts on workers' wages to long-term scarring effects on various labor and nonlabor worker outcomes. Prior studies on the effects of privatization at the worker level focus almost exclusively on the effect on short-run wages, ignoring long-run effects and their dynamics over time. The literature is also silent about other labor market outcomes, such as retirement or entrepreneurship, or about nonlabor worker outcomes related to family, health, and household financial well-being. Our data allow us to demonstrate that in Sweden, the primary costs to workers are reductions in wage income and increased long-term unemployment, while spillovers to nonlabor worker outcomes are minimal. Understanding the extent to which privatizations impact workers beyond wage reductions is crucial to inform policy decisions. For example, our finding that privatization lead to long-term unemployment suggests that governments could mitigate these effects through the adoption of evidence-based strategies to combat unemployment, such as training programs and private-sector employment initiatives.

We next present empirical evidence demonstrating that the social safety net can cushion workers from the adverse effects of privatization. This particular aspect of privatization remains underexplored. Our analysis suggests that policymakers in countries with robust social safety nets can leverage these existing frameworks to partially offset the costs of privatization. Furthermore, our findings indicate that privatization may have fiscal externalities on government budgets in the form of increased unemployment benefit payouts.

Finally, our detailed firm-level data enable us to provide novel evidence on the role of operational, financial, and governance engineering postprivatization. Specifically, this study explores these mechanisms behind productivity gains after privatization and estimates productivity gains relative to workers' income losses and government transfers. Our results suggest that from an economic perspective, Sweden has benefited from privatization through an increase in firm profitability, despite the corresponding losses in worker income.

Our findings have implications for privatization policies worldwide, as we provide detailed firm- and worker-level evidence from an institutional setting that is similar to settings in various countries currently pursuing privatization on a case-by-case basis. Figure 1 illustrates that, like Sweden, numerous developed economies have both a high proportion of the labor force in SOEs and a high level of social expenditures, that is, an extensive social safety net. These include the other Scandinavian countries and large European countries such as France, Germany, Portugal, and Italy. The labor market in these countries is also similar to that in Sweden.[4] Furthermore, in many developing countries,

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[4] Lazear and Shaw (2009) report strong similarities between labor markets in Scandinavian countries and Belgium, France, Germany, Italy, the Netherlands, and the United States.
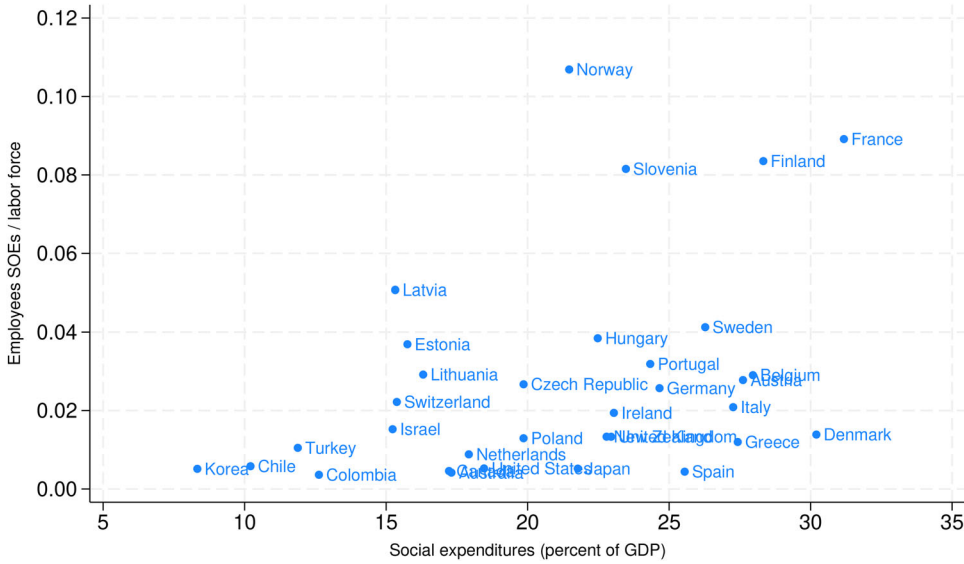
Figure 1. SOE size and government social expenditures. This figure plots the relationship between the fraction of employees in SOEs and social expenditures as a percent of GDP using OECD data from 2012 (the latest year for which the OECD has information on employees in SOEs). *Employees in SOEs* is based 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. (Color figure can be viewed at wileyonlinelibrary.com)

unprofitable SOEs are supported by government subsidies. Our paper shows that government transfers to employees can offset the income losses from privatization, costing only a fraction of the post privatization productivity gains per worker. In particular, our study quantifies the size of transfers— relative to wages—necessary to shield workers from the resulting income losses. We also document that privatization leads to an increase in unemployment. In light of this finding, policy makers could adopt research-backed measures to mitigate long-term unemployment, thereby mitigating the adverse effects from privatization on workers. For example, in a review of over 200 studies on active labor market programs, Card, Kluve, and Weber (2018) document that training programs and private-sector employment initiatives significantly benefit the long-term unemployed. Job search assistance programs have comparatively less impact.

The rest of the paper is structured as follows. Section I provides a brief discussion of the institutional background. Section II summarizes our data sources, sample, and discusses the empirical design. Worker-level findings are presented in Section III, while Section IV provides results on firm-level outcomes and their underlying mechanisms. Section V presents results of robustness tests and additional analyses such as heterogeneity analyses across industry and macroeconomic conditions. Section VI concludes.

I. Institutional Background

A. *Privatization in Sweden*

As in other countries, the main motivation for state ownership of firms in Sweden has 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, and banking) (Munkhammar (2007)). Following the European trend, privatizations in Sweden aimed to increase operating efficiency and raise revenue as policymakers and practitioners alike had come to believe that state ownership hindered SOEs' growth and that many SOEs' no longer needed state involvement. The government was free to choose the timing of privatization to maximize revenues, and it imposed few restrictions on potential buyers. Guidelines recommended only that 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). As in other countries, a key concern about privatization was the fear of job losses if foreign corporations or investors acquired SOEs (Dansbo and Wallner (2007)).

Sweden did not set out formal rules for privatization; rather, it decided which firms to privatize and when on a case-by-case basis. This approach is more common in countries with fewer privatization candidates and in countries where privatization targets are small compared to the size of the entire corporate sector. In contrast, explicit privatization programs are more common where the scale and scope of privatization are 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)).

B. *Employment Legislation around Privatization*

Sweden has no specific rules regarding employment conditions after privatization. This is due in part to the government's ownership policy, which requires SOEs to operate under similar conditions as private firms and to prioritize long-term firm value, while, in some cases, also 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 their contract. The change in ownership itself, therefore, is not a valid reason for terminating the employment contract unless the firm faces significant reorganization needs for 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 a separation agreement is in place, the compensation is paid out as regular labor income (thus, any buyouts of

workers will be captured in our measure of wages). If SOEs are incorporated in the years leading up to privatization, the government negotiates with unions concerning the employment contracts of public employees and reserves the right to terminate the employment of workers who refuse to become employed by the newly incorporated entity (OECD (2009)).

C. 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 across all regions in Sweden (Sianesi (2004)). First, Sweden has a relatively generous unemployment insurance system, with a replacement rate of around 80% of lost wage income for up to 60 weeks. To be eligible, an unemployed person needs to be registered at the unemployment office, to be actively searching for work, and they must accept a job offer if offered or risk losing their benefits. Second, active labor market programs are an essential feature of the Swedish social safety net. These programs 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 or internship programs, relocating, or entering entrepreneurship. The purpose of these programs is to prevent long-term unemployment and keep people in the labor force. Third, 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.

II. Data and Empirical Strategy

A. Administrative Registers

We rely on administrative data maintained by Statistics Sweden, the government agency responsible for the country's official statistics. For individual-level data, we rely on the LISA database, which consolidates government registers, such as, tax records, population registers, and surveys. The database provides annual data on all individuals aged 15 and above who are registered in Sweden. Our data cover the period from 1990 to 2017 and include information on individual-level characteristics such as age, gender, marital status, family size, education, university student status, immigrant status, municipality of residence, employer, wage income, capital income, unemployment benefits, active labor market program benefits, social welfare payments, retirement income, unemployment days, entrepreneurship status, and date of death. To supplement our analysis, we also obtain 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 merge ownership data from the LISA database with firm performance data from the Structural Business Statistics (FEK) database, which encompasses all operating firms with employees in Sweden. The FEK database provides information on industry, ownership status, productivity

(value-added per employee), and operating return on assets (ROA). These data cover the period from 1997 to 2017. We link firms and workers in November each year, which enables us to calculate firm tenure and labor market experience by tracking an individual's employment history from 1990 to 2017. Table IA.I in the Internet Appendix provides a detailed overview of the variables used in our analysis.[5]

B. Defining Privatization

We use LISA database records to identify privatizations in the registry data, defining a firm as state-owned if the state, regional government, or municipality holds a controlling stake in an incorporated company. Consistent with much of the literature, we focus on the privatization of SOEs rather than the sale of government-controlled establishments to private firms (as in Oreland (2010)) due to the lack of establishment-level performance outcome data for the latter. We define a firm's privatization as a change in ownership classification from state to private between two consecutive years during the 1997 to 2017 period, and focus on firms with at least five employees. This ensures that we have access to both financial accounts and individual information from LISA.[6] In Section V, we present results that demonstrate the robustness of our main findings to alternative ways of defining privatization, such as partial privatization, share-issue privatization, or privatization through sales to specific private actors.

Figure 2 shows the distribution of privatizations in Sweden over time and across industries. Although privatizations occurred each year except one, the majority occurred earlier in the sample period. Privatizations are most prevalent in the business activities and financial intermediaries sectors, despite these firms being relatively small on average. The transportation and telecommunications sectors have the largest number of workers affected by privatizations. This industry distribution is consistent with the historical motivations for state ownership in Sweden, which include controlling natural monopolies and resources, and rescuing struggling firms and industries to save jobs and reduce regional unemployment (Munkhammar (2007)). In addition, the industry distribution is broadly consistent with the overall distribution of privatization by industry in other Organization for Economic Co-operation and Development (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)).

We next estimate a selection regression model on the sample of SOEs in Sweden to examine whether those SOEs that are privatized differ

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[5] The Internet Appendix may be found in the online version of this article.

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

Panel A: Number of privatizations per year

Panel B: Number of workers per year

Panel C: Number of privatizations per industry

Panel D: Number of workers per industry
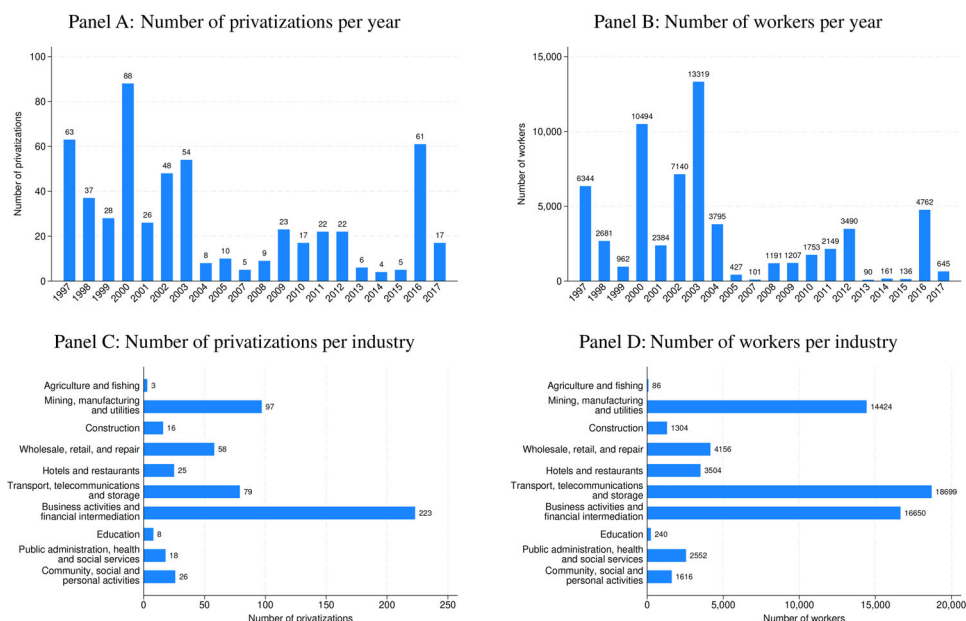
Figure 2. Privatizations vary over time and across industries. The sample consists of privatized firms in Sweden between 1997 and 2017. The number of workers refers to the number of employees in the privatized firms one year before the privatization. The industry classification is based on NACE Rev 1.1. (Color figure can be viewed at wileyonlinelibrary.com)

systematically from those that remain state-owned. The model incorporates industry and year fixed effects. The estimates, reported in Table IA.II, indicate that the conditional probability of privatization increases with the firm's payroll but decreases with the number of employees, productivity, and leverage. However, the economic significance of these estimates is low. Moreover, a firm's ROA and investment ratio do not differ significantly between privatized SOEs and those that remain state-owned, and the adjusted R^2 is only 3%. These results indicate that selection on these key economic observables is negligible.

C. Empirical Challenges

Analyzing the impact of privatization on workers requires that we address several empirical challenges. The primary challenge is the nonrandom nature of privatization, which makes direct comparisons between privatized and nonprivatized SOEs susceptible to selection bias at both the firm and the worker levels. To address this concern, we adopt an event study difference-in-differences approach, forming control groups of workers and firms that remain state-owned but possess characteristics similar to their privatized counterparts. This approach helps mitigate observable differences and, by

incorporating an event study design, addresses unobservable factors that remain constant over time between treated and control units, such as unobserved difference in ability across workers.[7] However, this design does not account for time-varying unobserved differences, which remain a potential source of bias. Nevertheless, analyzing pretreatment trends allows us to gauge the extent of such differences.

We also need to account for the fact that privatizations in our sample occur in different years. A standard two-way fixed effects model is unsuitable when units receive treatment at different times due to its inability to manage heterogeneous treatment effects over time (Goodman-Bacon (2021), de Chaisemartin and D'Haultfœuille (2020)). The difference-in-differences estimate in this model is a weighted average of all possible 2×2 difference-in-differences estimates arising from the staggered timing of treatment. When the treatment effect varies over time, comparing units treated later to those treated earlier can yield a biased estimate of the treatment effect.[8] To address these issues, we apply a stacked difference-in-differences model, which, when combined with matching, is commonly used in the literature on privatizations and other ownership changes (Brown, Earle, and Vakhitov (2006), Bastos, Monteiro, and Straume (2014), Agrawal and Tambe (2016), Olsson and Tåg (2017), Antoni, Maug, and Obernberger (2019), Arnold (2022)).

In the worker-level analysis, another challenge arises from potential worker sorting in anticipation of privatization, which could distort the results. To address this concern, we match workers one year before the privatization event and confirm the robustness of our results with matches made three years earlier.

D. Constructing the Control Group

Our analysis investigates the effects of privatization on both workers and firms, necessitating the creation of two matched control groups. For the worker-level analysis, we identify all employees working in incorporated firms that were state-owned one year prior to going private, which we designate as treated workers. We limit our sample to individuals aged 20 to 60 years to ensure completion of secondary education and to include only those with at least five years remaining until the Swedish guaranteed retirement age of 65. In total, we identify 70,079 treated workers.

A comparable control group is crucial for the validity of our empirical strategy, which uses the trend of the control group as the counterfactual trend of the treated group. To find a comparable control group of workers, we employ cell matching with over 3.3 million workers in SOEs that

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[7] Smith and Todd (2005) show that matching together with difference-in-differences regressions performs well in mitigating selection biases.

[8] The reason is that the weights of each 2×2 difference-in-differences estimate depend on the sample size of each subsample and the treatment variance, so units treated toward the middle of the sample period receive more weight than those treated earlier or later.

were not privatized to select controls for our treated workers. A large pool of potential controls is vital as the number of dimensions to account for increases exponentially with the number of characteristics considered in the match.

The matching involves the following steps. For each year of privatizations in our sample, we randomly match each treated worker with a control worker based on *age*, *gender*, *industry*, *region*, and *wage* (as measured by labor income).[9] The matching occurs one year prior to privatization and is executed without replacement, yielding a final sample of 63,231 matched pairs.[10] As a robustness check, we replicate our main-worker-level results to demonstrate their consistency when employing propensity score matching at the firm level to find controls, ensuring that no treated workers are lost in the process, as shown in Table IA.IX and Figure IA.6 in the Internet Appendix. Finally, we collect information on treated and control workers in a window from three years before the match and eight years after and we create event-specific data sets, one for each year of privatization in our sample.[11]

We adopt a similar matching methodology for the firm-level analysis. First, we identify 368 privatized firms with information on all outcomes for at least three years before privatization. This allows us to analyze pretrends in firm outcomes. Second, we match, randomly and without replacement, each treated firm a year before the privatization to another SOE in the same *industry*, *region*, and *year* that is of similar size in terms of *number of employees* and that shares a positive versus negative *ROA*, to one that is not privatized.[12] Accounting for the number of employees and profitability is important, as these measures are correlated with whether an SOE is privatized or not (see the selection results in Table IA.II). We then collect time-series information for each treated and control firm and combine them into event-specific data sets, which we 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

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[9] 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*, which divides the EU and the United Kingdom into major socioeconomic regions. For industry, we use four broad industry classifications: Mining, manufacturing, and others; Transport, communication, and storage; Business activities and financial intermediaries; and Other.

[10] An alternative to using 1:1 cell matching 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.

[11] Attrition is negligible in these data sets, as the LISA database encompasses individuals over the age of 15 registered in Sweden, and thus, attrition can occur only if people move abroad or die.

[12] In the Internet Appendix, we show that our firm-level results continued to hold when using a propensity score matching strategy (Tables IA.X and IA.XI and Figures IA.7 and IA.8).

postperiod to four years, corresponding to the short- and medium-run periods used in the individual-level analysis.[13]

The effectiveness of our matching strategy in creating comparable treated and control groups on observables is evident when looking at worker characteristics related to demographics, education, and careers in Panel A of Table I. Columns (1) and (2) display the mean values for treated and control workers one year before privatization, respectively, and column (3) displays the difference. To test for mean differences between treated and control workers, we use the normalized t-value displayed in column (4).[14] A normalized t-value above 0.25 in absolute terms indicates a substantial difference in means. However, on average, the treated and control workers share similar demographic, educational, and career characteristics, and the absolute normalized t-values are all well below 0.25. Most workers are men and 40 or more years old, while few are immigrants. Most 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 earn approximately 270,000 SEK in annual wage income on average.[15] Wage income for the control workers is slightly lower but not significantly different. The treated workers are more likely to have tenure of two years or less, while the control workers are more likely to have tenures of three to five and 6 to 10 years (but once again, the difference is small and not statistically significant).

Panel B of Table I displays differences in observable characteristics between the treated and the matched control firms. Finding good controls at the firm level is harder than at the worker level due to the number of SOEs that operate in Sweden; as a result, treated and control firms differ slightly in observables before privatization. For instance, control firms have, on average, around 10% more employees and slightly higher profitability, productivity, and leverage. These differences, however, are economically small and statistically insignificant, with t-values well below 0.25.

Overall, there are some economically small observable differences in means between the treated and control workers and firms, but normalized t-tests show that these differences are not statistically significant. Nevertheless, we add several of these preprivatization variables to our regressions as controls to account for any remaining differences these between treated and control workers and firms.

[13] During the four-year postperiod, the attrition rate is around 21% for treated firms and 11% for control firms (78 treated firms and 41 control firms). However, Table IA.XII confirms that attrition among treated firms is not systematically correlated with preprivatization observables.

[14] The 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).

[15] As of March 2024, the exchange rate is 0.1 USD per 1 SEK.

Table I

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, *Transfers* is the sum of unemployment benefits, activity support, and social benefits in thousands of SEK, *Total income* is wages plus transfers, *Labor market experience* is the number of years since entering the labor market, and *Tenure* is the 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, *ROA* is operating return on assets, and *Productivity* is the value-added per employee in thousands of SEK. *Investment ratio* is capital expenditures 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)
	Panel A: Worker Level			
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	271.08	265.72	5.36	0.02
Transfers	4.07	3.77	0.31	0.01
Total income	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	63,231	63,231		
	Panel B: Firm Level			
Employees	107.00	118.90	−11.90	−0.02
Payroll	279.58	302.37	−22.79	−0.02
ROA	0.00	0.02	−0.02	−0.06
Productivity	270.93	308.01	−37.08	−0.08

Table I—*Continued*

Panel B: Firm Level				
Investment ratio	0.06	0.08	−0.01	−0.08
Leverage	0.63	0.68	−0.05	−0.14
Observations	368	368		

E. The Stacked Difference-in-Differences Model

We use a stacked difference-in-differences model in both the worker-level and the firm-level analyses. In effect, this means compiling all matched, event specific data sets—comprising both treated and control units (firms or workers)—into a single data set aligned using normalized time relative to the year of privatization, denoted as $k + 1$. We then estimate the model using a standard difference-in-difference regression model operating in event time. At the worker level, we specify the model:

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

where $Y_{i,f,k,t}$ represents the outcome for worker i employed in SOE f at event year $k - 0$ (recall that the matching was done one year before the privatization) and calendar year t, $After_k$ takes the value one in the year of the privatization and all thereafter, and 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). The interaction term $After_k \times D_i$ takes a value of one for treated workers in the year of the privatization and thereafter and zero otherwise. The coefficient β therefore captures the average intention-to-treat effect under the key assumption that the average outcomes among treated and controls would have had parallel trends in the absence of privatization.[16] The parameter ω_t represents *calendar year fixed effects*, X_i includes controls for *individual age, gender, tenure, and region fixed effects*, and X_f includes *industry fixed effects*. We measure all worker- and firm-level controls in event time $k - 0$, that is, one year before privatization.

To evaluate the dynamic effects surrounding privatization, we substitute $After_k$ with event-time specific dummies τ_k, ranging from $k - 3$ to $k + 8$, with $k - 0$ serving as the reference period:

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

In this model, β_k captures the average intention-to-treat effect during event time k. In our figures, we plot the event-time effects β_k to assess whether there

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[16] The model estimates an intention-to-treat effect because treated and control workers in our sample are only required to work in an SOE in $k + 0$, not in any other year. Consequently, β can be interpreted as the career effect following privatization, capturing both direct and indirect effects.

are parallel trends before privatization and how the dynamics look after privatization. In our tables, we distinguish between short-, medium-, and long-run effects by grouping event times $k+1$ to $k+2$, $k+3$ to $k+4$, and $k+5$ to $k+8$ and using the preyears 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 $Y_{f,k,t}$ of firm f in event time k in calendar year t as

$$Y_{f,k,t} = \alpha + \pi \, After_k + \gamma \, D_f + \beta \, After_k \times D_f + \omega_t + X_f + \epsilon_{f,k,t}, \qquad (3)$$

where the corresponding elements of the model are defined as in equation (1), X_f includes *firm age* (measured in event time $k - 0$), *industry fixed effects*, *region fixed effects*, and standard errors are clustered at the firm level. 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 addressing the heterogeneous treatment effect problem—is that it addresses 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. This mean-reversion pattern creates a problem similar to the Ashenfelter's dip where enrollment 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 increased earnings growth (Ashenfelter (1978)).

F. 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 a case-by-case basis. As such, we expect our results to have high external validity in countries that are similar to Sweden and that have many SOEs that are 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 a similar level of social expenditures as 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, prior 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 case-by-case

approach to privatizing companies is in line with privatization efforts in, for instance, Switzerland, Finland, and Denmark. Given that large privatization programs are not as common now as they were previously, the approach that Sweden followed in privatizing SOEs is likely to be common going forward as well.

Furthermore, in many developing countries, loss-making SOEs are supported by government subsidies. Our paper finds that government transfers to employees can offset their income losses, costing only a fraction of the productivity gains per worker. This finding has significant policy implications for countries encountering worker resistance to privatization. While some countries have introduced compensation schemes to alleviate worker opposition, our study quantifies the size of transfers—relative to wages—necessary to shield workers from income losses.

III. What is the Cost of Privatization for Workers?

A. *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 allowed managers to avoid firing workers, and the ownership change itself could breach implicit contracts with workers.

Table II repeats results of our regressions using equation (1), and Figure 3 plots the yearly difference-in-differences estimates (relative to event time 0) using equation (2). In Panels A to D of Figure 3, we see that before privatization, treated and control workers have almost identical trends in income and unemployment. None of the coefficients in this period differs significantly from zero at the 5% level. Table II, Panel A, reports results for the entire postperiod, while Panel B presents 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 postperiod. Panel B shows that in the short-, medium, and long-run, mean wage income falls by 5.8% ($t = 3.47$), 9.3% ($t = 4.13$), and 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.[17] 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 I).

These results can be related to previous studies on the costs of privatization for workers in terms of wages (our other results cannot, as the other outcomes

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[17] Seim (2019) reports that job displacement in Sweden in general leads, on average, to an earnings drop of 23.5% in the first year and a loss of 16.4% seven years after being displaced.

Table II

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 parentheses). 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 *privatization year*. All controls, except calendar and privatization years, are measured one year before the privatization. Standard errors are clustered at the municipality level. %-change uses the mean for workers in privatized firms in the years before privatization as the baseline. *Wage* is the log of annual gross salary income (using the 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 (unemployment benefits, active support payments, and social benefits (using the inverse hyperbolic sine transformation)), and *Income* is the log of the sum of wages and transfers (using the inverse hyperbolic sine transformation).

Dependent Variable Specification	*Wages* (1)	*Unemployment* (2)	*Transfers* (3)	*Income* (4)
Panel A: Average Effect				
Full period	−0.079	0.013	0.119	−0.035
	(−2.96)	(5.16)	(5.82)	(−1.52)
%-Change	−7.9%	12.4%	11.9%	−3.5%
Adjusted R^2	0.121	0.072	0.064	0.129
Panel B: Dynamic Effect				
Short run (1–2 years)	−0.058	0.011	0.099	−0.029
	(−3.47)	(4.02)	(4.15)	(−2.61)
Medium run (3–4 years)	−0.093	0.012	0.112	−0.043
	(−4.13)	(4.27)	(5.27)	(−2.15)
Long run (5–8 years)	−0.084	0.015	0.135	−0.034
	(−2.23)	(5.27)	(6.09)	(−1.00)
%-Change				
Short run	−5.8%	10.5%	9.9%	−2.9%
Medium run	−9.3%	11.4%	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 dep. var.	7.995	0.105	0.723	8.142
Observations	1,414,270	1,414,270	1,414,270	1,414,270

that we investigate have not been studied previously). The temporary wage declines for workers, that we document, are in line with evidence from two firms in Switzerland studied by Melly and Puhani (2013), who find that average wages follow a J-curve characterized by an immediate drop and then a recovery. Our findings are also in line with Arnold (2022), who finds evidence of wage decreases in Brazil. However, the wage drop in Brazil, on average 25%, is much larger than that in Sweden. A possible explanation for this difference is that SOEs in Brazil pay a wage premium to workers that declines after privatization; we are unaware of any studies documenting such wage premia
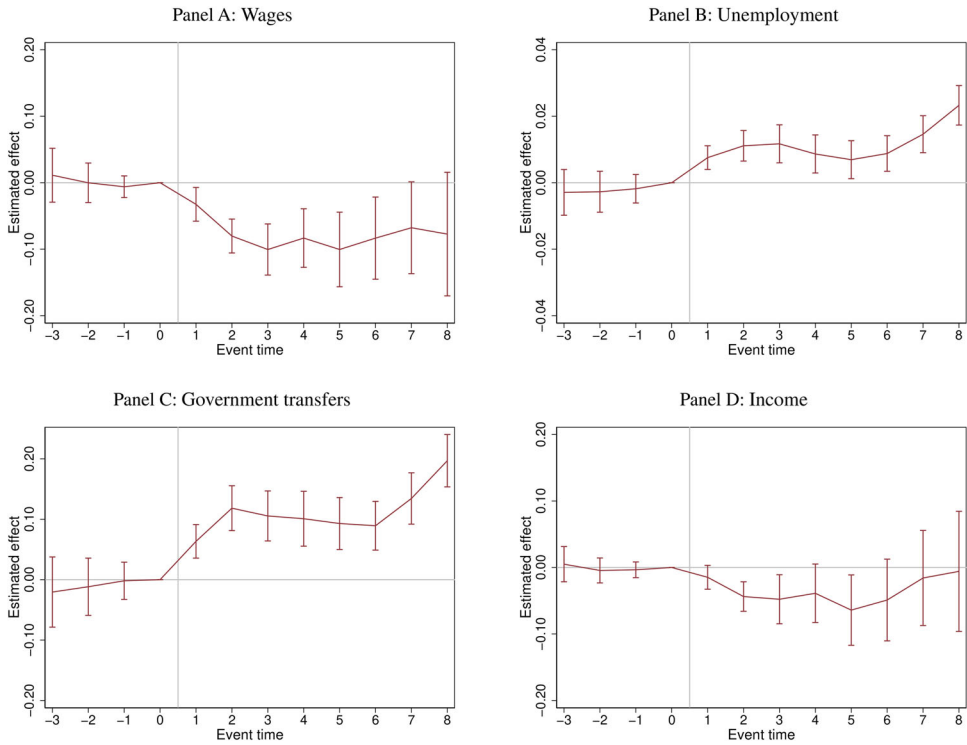
Figure 3. Effects of privatization on wages, unemployment, government transfers, and income. The figures display dynamic difference-in-differences estimates relative to the year prior to the privatization (event time 0) using the model in equation (2). The vertical bars display 95% confidence intervals using robust standard errors clustered at the municipality level. Variable descriptions are available in Table II. (Color figure can be viewed at wileyonlinelibrary.com)

for SOEs in Sweden. Brown, Earle, and Telegdy (2006) similarly document wage declines for Ukraine. One study documents an increase in wages after privatization, counter to what we find. In particular, Bastos, Monteiro, and Straume (2014) show that in Portugal wages following privatizations increase because, the wage floor negotiated via collective bargaining increases and because privatized firms pay larger markups on union wage floors. Given the mechanisms for wage increases documented in Bastos, Monteiro, and Straume (2014), the diverging wage results in Portugal and Sweden are likely due to differences in wage setting institutions.

We next turn to the effects on unemployment. The full postperiod effect on unemployment in column (2) of Panel A shows that privatization leads to greater unemployment with the unemployment rate rising by 1.3 percentage points ($t = 5.16$), a 12.4% increase compared to the preprivatization level among treated workers. Panel B shows that unemployment is persistently higher in the postperiod, with increases of 1.1 percentage points (10.5%, $t = 4.02$) in the short run, 1.2 percentage points (11.4%, $t = 4.27$) in the medium run, and 1.5 percentage points (14.3%, $t = 5.27$) in the long run.

Because we can follow workers over time regardless of their labor market status, an interesting question that we can ask is how much of the wage cut is a mechanical effect of higher unemployment. While we cannot calculate the exact figure, we can bound the effect. Our estimates show that 822 more treated workers become unemployed each year following privatization. Assuming that wages for these 822 workers equal the pretreated mean for all treated workers, they stay unemployed for a full year and have no wage income during this period, this implies a total wage loss of 222,827,760 SEK.[18] Thus, under these assumptions, the negative wage effect from the 822 persons becoming unemployed would be 3,524 SEK per treated worker-year.[19] Comparing this figure to the estimated average wage cut for all treated workers suggests that the mechanical effect of unemployment can explain only 16.4% of the actual wage cut that we estimate.[20]

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 increase by 11.9% ($t = 5.82$), which reduces the change in total income (column (4)) to 3.5% ($t = 1.5$). 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 and 13.5% ($t = 6.09$) in the long run. The dynamic effect on total income in column (4) of Panel B is roughly half that on wage income in column (1) for each subpost period. To shed more light on the government transfers, we separate the privatization effect into the three key components discussed in Section I—unemployment benefits, activity support payments, and social benefits—in Table IA.III and Figure IA.1. All three outcomes have parallel pretrends between the treated and control groups: unemployment benefits increase immediately after privatization, with a pattern similar to that for unemployment incidence, activity support payments also increase just after privatization, with a slight upward trend over time, while privatization has no effect on social benefit payouts. Overall, the point estimates for the full period show an 11.1% ($t = 7.45$) increase in unemployment benefits, a 4.3% ($t = 6.69$) increase in activity support payments, and virtually no effect (-0.02%, $t = 0.23$) on social benefit payouts.

B. Other Worker Outcomes

B.1. Labor Market Status

We complement our analyses on income and unemployment with analyses of the number of unemployment days, labor force exits, and early retirement.

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[18] The pretreated mean is 271,080 SEK, so total wage cut equals $822 \times 271,080 = 222,827.76$ SEK.

[19] To calculate the mechanical effect on wage income, we divide the total annual wage decline for the 822 workers by the number of treated workers in privatized firms: $222,827.76/63,231 = 3.524$.

[20] The calculation of the mechanical effect on wage income is $3.524/21.420 = 0.164$.

Table III
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 parentheses). See Table II for details on the sample and control variables. *Unemployment days* is the yearly total number of unemployment days, *Out of labor force* takes the value of 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) that 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, and *Mortality* indicates if the individual passed away during a given year and is defined as deaths per thousand. The model for mortality uses only postyears, with the dynamic estimates estimated by a model with no constant.

Dependent Variable Specification	U. days (1)	Out of LF (2)	Retire (3)	Self-emp. (4)	Bus. own. (5)	Divorce (6)	Mortality (7)
Panel A: Average Effect							
Full period	2.119	0.003	−0.001	0.0005	0.0015	0.00063	−0.071
	(5.99)	(1.33)	(−0.34)	(0.53)	(3.61)	(2.22)	(−0.76)
%-Change	19.5%	17.6%	−39.5%	19.1%	96.8%	8.3%	−
Adjusted R^2	0.035	0.084	0.221	0.011	0.005	0.002	0.002
Panel B: Dynamic Effect							
Short run	1.501	0.002	−0.001	0.00008	0.00056	0.00053	0.088
	(3.85)	(2.05)	(−0.63)	(0.13)	(1.81)	(1.11)	(0.53)
Medium run	2.100	0.003	0.002	0.00073	0.00146	0.00052	−0.062
	(4.94)	(1.43)	(0.72)	(0.85)	(3.08)	(1.14)	(−0.20)
Long run	2.487	0.003	−0.003	0.00073	0.00200	0.00076	−0.301
	(6.27)	(1.03)	(−0.60)	(0.49)	(3.52)	(2.34)	(−1.45)
%-Change							
Short run	13.8%	13.2%	−23.5%	2.7%	36.9%	6.9%	−
Medium run	19.3%	16.3%	71.7%	25.4%	96.3%	6.8%	−
Long run	22.8%	20.8%	−108.1%	25.2%	131.7%	10.0%	−
Adjusted R^2	0.035	0.085	0.221	0.011	0.005	0.002	0.005
Mean dep. var.	10.894	0.016	0.003	0.0029	0.0015	0.0076	−
Observations	1,414,270	1,414,270	1,414,270	1,414,270	1,414,270	1,414,270	910,276

Table III presents the results for these outcomes, and Figure 4 depicts their dynamic effects (all have parallel pretrends). 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 postperiod, which is an increase of 19.5% relative to the preperiod mean. Panel B shows that this effect grows over time, up to 22.8% 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 the medium or long run (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
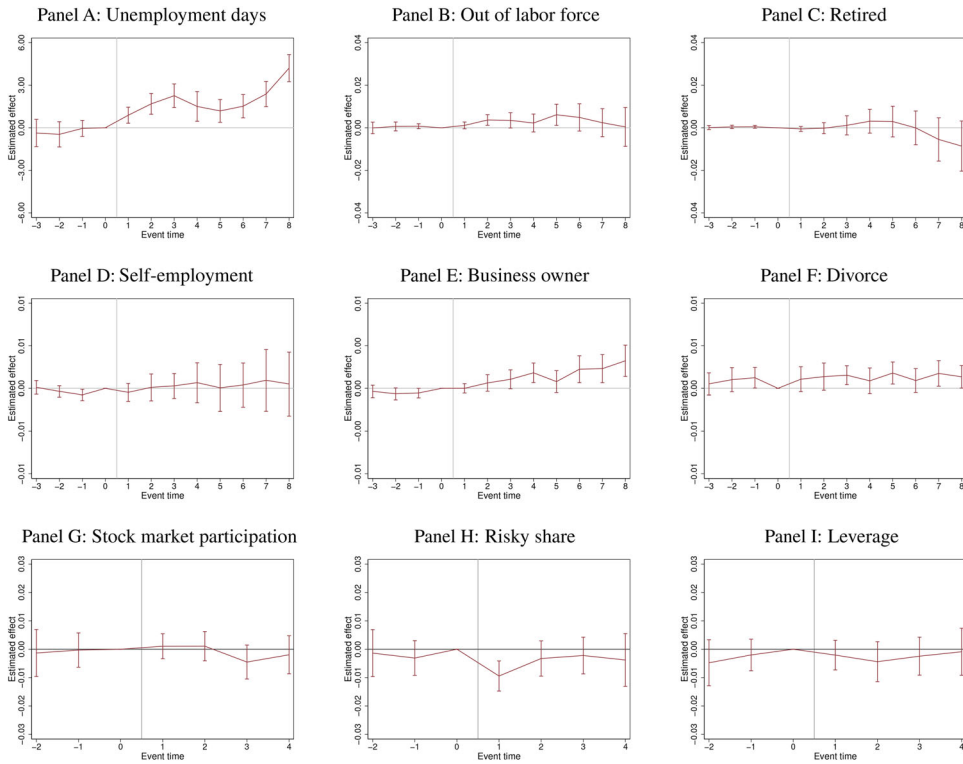
Figure 4. Effects of privatization on other worker outcomes. The figures display dynamic difference-in-differences estimates relative to the year prior to the privatization (event time 0) using the model in equation (2). The vertical bars display 95% confidence intervals using robust standard errors clustered at the municipality level. Variable descriptions are available in Tables III and IV. (Color figure can be viewed at wileyonlinelibrary.com)

effects of privatization on early retirement, either overall ($t = 0.34$) or in any sub postperiod ($t = 0.63$, $t = 0.72$, and $t = 0.60$).

The estimates for the unemployment rate and the number of unemployment days together imply that privatization leads to an extra 822 unemployment spells and 133,986 days of unemployment per year, which amounts to a total of 6,576 unemployment spells and 1,071,888 unemployment days over the eight-year postperiod 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.[21]

[21] Unemployment incidence increases by 1.3 percentage points per year on average, which results in $0.013 \times 63,231 = 822$ extra workers becoming unemployed per year during an eight-year postperiod. The number of unemployment days increases on average by 2.119 days per year (see Table III), which results in a total of $2.119 \times 63,231 = 133,986$ extra days per year and 1,071,888

B.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 et al. (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 shows 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.[22] Second, reorganization and new ownership may motivate entrepreneurial workers to see the change as an opportunity to bring new ideas and innovations to the market. Recent studies show that corporate R&D spending encourages workers to join founding startup teams (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).[23] Table III, columns (4) and (5), and Figure 4, Panels D and E present 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 subperiods postprivatization ($t = 0.13$, $t = 0.85$, and $t = 0.49$). Business ownership, in contrast, increases by 14.7 basis points (bps) (96.8%, $t = 3.61$) following privatization. Panel B, column (5), indicates

days during an eight-year postperiod. 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.

[22] 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 nonentrepreneurial people select into the public sector. Tåg, Åstebro, and Thompson (2016) find similar patterns for selection into more bureaucratic firms.

[23] The literature often separates necessity- and opportunity-driven entrepreneurship, although the terms may vary. See, for example, Schoar (2010), Tåg, Åstebro, and Thompson (2016), Levine and Rubinstein (2017), Rider et al. (2019), and Guzman and Stern (2020). Åstebro and Tåg (2017) show that most of the 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 firms since it has lower initial capital and reporting requirements than a limited liability company.

that the increase is gradual over time, with no increase in the short run ($t = 1.81$), a 14.6 bps increase in the medium run (96.3%, $t = 3.08$), and a 20 bps 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 1,000 workers affected by privatization.[24]

We examine the economic importance of the firms run by entrepreneurs in Table IA.IV. The results suggest that necessity-driven entrepreneurship is the main driver of increased entrepreneurship. First, most new entrepreneurs (69%) are self-employed and run unlimited liability firms that rarely hire other employees (Åstebro and Tåg (2017)). Second, entrepreneurship is not profitable. The total income of those who entered entrepreneurship (170,000 SEK) is about half of their income before privatization (300,000 SEK), while treated workers earn more than control workers. Third, almost all (95%) of the entrepreneurs run small firms with fewer than 10 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 1,000 privatized workers. They also generate additional value-added equal to only 0.7% of the value-added of the privatized firms.[25]

We explore three alternative mechanisms that could explain the increase in entrepreneurship. First, the increase in business ownership may result from management buyouts (MBOs), whereby management buys the company from the state and becomes a business owner. This occurs in our data, but only in 27 cases for the treated firms in our sample. Also, if this were a main mechanism, we would see a sudden increase in business ownership upon privatization in Figure 4, not a gradual increase over time. Thus, MBOs are too rare to account for the pattern we document. Second, higher entrepreneurship rates may stem from the deregulation of entire industries, creating entrepreneurial opportunities for treated workers but not for control workers. This possibility, however 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 our 1997 to 2017 sample period are the domestic EU/EES airline industry (1997), the railroad and public transport industry (2007 to 2012), the preschool market (2006), the pharmacy industry (2009), primary

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[24] With a difference of 336 unique entrepreneurs between the treated and control groups (see Table IA.IV), 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 $336/63{,}231 = 0.005$, or about five more entrepreneurs per 1,000 workers affected by privatization.

[25] Our sample consists of 368 SOEs and 63,231 workers who experience privatization, generating a total value-added of $270.93 \times 63{,}231 = 17{,}316{,}174.8$ TSEK before privatization. Column (3) in Table IA.IV indicates that we have 714 more firm-year observations, with a mean employment difference of -0.01 workers, resulting in seven fewer job-years. For value-added, using the numbers in Table IA.IV, we obtain $((341.99 \times 6.17) - (312.30 \times 6.19)) \times 714 = 126{,}366.1$ TSEK in extra value-added.

health care (2010), and the technical testing and analysis of vehicles industry (2010). In our sample, 23 privatizations occurred in these industries in or after the year they were deregulated, 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 Schmieder (2017) show that these "domestic outsourcing" practices are significant in Germany. However, this does not seem to be a major driver of the small increases in business ownership that we observe: only 4% of the businesses started by treated entrepreneurs in our sample are in the industries that Goldschmidt and Schmieder (2017) classify as "domestic outsourcing" industries: logistics, cleaning, security, and food services (see Table IA.IV).

B.3. Family and Health

Existing research shows that unemployment increases the probability of divorce and mortality (Gerdtham and Johannesson (2003), Eliason (2012)). Because privatization increases unemployment, it could also have negative spillovers on workers' families and health. We examine the effect of privatization on the probability of divorce and death in columns (6) and (7) of Table III. Figure 4 plots the dynamic effects and shows that there are no pretrends in the outcomes. We find an effect of 0.63 divorces more per thousand workers (8.3%, $t = 2.22$) that is statistically significant in the long run (7.6 bps, 10.0%, $t = 2.34$) but no effect on mortality ($t = -0.76$). Thus, privatization appears to weakly increase divorce rates but to have no effect on mortality.

B.4. Household Finance

We also 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, Guiso, and Pistaferri (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, risky share of financial portfolios (including risky assets and cash), and total debt (such as mortgages and credit card debt).[26] We

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[26] See Calvet, Campbell, and Sodini (2007), Calvet and Sodini (2014), or Bach, Calvet, and Sodini (2020) for more details about these data.

Table IV
Household Finance 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 parentheses). See Table II for details on the sample and control variables. *Stock market participation* takes the value one if an individual owns risky stocks, *Risky share* measures the share of risky assets to total assets of an individual's portfolio, and *Leverage* measures total personal debt to total personal assets.

Dependent Variable Specification	*Participation* (1)	*Risky share* (2)	*Debt ratio* (3)
Panel A: Average Effect			
Full period	−0.0005	−0.0032	−0.0002
	(−0.15)	(−0.88)	(−0.05)
%-Change	−0.1%	−0.7%	0.0%
Adjusted R^2	0.045	0.032	0.108
Panel B: Dynamic Effect			
Short run	0.0016	−0.0049	−0.0010
	(0.48)	(−1.64)	(−0.24)
Medium run	−0.0027	−0.0015	0.0006
	(−0.67)	(−0.32)	(0.12)
%-Change			
Short run	0.3%	−1.0%	−0.2%
Medium run	−0.4%	−0.3%	0.1%
Adjusted R^2	0.045	0.032	0.108
Mean dep. var.	0.639	0.470	0.560
Observations	342,554	342,554	342,554

limit our pre- and postperiods to two years and focus only on privatizations between 1999 and 2007 since these data are available only for this period.

Table IV, columns (1) to (3) and Figure 4, Panels D and E, present the results and parallel trends for the three household finance outcomes prior to privatization. We find no effect of privatization on stock market participation (column (1), $t = 0.15$), 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 (2), $t = 0.88$) or the debt ratio (column (3), $t = 0.05$). The only marginally significant effect is a short-run decrease of −1.0% in the risky share ($t = 1.64$), which leads us to examine the components of the risky share and the debt ratio. In the Internet Appendix, Table IA.V and Figure IA.2 show that cash holdings increase by 9.8% (3,912 SEK, $t = 3.14$) but privatization does not affect risky assets, debt, or overall wealth. These results suggest that workers increase their cash holdings slightly as a precautionary measure when their labor market status becomes more uncertain after privatization, but there are no other large effects on household financial outcomes.

IV. What are the Benefits of Privatization for Firms?

A. *Cutting Labor Costs to Increase Productivity*

Our worker-level analysis documents heightened unemployment and decreased wages after privatization. This pattern supports the view that, before privatization, employment at SOEs might exceed what would be optimal from a purely profit-maximizing perspective. Such a discrepancy could result from SOEs prioritizing stable employment and low unemployment rates, or from soft budget constraints that allowed 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 for 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 (*ROA*), and productivity (*value-added per employee*). Following Davis, Haltiwanger, and Schuh (1998), the employment growth rate from year $t - 1$ to t at firm f is defined as

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

This growth rate accounts for firm entries and exits and is bounded between -2 (exits) and 2 (entries). The job destruction and job creation rates are then:

$$JDR_{f,t} = |min\{g_{f,t}, 0\}|, \tag{5}$$

$$JCR_{f,t} = max\{g_{f,t}, 0\}. \tag{6}$$

Table V presents the outcomes of our regression analysis using equation (3), while Figure 5 depicts the annual difference-in-differences estimates (in relation to event time zero). In Panels A to F of Figure 5, it is clear that prior to privatization, the trends in all outcomes for treated and control firms are nearly identical. During this period, none of the coefficients is significantly distinct from zero at the 5% level. Table V contains two panels: Panel A reports results for the entire postprivatization period, whereas Panel B separates results into short-, medium-, and long-term effects (event times 1–2, 3–4, and 5–8, respectively).

Panel A, column (1), reports a 16.3% decrease in 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 is unchanged (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 immediately after privatization and persist into the medium term.

Turning to performance, column (5) demonstrates a marginally significant increase in profitability of 2.1 percentage points ($t = 1.82$) from a baseline approximating zero, and column (6) reveals a 35.7% ($t = 2.77$) increase in

Table V
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 parentheses). The sample consists of treated firms and matched control firms and includes information for three years before and four years after the privatization. All models include fixed effects for privatization year, calendar year, industry, region, and firm age. All controls, except calendar and privatization years, are measured one year before the privatization. %-change uses the mean for privatized firms over the years before the privatization as the baseline. *Employees* is the number of employees, *Destruction (Creation)* measures the job destruction and creation rates as defined in equations (5) and (6), *Payroll* is the sum of the annual wage income of workers employed in the firm in hundreds of SEK, *ROA* is operating return on assets, and *Productivity* is the value-added per employee in thousands of SEK.

Dependent Variable Specification	Employees (1)	Destruction (2)	Creation (3)	Payroll (4)	ROA (5)	Productivity (6)
Panel A: Average Effect						
Full period	−0.163	0.109	−0.025	−0.122	0.021	109.454
	(−2.82)	(4.92)	(−1.40)	(−2.15)	(1.82)	(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
Panel B: Dynamic Effect						
Short run (1–2 years)	−0.141	0.118	−0.027	−0.124	0.018	101.936
	(−2.81)	(4.40)	(1.49)	(−2.45)	(1.46)	(2.91)
Medium run (3–4 years)	−0.196	0.098	−0.022	−0.122	0.026	122.514
	(−2.32)	(3.23)	(−1.06)	(−1.45)	(1.69)	(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	0.072
Mean dep. var.	0.186	0.072	0.375	11.392	0.007	306.555
Observations	4,804	4,611	4,611	4,804	4,804	4,804

productivity. Panel B indicates that these effects are hold in both the short and medium term, albeit are slightly more pronounced in the medium term. The large increase in productivity warrants additional attention. Table IA.VI shows that large positive outliers drive the effect. We run both a 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 percentiles (16.7% with $t = 2.60$ and 26.5% with $t = 2.21$) and that log 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% to 35.7%.

The observed reductions in firm-level employment and payroll combined with improvements in performance and productivity are consistent with the interpretation that privatization is followed by a cost-saving strategy of reducing the firm's workforce. The increase in profitability and productivity is
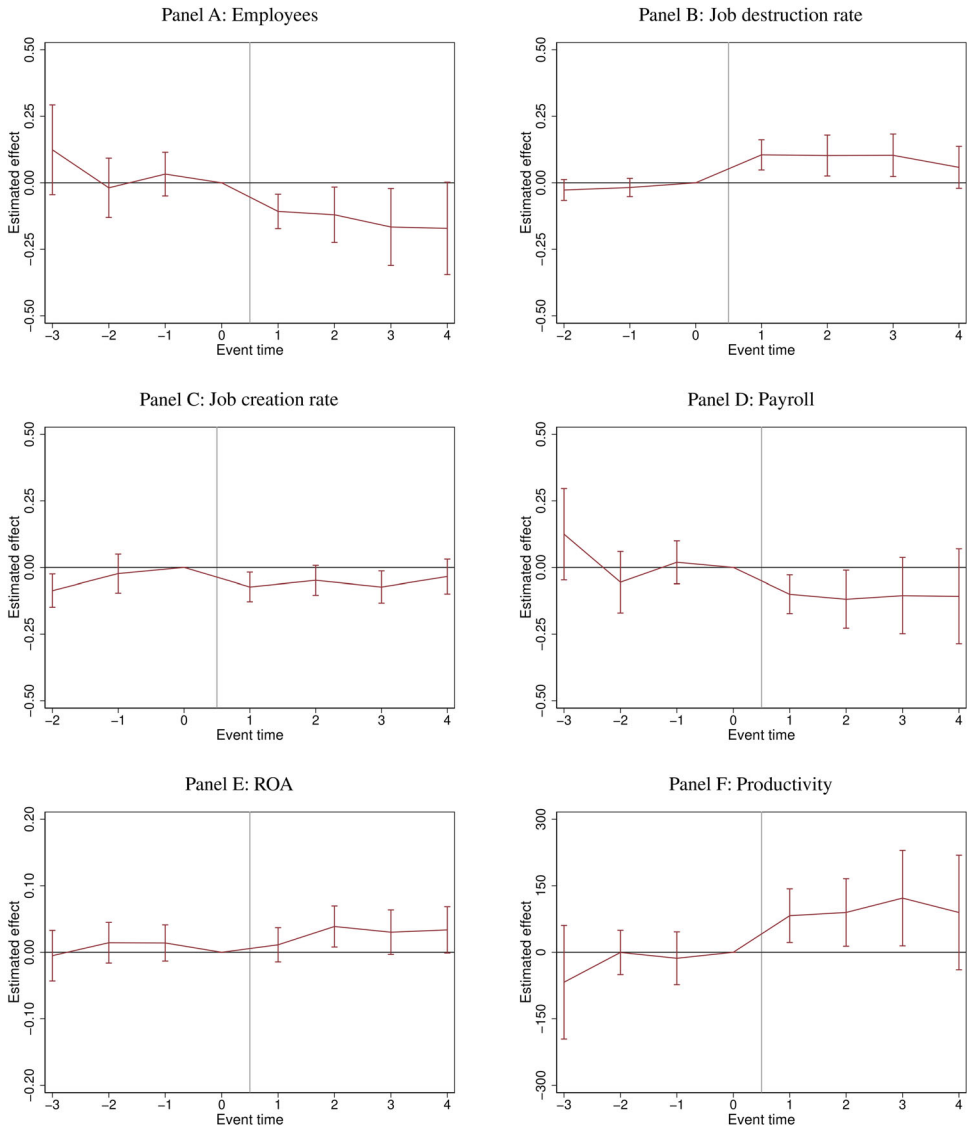
Figure 5. Effects of privatization on firm-level outcomes. The figures display dynamic difference-in-differences estimates relative to the year prior to the privatization (event time 0) using the model in equation (3) adjusted to account for dynamic effects. The vertical bars display 95% confidence intervals using robust standard errors clustered at the firm level. Variable descriptions are available in Table V. (Color figure can be viewed at wileyonlinelibrary.com)

broadly consistent with the existing literature. For instance, Megginson and Netter (2001) survey many firm-level studies on privatization and conclude that productivity and profitability usually increase. Our results also align well with those of D'souza and Megginson (1999) on profitability and productivity

in 28 countries and those with LaPorta and Lopez-de Silanes (1999) in Mexico on profitability and employment.

B. Alternative Drivers of Productivity Improvement

While cutting labor costs can explain much of the increase in productivity following privatization, other mechanisms may be at play as well. We next consider three candidate explanations: labor quality engineering, financial engineering, and governance engineering.

B.1. Upgrading Labor Quality

The first alternative determinant of productivity gains that we consider is an upgrade in 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 provide evidence on this potential mechanism, we investigate whether hired workers are of higher quality post-privatization while those who separate are of relatively lower quality. To this end, we rely on measures of cognitive and noncognitive skills from the Swedish military draft test. Cognitive skills correspond to a person's capacity to execute a range of mental tasks closely related to learning and problem solving. In contrast, noncognitive 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 closely related to labor market success (Lindqvist and Vestman (2011), Baghai et al. (2021)). To measure the quality of hires and separations, we average the sum of cognitive and noncognitive scores for males of all hired workers and all separated workers and relate it to the average quality of workers at the firm.

Panels A and B in Figure 6 and columns (1) and (2) in Table VI display the results. We find no differential pretrends between treated and control firms. Overall, for the full postperiod, 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 4.8% increase in the quality of hires in the medium run (column (1), $t = 2.12$). 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 complementary driver of the explanation for the productivity gains we observe.

B.2. Financial Engineering

We next study changes in privatized firms' financial strategies. Significant shifts in capital investments could indicate increases in capital quality, which could help explain improved productivity postprivatization. Capital investments may also potentially account for workforce reductions to the extent that capital replaces labor in a company's production function. Furthermore, we
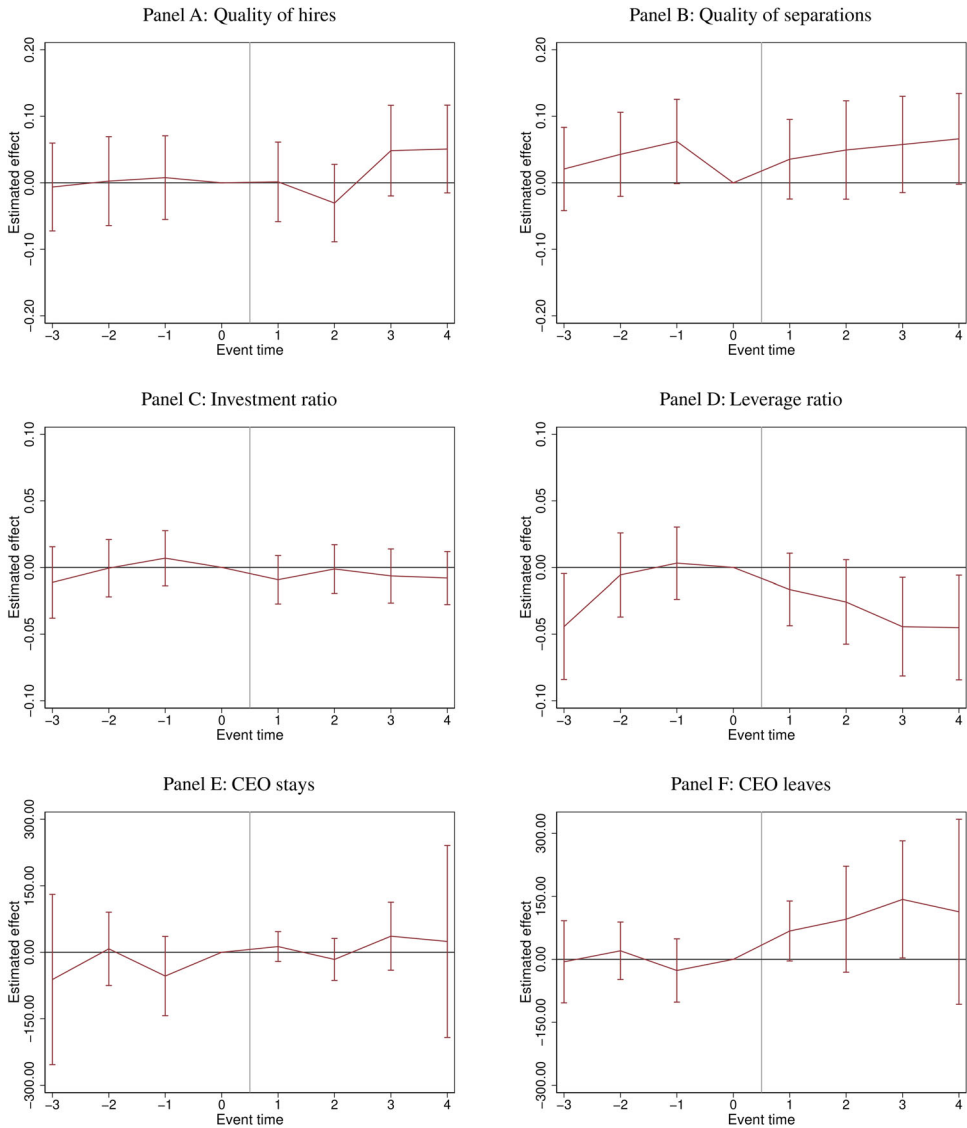
Figure 6. Effects on additional firm-level outcomes. The figures display dynamic difference-in-differences estimates relative to the year prior to the privatization (event time 0) using the model in equation (3) adjusted to account for dynamic effects. The vertical bars display 95% confidence intervals using robust standard errors clustered at the firm level. Variable descriptions are available in Table VI. (Color figure can be viewed at wileyonlinelibrary.com)

examine whether there are changes in leverage postprivatization. This might occur if the firm's risk profile changes under new ownership or if businesses increase leverage in an effort to gain increased bargaining power vis à vis employees. Panels C and D in Figure 6 and columns (3) and (4) in Table VI display

<div align="center">

Table VI
Additional Firm-Level Outcomes

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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 parentheses). See Table V for details on the sample and control variables. *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 noncognitive 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 capital expenditures 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 subsample of privatizations in which the CEO is not replaced in the postperiod. Specifications (1) to (4) use the full sample while the sample in specification (5) includes all control firms but only treated firms in which the CEO remained the same during the postperiod. The sample in specification (6) includes all control firms but only treated firms in which the CEO was replaced during the postperiod. Standard errors are clustered at the firm level.

Dependent Variable Sample Specification	Qual. Hir Full (1)	Qual. Sep Full (2)	Investment r. Full (3)	Leverage Full (4)	Prod. CEO stays (5)	Prod. CEO leaves (6)
Panel A: Average Effect						
Full period	0.012	0.020	−0.006	−0.022	39.942	97.875
	(0.72)	(1.15)	(−0.93)	(−1.61)	(0.81)	(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
Panel B: Dynamic Effect						
Short run (1–2 years)	−0.015	0.011	−0.005	−0.012	26.624	81.786
	(−0.78)	(0.52)	(−0.76)	(−0.91)	(0.64)	(1.82)
Medium run (3–4 years)	0.048	0.031	−0.007	−0.036	58.083	130.071
	(2.12)	(1.41)	(−0.91)	(−2.02)	(0.77)	(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 dep. var.	1.011	1.012	0.069	0.630	311.749	311.749
Observations	2,957	2,757	4,804	4,804	2,433	2,278

the results. The figures show no evidence of pretrends in the *investment ratio* (defined as capital investments over total assets) nor in the *leverage ratio* (total debt to total assets). Similarly, Panel A, columns (3) and (4), show no overall effects of privatization in the postperiod for the *investment ratio* ($t = 0.93$) or *leverage* ($t = 1.61$). There is, however, a 5.7% reduction in the l*everage ratio* in the medium run ($t = 2.02$). This impact is statistically but not economically significant. Thus, overall, postprivatization financial engineering has little ability to explain the productivity gains we observe postprivatization.[27]

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[27] Is the privatization event itself accompanied by raising additional capital? To address this question, we examine whether privatization results in a change in firms' total equity. Figure IA.9

B.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 in support of the importance of governance. Djankov and Murrell (2002) review the privatization literature and highlight the significant impact of replacing underperforming CEOs. Gupta (2005), for instance, examines the role of management in partial privatizations in India. CEOs in SOEs may be promoted based on connections rather than merit, leading to substantial improvements in managerial quality following 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 postprivatization productivity gains are related to CEO changes, we rerun our regressions using productivity as a firm outcome. However, we now differentiate between privatizations in which the CEO remains in charge and those in which the CEO is replaced.

The results are displayed in Panels E and F of Figure 6 and columns (5) and (6) in Table VI. Figure 6 reveals no differential pretrends in *productivity* and indicate that productivity increases postprivatization only if the CEO departs the company after privatization. Table VI demonstrates 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 IA.VII replicates the analysis for the other firm-level outcomes we consider and shows 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 changes appear to be associated with productivity improvements following privatization.

What could be possible reasons for the significant impact of CEO turnover? One possible channel is that management changes could disrupt implicit contracts between CEOs and workers, making labor cost reductions more probable. Managers may shift their alignment from workers to shareholders, resulting in layoffs to boost productivity and profitability (Shleifer and Summers (1988)). This could manifest as a reduction in *employment* and payroll, as observed when the CEO leaves (Panel C in Table IA.VII). An ownership change without a corresponding change in management would not necessarily lead to similar breaches of contract (Panel B in Table IA.VII). In addition, a new CEO may implement other changes at the firm. However, we do not observe that a CEO departure is associated with increased financial engineering (Panels B and C, columns (7) and (8), Table IA.VII) or with significant changes in hiring and firing strategies concerning the quality of hired versus separated workers (Panels B and C, columns (5) and (6), Table IA.VII).

presents trends over event time for total equity in the treated and control firms around the privatization event. Consistent with the fact that most privatizations in our sample are trade sales, we find no evidence that privatization events are associated with equity injections in privatized firms.

V. Robustness and Additional Analysis

A. Anticipation Effects

If retrenchment and restructuring programs are implemented prior to privatization to increase the selling price, or if certain workers choose to leave prior to privatization because they do not want to work for a private company or they dislike the uncertainty that comes with a new owner, then our sample of incumbent workers may be endogenous to the privatization event itself. Indeed, Lopez-de Silanes (1997) shows that a labor reorganization before privatization leads to a higher sales price, and Chong, Guillen, and López-de Silanes (2011) show that retrenchment programs before a privatization 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 prior to privatization (Munkhammar (2007)). These regulations are more likely to be binding for larger firms that are privatized. Rather, 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 restructure the company in any case and thus are not willing to pay a premium for a restructured firm. OECD (2003) also observes that restructuring prior to privatization is not common in trade sales, which is the typical method of privatization 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 ahead of privatization, suggesting that large retrenchment programs affecting workers in privatized firms tend not to be implemented.

Finally, in our main analysis we match treated and control workers one year before a privatization takes place, which mitigates any short-term anticipation effects. Our key results continue to hold when we instead match three years before privatization. However, matching three years before the privatization biases the estimates toward zero in our setting, as many workers may leave the firm prior to the privatization, thus, are not affected by the ownership change. Nevertheless, this analysis has the added benefit of accounting for potential separations before privatization due to potential retrenchment programs. Table IA.VIII reports the results.

B. 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 privatizations through sales to specific actors.

B.1. Partial Privatization

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

To investigate whether our results are robust to redefining our privatization dummy to focus on partial privatization, we identify partial privatization by looking at SOEs that get listed on a Swedish stock exchange while still remaining controlled by the state (i.e., private investors that own less than 50% of the firm). If the listed SOE is part of a corporate group, all of 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.[28] The final sample contains 28,694 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 VII presents the results (Figure IA.3 displays dynamic effects showing parallel pretrends in all outcomes). Column (1) shows that wages drop by 16.5% ($t = 4.11$) on average in the full postperiod. 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 unemployment incidence rising by 2.5 percentage points ($t = 5.77$) or 42%. Columns (3) and (4) show that government transfers increase by 16.7% ($t = 6.19$), reducing the impact of privatization on workers' income to -10.9% ($t = 3.25$). Thus, partial privatization have similar effects on workers, but with larger magnitudes than full privatization.[29]

B.2. Share-Issue Privatization

In this subsection, 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 of their performance and allows their compensation contracts to be tied to the stock price (Gupta (2005)). Thus, share-issue privatization could lead to stronger reallocation of labor relative to asset sales as managers may have stronger incentives to improve performance. Alternatively, share-issue privatization

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[28] 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.

[29] In the worker sample used in the main analysis, only 27 out of 553 privatizations are partially privatized before we define the treatment. Thus, partial sales play little role in the overall effects in our main analysis.

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Table VII
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 parentheses). The sample consists of treated workers employed one year prior to the partial privatization and matched control workers. See Table II for details on the variables and the controls used in the regressions.

Dependent Variable Specification	Wage (1)	Unemployment (2)	Transfers (3)	Total income (4)
Panel A: Average Effect				
Full period	−0.165	0.025	0.167	−0.109
	(−4.11)	(5.77)	(6.19)	(−3.25)
%-Change	−16.5%	41.7%	16.7%	−10.9%
Adjusted R^2	0.137	0.062	0.051	0.150
Panel B: Dynamic Effect				
Short run (1–2 years)	−0.061	−0.000	0.006	−0.037
	(−2.77)	(−0.10)	(0.29)	(−2.15)
Medium run (3–4 years)	−0.090	0.025	0.134	−0.073
	(−2.81)	(6.50)	(5.59)	(−2.57)
Long run (5–8 years)	−0.255	0.037	0.266	−0.163
	(−4.56)	(6.66)	(7.47)	(−3.48)
%-Change				
Short run	−6.1%	0.5%	0.6%	−3.7%
Medium run	−9.0%	41.7%	13.4%	−7.3%
Long run	−25.5%	61.7%	26.6%	−16.3%
Adjusted R^2	0.137	0.062	0.050	0.150
Mean dep. var.	8.086	0.060	0.387	8.166
Observations	681,113	681,113	681,113	681,113

tend to be more common among more profitable SOEs as governments want share-issue privatizations to succeed to build support for the privatization program among domestic shareholders (Megginson et al. (2004)). If a higher profitability correlates with a lower need for labor reallocation, workers in share-issue privatizations could do better than workers affected by privatizations via asset sales in the private capital market. Moreover, dispersed ownership through share-issue privatization tends to make agency problems worse and give more power to managers who may not be so keen on implementing unpopular reorganization efforts.

To investigate whether workers do better or worse under share-issue privatization, we split our sample of privatizations into share-issue privatization and asset sales. Columns (1) to (2) in Table VIII and Panels A and B in Figure IA.4 display the results. In all subsamples, we have parallel pretrends in outcomes. Column (1) of Table VIII shows that share-issue privatizations tend to be associated with an increase in unemployment by 2 percentage points (22.8%, $t = 7.15$), while workers' total income remains unchanged (4.1%, $t = 1.56$).

Table VIII

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 parentheses). Each subsample consists of treated workers employed one year prior to privatization and matched control workers. See Table II for details on the variables and the controls used in the regressions. *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, that is, the firm is privatized through a management buyout.

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)
Panel A: Unemployment								
Full period	0.021	0.010	0.032	0.005	0.010	0.013	−0.020	0.013
	(7.15)	(3.27)	(8.59)	(1.86)	(1.36)	(4.95)	(−0.96)	(5.21)
%-Change	22.8%	10.9%	32.0%	5.0%	10.2%	13.3%	−20.7%	13.9%
p-Value for diff.		0.008		0.000		0.651		0.110
Mean 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	0.083	0.071	0.081	0.072
Panel B: Total Income								
Full period	0.041	−0.060	−0.166	0.021	0.034	−0.039	0.073	−0.036
	(1.56)	(−2.51)	(−6.07)	(0.99)	(0.88)	(−1.76)	(0.54)	(−1.54)
%-Change	4.1%	−6.0%	−16.6%	2.1%	3.4%	−3.9%	7.3%	−3.6%
p-Value for diff.		0.000		0.000		0.022		0.435
Mean dep. var.	8.164	8.164	8.113	8.113	8.126	8.126	8.113	8.113
Adjusted R^2	0.136	0.129	0.135	0.135	0.129	0.129	0.134	0.129
Observations	888,859	1,233,108	912,620	1,209,347	759,558	1,362,409	713,262	1,408,705

Column (2) reveals that the effect on unemployment is half of that (1.0 percentage points) for non-share-issue privatizations (10.9%, $t = 3.27$). (The *p*-value for the difference is 0.008.) That workers do worse under share-issue privatization is consistent with the results of the partial privatization in the previous subsection.

B.3. Privatization through Sales to Specific Actors

Does the cost of privatization for workers differ depending on the buyer's ownership type? Existing literature finds 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 therefore be better positioned to undertake substantial reorganization, which might bring with it higher costs to workers.[30]

To investigate this dimension of heterogeneity, we perform three sample splits: domestic versus foreign buyer, financial versus nonfinancial buyer, and MBOs versus non-MBOs. Columns (3) to (8) of Table VIII display the full post-period results. Panels C to H in Figure IA.4 display the dynamic effects, showing that we have good pretrends for all outcomes.

Column (3) shows that foreign buyers lead to an increase in unemployment by 3.2 percentage points (32.0%, $t = 8.59$), accompanied by a decrease in the total income of 16.6% ($t = 6.07$). When the buyer is domestic, column (4) shows that unemployment increases by only 0.5 percentage points (5.0%, $t = 1.86$) and total income remains unchanged (2.1%, $t = 0.99$). The difference between the two groups is statistically and economically significant (p-values of 0.000) 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 the unemployment incidence (10.2%, $t = 1.36$) and total income (3.4%, $t = 0.88$) remain unchanged if the buyer is a financial buyer, whereas column (6) shows that unemployment increases (13.3%, $t = 4.95$) and total income remains unchanged (-3.9%, $t = 1.76$) for nonfinancial buyers. The difference between the two is not statistically significant for unemployment (p-value of 0.651), but there is a relative statistically significant difference for total income with a p-value of 0.022.

Finally, columns (7) and (8) show that MBOs appear to be accompanied by no statistically significant effects on unemployment (-20.7%, $t = 0.96$) or income (7.3%, $t = 0.54$). Thus, all the negative effects on workers come from non-MBO privatizations, where the unemployment increases (13.9%, $t = 5.21$) and income remains unchanged (-3.6%, $t = 1.54$). These results are 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, however, are not statistically significant (p-values of 0.110–0.435).

C. 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

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[30] 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)."

Table IX

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 parentheses). Each subsample consists of treated workers employed one year prior to privatization and matched control workers. See Table II for details on the variables and the controls used in the regressions. *TB* 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.

Dependent Variable	Unemployment			Total Income		
Split Specification	*TB* (1)	*High unemp.* (2)	*Recession* (3)	*TB* (4)	*High unemp.* (5)	*Recession* (6)
DDD	−0.003	0.011	−0.000	−0.045	−0.010	−0.015
	(−0.30)	(1.83)	(−0.05)	(−1.23)	(−0.26)	(−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. var.	0.091	0.134	0.091	8.227	8.107	8.211
Observations	1,414,270	1,414,270	1,414,270	1,414,270	1,414,270	1,414,270

between 1997 and 2004, so we investigate whether our results could be driven by what happened in these industries. To do so, we run a triple-difference regression that compares before versus after treated-control and non-TB versus TB industries. The results are presented in IX, column (1), for unemployment and, column (4), for total income (see Figure IA.5 for trends over time). 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 macroeconomic conditions. We therefore run triple-difference regressions that distinguish between privatizations in regions with above versus 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 IX (see Figure IA.5 for trends over time). 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 examine labor income and unemployment over an individual's career. Here, we 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 improve productivity but that negatively impact workers. 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 upgrades 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 postprivatization, we perform a postprivatization analysis on how the composition of departing workers varies between treated and control groups. The sample here therefore includes only those employees that exit the firm they are employed at time $t - 0$. We take this approach because the difference-in-differences framework does not allow to condition on posttreatment outcomes. Table IA.XIII shows negligible differences in who exits the firm across treated and control groups in terms of age, skill, tenure, and gender.

D. Policy

D.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 the prices or quality of goods and services, and the long-term dynamic effects on the economy. In addition, 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. Capturing all of these dimensions is challenging, however, even with the detailed data at our disposal. Our paper provides estimates of the direct costs to workers in terms of income losses and of the benefits to firms in terms of productivity gains (Tables II and V). Moreover, our results on other worker-level outcomes (Table III) indicate that the costs for workers are paid primarily in terms of lost income and not other nonlabor outcomes.

Table IA.XVI summarizes per-worker costs and per-worker productivity gains and calculates a few key ratios that we summarize here. First, workers

observe costs in the form of lost income, resulting mainly from increased unemployment. Table II 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 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 V) and by 11.5% using logged value-added per employee, which puts less weight on outliers, see Table IA.VI (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 firm owners through the increased revenues generated by 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 10% to 30% of the productivity gains per worker to firms. This implies that workers could be compensated to the full extent of their losses, and there would still be a positive surplus left over to share between the new owners of the firm and the government.

D.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 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. In addition, various schemes for employee participation in privatization have been introduced in many OECD countries. As noted in Section I, privatization in Sweden typically is 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, Kluve, and Weber (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.

VI. Conclusion

In this paper, we exploit Swedish registry data to present new evidence on the costs and benefits of privatization, in particular on the impact of privatization on workers and firms in advanced economies. Our key economic insight is that firm-level improvements postprivatization are primarily accompanied by 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.

Acknowledgments

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Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's website:

Appendix S1: Internet Appendix.
Replication Code.