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DOES PRIVATIZATION HURT WORKERS? EVIDENCE FROM COMPREHENSIVE MANUFACTURING FIRM DATA IN HUNGARY, ROMANIA, RUSSIA, AND UKRAINE

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We analyze the effects of privatization on wages and employment using comprehensive manufacturing firm panel data in Hungary, Romania, Russia, and Ukraine. Contrary to workers' fears, aggregate correlations, and ordinary least squares estimates, our fixed-effects and random trend models show little effect of domestic privatization, and they provide some evidence of positive foreign effects on both wages and employment. The negligible employment impact of domestic privatization results from large, positive, but offsetting effects on efficiency and scale in Hungary and Romania, and from small effects of both types in Russia and Ukraine. The positive foreign effect on employment reflects scale expansion dominating the efficiency effect. The results imply that efficiency-enhancing owners may sometimes be beneficial for workers.

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Д. Браун, Дж. Эрл, А. Телегди. Вредит ли приватизация рабочим? Результаты анализа представительных панельных данных по промышленным предприятиям в Венгрии, Румынии, России, Украине. Препринт WP3/2006/01. — М.: ГУ ВШЭ, 2006. — 36 с. (in English).

В работе анализируется влияние приватизации на заработную плату и занятость на основе представительных панельных данных по промышленным предприятиям в Венгрии, Румынии, России и Украине. В отличие от распространенных опасений работников, простых корреляций и МНК-оценок, модели с фиксированными и случайными эффектами не показывают значимого эффекта от приватизации российскими собственниками. В то же время есть позитивное влияние иностранных собственников на зарплату и занятость. Отсутствие влияния на занятость при приватизации отечественными собственниками складывается в результате значительных позитивных, но взаимопогашающих влияний на эффективность и численность в Венгрии и Румынии, и соответствующих слабых влияний в России и Украине. Позитивное влияние на занятость при приватизации иностранцами отражает эффект экспансии занятости, доминирующей над эффектом роста производительности. Результаты означают, что рабочим могут быть выгодны собственники, повышающие эффективность производства.

1. Introduction

The greatest opposition to privatizing a firm usually comes from the firm's own workers, fearful of wage cuts and job losses. The fears are consistent with standard economic analyses of privatization, whereby new private owners reduce costs in response to harder budget constraints and stronger profit-related incentives (e.g., Vickers and Yarrow, 1991; Shleifer and Vishny, 1994; Boycko, Shleifer, and Vishny, 1996; Aghion and Blanchard, 1998). Discussions of this "efficiency effect" of privatization, however, implicitly assume that the firm's output remains constant or at least does not increase. But lower costs may increase the firm's market share as well as total quantity demanded for the industry. Moreover, the new private owners may be more entrepreneurial in marketing, innovation, and entering new markets (Frydman, Gray, Hessel, and Rapaczynski, 1999). In such cases, the firm's output may tend to rise, and if this "scale effect" dominates then privatization could cause a net employment rise.

The implications of privatization for wages are also ambiguous. New owners may reduce wages as part of a general cost-cutting policy, and they may expropriate workers' rents, similar to a hostile takeover (e.g., Shleifer and Summers, 1988; Gokhale, Groshen, and Neumark, 1995). On the other hand, if the firm expands, it may have to offer higher wages to attract new workers. New private owners may also be more likely to adopt skill-biased technologies, resulting in a compositional shift toward higher-paid workers. Privatized firms are freer to use incentive pay, which could raise wages if, for example, some form of efficiency wages would reduce quits or enhance effort. Wages may also rise if privatization permits the firm to exercise market power and rents are shared with workers. Depending on the relative strength of these factors, wages may either rise or fall as a result of privatization.

Not only does theoretical analysis fail to provide definitive predictions on the wage and employment effects of privatization, but also the existing empirical evidence is quite scant.[1] Research has been hampered by small sample sizes, short time series, and the difficult problem of defining a comparison group of firms. In the first study of effects of privatization on employment and wages, for example, Haskel and Szymanski (1993) analyze 14 British publicly owned companies, of which only four were actually privatized (the others experienced liberalization). Kikeri (1998) summarizes a number of case studies, mostly carried out by the World Bank, of privatization effects on labor in several developing economies. The largest sample in the existing literature is found in La Porta and Lopez-de-Silanes'

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[1] The little attention to the effects of privatization on employment and wage contrasts with the voluminous literature on the consequences for firm performance; see the recent surveys by Megginson and Netter (2001) and Djankov and Murrell (2002).

(1999) analysis of 170 privatized firms in Mexico, although the post-privatization information is limited to a single year. Other studies have sometimes included employment as a possible indicator of firm performance, but only one also examines wages.[2] Overall, the results from this previous research are inconclusive, containing both negative and positive estimates.[3]

In this paper, we undertake an empirical analysis of the effects of privatization on the wage bill, employment, and wage rates of firms in Hungary, Romania, Russia, and Ukraine − where thousands of businesses were privatized in a relatively short period of time during the 1990s. These four countries span the range of transition economies in terms of evaluations of their reforms, with Hungary considered one of the most successful, Russia and Ukraine among the least successful, and Romania somewhere in the middle.[4] Figure 1 provides some initial evidence on the relationship of privatization and the wage bill, graphing the evolution of the average wage bill and percentage of firms privatized in our data. At this aggregate level of analysis, a strong negative correlation is evident in all four countries, which would seem to corroborate workers' fears and most economists' expectations. A number of other events which could affect the wage bill occurred during these years (e.g., macroeconomic shocks and market liberalization), however, and the firms selected for privatization may have been declining for extraneous reasons. To deal with these potentially confounding factors and estimate the causal effects of privatization on workers, one must analyze microdata (Figure 1).

For this purpose, we have assembled much longer time series and more comprehensive coverage than was available in earlier research. The time series information on individual manufacturing firms runs from as early as 1985, when the Communist Party still held power in all these countries, until 2002, well after most firms had been privatized. The coverage of our data is quite comprehensive, including most manufacturing firms inherited from the former planned economy, both those

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[2] Studies of firm performance that also estimate employment equations include Megginson, Nash, and van Randenborgh (1994), Boubakri and Cosset (1998), D'Souza and Megginson (1999), Frydman, Gray, Hessel, and Rapaczynski (1999), and Lizal and Svejnar (2002); two of these find a positive effect of privatization on employment, two no effect, and one a negative effect. Only Lizal and Svejnar (2002) examine wages, finding a short-term negative effect and longer-term positive effect, particularly in foreign-owned firms.

[3] Another related branch of literature examines differences in wages for workers employed in public and private sectors (e.g., Gyourko and Tracy, 1988; Brainerd, 2002; Lee, 2004). While a common approach to identify the public wage differential in this research is to examine wage changes of workers who switch sectors, our approach is instead to examine firms that switch sectors.

[4] The World Bank's (1996) four-group classification of 26 transition economies, for example, puts Hungary in the first group of leading reformers, Romania in the second group, Russia in the third, and Ukraine in the last. Similarly, the EBRD's annual indicators of "progress in transition" invariably place Hungary at or close to the top of all transition economies; according to the overall "institutional performance" measure in EBRD (2000), Hungary is ranked first, with a score of 3.5 overall, while Romania is awarded 2.3, Russia 1.9, and Ukraine 2.1.
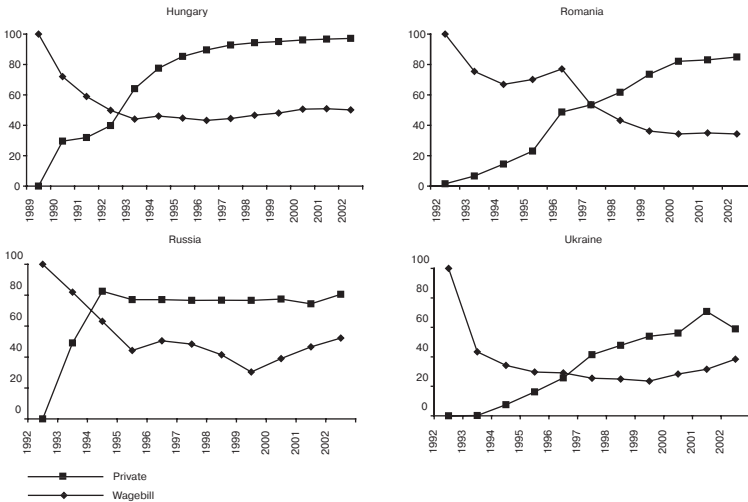
Figure 1. Evolution of Average Wage Bill and Private Ownership

Notes: The graphs show the the average real wage bill and percent of majority private firms, calculated from our data. The real wage bill is set at 100 in 1989 in Hungary and 1992 for Romania, Russia and Ukraine.

slated for privatization and those remaining under state ownership. In all four countries, we have comparable information on average employment and the total wage bill for each firm on an annual basis, and the ownership data permit a distinction not only between privatized and state-owned firms but also between firms privatized to foreign investors and those privatized to domestic companies and individuals; they also allow us to infer the precise year in which ownership change occurred. Unfortunately, the data do not contain measures of other variables that may have been influenced by privatization, such as worker turnover or fringe benefits, nor are we able to measure different types of employment or the fate of displaced workers. For investigating the impact of privatization on a firm's wages and employment, however, these panel data provide a nearly ideal setting.[5]

Our basic aim in this paper is to provide robust estimates of the wage bill, employment, and wage effects of privatization using much larger and longer panels than were available to earlier researchers, but we also exploit the advantages of our data in several ways. The first concerns the relative effects of foreign and domestic ownership. Workers appear to fear foreign much more than domestic investors, but there is little evidence whether this perception is warranted. Second, we investigate

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[5] This paper considers only the direct effect of privatization on a firm's employment and wage, ignoring any indirect effects working through spillovers in the region, industry, or related industries, a topic we leave for future research.

the dynamics of employment and wages before and after privatization. Estimates of pre-privatization effects are useful for taking into account possible biases in the selection of firms to be privatized and for assessing the extent to which anticipation of privatization may affect employment and wage determination; indeed, some previous studies (e.g., La Porta and Lopez-de-Silanes, 1999; Chong and Lopez-de-Silanes, 2002) find that employment tends to decline in firms awaiting privatization. The post-privatization dynamics shed light on the speed of the changes and the long-term consequences experienced by employees.

Finally, we apply econometric methods developed for dealing with selection bias in labor market program evaluations. The long time series in our firm-level data permit us to estimate regression models including not only firm fixed effects but also firm-specific time trends, sometimes referred to as "random trend models."[6] Applied to the privatization context, these models control not only for fixed differences among firms but also differing trend productivity growth rates that may affect the probability of privatization and whether the new owners are domestic or foreign investors. We compare alternative estimators using several specification tests, including the Heckman-Hotz (1989) "pre-program" test which measures selection bias under an estimator as the difference in the dependent variable prior to treatment between the treated and comparison groups. In the privatization context, this test must be evaluated before the privatization year to avoid possible contamination through anticipatory effects.

The next section describes our data for each of the four countries, and Section 3 discusses their privatization programs. Section 4 explains the estimation procedures, and Section 5 presents the results. Conclusions are summarized in Section 6.

2. Data

Our analysis draws upon annual data for most of the manufacturing firms inherited from the socialist period in each of the four countries we study. The sources and variables are quite similar across countries. The State Committees for Statistics in Russia and Ukraine (*Goskomstat* in Russia and *Derzhkomstat* in Ukraine) are the successors to the branches of the corresponding Soviet State Committee. They compile the basic databases for our analysis in these countries, the annual industrial enterprise registries. These are supplemented by by joint venture registries that are available in Russia and a database from the State Property Committee in Ukraine,

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[6] Ashenfelter and Card (1985) and Heckman and Hotz (1989) use the random trend model in evaluating training programs, while Jacobson, LaLonde, and Sullivan (1993, 2005) apply it to the effects of job displacement and community college on wages. To our knowledge, however, no previous study of privatization, corporate governance, or firm performance has used this method.

which we have linked together across years. The industrial registries are supposed to include all industrial firms with more than 100 employees plus those that are more than 25 percent owned by the state and/or by legal entities that are themselves included in the registry. In fact, the practice seems to be that once firms enter the registries, they continue to report even if the original conditions for inclusion are no longer satisfied. The data may therefore be taken as corresponding to the "old" sector of firms (and their successors) that were inherited from the Soviet system. Certainly with respect to this set of firms, the databases are quite comprehensive.

The Russian and Ukrainian databases include the years 1989 and 1992-2002.[7] Employment in Russia in all years and in Ukraine from 1989 to 2000 is defined as the average number of registered employees in industrial production divisions of the enterprise; this definition includes non-production workers but excludes employees in "nonindustrial divisions," most of which provide employee benefits. Although information on the size of these divisions is scant, by all accounts they tend to be very small fractions of total firm employment. In Ukraine, the available employment variable includes employees in all divisions in the years 2001 and 2002. The wage variable in Russia in all years and in Ukraine for 1992-1999 refers to the wage bill for registered employees of industrial divisions, including both monetary and in-kind accrued payments (the latter valued at "market prices"), divided by employment. For 2000-2002, the Ukrainian concept covers all employees. Wages in both cases are deflated by national consumer price indices.

The Hungarian and Romanian data tend to be more similar to each other than to those in the Soviet successor states. In both cases, the basic data source is balance sheets and income statements associated with tax reporting: the National Tax Authority in Hungary and the Ministry of Finance in Romania. These data are available for all legal entities engaged in double-sided bookkeeping. In addition, the Romanian data are supplemented by the National Institute for Statistics' enterprise registry and the State Ownership Fund's portfolio data. The Hungarian data are available for 1986-2002; the Romanian for 1992-2002. The employment definitions in both cases refer to average employment over the year, and wages are defined as the annual wage bill (including monetary and non-monetary benefits) for all employees divided by employment. Again, they are deflated by national consumer prices.

In order to make the samples comparable across countries, some truncation of the Hungarian and Romanian data were necessary. Firms are included if at first observation they operate in an industrial sector, because the Russian and Ukrainian data do not include non-industrial firms, and they appear to also exclude industrial firms that were classified as non-industrial when they first appeared in the

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[7] The Russian employment data also include 1985-1988 and 1990-1991, but wages are unavailable in those years. We use a consistent sample across equations, but the results are quite similar with the full employment sample in Russia.

data. In all four countries, the data are restricted to manufacturing (NACE 15-36) because some of the nonmanufacturing industrial sectors (chiefly mining) are defined noncomparably in the Russian and Ukrainian classification system (*OKONKh*).[8] We include only "old" firms, defined as existing prior to 1992 (1990 in Hungary) or any state ownership at first observation, both because the Russian and Ukrainian data do not cover most de novo firms, and because de novo firms are not at risk of privatization. In addition, privatized firms are included only if they are majority state in their first observation in the regressions, so that the base category consists exclusively of state firms.[9]

The total number of firms and their total employment in 1994, as a fraction of all old firms and their corresponding employment, are shown in Table 1. Missing values do not reduce the sample greatly in any country, and we have no reason to expect that the sample is biased in any particular direction. The numbers of firms appearing in the samples are 2,388 in Hungary, 2,475 in Romania, 18,578 in Russia, and 5,976 in Ukraine. A total of 229,574 firm-years are available for analysis. Among privatized firms, an average of 3.7 Hungarian, 5.4 Romanian, 2.9 Russian, and 6.3 Ukrainian observations per firm are included pre-privatization, and 7.9 Hungarian, 4.8 Romanian, 5.3 Russian, and 4.1 Ukrainian observations per firm are included post-privatization.

Table 1   **Sample Sizes, 1994**

	Number of firms	Percent of all old firms	Total employment	Percent of old firm employment
Hungary	1,541	66.6	318,343	73.3
Romania	2,061	84.0	1,978,895	96.2
Russia	14,377	92.4	10,238,688	96.5
Ukraine	5,645	96.5	3,358,955	98.1

Note: The table shows the number of manufacturing firms available for analysis and their total employment in 1994 as a percentage of the set of all old firms (manufacturing firms inherited from the socialist period) and the total employment of that set of firms, respectively.

Table 2 provides summary statistics for employment and wages. The data imply that average employment size has declined substantially in all four economies (although most in Romania). Real wages have increased in Hungary and Romania and fallen in Russia and Ukraine.

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[8] Recycling (NACE 37) is also excluded because of noncomparability with the *OKONKh* classification system.

[9] In Russia and Ukraine, privatization started only after 1992, so that firms that existed before this year in our data must be old. The fact that in Romanian data start in 1992 does not reduce our old sample, as in this country privatization started only in that year.

Table 2   ***Mean Employment and Wages in the First Year of Analysis, 1994, and 2002***

	Employment			Wage		
	First year	1994	2002	First year	1994	2002
Hungary	613.7	206.6	165.3	981.6	1,344.7	1,518.0
	(1,214.7)	(594.1)	(422.9)	(352.9)	(930.5)	(1,896.3)
Romania	1234.0	960.2	414.3	60,847.6	52,121.8	69,920.5
	(2,169.9)	(2,258.3)	(924.1)	(25,172.5)	(22,946.2)	(483,597)
Russia	621.4	712.2	506.6	65,814.2	44,343.5	40,168.7
	(1296.2)	(2277.0)	(1999.4)	(16,826.7)	(23,402.3)	(24,503.4)
Ukraine	805.2	595.0	472.0	9,516.6	6,193.0	6,708.8
	(1,863.9)	(1,524.2)	(1,901.1)	(1,603.4)	(2,951.1)	(5,819.7)

Note: The first year of analysis is 1986 in Hungary, 1992 in Romania, and 1989 in Russia and Ukraine. Wage is annual, expressed in constant 2002 prices (thousands of HUF for Hungary, thousands of ROL for Romania, RUB for Russia, and UAH for Ukraine). Precise definitions and sources are provided in the data appendix. Standard deviations are shown in parentheses. NA = not available.

These data have been extensively cleaned to remove inconsistencies and to improve longitudinal linkages that may have been broken due to change of firm identifier from one year to the next (associated with reorganizations and changes of legal form, for instance). The inconsistencies were evaluated using information from multiple sources (including not only separate data providers, but also previous year information available in Romanian balance sheets and Russian and Ukrainian registries). The longitudinal linkages were improved using all available information, including industry, region, size, multiple sources for the same financial variables, and some exact linking variables (e.g., firm names and addresses in all countries except Hungary, where this information was not available) to match firms that exited in a given year with those that entered in the following year.[10] Although this issue has not received much attention in previous research, it is clear that accurate and complete links are crucial to any identification strategy such as ours that requires observations both before and after privatization. In some cases, however, it proved impossible to link large apparent exits and entries across years, and we have little doubt that even after all of our efforts that the links are still incomplete. This data problem, which is common in longitudinal data, is a caveat for our analysis.

Next we turn to a brief description of the privatization policies in the four countries as well as a presentation of the privatization ownership outcomes.

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[10] In Hungary, we also used a separate Central Statistical Office dataset with information on reorganizations that broke links across years.

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3. Privatization Policies and Their Implications

This section develops hypotheses on the effects of privatization on workers, with particular attention paid to the relative strength of the effects across countries that may be associated with differences in the design of the privatization programs.

The methods and tempos of large enterprise privatization differed quite significantly across the four countries. Hungary got off to an early start in ownership transformation and maintained a consistent case-by-case method throughout the transition. At the very beginning, the transactions tended to be "spontaneous," initiated by managers, who were also usually the beneficiaries, sometimes in combination with foreign or other investors (Voszka, 1993). From 1991, the sales process became more regularized, generally relying upon competitive tenders open to foreign participation although management usually still had control over the process. Unlike many other countries, there were no significant preferences given to workers to acquire shares in their companies, nor was there a mass distribution of shares aided by vouchers. Hungarian privatization thus resulted in very little worker ownership (involving only about 250 firms), very little dispersed ownership, and instead concentrated blockholdings, with a large foreign share. Although the process appeared at times to be slow and gradual, in fact it was completed earlier than in most other East European countries.

In Romania, by contrast, the early attempts to mimic voucher programs and to sell individual firms produced few results, and privatization really began only in late 1993, first with the program of Management and Employee Buyouts, and secondly with the mass privatization of 1995-96 (Earle and Telegdy, 1998). The consequences of these programs were large-scale employee ownership and dispersed shareholding by the general population, with little foreign involvement. Beginning in 1997, greater efforts were made to involve foreign investors, and blocks of shares were sold both to foreigners and domestic entities (Earle and Telegdy, 2002). The result was a mixture of several types of ownership and a moderate speed compared to Hungary.

Russia and Ukraine's earliest privatization experiences have some similarities to the "spontaneous" period in Hungary, as the central planning system dissolved in the late 1980s and decision-making power devolved to managers and work collectives. The provisions for leasing enterprise assets (with eventual buyout) represented the first organized transactions in 1990-1992, but the big impetus for most industrial enterprise privatization in Russia was the mass privatization from October 1992 to June 1994, when the bulk of shares were transferred primarily to the concerned firms' managers and workers, who had received large discounts in the implicit prices they faced (Boycko, Shleifer, and Vishny, 1995). Some shares (generally 29 percent) were reserved for voucher auctions open to any participant, and these resulted in a variety of ownership structures, from dispersed outsiders holding their shares through voucher investment funds to domestic investors who acquired significant blocks; sometimes

managers and workers acquired more shares through this means, but there were few cases of foreign investment. Blockholding and foreign ownership became more significant through later sales of blocks of shares and through secondary trading that resulted in concentration. Ukraine followed Russia's pattern at a somewhat slower pace. In both countries, the initial consequence was large-scale ownership by managers and workers, some blockholding by domestic entities, and continued state ownership. Subsequently, blocks formed and foreigners made partial inroads.

These general patterns are reflected in Table 3, which contains our computations of private ownership, defined here as a strict majority of shares held in private hands, based on our regression samples.[11] Ownership is measured at the reporting date, the end of each calendar year. Privatization is therefore measured as a change in ownership type from the end of one year to the end of the next. As of late 1992, 36.4 percent of the Hungarian firms had already been privatized, while privatization had not yet started in Romania, Russia, and Ukraine. By the end of the period, most firms had been privatized in all four countries, although there remain enough state-owned firms in each country to serve as a control group in our estimations.[12]

Table 3   ***Percentage of Firms Privatized—Majority Private and Majority Foreign***

		1992	**1994**	**2002**
Hungary				
	Private	36.4	90.3	93.2
	Foreign	4.7	13.5	15.8
Romania				
	Private	0.0	4.7	84.6
	Foreign	0.0	0.1	5.3
Russia				
	Private	0.0	79.5	70.0
	Foreign	0.0	0.4	0.4

[11] The Russian data do not contain an ownership variable prior to 1993, nor, unlike the other countries, do they distinguish between minority and majority shares, but virtually all the privatizations in our data are mass privatizations (not lease buyouts), so the earliest they could have taken place was October 1992, and other sources suggest that nearly all of these led to majority private ownership (e.g., Boycko, Shleifer, and Vishny, 1995).

[12] We assume a single change of ownership and recoded cases of multiple switches to the modal category after the first change (ties were decided in favor of private and foreign, unless only two years of data were available). In Hungary there were 71 cases, in Romania 15, and in Ukraine 4. Russia had 2,811 firms private since 1995 reclassified as state in 2000 or 2001, when ownership codes changed drastically; such mass renationalization did not occur, so our recoding corrects this problem. The nonmonotonicity of percent privatized in Table 3 is due to split-ups of state-owned firms.

Table 3

	1992	**1994**	**2002**
Ukraine			
Private	0.0	7.9	81.3
Foreign	0.0	0.1	1.5

Note: "Private" refers to firms with more than 50% privately held shares. "Foreign" refers to privatized firms with more than 50% foreign-owned shares. The residual category consists of privatized firms that are not majority foreign; most of these are majority-owned by domestic private owners, but some of them also have minority foreign ownership.

The table also contains the percentage of firms majority privatized to foreigners.[13] This fraction is by far the highest in Hungary, reaching nearly 16 percent of all entities by the end of our observation period. In Romania, the percentage reaches 5 percent, in Ukraine 1.5 percent, and in Russia just 0.4 percent. Given our sample sizes, these are sufficient to estimate coefficients. The residual category – the difference between private and foreign – consists of majority privatized firms that are not majority foreign. Because foreign investment in these countries usually takes the form of controlling investments, the residual firms are therefore usually majority owned by domestic private groups, and we label them "domestic" in the discussion below. But some cases of minority foreign investment (particularly in Hungary) are also included in this category.

The cross-country differences in privatization policy design could affect the measured impact of privatization on employment and wages. As we discussed in the introduction, two mechanisms may affect the firm's employment and wage setting: efficiency and scale effects. Worker-owners are likely to oppose labor-saving restructuring, and they are unlikely to have incentives or resources to expand output (Bonin, Jones. and Putterman, 1993). Outside blockholders, on the other hand, should favor cost-saving restructuring, particularly foreign investors with access to management skills, new technologies, and financing. These new owners are also more likely to respond to opportunities for expansion. Outsiders with small shareholdings may also benefit from efficiency improvements and scale expansion, but they are unlikely to influence the firm's behavior. Therefore, both the efficiency and scale effects of privatization are likely to be smallest for domestic owners in countries where insider and mass privatization predominated, larger in cases where domestic outsiders acquired blocks of shares, and largest for privatization to foreign investors. Because these mechanisms are offsetting, however, the relative magnitudes of the effects of different types of privatization on workers are ambiguous.

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[13] The Russian registries contain codes for state, domestic, joint ventures, and 100 percent foreign firms, but foreign shares are available only for a subset of firms in four years. We classify all joint ventures as foreign, but the results are very similar if we include only those foreign firms with a majority foreign share in at least one of the four years.

4. Empirical Strategy

We follow the broader literature on the effects of privatization in estimating reduced form equations, while trying to account for potential problems of heterogeneity and simultaneity bias (Djankov and Murrell, 2002; Megginson and Netter, 2001). A structural approach considering employment and wages as joint outcomes would be useful for some purposes, including for estimating changes in labor demand elasticities associated with privatization, but it raises thorny simultaneity issues, and thus we leave this for future research. The reduced form approach is a simpler starting point to gather evidence on the possible effects of ownership change. Estimating these effects nevertheless faces several potential problems.

To start with, aggregate and industry-specific shocks may affect employment, wages, and ownership. Moreover, the available deflators may not perfectly capture price changes. We therefore include a full set of industry-year interactions to control for such factors. A more difficult problem is the possibility of selection bias in the privatization process. Politicians, investors, and employees of the firms themselves all influence whether a firm is privatized. Politicians concerned with unemployment may keep firms with the least good prospects from being privatized so as to protect workers from layoffs and wage cuts, or the employees themselves may work to prevent privatization in such cases. Meanwhile, investors may only be interested in purchasing firms with better prospects. In some specifications we therefore include firm fixed effects (FE) so as to remove any time-invariant differences across firms. Since firms could also differ in their trend growth, we also estimate specifications including firm-specific trends (FE&FT).

The final estimation problem involves ambiguities in timing, both in the precise date of privatization (sometime in the year between observation dates) and in how long it takes for any effects to emerge. We address these issues by investigating the dynamics of the effect before and after the privatization year. Examining the pre-privatization dynamics provides information on whether firms were already adjusting employment and wages prior to the ownership change. Such anticipatory effects seem most likely to be negative, particularly if the expectation of post-privatization loss of control – or of job – leads to increased asset stripping by managers.[14] As discussed in more detail below, we conduct specification tests of whether the inclusion of firm fixed effects or both firm fixed effects and firm-specific trends can help control for this selection bias.

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[14] This argument is made by Aghion, Blanchard, and Burgess (1994) and Roland and Sekkat (2000). La Porta and Lopez-de-Silanes (1999) find negative anticipatory effects in their study of Mexican privatization.

The basic specification for the panel data model takes the following form for each country separately:

$$y_{it} = \mathbf{D}_{jt}\gamma_{jt} + \mathbf{w}_t\alpha_i + \theta_{it}\delta + u_{it}, \tag{1}$$

where i indexes firms from 1 to N, j indexes industries from 1 to J, and t indexes time periods (years) from 1 to T.[15] In alternative specifications, y_{it} is the natural logarithm of the firm's wage bill, employment, and average wage rate (per worker); because the ln(wage bill) is the sum of the other two variables, the linearity of our estimators implies that the coefficients on all independent variables also sum across equations. $\mathbf{D}_{jt}$ is a 1 x JT vector of industry-year interaction dummies; γ_{jt} is the associated JT x 1 vector of coefficients; and u_{it} is an idiosyncratic error.[16] The specifications of the other terms in the equation vary across specifications: $\mathbf{w}_t$ is a vector of aggregate time variables, α_i is the vector of associated individual-specific slopes, θ_{it} is the vector of ownership measures, and δ are the ownership effects of interest in this paper. In the OLS regressions $\mathbf{w}_t \equiv 0$. In the FE regressions $\mathbf{w}_t \equiv 1$, so that $\alpha_i \equiv \alpha_i$ is the unobserved effect. The FE&FT model specifies $\mathbf{w}_t \equiv (1, t)$, so that $\alpha_i \equiv (\alpha_{1i}, \alpha_{2i})$, where α_{1i} is a fixed unobserved effect and α_{2i} is the random trend for firm i. In practice, the FE&FT model is estimated in two steps, the first detrending all variables for each firm separately and the second estimating the model on the detrended data. Standard errors in the second step are adjusted for the loss of degrees of freedom associated with detrending.

We investigate three alternative specifications of the ownership variables θ_{it}. The simplest uses a single post-program dummy $Private_{it-1}$, defined $= 1$ if the firm is majority privately owned at the end of the previous year.[17] The coefficient of interest δ is then the mean within-country-industry-year difference in the dependent variable between firms majority private and majority state-owned. A second specification disaggregates ownership by nationality of the new private owners so that $\theta_{it} \equiv (Domestic_{it-1}, Foreign_{it-1})$, and $\delta \equiv (\delta_d, \delta_f)$ are the parameters of interest. Third, we estimate dynamic specifications, where dummy variables for the years before

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[15] $J=10$ industries, which we have chosen based on the trade-off between disaggregation and number of observations, specifying a minimum of 50 observations per year per country for each industry. T varies by country: 17 in Hungary, 11 in Romania, and 12 in Russia and Ukraine.

[16] Our estimates permit general within-firm correlation of residuals using Arellano's (1987) clustering method. The standard errors of all our test statistics are robust to both serial correlation and heteroskedasticity. See Kézdi (2003) for a detailed analysis of autocorrelation and the robust cluster estimator in panel data models.

[17] Our data do not specify an exact privatization date, and we infer privatization by observing a change in status from the end of one year to the next. This implies that the date on which the new private owners acquire formal authority (e.g., the first post-privatization shareholders' meeting) varies across firms, with some of them early in the final pre-privatization year. But some such assumption is necessary about which should be defined as the first "post" year, and we discuss this issue further in connection with the dynamics of the effect below.

and after privatization are interacted with indicators for whether the firm is ever domestically privatized or foreign privatized. Designating τ as the index of event time, the number of years since privatization, so that $\tau < 0$ in the pre-privatization years, $\tau = 0$ in the year in which ownership change occurs, and $\tau > 0$ in the post-privatization years, then $\theta_{it} \equiv (\textbf{Domestic}_{it\tau}, \textbf{Foreign}_{it\tau})$, $\delta \equiv (\delta_{\tau d}, \delta_{\tau f})$, and $\tau = -2, -1,$ 0, 1, 2, 3+, where 3+ is three and more years after privatization. We assume that privatization has no effect until 2 years before the ownership change appears in our data, so that $\delta_{\tau d} = \delta_{\tau f} = 0$ for $\tau < -2$.

We implement specification tests to help determine whether the OLS, FE, or FE&FT models are more appropriate. Our method generalizes the Heckman-Hotz (1989) "pre-program" test, which requires the same conditional expectation of the outcome for both treated and control groups in a single pre-treatment period. The assumption is that, once the test is satisfied, the only cause of differences between the two groups after that period is the treatment itself. We carry out F tests for the joint significance of the $\tau = -2$ and $\tau = -1$ dummies and t tests on the $\tau = -2$ dummies in the dynamic specifications. The F tests address Heckman, LaLonde, and Smith's (1999) concern that if a shock close to the treatment date affects one group but not the other, then the results will be highly sensitive to the choice of pre-treatment period. Studying each available pre-privatization year avoids this pitfall and does not require any a priori assumptions on which year is most appropriate. The t tests on the $\tau = -2$ dummies avoid the possibility that the $\tau = -1$ dummies display anticipatory effects of privatization. In addition to the pre-program test, we conduct F tests on the joint probability that all FEs = 0, and on the joint probability that all FTs = 0 in regressions with a single post-dummy for privatization. Finally, we conduct Hausman-type specification tests of the differences in the entire vector of coefficients resulting from adding FEs to the OLS specification, and from adding FTs to the FE specification.

To provide diagnostic information about the direction and magnitude of possible selection bias in the data, we estimate other variants of equation (1). Here we restrict the sample to state-owned firms (either never or not yet privatized, so that the single post dummy variable $Private_{it-1} = 0$ in this sub-sample), and we set $\mathbf{w_t} \equiv 0$. $\theta_{it} \equiv Pre\text{-}Private_{it}$ in one specification, and $\theta_{it} \equiv (Pre\text{-}Domestic_{it}, Pre\text{-}Foreign_{it})$ in another. We retain the full set of industry-year interactions, $\mathbf{D_{jt}}$, so that all effects are measured within industry-year cells. Under these assumptions, wage bill, employment, and wage differences between firms never privatized and those privatized in the future can be estimated from the equation

$$y_{it} = \mathbf{D_{jt}}\gamma_{jt} + \theta_{it}\delta + u_{it}. \tag{2}$$

In order to assess the relative importance of the efficiency and scale effects of privatization, we decompose the employment changes by estimating specifications of equation (1) where the dependent variables are the natural logarithms of output and

labor productivity (output divided by employment); and we similarly decompose the wage bill changes by estimation equations with unit labor cost (the wage bill divided by output) and output. Linearity of the estimators implies that the estimated wage bill effect of privatization is equal to the output effect minus the unit labor cost effect, the former measuring the scale of the firm, and the latter the efficiency effect. The employment effect of privatization can be decomposed analogously, with labor productivity serving as the efficiency measure. In these regressions, ownership is parameterized as single post-dummies for domestic and foreign privatization. FE and FE&FT models are estimated, and industry-year effects are included as controls.

The final estimation issue, which is relevant to all of these methods and all previous research on this topic, concerns the use of information only on reporting firms. An important problem is how to handle exit, given that, as discussed in Section 2, the permanent disappearance of a firm from the data may represent a genuine shutdown or merely a change in name or legal form (or some type of reorganization). In the former case, it would be desirable to count these as job losses, while in the latter, it would not. Despite extensive cleaning of the longitudinal linkages, we can distinguish shut-downs from boundary changes only imperfectly. To assess the potential of such exits to influence our results, however, we estimate probit equations similar in form to (1) except that the dependent variable is a dummy for exit (=1 if the firm exits) and industry and year dummies are included separately rather than as interactions with industry (because many industry-year cells contain no exits). The next section reports the results.

5. Results

We begin the analysis by exploring pre-privatization differences in wages and employment between firms that are eventually privatized and those that remain state-owned. Table 4 shows results from the estimates of Equation (2), where the sample contains firm-year observations when the firm is state owned. The estimated differences vary greatly across countries, ownership types, and dependent variables. Romanian and Hungarian firms that are domestically privatized by the end of the period tend to have much smaller wage bills than the average always state-owned firm, but the pre-domestic effect on the wage bill is positive in Russia and Ukraine. Pre-privatization employment shows a very similar pattern to the wage bill, except that the magnitude of the coefficients is smaller in each of the countries, except in the case of Romania. Wages, however, tend to be larger in firms to be privatized everywhere but for domestic firms in Hungary. The foreign results are much more consistent, as firms that will be foreign-owned have higher wage bills, employment, and wage rates than either pre-domestic firms or always state firms in all four countries.

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Table 4   ***Pre-Privatization Relative Wage Bill, Employment, and Wage***

	Hungary	**Romania**	**Russia**	**Ukraine**
Wage Bill				
Pre Private	−0.605**	−0.098	0.830**	0.227**
	(0.081)	(0.090)	(0.024)	(0.042)
Pre Domestic	−0.714**	−0.190*	0.827**	0.213**
	(0.081)	(0.090)	(0.024)	(0.042)
Pre Foreign	0.361*	0.976**	1.342**	1.038**
	(0.167)	(0.162)	(0.136)	(0.184)
Employment				
Pre Private	−0.582**	−0.154	0.722**	0.186**
	(0.080)	(0.082)	(0.021)	(0.037)
Pre Domestic	−0.678**	−0.243**	0.720**	0.175**
	(0.080)	(0.082)	(0.021)	(0.037)
Pre Foreign	0.263	0.791**	1.199**	0.837**
	(0.179)	(0.142)	(0.127)	(0.154)
Wage				
Pre Private	−0.023	0.065**	0.107**	0.041**
	(0.019)	(0.019)	(0.010)	(0.014)
Pre Domestic	0.036	0.053**	0.107**	0.038**
	(0.019)	(0.019)	(0.010)	(0.014)
Pre Foreign	0.099*	0.185**	0.143**	0.201**
	(0.041)	(0.035)	(0.038)	(0.067)
N	8,593	13,481	69,294	40,676

Note: The pre-privatization characteristics of firms subsequently privatized relative to enterprises always in state ownership are estimated as the coefficients on a group effect, Ever Private, in regressions also including industry-year interactions. The Ever Foreign and Ever Domestic effects are estimated analogously in equations disaggregating Private into Foreign and Domestic. Standard errors (corrected for firm clustering) are shown in parentheses. * = significant at 5-percent level. ** = significant at 1-percent level.

The results from estimating relation (1) with the natural log of the wage bill as the dependent variable are displayed in Table 5. Equations are fitted by OLS, fixed firm effect (FE), and firm-specific trends (FE&FT). Starting with the specification estimating the average post-privatization effect ($Private_{it-1}$), the OLS estimates of δ_p are negative in Hungary, positive in Russia and Ukraine, and essentially zero in Romania. Controlling for FEs and FTs changes the estimates dramatically: each significant coefficient drops close to zero, while the Romanian becomes large and significant in the FE but drops to -0.015 and loses significance in the FE&FT. The FE&FT coefficients are essentially zero in Hungary and Ukraine, but small and negative in Russia. The data thus suggest that on average privatization has had little effect on the wage bill. If the wage bill represents a summary indicator of worker welfare, our firm-level analysis does not support the common belief that privatization hurt workers.

Table 5   ***Estimated Wage Bill Effects of Privatization***

	Hungary	**Romania**	**Russia**	**Ukraine**
OLS	Private			
$\hat{\delta}_p$	−0.431**	−0.065	0.850**	0.146**
	(0.068)	(0.065)	(0.027)	(0.038)
R²	0.165	0.181	0.358	0.309
FE				
$\hat{\delta}_p$	0.038	0.187**	−0.052**	0.026
	(0.032)	(0.027)	(0.012)	(0.015)
R²	0.293	0.466	0.478	0.586
FE&FT				
$\hat{\delta}_p$	−0.008	−0.015	−0.026**	−0.008
	(0.023)	(0.019)	(0.008)	(0.013)
R²	0.080	0.430	0.194	0.285
OLS	Domestic and Foreign			
$\hat{\delta}_d$	−0.657**	−0.127*	0.844**	0.134**
	(0.069)	(0.066)	(0.027)	(0.038)
$\hat{\delta}_f$	0.848**	1.396**	1.823**	1.079**

Table 5

	Hungary	Romania	Russia	Ukraine
	(0.122)	(0.136)	(0.203)	(0.252)
$\Pr(\hat{\delta}_{f=\hat{\delta}_d})$	0.000	0.000	0.000	0.000
FE				
$\hat{\delta}_d$	−0.056	0.164**	−0.054**	0.020
	(0.034)	(0.027)	(0.012)	(0.015)
$\hat{\delta}_f$	0.735**	0.520**	0.396**	0.439**
	(0.066)	(0.084)	(0.083)	(0.141)
$\Pr(\hat{\delta}_{f=\hat{\delta}_d})$	0.000	0.000	0.000	0.003
FE&FT				
$\hat{\delta}_d$	−0.044	−0.024	−0.027**	−0.010
	(0.024)	(0.020)	(0.008)	(0.013)
$\hat{\delta}_f$	0.220**	0.116*	0.062	0.109
	(0.052)	(0.057)	(0.074)	(0.122)
$\Pr(\hat{\delta}_{f=\hat{\delta}_d})$	0.000	0.020	0.231	0.334
N	19,382	22,447	131,531	56,214

Note: Full sets of unrestricted industry-year dummies are included in the regressions. Private = 1 if the firm is majority private at end of year t-1. FE=specification including firm fixed effects; FT= all variables have been detrended using individual firm trends. Standard errors (corrected for firm clustering) shown in parentheses. * = significant at 5-percent level. ** = significant at 1-percent level.

Turning to the distinction between new domestic and foreign ownership, the domestic results tend to be similar to the private results, as domestic owners dominate in most privatized companies. The OLS estimates of δ_d are negative in Hungary and Romania and positive in Russia and Ukraine, but again the coefficients are reduced in magnitude when the FEs and FTs are included, the main exception being Romania, where as with δ_p the FE estimate is positive and the FE&FT is essentially zero. In Hungary, the domestic wage bill effect is negative although small (about −0.05) and statistically insignificant in both the FE and FE&FT specifica-

tions. As with δ_p, the Russian estimate of δ_d is negative and significant, but small (-0.027).

The effects of foreign privatization reported in Table 5 are estimated to be large and positive in the OLS and FE specifications in all four countries, the coefficients varying between 0.396 and 0.735. When trends are added, the coefficients fall, and they lose significance in Russia and Ukraine, but they remain positive in all four countries. Thus, contrary to the fears of both employees and policymakers, our data provide strong evidence that foreign owners increased the welfare of workers in the two Central and East European countries in our study, and in the two FSU republics the effect seem to be zero in the most pessimistic case. The difference between the domestic and foreign effects is highly statistically significant except in the Russian and Ukrainian specifications with firm-specific trends. In the remainder of the paper, we display only specifications with the domestic/foreign disaggregation, since the two ownership types clearly behave quite differently.

We next decompose the wage bill effect into its component parts in Table 6. Again, while the OLS estimates are usually large in magnitude and highly significant, the coefficients tend to be much smaller and less significant in the FE and FE&FT specifications. In Hungary, Russia, and Ukraine, domestic ownership has essentially no effect on employment. The only large (positive) effect is measured for Romanian employment in the FE specification, but it becomes negative when FTs are added. In Hungary and Russia, domestic privatization is estimated to reduce wages by about $3-5$ percentage points. In Romania, the FE specification also shows a small negative effect, but the coefficient is statistically insignificant, and it shrinks even more when FTs are added. In Ukraine, the wage effect is zero in both specifications. Foreign ownership effects are estimated to be positive for both employment and wages in every specification and in every country. The magnitudes are large and highly statistically significant in all OLS and FE specifications, and they remain so in the FE&FT for employment in Hungary and for wages in Hungary and Romania.

Table 6   ***Employment and Wage Effects of Domestic and Foreign Privatization***

	Hungary	**Romania**	**Russia**	**Ukraine**
OLS	Employment			
$\hat{\delta}_d$	-0.621**	-0.176**	0.764**	0.080**
	(0.067)	(0.060)	(0.022)	(0.029)
$\hat{\delta}_f$	0.367**	0.966**	1.432**	0.661**
	(0.117)	(0.122)	(0.166)	(0.170)
$\Pr(\hat{\delta}_f = \hat{\delta}_d)$	0.000	0.000	0.000	0.001
FE				

Table 6

	Hungary	Romania	Russia	Ukraine
$\hat{\delta}_d$	−0.030	0.187**	−0.007	0.017
	(0.035)	(0.026)	(0.006)	(0.009)
$\hat{\delta}_f$	0.428**	0.285**	0.152**	0.135
	(0.073)	(0.086)	(0.043)	(0.077)
$Pr(\hat{\delta}_f = \hat{\delta}_d)$	0.000	0.255	0.000	0.127
FE&FT				
$\hat{\delta}_d$	0.002	−0.030	0.005	−0.006
	(0.024)	(0.017)	(0.004)	(0.008)
$\hat{\delta}_f$	0.154**	0.000	0.043	0.030
	(0.050)	(0.068)	(0.041)	(0.070)
$Pr(\hat{\delta}_f = \hat{\delta}_d)$	0.003	0.662	0.358	0.614
OLS	Wage			
$\hat{\delta}_d$	−0.035	0.049**	0.080**	0.055*
	(0.020)	(0.015)	(0.011)	(0.017)
$\hat{\delta}_f$	0.481**	0.430**	0.391**	0.418**
	(0.036)	(0.050)	(0.074)	(0.123)
$Pr(\hat{\delta}_f = \hat{\delta}_d)$	0.000	0.000	0.000	0.003
FE				
$\hat{\delta}_d$	−0.027	−0.023	−0.047**	0.003
	(0.015)	(0.012)	(0.008)	(0.011)
$\hat{\delta}_f$	0.307**	0.235**	0.244**	0.304**
	(0.033)	(0.054)	(0.064)	(0.095)
$Pr(\hat{\delta}_f = \hat{\delta}_d)$	0.000	0.000	0.000	0.002
FE&FT				
$\hat{\delta}_d$	−0.045**	0.006	−0.032**	−0.004
	(0.016)	(0.013)	(0.007)	(0.011)
$\hat{\delta}_f$	0.066*	0.116*	0.019	0.079
	(0.033)	(0.057)	(0.063)	(0.097)
$Pr(\hat{\delta}_f = \hat{\delta}_d)$	0.001	0.060	0.419	0.397

Note: Foreign = 1 if the majority of the firm's shares are owned by foreigners in year t-1. Domestic =
= 1 if the firm was private in year t-1 but not majority-owned by foreigners. FE=specification including
firm fixed effects; FT= all variables have been detrended using individual firm trends. Standard errors
(corrected for firm clustering) are shown in parentheses. The P values for the F test on the difference
between the Foreign and Domestic coefficients are reported below the foreign standard errors. The
number of observations in each country is the same as in Table 5. * = significant at 5-percent level. ** =
= significant at 1-percent level.

The estimated coefficients from the dynamic FE and FE&FT specifications for employment and the wage rate are plotted in Figures 2 and 3.[18] Results are shown separately for domestic and foreign effects and by country. In each case, the general shapes of the FE and FE&FT are usually quite similar. The domestic privatization effects are generally small (less than 10 percent in magnitude) in both the pre- and post-privatization periods. The single exception concerns employment in the Romanian FE specification, where the average domestic effect three and more years after privatization jumps to 40 percent, although this is reduced to 7 percent when FTs are added. The domestic privatization effects exhibit negative trends only for wages in Hungary and Russia, but the coefficients are statistically insignificant in the FE&FT specification in Russia, and they are small in magnitude in both. The graphs also show some pre-privatization increase of wages in Hungary and Ukraine, which may reflect anticipatory effects of domestic privatization or some form of selection bias.
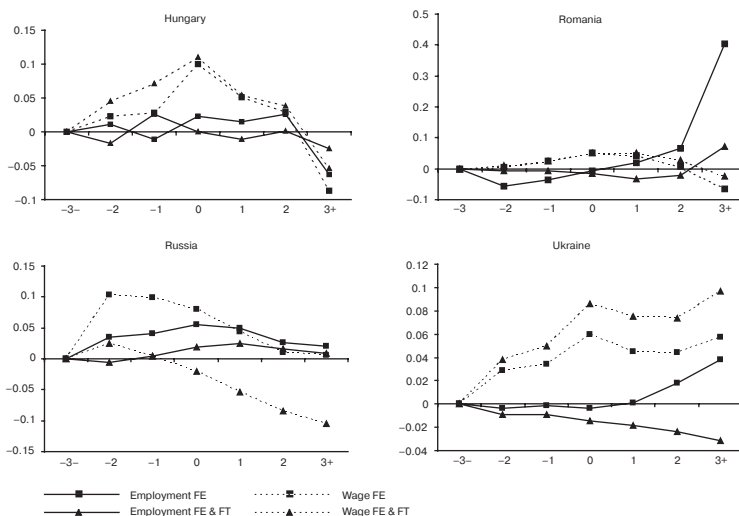

Figure 2. Dynamics of the Domestic Privatization Effects

Notes: The graphs present regression coefficients of interactions between dummy variables for the years before and after privatization and an indicator for whether the firm is ever domestically privatized (privatization year = 0). Full sets of unrestricted industry-year dummies are included in the regressions. FE=specification including firm fixed effects; FT = all variables have been detrended using individual firm trends.

[18] The graphs report only coefficient estimates, and only from FE and FE&FT specifications for the wage rate and employment, to save space. The full set of regression results, including standard errors, OLS estimates, and the wage bill, are available on request.

Consistent with the average effects in Table 6, the dynamics of the foreign privatization effects show much larger changes compared to the domestic effects. These changes emerge only gradually, however, not as one-time jumps just after privatization occurs. Starting from the privatization year, $\tau = 0$, nearly all the effects — for both employment and wages and for all four countries — trend upwards, some of them quite strongly. For example, the FE employment effect in Hungary rises from -0.2 at $\tau = 0$ to 0.4 at $\tau = 3+$, and in Romania from 0.2 to almost 0.6. The FE&FT results have a similar shape but are much smaller in nearly every case, and they are usually statistically insignificant. In no case, however, do the foreign dynamics exhibit negative trends.
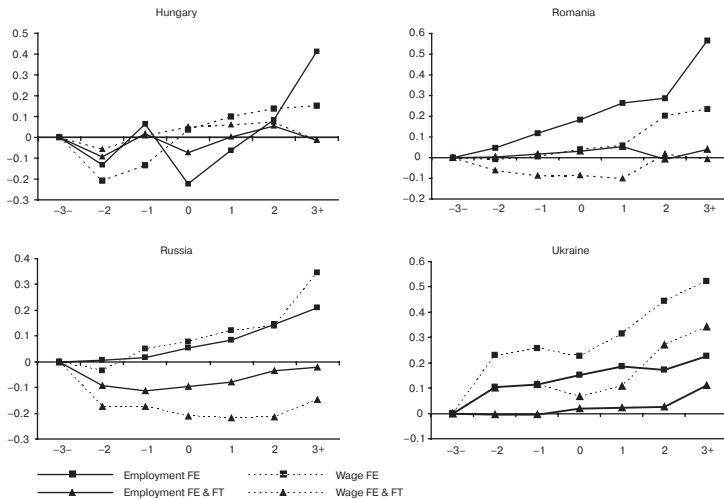

Figure 3. Dynamics of the Foreign Privatization Effects

Notes: The graphs present regression coefficients of interactions between dummy variables for the years before and after privatization and an indicator for whether the firm is ever foreign privatized (privatization year $= 0$). Full sets of unrestricted industry-year dummies are included in the regressions. FE=specification including firm fixed effects; FT = all variables have been detrended using individual firm trends.

These dynamic specifications are useful for carrying out specification tests on pre-privatization behavior, variants of the Heckman-Hotz (1989) pre-program tests. Table 7 shows the results of F tests of the joint probability that the privatization effects one and two years before privatization are different from zero.[19] The OLS

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[19] We also carried out t tests on the effect two years before privatization. The values of the tests and the coefficients (which are plotted in Figures 2 and 3) lead to qualitatively similar conclusions as the F tests in Table 7. These results are available on request.

specifications almost invariably produce large, highly significant F statistics. The sole exception is the foreign effect in the Hungarian wage equation, where the F statistic is actually larger in the FE specification than the OLS. The differences between FE and FE&FT pre-program tests are more complex, however. In nine cases, the FE&FT is clearly superior: the domestic effects on employment and wages in Romania and Russia, the foreign effects on employment and wages in Hungary and Ukraine, and the foreign employment effect in Romania. But in five other cases the test prefers the FE specification: the domestic employment effect in Ukraine, the domestic wage effect in Hungary and Ukraine, and the foreign wage effect in Romania and Russia. In the remaining two cases (domestic employment effect in Hungary and foreign employment effect in Russia), the test is not decisive, because all the statistics are statistically insignificant, although the test statistics are slightly smaller for the FE.

Table 7   ***Pre-Program Tests***

	Hungary	**Romania**	**Russia**	**Ukraine**
OLS	Employment			
Domestic	38.86	18.35	516.10	13.62
	(0.000)	(0.000)	(0.000)	(0.000)
Foreign	12.51	27.69	38.01	12.25
	(0.000)	(0.000)	(0.000)	(0.000)
FE				
Domestic	0.40	11.54	7.46	0.42
	(0.672)	(0.000)	(0.001)	(0.656)
Foreign	5.24	7.31	0.12	4.18
	(0.005)	(0.001)	(0.890)	(0.015)
FE&FT				
Domestic	0.66	0.18	2.32	1.66
	(0.518)	(0.834)	(0.099)	(0.190)
Foreign	2.09	0.28	0.96	0.02
	(0.124)	(0.752)	(0.381)	(0.981)
OLS	Wage			
Domestic	3.03	2.72	62.95	10.25
	(0.049)	(0.066)	(0.000)	(0.000)
Foreign	0.74	18.51	6.40	3.62
	(0.478)	(0.000)	(0.002)	(0.027)
FE				

Table 7

	Hungary	Romania	Russia	Ukraine
Domestic	1.24	2.40	27.63	5.85
	(0.290)	(0.091)	(0.000)	(0.003)
Foreign	4.61	0.09	0.36	3.41
	(0.010)	(0.914)	(0.696)	(0.033)
FE&FT				
Domestic	6.83	1.83	1.97	9.56
	(0.001)	(0.161)	(0.140)	(0.000)
Foreign	0.48	2.06	2.57	0.75
	(0.617)	(0.127)	(0.076)	(0.470)

Note:. F-Statistics (P–Values) are shown for two hypotheses corresponding to tests of the estimated pre-privatization impact of privatization for domestic and foreign ownership, separately: $\delta_{-2d} = \delta_{-1d} = 0$, and $\delta_{-2f} = \delta_{-1f} = 0$.

We also carried out F tests on the joint probability that the FEs are all zero and on the joint probability that the FTs are all zero. For each country and each dependent variables, these tests were rejected at the 0.0001 level. Finally, we carried out Hausman-type tests of differences in the vectors of estimated coefficients from each of the models. Again, these always rejected equality between the OLS and FE coefficients, and between the FE and FE&FT coefficients. Taken together, these tests imply that the OLS specification is clearly dispreferred, but given the better performance of the FE specification in some cases that some weight should be given both to the FE and the FE&FT specifications.

Our results suggest – contrary to the expectations of many workers, policymakers, and economists – that workers have not, on average, been hurt by privatization. As we discussed in the introduction, however, privatization may produce opposing effects on wages and employment. The lack of negative consequences for workers, therefore, could result from new private owners failing to improve efficiency, or it could result from scale effects that offset the efficiency effects of private ownership. To explore these possibilities, we employ two decomposition techniques: first, the wage bill effect is decomposed into scale (output) expansion and unit labor cost reduction effects, and, second, the employment effect is decomposed into scale and productivity effects. The results from specifications including firm-specific trends can be seen in Figures 4 and 5, with the underlying coefficients and standard errors reported in Appendix Table A (along with those from fixed effect specifications).
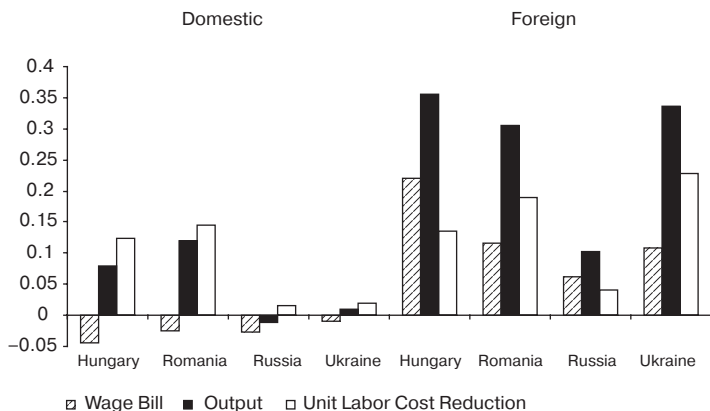
Figure 4. Wage Bill Decomposition

Notes: The graph presents regression coefficients of the effect of domestic and foreign privatizaion on unit labor cost reduction, output gorwth and employment growth. Full sets of unrestricted industry-year dummies are included in the regressions. All variables have been detrended using individual firm trends.
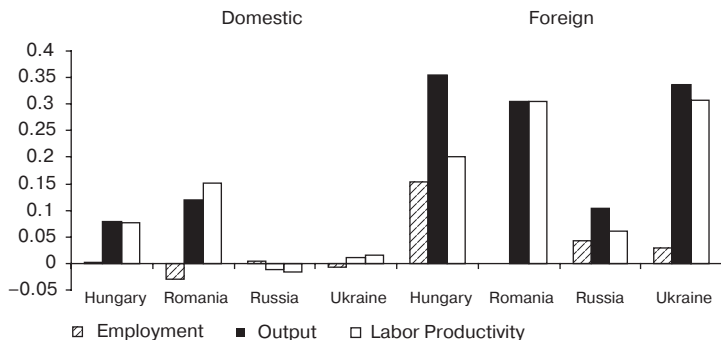

Figure 5. Employment Decomposition

Notes: The graph presents regression coefficients of the effect of domestic and foreign privatizaion on labor productivity, output and employment growth. Full sets of unrestricted industry-year dummies are included in the regressions. All variables have been detrended using individual firm trends.

A first striking regularity from both figures is that foreign owners have been much more active in both dimensions, expanding scale and enhancing efficiency much more than domestic owners. This regularity holds for the scale effect measured as the effect of privatization on output and for both definitions of the efficiency effect (unit labor cost reduction and labor productivity increase) within each

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country. The scale effect is not only positive and significant in each country for foreign privatization, but also for domestic privatization with the exception of Russia where it is negative, but small in magnitude (and statistically insignificant in the FE&FT). The efficiency effect measured as unit labor cost reduction is positive for all countries and both ownership forms, although again it is larger under foreign ownership. The effects vary widely across countries: while the foreign effects are similar for Hungary, Romania, and Ukraine, they are substantially smaller in Russia. But the domestic pattern is still more pronounced, as Hungary and Romania both show sizable scale and efficiency effects of domestic ownership, while both effects are negligible in Russia and Ukraine. Nevertheless, the wage bill effects of domestic privatization are quite similar – small and negative in each country. Foreign ownership, on the other hand, had a large positive effect on the wage bill everywhere.

The implications from the employment decomposition are similar. Both domestic and foreign privatization raises labor productivity in Hungary, Romania and Ukraine, but only foreign privatization does so in Russia. Under foreign ownership, again, the scale effect always dominates the efficiency effect, resulting in a positive net effect on employment. Domestic ownership, on the other hand, creates much smaller scale and efficiency effects that are similar in magnitude, resulting in very small net employment effects. Again, there is a pronounced contrast between the sizable domestic ownership effects in Hungary and Romania and the negligible domestic effects in Russia and Ukraine.

Comparing the two decompositions, it is apparent that productivity gains have been larger than unit labor cost savings, translating into larger wage than employment benefits for foreign-firm workers. This could reflect foreign owners' introduction of new technologies, the change of the labor force composition in favor of higher-skilled workers, or greater use of efficiency wages, unfortunately none of which are measurable in our data.

Finally, we investigate whether the above estimates may be biased due to nonrandom exit. As discussed in Section 2, above, it is difficult to distinguish genuine from spurious exits in our data, as in any panel of firms. As a check, however, we estimate exit probits to see whether there are significant differences in observed rates across ownership types, as shown in Table 8. The estimated δ_p and δ_d are always negative, significant only for Hungary, Ukraine and the Romanian $\delta_{d'}$ They are tiny everywhere except Hungary.[20] This implies that our estimates of the effects

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[20] In Hungary, the mean exit rate is also larger, than in the other countries, so the larger coefficient should be relative to this mean. The higher rate may be at least partially caused by the Hungarian bankruptcy law of 1992, which included a trigger mechanism for liquidation if the firm did not pay its obligations within a strict time limit. This procedure might increase not only genuine exit but also reorganizations, as the firm's management frequently exploited this tool to buy-in the firm during the liquidation process (Earle et al., 1994).

on workers are lower bounds on the true effects, confirming that the hypothesis of a negative effect on workers on average is rejected for every country in our sample, except possibly for Russia.

Table 8   *Estimated Effects of Privatization on the Probability of Exit*

	Hungary	**Romania**	**Russia**	**Ukraine**
$\hat{\delta}_d$	−0.058**	−0.003*	−0.002	−0.004**
	(0.006)	(0.001)	(0.001)	(0.001)
$\hat{\delta}_f$	−0.063**	−0.001	−0.007	−0.004*
	(0.003)	(0.002)	(0.009)	(0.001)
R²	0.091	0.051	0.071	0.128
Mean Exit	0.078	0.007	0.045	0.014
N	13,926	19,316	110,807	49,739

Note: Probit marginal effect estimates. Industry and year dummies are included in the regressions. Private = 1 if the firm is majority private at end of year t-1. Foreign = 1 if the majority of the firm's shares are owned by foreigners in year t-1. Domestic = 1 if the firm was private in year t-1 but not majority-owned by foreigners. Standard errors (corrected for firm clustering) are shown in parentheses. * = significant at 5-percent level. *** = significant at 1-percent level.

5. Conclusion

This paper has analyzed the effects of privatization on the wage bill, employment, and wages using comprehensive data on manufacturing firms in four economies, with long time series of annual observations both before and after privatization. The data contain comparable measurement concepts for the key variables, and we have applied consistent econometric methods to obtain comparable estimates across countries. The analysis is subject to a number of caveats we have discussed, including possibilities of measurement error, incomplete longitudinal links, function misspecification, and remaining simultaneity bias. To grapple with these issues, we have made great efforts to clean the data and improve the longitudinal links, investigated a variety of estimation and measurement methods, and have carried out extensions to the basic analysis that shed light on the gravity of the potential problems. While the caveats should be borne in mind when considering our findings, we believe that the results nonetheless provide important new evidence on the impact of privatization.

Contrary to the aggregate correlations between privatization and the wage bill, and the expectations of workers and many specialists, our firm-level regression re-

sults show a clear negative effect of privatization on workers only in Russian domestic privatization, and the effect even there is quite small. This demonstrates the danger of drawing conclusions about the effects of particular reforms using only aggregated data, especially when multiple economic changes take place at the same time.

At the beginning of the privatization process, workers feared foreign privatization most of all, assuming that new foreign owners would implement massive layoffs and wage cuts in their efforts to enhance efficiency. What has actually happened, however, is that privatization to foreign owners produced positive wage bill gains, at least compared to unprivatized and domestically privatized firms. They achieved this by having expanded scale to an even greater extent, than cutting employment cost for efficiency reasons, producing an overall increase in demand for labor. Regarding domestic ownership, the patterns in Hungary and Romania are quite different from those in Russia and Ukraine. The former have substantially enhanced efficiency, if not quite as much as their foreign counterparts, while the latter have not. The overall effect on workers has been similar in all four countries, however, due to a compensating increase in scale in Hungarian and Romanian domestically-privatized firms and no change in Russia and Ukraine.

Some economists theorized that managers would begin cutting employment and wages in anticipation of privatization in order to enhance their reputation as profit maximizers, and a few previous studies have found patterns consistent with this story. The evidence presented here, however, shows that workers tended to enjoy positive effects even in the year or two leading up to privatization.

Though the average effects on workers tend to be negligible or positive, worker opposition could still be justified if privatization reduces employment or wage security. No such evidence is found in Hungary or Romania. In fact, foreign privatization in those countries unambiguously improves worker welfare, not only increasing the average level, but also reducing downside risk. In Russia and Ukraine, though, privatization reduces security.

These cross-country and domestic vs. foreign patterns suggest that the trade-off between efficiency enhancement and worker welfare assumed by Aghion and Blanchard (1998) and others is questionable: efficiency-enhancing owners appear to be good for workers. Greater efficiency helps firms gain market share and reduces the likelihood of severe distress or exit, hence raising labor demand. Their workers' wages and employment prospects improve as a result, both on average and through minimization of downside risk.

Appendix

Table A   **Estimated Labor Productivity, Unit Labor Cost, and Output Effects of Foreign and Domestic Privatization**

	Hungary	Romania	Russia	Ukraine
FE	Labor Productivity			
$\hat{\delta}_d$	0.143**	0.300**	−0.026*	0.052**
	(0.023)	(0.025)	(0.013)	(0.019)
$\hat{\delta}_f$	0.535**	0.477**	0.340**	0.478**
	(0.055)	(0.080)	(0.135)	(0.145)
$\Pr(\hat{\delta}_f = \hat{\delta}_d)$	0.000	0.028	0.007	0.004
FE&FT				
$\hat{\delta}_d$	0.077**	0.151**	−0.017	0.016
	(0.025)	(0.020)	(0.010)	(0.017)
$\hat{\delta}_f$	0.201**	0.305**	0.060	0.307*
	(0.033)	(0.092)	(0.131)	(0.144)
$\Pr(\hat{\delta}_f = \hat{\delta}_d)$	0.025	0.100	0.559	0.044
FE	Unit Labor Cost			
$\hat{\delta}_d$	−0.170**	−0.323**	−0.021	−0.049**
	(0.021)	(0.025)	(0.012)	(0.015)
$\hat{\delta}_f$	−0.228**	−0.242**	−0.096	−0.173
	(0.045)	(0.062)	(0.103)	(0.101)
$\Pr(\hat{\delta}_f = \hat{\delta}_d)$	0.204	0.185	0.465	0.221
FE&FT				
$\hat{\delta}_d$	−0.123**	−0.145**	−0.015	−0.020
	(0.021)	(0.020)	(0.010)	(0.015)
$\hat{\delta}_f$	−0.135**	−0.190*	−0.041	−0.228
	(0.044)	(0.084)	(0.112)	(0.125)
$\Pr(\hat{\delta}_f = \hat{\delta}_d)$	0.789	0.603	0.819	0.097
FE	Output			
$\hat{\delta}_d$	0.114**	0.487**	−0.033*	0.069**
	(0.039)	(0.041)	(0.016)	(0.023)
$\hat{\delta}_f$	0.962**	0.762**	0.493**	0.613**

	Hungary	Romania	Russia	Ukraine
	(0.086)	(0.120)	(0.156)	(0.174)
$\Pr(\hat{\delta}_f = \hat{\delta}_d)$	0.000	0.022	0.001	0.002
FE&FT				
$\hat{\delta}_d$	0.079**	0.120	−0.012	0.010
	(0.029)	(0.026)	(0.011)	(0.019)
$\hat{\delta}_f$	0.355**	0.305**	0.103	0.337*
	(0.069)	(0.109)	(0.140)	(0.161)
$\Pr(\hat{\delta}_f = \hat{\delta}_d)$	0.000	0.100	0.413	0.044

Note: Foreign = 1 if the majority of the firm's shares are owned by foreigners in year *t*-1. Domestic = 1 if the firm was private in year t-1 but not majority-owned by foreigners. FE=specification including firm fixed effects; FT= all variables have been detrended using individual firm trends. The P values for the F test on the difference between the Foreign and Domestic coefficients are reported in the F test line. Standard errors (corrected for firm clustering) shown in parentheses. Ns are same as in Table 5. * = significant at 5-percent level. *** = significant at 1-percent level.

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Does Privatization Hurt Workers?
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