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## Does privatization hurt workers? Lessons from comprehensive manufacturing firm panel data in Hungary, Romania, Russia, and Ukraine

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# Does Privatization Hurt Workers?

## Lessons from Comprehensive Manufacturing Firm Panel Data

### in Hungary, Romania, Russia, and Ukraine

Upjohn Institute Staff Working Paper 05-125

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#### Abstract

We estimate the effects of privatization on firm-level wages and employment in four transition economies. Applied to longitudinal data on manufacturing firms, our fixed effect and random trend models consistently fail to support workers' fears of job losses from privatization, and they never imply large negative effects on wages; only for domestic privatization in Hungary and Russia are small (3-5%) negative wage effects found. Privatization to foreign investors has positive estimated impacts on both employment and wages in all four countries. The negligible consequences of domestic privatization for workers result from effects on scale, productivity, and costs that are large but offsetting in Hungary and Romania, and from small effects of all types in Russia and Ukraine. The positive employment outcome under foreign ownership results from a substantial scale-expansion effect that dominates the productivity-improvement effect, and the positive wage outcome from a productivity effect that dominates the effect on cost-reduction.

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## 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 raise productivity and 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 these productivity-improvement and cost-reduction effects 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 sales and 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 private owners may expropriate rents and quasi-rents of workers and other stakeholders, similar to a hostile takeover (e.g., Shleifer and Summers, 1988; Gokhale, Groshen, and Neumark, 1995). However, this cost-reduction effect may be attenuated if privatized firms expand and have to pay higher wages to attract new workers. Privatized firms may use more incentive pay or demand more effort, which could raise wages while increasing efficiency. If private firms earn more profits due to either higher efficiency or ability to exercise market power, those rents may be shared with workers. Productivity improvements imply higher wages for given unit labor costs. 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 employment and wage effects of privatization, but also the existing empirical evidence is quite scant.<sup>1</sup> Research has been hampered by small sample sizes, short time series, and difficulties in

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<sup>1</sup> 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).

defining a comparison group of firms. In the first systematic study of the effects of privatization on employment and wages, for example, Haskel and Szymanski (1993) analyze 14 British publicly owned companies, of which four were privatized and the others were deregulated. Bhaskar and Khan (1995) estimate employment effects in 1983 and 1988 in 62 Bangladeshi jute mills, half of which were privatized. The largest sample in the existing 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.<sup>2</sup> Other studies have sometimes included employment as one of several possible indicators of firm performance, but not the focus of analysis.<sup>3</sup> Still others use individual worker data to estimate the effect of privatization on wages (e.g., Brainerd, 1998 and 2002). Overall, the results from this small body of previous research are inconclusive, containing both negative and positive estimates of the effects on workers.<sup>4</sup>

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 reform experiences among transition economies, with Hungary considered one of the most successful, Russia and Ukraine among the least successful, and Romania in the middle.<sup>5</sup> Figure 1 provides some initial evidence for these countries 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

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<sup>2</sup> Lopez-de-Silanes and Chong (2003) summarize the results from several studies of privatization in some Latin American countries. Kikeri (1998) and Birdsall and Nellis (2003) also survey a number of case studies and small sample surveys of privatization effects on labor in several developing economies. 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.

<sup>3</sup> Studies of privatization and firm performance that include employment 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. Dewenter and Malatesta (2001) estimate a negative effect of privatization on labor intensity (employment/assets) in their sample of 63 firms.

<sup>4</sup> Another related literature examines the public sector wage differential (e.g., Gyourko and Tracy, 1988; Gregory and Borland, 1999), where one identification approach relies on workers who switch sectors; our approach can be understood instead as using firms that switch sectors.

<sup>5</sup> 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.

corroborate workers' fears and many economists' expectations. A number of other events that 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.

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.<sup>6</sup> The data are very well-suited, however, for investigating the effects of privatization on a firm's wages and employment, essential questions in such an evaluation.<sup>7</sup>

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

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<sup>6</sup> Research on trade unions, including the classic work of Dunlop (1944), sometimes posits the wage bill as the workers' maximand in bargaining with the employer. If one accepts this point of view, then the wage bill may be considered as a rough but measurable index of worker welfare, although it should be admitted that many potentially relevant factors are omitted from such an objective function.

<sup>7</sup> This paper considers only the direct effect of privatization on a firm's employment and wage. General equilibrium effects could also be relevant to a welfare evaluation, but we leave them for future research except for providing some assessment, using our analysis of dynamics, as to whether they may influence the direct estimates. See Javorcik (2004) for a recent analysis of productivity spillovers from foreign ownership in Lithuania.

warranted. Second, we explore the economic mechanisms underlying the employment and wage outcomes of privatization using some simple decompositional methods. The estimated employment impact is decomposed into a productivity-improvement effect that tends to lower employment (for given output) and a scale-expansion effect that tends to raise it (holding productivity constant). The wage impact is decomposed into cost-reduction and productivity-improvement effects, with expected negative and positive signs, respectively. For example, small estimated impacts of privatization on employment or wages could result either from little restructuring or from effects that are large but offsetting; the decompositional analysis sheds light on such questions. 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 provide information on how quickly any effects appear and whether they are merely temporary, for instance because state-owned employers adapt their own employment and wage behavior, or whether they represent 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.”<sup>8</sup> These models control not only for fixed differences among firms but also for 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

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<sup>8</sup> Ashenfelter and Card (1985) and Heckman and Hotz (1989) use the random trend model in evaluating training programs, and 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. With this exception, no previous study of privatization, corporate governance, or firm performance has used this method.

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 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.<sup>9</sup> 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,”

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<sup>9</sup> The Russian employment data also include 1985-1988 and 1990-1991, but wages are unavailable in those years. We report our results below using a consistent sample across equations, but the employment results are quite similar to those using the full employment sample.

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*).<sup>10</sup> We include only “old” firms, defined as existing prior to 1992 (1990 in Hungary) or state-owned 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

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<sup>10</sup> Recycling (NACE 37) is also excluded because of noncomparability with the *OKONKh* classification system.



included only if they are majority state in their first observation in the regressions, so that the base category consists exclusively of state firms.<sup>11</sup>

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 in any year 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 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). Average real wages have increased in Hungary and Romania but fallen in Russia and Ukraine.

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, 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

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<sup>11</sup> In Russia and Ukraine, privatization started only after 1992, so firms that existed before this year in our data must be old. Romanian privatization was very gradual, starting with only a few firms in 1992 and 1993, so the old sample is not reduced by the lack of data in earlier years.

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.<sup>12</sup>

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) to match firms that exited in a given year with those that entered in the following year. These variables are available in all countries except Hungary, where we were able to use a separate Central Statistical Office dataset with information on reorganizations that broke links across years. 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 some links may still be 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

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<sup>12</sup> 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.

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 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, after a few “pilots,” privatization really began in earnest 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 (Frydman et al., 1993b). 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 used somewhat different mechanisms, but in general 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.<sup>13</sup> 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 of the manufacturing firms in our database 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.<sup>14</sup>

The table also contains the percentage of firms majority privatized to foreigners.<sup>15</sup> This fraction is by far the highest in Hungary, reaching nearly 16 percent of all entities by the end of

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<sup>13</sup> The Russian data do not contain an ownership variable before 1993, nor do they provide percentage shareholding. But virtually all the privatizations in our data are mass privatizations, so the earliest they could take place was October 1992, and nearly all led to majority private ownership (see, e.g., Boycko, Shleifer, and Vishny, 1995).

<sup>14</sup> We assume a single ownership change 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; 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 firms.

<sup>15</sup> 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.

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.<sup>16</sup> 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 extent of selection bias in the privatization process as well as the measured impact of privatization on employment and wages. Privatization through competitive sales (auctions or tender) to outside investors implies that the buyers fully assess the firm’s operating and financial performance as well as its potential for growth.<sup>17</sup> Thorough assessments of this sort are less likely under various types of giveaway privatization methods, where the buyer’s own capital is not put at risk. With mass privatization leading to highly dispersed ownership structures, there is also little incentive for any single acquirer to spend much effort on gathering such information. Comparing foreign to domestic investors, however, the latter may have special information on the extent of overstaffing and excessive wages, and thus the potential for restructuring. If these differences across firms tend to be fixed, then they will be removed by firm fixed effects, and if they tend to grow at a constant rate they will be removed by firm-specific trends.<sup>18</sup>

In considering the likely consequences of various privatization methods on employment and wages, it is useful to consider three mechanisms through which privatization may affect the firm’s behavior: productivity improvement, cost reduction, and scale expansion. Worker-

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<sup>16</sup> While smaller than the sample for domestic privatization, the foreign privatization sample nevertheless contains 519, 798, 194, and 452 pre-privatization and 1,596, 415, 333, and 213 post-privatization firm-year observations for Hungary, Romania, Russia, and Ukraine, respectively. Only in Hungary, because of the earlier timing of privatization and the less comprehensive nature of the available pre-1992 data, do we face a significant problem in tracking foreign-owned firms back to their state origins. In the other countries, our foreign samples represent nearly all privatizations of manufacturing firms to foreign investors (save those with missing values for essential variables).

<sup>17</sup> For legal entities buying state-owned companies or assets, some assessments may be legally required, as “due diligence.”

<sup>18</sup> Appendix A contains a brief analysis of the preprivatization characteristics of firms later privatized as possible indicators of selection bias in the privatization process.

owners are likely to oppose wage cuts and labor-saving restructuring, and they are unlikely to have incentives or resources to expand (Bonin, Jones, and Putterman, 1993). Outside blockholders, on the other hand, should favor productivity-enhancing and cost-reducing restructuring, and they are also more likely to respond to opportunities for expansion. This seems especially true for foreign investors with access to management skills, new technologies, and financing. Outsiders with small shareholdings may also benefit from efficiency improvements and scale expansion, but they are less likely to influence the firm's behavior. Therefore, the productivity, cost, 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 tend to be offsetting, however, their net effects on employment and wages are a priori ambiguous. The next section describes our methods for estimating these effects empirically.

#### **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 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.

Most studies have too few observations at their disposal to be able to account for industry-specific fluctuations; yet if these are correlated with privatization, the estimates may be biased. Taking advantage of the large samples in our data, we include a full set of industry-year interactions to control for such factors.<sup>19</sup> 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 in ways that are correlated with ownership change, for instance because potential investors see growth opportunities, we add firm-specific trends to some specifications (labeled FE&FT). Taken together with the full set of industry-year interactions, the fixed effect and firm-specific trends also control for changes in the environment, including both competition from other firms and subsidies (implicit or explicit) from the government, that may also influence wage and employment behavior at the firm level.<sup>20</sup>

Another 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.

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<sup>19</sup> The industrial classification is chosen to be consistent across countries, based on the trade-off between disaggregation and number of observations, where we specify a minimum of 50 observations per year per country for each industry. As a result of this procedure, we distinguish 10 industries.

<sup>20</sup> Firm fixed effects and trends also control for regional differences in the economic environment, for instance in labor market conditions that may affect employment and wage behavior. We have also estimated equations containing full-sets of region-year interactions, which control for region-specific shocks that create deviations from average and trend regional differences (since these are captured by the FE and FT). The results from these estimates are qualitatively quite similar to those we present in the paper.

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.<sup>21</sup> 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. The dynamic specification is also useful for assessing the possibility of general equilibrium effects resulting from labor market competition among employers. If foreign-owned firms tend to pay higher wages, for instance, then the others (under state and domestic private ownership) may respond by raising their own wages in order to compete for workers, and our estimates of the conditional difference of foreign ownership will be an understatement of the true effect. A complete general equilibrium analysis is beyond the scope of this paper, but if the spillovers are not instantaneous, then they may be reflected in the dynamics of the effects: large initially, but diminishing as domestic firms “catch up” to the foreign practice.

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  $T$ .<sup>22</sup> In alternative specifications,  $y_{it}$  is the natural logarithm of the firm’s wage bill ( $wb$ ), employment ( $e$ ), and average wage rate per worker ( $w$ ); because  $wb = e + w$ , 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.<sup>23</sup> 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

<sup>21</sup> 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. La Porta and Lopez-de-Silanes (1999) find negative anticipatory effects in their study of Mexican privatization.

<sup>22</sup>  $J=10$ , as noted above.  $T$  varies by country: 17 in Hungary, 11 in Romania, and 12 in Russia and Ukraine.

<sup>23</sup> 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.



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  $\alpha_{1i}$  is a fixed unobserved effect and  $\alpha_{2i}$  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.<sup>24</sup> The coefficient of interest  $\delta$  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  $\boldsymbol{\theta}_{it} \equiv (Domestic_{it-1}, Foreign_{it-1})$ , and  $\boldsymbol{\delta} \equiv (\delta_d, \delta_f)$  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  $\tau$  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  $\boldsymbol{\theta}_{it} \equiv (\mathbf{Domestic}_{it\tau}, \mathbf{Foreign}_{it\tau})$ ,  $\boldsymbol{\delta} \equiv (\boldsymbol{\delta}_{\tau d}, \boldsymbol{\delta}_{\tau f})$ , 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 use the dynamic specification to implement specification tests that help determine which of the OLS, FE, or FE&FT models are most appropriate. Our method generalizes the Heckman and 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  $\tau = -$

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<sup>24</sup> 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. Some assumption on the first “post” year is necessary in this analysis, but as our estimates of the dynamics of the effect suggest, the results are not at all sensitive to this assumption.

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.

We also conduct some further analyses to shed light on the economic mechanisms that underlie the estimated impacts of privatization on employment and wages. For this purpose, it is useful to decompose each of these into two further effects: the scale and productivity effects for employment, and the productivity and cost effects for the wage. In order to assess the relative importance of the productivity and scale effects in accounting for the employment impact of privatization, we decompose the latter by estimating specifications of equation (1) where the dependent variables  $y_{it}$  are the natural logarithms of output ( $x$ ) and labor productivity ( $lp = x - e$ , with  $e = \ln[\text{employment}]$ ). This decomposition follows from the basic identity:

$$e \equiv x - (x - e) \equiv x - lp. \quad (2)$$

Linearity of the estimators of the privatization effect with these dependent variables implies that the estimated impacts on employment and wages can also be decomposed. We estimate the equations with the ownership specification  $\theta_{it} \equiv (Domestic_{it-1}, Foreign_{it-1})$ , so that  $\delta \equiv (\delta_d, \delta_f)$ . Thus,  $\delta^e = \delta^x - \delta^{lp}$ , where superscripts represent the dependent variable in each equation. We similarly decompose the wage changes by estimating equations with the natural logarithm of unit labor cost ( $ulc = w + e - x$ ) and productivity ( $lp$ ), relying on the identity:

$$w \equiv (w + e - x) + (x - e) \equiv ulc + lp. \quad (3)$$

The estimated wage effect of privatization is therefore equal to the unit labor cost effect plus the productivity effect:  $\delta^w = \delta^{ulc} + \delta^{lp}$ . Since the unit labor cost effect is expected to be negative, the cost and productivity effects may tend to work in opposite directions in affecting wages. In these regressions, 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

The results from estimating relation (1) with the natural log of the wage bill as the dependent variable are displayed in Table 4. Equations are fitted by OLS, fixed firm effects (FE), and firm-specific trends (FE&FT). Starting with the specification estimating the average post-privatization effect ( $Private_{it-1}$ ), the OLS estimates of  $\delta_p$  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 hurts workers.<sup>25</sup>

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  $\delta_d$  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  $\delta_p$ , 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  $\delta_d$  and  $\delta_p$  are nearly identical.

By contrast, the estimated effects of foreign privatization in Table 4 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. 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 employment and wage components in Table 5. Again, while the OLS estimates are usually large in magnitude and highly significant,

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<sup>25</sup> The results from estimating the change in the wage bill from two years before to two years after privatization imply substantial negative effects in all countries. 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.

the coefficients tend to be much smaller and less significant in the FE and FE&FT specifications. Beginning with domestic ownership, the FE and FE&FT results for Hungary, Russia, and Ukraine imply essentially no effect on employment. The only large (positive) effect of domestic ownership is measured for Romanian employment in the FE specification, but it becomes essentially zero when FTs are added. Concerning wage effects, the OLS specification implies positive effects of domestic privatization in three of the four countries in the range of 5-8 percent; only in Hungary is the OLS coefficient statistically insignificant (and the sign is negative). With FEs and FTs added, all the coefficients drop in magnitude. In Romania and Ukraine, the coefficients are small and always statistically insignificant. But in the FE&FT model for Hungary and both the FE and FE&FT models for Russia, the domestic coefficients are statistically significant and negative, with magnitudes in the range of 3–5 percent. Thus, the data provide some evidence of negative wage impacts of domestic privatization in these two countries. The magnitudes are small, however. Differences of minus 3–5 percent are small relative to the fluctuations of wages in these countries during this period (as is evident, for instance, from Figure 1). They are also small compared with standard estimates of the union relative wage effect, which generally lie in the range from 15 to 20 percent.<sup>26</sup> Finally, there is some reason to believe that the coefficients may be downward biased by measurement error, as we discuss shortly.

By contrast with the small to negligible domestic results, the effects of privatization to foreign investors 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. Below, we provide the results from several specification tests that help to choose among these estimates, but the consistent finding across all of them is that there is no evidence whatsoever that privatization to foreign investors negatively affects either employment or wages.

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<sup>26</sup> For instance, Card's (1996) baseline estimate of the union relative wage effect is 0.17 and ranges from 0.14 to 0.21 depending on the misclassification rate assumed.

How might various types of potential measurement error affect these conclusions? There are several types that should be considered. First, as discussed in Section 2, wages may be systematically under-reported to avoid taxes. If the magnitude of under-reporting is correlated with privatization, then our estimates may be biased. However, as we noted earlier, knowledgeable local observers report that wage under-reporting is most common in small start-up firms that are not included in our sample; among the firms of the old sector we study, the practice is most common in firms acquired by domestic investors, while it is unusual in the state sector (where controls are tight) and among foreign firms (especially larger ones, like those in our sample). Therefore, our estimates of the effects of foreign privatization on wages are unlikely to be biased, while the domestic privatization effects could be downward biased. This implies that the small negative coefficients on domestic privatization that we receive for Hungary and Russia might be due to this under-reporting phenomenon, and it further supports the conclusion that privatization has not had a substantial negative effect on workers in these countries. Second, delays in wage payments (arrearages), which have been common in Russia and Ukraine (although not in Hungary and Romania), create another type of measurement error in accrued wage obligations; however, they appear to be uncorrelated with domestic privatization and negatively associated with foreign ownership (e.g., Earle and Sabirianova, 2002). This would imply that the foreign wage effect is understated, again strengthening the case that foreign owners benefit workers, at least on this measure. Third, there may be measurement error in employment associated with unpaid leaves, which result in employment being overstated since the workers are still officially employed. Again, however, the incidence of such absences does not seem to differ much between state-owned and domestic privatized firms, while it seems to be lower under foreign ownership. This implies that our estimated employment effects of foreign privatization may be understated. Finally, the privatization variables may also be measured with error, although our cleaning procedures paid particular attention to consistency in these measures, as discussed in Section 2. Moreover, while measurement error due to misclassification may bias coefficients towards zero, particularly in longitudinal data, the problem is less severe when there are substantial numbers of observations on “switchers,” as we

have for domestic privatization in our data.<sup>27</sup> This consideration could play a bigger role for foreign privatization, especially in Russia and Ukraine where the number of observations are smaller (although still comprehensively covering the manufacturing sector), and it could explain why we observe substantial attenuation of the coefficients as FEs and particularly FTs are added to the equations. In any case, it again implies that the estimated foreign effects are if anything understated.

Next, we turn to the results from permitting the privatization effects to vary around the privatization year. The estimated coefficients from the dynamic FE and FE&FT specifications for employment and the wage rate are plotted in Figures 2 and 3.<sup>28</sup> 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. Consistent with the average effects reported in Table 5, 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 more than 2 years after privatization jumps to 40 percent (which is also consistent with the large positive estimate in Table 5), 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 relatively small in magnitude in both countries (again, for instance, compared to a standard union relative wage effect). 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.

The dynamics of the foreign privatization effects show much larger changes compared to the domestic effects, again consistent with the results in Table 5. 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

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<sup>27</sup> In addition to Card (1996), see Freeman (1984) for an analysis of misclassification error and the relationship between the magnitude of bias and the fraction of switchers in the data.

<sup>28</sup> The graphs report only coefficient estimates, and only from FE and FE&FT specifications for employment and wages, to save space. The full set of regression results, including standard errors, is reported in Appendix B.

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  $\tau = 3+$  (3 and more years after privatization), 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 assessing possible general equilibrium effects associated with ownership change. One possibility, for example, is that foreign investors enter with a policy of paying higher wages than the current norm, but domestic owners (state and privatized) respond by increasing wages to compete with the foreign owners on the labor market. This would imply that the positive foreign effects we have estimated may be understated, at least in the long run. To take another example, if foreign investors enter with the goal of expanding their businesses by hiring additional labor, then the spillover may be negative, as workers move, employment falls in domestically owned businesses, and it rises at foreign-owned firms. If domestic private owners tend to pay lower wages and cut employment relative to the state, then these effects may work in the opposite direction. In either case, however, it stands to reason that the spillover effects may take time to manifest themselves: initially, privatization could produce a difference in employment or wage behavior, but the difference would fall as state firms adjust. However, none of the graphs in Figures 2 and 3 shows such a pattern. In general, the estimated effect of ownership, where it is substantial, tends to increase rather than decrease with the length of time since privatization. Of course, in many cases (particularly with domestic privatization) we estimate zero to negligible ownership effects, and we cannot fully rule out the possibility that this results from such a rapid response of state firms that the convergence cannot be detected in our data. But the steady widening of the foreign gap in a number of countries and specifications implies that any “catch-up” that may be occurring is dominated by the ownership difference, and that the evidence of positive impacts of foreign ownership on employment and wages represents long-run effects.

The results of the dynamic estimation are also valuable for carrying out specification tests on pre-privatization behavior, variants of the Heckman-Hotz (1989) pre-program tests. Table 6



shows the results of  $F$  tests of the joint probability that the privatization effects one and two years before privatization are different from zero.<sup>29</sup> 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.

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.

We next exploit the decompositions implied by the identities (2) and (3) to explore the economic mechanisms that underlie the estimated impacts of privatization on employment and wages. Our finding of essentially zero employment effects of domestic privatization in all four countries, for example, could result from new private owners failing to improve productivity, or it could result from scale expansion that offsets the productivity effect of private ownership. Our finding of some evidence of a positive impact of foreign privatization on employment indicates a

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<sup>29</sup>  $T$  tests on the effect two years before privatization (shown in Appendix B) imply similar conclusions as the  $F$  tests in Table 7.

positive scale effect, but it is not clear whether productivity improvements work to attenuate the net impact on employment. To address these questions, Figure 4 shows the results from estimating Equation (1) with  $y_{it}$  representing  $e = \ln(\text{employment})$ ,  $x = \ln(\text{output})$ , and  $lp = \ln(\text{labor productivity})$  in turn, and  $\mathbf{w}_t \equiv (1, t)$  – the FE&FT model.<sup>30</sup> We specify  $\boldsymbol{\theta}_{it} \equiv (\text{Domestic}_{it-1}, \text{Foreign}_{it-1})$ , so that  $\boldsymbol{\delta} \equiv (\delta_d, \delta_f)$  and, as a result of identity (2),  $\boldsymbol{\delta}^e = \boldsymbol{\delta}^x - \boldsymbol{\delta}^{lp}$ , where superscripts denote the relevant dependent variables. The first bar for each country and owner-type reproduces the coefficients  $\boldsymbol{\delta}^e$  from Table 5, while the next two contain  $\boldsymbol{\delta}^x$  and  $\boldsymbol{\delta}^{lp}$ , respectively.

In all four countries, the decomposition of the employment impact in Figure 4 shows scale and productivity effects that are much larger under foreign than domestic ownership. Except in Romania where the two effects are both large (0.3) and exactly cancel, the foreign scale effect dominates the productivity effect, resulting in a net positive employment impact. 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). Both domestic and foreign privatization raise productivity in Hungary, Romania, and Ukraine, but only foreign privatization does so in Russia.<sup>31</sup> In all countries, domestic ownership creates much smaller scale and productivity effects, but they are again similar in magnitude to each other for each country, resulting in very small net employment impacts. However, there is a pronounced contrast between sizable domestic ownership effects in Hungary and Romania and negligible domestic effects in Russia and Ukraine.

Turning to the wage decomposition on the basis of identity (3), Figure 5 shows the results of estimating Equation (1) with  $y_{it}$  representing  $w = \ln(\text{wage})$ ,  $ulc = \ln(\text{unit labor cost})$ , and  $lp = \ln(\text{labor productivity})$  in turn, so that  $\boldsymbol{\delta}^w = \boldsymbol{\delta}^{ulc} + \boldsymbol{\delta}^{lp}$ . As we found with the employment decomposition, a striking regularity in Figure 5 is that the foreign effects are much larger across the board: new foreign owners tend to lower costs and raise productivity more than private domestic owners. The effects tend to work in opposite directions in affecting wages, but the net

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<sup>30</sup> Appendix C contains the coefficient estimates and standard errors corresponding to the FE&FT estimates in Figures 4 and 5, as well as tests for differences between domestic and foreign effects and the analogous sets of results for otherwise similar FE models.

<sup>31</sup> We have also estimated a number of variants of total factor productivity, with similar results to those displayed for labor productivity.

impact is positive for foreign ownership in all four countries, while it is negative but small under domestic ownership in Hungary and Russia and almost exactly zero in Romania and Ukraine. The positive wage impact of foreign ownership therefore occurs in spite of greater success in reducing costs and could reflect the introduction of new technologies or incentives that raise productivity and wages.

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 7. The estimated  $\delta_d$  and  $\delta_f$  are always negative, and tiny everywhere except Hungary.<sup>32</sup> 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 a small negative effect in Russia.

## 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 when foreign ownership may be involved, 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.

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<sup>32</sup> 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).

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 from the program evaluation literature to assess and control for selection bias. Like most firm-level data, ours contain no information on fringe benefits, worker turnover, hours of work, and the composition of employment, nor can we track the subsequent experiences of any workers who happen to be displaced. Therefore, unless the wage bill is accepted as a sufficient indicator, 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 a crude indicator 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 for either outcome. Estimated by FE or FE&FT, the employment effects are never both 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 most of the foreign effects tend to grow steadily from the privatization year onwards.

Measurement error may play some role in these patterns, but our analysis shows that the direction of the bias tends to result in an understatement of privatization effects. Under-reporting of wages tends to bias the domestic privatization effect on wages downwards, wage arrears tend to bias the wage effect of foreign privatization downwards, unpaid leave tends to bias the employment effect of foreign privatization downwards, and misclassification error tends to bias both the wage and employment effects of foreign privatization towards zero. We also find that privatized firms, both domestic and foreign, are less likely to exit from our databases than state-owned firms. Concerning the possibility of general equilibrium effects that might bias our results, in particular from a foreign ownership effect on wages, our analysis of dynamics is inconsistent with substantial spillovers, which should appear as temporary impacts on wages that gradually disappear under the influence of labor market competition; we do not find such a pattern. To summarize, the analysis of these potential problems bolsters the evidence rejecting any substantial negative impact of privatization on either employment or wages.

We explore possible explanations for our findings by considering three alternative mechanisms through which privatization may affect outcomes for workers: productivity, cost, and scale effects. Our decomposition analysis of employment into output and labor productivity

shows that domestic privatization has tended to produce gains in both scale and productivity that have offset each other in their consequences for workers. Similarly, our decomposition of wages into unit labor cost and productivity shows domestic privatization bringing about cost reductions and productivity improvements that have offsetting effects on wages. In Hungary and Romania, however, the scale, cost, and productivity effects of domestic ownership have all been large, while in Russia and Ukraine they have all been small. Foreign privatization has resulted in still much larger scale, productivity, and cost effects in all four countries, but the scale effects dominate the productivity effects, which in turn dominate the cost effects. The consequences are the increased relative employment and wages in foreign firms that we observe after privatization.

These cross-country and domestic versus foreign patterns are inconsistent with the simple trade-off in privatization between efficiency and worker welfare that has been assumed by many economists. In our data, efficiency-enhancing owners frequently 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.

Can these results be extrapolated outside our samples? Maybe privatization in other sectors and settings tends to produce negative consequences for workers that we do not observe in the manufacturing industries of the four countries we study. We cannot rule out this possibility entirely, but it seems to us that the opposite is more likely to be true. From the beginning of the transition process, the manufacturing sectors of these economies have always been expected to shed large numbers of employees, because they were the most bloated, heavily subsidized parts of the socialist economies, notorious for employing excess labor to meet plan targets and for paying disproportionately high wages to their workers. One might expect to find the largest negative employment and wage effects of privatization in just this setting. But we do not find such effects – not large ones, at any rate. It seems even less likely that such effects would be found in the nonmanufacturing sectors, particularly in services that have expanded rapidly during the transition, and even less so in market economies where a variety of

disciplinary devices other than active owners (such as market competition and good governance) may prevent state-owned firms from hoarding labor and paying excess wages to the same extent as occurred under state socialism.

Moreover, the absence of large negative effects of privatization holds consistently across all four of the countries, which span the distribution of reform experiences. If we had found large negative effects in Russia and Ukraine, towards one end of the spectrum, then we might be able to infer that other less-developed economies, perhaps those in Central Asia, would face similar problems. Or if we had found large negative effects in Hungary, the Eastern European economy closest to a developed market economy at the beginning of the privatization process, then we might deduce that such effects are, contrary to expectation, largest where the deviation from market outcomes is the least. We do not find any such patterns, however; rather, our findings reject the hypothesis of large negative consequences for employment and wages in all four countries. Our results, therefore, carry no obvious implication that privatization would tend to produce much lower employment and wages in other contexts. On the contrary, our results suggest that in contexts where state-owned firms are relatively well-disciplined, where privatization produces effective control by outside investors, and where growth opportunities abound, it seems most plausible that the effect would be similar to what we observe with foreign privatization in Hungary and Romania – a positive impact on both employment and wages.

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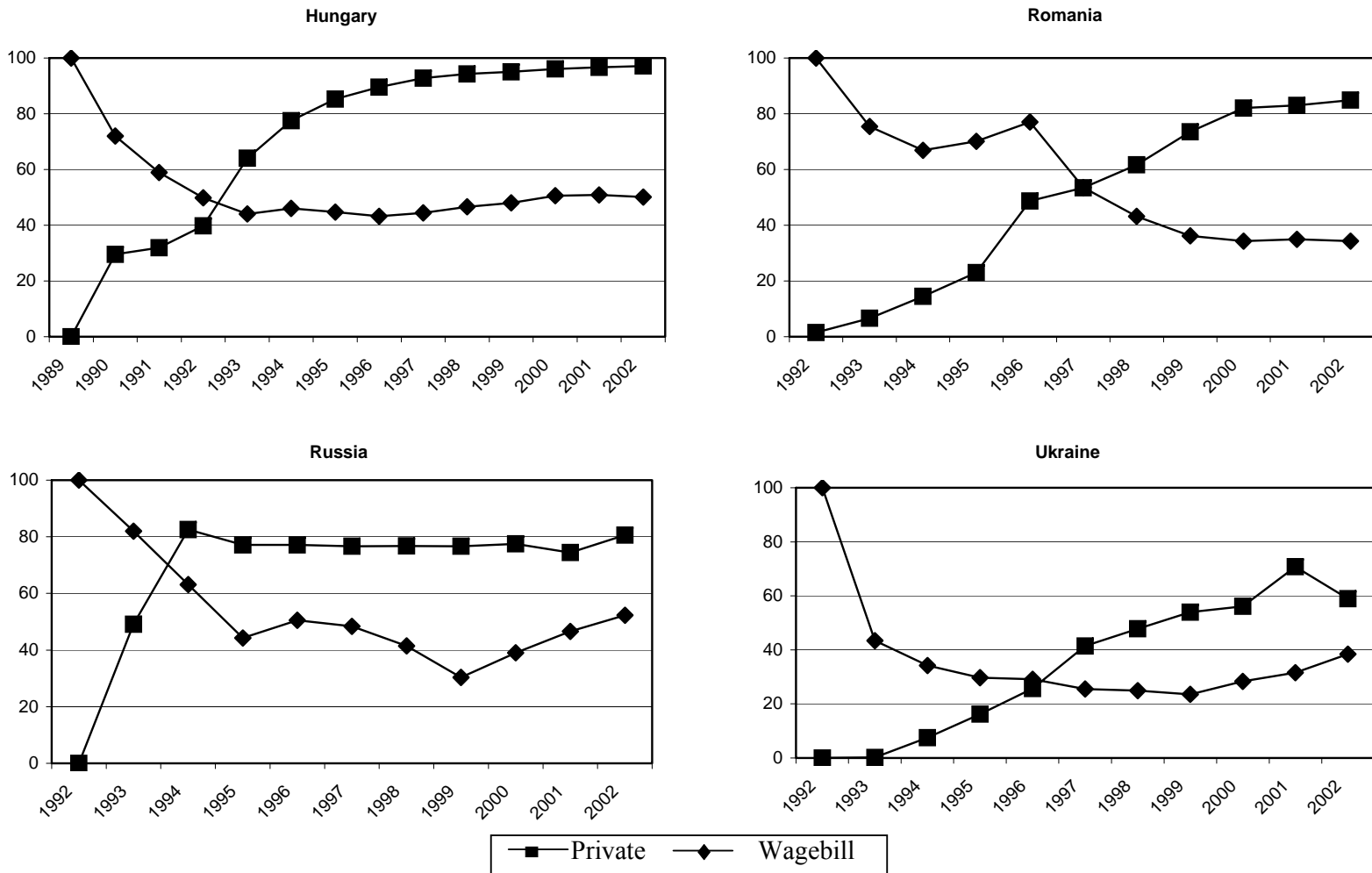
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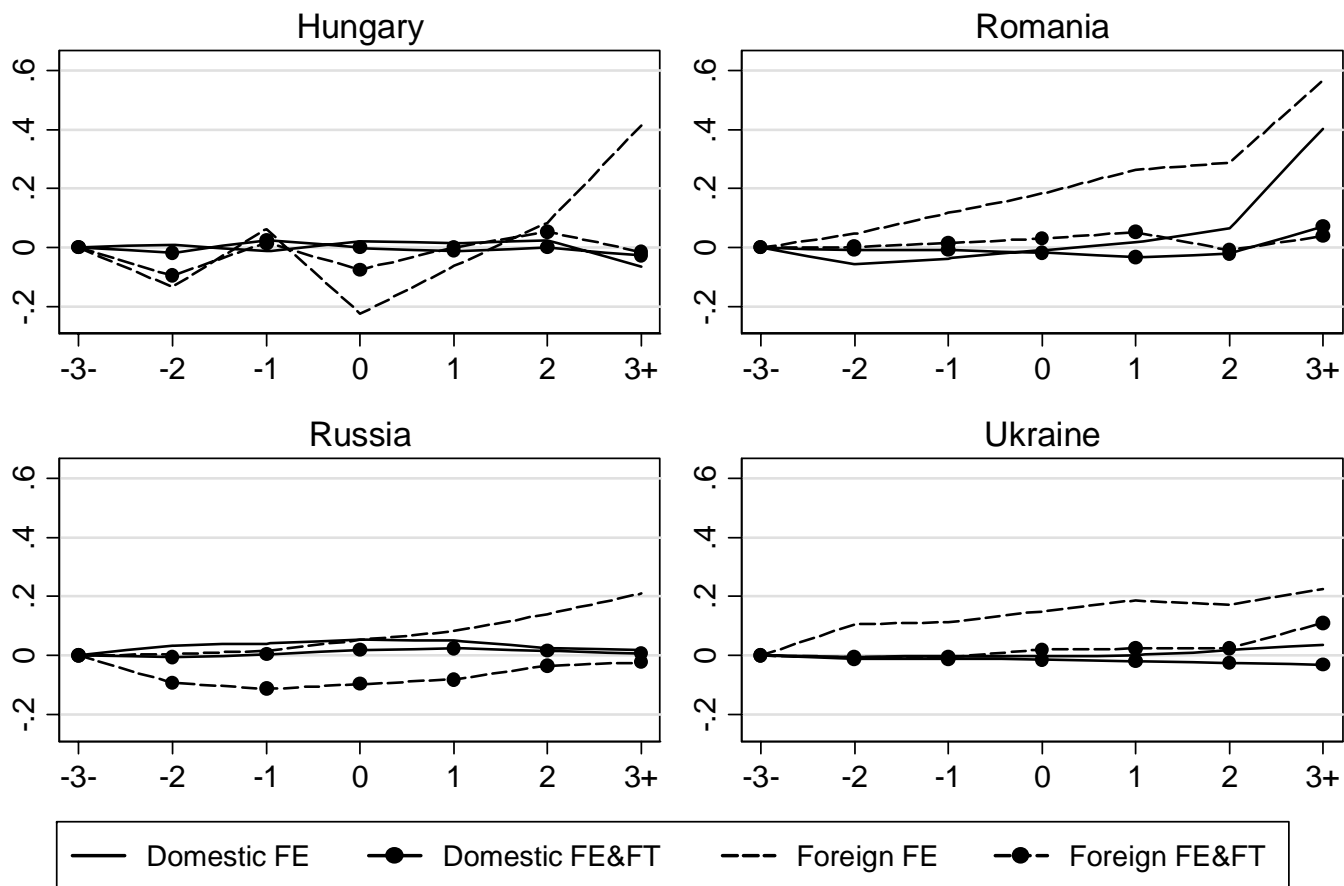
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Figure 1: Evolution of Average Real Wage Bill and Private Ownership



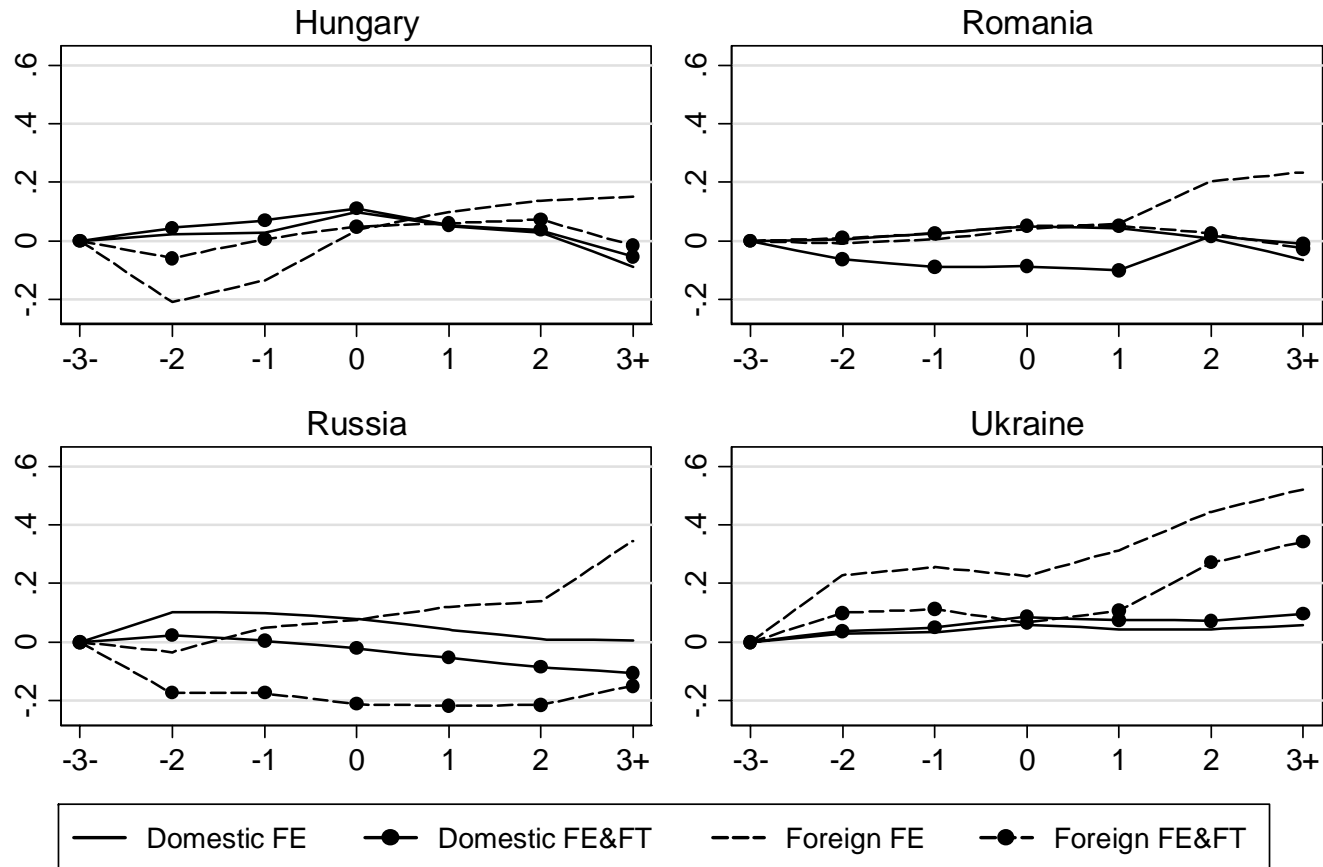
Notes: The graphs show the average real wage bill and percent of majority private firms, calculated from our data. The real wage bill is set at 100 in 1989 in Hungary and 1992 for Romania, Russia, and Ukraine.

Figure 2: Dynamics of Privatization Effect on Employment



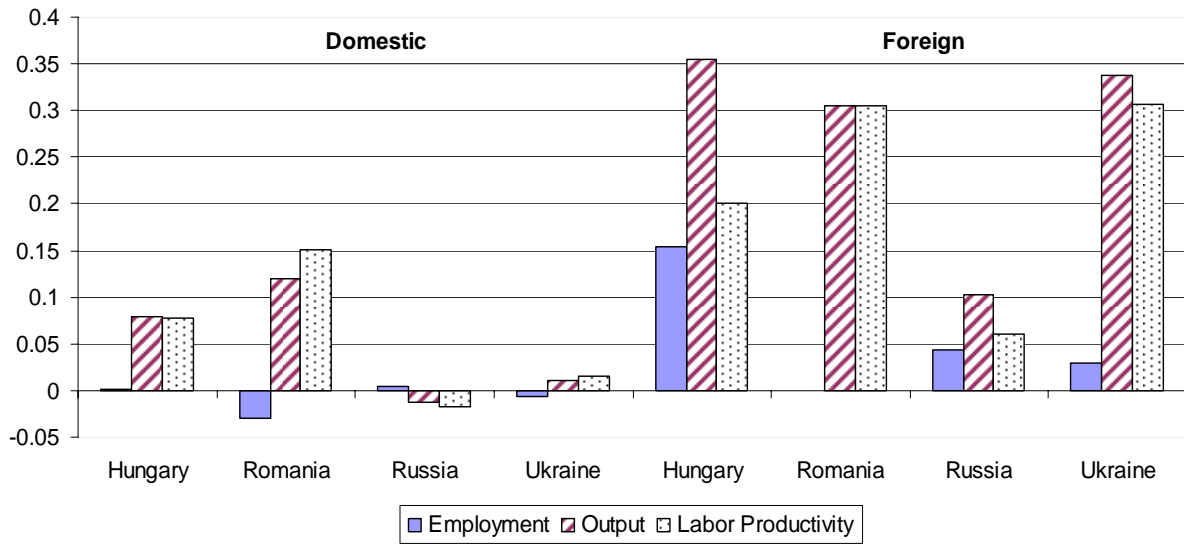
Notes: The graphs present regression coefficients on interactions between dummies for years before and after privatization and indicators for whether the firm is ever domestically or foreign privatized. The privatization year is “0,” “-3-” = 3 or more years before privatization (where effects are normalized at zero), and “3+” = 3 or more years after. Full sets of unrestricted industry-year dummies are included in each regression. FE includes firm fixed effects; FE&FT adds firm-specific trends.

Figure 3: Dynamics of Privatization Effect on Wages



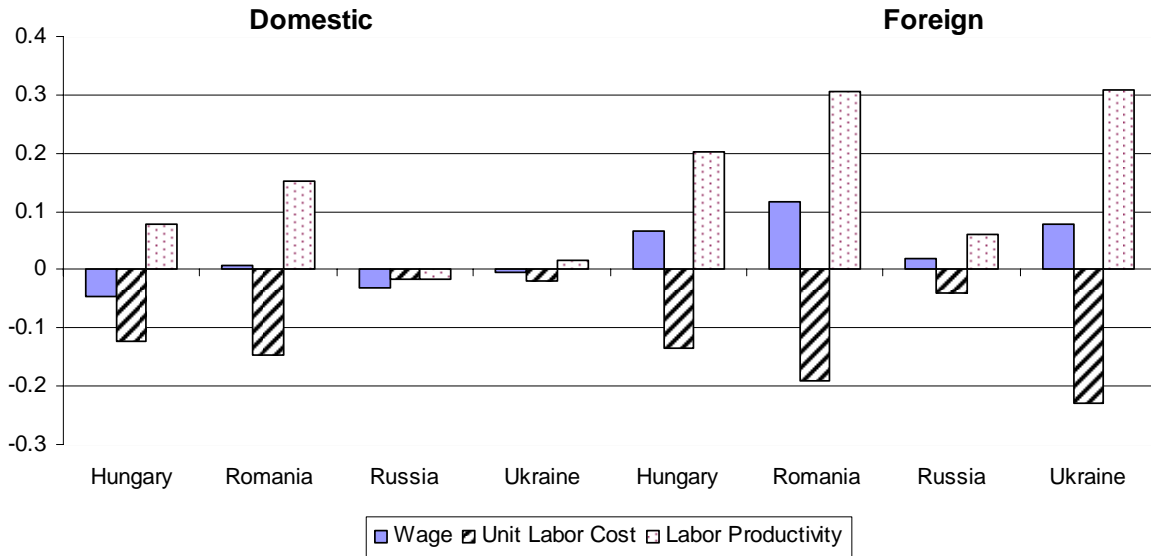
Notes: The graphs present regression coefficients on interactions between dummies for years before and after privatization and indicators for whether the firm is ever domestically or foreign privatized. The privatization year is “0,” “-3-” = 3 or more years before privatization (where effects are normalized at zero), and “3+” = 3 or more years after. Full sets of unrestricted industry-year dummies are included in each regression. FE includes firm fixed effects; FE&FT adds firm-specific trends.

**Figure 4: Decomposition of the Employment Effect into Scale and Productivity Effects**



Notes: The graph presents coefficients estimated separately by country from regressions of employment, output, and labor productivity on indicator variables for domestic and foreign privatization. Firm fixed effects, firm-specific trends, and full sets of unrestricted industry-year dummies are included in each regression. Coefficients (and standard errors) for the employment equation come from Table 5, and coefficients (standard errors) for the output and labor productivity equation estimates (and also for fixed effects specifications without trends) are shown in Appendix C.

**Figure 5: Decomposition of the Wage Effect into Cost and Productivity Effects**



Notes: These are estimated coefficients estimated separately by country from regressions of the natural log of the wage, unit labor cost, and labor productivity on indicators for domestic and foreign privatization. Firm fixed effects, firm-specific trends, and full sets of unrestricted industry-year dummies are included in each regression. Coefficients (and standard errors) for the wage equation come from Table 5, and coefficients (standard errors) for the unit labor cost and labor productivity equation estimates (and also for fixed effects specifications without trends) are shown in Appendix C.



**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

Notes: 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: Mean Employment and Wage in the First and Last Years of Analysis**

	Employment		Wage	
	First year	Last year	First year	Last year
Hungary	613.7 (1,214.7)	165.3 (422.9)	981.6 (352.9)	1,518.0 (1,896.3)
Romania	1234.0 (2,169.9)	414.3 (924.1)	60,847.6 (25,172.5)	69,920.5 (483,597.0)
Russia	621.4 (1296.2)	506.6 (1999.4)	65,814.2 (16,826.7)	40,168.7 (24,503.4)
Ukraine	805.2 (1,863.9)	472.0 (1,901.1)	9,516.6 (1,603.4)	6,708.8 (5,819.7)

Notes: The first year of analysis is 1986 in Hungary, 1992 in Romania, and 1989 in Russia and Ukraine; the last year is 2002 in all countries. Wage is annual, expressed in constant 2002 prices (thousands of HUF for Hungary, and thousands of ROL for Romania, RUB for Russia, and UAH for Ukraine). Precise definitions and sources are given in Section 2. Standard deviations are shown in parentheses.

**Table 3: Percentage of Firms Privatized – Majority Private and Majority Foreign**

	1992	1994	2002
<b>Hungary</b>			
Private	36.4	90.3	93.2
Foreign	4.7	13.5	15.8
<b>Romania</b>			
Private	0.0	4.7	84.6
Foreign	0.0	0.1	5.3
<b>Russia</b>			
Private	0.0	79.5	70.0
Foreign	0.0	0.4	0.4
<b>Ukraine</b>			
Private	0.0	7.9	81.3
Foreign	0.0	0.1	1.5

Notes: “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.

**Table 4: Estimated Wage Bill Effects of Privatization**

	Hungary	Romania	Russia	Ukraine
<b>OLS</b>				
	<b>Private</b>			
$\hat{\delta}_p$	-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
<b>FE</b>				
$\hat{\delta}_p$	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
<b>FE&amp;FT</b>				
$\hat{\delta}_p$	-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
<b>OLS</b>				
	<b>Domestic and Foreign</b>			
$\hat{\delta}_d$	-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)
$\text{Pr}(\hat{\delta}_f = \hat{\delta}_d)$	0.000	0.000	0.000	0.000
<b>FE</b>				
$\hat{\delta}_d$	-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)
$\text{Pr}(\hat{\delta}_f = \hat{\delta}_d)$	0.000	0.000	0.000	0.003
<b>FE&amp;FT</b>				
$\hat{\delta}_d$	-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)
$\text{Pr}(\hat{\delta}_f = \hat{\delta}_d)$	0.000	0.020	0.231	0.334
<i>Obs</i>	19,382	22,447	131,531	56,214

Notes: Full sets of unrestricted industry-year dummies are included in the regressions. Private = 1 if the firm is majority private at end of year  $t-1$ . 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=firm fixed effects; FT=firm-specific 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.

**Table 5: Estimated Employment and Wage Effects of Privatization**

	Hungary	Romania	Russia	Ukraine
<b>OLS</b>				
	<b>Employment</b>			
$\hat{\delta}_d$	-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
<b>FE</b>				
$\hat{\delta}_d$	-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
<b>FE&amp;FT</b>				
$\hat{\delta}_d$	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
<b>OLS</b>				
	<b>Wage</b>			
$\hat{\delta}_d$	-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
<b>FE</b>				
$\hat{\delta}_d$	-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
<b>FE&amp;FT</b>				
$\hat{\delta}_d$	-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

Notes: Foreign = 1 if the majority of shares are foreign-owned in year  $t-1$ . Domestic = 1 if private in year  $t-1$  but not majority-owned by foreigners. FE=firm fixed effects; FT=firm-specific 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 4. \* = significant at 5-percent level. \*\* = significant at 1-percent level.

**Table 6: Pre-Program Tests**

	Hungary	Romania	Russia	Ukraine
<b>OLS</b>				
	<b>Employment</b>			
<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)
<b>FE</b>				
<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)
<b>FE&amp;FT</b>				
<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)
<b>OLS</b>				
	<b>Wage</b>			
<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)
<b>FE</b>				
<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)
<b>FE&amp;FT</b>				
<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)

Notes: 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  $\delta_{2f} = \delta_{1f} = 0$ .

**Table 7: 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

Notes: 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.

### Appendix A: Preprivatization Characteristics of Firms Later Privatized

To provide diagnostic information about possible selection bias in the data, we estimate variants of equation (1) where the sample is restricted to state-owned firms (either never or not yet privatized, so that the single post dummy variable  $Private_{it-1} = 0$ ). We set  $\mathbf{w}_t \equiv 0$ , and  $\boldsymbol{\theta}_{it} \equiv Pre-Private_{it}$  in one specification, and  $\boldsymbol{\theta}_{it} \equiv (Pre-Domestic_{it}, Pre-Foreign_{it})$  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

$$y_{it} = \mathbf{D}_{jt}\boldsymbol{\gamma}_{jt} + \boldsymbol{\theta}_{it}\boldsymbol{\delta} + u_{it}. \quad (A1)$$

The table shows that the estimated differences vary greatly across countries, ownership types, and dependent variables. Romanian and Hungarian firms 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 similar pattern to the wage bill, except for a smaller magnitude of the coefficients in all countries but Romania. Wages, however, tend to be larger in firms to be privatized everywhere except for domestic firms in Hungary. The foreign results are more consistent, as firms that will be foreign-owned have higher wage bills, employment, and wages than either pre-domestic firms or always state firms in all four countries.

	Hungary	Romania	Russia	Ukraine
<b>Wage Bill</b>				
<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)
<b>Employment</b>				
<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)
<b>Wage</b>				
<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>Observations</i>	8,593	13,481	69,294	40,676

Notes: Estimates of Equation (A1). Standard errors (corrected for firm clustering) are shown in parentheses. \* = significant at 5-percent level. \*\* = significant at 1-percent level.

## Appendix B: Dynamics of Privatization Effects on Employment and Wages

	Hungary	Romania	Russia	Ukraine
<b>FE</b>				
		<b>Employment</b>		
$\hat{\delta}_{d\tau-2}$	0.011 (0.028)	-0.055** (0.012)	0.035** (0.013)	-0.004 (0.007)
$\hat{\delta}_{d\tau-1}$	-0.011 (0.027)	-0.036** (0.014)	0.041** (0.011)	-0.002 (0.009)
$\hat{\delta}_{d\tau0}$	0.023 (0.026)	-0.008 (0.016)	0.055** (0.012)	-0.003 (0.012)
$\hat{\delta}_{d\tau1}$	0.015 (0.027)	0.018 (0.018)	0.050** (0.013)	0.001 (0.014)
$\hat{\delta}_{d\tau2}$	0.026 (0.025)	0.065** (0.021)	0.026 (0.014)	0.018 (0.017)
$\hat{\delta}_{d\tau3+}$	-0.064 (0.038)	0.403** (0.036)	0.020 (0.017)	0.038 (0.021)
$\hat{\delta}_{f\tau-2}$	-0.132 (0.096)	0.047 (0.035)	0.005 (0.068)	0.106** (0.037)
$\hat{\delta}_{f\tau-1}$	0.064 (0.067)	0.118** (0.039)	0.017 (0.047)	0.113** (0.043)
$\hat{\delta}_{f\tau0}$	-0.223** (0.065)	0.183** (0.046)	0.053 (0.046)	0.150** (0.060)
$\hat{\delta}_{f\tau1}$	-0.062 (0.061)	0.264** (0.053)	0.082 (0.056)	0.187** (0.069)
$\hat{\delta}_{f\tau2}$	0.084 (0.051)	0.287** (0.082)	0.014* (0.060)	0.172 (0.113)
$\hat{\delta}_{f\tau3+}$	0.413*** (0.073)	0.566** (0.120)	0.209** (0.072)	0.225* (0.117)
<b>FE&amp;FT</b>				
$\hat{\delta}_{d\tau-2}$	-0.017 (0.022)	-0.007 (0.012)	-0.006 (0.016)	-0.009 (0.006)
$\hat{\delta}_{d\tau-1}$	-0.026 (0.023)	-0.008 (0.014)	0.004 (0.018)	-0.009 (0.009)
$\hat{\delta}_{d\tau0}$	-0.000 (0.023)	-0.016 (0.015)	0.019 (0.021)	-0.014 (0.013)
$\hat{\delta}_{d\tau1}$	-0.011 (0.020)	-0.031* (0.015)	0.025 (0.024)	-0.018 (0.017)
$\hat{\delta}_{d\tau2}$	-0.001 (0.015)	-0.020 (0.013)	0.016 (0.027)	-0.024 (0.021)
$\hat{\delta}_{d\tau3+}$	-0.025 (0.022)	0.071** (0.023)	0.008 (0.031)	-0.032 (0.027)
$\hat{\delta}_{f\tau-2}$	-0.094 (0.068)	0.003 (0.041)	-0.093 (0.075)	-0.005 (0.033)
$\hat{\delta}_{f\tau-1}$	0.017 (0.057)	0.017 (0.049)	-0.112 (0.081)	-0.005 (0.049)
$\hat{\delta}_{f\tau0}$	-0.074 (0.055)	0.031 (0.057)	-0.097 (0.100)	0.020 (0.070)
$\hat{\delta}_{f\tau1}$	0.002 (0.045)	0.053 (0.054)	-0.079 (0.118)	0.024 (0.093)
$\hat{\delta}_{f\tau2}$	0.054 (0.032)	-0.008 (0.040)	-0.034 (0.133)	0.025 (0.159)
$\hat{\delta}_{f\tau3+}$	-0.014 (0.037)	0.039 (0.081)	-0.023 (0.157)	0.111 (0.183)



Appendix B (continued)

	Hungary	Romania	Russia	Ukraine
<b>FE</b>			<b>Wages</b>	
$\hat{\delta}_{d\tau-2}$	0.023 (0.021)	0.006 (0.010)	0.103** (0.018)	0.028** (0.009)
$\hat{\delta}_{d\tau-1}$	0.028 (0.019)	0.026* (0.012)	0.099** (0.013)	0.034** (0.011)
$\hat{\delta}_{d\tau0}$	0.100** (0.015)	0.050** (0.011)	0.080** (0.013)	0.060** (0.131)
$\hat{\delta}_{d\tau1}$	0.051** (0.014)	0.044** (0.011)	0.043** (0.014)	0.045** (0.015)
$\hat{\delta}_{d\tau2}$	0.029* (0.012)	0.008 (0.016)	0.010 (0.015)	0.044** (0.018)
$\hat{\delta}_{d\tau3+}$	-0.087** (0.014)	-0.064** (0.017)	0.006 (0.017)	0.057** (0.021)
$\hat{\delta}_{f\tau-2}$	-0.208** (0.085)	-0.008 (0.031)	-0.034 (0.139)	0.229** (0.088)
$\hat{\delta}_{f\tau-1}$	-0.134* (0.055)	0.005 (0.028)	0.049 (0.101)	0.256* (0.115)
$\hat{\delta}_{f\tau0}$	0.037 (0.035)	0.041 (0.032)	0.075 (0.101)	0.226* (0.107)
$\hat{\delta}_{f\tau1}$	0.100** (0.035)	0.059 (0.044)	0.121 (0.105)	0.313* (0.132)
$\hat{\delta}_{f\tau2}$	0.138** (0.026)	0.203** (0.062)	0.139 (0.127)	0.445** (0.114)
$\hat{\delta}_{f\tau3+}$	0.152** (0.032)	0.235** (0.045)	0.345** (0.130)	0.521** (0.140)
<b>FE&amp;FT</b>				
$\hat{\delta}_{d\tau-2}$	0.045* (0.020)	0.010 (0.011)	0.025 (0.020)	0.038** (0.009)
$\hat{\delta}_{d\tau-1}$	0.071** (0.020)	0.026 (0.014)	0.005 (0.020)	0.049** (0.013)
$\hat{\delta}_{d\tau0}$	0.110** (0.015)	0.051** (0.013)	-0.020 (0.024)	0.086** (0.016)
$\hat{\delta}_{d\tau1}$	0.054** (0.013)	0.052** (0.012)	-0.054 (0.027)	0.075** (0.020)
$\hat{\delta}_{d\tau2}$	0.038** (0.012)	0.027** (0.011)	-0.085** (0.013)	0.074** (0.025)
$\hat{\delta}_{d\tau3+}$	-0.054** (0.014)	-0.025 (0.016)	-0.105** (0.035)	0.097** (0.030)
$\hat{\delta}_{f\tau-2}$	-0.058 (0.068)	-0.062 (0.039)	-0.174 (0.114)	0.100 (0.083)
$\hat{\delta}_{f\tau-1}$	0.007 (0.055)	-0.088* (0.044)	-0.174* (0.089)	0.112 (0.111)
$\hat{\delta}_{f\tau0}$	0.050 (0.036)	-0.086 (0.048)	-0.212* (0.110)	0.067 (0.117)
$\hat{\delta}_{f\tau1}$	0.060* (0.031)	-0.101* (0.051)	-0.217 (0.117)	0.108 (0.148)
$\hat{\delta}_{f\tau2}$	0.074** (0.022)	0.018 (0.031)	-0.214 (0.174)	0.270 (0.165)
$\hat{\delta}_{f\tau3+}$	-0.015 (0.022)	-0.008 (0.042)	-0.148 (0.165)	0.342 (0.191)

Notes: These coefficients correspond to those graphed in Figures 2 and 3. FE includes firm fixed effects; FT add firm-specific trends. Standard errors corrected for firm clustering are shown in parentheses. \* = significant at 5-percent level. \*\* = significant at 1-percent level.

**Appendix C: Estimated Labor Productivity, Unit Labor Cost,  
and Output Effects of Foreign and Domestic Privatization**

	Hungary	Romania	Russia	Ukraine
<b>FE</b>				
<b>Labor Productivity</b>				
$\hat{\delta}_d$	0.143** (0.023)	0.300** (0.025)	-0.026* (0.013)	0.052** (0.019)
$\hat{\delta}_f$	0.535** (0.055)	0.477** (0.080)	0.340** (0.135)	0.478** (0.145)
Pr( $\hat{\delta}_f = \hat{\delta}_d$ )	0.000	0.028	0.007	0.004
<b>FE&amp;FT</b>				
$\hat{\delta}_d$	0.077** (0.025)	0.151** (0.020)	-0.017 (0.010)	0.016 (0.017)
$\hat{\delta}_f$	0.201** (0.033)	0.305** (0.092)	0.060 (0.131)	0.307* (0.144)
Pr( $\hat{\delta}_f = \hat{\delta}_d$ )	0.025	0.100	0.559	0.044
<b>FE</b>				
<b>Unit Labor Cost</b>				
$\hat{\delta}_d$	-0.170** (0.021)	-0.323** (0.025)	-0.021 (0.012)	-0.049** (0.015)
$\hat{\delta}_f$	-0.228** (0.045)	-0.242** (0.062)	-0.096 (0.103)	-0.173 (0.101)
Pr( $\hat{\delta}_f = \hat{\delta}_d$ )	0.204	0.185	0.465	0.221
<b>FE&amp;FT</b>				
$\hat{\delta}_d$	-0.123** (0.021)	-0.145** (0.020)	-0.015 (0.010)	-0.020 (0.015)
$\hat{\delta}_f$	-0.135** (0.044)	-0.190* (0.084)	-0.041 (0.112)	-0.228 (0.125)
Pr( $\hat{\delta}_f = \hat{\delta}_d$ )	0.789	0.603	0.819	0.097
<b>FE</b>				
<b>Output</b>				
$\hat{\delta}_d$	0.114** (0.039)	0.487** (0.041)	-0.033* (0.016)	0.069** (0.023)
$\hat{\delta}_f$	0.962** (0.086)	0.762** (0.120)	0.493** (0.156)	0.613** (0.174)
Pr( $\hat{\delta}_f = \hat{\delta}_d$ )	0.000	0.022	0.001	0.002
<b>FE&amp;FT</b>				
$\hat{\delta}_d$	0.079** (0.029)	0.120 (0.026)	-0.012 (0.011)	0.010 (0.019)
$\hat{\delta}_f$	0.355** (0.069)	0.305** (0.109)	0.103 (0.140)	0.337* (0.161)
Pr( $\hat{\delta}_f = \hat{\delta}_d$ )	0.000	0.100	0.413	0.044

Notes: The FE&FT coefficient estimates correspond to those reported in Figures 4 and 5, while the standard errors and FE estimates supplement those results. Other notes and numbers of observations are same as in Table 5. \* = significant at 5-percent level. \*\* = significant at 1-percent level.