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Abstract

This paper estimates the effect of privatization on multifactor productivity (MFP) using long panel data for nearly the universe of initially state-owned manufacturing firms in four economies. We exploit the key longitudinal feature of our data to measure and control for pre-privatization selection bias and to estimate long-run impacts. We find that the magnitudes of our estimates are robust to alternative functional forms, but sensitive to how we control for selection. Our preferred random growth models imply that majority privatization raises MFP about 15% in Romania, 8% in Hungary, and 2% in Ukraine, while in Russia it lowers it 3%. Privatization to foreign rather than domestic investors has a larger impact, 18–35%, in all countries. Positive domestic effects appear within a year in Hungary, Romania, and Ukraine and continue growing thereafter, but take 5 years after privatization to emerge in Russia.

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1. Introduction

The privatization process in the East European transition provides a huge laboratory for studying the relationship of corporate ownership and performance. The usefulness of analyzing the results of this privatization experience derives not only from the large numbers of observations, many times greater than those in Western studies, but also from the much more indiscriminate process determining ownership.[1] Unlike the West, where state-owned enterprises usually operate in only a few sectors and tend to differ systematically from other firms, state ownership accounted for nearly all productive assets during the socialist period in Eastern Europe. The privatization policies adopted in the aftermath of the sudden collapse of Communist Party control have been almost equally indiscriminate, involving rapid transfers of large numbers of companies in just a few years, by contrast to the careful selection and long preparation of firms for privatization typical in the West. At the same time, most East European countries have retained significant numbers of firms in state hands, thus providing a useful comparison group for estimating the impact of ownership change.

The attractiveness of this research setting, however, has been little exploited by studies with corresponding research designs. Notwithstanding the large number of privatized firms in many East European countries and what by now has become a fairly long time period for analysis, data limitations have prevented most research from including more than a few hundred firms, and few studies have more than three or four annual observations on each firm.[2] Constrained by only one or two years of both pre- and post-privatization information, researchers face difficulties reliably identifying a privatization effect and judging pre-privatization differences that might reflect selection bias in the privatization process. Some study data only on privatized firms, thus failing to exploit the possibility of a state enterprise comparison group.

This paper analyzes the productivity effects of privatization using much longer time series and more comprehensive coverage than in earlier research. We have assembled annual information on manufacturing firms from as early as 1985, when the Communist Party still held power throughout Eastern Europe, until 2002, well after most firms had been privatized. The

[1] Megginson and Netter (2001) provide a comprehensive review of Western research on privatization and discuss some early studies of the results in transition economies.

[2] For example, Frydman et al. (1999) study survey data on about 200 privatized Czech, Hungarian, and Polish firms from 1990–1993, and Earle and Estrin (1997) study survey data on about 380 Russian firms, both state-owned and privatized. Djankov and Murrell (2002) provide a review of previous studies of the effects of large enterprise privatization in transition economies.

data cover four important transition economies—Hungary, Romania, Russia, and Ukraine – and they include most manufacturing firms inherited from the former planned economy, both those slated for privatization and those remaining under state ownership. In all four countries, comparable financial information enables us to estimate multifactor productivity for each firm on an annual basis, and the ownership data permit a distinction not only between privatized and state-owned firms but also between firms privatized to foreign investors and those privatized to domestic companies and individuals; they also allow us to infer the precise year in which ownership change occurred. Absent a genuinely randomized experiment, these panel data provide a nearly ideal setting for investigating the relationship between ownership and productivity.

Our basic aim in this paper is to estimate this relationship using much larger and longer panels than were previously available. Unlike some previous research that examined other outcome variables, we focus on productivity, a variable that is more convincing as a performance measure than qualitative measures of restructuring and more closely linked to economic welfare than sales, profit, or employment. When productivity has been considered in the past, attention is frequently limited to either labor productivity or a single specification of multifactor productivity, and analysis of the latter is often conditioned on auxiliary assumptions such as constant returns to scale, no factor bias associated with ownership, no unobserved firm heterogeneity correlated with productivity and ownership, and a common technology across diverse industries. Clearly, the value of the estimates is reduced when such assumptions are imposed. In this paper, we take into account differences in production technology across industries and investigate the robustness of the ownership effects on productivity across a variety of measurement specifications.

The most significant innovation in our approach, however, is to apply methods developed for dealing with selection bias in labor market program evaluations. We exploit the long time series in our firm-level data to estimate regression models including not only firm fixed effects but also firm-specific time trends, sometimes referred to as "random growth models."[3] Applied to the privatization context, these models control not only for fixed differences among firms but also differing trend productivity growth rates that may affect the probability of privatization and

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

whether the new owners are domestic or foreign investors. Moreover, the availability of several years of pre-privatization data is useful for taking into account possible biases in the selection of firms to be privatized. We analyze these data using specification tests we have also borrowed from program evaluation research, including the Heckman-Hotz (1989) "pre-program" test which measures selection bias under an estimator as the difference in the dependent variable prior to treatment between the treated and comparison groups. In the privatization context, this test must be evaluated significantly prior to privatization because of possible contamination through anticipatory effects.

We also analyze the dynamics of firm performance in the immediate pre-privatization period—to measure possible anticipatory effects—as well as for several years after. Once it has been decided and announced that a firm will be privatized, the firm's performance could either improve as managerial incentives are increased by the expected benefits under new owners, or it could be diminished as managers see little future with the firm and resort to asset-stripping (Aghion, Blanchard, and Burgess, 1994; Roland and Sekkat, 2000). Either type of behavior would result in a biased estimate of the privatization effect in a simple comparison of pre- and post-privatization performance. Estimates of post-privatization dynamics are also interesting as they shed light on how quickly any benefits from privatization are realized and whether they are sustained or tend to diminish over time.

Finally, we investigate the effects of different types of new private ownership structures: domestic versus foreign. While most previous studies tend to find an overall positive impact of privatization on performance, the level of confidence in the results disaggregated by owner-type is further reduced by small sample size problems. Our data, however, contain substantial numbers of observations with both foreign and domestic ownership, although they do not have consistent measures of other ownership classifications. The data permit us to test some common hypotheses about the relative advantages of each of these types of ownership in raising firm performance and about the selection bias in each type of privatization.

The rest of the paper is organized as follows. Section 2 describes our data and Section 3 discusses the privatization programs in the four countries we study. Section 4 describes the estimation procedures, and Section 5 presents results. Conclusions are summarized in Section 6.

2. Data

Our analysis draws upon annual census-type data available for manufacturing firms in each of the four countries we study. Although the sources and variables are somewhat similar across countries, considerable effort has been necessary to prepare and clean the data, to construct longitudinal links, and to render them sufficiently comparable to justify cross-country comparisons.

The countries with the most conceptually similar data are Russia and Ukraine, where common statistical methodologies and data collection mechanisms were inherited from the Soviet Union. The national statistical offices (*Goskomstat* in Russia and *Derzhkomstat* in Ukraine) are the successors to the branches of the former Soviet State Committee. The basic sources in these countries are industrial enterprise registries, supplemented by joint venture registries in Russia, databases from the State Property Committee and the State Securities Commission in Ukraine, and balance sheet data in both countries. 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) inherited from the Soviet system. Certainly with respect to this set of firms, the databases are quite comprehensive. At the beginning of the transition process in 1992, the firms in the Russian industrial registry accounted for 91 percent and in Ukraine for 94 percent of officially reported total industrial employment. The Russian data are available for every year from 1985 to 2002, and for Ukraine they are available for 1989 and each year from 1992 to 2002.

The Hungarian and Romanian data tend to be more similar to each other than to those in the Soviet successor states. In both cases, the basic data source is balance sheets and income statements associated with tax reporting: to the National Tax Authority in Hungary and to the Ministry of Finance in Romania. These data are available for all legal entities engaged in double-sided bookkeeping, except in Hungary before 1992—when only a sample consisting of most firms with at least 20 employees and some smaller firms is available.[4] In addition, the Romanian data are supplemented by the National Institute for Statistics' enterprise registry and

[4] Despite this sampling, the coverage before 1992 in Hungary is still high: total employment in the sample in 1991 is 72 percent of employment in the 1992 sample.

the State Ownership Fund's portfolio and transactions data. The Romanian data contain 95 percent of reported total manufacturing employment in 1992, and the Hungarian figure, where entry of new private firms started earlier, is 85 percent. The Hungarian data are annual from 1986 to 2002, and the Romanian cover 1992 to 2002.

In order to make the samples comparable across countries, some truncation 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 also to exclude industrial firms that were previously non-industrial). 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*). The recycling industry (NACE 37) is also excluded because of noncomparability with *OKONKh*. We include only "old" firms, defined as existing prior to 1992 (1990 in Hungary) or having any state ownership at first observation, both because the Russian and Ukrainian data do not cover de novo firms and because, even if we could measure them, de novo firms are not at risk of privatization. Non-profit organizations in all four countries are excluded, as are firms subordinated to the State Committee for the Defense Industry after 1998 in Russia. Finally, we retain firm-years in the sample only when they contain complete information (nonmissing values for ownership, employment, output, and capital).

The total numbers of firms and their total employment in 1994, plus the fractions of these in 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 total number of firms ever in the sample is 30,647 and the average number of annual observations per firm is just over 10, making 310,888 firm-years available for analysis.

Summary statistics and definitions for the basic variables used to estimate productivity—output, capital, and employment —are provided for selected years in Table 2. Data on material costs are unfortunately not available for all countries and years in the data; our specification of production technologies therefore assumes the only inputs are capital and labor. Reflecting aggregate statistics, the data imply declining average employment size in all four economies (although most in Romania), while mean output has fallen through most of the period everywhere but in Hungary (and Russia and Ukraine since 1999). Capital stock has also tended to fall in most years, the main exceptions being recent rises in Russia and Ukraine. This last

result is somewhat puzzling, but it may reflect imperfect deflators that fail to distinguish true price and quantity changes. Our econometric analysis handles this problem by controlling for a full set of industry-year interactions.

These data have been extensively cleaned to remove inconsistencies and to improve missing longitudinal linkages due to change of firm identifier from one year to the next (associated with reorganizations and changes of legal form, for instance). The inconsistencies were evaluated using information from multiple sources (including not only separate data providers, but also previous year information available in Romanian balance sheets and Russian and Ukrainian registries). The longitudinal linkages were improved using all available information, including industry, region, size, multiple sources for the same financial variables, and some exact linking variables (e.g., firm names and addresses in all countries except Hungary, where this information was not available) to match firms that exited the data in a given year with those that entered in the following year. In Hungary, we also used a database with direct information on longitudinal linkages: if a firm changed its identification number for some reason (and it appeared in the data as a new entry or an exit), the database indicated whether it had a predecessor or successor and, if so, that firm's identification number. 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. For example, if firms that change their legal form are systematically different—engaging in greater restructuring, for example—then it is critical that they not be excluded from the analysis.

Turning to our ownership variables, we have drawn not only on the sources for output, capital, and labor, but also on additional information that we have linked across databases. For the purposes of this paper (and for cross-country comparability), we define a firm as privately owned if a strict majority of shares is held in private hands. Both the Hungarian and Romanian data contain information on state, domestic private, and foreign private shareholdings, the former since 1990 and the latter since 1992. The Romanian balance sheet and registry data are supplemented by detailed ownership data from the state privatization authority (originally FPS, now APAPS). The Russian data do not contain an ownership variable prior to 1993, nor, unlike the other countries, do they distinguish between minority and majority shares, but virtually all the privatizations in our data are mass privatizations (not lease buyouts), so the earliest they could have taken place was October 1992, and other sources suggest that nearly all of these led

to majority private ownership (e.g., Boycko, Shleifer, and Vishny, 1995; Earle and Estrin, 1997).[5] The Ukrainian private ownership codes are constructed from State Property Committee private share information, while the foreign codes in Ukraine are constructed from a State Stock Market and Securities Commission database on shareholdings and trading dates for shareholders with at least 10% stakes in joint stock companies.

The timing of the ownership variables has a bearing on the interpretation of the estimation results. Ownership is measured as of the reporting date, the end of the calendar year (except in Ukraine where it is January 1—we treat this as identical to the end of the previous year). The privatization year is thus defined as the year in which the ownership variable changes from state at the end of t-1 to private in t.[6] The actual transfer of shares could take place anytime during this year, and the transfer of effective control is even more ambiguous: for instance, it could take place even before the shares are legally conveyed, if it is quite clear who the new owners will be, and it could take place significantly afterwards, if for example it takes time to call a general shareholders' meeting to replace the board and management. These ambiguities imply that the analysis should not be confined to comparisons of the immediate period just before and after the privatization year but instead take a longer perspective on both pre- and post-privatization performance. Our data contain substantial numbers of firms with several observations before and after privatization, facilitating such an analysis.[7]

3. Privatization Policies

The tempos and methods of large enterprise privatization differ across the four countries we study in this paper. The differences in tempos are reflected in Table 4, which contains our computations of share of majority privatized firms, based on the manufacturing firms in our database. As of end-1992, 36.1 percent of the Hungarian firms had already been privatized,

[5] 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 we know are majority foreign in at least one of the four years.

[6] We assume a single change of ownership and recoded cases of multiple switches to the modal category after the first change (ties were decided in favor of private and foreign, unless only two years of data were available). This procedure resulted in 79 recodings in Hungary, 15 in Romania, and 4 in Ukraine. Russia had 2811 firms private since 1995 reclassified as state in 2000 or 2001, when ownership codes changed drastically; such mass renationalization did not occur, so our recoding corrects this problem.

[7] In Hungary there are 5822 pre-privatization firm-year observations with a median of 4 annual pre-privatization observations per firm, and 11,466 post-privatization observations with a median of 8 post observations per firm. The corresponding numbers for Romania are 10,830 pre-privatization observations (median of 5 per firm) and 7641 post (median=4); for Russia, they are 94,620 pre (median=9) and 63,450 post (median=4); and for Ukraine, they are 28,749 pre (median=6) and 15,929 post (median=4). More details are available on request.

while the percentage was only 0.2 in Romania and 0.0 in Russia and Ukraine. By the end of the period, however, most firms had been privatized in all four countries.[8]

The table also contains the percentage of firms majority privatized to foreigners. This fraction is by far the highest in Hungary, reaching 16 percent of all entities by the end of our observation period. In Romania, the percentage reaches over 5 percent, and in Russia and Ukraine about 1 percent, which given our sample sizes are sufficient to estimate coefficients. The residual category—the difference between private and foreign—consists of majority privatized firms that are not majority foreign. Because foreign investment in these countries usually takes the form of controlling investments, the residual firms are therefore usually majority owned by domestic private groups, and hence we label them "domestic" in the discussion below. But some cases of minority foreign investment (particularly in Hungary) are also included in this category.

Not only the pace but also the method of privatization differs across these countries. Hungary got off to an early start in ownership transformation and maintained a consistent case-by-case sales 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. Unlike many other countries, there were few preferences given to workers to acquire shares in their companies (except for about 350 management-employee buyouts), nor was there a mass distribution of shares aided by vouchers. Hungarian privatization thus resulted in very little worker ownership, very little dispersed ownership, and instead concentrated blockholdings, with a large foreign share (see, e.g., Frydman et al., 1993a; Earle et al., 2005). Although the process appeared to be gradual, in fact it was accomplished more quickly than in most other East European countries.

In Romania, by contrast, the early attempts to mimic voucher programs and to sell individual firms produced few results, and privatization really began only in late 1993, first with the program of Management and Employee Buyouts, and secondly with the mass privatization of 1995-96 (Earle and Sapatoru, 1993 and 1994; Earle and Telegdy, 1998). The consequences of these programs were large-scale employee ownership and dispersed shareholding by the general

[8] The nonmonotonicity of percent privatized in Table 4 is due not to renationalization but to private firms exiting the data and split-ups of state-owned firms, which are subject to later privatization and thus included in our sample.

population, with little foreign involvement. Beginning in 1997, greater efforts were made to involve foreign investors, and blocks of shares were sold both to foreigners and domestic entities. The result was a mixture of several types of ownership and a moderate speed compared to neighboring countries (Earle and Telegdy, 2002).

Russia and Ukraine's earliest privatization experiences have some similarities to the "spontaneous" period in Hungary, as the central planning system dissolved at the end of the 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 the bulk of shares were transferred primarily to the concerned firms' managers and workers, who had received large discounts in the implicit prices they faced (Boycko, Shleifer, and Vishny, 1995; Earle and Estrin, 1997). Some shares (generally 29 percent) were reserved for voucher auctions open to any participant, and these resulted in a variety of ownership structures, from dispersed outsiders holding their shares through voucher investment funds to domestic investors who acquired significant blocks; sometimes managers and workers acquired more shares through this means, but there were few cases of foreign investment. Blockholding and foreign ownership became more significant through later sales of blocks of shares and through secondary trading that resulted in concentration. Ukraine followed Russia's pattern at a somewhat slower pace, and the initial design provided even greater advantages to insiders acquiring shares in their companies (Frydman et al., 1993b). In both countries, the initial consequence was large-scale ownership by managers and workers, some blockholding by domestic entities, and continued state ownership. Subsequently, blocks formed and foreigners made partial inroads.

The differences in the method of privatization method may affect the impact of the policy on firm productivity. Case-by-case sales of large blocks of shares is usually considered the most effective method, but its advantages may be reduced by contractual obligations on future investment and employment that inhibit restructuring. The productivity effect of new foreign owners seems likely to be higher than that of domestic investors, as a result of better management skills and access to finance and new technologies, but foreigners may face worse obstacles when layoff decisions are highly politicized and when local networks and knowledge of local conditions are nontransparent. Transfers to employees and mass privatization have been politically attractive, and have been common in most East European economies, but each faces

disadvantages. Employees may lack the necessary skills, capital, access to markets, and technologies necessary to turn their firms around, and corporate governance by employees may function particularly poorly when the firm requires difficult restructuring choices involving disparate distributional impacts within the firm.[9] Mass and voucher privatization programs were intended to increase the speed of privatization by overcoming the problems of insufficient demand due to low domestic savings and reluctance of foreign investors, and if possible to jump-start domestic equity markets with a rapid release of shares. But they have sometimes been combined with strong preferences for employees to use their vouchers in acquiring shares in their employer (for instance, in Russia and Ukraine), and they typically create highly dispersed ownership structures, resulting in unmonitored managerial control and—according to some—unfettered asset-stripping.[10]

The effects of different privatization methods may also manifest themselves differently over time. For example, if concentrated private ownership is necessary to achieve restructuring, then one would expect to see more immediate effects from sales to concentrated outsiders than from voucher or insider privatization, where it takes time for concentrated blocks to form. In this case, the major differences across countries could arise in the timing of the potential benefits from privatization. A possible hypothesis would be that the speed of the impact of privatization is increasing in the fraction of sales in all privatization transactions. The subsequent dynamics of the privatization effect may reflect secondary trading leading to increased concentration, however, and countries with high initial levels of inside and dispersed outside ownership may catch up so that the final impact after several years is not very different across countries. We will examine these possibilities empirically below.

4. Empirical Strategy

We follow the broader literature on the effects of privatization in estimating reduced form equations for firm performance, while trying to account for potential problems of unobserved heterogeneity and simultaneity bias (Djankov and Murrell, 2002; Megginson and Netter, 2001). Our goal is to assess the robustness of the estimated privatization effects to alternative

[9] Frydman and Rapaczynski (1994) and Lipton and Sachs (1990), for instance, argue against privatization to employees, while Ellerman (1993), Stiglitz (1999) and Weitzman (1993) argue in favor. Earle and Estrin (1996) discuss the advantages and disadvantages of worker and manager ownership in the transition setting.

[10] See, e.g., Stiglitz (1999); Black, Kraakman, and Tarassova (2000); Kornai (2000); and Roland (2001). Proponents of such programs include Lipton and Sachs (1990), Blanchard et al. (1993), Frydman and Rapaczynski (1994), and Boycko, Shleifer, and Vishny (1994, 1995).

econometric methods, and therefore our approach to measuring productivity is eclectic. In all cases, we estimate separate equations for each country both to permit functional forms to vary across countries and to investigate differences in the estimated privatization effect. We also permit the technology parameters to vary across industries, and we include a full set of industry-year effects to control for time-varying industry characteristics and shocks that may be correlated with both ownership and productivity.

Our approach to handling potential selection bias is to use the longitudinal structure of the data, including the multiple pre-privatization observations available for most firms, to estimate alternative models based on different identifying assumptions. The simplest model assumes no selection into ownership type (private versus state in one specification, domestic private versus foreign versus state in the other) based on either the level or growth of productivity. This model is estimated by applying ordinary least squares (OLS) to the production function augmented with ownership variables. The second model assumes no selection based on productivity growth, adding firm fixed effects (FE) to the equation. The third model permits each firm to have its own intrinsic growth rate by adding not only FE but also firm-specific time trends (FT); the identifying assumption is that the data-generating process for ownership is independent of productivity once all the other variables, including the FE and FT, are taken into account. We evaluate these alternative models based on standard specification tests as well as the degree to which they generate similar pre-privatization productivity behavior of firms of different ownership types.

The equations we estimate for each country may be written in the following general form:

$$y_{it} = \mathbf{f_j}(k_{it}, l_{it}) + \mathbf{D_{jt}}\boldsymbol{\gamma}_{\mathbf{jt}} + \mathbf{w_t}\boldsymbol{\alpha}_{\mathbf{i}} + \boldsymbol{\theta}_{\mathbf{it}}\boldsymbol{\delta} + u_{it}, \quad (1)$$

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. y_{it} is ln(output), $\mathbf{f_j}$ is a 1 x J vector of industry-specific production functions, k_{it} is ln(capital stock), l_{it} is ln(employment), $\mathbf{D_{jt}}$ is a 1 x JT vector of industry-year interaction dummies, $\boldsymbol{\gamma}_{\mathbf{jt}}$ is the associated JT x 1 vector of coefficients, and u_{it} is an idiosyncratic error.[11] The specifications of the other terms in the equation vary across specifications: $\mathbf{w_t}$ is a vector of aggregate time variables, $\boldsymbol{\alpha}_{\mathbf{i}}$ is the vector of associated individual-specific slopes, $\boldsymbol{\theta}_{\mathbf{it}}$ is the vector

[11] Our analysis of serial correlation in the residuals implies that the process is not a simple AR(1), and the lagged residuals are frequently significant (with varying signs) up to 4 lags, the patterns differing across countries. Our estimates therefore permit general within-firm correlation of residuals using the clustering method of Arellano (1987), and the standard errors and all of our test statistics are robust to both serial correlation and heteroskedasticity. Kézdi (2003) contains a detailed analysis of autocorrelation and the robust cluster estimator in panel data models.

of ownership measures, and $\boldsymbol{\delta}$ are the ownership effects of interest in this paper. We provide further explanation of each element in turn.

Starting with the functional form of $\mathbf{f_j}$, we focus our presentation of results in the text and tables on the simplest Cobb-Douglas specification. But we have also permitted to vary across a wide variety of other specifications, including translog, Cobb-Douglas imposing the restriction of constant-returns to scale, and labor productivity. We also investigate the implications of assuming arbitrary Cobb-Douglas production structures, such as assigning 0.3 as the contribution for capital and 0.7 for labor. These results are very similar to those for the estimated unrestricted Cobb-Douglas, and we report them in the Appendix. In all cases of estimated functions, we permit the function to vary across industries, and we always include a full set of unrestricted industry-year interactions, the $\mathbf{D_{jt}}$.[12] This specification thus permits different productivity levels for each industry in each year, controlling for any time- and industry-varying factors, such as price changes not captured by deflators, unmeasured factors of production, and quality differences that are time-industry-specific.

Our methods of controlling for selection bias are embodied in the specification of $\mathbf{w_t}$.[13] The OLS model assumes $\mathbf{w_t} \equiv 0$. The FE model specifies $\mathbf{w_t} \equiv 1$, so that $\boldsymbol{\alpha_i} \equiv \alpha_i$ is the unobserved effect. Any fixed differences in productivity across firms are removed in the FE model. The FE&FT model with firm-specific trends specifies $\mathbf{w_t} \equiv (1, t)$, so that $\boldsymbol{\alpha_i} \equiv (\alpha_{1i}, \alpha_{2i})$, where α_{1i} is a fixed unobserved effect and α_{2i} is the random trend for firm i. In practice, the FE&FT model is estimated in two steps, the first detrending all variables for each firm separately and the second estimating the model on the detrended data. Standard errors in the second step are adjusted for the loss of degrees of freedom associated with detrending.

We investigate several 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.[14] The coefficient of interest δ is then the mean

[12] J=10 industries, which we have chosen based on the trade-off between disaggregation and number of observations, specifying a minimum of 50 observations per year per country for each industry. T varies by country: 17 in Hungary, 11 in Romania, 12 in Ukraine, and 18 in Russia.

[13] Our exposition of these aggregate time variables follows Wooldridge (2005).

[14] As we noted when describing the data in Section 2, our data do not specify an exact privatization date, and we infer privatization by observing a change in status from the end of one year to the next. This implies that the date on which the new private owners acquire formal authority (e.g., the first post-privatization shareholders' meeting) varies across firms, with some of them early in the final pre-privatization year. But some such assumption is necessary about which should be defined as the first "post" year, and we discuss this issue further in connection with the dynamics of the effect below.

within-country-industry-year difference in MFP between firms majority private and majority state-owned. A second specification of ownership distinguishes 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.

Using another specification of ownership we undertake an analysis of the dynamics of the privatization effect before and after privatization takes place. Our motivation is threefold: first, estimating pre-privatization dynamics provides information on whether firms were already improving productivity prior to the ownership change. Such behavior could be the result of some dynamic selection bias that the model does not account for, and we use the estimated effects of privatization in the period of 4 to 2 years before the privatization year to evaluate the magnitude of selection bias. Second, estimating dynamics just before the privatization year permits an assessment of changes in incentives in anticipation of privatization; such anticipatory effects could be positive if they reflect career concerns of managers hoping either to show new owners their skills or to acquire their companies themselves, or they could be negative if the expectation of post-privatization loss of control—or of job—leads to increased asset-stripping (Aghion, Blanchard, and Burgess, 1994; Roland and Sekkat, 2000). Privatization may be such a disruptive process that any firm suffers a short-run decline in productivity.[15] Finally, examining post-privatization dynamics is useful for ascertaining the speed with which any estimated effect occurs: is the effect immediate or gradual, becoming significant only with a long lag? Does it tend to be a single jump in productivity, or is it more sustained, with a series of increases over several years? Is it only temporary, as state firms tend to catch up, or does the effect appear to be permanent?

We implement the dynamic specification by interacting dummy variables for the years before and after privatization with $Domestic_{it}$ and $Foreign_{it}$. Designating τ as the index of event time, the number of years since privatization, so that $\tau < 0$ in the pre-privatization years, $\tau = 0$ in the year in which ownership change occurs, and $\tau > 0$ in the post-privatization years, then $\boldsymbol{\theta}_{it} \equiv (\mathbf{Domestic}_{it\tau}, \mathbf{Foreign}_{it\tau})$ and $\boldsymbol{\delta} \equiv (\boldsymbol{\delta}_{\tau d}, \boldsymbol{\delta}_{\tau f})$. We assume that privatization has no effect until 5 years before the ownership change appears in our data, so that $\delta_{\tau d} = \delta_{\tau f} = 0$ for $\tau \leq -5$. We permit the effects to vary by year through $\tau = 7$ for domestic and $\tau = 5$ for foreign, based on the small sample sizes available for estimating separate effects for firms privatized many years before;

[15] The dynamics just before the privatization year may show effects analogous to "Ashenfelter's dip" in training evaluations – where workers about to enroll in a training program experience a drop in earnings.

subsequent years are pooled together.[16] Expressed in our notation, therefore, we estimate the vector of parameters $\boldsymbol{\delta}_{\tau d}$ corresponding to the vector of variables $\mathbf{Domestic}_{it\tau}$ for τ = -4, -3,…0,…7+, where 7+ represents event-time at least 7 years after privatization to domestic investors. Similarly, we estimate $\boldsymbol{\delta}_{\tau f}$ corresponding to $\mathbf{Foreign}_{it\tau}$ for τ = -4, -3,…0,…5+, where 5+ represents event-time at least 5 years after privatization to foreign investors.

We implement specification tests based on the dynamic estimation results for the pre-privatization period. Our method generalizes the Heckman-Hotz (1989) "pre-program" test, which requires the same conditional expectation of the outcome for both treated and control groups in a single pre-treatment period. The assumption is that, once the test is satisfied, the only cause of differences between the two groups after that period is the treatment itself. As Heckman, LaLonde, and Smith (1999) point out, 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, what they call the "fallacy of alignment" (p. 2025ff).[17] Our method, however, is to analyze all the available data prior to privatization, and we examine the magnitude and statistical significance of each pre-privatization effect, where $\tau < 0$. We focus on τ =-4, -3, and -2 because, as we have argued, τ = -1 may display some anticipatory effects (positive or negative). We also report F-tests on each of the following joint hypotheses: $\delta_{-4d} = \delta_{-3d} = \delta_{-2d} = 0$, and $\delta_{-4f} = \delta_{-3f} = \delta_{-2f} = 0$.

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

$$y_{it} = \mathbf{D_{jt}}\boldsymbol{\gamma}_{\mathbf{jt}} + \boldsymbol{\theta}_{\mathbf{it}}\boldsymbol{\delta} + u_{it}. \qquad (2)$$

Pre-privatization differences in MFP (productivity level) are estimated from

[16] This specification was also superior in terms of the pre-program test discussed below.

[17] In support of their argument, HLS cite the example of a training program in which earnings of trainees drop in τ = -2 and recover partially in τ = -1, and they criticize studies that choose one or the other of these years as the basis for comparisons. Studying the whole time series avoids this pitfall and does not require any a priori assumptions on which year is most appropriate.

$$y_{it} = \mathbf{f_j}(k_{it}, l_{it}) + \mathbf{D_{jt}\gamma_{jt}} + \boldsymbol{\theta}_{it}\boldsymbol{\delta} + u_{it}, \quad (3)$$

and pre-privatization differences in MFP trend (productivity growth) from $\boldsymbol{\beta}$ in an equation with a linear trend interacted

$$y_{it} = \mathbf{f_j}(k_{it}, l_{it}) + \mathbf{D_{jt}\gamma_{jt}} + \boldsymbol{\theta}_{it}\boldsymbol{\delta} + \tau\boldsymbol{\theta}_{it}\boldsymbol{\beta} + u_{it}, \quad (4)$$

where τ is defined as before, which with the sample truncation above $\tau = 0$ implies a linear pre-privatization trend normalized at zero in the privatization year. The results are reported in the next section.

5. Results

Table 4 contains the estimates of equations (2) – (4) for both the specification with $\boldsymbol{\theta}_{it} \equiv$ *Pre-Private*$_{it}$, and for the specification with $\boldsymbol{\theta}_{it} \equiv$ (*Pre-Domestic*$_{it}$, *Pre-Foreign*$_{it}$). Every variable exhibits large, statistically significant pre-privatization differences in some countries, but the magnitudes and even the direction of the estimated differences vary widely. For output, for example, the average *Pre-Private*$_{it}$ difference is very large in Russia, implying that privatized firms are twice the size of those remaining state-owned. But in Ukraine they are only about 25 percent larger, in Romania the difference is negligible, and in Hungary they are about 32 percent smaller. Similar inconsistencies in relative pre-privatization characteristics appear for MFP, which is estimated to be 19 percent larger in pre-privatized firms in Russia relative to the always state-owned, but the difference is only about 9 percent in Ukraine and 7 percent in Romania, and it is estimated to be slightly negative (although not statistically significant) in Hungary. The MFP trend difference is a positive 1 percent in Romania, a negative 2 percent in Russia, and tiny and statistically insignificant in Hungary and Ukraine.

A more systematic pattern appears in the specification distinguishing *Pre-Domestic* and *Pre-Foreign*. In all four countries, firms that were eventually acquired by foreigner investors are larger and more productive than those either remaining state-owned or acquired by domestic investors. The coefficients on these variables are quite similar across countries, ranging from 120 to 200 percent larger in size and from 16 to 36 percent more productive. In the productivity trend equation, the estimated coefficients on *Pre-Foreign* are negative in all four countries, although the result is statistically insignificant in Romania and only marginally significant in Hungary. The *Pre-Domestic* coefficients vary much more widely across countries.

Taken together, these findings imply that, despite the apparently indiscriminate privatization policies of these countries, significant selection bias in the privatization process

may exist. Moreover, they suggest that the selection processes may be quite different for firms acquired by foreign compared with domestic investors. Across countries, the foreign selection process looks much more similar than the domestic selection process, which is probably a reflection of the greater diversity of methods used for domestic relative to foreign acquisitions: domestic privatization could be accomplished by mass and insider methods, while foreign privatizations were mostly sales. It is notable, however, that the pre-privatization characteristics of firms domestically privatized in Hungary look quite different from those sold to foreigners, even though domestic privatizations in Hungary nearly always involved sales.

With these pre-privatization patterns as background, we turn to the basic results from estimation of the average privatization effect δ for each country in Table 5. We display regression estimates of δ in the three alternative specifications of the $\mathbf{w_t}$, each of them unweighted and weighted (by employment shares).[18] Both types of OLS estimates imply large positive effects of privatization on productivity, with the largest effect in Romania followed by Hungary, Ukraine, and Russia. Weighting the regression produces a similar order of coefficients. Introducing firm fixed effects (the FE specification) reduces all the coefficients by about 0.2, with the Russian coefficients becoming negative. In all the other countries, adding firm-specific trends (the FE&FT specification) attenuates the coefficients still further. Yet the cross-country order of the estimated magnitude of the effects remains the same as in the OLS results. In Romania, the smaller coefficient in the weighted compared to the unweighted regression implies that privatization tended to increase productivity more in smaller firms. The opposite is true for Ukraine, while in Hungary and Russia the differences between weighted and unweighted regressions are small and inconsistent in sign.

Before considering the question of specification choice and discussing other functional forms, we report our basic estimates of the domestic effect δ_d and the foreign effect δ_f in Table 6. In all specifications and countries, we estimate the foreign privatization effect δ_f to be large and, with a single exception, highly significant.[19] Within specifications, the magnitudes are very similar across countries, averaging about 0.6 in the OLS, 0.4 in the FE, and 0.25 in the FE&FT. Not only is this an important result in its own right, but our finding of consistently high foreign effects has implications for some potential explanations of cross-country differences in the

[18] Full sets of regression coefficients, including the implied marginal products, are available from the authors in an unpublished appendix.

[19] The single insignificant result for δ_f could result from a small sample size (68 foreign firms in Russia) coupled with a demanding specification (FE&FT).

average privatization effect. In particular, it casts doubt on accounts that stress the importance of the macroeconomic and business environments, as foreign-owned firms may be equally vulnerable to volatility, corruption, and excessive regulation.[20] It also implies that the cross-country differences in the estimated privatization effects are unlikely to be due to any differences across countries in types of data or measurement methods.

The uniformly high estimate of the foreign privatization effect implies that the effects of domestic privatization vary widely, as the results for δ_d confirm. We find positive and statistically significant domestic effects for Hungary and Romania in all three specifications, but the δ_ds in Hungary fall more relative to the overall privatization effects in Table 5. As a consequence, the difference between the estimated effects in Hungary and Romania is slightly greater than before. Because of the very small foreign shares in Russia and Ukraine, the domestic effects have only marginally smaller estimates than the average privatization effects we have already seen.[21]

These results are highly robust to the choice of functional form for the production function, and we provide the results from some alternative choices in the Appendix. As shown in Table A1, for example, the FE&FT specifications produce average privatization effects of domestic effects (standard errors) of 0.075(0.026) in Hungary, 0.136(0.021) in Romania, -0.018(0.014) in Russia, and 0.019(0.019) in Ukraine. Table A2 shows comparable results and calculations of average estimated effects and standard errors for the domestic versus foreign distinction, which are again highly robust to the specification of technology.[22] Furthermore, the cross-country differences reflected in these estimates are highly statistically significant and robust to changes in functional form.[23]

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[20] If foreign firms were concentrated in few regions with unusual characteristics (such as a superior business environment), then this inference could be incorrect, but in fact the foreign firms in our data are quite dispersed. The highest percentage of a country's foreign firms in any one region is in Hungary (30.5 percent in Budapest), while no region has more than 13 percent in the other countries.

[21] In Ukraine, the domestic effects are sometimes slightly larger, due to the different sample available for the disaggregated ownership analysis.

[22] The ownership coefficients could be biased if the exit is correlated with both ownership and productivity. Brown et al. (2005) report exit regressions using samples similar to those in this paper, and they find negligible effects of both domestic and foreign privatization in Romania, Russia, and Ukraine. Only in Hungary are there sizable effects, and both are negative. Although the analysis does not control for productivity, if it tends to be lower for firms about to exit and if privatized firms in Hungary are less likely to exit at a given productivity level, then the Hungarian coefficients could be downward biased through this mechanism.

[23] The results of regressions on pooled data (allowing the factor coefficients and industry-year effects to vary across countries) for all four countries suggest that the differences in the δ_ds are usually significant at the one percent level. The Romanian δ_d is always significantly greater than the Hungarian, Russian, or Ukrainian δ_d. On the other hand, cross-country differences between the δ_fs are statistically insignificant. These results are available from the authors

As Tables 5 and 6 make plain, however, the magnitudes of the estimated effects vary considerably depending on the specification of the aggregate time variables $\mathbf{w_t}$. The FE coefficients are systematically smaller than the OLS, and the FE&FT smaller than the FE. The only exception is domestic ownership in Russia where both FE and FE&FT specifications produce negative coefficients, and the difference is small. With this one exception, the pattern of results holds for the ownership variables in every specification, whether *Private*, *Domestic*, or *Foreign*, and in every production functional form we have tested. To discriminate among the OLS, FE, and FE&FT specifications, we conduct a number of tests. First, we conduct F-tests on the joint probability that all FEs = 0, and on the joint probability that all FTs = 0. The F-statistics reject these null hypotheses at P-values less than 0.0001 for both the FEs and the FTs in every country. Next, 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. The χ^2 statistics reject the hypotheses of no differences across these comparisons, again at P-values less than 0.0001 in each case.[24] We discuss the comparison of these specifications further in the context of the dynamics of the privatization effects around the privatization date, the next subject to which we turn.

Figures 1 and 2 show these dynamics for firms privatized to domestic owners and foreigners, respectively (coefficients and standard errors are provided in Appendix Tables A3 and A4). As described in the previous section, the dynamics are estimated for τ = -4, …, 0, …7+ for domestic privatization and τ = -4,…,0,…5+ for foreign privatization; in both cases the reference group is τ = -5- (all years until 5 years before privatization). Starting with the post-privatization results, the domestic privatization effect in Romania and Ukraine grows fairly continuously after privatization, although with some variation in slope. In Hungary, the domestic postprivatization effect increases to τ = 2, declining slightly thereafter. In Russia, however, it is negative and declines for several years, with some upturn starting only very late—more than five years after privatization. The OLS, FE, and FE&FT specifications differ mainly in the level of the estimated effects, while the profiles of post-privatization changes in the effects are similarly shaped.[25]

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in an unpublished appendix.

[24] The results of the tests also imply that the pattern of coefficients – smaller in the FE compared to the OLS and in the FE&FT compared to the FE – is not driven entirely by measurement error, at least of the classical variety. The details of these tests and test statistics are available in the unpublished appendix.

[25] Part of the difference across specifications in the post-privatization levels of the effects could be measurement error, but as long as the data generating process for measurement error did not alter with privatization, this would

By contrast with the cross-country differences in the domestic effects, the post-privatization profile for the foreign effect is steadily increasing—aside from a couple wobbles in the data—in all four countries. Our results imply that the large overall foreign effect is the result of not only rapid, but sustained relative productivity growth in these firms. This general pattern of steadily increasing relative productivity among firms sold to foreign investors is again robust across estimation methods, with the choice between OLS, FE, and FE&FT affecting mostly the level of the profile.

The post-privatization dynamics, therefore, do not appear to help us to understand cross-country differences in the estimated domestic privatization effect as resulting from differences in how quickly the effect emerges across countries. Counterfactual calculations assuming different timing of the privatization process actually work in the opposite direction: if Romania had privatized as quickly as Hungary, its privatization effect would be still larger, and the same is true for the Ukraine-Russia comparison. A further implication of these results is that the longer run effect of privatization in Romania and Ukraine would be greatly understated by research using data with information through only the first year or two after privatization, while in Russia it would be overstated, at least through four years after privatization had taken place.

While the post-privatization profiles of the OLS, FE, and FE&FT specifications tend to be similar in shape if not level, the pre-privatization differences are striking. In almost every case, the OLS results show substantial productivity growth of firms in the pre-privatization period. In Romania, for instance, MFP is 25 percent higher in τ=-2 than its average in τ=-5 and earlier years among firms domestically privatized, and it is 40 percent higher in firms privatized to foreign investors. The magnitude of this growth in the FE specification is reduced to 20 percent for both domestic and foreign privatization, and the FE&FT specification reduces both to zero. The profile becomes completely flat, implying no differences in productivity behavior between firms subsequently privatized and those remaining state-owned.

Russia shows a similar pre-privatization pattern of much higher productivity growth both for domestic and foreign privatization, and these differences are reduced to almost zero in both the FE and FE&FT specifications. The same is true of the relative behavior of firms later privatized to foreign investors in Hungary and Ukraine. The comparison of specification for the pre-domestic privatization profile in Hungary shows some movement in the other direction,

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not account for the change in slopes we observe: the slope of the OLS profile tends to be higher pre-privatization (as we show below), but lower post-privatization, compared to the slopes in the FE and FE&FT specifications.

however: the OLS results imply almost no relative productivity growth, while the FE and FE&FT imply up to about 10 percent. The behavior of these specifications in Ukraine implies the highest relative growth under OLS and the least under FE, with the FE&FT specification occupying a place in the middle. In both of these cases, however, the magnitude of the estimated effects are small.

We may use these pre-privatization estimates to implement a version of the Heckman-Hotz (1989) pre-program specification test discussed in the previous section. Appendix Tables A3 and A4 enable T-tests on each individual coefficient over the τ = -4, -3, -2 period, and Table 7 reports the results of F-tests for each of the joint hypotheses: $\delta_{-4d} = \delta_{-3d} = \delta_{-2d} = 0$ and $\delta_{-4f} = \delta_{-3f} = \delta_{-2f} = 0$. Both approaches imply in every case that the OLS specification fails to align the behavior of pre-privatized firms with those remaining state-owned, as nearly all the T- and F-statistics are statistically significant. The FE specification substantially improves alignment, particularly in Hungary and Ukraine, where the F-statistics are no longer statisticially significant, even though some individual coefficients remain significant. In Romania, however, both F-statistics and all T-statistics except for the one corresponding to $Foreign_{-4}$ remain significant. In Russia, the F-statistic remains significant in the FE specification only for domestic, not for foreign privatization. The FE&FT specification eliminates the significance of the test statistics in Hungary and Romania; the magnitude of the F-statistic declines drastically in Romania and declines somewhat for domestic privatization in Hungary, but it rises slightly for foreign privatization in Hungary. Except for a similarly slight rise of the F-statistic for foreign privatization in Ukraine, the F-statistics fall under FE&FT in both Russia and Ukraine. The coefficients on the pre-privatization variables are small in both of these countries, however. These results suggest that the random growth (FE&FT) model is useful in eliminating any selection bias that manifests itself as differences in pre-privatization productivity levels.

Finally, it is worth noting the dynamics immediately around the privatization year.[26] These jumps are sizable and fairly similar for foreign and domestic privatizations in Hungary and Romania, although they are larger for foreign privatization in Hungary and for domestic privatization in Romania. In Russia and Ukraine, however, the pattern is much less clear. The

[26] Recall that our basic model defines the private effect with respect to ownership at the end of the previous year, $Private_{it-1}$, because the ownership information is annual. As we pointed out, some firms could be privatized early in the year, resulting in a possible misattribution of the productivity effect that year to the pre-privatization period. The results here imply that if we instead used contemporaneous $Private_{it}$, the estimated δ would be higher in Hungary and Romania, but little changed in Russia and Ukraine.

trends are clearly downwards in the two years preceding sales to foreigners, but they were flat for domestic privatizations. A possible interpretation of these patterns would focus on pre-privatization incentives of managers: in foreign privatizations in Russia and Ukraine, managers may have expected they would be quickly replaced and thus had little to lose and everything to gain from asset-stripping in a nebulous property rights environment; while the flat pre-privatization profiles in the domestic privatizations could result from majority ownership usually going to workers and managers, resulting in little incentive to either improve performance to demonstrate their skills or to strip assets in the run-up to privatization. The sharply positive pre-privatization trend in Hungary for firms sold to foreign investors is consistent with the strong role of Hungarian managers in the privatization process, providing them with incentives to prove their worth to their anticipated new partners and with more certainty that they would stay on the job. The more moderate trend for Romanian firms sold to foreigners may indicate an intermediate situation between managers' expectations of high probability of replacement in Russia and Ukraine and relatively low probability in Hungary.

6. Conclusion

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

The estimation results imply a substantial positive effect of privatization on productivity in Romania, with a range of estimates from 15 to 50 percent, depending on the precise econometric specification employed. The estimated effects are also positive for Hungary, with a range from 8 to 28 percent, but for every estimation method they are lower than the Romanian

estimate. In both of these countries, the estimated effects are always highly significantly different from zero. For Ukraine, the estimated effects are positive, but always much lower—from 2 to 16 percent—and they are sometimes statistically insignificant. Finally, we estimate an effect from -5 to 14 percent in Russia.

The wide range of these estimates for each country does not result from sensitivity with respect to the choice of production functional form; on the contrary, the estimates are highly robust along this dimension. Rather, the range of estimates results from varying approaches taken to dealing with selection bias. We have considered three principal methods: OLS, FE, and FE&FT (fixed effect and firm-specific trends). OLS uses within-industry-year differences in productivity between state-owned and privatized firms; as a within-estimator, FE uses deviations from the firm's mean productivity, while controlling for industry-year shocks; while, as a random growth estimator, FE&FT uses deviations from the firm's trend value of productivity, controlling for industry-year shocks. Consistent with our analysis of pre-privatization productivity levels and trends, which suggested selection bias in both the level and growth of productivity, we find that FE estimates are systematically smaller than OLS, and that FE&FT are smaller than FE.

To better understand these differences, we employ several specification tests. F-tests on the FE fail to reject their inclusion in every country, and similar tests for the FT consistently fail to reject the inclusion of the trends. Hausman-type χ^2 tests reject equality of the vectors of coefficients in the OLS, FE, and FE&FT specifications, again in every country and at very high levels of significance. We also employ some "pre-program" specification tests, along the lines suggested by Heckman and Hotz (1989) that measure for any particular estimator the statistical significance of the pre-privatization deviations in productivity relative to firms remaining state-owned. While this test consistently rejects the OLS specification, sometimes it rejects FE and sometimes it fails to reject it. Nearly always, however, the test statistics are improved when the FT are included. Taken together, the results of these specification tests suggest that the FE&FT specification is preferred in our data.

We find that privatization to foreign investors has effects that are statistically insignificantly different from each other across countries and similar in magnitude, the FE&FT estimates falling in the range of 18 to 36 percent. In all four countries, these estimates are much higher than those for privatization involving predominantly domestic ownership. The consistency of the estimated foreign effects suggests that the substantial cross-country

differences in the average privatization effects are unlikely to be due to any differences across countries in types of data, measurement methods, etc. It also implies that the variation in the domestic privatization effect is a substantial puzzle to be explained; indeed, removing the foreign firms magnifies the difference between Hungary and Romania.

Our estimation of the dynamics of the effects implies that the impact of privatization is immediate in Hungary and Romania, and nearly immediate (one year later) in Ukraine; in these countries, the impacts are sustained and in Romania and Ukraine they continue to increase even after three years. By contrast, the profile of the dynamics remains negative in Russia until the fifth year after privatization. Comparing across countries, the profiles tend to fan out over time, implying that studies relying on data only for the immediate post-privatization period may understate cross-country differences.

Our analysis of dynamics in the pre-privatization period shows that productivity tends to grow in Hungarian and Romanian firms that are eventually privatized, especially so in Hungarian firms acquired by foreign investors. In Russia and Ukraine, however, the pre-privatization profile of productivity is flat for domestic privatizations and sharply negative in the two years before acquisition by foreign investors. We conjecture that the differences in the foreign pre-privatization dynamics may be due to anticipatory effects, whereby managers in Hungary experience enhanced career-concern incentives to demonstrate their skills to their anticipated foreign partners, while those in Russia and Ukraine expect to be automatically fired and therefore engage in asset-stripping.

The dynamics of the privatization effects (domestic and foreign) display a clear jump around the privatization date in all three countries where we estimate a positive effect: Hungary, Romania, and Ukraine. Given that we are controlling for industry-year effects, and in our preferred random growth model for firm fixed effects and trends, this suggests that the effect of privatization on productivity is causal, not the result of some unobservable tendency for firms undergoing privatization to grow faster through the whole period. It is not inconceivable that the privatization coefficient could be biased upward if investors have private information on growth potential and they are able to buy firms with such potential just before the growth spurt is realized. On the other hand, the privatization process in the countries we are studying was driven at least as much by policymakers as by investors, and it frequently took several years to execute a privatization transaction, so it seems hard to believe that this would result in the nearly contemporaneous jump we observe in the data. It should also be noted that the private

information of investors, in this story, would have had to pertain to the future evolution of a firm's productivity controlling for industry-years; in other words, they would have to know that a firm would experience a positive productivity shock relative to other firms in the same industry and year, and then they would have to arrange to acquire the firm just before the shock was realized. This strikes us as implausible but of course not impossible, and while our identifying assumption is that such effects are uncorrelated with ownership change, we cannot entirely rule it out.

These findings strongly support the view that privatization matters, and they provide some evidence that the method of privatization matters as well. In our data, the only relevant distinction we can directly measure is predominant foreign versus domestic ownership, and we find strong evidence that the former has a bigger impact than the latter in all four countries. Moreover, in terms of the average effect over the whole post-privatization period, we find a large gap between the two East European countries (Hungary and Romania) and the two former Soviet Republics (Russia and Ukraine). The difference in privatization outcomes between these regions may be attributable to differences in the "quality" of privatization, especially the extent of concentrated outside ownership; our discussion of the policies in these countries shows that it is correlated. In this sense, our results are consistent with Djankov and Murrell's (2002) hypothesis of such a difference between countries of Eastern Europe and the former Soviet Union and with the international ratings of transition economies according to their "progress in transition" (e.g., EBRD, 2000; World Bank, 1996).

But our findings also pose some problems for the common view that the biggest differences in the results of transition are between these groups of countries. To start with, we find substantial within-region variation in the estimated domestic privatization effects. The estimates consistently imply larger estimated effects of privatization in Romania compared to Hungary, and in Ukraine compared to Russia; both of these reverse the order suggested by international organizations' rankings. Moreover, the dynamics estimates show that both the Romanian and Ukrainian domestic effects continue to rise more than two years after privatization, while the Hungarian effects levels off. As a result, the gap widens between the Romanian and Hungarian effects, and the Ukrainian effect nearly catches up to the Hungarian by 6 years or so after privatization. But the ratings of Romania's policies remained well below those of Hungary, and Ukraine's remained still farther below Romania's, throughout this period.

The results of our analysis of privatization to foreign investors also do not fully square

with the standard view that East European privatization and economic policies more generally have been clearly superior to those of the post-Soviet Republics. If the quality of privatization method is the determinant of outcomes, then we would expect that differences in foreign ownership share would contribute substantially to the differences between Hungary (the country with by far the largest share) and Russia and Ukraine (the countries with the least). Yet our results imply that only a small fraction of the total gap in the privatization effect is accounted for by this difference. The composition effect of foreign ownership also deepens the puzzle about the difference between Hungary and Romania: because the Romanian domestic effect exceeds the Hungarian and the foreign exceeds the domestic effect in both countries, increased foreign ownership in Romania would have increased the difference between the Romanian and Hungarian overall effects. Finally, our finding of similarly large, robust foreign effects across these different countries calls into question the view that complementary aspects of the macroeconomic or business environment affect the return to privatization, unless foreign firms happen to be less sensitive to these conditions. The cross-country variation in the productivity effects of privatization remains an important question to address in future research.

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Table 1: Sample Sizes, 1994

	Number of firms	Percent of all old firms	Total employment	Percent of old firm employment
Hungary	1,633	70.5	351,415	80.8
Romania	1,933	86.3	1,939,852	95.9
Russia	14,620	94.0	10,306,709	97.1
Ukraine	5,698	97.4	3,329,037	97.3

Note: Sample size is expressed in terms of the number of firms, the percent of the number in all old firms (manufacturing firms inherited from the socialist period), the total employment in the sample firms, and the percent of sample employment in all old firm employment.

Table 2: Mean (standard deviation) of Production Function Variables (1st year available, 1994, and 2002)

	Output			*Employment*			*Capital*		
	1st Year	1994	2002	1st Year	1994	2002	1st Year	1994	2002
Hungary	4,313 (12,000)	2,405 (26,800)	3,767 (41,400)	625 (1,225)	215 (644)	169 (443)	1,383 (4,701)	1,435 (22,500)	1,127 (11,000)
Romania	369 (1,290)	399 (1,760)	208 (1,170)	1,224 (2,164)	1,004 (2,324)	435 (955)	608 (2,490)	423 (1,780)	111 (589)
Russia	352 (1,293)	296 (3,414)	344 (3,372)	798 (2,607)	705 (2,258)	519 (2,064)	101 (5,536)	127 (7,055)	164 (1,982)
Ukraine	44 (137)	21 (140)	31 (222)	785 (1,852)	584 (1,511)	469 (1,895)	32 (143)	17 (81)	41 (240)

Note: Capital and output are expressed in constant 2002 prices: mln HUF for Hungary, bln ROL for Romania, mln RUB for Russia, and mln UAH for Ukraine. Output equals the value of sales in Hungary and, the value of gross output in Romania, Russia and Ukraine. Capital in Hungary and Romania equals book value of tangible assets averaged over the beginning of the current year and the beginning of the next year, imputed for missing values as the predicted value of the average capital from a regression on current year capital, year, and industry dummies. Capital in Russia and Ukraine equals average book value of fixed assets used in the main activity of the enterprise, adjusted for revaluations. Employment in Hungary and Romania equals the average number of registered employees; in Russia and Ukraine it equals the average number of registered industrial production personnel, which includes non-production workers, but excludes "nonindustrial" employees who mainly provide employee benefits. 1st year is 1986 in Hungary, 1992 in Romania, 1985 in Russia, and 1989 in Ukraine.

Table 3: Percentage of Sample Firms Privatized——Total, Domestic, and Foreign

	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002
Hungary											
Private	36.1	56.6	72.5	82.4	87.8	91.5	92.8	92.5	93.8	94.0	93.6
Domestic	31.3	48.8	62.7	70.3	74.8	77.3	78.3	77.4	78.2	78.5	77.3
Foreign	4.8	7.8	9.8	12.1	13.0	14.2	14.4	15.0	15.6	15.5	16.2
Romania											
Private	0.2	3.0	7.8	19.4	41.5	46.1	55.3	70.0	78.8	85.3	89.9
Domestic	0.2	3.0	7.7	19.2	41.1	44.9	52.5	65.8	73.5	79.9	84.5
Foreign	0.0	0.1	0.2	0.2	0.4	1.1	2.8	4.3	5.3	5.4	5.4
Russia											
Private	0.0	47.9	79.5	82.5	74.9	74.9	73.1	69.4	68.1	68.2	73.0
Domestic	0.0	47.7	79.1	82.2	74.7	74.3	72.5	68.8	67.5	67.6	72.5
Foreign	0.0	0.2	0.4	0.3	0.2	0.5	0.6	0.6	0.6	0.6	0.5
Ukraine											
Private	0.0	0.2	8.2	17.7	27.8	45.1	57.2	65.5	68.4	84.2	77.1
Domestic	0.0	0.2	8.1	17.6	27.6	44.5	56.4	64.6	67.4	82.8	76.3
Foreign	0.0	0.0	0.1	0.1	0.2	0.6	0.8	0.9	1.0	1.4	0.8

Note: "Private" refers to firms with more than 50% privately held shares. "Foreign" refers to privatized firms with more than 50% foreign-owned shares. The residual category of privatized firms that are not majority foreign is labeled "Domestic."

Table 4: Pre-Privatization Relative Output, Productivity, and Productivity Trend

	Hungary	Romania	Russia	Ukraine
			Output	
Pre-Private	-0.317***	0.031	1.055***	0.254***
	(0.082)	(0.082)	(0.026)	0.041)
Pre-Domestic	-0.494***	-0.053	1.050***	0.265***
	(0.084)	(0.082)	(0.025)	(0.041)
Pre-Foreign	1.117***	1.300***	1.994***	1.406***
	(0.206)	(0.200)	(0.153)	(0.214)
			MFP	
Pre-Private	-0.038	0.065**	0.193***	0.085***
	(0.029)	(0.030)	(0.013)	(0.021)
Pre-Domestic	-0.063**	0.057*	0.192***	0.084***
	(0.031)	(0.030)	(0.013)	(0.021)
Pre-Foreign	0.155**	0.173***	0.360***	0.313***
	(0.071)	(0.072)	(0.101)	(0.109)
			MFP Trend	
Pre-Private	-0.006	0.012***	-0.021***	-0.002
	(0.010)	(0.006)	(0.002)	(0.004)
Pre-Domestic	0.011	0.013**	-0.021***	-0.002
	(0.011)	(0.006)	(0.002)	(0.004)
Pre-Foreign	-0.034*	-0.011	-0.078***	-0.057**
	(0.019)	(0.016)	(0.024)	(0.025)

Note: The relative pre-privatization characteristics of firms are estimated on the sample of never privatized and pre-privatized firms (through the privatization year) in each country. One specification measures pre-privatization differences of all firms subsequently privatized with a single *Pre-Private* dummy, while the second specification permits the differences to vary over *Pre-Domestic* and *Pre-Foreign* firms, in both cases relative to those always state-owned. All equations have output as the dependent variable and include a full set of industry-year interactions. The MFP (multi-factor productivity) and MFP Trend regressors also include labor and capital interacted with industry dummies. In addition, the MFP Trend equation includes *Pre-Private* (respectively, *Pre-Domestic* and *Pre-Foreign*) interacted with a linear time trend normalized to zero in the privatization year. Standard errors are adjusted for clustering on firms. * = significant at 10%; ** = significant at 5%; *** = significant at 1%. N = 8,827 in Hungary, 13,482 in Romania, and 147,763 in Russia for both specifications; N = 42,330 in Ukraine for the specification with *Pre-Private* and 41,367 for the specification with *Pre-Domestic* and *Pre-Foreign*.

Table 5: Estimated Productivity Effects of Privatization

	Hungary	Romania	Russia	Ukraine
		OLS		
		unweighted		
$\hat{\delta}$	0.281***	0.500***	0.138***	0.161***
	(0.035)	(0.031)	(0.018)	(0.026)
R^2	0.826	0.847	0.809	0.744
		weighted		
$\hat{\delta}$	0.304***	0.375***	0.212***	0.250***
	(0.046)	(0.033)	(0.046)	(0.040)
R^2	0.921	0.882	0.824	0.842
		FE		
		unweighted		
$\hat{\delta}$	0.200***	0.250***	-0.047***	0.041**
	(0.022)	(0.022)	(0.015)	(0.018)
R^2	0.684	0.694	0.710	0.606
		weighted		
$\hat{\delta}$	0.156***	0.128***	-0.049	0.080***
	(0.037)	(0.023)	(0.037)	(0.032)
R^2	0.763	0.715	0.597	0.647
		FE&FT		
		unweighted		
$\hat{\delta}$	0.078***	0.146***	-0.026***	0.016
	(0.023)	(0.020)	(0.010)	(0.017)
R^2	0.414	0.415	0.588	0.322
		weighted		
$\hat{\delta}$	0.061*	0.081***	0.004	0.035
	(0.034)	(0.022)	(0.025)	(0.028)
R^2	0.581	0.584	0.494	0.393
N	20,293	21,123	211,213	58,259

Note: Estimated coefficients $\hat{\delta}$ (and their corresponding standard errors) are shown for *Private* (= 1 if the firm is majority privately-owned at the end of year *t*-1). The dependent variable is log(*Output)*, and independent variables include log(*Capital*) and log(*Employment*), with coefficients permitted to vary across industries, as well as full sets of unrestricted industry-year interaction dummies. FE = firm fixed effects; FT = firm-specific trends. The weighted regressions use current employment weights. All statistics are adjusted for clustering on firms and in the FE&FT specifications for the loss of degrees of freedom due to detrending. Both the FE and the FT are statistically significantly different from zero at the 0.0001 level in all countries. * = significant at 10%; ** = significant at 5%; *** = significant at 1%.

Table 6: Estimated Effects of Domestic and Foreign Privatization

	Hungary	Romania	Russia	Ukraine
			OLS	
$\hat{\delta}_d$	0.176***	0.488***	0.135***	0.166***
	(0.035)	(0.031)	(0.018)	(0.027)
$\hat{\delta}_f$	0.761***	0.725***	0.670***	0.451***
	(0.061)	(0.098)	(0.135)	(0.182)
R^2	0.832	0.847	0.810	0.745
$P(\hat{\delta}_d = \hat{\delta}_f)$	(0.000)	(0.015)	(0.000)	(0.117)
			FE	
$\hat{\delta}_d$	0.150***	0.240***	-0.050***	0.044**
	(0.022)	(0.022)	(0.015)	(0.019)
$\hat{\delta}_f$	0.554***	0.399***	0.362***	0.408***
	(0.052)	(0.088)	(0.143)	(0.149)
R^2	0.687	0.694	0.710	0.606
$P(\hat{\delta}_d = \hat{\delta}_f)$	(0.000)	(0.070)	(0.004)	(0.015)
			FE&FT	
$\hat{\delta}_d$	0.053**	0.136***	-0.027***	0.015
	(0.024)	(0.020)	(0.010)	(0.017)
$\hat{\delta}_f$	0.226***	0.303***	0.183	0.355***
	(0.053)	(0.105)	(0.115)	(0.143)
R^2	0.415	0.415	0.588	0.323
$P(\hat{\delta}_d = \hat{\delta}_f)$	(0.002)	(0.115)	(0.069)	(0.018)
N	20,293	21,123	211,213	56,892

Note: Estimated coefficients $\hat{\delta}_d$ and $\hat{\delta}_f$ (and their corresponding standard errors) are shown for *Domestic* (= 1 if the firm is majority private but not majority foreign-owned at the end of year *t*-1) and *Foreign* (= 1 if the firm is majority foreign-owned at the end of year *t*-1), respectively. The dependent variable is log(*Output)*, and independent variables include log(*Capital*) and log(*Employment*), with coefficients permitted to vary across industries, as well as full sets of unrestricted industry-year interaction dummies. FE = firm fixed effects; FT = firm-specific trends. Regressions are not weighted. All statistics are adjusted for clustering on firms and in the FE&FT specifications for the loss of degrees of freedom due to detrending. Both the FE and the FT are statistically significantly different from zero at the 0.0001 level in all countries. P is the P value from an F test for the equality of the domestic and foreign ownership dummies. * = significant at 10%; ** = significant at 5%; *** = significant at 1%.

Table 7: Pre-Program Specification Test Results

	Hungary	Romania	Russia	Ukraine
		OLS		
Domestic	2.24	23.33	71.87	11.11
	(0.08)	(0.00)	(0.00)	(0.00)
Foreign	2.40	9.16	3.57	2.42
	(0.07)	(0.00)	(0.01)	(0.06)
		FE		
Domestic	1.57	7.50	14.91	1.22
	(0.19)	(0.00)	(0.00)	(0.30)
Foreign	0.48	2.96	1.56	0.92
	(0.70)	(0.03)	(0.20)	(0.43)
		FE&FT		
Domestic	1.32	0.40	9.17	2.19
	(0.27)	(0.75)	(0.00)	(0.09)
Foreign	0.52	0.17	0.60	1.00
	(0.67)	(0.91)	(0.62)	(0.49)

Note:. F-Statistics (P–Values) are shown for two hypotheses corresponding to tests of the estimated pre-privatization impact of privatization for domestic and foreign ownership, separately: $\delta_{-4d} = \delta_{-3d} = \delta_{-2d} = 0$, and $\delta_{-4f} = \delta_{-3f} = \delta_{-2f} = 0$. The corresponding coefficients are graphed in Figures 1 and 2.

Figure 1: Dynamics of Domestic Privatization Effects
(effects by year before and after; privatization year = 0)

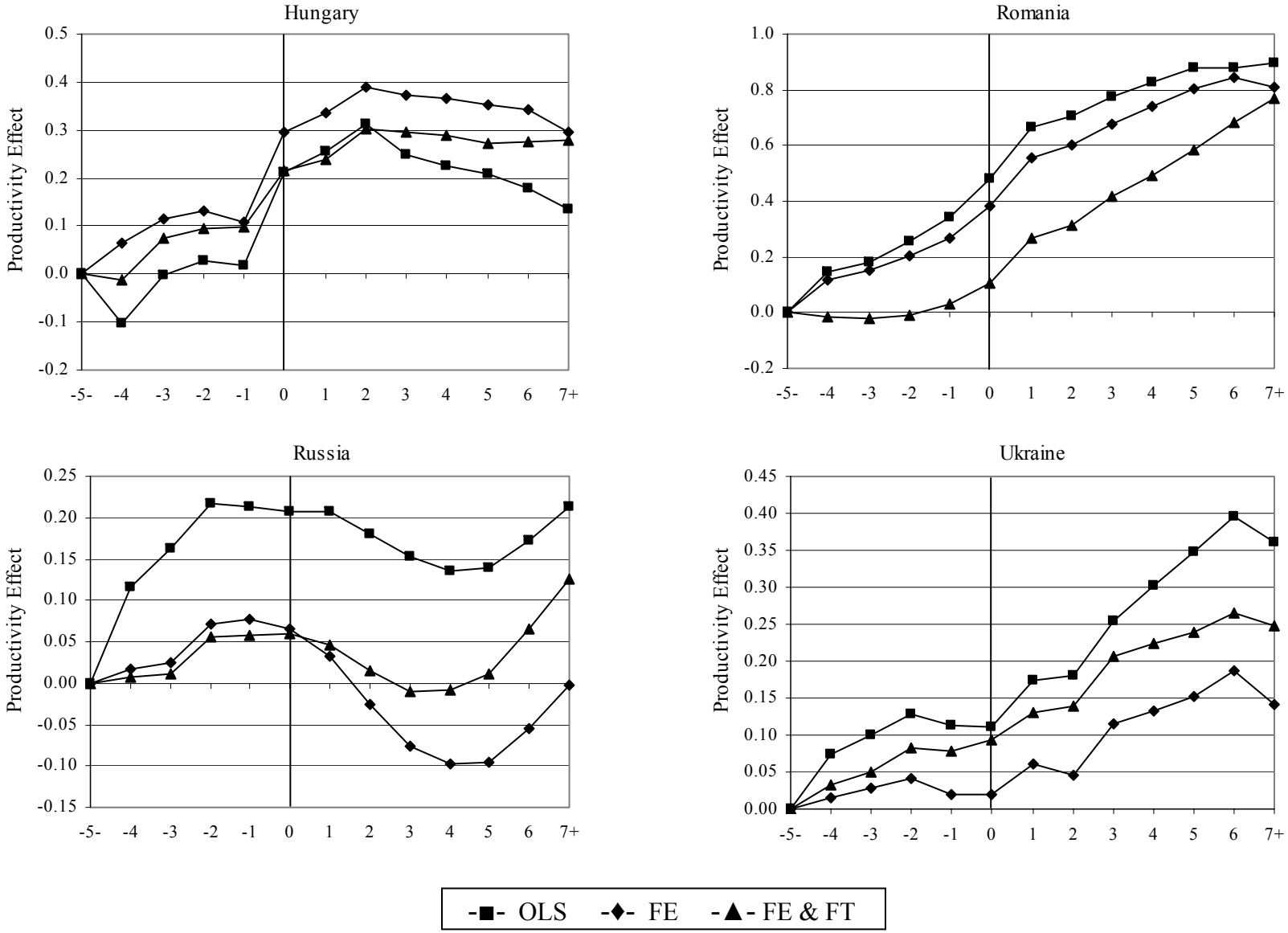

Figure 2: Dynamics of Foreign Privatization Effects
(effects by year before and after; privatization year = 0)

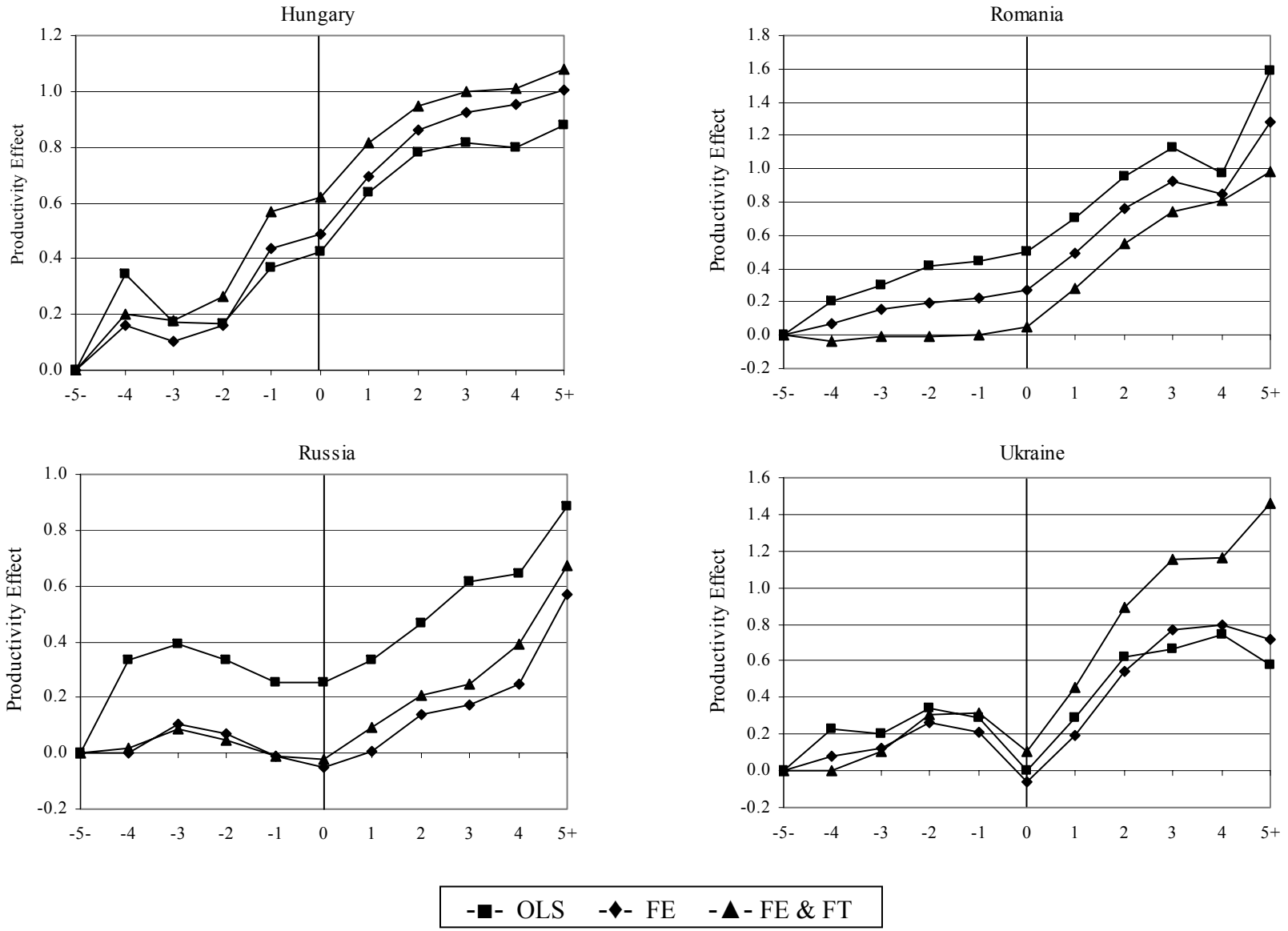

Table A1:

Privatization Effects with Alternative Production Functional Forms

	Hungary	Romania	Russia	Ukraine
		CRS		
$\hat{\delta}$	0.079***	0.148***	-0.026***	0.017
	(0.024)	(0.020)	(0.011)	(0.017)
R^2	0.072	0.117	0.460	0.238
		TL		
$\hat{\delta}$	0.072***	0.141***	-0.021**	0.017
	(0.022)	(0.020)	(0.010)	(0.017)
R^2	0.424	0.425	0.591	0.337
		LP		
$\hat{\delta}$	0.083***	0.149***	-0.026***	0.017
	(0.025)	(0.020)	(0.011)	(0.017)
R^2	0.055	0.104	0.459	0.236
		.3/.7		
$\hat{\delta}$	0.079***	0.162***	-0.022**	0.010
	(0.025)	(0.021)	(0.011)	(0.017)
R^2	0.063	0.079	0.431	0.245
		Mean Coefficient (standard error)		
$\bar{\hat{\delta}}$	0.075	0.136	-0.018	0.019
	(0.026)	(0.021)	(0.014)	(0.019)

Note: Specifications are similar to those in Table 5 (FE&FT) except for the assumed form of the production function: CRS imposes constant returns to scale on the Cobb-Douglas function, TL refers to the translog, LP is labor productivity, and .3/.7 assumes coefficients of .3 on capital and .7 on labor. All statistics are adjusted for clustering on firms and for the loss of degrees of freedom associated with detrending. * = significant at 10%; ** = significant at 5%; *** = significant at 1%. Mean coefficient (mean standard error) includes FE&FT values from Table 5.

Table A2:

Domestic and Foreign Effects with Alternative Production Functional Forms

	Hungary	Romania	Russia	Ukraine
		CRS		
$\hat{\delta}_d$	0.059**	0.138***	-0.027***	0.015
	(0.026)	(0.020)	(0.011)	(0.017)
$\hat{\delta}_f$	0.206***	0.298***	0.189*	0.374***
	(0.052)	(0.104)	(0.116)	(0.143)
R^2	0.073	0.117	0.460	0.239
		TL		
$\hat{\delta}_d$	0.049**	0.129***	-0.022**	0.015
	(0.023)	(0.020)	(0.010)	(0.017)
$\hat{\delta}_f$	0.218***	0.319***	0.179*	0.365***
	(0.052)	(0.103)	(0.115)	(0.141)
R^2	0.425	0.425	0.591	0.338
		LP		
$\hat{\delta}_d$	0.064***	0.138***	-0.027***	0.016
	(0.026)	(0.020)	(0.011)	(0.017)
$\hat{\delta}_f$	0.203***	0.320***	0.183	0.363***
	(0.052)	(0.107)	(0.116)	(0.144)
R^2	0.056	0.104	0.459	0.237
		.3/.7		
$\hat{\delta}_d$	0.058**	0.156***	-0.024**	0.009
	(0.027)	(0.021)	(0.011)	(0.017)
$\hat{\delta}_f$	0.209***	0.249**	0.253**	0.314**
	(0.055)	(0.107)	(0.122)	(0.149)
R^2	0.064	0.079	0.431	0.246
		Mean Coefficient (standard error)		
$\bar{\hat{\delta}}_d$	0.057	0.134	-0.025	0.014
	(0.025)	(0.020)	(0.011)	(0.017)
$\bar{\hat{\delta}}_f$	0.212	0.298	0.197	0.354
	(0.053)	(0.105)	(0.117)	(0.144)

Note: Specifications are similar to those in Table 6 (FE&FT) except for the assumed form of the production function: CRS imposes constant returns to scale on the Cobb-Douglas function, TL refers to the translog, LP is labor productivity, and .3/.7 assumes coefficients of .3 on capital and .7 on labor. All statistics are adjusted for clustering on firms and for the loss of degrees of freedom associated with detrending. * = significant at 10%; ** = significant at 5%; *** = significant at 1%. Mean coefficient (mean standard error) includes FE&FT values from Table 6.

Table A3:
Dynamics of the Domestic Privatization Effect

	Hungary			Romania			Russia			Ukraine		
	OLS	**FE**	**FE&FT**	**OLS**	**FE**	**FE&FT**	**OLS**	**FE**	**FE&FT**	**OLS**	**FE**	**FE&FT**
Domestic$_{-4}$	-0.104**	0.066	-0.013	0.148***	0.119***	-0.011	0.115***	0.017***	0.007	0.073***	0.015	0.032**
	(0.054)	(0.062)	(0.068)	(0.027)	(0.028)	(0.028)	(0.011)	(0.005)	(0.006)	(0.020)	(0.016)	(0.016)
Domestic$_{-3}$	-0.002	0.114*	0.076	0.183***	0.150***	-0.015	0.162***	0.025***	0.011	0.099***	0.028	0.049**
	(0.042)	(0.060)	(0.073)	(0.029)	(0.036)	(0.042)	(0.014)	(0.009)	(0.012)	(0.021)	(0.019)	(0.024)
Domestic$_{-2}$	0.027	0.131**	0.093	0.254***	0.203***	0.001	0.216***	0.072***	0.056***	0.128***	0.041*	0.082***
	(0.043)	(0.064)	(0.088)	(0.031)	(0.044)	(0.058)	(0.015)	(0.012)	(0.017)	(0.022)	(0.022)	(0.033)
Domestic$_{-1}$	0.016	0.109*	0.100	0.342***	0.270***	0.040	0.213***	0.077***	0.058***	0.112***	0.021	0.078*
	(0.042)	(0.065)	(0.103)	(0.035)	(0.052)	(0.078)	(0.015)	(0.015)	(0.022)	(0.025)	(0.026)	(0.043)
Domestic$_{0}$	0.211***	0.295***	0.214*	0.479***	0.382***	0.125	0.207***	0.066***	0.059**	0.111***	0.021	0.094*
	(0.040)	(0.068)	(0.118)	(0.040)	(0.062)	(0.101)	(0.017)	(0.018)	(0.027)	(0.028)	(0.030)	(0.055)
Domestic$_{1}$	0.257***	0.334***	0.240*	0.665***	0.555***	0.285**	0.208***	0.033	0.046	0.173***	0.061*	0.130**
	(0.042)	(0.070)	(0.133)	(0.043)	(0.070)	(0.126)	(0.019)	(0.020)	(0.033)	(0.032)	(0.034)	(0.068)
Domestic$_{2}$	0.314***	0.388***	0.302**	0.707***	0.60***	0.346**	0.180***	-0.026	0.016	0.180***	0.046	0.138*
	(0.045)	(0.075)	(0.146)	(0.047)	(0.081)	(0.154)	(0.021)	(0.024)	(0.039)	(0.037)	(0.039)	(0.083)
Domestic$_{3}$	0.250***	0.374***	0.296*	0.777***	0.678***	0.449**	0.153***	-0.076***	-0.010	0.254***	0.114***	0.206**
	(0.049)	(0.080)	(0.161)	(0.053)	(0.091)	(0.187)	(0.024)	(0.027)	(0.045)	(0.041)	(0.043)	(0.098)
Domestic$_{4}$	0.226***	0.367***	0.288*	0.824***	0.743***	0.530**	0.135***	-0.098***	-0.009	0.303***	0.133***	0.223*
	(0.054)	(0.083)	(0.170)	(0.059)	(0.103)	(0.220)	(0.028)	(0.030)	(0.050)	(0.046)	(0.048)	(0.116)
Domestic$_{5}$	0.209***	0.353***	0.273	0.879***	0.806***	0.642***	0.140***	-0.096***	0.011	0.347***	0.153***	0.239*
	(0.059)	(0.087)	(0.180)	(0.063)	(0.113)	(0.257)	(0.030)	(0.032)	(0.056)	(0.053)	(0.054)	(0.135)
Domestic$_{6}$	0.177***	0.342***	0.276	0.879***	0.845***	0.740***	0.172***	-0.055*	0.066	0.396***	0.186***	0.266*
	(0.065)	(0.091)	(0.189)	(0.068)	(0.122)	(0.294)	(0.031)	(0.033)	(0.060)	(0.061)	(0.061)	(0.156)
Domestic$_{7+}$	0.134*	0.294***	0.278	0.897***	0.811***	0.837**	0.214***	-0.002	0.126**	0.361***	0.141*	0.248
	(0.074)	(0.097)	(0.201)	(0.089)	(0.142)	(0.347)	(0.031)	(0.034)	(0.065)	(0.076)	(0.074)	(0.185)

Table A4:
Dynamics of the Foreign Privatization Effects

	Hungary			Romania			Russia			Ukraine		
	OLS	**FE**	**FE&FT**	**OLS**	**FE**	**FE&FT**	**OLS**	**FE**	**FE&FT**	**OLS**	**FE**	**FE&FT**
Foreign-4	0.343***	0.161	0.200	0.206**	0.069	-0.037	0.335***	0.000	0.016	0.230*	0.083	0.004
	(0.134)	(0.144)	(0.184)	(0.090)	(0.079)	(0.075)	(0.118)	(0.038)	(0.079)	(0.130)	(0.113)	(0.118)
Foreign-3	0.170	0.105	0.177	0.303***	0.155**	-0.006	0.394***	0.102*	0.086	0.199	0.124	0.102
	(0.133)	(0.171)	(0.207)	(0.074)	(0.068)	(0.110)	(0.124)	(0.055)	(0.114)	(0.137)	(0.137)	(0.175)
Foreign-2	0.168	0.160	0.262	0.413***	0.193***	-0.002	0.335***	0.072	0.046	0.344***	0.265*	0.304
	(0.134)	(0.172)	(0.225)	(0.086)	(0.075)	(0.157)	(0.122)	(0.098)	(0.160)	(0.139)	(0.161)	(0.240)
Foreign-1	0.366***	0.434***	0.570**	0.442***	0.221**	0.010	0.253**	-0.011	-0.010	0.293*	0.212	0.318
	(0.086)	(0.152)	(0.244)	(0.100)	(0.094)	(0.209)	(0.107)	(0.114)	(0.180)	(0.163)	(0.190)	(0.293)
Foreign0	0.423***	0.485***	0.620**	0.503***	0.273**	0.053	0.253**	-0.052	-0.024	0.002	-0.062	0.104
	(0.074)	(0.156)	(0.262)	(0.112)	(0.119)	(0.249)	(0.119)	(0.134)	(0.193)	(0.223)	(0.239)	(0.344)
Foreign1	0.635***	0.695***	0.813***	0.702***	0.494***	0.286	0.332**	0.005	0.093	0.287	0.190	0.452
	(0.066)	(0.156)	(0.276)	(0.092)	(0.105)	(0.308)	(0.162)	(0.155)	(0.238)	(0.215)	(0.229)	(0.361)
Foreign2	0.780***	0.862***	0.950***	0.954***	0.762***	0.561	0.464**	0.141	0.209	0.617***	0.542***	0.895**
	(0.066)	(0.158)	(0.285)	(0.130)	(0.152)	(0.368)	(0.195)	(0.156)	(0.248)	(0.180)	(0.200)	(0.399)
Foreign3	0.814***	0.925***	0.998***	1.129***	0.927***	0.754*	0.618***	0.170	0.245	0.663***	0.773***	1.152***
	(0.074)	(0.157)	(0.295)	(0.136)	(0.163)	(0.405)	(0.194)	(0.214)	(0.306)	(0.229)	(0.249)	(0.463)
Foreign4	0.795***	0.953***	1.009***	0.970***	0.844***	0.829*	0.644***	0.250	0.393	0.743***	0.795***	1.164**
	(0.075)	(0.161)	(0.308)	(0.141)	(0.158)	(0.449)	(0.183)	(0.202)	(0.327)	(0.207)	(0.216)	(0.521)
Foreign5+	0.878***	1.008***	1.077***	1.590***	1.277***	1.008**	0.886***	0.568***	0.675*	0.579	0.716**	1.457***
	(0.089)	(0.163)	(0.317)	(0.215)	(0.194)	(0.498)	(0.166)	(0.207)	(0.358)	(0.417)	(0.353)	(0.583)
R^2	0.835	0.687	0.415	0.853	0.698	0.418	0.813	0.711	0.546	0.746	0.606	0.324
N		18,735			20,957			205,297			56,498	