

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

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Executive Summary

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.

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.¹ Research has been hampered by small sample sizes, short time series, and difficulties in

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

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.² Other studies have sometimes included employment as one of several possible indicators of firm performance, but not the focus of analysis.³ 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.⁴

In this paper, we undertake an empirical analysis of the effects of privatization on the wage bill, employment, and wage rates of firms in Hungary, Romania, Russia, and Ukraine – where thousands of businesses were privatized in a relatively short period of time during the 1990s. These four countries span the range of 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.⁵ Figure 1 provides some initial evidence for these countries on the relationship of privatization and the wage bill, graphing the evolution of the

² 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.

³ 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.

⁴ 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.

⁵ The World Bank's (1996) four-group classification of 26 transition economies, for example, puts Hungary in the first group of leading reformers, Romania in the second group, Russia in the third, and Ukraine in the last. Similarly, the EBRD's annual indicators of "progress in transition" invariably place Hungary at or close to the top of all transition economies; according to the overall "institutional performance" measure in EBRD (2000), Hungary is ranked first, with a score of 3.5 overall, while Romania is awarded 2.3, Russia 1.9, and Ukraine 2.1.

average wage bill and percentage of firms privatized in our data. At this aggregate level of analysis, a strong negative correlation is evident in all four countries, which would seem to corroborate workers' fears and 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.⁶ 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.⁷

⁶ 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.

⁷ 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.

Our basic aim is to provide robust estimates of these effects using much larger and longer panels than were available to earlier researchers, but we also exploit the advantages of our data in several additional ways. First, we are able to distinguish between firms privatized to foreign investors and those privatized to domestic companies and individuals. Workers appear to fear foreign much more than domestic investors, but there is little evidence whether this perception is warranted. Second, we 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.”⁸ These models control not only for fixed differences among firms but also differing trend growth rates that may affect the probability of privatization and whether the new owners are domestic or foreign investors. We compare alternative estimators using several specification tests, including variants of the Heckman-Hotz (1989) “pre-program” test which measures selection bias under an estimator as the difference in the dependent variable prior to treatment between the treated and comparison groups. In the privatization context, this test must be evaluated before the privatization year to avoid possible contamination through anticipatory effects.

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

2. Data

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

The industrial registries are supposed to include all industrial firms with more than 100 employees plus those that are more than 25 percent owned by the state and/or by legal entities that are themselves included in the registry. In fact, the practice seems to be that once firms enter the registries, they continue to report even if the original conditions for inclusion are no

⁸ 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.

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.

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.

3. Privatization Policies and Their Implications

The methods and tempos of large enterprise privatization differed quite significantly across the four countries we study in this paper. Hungary got off to an early start in ownership transformation and maintained a consistent case-by-case method throughout the transition. At the very beginning, the transactions tended to be “spontaneous,” initiated by managers, who were also usually the beneficiaries, sometimes in combination with foreign or other investors (Voszka, 1993).

From 1991, the sales process became more regularized, generally relying upon competitive tenders open to foreign participation, although management usually still had control over the process. Unlike many other countries, there were no significant preferences given to workers to acquire shares in their companies, nor was there a mass distribution of shares aided by vouchers. Hungarian privatization thus resulted in very little 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.

Private ownership is defined here as a strict majority of shares held in private hands,

based on our regression samples.⁹ 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.¹⁰

The percentage of firms majority privatized to foreigners is by far the highest in Hungary, reaching nearly 16 percent of all entities by the end of our observation period.¹¹ 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.¹² 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.

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

⁹ 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).

¹⁰ 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.

¹¹ 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.

¹² 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).

firm's behavior: productivity improvement, cost reduction, and scale expansion. Worker-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). Estimating these effects nevertheless faces some potential problems. The most 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.¹³

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. Such anticipatory effects seem most likely to be negative, particularly if the expectation of post-privatization loss of control – or of job – leads to increased asset stripping by managers.¹⁴

As discussed in more detail below, we conduct specification tests of whether the inclusion of firm fixed effects or both firm fixed effects and firm-specific trends can help control for this selection bias. 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.

¹³ 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.

¹⁴ 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.

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

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 δ_p are negative in Hungary, positive in Russia and Ukraine, and essentially zero in Romania. Controlling for FEs and FTs changes the estimates dramatically: each significant coefficient drops close to zero, while the Romanian becomes large and significant in the FE but drops to -0.015 and loses significance in the FE&FT. The FE&FT coefficient is essentially zero in Hungary and Ukraine, and close to zero although negative in Russia. These results therefore imply that privatization has had little effect on the wage bill. If the wage bill represents a summary indicator of worker welfare, our firm-level analysis does not support the common belief that privatization hurts workers.¹⁵

Turning to the distinction between domestic and foreign ownership, the domestic results tend to be similar to the private results, as domestic owners dominate in most privatized companies. The OLS estimates of δ_d are negative in Hungary and Romania and positive in

¹⁵ 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.

Russia and Ukraine, but again the coefficients are reduced in magnitude when the FEs and FTs are included. The main exception is Romania, where as with δ_p , the FE estimate is positive and the FE&FT is essentially zero. In Hungary, the domestic wage bill effect is negative although small (about -0.05) and statistically insignificant in both the FE and FE&FT specifications. Foreign-owned firms account for only very small fractions of the observations in Russia and Ukraine, so the estimates of δ_d and δ_p are nearly identical.

By contrast, the estimated effects of foreign privatization 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. Again, while the OLS estimates are usually large in magnitude and highly significant, the coefficients tend to be much smaller and less significant in the FE and FE&FT specifications. 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.¹⁶ 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.

The results of the dynamic estimation are valuable for carrying out specification tests on pre-privatization behavior, variants of the Heckman-Hotz (1989) pre-program tests. We examine the results of F tests of the joint probability that the privatization effects one and two years before privatization are different from zero. The OLS specifications almost invariably produce large, highly significant F statistics. The sole exception is the foreign effect in the Hungarian

¹⁶ 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.

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 decompositions 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 positive scale effect, but it is not clear whether productivity improvements work to attenuate the net impact on employment. In all four countries, the decomposition of the employment impact in Figure 2 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.¹⁷ 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, Figure 3 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 $\delta^w = \delta^{ulc} + \delta^{lp}$. 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 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

¹⁷ We have also estimated a number of variants of total factor productivity, with similar results to those for labor productivity.

there are significant differences in observed rates across ownership types. The estimated δ_d and δ_f are always negative, and tiny everywhere except Hungary.¹⁸ 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.

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

¹⁸ 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).

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.

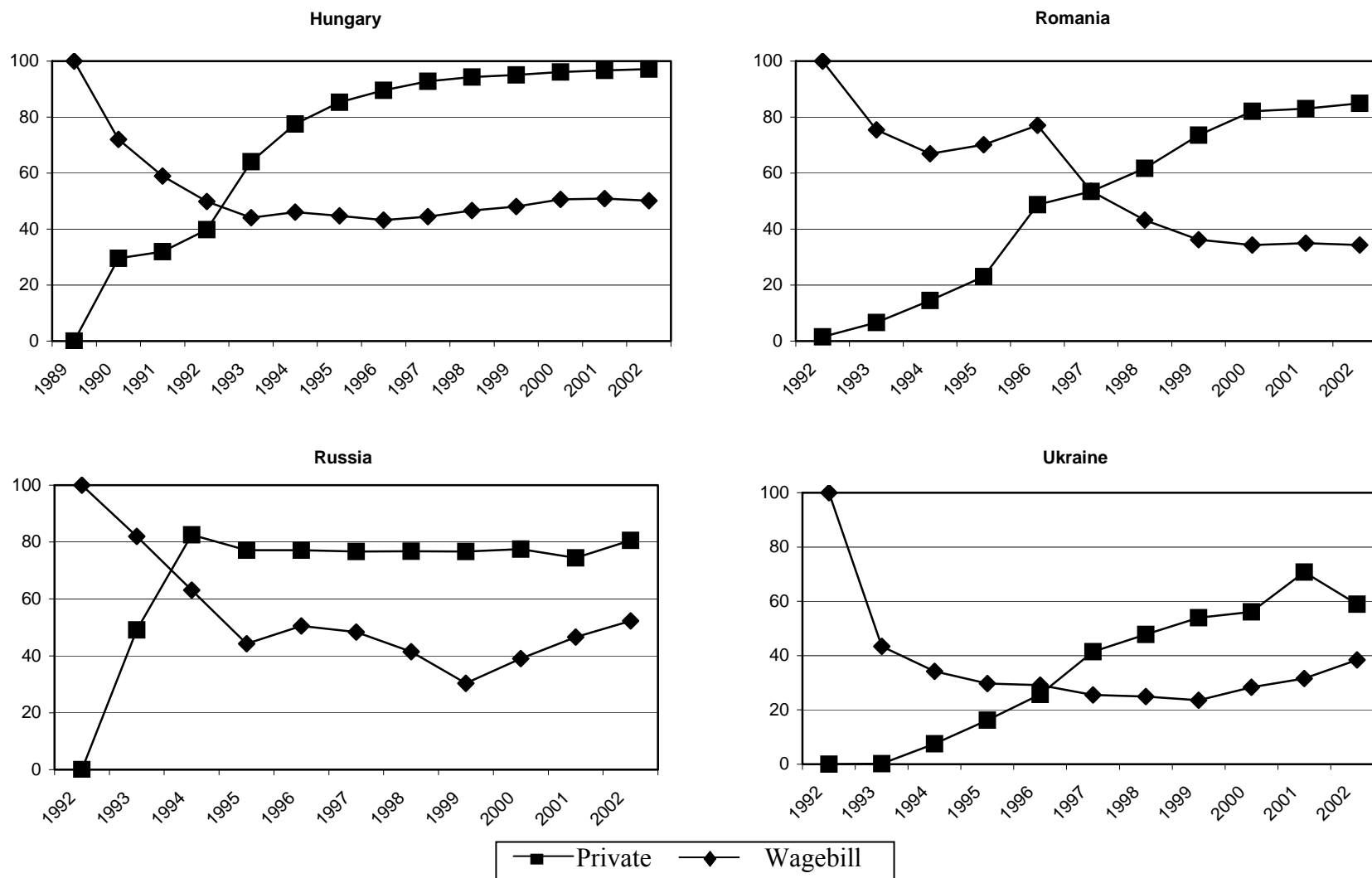
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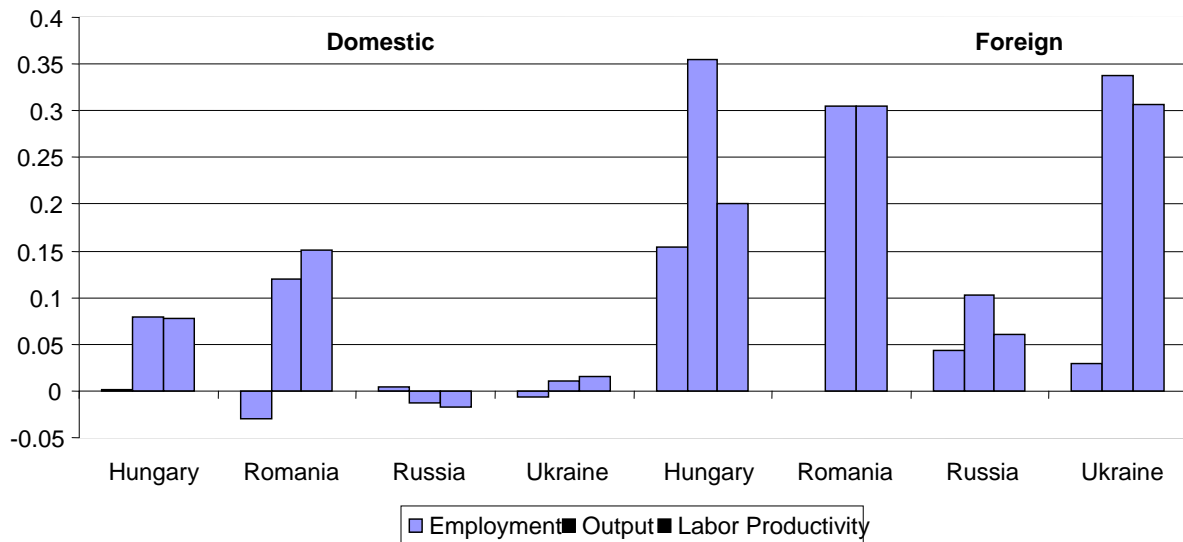
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Figure 1: Evolution of Average Real Wage Bill and Private Ownership



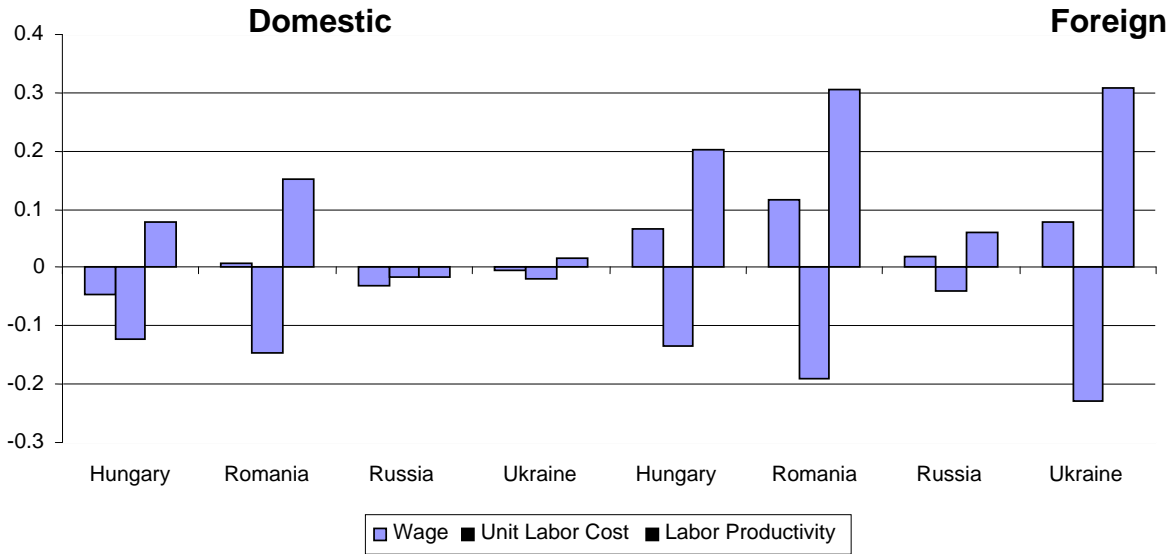
Notes: The graphs show the average real wage bill and percent of majority private firms, calculated from our data. The real wage bill is set at 100 in 1989 in Hungary and 1992 for Romania, Russia, and Ukraine.

Figure 2: 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 3: 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.