

**BUDAPEST WORKING PAPERS
ON THE LABOUR MARKET**

BWP. 2005/10

Does Privatization Hurt Workers?

Lessons in Comprehensive Manufacturing

Firm Panel Data

In Hungary Romania, Russia and Ukraine

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Budapest, 2005.

Budapest Working Papers No.2005/10
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This research was supported by the National Council for East European and Eurasian Research. We thank Jeff Nault for help with tables and figures, Joanne Lowery for editorial assistance, Philipp Jonas, Gábor Kézdi, and Mark Schaffer for econometric help and advice, and participants at presentations at the Upjohn Institute, Central European University, EERC-Kiev, and the 2005 AEA meetings for helpful comments. Assembling and preparing the data for this project involved large teams of collaborators, and we are grateful for conscientious work by Anna Horváth, Anna Lovász, Béla Személy, and Ágnes Törőcsik on the Hungarian data; Ioana Dan, Victor Kaznovsky, Catalin Pauna, Irina Vantu, and especially Ruxandra Visan on the Romanian data; and Natalia Akhmina, Tatiana Andreyeva, Serhiy Biletsky, Ivan Maryanchyk, Alexander Scherbakov, and Vladimir Vakhitov on the Ukrainian data. We are also grateful to the Hungarian National Bank for cooperation and data support on the Hungarian analysis, and to EROC (Economic Research and Outreach Center at the Kyiv-Mohyla School of Economics) for support of Ukrainian data collection and preparation. All errors are our own.

ISSN 1785-3788
ISBN 963 9588 66 0

Published by the Institute of Economics, Hungarian Academy of Sciences
Budapest, 2005.

With financial support from the Foundation for Job Creation and the Hungarian
Economic Foundation

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**A PRIVATIZÁCIÓ HATÁSAI A VÁLLALATOK LÉTSZÁMÁRA
ÉS A DOLGOZÓK BÉREIRE**
PANEL BECSLÉSEK MAGYAR, ROMÁN, OROSZ ÉS UKRÁN
FELDOLGOZÓIPARI ADATOKON

Összefoglaló

Négy tranzíciós ország vállalati szintű adataival elemezzük a privatizáció hatásait az átlagbérekre és a vállalati létszámra. Ellentétben a dolgozók félelmeivel, nem találunk komoly hatásokat, kivéve Oroszországban, ahol a privatizációnak kis negatív hatását identifikáltuk. A külföldi privatizáció hatása általában pozitív. Az elhanyagolható belföldi privatizációs hatást a hatékonyság- és a skálanövekedés kioltó hatásaival magyarázzuk, a többnyire pozitív külföldi hatást pedig azzal, hogy a skálanövekedés sokkal nagyobb volt, mint a szintén nagy hatékonyságnövekedés.

Kulcsszavak: privatizáció, vállalati létszám, bérek, külföldi tulajdon, Magyarország, Oroszország, Románia, Ukrajna

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

BY

J. DAVID BROWN – JOHN S. EARLE – ÁLMOS TELEGDY

Abstract

We analyze the effects of privatization on firm-level wages and employment in four transition economies. Contrary to workers' fears, our fixed effect and random trend estimates imply little effect of domestic privatization, except for a slight negative effect in Russia, and they provide some evidence of positive foreign effects on both wages and employment in all four countries. The negligible employment impact of domestic privatization results from effects on efficiency and scale that are large, positive, but offsetting in Hungary and Romania, and from small effects of both types in Russia and Ukraine. The positive employment and wage bill consequences of foreign ownership result from a substantial scale-expansion effect that dominates the efficiency effect.

Keywords: privatization, employment, wages, foreign ownership, Hungary, Romania, Russia, Ukraine

JEL codes: D21, G34, J23, J31, L33, P31

1. INTRODUCTION

The greatest opposition to privatizing a firm usually comes from the firm's own employees, who are fearful of wage cuts and job losses. Workers' apprehensions about privatization are consistent with standard economic analyses, whereby new private owners reduce the firm's labor 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 will tend to rise, and if this "scale effect" dominates, then privatization could cause a net employment increase.

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) and Birdsall and Nellis (2003) summarize a number of case studies and small sample surveys of privatization effects on labor in several developing economies. The largest sample in the existing

¹ The little attention to the effects of privatization on workers contrasts with the large literature on privatization and firm performance; see for example the surveys by Megginson and Netter (2001) and Djankov and Murrell (2002).

literature is the 170 privatized firms in Mexico studied by La Porta and Lopez-de-Silanes (1999), although the post-privatization information is limited to a single year.² Other studies have sometimes included employment as one of many possible indicators of firm performance, but not the focus of analysis.³ Overall, the results from this small body of previous research are inconclusive, containing both negative and positive estimates of the effects on workers.⁴

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

² Lopez-de-Silanes and Chong (2003) also summarize the results from several studies of privatization in some Latin American countries. In research with a different focus, Chong and Lopez-de-Silanes (2002) study pre-privatization retrenchment programs designed to increase the attractiveness of state-owned firms to potential investors.

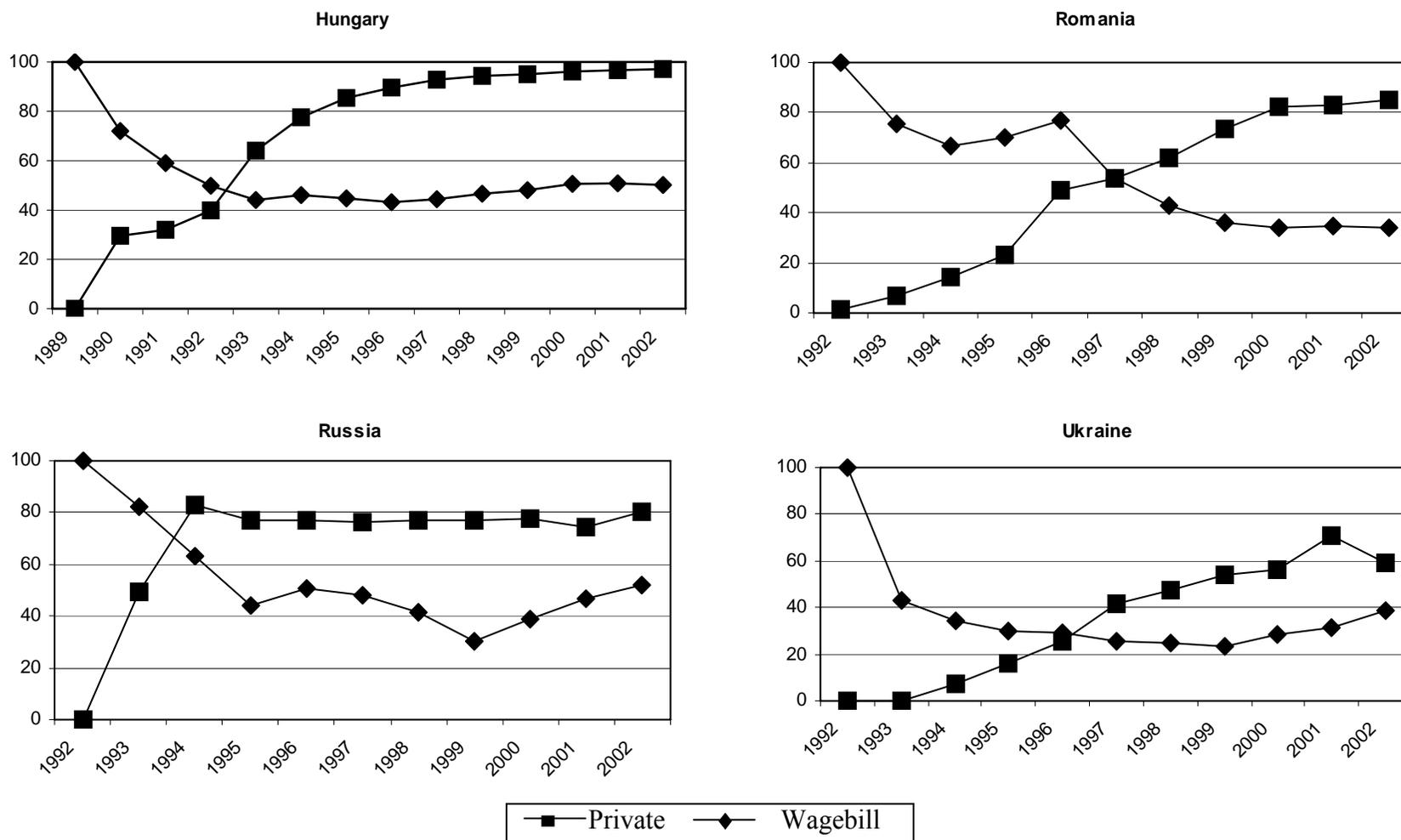
³ Studies of firm performance that include employment equation estimates are Megginson, Nash, and van Randenborgh (1994), Boubakri and Cosset (1998), D'Souza and Megginson (1999), Frydman, Gray, Hessel, and Rapaczynski (1999), and Lizal and Svejnar (2002); two of these find a positive effect of privatization on employment, two no effect, and one a negative effect.

⁴ Another related literature examines the public sector wage differential (e.g., Gyourko and Tracy, 1988), where one identification approach relies on workers who switch sectors; our approach instead uses firms that switch sectors.

least successful, and Romania somewhere in the middle.⁵ 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.

⁵ 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 Real Wage Bill and Private Ownership



Notes: The graphs show 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.

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 manufacturing firms runs from the Communist and immediate post-Communist period, when all were state-owned, through 2002, well after most had been privatized. The coverage of our data is quite comprehensive, including most manufacturing firms inherited from the former planned economy, both those eventually privatized 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 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 the fate of displaced workers, the origins of newly hired workers, or the prices and availability of goods. A complete welfare evaluation of privatization is therefore not possible with our data. The data are very well-suited, however, for investigating the effects of privatization on a firm's wages and employment, essential questions for such an evaluation.⁶

Our basic aim is to provide robust estimates of these effects using much larger and longer panels than were available to earlier researchers, but we also exploit the advantages of our data in several additional ways. First, we are able to distinguish between firms privatized to foreign investors and those privatized to domestic companies and individuals. Workers appear to fear foreign much more than domestic investors, but there is little evidence whether this perception is warranted. Second, we decompose the estimated wage bill and employment effects of privatization into efficiency and scale effects to shed light on these underlying mechanisms. Efficiency is measured alternatively as unit labor cost and labor productivity, and scale is measured as output for these calculations. Third, we investigate 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.

⁶ 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; these would also be relevant considerations for a welfare evaluation, but we leave them for future research.

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.”⁷ These models control not only for fixed differences among firms but also differing trend 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 variants of 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 joint venture registries that are available in Russia and a database from the State Property Committee in Ukraine, 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

⁷ 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. Brown, Earle, and Telegdy (2006) estimate random trend models of the effect of privatization on multifactor productivity in Hungary, Romania, Russia, and Ukraine. To our knowledge, however, no other study of privatization, corporate governance, or firm performance has used this method.

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 1992-2002.⁸ 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. The basic data sources are the National Tax Authority in Hungary and the Ministry of Finance in Romania, which provide data 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 a 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 was 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 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*).⁹ We include only “old” firms, defined as existing prior to 1992 (1990 in Hungary) or state-owned at first observation, both because the

⁸ 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.

⁹ Recycling (NACE 37) is also excluded because of noncomparability with the *OKONKh* classification system.

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.¹⁰

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.

¹⁰ In Russia and Ukraine, privatization started only after 1992, so firms that existed before this year in our data must be old. Romanian privatization started in 1992, so the old sample is not reduced by the lack of data in earlier years.

Table 2

Mean Employment and Wage in the First Year of Analysis, 1994, and 2002

	Employment			Wage		
	First year	1994	2002	First year	1994	2002
Hungary	613.7 (1,214.7)	206.6 (594.1)	165.3 (422.9)	981.6 (352.9)	1,344.7 (930.5)	1,518.0 (1,896.3)
Romania	1234.0 (2,169.9)	960.2 (2,258.3)	414.3 (924.1)	60,847.6 (25,172.5)	52,121.8 (22,946.2)	69,920.5 (483,597)
Russia	621.4 (1296.2)	712.2 (2277.0)	506.6 (1999.4)	65,814.2 (16,826.7)	44,343.5 (23,402.3)	40,168.7 (24,503.4)
Ukraine	805.2 (1,863.9)	595.0 (1,524.2)	472.0 (1,901.1)	9,516.6 (1,603.4)	6,193.0 (2,951.1)	6,708.8 (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). One type of measurement error, the under-reporting of wages to avoid taxes and social security contributions, could be especially important for our analysis. Discussions with knowledgeable observers in these countries suggest that while under-reporting is a common problem in small service sector firms, however, it is unlikely to be a very serious problem in our samples of medium and large manufacturing firms because of the cumbersomeness of paying large numbers of employees under the table. The discussions also indicate that to the extent under-reporting in these firms does occur, it is most likely to happen in firms that are privatized to domestic owners; state-owned firms are subject to tight controls and have fewer incentives to avoid taxes, while foreign-owned firms, especially larger ones like those in our samples, are less likely to engage in this practice. This implies that our estimates of the wage effects of domestic privatization will if anything be

downward-biased, so that an estimated effect of zero (or slightly negative) might reflect a true effect that is positive.¹¹

Finally, the longitudinal linkages in the databases 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.¹² 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 is common in longitudinal data, and it motivates us to carry out a separate analysis of the relationship between privatization and exit behavior, in order to assess how significantly this may qualify our analysis. Before describing these methods, however, we first provide a brief description of privatization policies and ownership outcomes.

3. PRIVATIZATION POLICIES AND THEIR IMPLICATIONS

The methods and tempos of large enterprise privatization differed quite significantly across the four countries we study in this paper. 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

¹¹ Our wage variable represents accrued obligations to employees, and another potential form of bias could result from differential wage arrears across ownership types. Studies of arrears find little difference between state-owned firms and those privatized domestically (e.g., Earle and Sabirianova, 2002), suggesting little bias in that comparison. The evidence on foreign-owned firms is scant, but if foreign employers are less likely to have arrears, then their actual paid wages would be relatively understated – implying a downward bias on the estimated foreign wage effect.

¹² In Hungary, we also used a separate Central Statistical Office dataset with information on reorganizations that broke links across years.

worker ownership (involving only about 250 firms), very little dispersed ownership, and instead significant managerial ownership and highly concentrated blockholdings, many of them foreign (Frydman et al., 1993a). 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, foreign investors became more involved, and blocks of shares were sold to both 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 most 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 and some blockholding by domestic entities. Subsequently, privatization through sales became more common, secondary trading increased concentration, 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.¹³ Ownership is

¹³ 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

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.¹⁴

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
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.

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 (see, e.g., Boycko, Shleifer, and Vishny, 1995).

¹⁴ 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.

The table also contains the percentage of firms majority privatized to foreigners.¹⁵ 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.

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

¹⁵ 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.

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 for gathering evidence on the possible effects of ownership change.

Estimating these effects nevertheless faces some potential problems. The first is the possibility that aggregate shocks may affect employment, wages, and ownership. Studies that estimate a privatization effect as the difference between pre- and post-privatization levels for a sample of privatized firms (e.g., Megginson, Nash, and van Randenborgh, 1994) are unable to distinguish the effect of privatization from such aggregate fluctuations. Moreover, the shocks may be industry-specific, and the available deflators may not perfectly capture price changes. Yet most studies have too few observations at their disposal to be able to account for industry-specific fluctuations, which if correlated with privatization may produce biased estimates. Taking advantage of the large samples in our data, we include a full set of industry-year interactions to control for such factors. Unlike most previous studies, our data also contain a comparison group of firms that remain in state ownership throughout the period of observation.

A more difficult problem is the possibility of selection bias in the privatization process. Politicians, investors, and employees of the firms may all influence whether a firm is privatized, and whether the new owners are domestic or foreign. Politicians concerned with unemployment may prefer to retain firms with the worst prospects in state ownership in order to protect workers from layoffs and wage cuts, and the employees themselves may work to prevent privatization in such cases. Potential investors are also likely to be most interested in purchasing firms with better prospects. To remove such time-invariant differences across firms, we therefore include firm fixed effects (FE) in some specifications. Since firms could also differ in their trend growth rates, we estimate some 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.¹⁶ As discussed in more detail below, we

¹⁶ This argument is made by Aghion, Blanchard, and Burgess (1994). Roland and Sekkat (2000) conclude that good managers will restructure their companies prior to privatization.

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.

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

$$y_{it} = \mathbf{D}_{jt}\boldsymbol{\gamma}_{jt} + \mathbf{w}_t\boldsymbol{\alpha}_i + \boldsymbol{\theta}_{it}\boldsymbol{\delta} + u_{it}, \quad (1)$$

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 .¹⁷ In alternative specifications, y_{it} is the natural logarithm of the firm's wage bill, employment, and average wage rate (per worker); because the $\ln(\text{wage bill})$ 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 $1 \times JT$ vector of industry-year interaction dummies; $\boldsymbol{\gamma}_{jt}$ is the associated $JT \times 1$ vector of coefficients; and u_{it} is an idiosyncratic error.¹⁸ The dimensions of the other terms in the equation vary across specifications: \mathbf{w}_t is a vector of aggregate time variables, $\boldsymbol{\alpha}_i$ is the vector of associated individual-specific slopes, $\boldsymbol{\theta}_{it}$ is the vector of ownership measures, and $\boldsymbol{\delta}$ 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 $\boldsymbol{\alpha}_i \equiv \alpha_i$ is the unobserved effect. The FE&FT model specifies $\mathbf{w}_t \equiv (1, t)$, so that $\boldsymbol{\alpha}_i \equiv (\alpha_{1i}, \alpha_{2i})$, 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 $\boldsymbol{\theta}_{it}$. 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.¹⁹ The coefficient of interest δ is then the mean within-country-industry-year difference

La Porta and Lopez-de-Silanes (1999) find negative anticipatory effects in their study of Mexican privatization.

¹⁷ $J=10$ 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.

¹⁸ 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.

¹⁹ Privatization is inferred in our data when a firm changes status from the end of one year to the next. This implies that the date the new owners acquire formal authority (e.g., the first post-privatization shareholders' meeting) varies across firms, with some early in the final pre-privatization year. But the first "post" year must somehow be defined, and we discuss this issue further in connection with the dynamics of the effect below.

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 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 $\theta_{it} \equiv (\mathbf{Domestic}_{it\tau}, \mathbf{Foreign}_{it\tau})$, $\delta \equiv (\delta_{\tau d}, \delta_{\tau f})$, 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 subsample), and we set $\mathbf{w}_t \equiv 0$. $\theta_{it} \equiv Pre-Private_{it}$ in one specification, and $\theta_{it} \equiv (Pre-Domestic_{it}, Pre-Foreign_{it})$ 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

$$y_{it} = \mathbf{D}_{jt}\gamma_{jt} + \boldsymbol{\theta}_{it}\boldsymbol{\delta} + u_{it}. \quad (2)$$

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. A difficult problem is how to handle exit because, 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 reregistrations and 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.

Table 4

Pre-Privatization Relative Wage Bill, Employment, and Wage

	Hungary	Romania	Russia	Ukraine
Wage Bill				
<i>Pre Private</i>	-0.605** (0.081)	-0.098 (0.090)	0.830** (0.024)	0.227** (0.042)
<i>Pre Domestic</i>	-0.714** (0.081)	-0.190* (0.090)	0.827** (0.024)	0.213** (0.042)
<i>Pre Foreign</i>	0.361* (0.167)	0.976** (0.162)	1.342** (0.136)	1.038** (0.184)
Employment				
<i>Pre Private</i>	-0.582** (0.080)	-0.154 (0.082)	0.722** (0.021)	0.186** (0.037)
<i>Pre Domestic</i>	-0.678** (0.080)	-0.243** (0.082)	0.720** (0.021)	0.175** (0.037)
<i>Pre Foreign</i>	0.263 (0.179)	0.791** (0.142)	1.199** (0.127)	0.837** (0.154)
Wage				
<i>Pre Private</i>	-0.023 (0.019)	0.065** (0.019)	0.107** (0.010)	0.041** (0.014)
<i>Pre Domestic</i>	0.036 (0.019)	0.053** (0.019)	0.107** (0.010)	0.038** (0.014)
<i>Pre Foreign</i>	0.099* (0.041)	0.185** (0.035)	0.143** (0.038)	0.201** (0.067)
<i>N</i>	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 coefficient is essentially zero in Hungary and Ukraine, and close to zero although negative in Russia. These results therefore imply that 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.

Turning to the distinction between 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 is 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 specifications. Foreign-owned firms account for only very small fractions of the observations in Russia and Ukraine, so the estimates of δ_d and δ_p are nearly identical.

By contrast, the estimated effects of foreign privatization in Table 5 are large, positive, and highly significant in the OLS and FE specifications in all four countries, the FE coefficients varying between 0.396 and 0.735. When trends are added, the coefficients fall, but they remain positive in all four countries. They remain statistically significantly different from zero in Hungary and Romania, while in Russia and Ukraine they are imprecisely estimated, probably due to the small number of foreign firms in those countries. In any case, our results provide no support for the widespread fear of foreign owners; on the contrary, they provide strong evidence that foreign owners increased the wage bill in the two Central and East European countries in our study, and in the two FSU republics the effect seems to be zero in the most pessimistic case.

Table 5

Estimated Wage Bill Effects of Privatization

	Hungary	Romania	Russia	Ukraine
OLS				
$\hat{\delta}_p$	-0.431** (0.068)	-0.065 (0.065)	0.850** (0.027)	0.146** (0.038)
R^2	0.165	0.181	0.358	0.309
FE				
$\hat{\delta}_p$	0.038 (0.032)	0.187** (0.027)	-0.052** (0.012)	0.026 (0.015)
R^2	0.293	0.466	0.478	0.586
FE&FT				
$\hat{\delta}_p$	-0.008 (0.023)	-0.015 (0.019)	-0.026** (0.008)	-0.008 (0.013)
R^2	0.080	0.430	0.194	0.285
OLS				
			Domestic and Foreign	
$\hat{\delta}_d$	-0.657** (0.069)	-0.127* (0.066)	0.844** (0.027)	0.134** (0.038)
$\hat{\delta}_f$	0.848** (0.122)	1.396** (0.136)	1.823** (0.203)	1.079** (0.252)
Pr($\hat{\delta}_f = \hat{\delta}_d$)	0.000	0.000	0.000	0.000
FE				
$\hat{\delta}_d$	-0.056 (0.034)	0.164** (0.027)	-0.054** (0.012)	0.020 (0.015)
$\hat{\delta}_f$	0.735** (0.066)	0.520** (0.084)	0.396** (0.083)	0.439** (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)	-0.024 (0.020)	-0.027** (0.008)	-0.010 (0.013)
$\hat{\delta}_f$	0.220** (0.052)	0.116* (0.057)	0.062 (0.074)	0.109 (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 and for loss of degrees of freedom in the FE&FT specification) are shown in parentheses. R^2 is the second-stage R^2 for FE&FT. * = significant at 5-percent level. ** = significant at 1-percent level.

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.²⁰

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 small and 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 is even closer to zero when FTs are added.²¹ 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.

²⁰ The results from estimating the change in the wage bill from two years before to two years after privatization imply substantial negative effects of both foreign and domestic privatization in all countries (except for foreign privatization in Hungary). This approach controls for fixed heterogeneity across privatized firms (by differencing). But it does not use the state-owned control group, nor does it control for aggregate time effects, industry-specific shocks, or firm-specific trends.

²¹ As discussed in Section 2, wage under-reporting is more likely to occur in firms privatized domestically than in those remaining state-owned or sold to foreign investors (where such under-reporting appears to be uncommon), which would imply that our small negative coefficients on domestic privatization are downward-biased estimates of the true effects.

Table 6

**Estimated Employment and Wage Effects
of Domestic and Foreign Privatization**

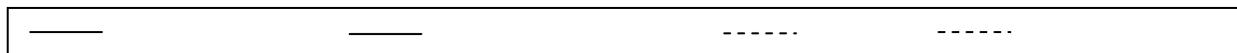
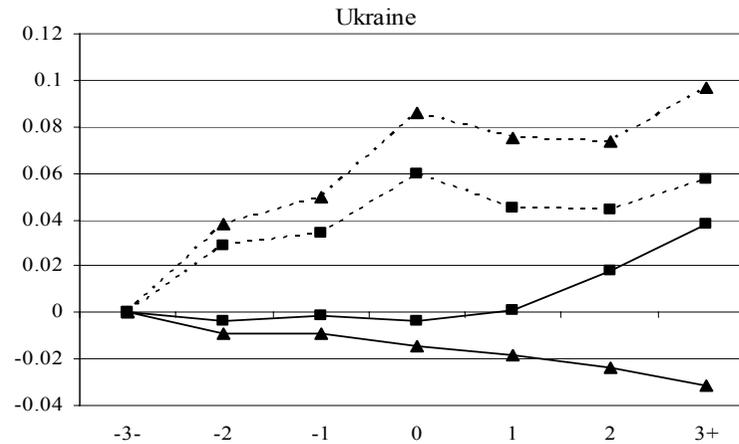
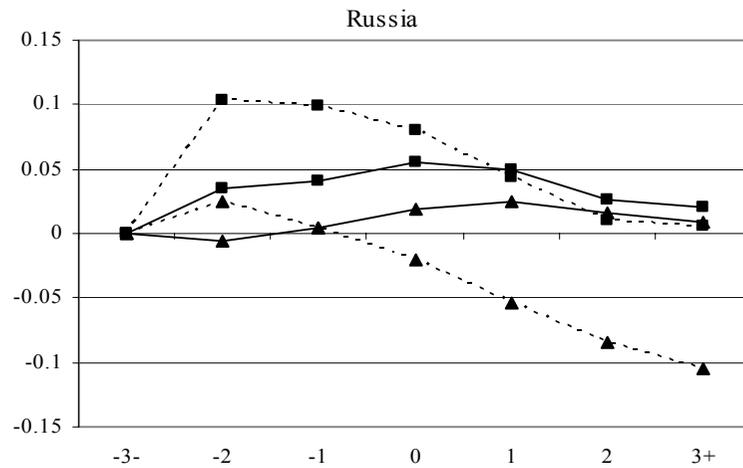
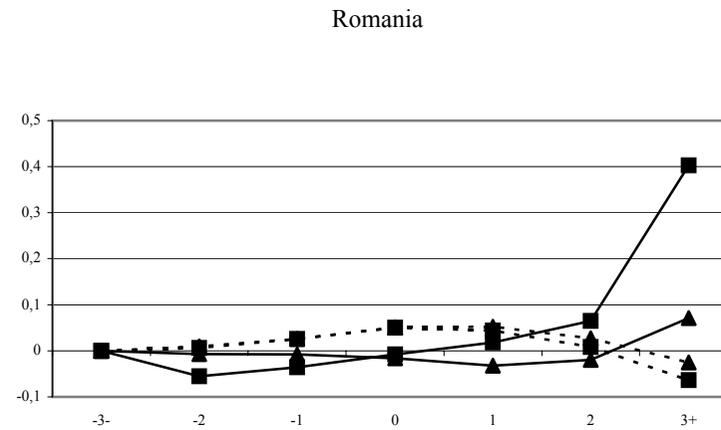
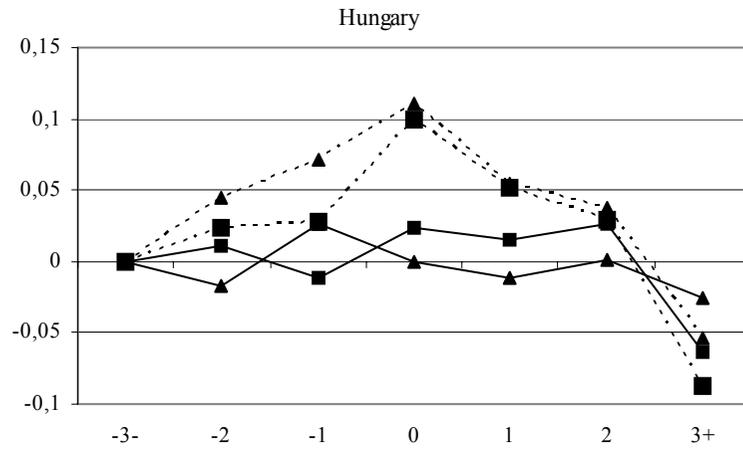
	Hungary	Romania	Russia	Ukraine
OLS				
	Employment			
$\hat{\delta}_d$	-0.621** (0.067)	-0.176** (0.060)	0.764** (0.022)	0.080** (0.029)
$\hat{\delta}_f$	0.367** (0.117)	0.966** (0.122)	1.432** (0.166)	0.661** (0.170)
Pr($\hat{\delta}_f = \hat{\delta}_d$)	0.000	0.000	0.000	0.001
FE				
$\hat{\delta}_d$	-0.030 (0.035)	0.187** (0.026)	-0.007 (0.006)	0.017 (0.009)
$\hat{\delta}_f$	0.428** (0.073)	0.285** (0.086)	0.152** (0.043)	0.135 (0.077)
Pr($\hat{\delta}_f = \hat{\delta}_d$)	0.000	0.255	0.000	0.127
FE&FT				
$\hat{\delta}_d$	0.002 (0.024)	-0.030 (0.017)	0.005 (0.004)	-0.006 (0.008)
$\hat{\delta}_f$	0.154** (0.050)	0.000 (0.068)	0.043 (0.041)	0.030 (0.070)
Pr($\hat{\delta}_f = \hat{\delta}_d$)	0.003	0.662	0.358	0.614
OLS				
	Wage			
$\hat{\delta}_d$	-0.035 (0.020)	0.049** (0.015)	0.080** (0.011)	0.055* (0.017)
$\hat{\delta}_f$	0.481** (0.036)	0.430** (0.050)	0.391** (0.074)	0.418** (0.123)
Pr($\hat{\delta}_f = \hat{\delta}_d$)	0.000	0.000	0.000	0.003
FE				
$\hat{\delta}_d$	-0.027 (0.015)	-0.023 (0.012)	-0.047** (0.008)	0.003 (0.011)
$\hat{\delta}_f$	0.307** (0.033)	0.235** (0.054)	0.244** (0.064)	0.304** (0.095)
Pr($\hat{\delta}_f = \hat{\delta}_d$)	0.000	0.000	0.000	0.002
FE&FT				
$\hat{\delta}_d$	-0.045** (0.016)	0.006 (0.013)	-0.032** (0.007)	-0.004 (0.011)
$\hat{\delta}_f$	0.066* (0.033)	0.116* (0.057)	0.019 (0.063)	0.079 (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 and for loss of degrees of freedom in the FE&FT specification) 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 3.²² 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 countries. 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.

²² 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.

Figure 2: Dynamics of Domestic Privatization Effect on Employment and Wages



■ Employment FE ▲ Employment FE & FT ■ Wage FE ▲ Wage FE & FT

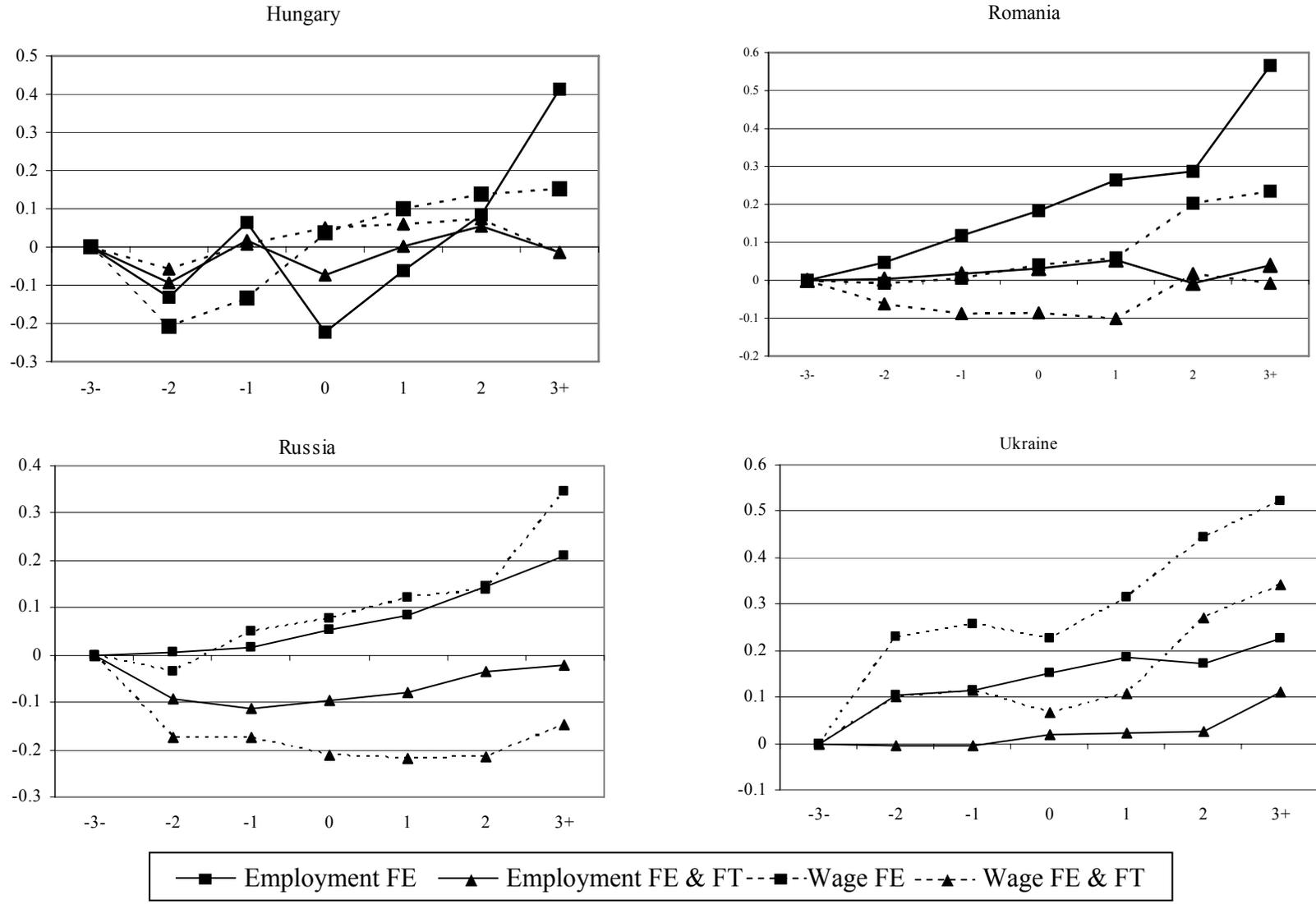
Notes: The graphs present regression coefficients of interactions between dummy variables for the years before and after privatization and an indicator for whether the firm is ever domestically privatized (privatization year = 0). Full sets of unrestricted industry-year dummies are included in the regressions.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

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.

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.²³ The OLS 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.

²³ 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.

Figure 3: Dynamics of Foreign Privatization Effect on Employment and Wages



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.

Table 7

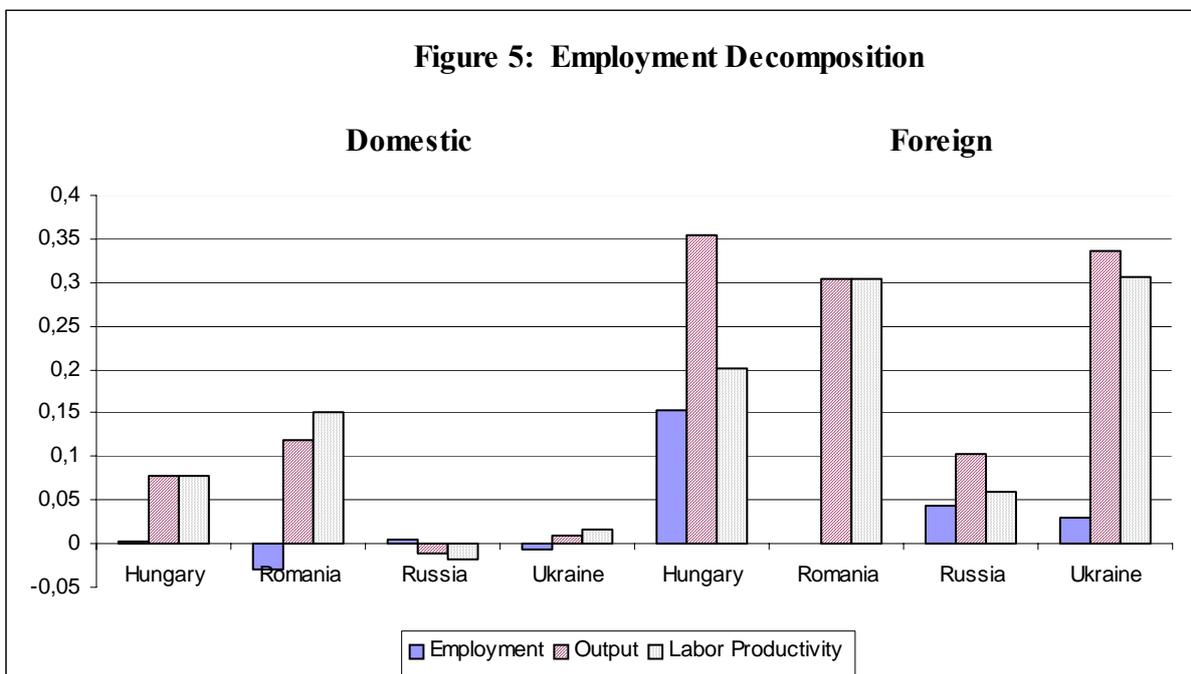
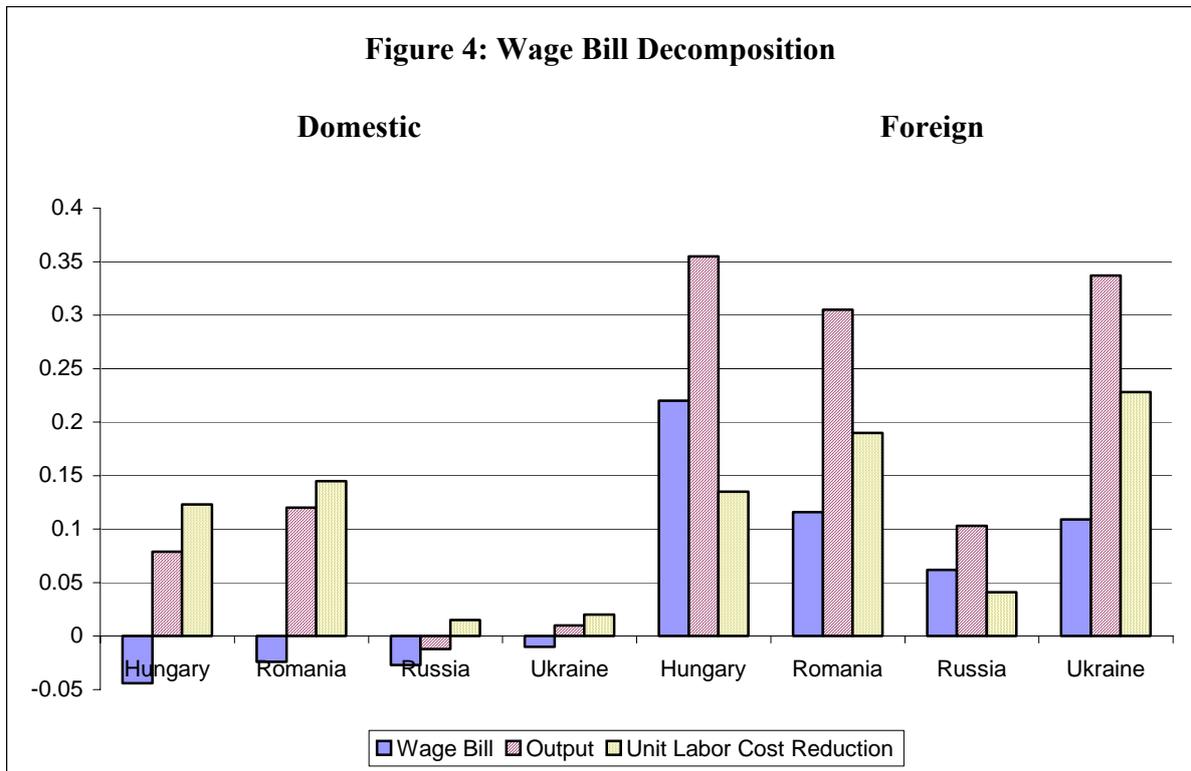
Pre-Program Tests

	Hungary	Romania	Russia	Ukraine
OLS				
Employment				
<i>Domestic</i>	38.86 (0.000)	18.35 (0.000)	516.10 (0.000)	13.62 (0.000)
<i>Foreign</i>	12.51 (0.000)	27.69 (0.000)	38.01 (0.000)	12.25 (0.000)
FE				
<i>Domestic</i>	0.40 (0.672)	11.54 (0.000)	7.46 (0.001)	0.42 (0.656)
<i>Foreign</i>	5.24 (0.005)	7.31 (0.001)	0.12 (0.890)	4.18 (0.015)
FE&FT				
<i>Domestic</i>	0.66 (0.518)	0.18 (0.834)	2.32 (0.099)	1.66 (0.190)
<i>Foreign</i>	2.09 (0.124)	0.28 (0.752)	0.96 (0.381)	0.02 (0.981)
OLS				
Wage				
<i>Domestic</i>	3.03 (0.049)	2.72 (0.066)	62.95 (0.000)	10.25 (0.000)
<i>Foreign</i>	0.74 (0.478)	18.51 (0.000)	6.40 (0.002)	3.62 (0.027)
FE				
<i>Domestic</i>	1.24 (0.290)	2.40 (0.091)	27.63 (0.000)	5.85 (0.003)
<i>Foreign</i>	4.61 (0.010)	0.09 (0.914)	0.36 (0.696)	3.41 (0.033)
FE&FT				
<i>Domestic</i>	6.83 (0.001)	1.83 (0.161)	1.97 (0.140)	9.56 (0.000)
<i>Foreign</i>	0.48 (0.617)	2.06 (0.127)	2.57 (0.076)	0.75 (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 variable, 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 not preferred. Given the better performance of the FE specification in

some cases, some weight should be placed both on the FE and the FE&FT specifications.



Our results suggest – contrary to the expectations of many workers, policymakers, and economists – that average wages and employment have not been substantially reduced by either domestic or foreign privatization. As we discussed in the introduction, however, privatization may affect firm scale and efficiency in ways that produce opposing effects on workers. The lack of negative consequences that we find 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).

A first striking regularity from both figures is that foreign owners have been much more active in both dimensions 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 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 show sizable scale and efficiency effects of domestic ownership, while both effects are negligible in Russia and Ukraine. Thus, the cross-country domestic wage bill patterns (small and negative everywhere) mask large differences in scale and efficiency effects.

The implications from the employment decomposition are similar. Both domestic and foreign privatization raise 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. And again, there is a pronounced contrast between sizable domestic ownership effects in Hungary and Romania and 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, a change in labor force composition in favor of higher-skilled workers, or greater use of efficiency wages, none of which, unfortunately, 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 $\hat{\delta}_d$ and $\hat{\delta}_f$ are always negative, and tiny everywhere except Hungary.²⁴ This implies that our estimates of the effects 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** (0.006)	-0.003* (0.001)	-0.002 (0.001)	-0.004** (0.001)
$\hat{\delta}_f$	-0.063** (0.003)	-0.001 (0.002)	-0.007 (0.009)	-0.004* (0.001)
R^2	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.

²⁴ The Hungarian coefficient should be interpreted in light of a mean exit rate that is also much larger than in the other countries. The higher rate in Hungary may be at least partially caused by the 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, which was frequently exploited by managers to buy-in the firm during the liquidation process, might increase both genuine shutdowns and spurious exits from our data (Earle et al., 1994).

6. CONCLUSION

Although economic analyses of the effects of privatization have focused almost entirely on firm performance, the greatest political and social controversies have usually concerned the consequences for the firm's employees. In most cases, it has been assumed that the employment and wage effects would be negative, and workers all around the world have reacted to the prospect of privatization, especially that to foreigners, with protests and strikes. Yet there have been very few systematic studies of the relationship between privatization and outcomes for the firm's workers, and previous research has been hampered by small sample sizes, short time series, and little ability to control for selection bias. It has therefore remained unclear whether workers' fears of privatization are in fact warranted.

In this paper, we have analyzed the effects of privatization on the firm's workers using comprehensive data on manufacturing firms in four economies, with long time series of annual observations both before and after privatization. The data contain similar measurement concepts for the key variables, and we have applied consistent econometric procedures to obtain comparable estimates across countries. In particular, we have exploited the longitudinal strength of our data and adopted methods commonly employed in the program evaluation literature to assess and control for selection bias. Like most firm-level data, ours do not contain information on changes in fringe benefits, worker turnover, and the composition of employment, nor can we track the subsequent experiences of any workers who happen to be displaced. Therefore, we cannot carry out a complete evaluation of the effects of privatization on worker welfare. But we can address some important components of such an evaluation, in particular the consequences for the firm's wage bill, employment, and wage rate.

Contrary to workers' expectations, we find no evidence for strong negative effects of any form of privatization on any of these variables. Concerning the wage bill, which might be taken as an overall measure of worker welfare, OLS estimates of the effect of privatization to domestic owners are negative in Hungary and Romania and positive in Russia and Ukraine, but these are subject to severe selection bias. In our fixed effects (FE) and random firm trend (FE&FT) models, which are preferred to OLS by several specification tests, a statistically significant negative effect on the wage bill emerges only in the case of domestic private ownership in Russia, and the magnitude is slight (-3 to -5 percent).

By contrast, we estimate that privatization to foreign investors produces consistently positive effects on the wage bill in all four countries, regardless of

estimation technique. The OLS coefficients are very large (0.8–1.8), and while they are attenuated in the FE specification (0.4–0.7), they remain highly significant. Adding the firm trends induces further attenuation and, together with the small sample sizes in Russia and Ukraine, inhibits precise estimation, but the coefficients remain positive everywhere, and they are statistically significant in Hungary and Romania (with magnitudes of 0.22 and 0.12, respectively).

Decomposing the effects on the wage bill into separate employment and wage effects, we find no evidence of strong negative consequences on either variable. Estimated by FE or FE&FT, the employment effects are never negative and statistically significant, while for the wage rate they are significantly negative only in Hungary and Russia, but small in magnitude (-3 to -5 percent in both countries). The estimated coefficients on foreign ownership again stand in stark contrast, with signs that are uniformly positive for all countries and both dependent variables. The magnitudes of the foreign effects are consistently large and statistically significant in the OLS and FE specifications. For employment, only in Hungary does this result remain under FE&FT, while for wages, it remains for both Hungary and Romania. Our estimated dynamic effects around the privatization year show only minor fluctuations in the domestic effects before and after privatization, while the foreign effects tend to grow strongly and consistently from the privatization year onwards.

We explore possible explanations for these patterns by considering two alternative mechanisms through which privatization may affect outcomes for workers: efficiency and scale. Our decomposition analysis of the wage bill into output and unit labor cost and of employment into output and labor productivity shows that domestic privatization has tended to produce gains in scale (output) and efficiency (unit labor cost and labor productivity) that have offset each other in their consequences for workers. In Hungary and Romania, however, these offsetting scale and efficiency effects have both been large, while in Russia and Ukraine they have been small. Foreign privatization has resulted in much larger efficiency effects in all four countries, but still much larger scale effects, resulting in the increased employment and wages in foreign-owned firms that we observe after privatization.

These cross-country and domestic versus foreign patterns are inconsistent with the trade-off in privatization between efficiency and worker welfare that has been assumed by many economists (e.g., Aghion and Blanchard, 1998). In our data, efficiency-enhancing owners appear to be good for workers, at least in terms of average employment and wage levels. Greater efficiency helps firms expand sales, reducing the likelihood of severe distress and raising labor demand. We find that workers' employment and wage prospects are never substantially diminished by privatization, and in some cases – particularly with foreign ownership – they actually brighten.

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