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Does Privatization Raise Productivity?

Evidence from Comprehensive Panel Data on Manufacturing Firms in Hungary, Romania, Russia, and Ukraine

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Abstract

We analyze the impact of privatization on multifactor productivity (MFP) using long panel data for nearly the universe of initially state-owned manufacturing firms in four economies. Controlling for firm and industry-year fixed effects and employing a wide variety of measurement approaches, we estimate that majority privatization raises MFP about 28% in Romania, 22% in Hungary, and 3% in Ukraine, with some variation across specifications, while in Russia it lowers it about 4%. Privatization to foreign rather than domestic investors has a larger impact (about 44%) and is much more consistent across countries. The positive effects emerge within a year in Hungary, Romania, and Ukraine and continue to grow thereafter, but are still ambiguous even after 5 years in Russia. Pre-privatization MFP exceeds that of firms remaining state-owned in all countries, implying that cross-sectional estimates overstate privatization effects. The patterns of the estimated effects cast doubt on a number of explanations for “when privatization works.”

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1. Introduction

The privatization of tens of thousands of manufacturing firms in Eastern Europe during the 1990s represents a gigantic experiment in corporate ownership and performance. The usefulness of investigating these dramatic changes derives not only from the large numbers of observations available for analysis, much larger than those in Western studies, but also from several additional factors.¹ Unlike the situation in the West, where state-owned enterprises usually operate in only a few sectors and tend to differ systematically from other firms, state ownership was indiscriminate during the socialist period in Eastern Europe, accounting for nearly all productive assets. The privatization policies adopted in the aftermath of the sudden collapse of Communist Party control were almost equally indiscriminate, involving rapid transfers of massive numbers of companies in just a few years, in 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. These characteristics imply that the impact on firm performance can be treated as arising in a quasi-experimental situation in which the standard problems in identifying a privatization effect are mitigated.

The East European setting also offers remarkable variation in the design of privatization programs and in the broader economic policy and business environment, all of which carry potential consequences for the effects of privatization on firm behavior. Variables in privatization design include the choice of mass privatization techniques versus individual sales, the extent to which different types of owners acquire shares, and the extent of ownership concentration resulting from the programs (e.g., Frydman et al., 1993a, 1993b). The policy and business environment includes barriers to competition, access to finance, macroeconomic stability, security of property rights, and enforcement of contracts (e.g., EBRD, 1999; Johnson, McMillan, and Woodruff, 2000, 2002; World Bank, 2002). Cross-country variation in these factors may result in differences in the abilities and incentives of new private owners to engage in productivity-enhancing restructuring and may thus account for differences in privatization outcomes.

Despite the attractiveness of this research setting and the inherent scholarly and policy-oriented interest in these issues, however, there have been surprisingly few studies that fully

¹ Megginson and Netter (2001) review Western studies of privatization and some early studies of the results in transition economies; Djankov and Murrell (2002) discuss studies in transition economies.

exploited the possibilities with a corresponding research design. 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 from a single country, and few studies have more than three or four annual observations on each firm.² Marshalling only one or two years of both pre- and post-privatization information, researchers have faced 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. Few have been able to draw on data from more than one country, rendering it difficult to assess the generality of the results as well as the effects of the specific privatization design and the broader policy and business environment on the privatization-performance relationship.

This paper analyzes the productivity effects of privatization using much longer time series and more comprehensive coverage than in earlier research. We have assembled information on manufacturing firms from as early as 1985, when the Communist Party still held power, until 2002, well after most firms had been privatized. The data come from four transition economies – Hungary, Romania, Russia, and Ukraine – which followed very different policy strategies and are frequently alleged to have had very different outcomes (e.g., World Bank, 1996). Furthermore, the coverage of our data for these four countries is quite comprehensive, including 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 privatization and productivity.

² We build on this previous research, including Anderson, Lee, and Murrell (2000), who study 211 privatized firms in Mongolia from 1990 to 1995; Barberis, Boycko, Shleifer, and Tsukanova (1996), 452 Russian shops in 1992-1993; Claessens and Djankov (1999), 706 Czech firms, 1992-1997; Claessens and Djankov (2002), large samples in seven economies, 1992-1995; Djankov (1999), 960 firms in the NIS, 1995-1997; Earle and Estrin (1997), 439 Russian firms in 1994; Frydman et al. (1999), 200 privatized Czech, Hungarian, and Polish firms, 1990-1993; and Smith, Cin, and Vodopivec (1997), about 1000 Slovenian firms, 1989-1992. More similar to our study in analyzing longer panels, although each concerns only a single country, are Earle and Telegdy (2002), with 1992-1999 data in Romania; Lizal and Svejnar (2002), 1992-1998 in the Czech Republic; and Orazem and Vodopivec (2003), 1994-2001 in Slovenia. See also Li (1997), who estimates the effect of reforms on productivity in 272 Chinese firms.

Our basic aim in this paper is to provide robust estimates of this relationship using much larger and longer panels than were available to earlier researchers. Previous studies have also tended to treat productivity as only one of several possible outcome variables, despite the fact that productivity is both more convincing as a performance measure than qualitative measures of restructuring and more closely linked to economic welfare than firm-level sales, profit, or employment. When productivity has been considered, 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 characteristics 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, our approach always takes into account correlated effects (i.e., firm fixed effects) and differences in production technology across industries, and we provide the results of a systematic investigation of the robustness of the ownership effects on productivity across a wide variety of measurement specifications.

In addition to providing robust estimates for each country, we exploit the advantages of these data to shed light on three issues. The first concerns the effects of different types of new private ownership structures. While most studies tend to find an overall positive impact on performance, the level of confidence in the results disaggregated by owner-type is still further reduced by the small sample size problems described above. Our data, however, contain substantial numbers of observations with both foreign and domestic ownership, permitting us to test some common hypotheses about the relative advantages of each of these types of ownership in raising firm performance. On the other hand, these databases do not contain consistent measures of other ownership classifications.

Second, the long-time series in our data permit us to extensively investigate the dynamics of firm performance before and after privatization. Estimates of post-privatization dynamics shed light on how quickly any benefits from privatization are realized and whether they are sustained or tend to diminish over time. Estimates of pre-privatization effects are useful for taking into account possible biases in the selection of firms to be privatized and for assessing the extent to which anticipation of privatization may affect firm performance. Pre-privatization performance could either be enhanced 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; Pinto, Belka, and Krajewski, 1993; 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.

The final issue, one which partially subsumes the previous two, concerns cross-country differences in the effects of privatization. By contrast with previous studies, which either focus on single countries, pool several countries' data together, or apply meta-analysis to the results from a large variety of types of studies and data, our investigation of four countries that have adopted very different reform programs is designed to produce results that can be compared by covering the same time period, using similar datasets, and applying the same econometric techniques.³ We have systematically built up the database to measure all variables as similarly as possible, in order to produce greater comparability across countries. Although the sample of countries is not large enough to permit statistical analysis of the association between the privatization effects and aspects of the economic environment, the patterns we find are relevant for the possibility of such associations, which have been the subject of considerable speculation in previous research. In particular, the hypothesis that weak institutions may attenuate any positive effects of privatization (Anderson, Lee, and Murrell, 2000; Djankov and Murrell, 2002) suggests that our estimated coefficients should vary systematically across countries according to the strength of property rights, enforcement of contracts, and related institutional factors. We also investigate the possibilities that the estimated effects reflect the different methods of privatization and resulting ownership structures, the macroeconomic environment, and initial relative productivity distributions in the four countries.⁴

The rest of the paper is organized as follows. Section 2 describes our data for the four countries we study, and Section 3 discusses the policy environment and privatization programs in order to develop hypotheses on the effects of privatization and how these may vary across countries. Section 4 describes the estimation procedures, and Section 5 presents results. Conclusions are summarized in Section 6.

³ Megginson, Nash, and van Randenbourgh (1994) study data on 61 firms in 18 countries; Boubakri and Cossett (1998), 79 firms in 21 developing countries; and Frydman et al. (1999), 200 firms in 3 transition economies; but none of these estimates country-specific privatization effects. Djankov and Murrell (2002) estimate privatization effects for two regions (Eastern Europe and CIS) from a meta-analysis of studies that typically rely on cross-section data (or very short panels) from a wide variety of sources (mostly small firm surveys and some individual data), use different econometric methods from one another, and analyze outcomes other than productivity (e.g., sales, new products, wage arrears, debt default, qualitative restructuring, successful transactions, etc.). We use nearly universal firm-level data with long time-series and similar variables, focus on productivity, and apply the same methods to estimate comparable country-specific effects; these also shed light on regional differences, discussed below.

⁴ Zinnes, Eilat, and Sachs (2001) present an alternative, complementary approach relating GDP and other measures of macroeconomic performance to the extent of privatization and other variables in 24 transition economies, although it is questionable whether a privatization effect can be identified in aggregate data.

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. This section explains our sources and measures, except for the ownership variables in our data, which are described in the next section together with our discussion of privatization policies in these countries.⁵

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

⁵ A more detailed data appendix is available from the authors upon request.

least 20 employees and some smaller firms is available.⁶ In addition, the Romanian data are supplemented by the National Institute for Statistics' enterprise registry and 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 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 most 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, as a fraction of all old firms and their corresponding employment, are shown in Table 1. Missing values do not reduce the sample greatly in any country, and we have no reason to expect that the sample is biased in any particular direction. The total number of firms ever in the sample is 31,798 and the average number of annual observations per firm is just under ten, making 314,485 firm-years available for analysis.

Table 1: Sample Sizes, 1994

Summary statistics and definitions for the basic variables used to estimate productivity – output, capital, and employment – are provided in Table 2. To save space, we report means and standard deviations only for selected years. 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

⁶ The coverage before 1992 in Hungary is still quite high: total employment in the sample in 1991 is 72 percent of employment in the 1992 sample.

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.

Table 2: Mean Employment, Capital, and Output, 1986, 1994, and 2002

These data have been extensively cleaned to remove inconsistencies and to improve longitudinal linkages that may have been broken due to change of firm identifier from one year to the next (associated with reorganizations and changes of legal form, for instance). The inconsistencies were evaluated using information from multiple sources (including not only separate data providers, but also previous year information available in Romanian balance sheets and Russian and Ukrainian registries). The longitudinal linkages were improved using all available information, including industry, region, size, multiple sources for the same financial variables, and some exact linking variables (e.g., firm names and addresses in all countries except Hungary, where this information was not available) to match firms that exited in a given year with those that entered in the following year.⁷ 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.

Despite these efforts to clean the data, improve the links, and to make them as comparable as possible across countries and over time, measurement errors may still remain. Mismeasurement of productivity – due to errors in output, capital, or labor – could raise the variance of the estimated productivity effect, for instance, while mismeasured ownership could bias the effect towards zero. Reporting practices that vary across ownership types, for example a tendency for privatized firms to under-report inputs relative to output, could also produce biased coefficients. Our approach of analyzing a wide variety of alternative estimation methods is partly motivated by the possibility of

⁷ Firms with more than 50 employees were examined for inconsistencies and missing links. Where they appeared, inconsistencies were resolved following the majority of sources wherever possible. When all sources disagreed, the inconsistencies in the largest firms (more than 500 employees) were resolved through a detailed case-by-case resolution of mistakes associated with miscodings, misplaced decimals, etc. For smaller firms, inconsistencies were resolved in favor the “preferred source,” defined as the source with the most accurate record based on being more frequently in the majority or more often correct in case-by-case evaluations.

such measurement problems, as well as by a desire to assess the robustness of our findings. None of the methods, however, can ameliorate the effects of measurement error entirely, a caveat that should be borne in mind when evaluating the results.

3. Institutional Environment, Privatization Policies, and Implications

This section develops hypotheses on the effects of privatization on productivity, paying particular attention to the relative size of the effects that may be associated with differences in the economic environment and privatization program design across countries. We begin with a general description of reforms in the four countries and how they have been evaluated by external observers, continue with discussions of privatization program designs and macroeconomic developments, and conclude by drawing out the implications for cross-country differences in the effectiveness of privatization at raising productivity. Table 3 summarizes the differences in privatization policy designs, reform progress, and the institutional and macroeconomic environment across the four countries.

Table 3: Summary of Country Characteristics

Reforms and the Institutional Environment

The four countries we study in this paper cover the spectrum of transition economies, at least as conventionally measured in evaluations of “progress” in reform and transition by international organizations such as the European Bank for Reconstruction and Development (EBRD) and the World Bank. The World Bank’s (1996) four-group classification of 26 transition economies, for example, puts Hungary in the first group of leading reformers, Romania in the second group, Russia in the third, and Ukraine in the fourth. Similarly, the EBRD’s annual indicators of “progress in transition” invariably place Hungary at or close to the top of all transition economies; according to the overall “institutional performance” measure in EBRD (2000), Hungary is ranked first, with a score of 3.5 overall. Romania, Russia, and Ukraine are generally placed much lower, and usually ranked in this same order.

One of the most thorough and well-documented ratings of some aspects of the institutional environment for business is Kaufmann, Kraay, and Mastruzzi’s (2003) evaluation of the quality of governance worldwide. The results for our four countries show a similar picture: in 2002, “government effectiveness” is rated most highly in Hungary at 0.78 (by comparison, the U.S. has 1.70 and Singapore and Switzerland both have 2.26), Romania is second at -0.33, Russia is third at -0.40, and Ukraine is last at -0.74 (by comparison, Haiti has -1.56 and Sierra Leone -1.54). On a

similar scale for “regulatory quality,” Hungary receives 1.21 in 2002 (although this represents a rapid rise from a level of 0.47 in 1996), Romania 0.04, Russia -0.30, and Ukraine is again the laggard at -0.62 (much better than Libya at -1.59 or Uzbekistan at -1.44, however). Finally, a similar rating scale for “rule of law” again finds Hungary most highly rated, at 0.90 (Finland has 1.99 and the U.S. 1.70), while Romania follows at -0.12, Russia at -0.78, and Ukraine at -0.79. Thus all available evaluations suggest substantial differences in the business and policy environments across these countries.

Privatization Policy Designs

Kaufmann, Kraay, and Mastruzzi (2003) do not evaluate privatization policies, but the rankings of these countries available from other sources are similar on this dimension. EBRD (1995), for example, used a scale from 1 to 4+ to rate large-scale privatization and other policies of 25 countries. Hungary’s score is “4,” Romania’s is “2.7,” Russia receives a “3,” and Ukraine’s grade is “2.” These scores tend to converge somewhat through the 1990s.

In fact, the methods and tempos of large enterprise privatization differed quite significantly across the four countries. Our evaluations, based on our reading of a long literature (e.g., Frydman et al., 1993a, 1993b), are also summarized in Table 3. Hungary got off to an early start in ownership transformation and maintained a consistent case-by-case method throughout the transition. At the very beginning, the transactions tended to be “spontaneous,” initiated by managers, who were also usually the beneficiaries, sometimes in combination with foreign or other investors (Voszka, 1993). From 1991, the sales process became more regularized, generally relying upon competitive tenders open to foreign participation. Unlike many other countries, there were no significant preferences given to workers to acquire shares in their companies, nor was there a mass distribution of shares aided by vouchers. Hungarian privatization thus resulted in very little worker ownership, very little dispersed ownership, and instead concentrated blockholdings, with a large foreign share. Although the process appeared at times to be slow and gradual, in fact it was 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. The consequences of these programs were large-scale employee ownership and dispersed shareholding by the general population, with little foreign involvement. Beginning in 1997, greater efforts were made to involve foreign investors, and blocks of shares were sold both to foreigners and domestic entities.

The result was a mixture of several types of ownership and a moderate speed compared to neighboring countries.

Russia and Ukraine's earliest privatization experiences have some similarities to the "spontaneous" period in Hungary, as the central planning system dissolved in the late 1980s and decision-making power devolved to managers and work collectives. The provisions for leasing enterprise assets (with eventual buyout) represented the first organized transactions in 1990-1992, but the big impetus for most industrial enterprise privatization in Russia was the mass privatization from October 1992 to June 1994, when the bulk of shares were transferred primarily to the concerned firms' managers and workers, who had received large discounts in the implicit prices they faced (Boycko, Shleifer, and Vishny, 1995). Some shares (generally 29 percent) were reserved for voucher auctions open to any participant, and these resulted in a variety of ownership structures, from dispersed outsiders holding their shares through voucher investment funds to domestic investors who acquired significant blocks; sometimes managers and workers acquired more shares through this means, but there were few cases of foreign investment. Blockholding and foreign ownership became more significant through later sales of blocks of shares and through secondary trading that resulted in concentration. Ukraine followed Russia's pattern at a somewhat slower pace and with even greater preferences granted to insiders in acquiring shares in their companies. In both countries, the initial consequence was large-scale ownership by managers and workers, some blockholding by domestic entities, and continued state ownership. Subsequently, blocks formed and foreigners made partial inroads.

These general patterns are reflected in Table 4, which contains our computations of private ownership, defined here as a strict majority of shares held in private hands, based on the manufacturing firms in our database.⁸ Ownership is measured at the reporting date, the end of each calendar year. As of late 1992, 43.0 percent of the Hungarian firms had already been privatized, while the percentage was only 0.2 in Romania and 0 in Russia and Ukraine. By the end of the period, however, most firms had been privatized in all four countries.⁹

⁸ 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).

⁹ We assume a single change of ownership and recoded cases of multiple switches to the modal category after the first change (ties were decided in favor of private and foreign, unless only two years of data were available). In Hungary there were 71 cases, in Romania 15, and in Ukraine 4. Russia had 2,811 firms private since 1995 reclassified as state in 2000 or 2001; when ownership codes changed drastically; such mass renationalization did not occur, so our recoding corrects this problem. The nonmonotonicity of percent privatized in Table 4, therefore, is

Table 4: Percentage of Sample Firms Privatized—Total, Foreign, and Domestic

The table also contains the percentage of firms majority privatized to foreigners.¹⁰ This fraction is by far the highest in Hungary, reaching nearly 22 percent of all entities by the end of our observation period. In Romania, the percentage reaches 6 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 we label them “domestic” in the discussion below. But some cases of minority foreign investment (particularly in Hungary) are also included in this category.

Macroeconomic Environment

Like all the transition economies of Central and Eastern Europe and the former Soviet Union, the four countries we study in this paper experienced severe declines in output and bursts of inflation in the early 1990s. Hungary emerged from recession and reduced inflation most quickly, followed by Romania, which went back into recession, however, after only a couple years of growth. Inflation was also more volatile in Romania, which resumed growth only at the very end of the 1990s.

The magnitude and duration of output decline was greater in Russia than in either of the two East European economies, and it was still greater in Ukraine. Both former Soviet Republics experienced severe shocks in 1998-1999 following the Russian default of August 1998, and both have experience strong growth since then (starting in 1999 in Russia and 2000 in Ukraine), associated with increases in oil, gas, and commodity prices. The countries in our analysis therefore display large variation in macroeconomic conditions during this period. The patterns of industrial production and producer price inflation are summarized in Table 3.

Implications for Estimates of Privatization Effects

What do these substantial differences in institutional environment, macroeconomic conditions, and privatization design imply for hypotheses on the effectiveness of privatization in raising productivity? Starting with privatization policy design, the implications are controversial.

due to split-ups of state-owned firms, which are subject to later privatization and thus included in our sample.

¹⁰ The Russian registries contain codes for state, domestic, joint ventures, and 100 percent foreign firms, but foreign shares are available only for a subset of firms in four years. We classify all joint ventures as foreign, but the results are very similar if we include only those foreign firms with a majority foreign share in at least one of the four years.

Privatization through transfers to employees has been common in transition economies due to relative ease of administrative and political implementation, but it has been criticized as ill-suited to the restructuring demands of the transition. 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.¹¹ 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.¹² Finally, case-by-case sales of large blocks of shares is the method usually considered most effective, but it also has disadvantages: insufficient demand and political difficulties compounded by problems of valuation, plus the frequent practice of imposing contractual obligations on future investment and employment that may reduce restructuring. Among the types of buyers, foreign owners are likely to have better access to finance, management skills, new technologies, and knowledge of markets, which would suggest a higher productivity effect relative to domestic ownership. On the other hand, foreigners may face special difficulties restructuring firms when layoff decisions are highly politicized, and local networks and knowledge of local conditions are nontransparent.

The implications of the policy design for the relative effectiveness of privatization in raising productivity across countries depend on how one evaluates these divergent arguments. If privatization works most effectively when the new owners are concentrated outside investors, and even more so when they are foreign investors, then we would expect a stronger impact on enterprise productivity the closer the program comes to producing such ownership structures, i.e., in Hungary. In order of the importance of concentrated outsiders and foreign investors, the effect of privatization would be second largest in Romania, followed by Russia and Ukraine. If instead, however, insiders

¹¹ 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.

¹² See, e.g., Stiglitz (1999); Black, Kraakman and Tarassova (2000); Kornai (2000); Spicer, McDermott, and Kogut (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).

are the most effective at restructuring and running their companies, or if it is the case that an initial privatization to dispersed outsiders might lead to a better ultimate selection of a controlling owner than would have resulted from an initial sale of a controlling block, then the implications could be different, possibly even suggesting that the magnitude of the effect could decline across our four countries in inverse alphabetical order: Ukraine, Russia, Romania, and Hungary.

Even if the relative magnitudes of the privatization effects are not reversed, the consequences of different privatization methods might 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 reflect the possibilities for secondary trading leading to increased concentration, however, and countries with high initial levels of inside and dispersed outside ownership initially may tend to catch up so that the final impact after several years is not very different across countries.

Turning to macroeconomic conditions, it is possible that the effectiveness of new private owners in restructuring and increasing productivity varies with price stability and demand conditions. For instance, without reliable price signals it may be difficult to make choices that improve firm performance. The comparative advantage of private ownership may be higher when demand conditions are strong, and the new owners respond with expansion and innovation. Alternatively, it is possible that private owners are superior at cost-cutting in a more constrained environment. In any of these cases, the estimated privatization may vary due to the large fluctuations in aggregate inflation and output growth rates across the countries and time periods we observe.

Concerning the business and policy environment, a natural hypothesis is that privatization works best in a business environment that protects property rights and enforces contracts, which private owners require to ensure a return on their investment and effort (Anderson, Lee, and Murrell, 2000; Black, Kraakman, and Tarassova, 2000). In this case, countries with better institutional ratings should also have the strongest privatization effects. Similarly, privatization may work better when market signals are clear and when entrepreneurial opportunities are particularly rife, for instance when inflation is stable and there is some macroeconomic growth. An alternative view of the business and policy environment might be that ownership matters least when the environment

functions well, as regulation, competition, and hard budget constraints serve to discipline firm behavior, and it may be that any type of firm does well in a good macroeconomic context. From that point of view, the institutional and macroeconomic environment could be a substitute rather than a complement for private ownership. This argument is similar to La Porta, Lopez-de-Silanes, and Shleifer's (1999) claim that concentrated ownership is more effective when legal protections are weaker.

The quality of the institutional environment may also affect the dynamics of the privatization effect. For example, if better institutions result in faster development of financial markets that facilitate ownership reallocation and concentration, then a country with a relatively poor initial ownership structure but good institutions may tend to start off with a low privatization effect but then catch up over time. Under the conventional assumption that concentrated outside ownership is the most likely to deliver productivity improvements, these arguments imply that the initial differential privatization effect may not wear off so quickly, and it could even persist, as the same countries that start with greater ownership concentration would also have the best chances for further productivity-enhancing reallocation of ownership.

Yet another possibility would be that the nature of the privatization policy design and the quality of the business and economic policy environment have offsetting effects. For example, it might be the case that private ownership is generally most effective in a poor institutional environment but that concentrated outside investors tend to have the strongest effect on productivity in all types of environments. Or perhaps the reverse is true, or perhaps the factors interact; for instance, inside ownership might be relatively efficacious in a poor environment and outside ownership might be superior when institutions function well. In any of these situations, the predicted cross-country ranking of the coefficient magnitudes becomes ambiguous. How the effects of privatization vary is ultimately an empirical question, one for which our results below provide evidence.

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 econometric methods, and therefore our approach to measuring productivity is eclectic. We investigate the

implications of using a wide variety of production functional forms as well as assumptions of arbitrary production structures. Separate functions are estimated for each country both to permit functional forms to vary across countries and to investigate differences in the estimated privatization effect. In all estimated equations, we permit the production technology to vary across industries. Throughout, we include both firm fixed effects and a full set of industry-year effects, the former to control for firm characteristics and the latter to control for time-varying industry characteristics that may be correlated with both ownership and productivity.

Using a model for panel data, in which i indexes firms, j indexes industries, and t indexes time periods (years), our approach to estimating the privatization effect simultaneously with the technology parameters takes the following general expression for each country separately:

$$y_{it} = \sum_j f_j(k_{it}, l_{it}) * D_j + u_{it}, \quad (1)$$

where y_{it} is $\ln(\text{output})$, f_j is an industry-specific production function, k_{it} is $\ln(\text{capital stock})$, l_{it} is $\ln(\text{employment})$, D_j are industry dummies, and u_{it} is the residual “productivity.” Our approach to identifying the effect of privatization decomposes u_{it} as

$$u_{it} = \sum_j \sum_t \gamma_{jt} D_j D_t + \omega_i + \delta \text{Private}_{it-1} + \varepsilon_{it}, \quad (2)$$

where D_t are year effects (10-17 years, varying by country), ω_i is a firm fixed effect, and Private_{it-1} is a dummy variable = 1 if the firm is majority privately owned at the end of the previous year.¹³ This specification thus permits different productivity levels for each industry in each year by including a full set of industry-year interactions, 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.¹⁴ The specification further includes firm fixed effects ω_i controlling for the possibility of correlated effects at the firm level, for instance some aspect of firm “quality” correlated with both ownership and productivity but unobserved in our data. The residual ε_{it} includes the effects of measurement error, specification error, and within-firm and within-industry-year-cell variation in unmeasured factors of production.¹⁵ Our identifying assumption is that these

¹³ 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 (i.e., the first post-privatization shareholders’ meeting) varies across firms, with some of them already fully private early in the year we assume is the final pre-privatization year. But some such assumption is necessary, and we discuss this issue further in connection with the dynamics of the effect below.

¹⁴ We distinguish 10 industry-level production functions based on the trade-off between disaggregation and number of observations, specifying a minimum of 50 observations per year per country for each industry.

¹⁵ 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 first proposed by Arellano (1987); the standard errors are therefore robust to both serial correlation and heteroskedasticity, and they

components are uncorrelated with firm ownership. The coefficient of interest is δ , the mean within-country-industry-year difference in MFP between firms majority private and majority state-owned. Note that (1) and (2) are estimated in a single step, so that the technology parameters are permitted to be correlated with ownership.¹⁶

We specify the industry-specific technologies f_j using a wide variety of methods and function forms. Among these are the standard Cobb-Douglas and Translog forms:

$$\sum_j f_j(k_{it}, l_{it}) * D_j = \sum_j \alpha_j k_{it} * D_j + \sum_j \beta_j l_{it} * D_j \quad (3)$$

$$\sum_j f_j(k_{it}, l_{it}) * D_j = \sum_j \alpha_{1j} k_{it} * D_j + \sum_j \beta_{1j} l_{it} * D_j + \sum_j \alpha_{2j} k_{it}^2 * D_j + \sum_j \beta_{2j} l_{it}^2 * D_j + \sum_j \alpha_{12j} k_{it} * l_{it} * D_j. \quad (4)$$

The Cobb-Douglas form is estimated several ways: imposing constant returns to scale and with free parameters, and instrumenting the factors with their one-year lagged values and simply as OLS.

We also consider the arguments of Olley and Pakes (1996) and Levinsohn and Petrin (2003) that unobserved productivity shocks leading to correlation between factor inputs and the error term can be controlled by including an investment proxy and other inputs in the equation. Our data lack information on investment, so our method is to use the first-difference of the capital stock as proxy for (gross) investment. To the extent this approach is successful, it controls for the unobserved shock and bias associated with unobserved factors. The estimator also uses a control function to account for sample selection bias due to nonrandom exit. Our exclusion restrictions are similar to those in Olley and Pakes (1996), relying on higher order polynomials in investment and capital as these reflect realized productivity and the threshold of productivity above which firms survive and below which they exit.¹⁷

The estimated output elasticities of capital and labor (for example, α_j and β_j from Equation (3)) are provided in Appendix Table A1. The low value for capital is common in analyses including fixed effects (e.g., Griliches and Mairesse, 1998), but the correlations of the estimated elasticities tend to be quite high across methods within countries, although they are less high (although still nearly always positive) across countries. Considering pairs of countries, the patterns are interesting: the correlations for Hungary are rather high with Romania, weaker with Ukraine, and quite weak

do not suffer from the autocorrelation problem of difference-in-differences estimators discussed by Bertrand, Duflo, and Mullainathan (2004). Kezdi (2003) contains a detailed analysis of autocorrelation and the robust cluster estimator in panel data models.

¹⁶ Conventional Hausman tests always imply the firm fixed effects are statistically significant in our data, but to test the sensitivity of our results we also estimated the privatization effect dropping these fixed effects; the qualitative findings (general magnitudes and ranking of countries) are very similar to those reported below.

¹⁷ We estimate all equations on an unbalanced panel, as Olley and Pakes (1996) recommend, and we always include firm fixed effects to control for the type of potential simultaneity bias that is our primary concern, namely in the privatization process.

with Russia; but they are quite high for Romania both with Russia and with Ukraine, and for Russia with Ukraine. These patterns presumably reflect the degree of technological similarity among these countries.

A second broad approach to measuring productivity involves the calculation of “Solow residuals,” the difference between output and the sum of inputs weighted by industry factor shares:

$$SR_{it} = y_{it} - \sum_j (1 - \theta_j) k_{it} * D_j - \sum_j \theta_j l_{it} * D_j \quad (5)$$

where θ_j is the labor share in industry j . Given that y represents output rather than value added, θ_j should be measured as the residual of output less labor and material input costs, but unfortunately our data lack materials cost measures on a comparable basis for each country in each year. Therefore, we investigate the implications of alternative assumed technologies for alternative values of θ_j such that $0 \leq \theta_j \leq 1$. Two extreme cases are labor productivity (which assumes $\theta_j = 1$ for all j) and capital productivity (which assumes $\theta_j = 0$ for all j), while intermediate cases involve alternative specifications of θ_j . The estimated SR calculated on this basis are then regressed on the $Private_{it-1}$ dummy, a full set of industry-year effects, and firm fixed effects:

$$SR_{it} = \delta Private_{it-1} + \sum_j \sum_t \gamma_{jt} D_j D_t + \omega_i + u_{it} \quad (6)$$

Like all our investigations of different functional forms, specifications, and methods, these assumed technologies are employed for the purpose of checking the robustness of our results across a very broad set of alternative approaches.¹⁸

Each of these methods can be used to estimate the effects of privatization to foreign and domestic investors. For example, for the analogous specification given by relations (1) and (2), we have:

$$y_{it} = \sum_j f_j(k_{it}, l_{it}) * D_j + \delta_f Foreign_{it-1} + \delta_d Domestic_{it-1} + \sum_j \sum_t \gamma_{jt} D_j D_t + \omega_i + \varepsilon_{it} \quad (7)$$

where $Foreign_{it-1}$ implies majority foreign ownership, $Domestic_{it-1}$ refers to private firms that are not majority foreign-owned (although they may be partly foreign, partly domestic, as discussed in the data description), and δ_f and δ_d are the effects of interest. Besides the evident interest in the relative performance of these two ownership types, this analysis permits us to address the hypothesis that the large cross-country differences in the relative share of privatization to foreign investors may help account for the differences in the estimated homogeneous privatization effect.

We also investigate the dynamics of the privatization effect before and after privatization

¹⁸ Our data do not contain any information that would be useful for instrumenting ownership; both pre-privatization performance and aspects of the privatization process (e.g. Earle and Estrin, 1997) are correlated with investor interest and future performance.

takes place. Our motivation is threefold: first, examining pre-privatization dynamics provides information on whether firms were already improving productivity prior to the ownership change. Such a finding could be interpreted as the result of some dynamic selection bias that our fixed effects methods do not account for, or it could be associated with changed 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). Second, 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? Third, the dynamics of the privatization effect may provide some help in understanding cross-country differences. As discussed in the previous section, the cumulative size of the effect could be similar in all countries while the speed of the effect differs, or the reverse could be true.

We implement this estimation of dynamics by interacting dummy variables for the years before and after privatization with $Private_{it}$. The privatization year (where $Private_{it-1} = 0$ and $Private_{it} = 1$) is omitted, so that all effects are calculated relative to productivity in that year. The first through fourth pre-privatization years are indicated by $PrePriv1_{it}$, $PrePriv2_{it}$, $PrePriv3_{it}$, and $PrePriv4_{it}$, respectively, while the fifth pre-privatization and earlier years are combined into a single variable $PrePriv5_{it}$. The post-privatization variables are defined analogously, with the labels $PostPriv1_{it} - PostPriv5_{it}$. Thus specified, the equation becomes:

$$\begin{aligned}
y_{it} = & \sum_j \alpha_j k_{it} * D_j + \sum_j \beta_j l_{it} * D_j \\
& + \pi_{.5} PrePriv5_{it} + \pi_{.4} PrePriv4_{it} + \pi_{.3} PrePriv3_{it} + \pi_{.2} PrePriv2_{it} + \pi_{.1} PrePriv1_{it} \\
& + \pi_1 PostPriv1_{it} + \pi_2 PostPriv2_{it} + \pi_3 PostPriv3_{it} + \pi_4 PostPriv4_{it} + \pi_5 PostPriv5_{it} \\
& + \sum_j \sum_t \gamma_{jt} D_j D_t + \omega_i + v_{it},
\end{aligned} \tag{8}$$

where $\pi_{.5}$ measures the average productivity effect among firms five or more years prior to their privatization, π_1 measures the average productivity effect among firms during the first year after privatization, etc., permitting an evaluation of the dynamics of changing productivity in firms as they go through the privatization process. We also compute these for foreign and domestic ownership and report the estimated π resulting from these analyses in graphical form below.

We conduct a number of additional exercises to try to account for cross-country differences in the estimated privatization effect. Motivated by the possibility that the impact of privatization varies with firm quality (which may also vary across countries), we examine pre-privatization productivity levels, relative to state-owned firms. We also interact the firm’s relative productivity in the year before privatization with the privatization effect, allowing a very general form for this relationship by specifying relative pre-privatization productivity as a quartic. Variation over time in the quality of firms selected for privatization and in the methods employed are investigated with a similar equation that permits the privatization effect to vary with year of privatization (cohort). The possibility that macroeconomic conditions and the business environment may influence the effectiveness of privatization is examined by allowing the privatization coefficient to vary with calendar years. The specific hypothesis that new private owners may be more responsive than the state to opportunities for growth is tested by interacting privatization with output growth in the 2-digit industry. Finally, we investigate compositional effects associated with variation in the industrial structure across countries and in the privatization effect across industries. The methodology used in each of these extensions is described in greater detail with the results, below.

5. Results

We begin with the basic results from estimation of the average privatization effect δ for each country, and then continue with estimates of the foreign effect δ_f and the domestic effect δ_d . Next we report our estimates of the dynamics of these effects for several years before and after the privatization date. The final set of results provides several extensions organized around potential explanations of the cross-country differences in our estimates of the basic privatization effects.

Basic Estimates

As discussed in the previous section, we consider a wide variety of alternative specifications of technology in estimating the effect of privatization on multifactor productivity. Table 5 includes results from three Cobb-Douglas functions (OLS, IV, and Olley-Pakes estimators) for Equation (1) and from two assumptions about the labor share in a Solow residual measure ($\theta = 1$ and $\theta = 0.7$) for Equation (5). The estimated δ s from some other specifications of Equations (1) and (5) – including a Cobb-Douglas with constant returns to scale imposed, a translog, and some other assumed technologies ($\theta = 0.0, 0.3, 0.5$) – are reported in Table A2.

Table 5: Estimated Productivity Effects of Privatization

Regardless of specification and econometric method, the estimated δ s are positive and highly

statistically significant in Hungary and Romania. The magnitudes vary somewhat across specifications, but the Romanian point estimate, which lies between 0.20 and 0.36, always exceeds the Hungarian, between 0.14 and 0.31. The 95-percent confidence intervals around the estimates are overlapping in 2 of the specifications in Table 5, while they do not overlap in the other 3 cases. If we compute the mean estimated effects (and standard errors) across all 10 estimation methods in Tables 5 and A2, we obtain 0.225(0.026) for Hungary and 0.288(0.027) for Romania.

By contrast, the estimated δ s for the two former Soviet Republics are much lower. In Ukraine, the point estimates are uniformly positive, but they are small in magnitude, lying in the range from 0.02 to 0.06, and they are not always significantly different from zero. In Russia, the point estimates are almost always negative, and while the magnitudes are not large (from -0.07 to 0.002), they tend to be precisely estimated. Even more than in Hungary and Romania, the magnitudes vary little across specifications and methods. The mean estimated effects (mean standard errors) across all 10 estimation methods are -0.041(0.017) for Russia and 0.040(0.019) for Ukraine.¹⁹

We find, therefore, robust evidence of large differences in the estimated privatization effect across the four countries in our sample. The implied ranking of countries by the magnitude of the effect, however, follows neither the alphabetic nor reverse alphabetic ordering suggested by some of the hypotheses in Section 2 above. While the effect is clearly larger in the two former Soviet “satellites” than in the two former Soviet member states, the ranking within these groups is rather surprising: the evidence implies a somewhat larger effect in Romania than Hungary, and in Ukraine relative to Russia. A constant theme in the rest of what follows, therefore, is the question of what factors might account for these cross-country differences.

Foreign versus Domestic Ownership

A first potential explanation for the cross-country differences involves the type of new private ownership, whether foreign or domestic. If the effects of these two types differ, then the different shares of foreign and domestic privatization (Table 4) would imply different average privatization effects across countries. Table 6 therefore reports similar specifications to Table 5, and Appendix Table A2 contains specifications where the privatization effect is permitted to vary between these two ownership types.

Table 6: Estimated Effects of Foreign and Domestic Privatization

In all four countries, we estimate the foreign privatization effect δ_f to be large and highly

¹⁹ The results of regressions on pooled data for Hungary and Romania and for Russia and Ukraine allowing all coefficients to vary by country suggest that the differences of the δ s are usually statistically significant.

significant. The magnitudes are remarkably similar to each other, nearly all in the range from 0.37 to 0.50. The mean(standard error) of the estimated effects across the 10 methods in Tables 6 and A3 is 0.481(0.052) for Hungary, 0.395(0.094) for Romania, 0.386(0.152) for Russia, and 0.431(0.156) for Ukraine. 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 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 subject to macroeconomic volatility and poor business environment.²⁰ Indeed, concerning the latter, foreign owners may even be relatively disadvantaged by lack of inside knowledge to cope with bureaucracy and corruption. The evidence is far from decisive, of course, but it is suggestive that the source of cross-country differences should be sought elsewhere.

The uniformly high estimate of the foreign privatization effect implies that the effects of domestic privatization vary widely, and our results provide direct confirmation of this reasoning. Again, we find positive and statistically significant effects for both Hungary and Romania, but the $\delta_i s$ in Hungary are reduced relative to the overall privatization effects shown in Table 5. As a consequence, the difference between the estimated effects in Hungary and Romania is slightly greater than before: while the difference in estimated δs in Table 5 varies from 0.03 to 0.10 across specifications, the difference in the $\delta_i s$ in Table 6 lie in the range 0.06 to 0.12. 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. The mean estimated domestic effect(standard error) is 0.197(0.027) for Hungary, 0.280(0.028) for Romania, -0.043(0.017) for Russia, and 0.043(0.021) for Ukraine.²¹

How much does the varying foreign-domestic composition of privatization contribute to the cross-country differences we have estimated in the average privatization effect? An interesting counterfactual question is how the δs in the three countries with relatively little foreign ownership – Romania, Russia, and Ukraine – would be changed if they had Hungary’s larger foreign ownership share (but kept their own estimated foreign and domestic privatization effects). In Romania, this

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 The results of regressions on pooled data (allowing the factor coefficients and industry-year effects to vary across countries) for Hungary and Romania and for Russia and Ukraine suggest that the differences between the $\delta_i s$ are statistically insignificant, while the differences in the $\delta_i s$ are usually significant at the one percent level.

would increase the average privatization effect by only about a percentage point, but imposing the Hungarian foreign ownership share in Russia and Ukraine would raise their δ s by about 5 percentage points. This difference would be enough to make the average effect in Russia almost exactly zero (i.e., even if Russia had imported as much foreign capital in proportion to size as did Hungary) and to raise that in Ukraine to close to 10 percent. Correspondingly, if we impose the Russian foreign ownership share on Hungary, the Hungarian δ falls by about 0.04.

We find, therefore, that the large differences in foreign share, combined with the significant advantage of foreign over domestic ownership in all four countries, can explain only a relatively small part of some of the differences in estimated δ across countries: namely, about 5 percentage points of the difference between the East European countries and the former Soviet Republics. It does not explain why the Romanian δ is greater than the Hungarian, and indeed it deepens this puzzle, because under this counterfactual the gap between the estimated Romanian and Hungarian domestic effect δ_d becomes even wider. In most of the extension below, therefore, we focus on results for δ_d .

Dynamics

We turn next to dynamics of the privatization effects around the privatization date. As discussed in Section 3, examining the development of the post-privatization effects may shed light on the possibility that the estimated cross-country differences result from a slower emergence of the positive effects of privatization in some countries compared to others. A slower impact might occur, for instance, if the initial post-privatization ownership structure is highly dispersed so that concentration through secondary transactions is necessary before any productivity-enhancing restructuring can be realized. Our specification of Equation (8), as discussed in detail in the previous section, also permits us to assess the temporariness or permanence of the impact of privatization as well as the possibility of selection effects or anticipatory incentive effects in pre-privatization behavior (which, as we noted, could be either positive or negative, reflecting either career concerns or asset-stripping). The temporal variation in effects relative to the year of privatization of the private ownership, foreign ownership, and domestic ownership effects, based on the π coefficients in this equation, are shown in Figures 1-3.²²

Figure 1: Dynamics of Privatization Effects

Figure 2: Dynamics of Foreign Privatization Effects

²² These results and the rest of those reported in the text rely on the basic Cobb-Douglas production function, but the results differ only trivially when we employ alternative specifications of technology.

Figure 3: Dynamics of Domestic Privatization Effects

The estimation results for the average privatization effect are graphed in Figure 1, with the horizontal axis indicating the number of years before and after privatization, and the magnitude of the effect (relative to productivity in the privatization year, denoted “0”) on the vertical. In Hungary, Romania, and Ukraine, the privatization effect grows fairly continuously after privatization, although with some variation in slope. In Russia, however, it is negative and declines for several years, with some upturn starting only very late – in the five and more years after privatization. The dynamics of the domestic effect tell a fairly similar story, but 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.

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 Hungary, 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.

The dynamics in the pre-privatization period also warrant close attention. While rather flat for δ and δ_t in Russia and Ukraine, the trends in Hungary and Romania are already positive well before the privatization year. As we have pointed out, such pre-privatization increases could reflect the selection of firms for privatization with higher growth potential, or they could reflect enhanced incentives among managers expecting privatization in the future. Arguing for the first interpretation is the fact that the increases begin several years before privatization actually happens, but arguing for the second is the fact of sizable jumps in both countries immediately around the privatization year.²³

²³ Recall that our basic specifications (1) and (5) define 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, in both cases by about 0.03, but the difference between them would remain about the same (because even though the Hungarian jump from year -1 to year 0 is greater, the jump receives a smaller weight than in Romania, where there are fewer post-privatization years). Using contemporaneous

Unfortunately, in the absence of an experiment (a random allocation of privatization) we have no way of eliminating selection bias completely, but we do find the large, roughly contemporaneous jumps suggestive that more than selection is at work and that privatization has had a real impact.²⁴

The pre-privatization dynamics are fairly similar for foreign and domestic privatizations in Hungary and Romania: in all cases in these countries, the trend is positive, although the slope is larger for Hungarian firms eventually sold to foreigners than to domestic agents, while the reverse is true for Romania. But the dynamics are very different in the comparison of foreign versus domestic privatizations in both Russia and Ukraine. In the former Soviet Republics, the trends are clearly downwards in the two years preceding sales to foreigners (while, as we noted, these 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, discussed in Section 3, 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.

Extensions

This sub-section considers a number of extensions to the basic estimates. The analysis is organized around potential explanations for the cross-country differences in the the privatization effects, although most of the extensions also have wider implications. We begin with a further

ownership would also produce a bigger gap between the estimated δ s in the two East European countries relative to Russia and Ukraine, because the latter would be unaffected.

²⁴ Alternatively, the possibility of anticipatory behavior could be eliminated if privatization had been completely unforeseen, but this is difficult to believe in the context of the huge public debates over privatization policies taking place in these countries during this period. It should also be noted that even finding a flat profile of pre-privatization productivity does not eliminate all possibility of selection bias, as potential investors could have information on growth potential that is unobserved in our data, although if such information concerns the industry rather than the firm, the industry-year interaction effects in our equations would control for it.

discussion of pre-privatization differences in the quality of firms slated for privatization, relative to firms remaining state-owned, and continue with analysis of the extent to which the estimated privatization effect varies with pre-privatization productivity level, year of privatization (cohort), calendar year, and industry growth rate. Finally, we examine possible composition effects in the cross-country differences. Most of the discussion is focused on the domestic effects because, as we have seen, the foreign effects are uniformly positive and of relatively similar magnitude across the four countries, while the domestic effects vary widely. Furthermore, for some of the analyses, the number of observations on foreign ownership is too small to permit confident conclusions.

Our examination of dynamics, above, highlighted cross-country differences in the pre-privatization evolution of productivity in firms slated for privatization. These different patterns could be related to a possible explanation for the cross-country differences in the privatization effect: policies that tend to select “better” or “worse” firms for privatization. As we noted in Section 3, the privatization policies in these countries were fairly indiscriminate, but each government has retained a significant number of firms in majority state ownership. If, for example, more productive firms are more likely to be privatized, then a simple cross-sectional comparison of state-owned versus privatized firm performance will lead to incorrect inferences about the effect of privatization. To gauge possible differences across countries in relative quality of firms slated for privatization, Table 7 provides estimates of pre-privatization productivity relative to state firms.²⁵ The pre-privatization advantage is estimated to be large in all four economies, ranging from 0.11 in Ukraine to 0.32 in Romania. For foreign privatization, the range is from 0.27 in Ukraine to 0.43 in Romania, while for domestic it is 0.11 to 0.31. These results imply that firms selected for privatization in all four countries are more productive than those kept in state hands, on average, and thus that there is positive selection in the privatization process. Studies that rely only on cross-section data or that use longitudinal data but fail to control for fixed differences between firms are likely to overstate the benefits of privatization.²⁶ This result is particularly salient for foreign privatizations, although it applies to domestic ownership as well.

Table 7: Pre-Privatization Relative Productivity

Our estimation method in this paper controls for pre-privatization productivity differences through the use of firm fixed effects, of course, but could such differences affect the subsequent,

²⁵ These results are computed as the coefficients on group effects (replacing the firm fixed effects) for “ever privatized” in Equation (1) and for “ever foreign” and “ever domestic” in Equation (7).

²⁶ For instance, this may account for the positive effects estimated by Earle and Estrin (1997), who analyze a cross-section Russian enterprise sample in 1994.

post-privatization potential for productivity to be improved? In other words, could the privatization effect be heterogeneous in the level of pre-privatization productivity? For example, it might be easier to restructure and improve the productivity of firms with relatively low productivity, or the opposite could hold. If such a relationship exists, and if pre-privatization productivity varies across countries, then this provides another possible explanation for the stark cross-country differences we have documented. To assess this, we re-estimate interacting δ_d with a quartic function of pre-privatization productivity, which is measured relative to productivity in state-owned firms in the same industry and in the year immediately prior to privatization.²⁷ The results are plotted in Figure 4, where, for ease of comparison across countries, pre-privatization productivity has been normalized to have zero mean and unit standard deviation, and the range constrained to [-2, +2].

Figure 4: Variation in Domestic Privatization Effect by Pre-Privatization Productivity

The results suggest a substantial negative effect in Hungary and Romania, implying that the effectiveness of privatization in raising productivity is negatively related to position in the pre-privatization productivity distribution. The profiles for these two countries are rather similar to one another, with an estimated domestic privatization effect about 0.6 greater for firms at the bottom of the distribution compared with those near the top.²⁸ But those for Russia and Ukraine are quite different – although similar to each other – in showing a relatively flat relationship. The Russian profile curves slightly downwards in the middle of the distribution, while the Ukrainian curves slightly upward, but the range of variation is quite small (practically always between -0.1 and +0.1), certainly in comparison with Hungary and Romania.²⁹

These findings cannot account for different privatization effects for the two East European countries versus the two former Soviet Republics, but could they explain the difference between Hungary and Romania? Suppose that Romanian firms are, on average, less productive than Hungarian firms by a third of a standard deviation of productivity in the joint productivity distribution for the two countries. Then the effect of privatization on Romanian firms would be higher on average by about 0.05, which is approximately the difference we found in Tables 5 and 6.³⁰

²⁷ We focus on the domestic privatization effect δ_d here because it varies sharply across countries, while the foreign effect δ_f varies comparatively little. Moreover, there are too few observations on foreign-owned firms in Russia and Ukraine for us to estimate many types of heterogeneity in these effects.

²⁸ We have also calculated the pre-privatization productivity interactions for δ_f in Hungary and Romania, which have sufficient observations, and these have similar shape – falling about 0.6 over the productivity distribution.

²⁹ The negative relationship together with the relative pre-privatization productivity level (in Table 7) implies that the expected improvement is negatively correlated with the probability of becoming privatized.

³⁰ These calculations assume an approximately linear relationship in Figure 4, with a negative slope of about -0.15, which is close to what we observe for these two countries.

The problem with actually measuring relative productivity is that it requires an assumption about the appropriate exchange rate – and for much of the period there are problems in choosing or calculating the correct rate due to high inflation in one or both countries, and officially fixed rates that may have borne little relationship with market prices. Nevertheless, if we estimate relative productivity using the average annual “market exchange rates” reported in the *International Financial Statistics* (IMF, 2004), we find that Romanian firms are less productive than Hungarian by 27.7 percent of one standard deviation of their joint productivity distribution.³¹ Thus, a greater impact on less productive firms, combined with cross-country differences in average productivity, may partially account for the higher estimated privatization effect in Romania compared with Hungary. But why the effect does not vary with pre-privatization productivity in Russia and Ukraine is unclear, and this analysis does not help explain the difference in privatization effects between the latter two countries.

Another possible source of variation in firm quality, and therefore possibly in the estimated privatization effect, could be differences over time in the selection of firms for privatization and in the methods used. The selection process could have been influenced by information about growth potential that is unobservable with our data (but observed to potential buyers or to the privatization agency), there might have been some learning process that improved the privatization process with greater experience, and there could have been some systematic variation over time in the methods that were used – which as we have discussed was quite pronounced in Romania, for instance. To assess the possibility of fluctuation of privatization effect by cohort, we permitted δ_i to vary with the year in which privatization took place, and the results are graphed in Figure 5.³²

Figure 5: Variation in Domestic Privatization Effect by Cohort Year

The figure shows little systematic variation in the effectiveness of privatization by cohort that might be useful for interpreting the cross-country differences, although they are somewhat consistent with differences in privatization method within countries. Romania is an apt case because of the high correlation of cohort with time, as described in Section 3. The figure shows that the Romanian δ_i increases from about 0.21-0.24 in 1994-1996 to 0.32 in 1997-1998, which would be consistent both with learning by the State Ownership Fund and with the large number of sales privatizations that started in 1997, after the insider buyouts of 1994-1995 and mass privatization of 1996. After 1998,

³¹ This estimate results from a pooled regression of Hungarian and Romanian data for 1992-2002 (years common to both countries in our data), with all monetary values in HUF2002, and with a specification similar to Equation (1): capital and labor coefficients vary across industries (but not countries), and industry-year interactions are included.

³² Again, our main focus is investigating alternative explanations for the sharp cross-country differences in δ_i , and there are too few foreign observations to permit a systematic analysis of the variation in δ_i by cohort.

δ_d tend to fall somewhat, but this could be partly due to the systematic tendency to observe earlier post-privatization years for more recent cohorts. In fact, the magnitude of the fall is reduced if we include a variable representing the length of time since privatization occurred, although the decline is still somewhat puzzling.³³ In any case, we find similar or larger privatization effects in Romania compared to Hungary in 1994-1996 and larger effects in later years. Therefore, it does not appear to be the case that our finding of a higher δ_d for Romania than Hungary is driven by the most recent years, when Romania – like Hungary earlier – largely pursued sales privatizations.

The pattern of cohort effects in Russia is also suggestive of the differences in privatization methods used over time. Unfortunately, the later period does not contain many observations, so we are forced to pool the years after 1995, but the pattern is nevertheless clear in the data.³⁴ Later cohorts, representing the so-called “second wave” privatizations, when sales became much more common, tend to have much higher estimated effects than those in 1993-1994, which were dominated by insider privatization. For Ukraine, however, the pattern is just the opposite, as the estimated domestic privatization effect falls steadily from 1996 to 2000, even though sales methods were becoming more common during this period. Moreover, the higher estimated effect of domestic privatization in Ukraine compared to Russia is driven mostly by the higher cohort effects in 1993-1994, when both countries pursued insider privatization almost exclusively.³⁵ Thus, while the patterns of cohort effects are mostly consistent with within-country variation in privatization methods over time, they do not appear very helpful in explaining the differences across countries.

Regardless of the particular year in which a firm was privatized, the productivity consequences of privatization could vary over time because of changes in the macroeconomic or business environment, as discussed in Section 3. To assess this possibility, we permit δ_d to vary by calendar year, and the results are graphed in Figure 6. Again, it is difficult to see any systematic variation, however. In Romania, the effect increases fairly steadily, which is consistent with improvements in the business environment but not with the macroeconomic volatility, including a second sharp recession, that Romania experienced during this period. The other countries show little trend over the whole period, except for some slight suggestion of a small decline in Hungary. The fluctuations, moreover, are not consistent with macroeconomic changes, for instance the 1995

³³ In this case, the cohort effect drops by 0.07 from 1998 to 2000; the difference is no longer statistically significant.

³⁴ There are 85 privatizations from 1995 to 2001 in our Russian data. If we disaggregate this period by years, then for the 3 years with at least 15 privatizations we find a suggestive pattern of an increasing effect (0.14 in 1995, 0.16 in 1997, and 0.35 in 1998), but the datapoints in this analysis are too few to permit strong conclusions.

³⁵ These basic trends are again little affected if we control for length of time since privatization.

stabilization program in Hungary. In fact, despite similar macroeconomic trends and shocks over these years, the Russian and Ukrainian effects appear in the figure as mirror images. While the rise in the Russian number from 1998 to 2001 seems to reflect economic recovery from the 1998 crisis, the Ukrainian figures trend downwards over the same years, unaccountably jumping substantially only in the last year in our sample, 2002. Like our earlier finding that the foreign ownership effect is uniformly high in all four countries, therefore, this analysis provides little support for the hypothesis that the business environment and macroeconomy affects the productivity gains from privatization.³⁶

Figure 6: Variation in Domestic Privatization Effect by Calendar Year

While the coefficients on year-ownership interactions appear to be completely uncorrelated with both inflation and aggregate output growth, a possible objection is that the variation in business conditions summarized by these interactions is too crude to capture relevant conditions at the firm-level. We therefore investigate heterogeneity in the domestic privatization effect with respect to growth in the firm's 2-digit NACE industry. Growth for this purpose is defined as $x = (X_t - X_{t-1}) / 0.5(X_t + X_{t-1})$, where X_t is real output in year t , which makes x symmetric (and bounds it in the range $[-2, +2]$). We include x in Equation (1) as a quartic interacted with *Domestic*. The results for x over the range $[-0.5, +0.5]$ (because of the very small number of observations on industry growth outside this range) are plotted in Figure 7.

Figure 7: Variation in Domestic Privatization Effect by Industry Growth

The results do not appear capable of helping to account for cross-country differences in the estimated privatization effect. The plot is uniformly highest for Romania and second-highest for Hungary; the former trends mildly upwards and the latter slightly downwards. The Russian and Ukrainian functions also head in opposite directions, and they actually cross, as the Russian has a strong positive slope. Why Russia should be so different is not clear – once again, just about any argument for Russia would appear to apply *a fortiori* to Ukraine – but the lack of consistency across countries would seem to rule out this factor as an explanation of cross-country differences in the privatization effect.

A final hypothesis is that the estimated impact of privatization is subject to compositional

³⁶ Subject to caveats about small sample sizes (although the problem is not as great towards the end of the period as it is for the cohort analysis, since the calendar year effects concern the cumulative number of firms privatized), the variation in δ_t over calendar years is volatile, again in ways that are hard to correlate with macroeconomic conditions. The trend is upward in all four countries, but given the lack of similar tendency in δ_{it} , improvements in the business environment are an implausible explanation. More likely is the increasing time since privatization during which foreign owners steadily increased productivity, as we saw in Figure 2.

effects. We have already investigated one type of compositional effect, namely for foreign versus domestic privatizations, although we found that taking this into account works in the opposite direction for the Hungary-Romania comparison (i.e., it increases the Romanian effect relative to the Hungarian), while it can explain only a small part of the differences between these countries and the former Soviet Republics. But another possibility is that differences in composition of these economies by industry, together with differences in privatization effects by industry, may account for the cross-country variation.

The industrial structures of the four countries do in fact differ from one another, although the two East European countries are more similar to each other, as are the two former Soviet Republics. For example, food and beverages account for 36 percent of all firm-year observations in Russia and Ukraine, but only 16 percent in Hungary and 23 percent in Romania, while the share of machinery is 9 and 11 percent among Russian and Ukrainian observations, respectively, but 13 and 15 percent for Hungary and Romania. When we permit the δ_d to vary by industry, we find some variation across countries, but this could be due to any number of factors including differences in the predominant methods of privatization used in various industries, in the timing of privatization by industry, and in the relative quality of industries.³⁷ In order to assess the impact of differing industrial structure, we can hold these factors constant by fixing the country- and industry-specific δ_d and permitting the industrial shares to vary, with the results shown in Table 8.

Table 8: Counterfactual Domestic Effects Assuming Other Countries' Industrial Structures

The results suggest only small adjustments to the estimated δ_d when a different country's industrial structure is imposed. For instance, the Hungarian δ_d rises about 0.015 with the Romanian structure, and the Romanian falls about 0.016 with the Hungarian structure, which does not account for a large fraction of the difference between the two countries. The Russian and Ukrainian δ_d change even less (0.007 and 0.005) when each other's industrial structure is assumed. Moreover, the gap between Hungary and Romania, on the one hand, and Russia and Ukraine, on the other, actually tends to widen, as the δ_d in both Hungary and Romania are increased when we impose either the Russian or Ukrainian industrial structure. Thus, compositional effects associated with different

³⁷ The pattern of correlations of the estimated δ by industry is interesting: the cross-country δ_d correlations tend to be high for Hungary and Romania (about 0.5) and even higher for Hungary and Russia and for Russia and Ukraine (about 0.6), but weaker for Romania and Russia (0.2), and, depending on method, zero or negative for both of the pairs Hungary-Ukraine and Romania-Ukraine. The cross-country correlations of the δ_f are lower than for the δ_d , although the small number of foreign-owned firms is again a caveat in interpreting this result. Within countries, the correlations of the δ by industry are high across estimation methods.

industry shares provide essentially no traction for understanding cross-country differences in the productivity impact of privatization, except for a small contribution to the Hungary-Romania comparison.

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.

To summarize, we find a substantial positive effect of privatization on productivity in Romania, with a range of estimates from 20 to 36 percent, depending on the precise econometric specification employed. The estimated effects are also positive for Hungary, but for every estimation method they are lower than the Romanian estimate, with a range from 14 to 31 percent. 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 6 percent – and they are sometimes statistically insignificant. Finally, we estimate a small negative effect, from 0 to 7 percent, in Russia.

These results are based on equations that control for correlated effects (firm fixed effects) and for a full set of industry-year interactions. For any measurement or specification problem to produce a biased estimate of the privatization effect would require that the extent of the problem vary systematically within firms and within industry-year cells. The results are also robust, in terms of approximate magnitudes and exact ranking of countries, to a wide variety of approaches to productivity measurement. The analysis, therefore, strongly supports the proposition that

privatization can have an substantial impact on firm behavior, but it also implies that the impact can vary with other factors.

We find that privatization to foreign investors has effects that are rather similar across countries, nearly all the estimates falling in the range of 37 to 50 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.

The impact of privatization appears to be 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. In general, the profiles tend to fan out as time passes, 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 firm fixed effects and industry-year effects, 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.

All four countries nevertheless show evidence of substantial selection effects in the privatization process, as the pre-privatization productivity of firms selected for privatization is higher than that of firms remaining in state ownership. The difference is especially large for firms to be sold to foreign owners, with a range of 27-43 percent in estimated pre-privatization productivity advantage. This finding may reflect the reluctance of governments to face the painful restructuring that may ensue from the privatization of weaker enterprises. Our estimation techniques control for such selection bias through fixed effects, but this result implies that studies using cross-sectional data and methods are likely to overstate the impact of privatization, particularly to foreigners.

In order to better understand the varied effects of privatization to domestic owners across countries, we also investigate some dimensions of heterogeneity in the domestic coefficient. The impact of privatization in Hungary and Romania is estimated to be strongly negatively related to relative pre-privatization productivity, but no such relationship is found for Russia and Ukraine. The domestic privatization effect does not vary systematically with cohort (year of privatization) or calendar year in any of the countries. The relationship with industry output growth is not systematic either. Finally, while some variation is found in the domestic privatization effect across industry, this variation is little correlated with differences in industrial structure across countries.

These findings provide some support for the view that the method of privatization matters. 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, we find that the largest cross-country differences concern the two East European countries (Hungary and Romania) versus the two former Soviet Republics (Russia and Ukraine), which may also be attributed to differences in the "quality" of privatization, especially the extent of concentrated outside ownership. In this sense, our results provide strong support for

Djankov and Murrell's (2002) hypothesis of such a difference between Eastern Europe and the former Soviet Union. We also find evidence of domestic privatization cohort effects within countries, particularly for Romania and Russia, which in each case correlates with the variation in the use of insider giveaways versus outside sales methods over time.

But our findings also present some puzzles for this viewpoint. To start with, differences in foreign ownership share contribute only a small fraction of the differences between Hungary (the country with by far the largest share) and Russia and Ukraine (the countries with the least). Out of a total gap in the privatization effect of about 0.2, only about 0.05 is accounted for by this difference. Secondly, the difference in foreign ownership only 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. Third, the cross-country variation in our estimated privatization cohort effects is frequently inconsistent with the implications of differences in methods. The estimated domestic effects in Romania, for instance, are at least as great as those in Hungary for practically every cohort, including some when Romanian privatization was dominated by management-employee buyouts (1994-1995) and the highly dispersed mass privatization program (1996). In both Russia and Ukraine, the early cohorts represent mass insider privatization, but the Ukrainian effects are much larger than the Russian. Sales become much more prevalent in the mid- and late 1990s in both of these countries, but while the cohort effects become larger in Russia, they actually decline in Ukraine.

The other main potential explanations for the cross-country differences founder similarly. Macroeconomic stability, economic growth, business environment, and hardness of budget constraints might help to explain either the interregional (Hungary and Romania versus Russia and Ukraine) differences or, alternatively, those within regions, but not both at the same time. Suppose, for example, that macroeconomic stability, economic growth, strong institutions, hard budget constraints, and sales methods contribute to more successful privatization. This could explain why Hungary and Romania's privatization effects are stronger than Russia and Ukraine's, but it cannot explain why Romania's is stronger than Hungary's and Ukraine's is stronger than Russia's. Alternatively, suppose these factors attenuate the privatization effect. That would explain the within-region differences, but not the interregional ones. These results thus cast doubt on stories commonly promulgated for cross-country variation.

Explanations associated with the business and macroeconomic environment have additional difficulties explaining the lack of systematic pattern in the privatization effect across calendar years. If what really matters is the quality of rule of law, contract enforcement, and supporting institutions, then the privatization effect should be higher initially in Hungary than in the other countries, and it should increase over time in all countries as the business environment improves. But the data do not display this pattern. If what really matters is the macroeconomic environment, then the privatization effect should be correlated with the fluctuations of the macroeconomy in each country. Again, while the privatization effect does fluctuate, the correlation with inflation and output growth is difficult to perceive. Even when we consider disaggregated industry growth rates as an indicator of demand conditions for the firm, we find no evidence that these matter for the privatization effect.

The one explanation that seems somewhat helpful for the intra-regional comparison of Hungary and Romania is the heterogeneity of the estimated privatization effect with respect to relative pre-privatization productivity. For both Hungary and Romania, we find a similar pattern whereby firms with lower pre-privatization productivity are improved more by privatization than those whose rank in the pre-privatization distribution was higher. The specific magnitudes are also suggestive, in that an increase of one standard deviation in the pre-privatization productivity distribution is estimated to lower the privatization effect by 15 percentage points, and we find that Hungarian firm productivity exceeds Romanian by about 27.7 percent, thereby accounting for about 4 percentage points of the 6-12 percentage point gap in the privatization effects of the two countries. This factor does not help to explain, however, the difference between Russia and Ukraine.

Thus, while our research provides a set of robust estimates of privatization effects, their heterogeneity along several dimensions, and their dynamics for four countries, it also raises some puzzles associated with the estimated cross-country differences. In particular, the conventional explanations of privatization method, business environment, and macroeconomic growth appear unable to explain much of the variation. Of course, it could be the case that our particular combination of countries fails to capture the relevant considerations. The countries in our sample not only have the strong advantage that we have been able to assemble nearly comprehensive microdata on the originally state-owned manufacturing sector, but they also nicely straddle the distribution of policy choices and outcomes among transition economies, at least according to conventional observers. Making further progress in understanding when privatization works, however, may require assembling and analyzing comparable data for still more countries.

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Figure 1: Dynamics of Privatization Effects

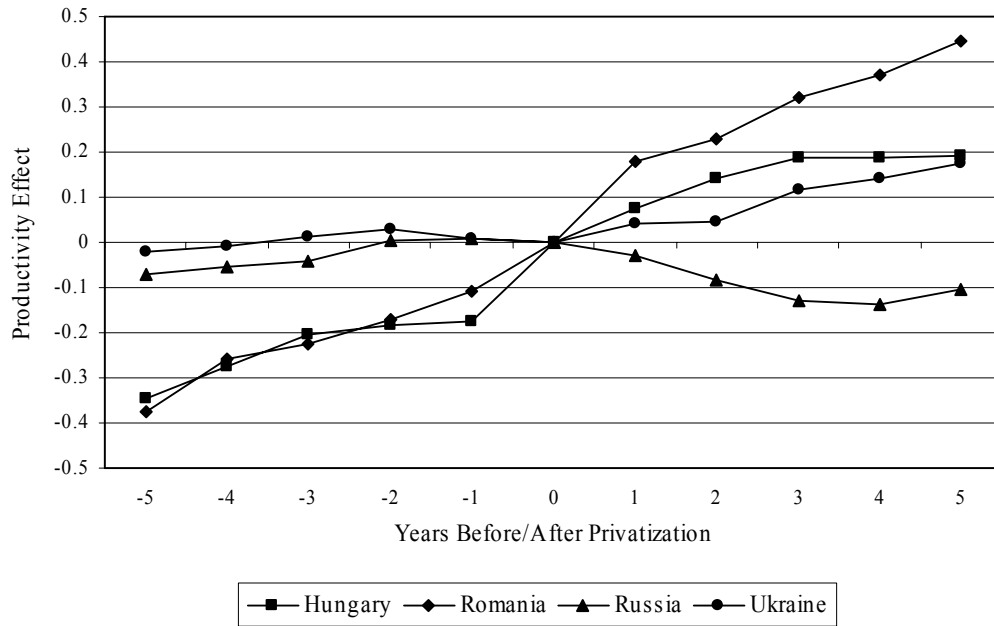


Figure 2: Dynamics of Foreign Privatization Effects

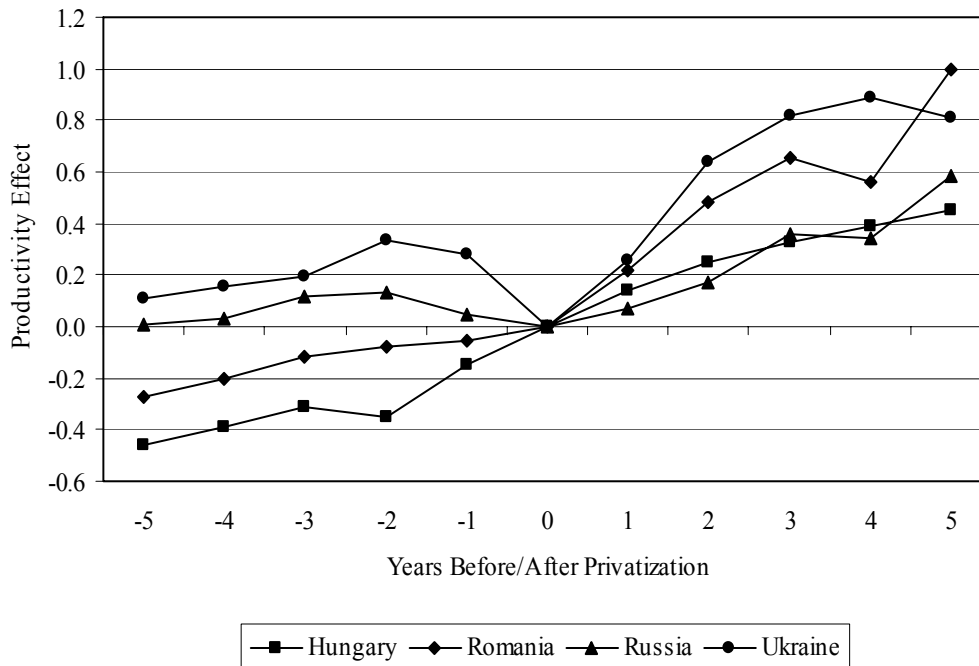


Figure 3: Dynamics of Domestic Privatization Effects

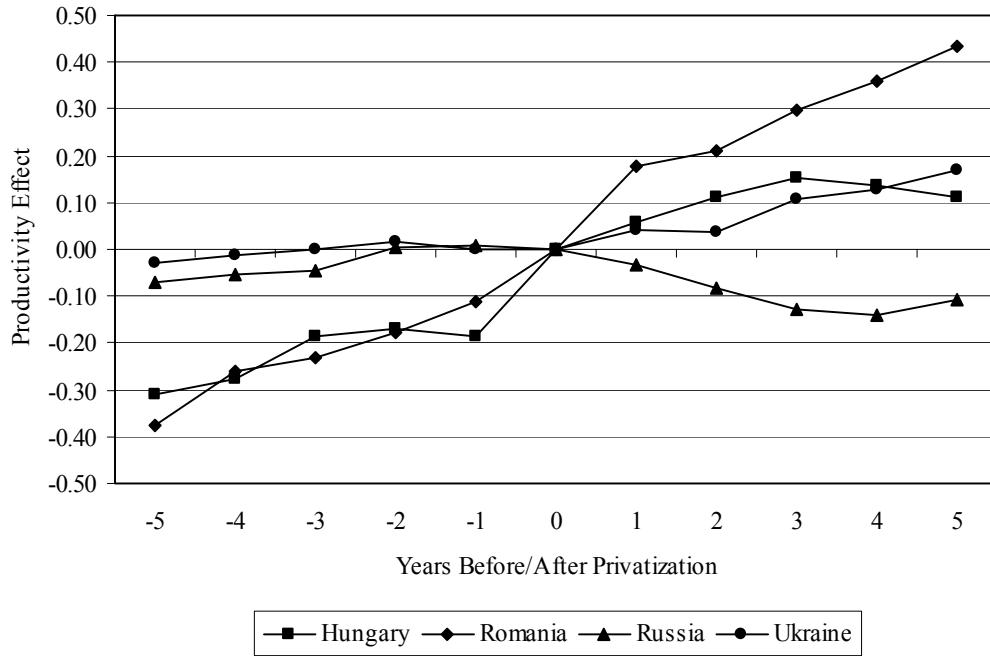


Figure 4: Variation in Domestic Privatization Effect by Pre-Privatization Productivity

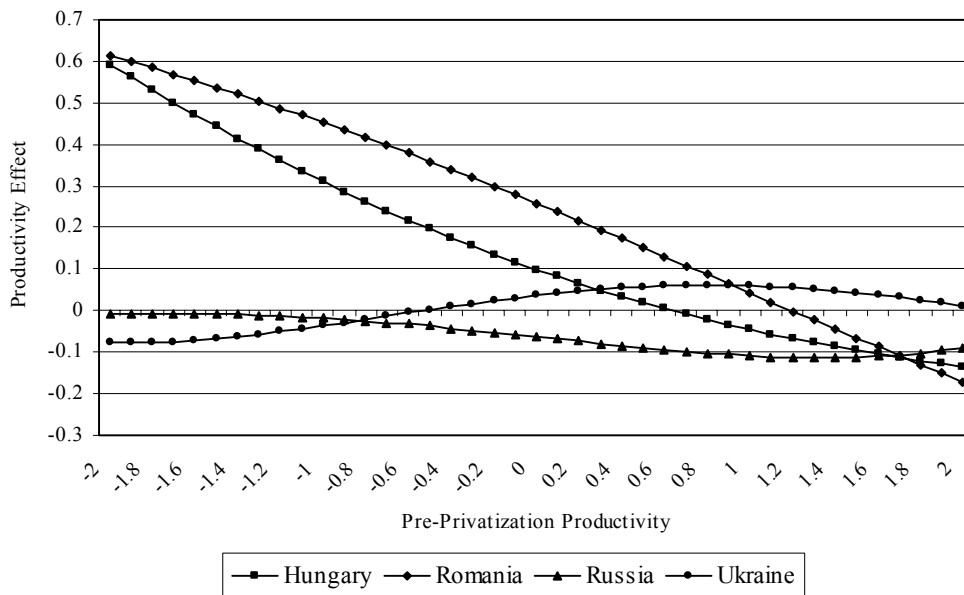


Figure 5: Variation in Domestic Privatization Effect by Cohort Year

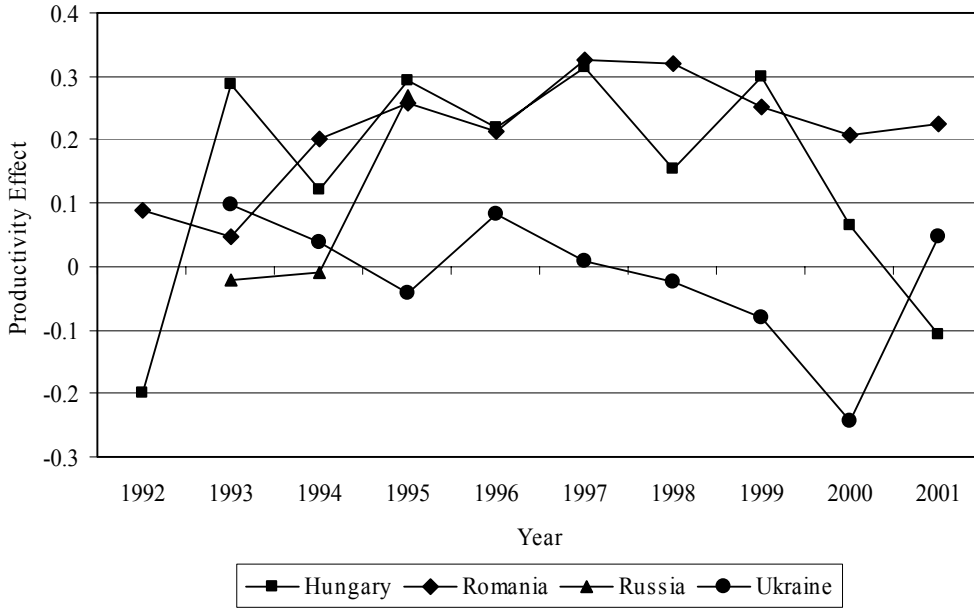
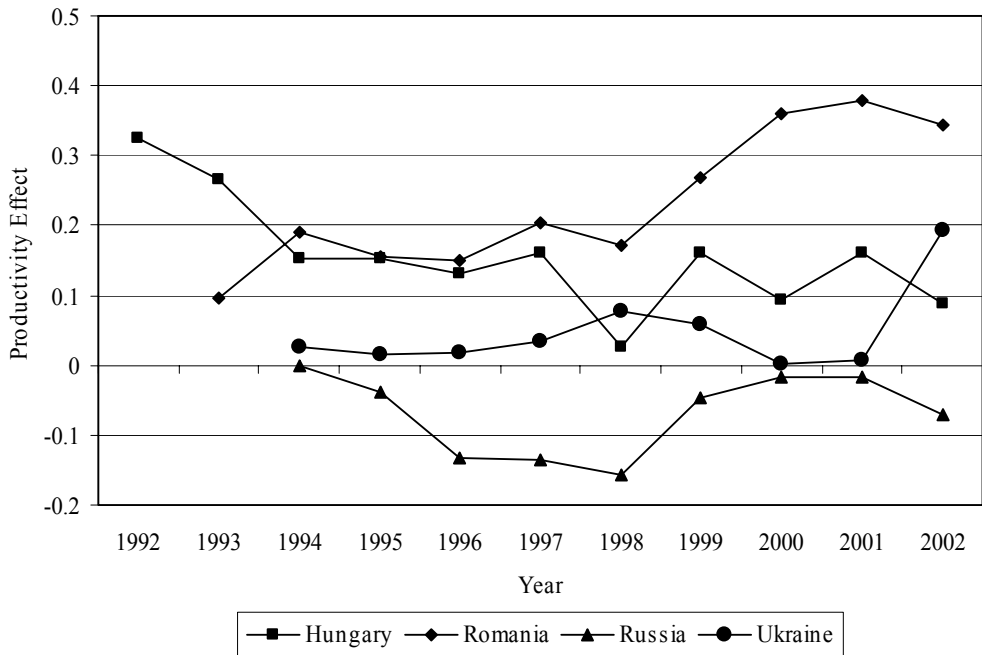


Figure 6: Variation in Domestic Privatization Effect by Calendar Year



**Figure 7: Variation in Domestic Privatization Effect
by Industry Growth**



Table 1: Sample Sizes, 1994

	Number of firms	Percent of all old firms	Total employment	Percent of old firm employment
Hungary	1,965	92.5	411,606	98.6
Romania	1,938	87.5	2,401,875	96.0
Russia	14,630	94.1	10,310,400	97.2
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 Employment, Capital, and Output, 1986, 1994, and 2002

	<i>Employment</i>			<i>Capital</i>			<i>Output</i>		
	1986	1994	2002	1986	1994	2002	1986	1994	2002
Hungary	636 (1,286)	209 (625)	166 (416)	1,492 (5,295)	1,359 (20,593)	1,077 (10,011)	4,322 (1,075)	2,282 (24,551)	3,591 (37,504)
Romania	NA	1,002 (2,321)	418 (931)	NA	422 (1,773)	115 (683)	NA	399 (1,756)	204 (1,137)
Russia	771 (2,595)	705 (2,257)	514 (2,041)	101 (5,536)	127 (7,051)	160 (1,935)	352 (1,293)	220 (1,356)	214 (1,672)
Ukraine	NA	584 (1,511)	469 (1,895)	NA	22 (105)	41 (240)	NA	21 (124)	31 (222)

Note: Capital and output are 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 for Hungary, the value of gross output for Romania, Russia and Ukraine. Capital equals average book value of tangible assets between beginning of current and next year for Hungary and Romania, imputed for missing values as the predicted value of the average capital from a regression on current year capital, year and industry dummies. Capital equals average book value of fixed assets used in the main activity of the enterprise, adjusted for revaluations of year values in Russia and Ukraine. Employment equals the average number of registered employees for Hungary and Romania, and average number of registered industrial production personnel in Russia and Ukraine; this measure includes non-production workers, but excludes “nonindustrial” employees who mainly provide employee benefits. Standard deviations are shown in parentheses. NA = not available.

Table 3: Summary of Country Characteristics

	Hungary	Romania	Russia	Ukraine
Privatization Policy				
Main methods ¹	Sales	Mixed	Insider/mass	Insider
Speed ¹	Fast	Moderate	Fast	Slow
EBRD Score for Large-Scale Privatization in 1995 ²	4.0	2.7	3.0	2.0
Overall Reform Evaluations				
World Bank Grouping (1996) ³	1 (advanced)	2 (second)	3 (lagging)	4 (way behind)
EBRD Average Score for Progress in Transition ⁴				
1994	3.13	2.29	2.43	1.46
2000	3.74	2.84	2.54	2.54
Policy and Business Environment⁵				
Government Effectiveness	0.78	-0.33	-0.40	-0.74
Regulatory Quality	1.21	0.04	-0.30	-0.62
Rule of Law	0.90	-0.12	-0.78	-0.79
Macroeconomic Environment⁶				
Industrial Production (average annual growth, %)				
1990-1994	-4.70	-5.87	-15.37	-12.14
1994-1998	7.83	-1.98	-2.65	-4.72
1998-2002	9.73	2.11	7.81	9.09
PPI (average annual change, %)				
1990-1994	29.76	177.26	491.48	971.23
1994-1998	32.73	67.19	46.58	195.99
1998-2002	21.95	48.77	33.61	10.82

Note: ¹ See discussion in Section 3. ² EBRD (1995). ³ World Bank (1996). ⁴ EBRD (1994, 2000). ⁵ Kaufmann, Kraay, and Mastruzzi (2003). ⁶ Statistical yearbooks for each country, various years.

Table 4: Percentage of Sample Firms Privatized—Total, Foreign, and Domestic

	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002
Hungary											
<i>Private</i>	43.0	68.3	79.5	86.8	91.2	94.2	94.7	94.7	95.7	95.7	96.1
<i>Foreign</i>	9.2	11.8	13.4	15.6	16.7	17.9	18.4	19.1	20.1	20.8	21.6
<i>Domestic</i>	33.8	56.5	66.1	71.2	74.5	76.3	76.3	75.6	75.6	74.9	74.5
Romania											
<i>Private</i>	0.2	3.0	8.0	20.3	42.3	46.8	56.1	70.6	79.1	86.4	90.6
<i>Foreign</i>	0.0	0.1	0.2	0.2	0.4	1.1	2.8	4.2	5.2	5.6	5.9
<i>Domestic</i>	0.2	2.9	7.8	20.1	41.9	45.7	53.3	66.4	73.9	80.8	84.7
Russia											
<i>Private</i>	0.0	49.6	84.9	82.7	75.4	75.4	73.9	70.6	69.5	70.2	73.9
<i>Foreign</i>	0.0	0.2	0.4	0.2	0.2	0.5	0.6	0.5	0.6	1.2	1.0
<i>Domestic</i>	0.0	49.4	84.5	82.5	75.2	74.9	73.3	70.1	68.9	69.0	72.9
Ukraine											
<i>Private</i>	0.0	0.2	8.2	17.7	27.8	45.1	57.2	65.5	68.4	84.2	77.1
<i>Foreign</i>	0.0	0.0	0.1	0.1	0.2	0.6	0.8	0.9	1.0	1.4	0.8
<i>Domestic</i>	0.0	0.2	8.1	17.6	27.6	44.5	56.4	64.6	67.4	82.8	76.3

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 5: Estimated Productivity Effects of Privatization

	Hungary	Romania	Russia	Ukraine
CD-OLS				
$\hat{\delta}$	0.222** (0.023)	0.250** (0.024)	-0.047** (0.016)	0.044* (0.019)
R ²	0.649	0.693	0.707	0.600
N	21,977	21,461	213,447	57,600
CD-IV				
$\hat{\delta}$	0.140** (0.015)	0.241** (0.016)	-0.041** (0.008)	0.030* (0.013)
R ²	0.662	0.694	0.701	0.519
N	19,118	18,154	184,353	48,054
Olley-Pakes				
$\hat{\delta}$	0.154** (0.031)	0.200** (0.025)	-0.070** (0.016)	0.035 (0.019)
R ²	0.674	0.703	0.702	0.521
N	19,118	18,154	184,353	48,054
Assuming $\theta = 1.0$				
$\hat{\delta}$	0.214** (0.024)	0.301** (0.026)	-0.060** (0.017)	0.058** (0.020)
R ²	0.804	0.671	0.772	0.706
N	21,977	21,461	213,447	57,600
Assuming $\theta = 0.7$				
$\hat{\delta}$	0.243** (0.024)	0.318** (0.028)	-0.041* (0.018)	0.046* (0.020)
R ²	0.737	0.631	0.745	0.698
N	21,977	21,461	213,447	57,600

Note: Coefficients (standard errors) are shown for Private (= 1 if the firm is majority private at the end of year $t-1$). Firm fixed effects and full sets of unrestricted industry-year dummies are included in all regressions. CD=Cobb-Douglas; θ = assumed labor share; see text for other explanations. Standard errors are adjusted for clustering on firms. * = significant at 5-percent level. ** = significant at 1-percent level.

Table 6: Estimated Effects of Foreign and Domestic Privatization

	Hungary	Romania	Russia	Ukraine
CD-OLS				
$\hat{\delta}_f$	0.529** (0.050)	0.400** (0.094)	0.400** (0.154)	0.408** (0.158)
$\hat{\delta}_d$	0.187** (0.023)	0.241** (0.024)	-0.048** (0.016)	0.044* (0.020)
N	21,976	21,461	213,447	56,892
CD-IV				
$\hat{\delta}_f$	0.369** (0.030)	0.365** (0.045)	0.414** (0.049)	0.453** (0.072)
$\hat{\delta}_d$	0.116** (0.016)	0.233** (0.017)	-0.043** (0.008)	0.032** (0.013)
N	19,118	18,154	184,353	46,705
Olley-Pakes				
$\hat{\delta}_f$	0.399** (0.054)	0.268** (0.093)	0.409** (0.162)	0.453** (0.161)
$\hat{\delta}_d$	0.117** (0.030)	0.195** (0.025)	-0.072** (0.016)	0.038* (0.020)
N	19,118	18,154	184,353	46,705
Assuming $\theta = 1.0$				
$\hat{\delta}_f$	0.545** (0.053)	0.460** (0.094)	0.436** (0.161)	0.482** (0.158)
$\hat{\delta}_d$	0.179** (0.024)	0.290** (0.026)	-0.061** (0.017)	0.057** (0.020)
N	21,976	21,461	213,447	56,892
Assuming $\theta = 0.7$				
$\hat{\delta}_f$	0.513** (0.048)	0.433** (0.095)	0.390* (0.161)	0.446** (0.164)
$\hat{\delta}_d$	0.215** (0.024)	0.310** (0.028)	-0.043 (0.018)	0.047* (0.021)
N	21,976	21,461	213,447	57,600

Note: Coefficients (standard errors) are shown for Foreign (= 1 if the majority of the firm's shares are owned by foreigners in year $t-1$) and Domestic (= 1 if the firm was private in year $t-1$ but not majority-owned by foreigners). Firm fixed effects and full sets of unrestricted industry-year dummies are included in all regressions. CD=Cobb-Douglas; θ =assumed labor share; see text for other explanations. Standard errors are adjusted for clustering on firms. * = significant at 5-percent level. ** = significant at 1-percent level.

Table 7: Pre-Privatization Relative Productivity

	Hungary	Romania	Russia	Ukraine
<i>Ever Private</i>	0.161** (0.043)	0.322** (0.044)	0.185** (0.014)	0.108** (0.026)
<i>Ever Foreign</i>	0.334** (0.056)	0.430** (0.079)	0.292** (0.105)	0.271* (0.117)
<i>Ever Domestic</i>	0.117** (0.041)	0.309** (0.044)	0.181** (0.014)	0.110** (0.026)

Note: The pre-privatization productivity of firms subsequently privatized relative to enterprises always in state ownership is estimated as the coefficient on a group effect, *Ever Private*, in a production function including *Private*, capital, labor, and industry-year interactions. The *Ever Foreign* and *Ever Domestic* effects are estimated analogously in an equation disaggregating *Private* into *Foreign* and *Domestic*. ** = significant at the 1 percent level.

**Table 8: Counterfactual Domestic Privatization Effects
Assuming Other Countries' Industrial Structures**

	<u>Assumed Industrial Structure</u>			
	Hungary	Romania	Russia	Ukraine
Hungary	0.182	0.197	0.203	0.207
Romania	0.223	0.239	0.263	0.262
Russia	-0.062	-0.024	-0.038	-0.031
Ukraine	0.045	0.049	0.039	0.044

Note: The assumed industrial structure comes from the countries listed in the top row and the effects of domestic privatization are calculated for countries in the left-hand column.

Table A1: Estimated Marginal Products, by Industry

Industry	Hungary				Romania				Russia				Ukraine			
	CD-OLS	CD-IV	OP	Translog	CD-OLS	CD-IV	OP	Translog	CD-OLS	CD-IV	OP	Translog	CD-OLS	CD-IV	OP	Translog
Capital																
1	0.096	0.425	0.141	0.164	0.090	0.053	0.115	0.088	0.034	0.004	0.040	0.061	0.046	-0.021	0.034	0.017
2	0.017	0.037	0.070	0.008	0.150	0.096	0.170	0.145	0.005	0.050	0.037	-0.006	0.209	0.074	0.221	0.127
3	0.130	0.209	0.254	0.133	0.124	0.252	0.206	0.058	0.025	0.012	0.032	0.042	0.261	0.219	0.210	0.255
4	0.208	0.362	0.281	0.103	0.102	0.144	0.149	0.076	0.083	0.105	0.121	0.059	0.186	0.105	0.126	0.146
5	0.086	0.150	0.130	0.106	0.035	0.110	0.137	0.053	0.075	0.059	0.079	0.063	0.084	0.038	0.079	0.083
6	0.133	0.132	0.219	0.157	0.119	0.160	0.240	0.129	0.133	0.159	0.165	0.123	0.031	0.114	0.119	0.090
7	0.102	0.403	0.133	0.154	0.051	0.090	0.104	0.004	0.029	0.021	0.022	0.030	0.155	0.219	0.194	0.089
8	0.099	0.241	0.181	0.137	0.101	0.084	0.186	0.085	0.044	0.042	0.071	0.090	0.108	0.094	0.080	0.121
9	0.073	0.165	0.141	0.046	0.101	0.037	0.132	0.087	0.004	0.000	0.011	0.007	0.094	0.094	0.089	0.048
10	0.145	0.315	0.222	0.151	0.105	-0.103	0.215	0.167	-0.011	-0.032	-0.022	-0.010	0.042	0.074	0.108	0.111
Labor																
1	1.108	0.850	1.041	0.964	1.388	1.436	1.375	1.350	1.357	1.471	1.351	1.343	1.444	1.448	1.456	1.645
2	0.838	0.803	0.754	0.867	1.066	1.208	1.039	1.110	1.355	1.286	1.373	1.337	1.122	0.969	1.051	1.263
3	0.996	1.025	0.935	1.010	0.762	0.726	0.721	0.938	1.105	1.098	1.119	1.094	1.107	0.901	1.046	1.150
4	0.847	0.678	0.739	0.904	1.086	1.096	0.981	1.169	1.403	1.495	1.424	1.433	1.194	1.132	1.196	1.335
5	0.904	0.888	0.795	0.856	0.692	0.565	0.519	0.696	0.939	1.116	0.872	0.966	1.035	1.121	0.938	0.997
6	0.887	0.899	0.802	0.929	1.220	1.306	1.115	1.329	0.998	1.059	0.964	1.294	1.418	1.331	1.459	1.380
7	0.987	0.634	0.886	0.938	0.933	0.899	0.844	0.979	1.378	1.389	1.379	1.338	1.472	1.492	1.413	1.773
8	0.875	0.849	0.824	0.872	1.153	1.158	1.061	1.183	1.236	1.343	1.225	1.199	1.366	1.188	1.425	1.277
9	0.803	0.785	0.749	0.823	0.842	0.860	0.763	0.902	1.341	1.447	1.370	1.361	1.190	1.031	1.159	1.369
10	0.779	0.710	0.687	0.786	0.886	0.590	0.606	0.639	1.248	1.149	1.244	1.241	1.334	1.123	1.302	1.421

Note: 1=Food, Beverages, and Tobacco; 2=Textiles; 3=Apparel, Fur, and Leather; 4=Wood, Pulp, Paper, Furniture, and Manufacturing NEC; 5=Publishing, Printing, and Recorded Media; 6= Fuels, Chemicals, Rubber, and Plastic; 7=Other Non-Metallic Mineral Products; 8=Basic Metals, and Fabricated Metals; 9=Machinery and Equipment NEC; 10=Electrical and Optical Equipment.

Table A2: Alternative Estimates of Privatization Effects

	Hungary	Romania	Russia	Ukraine
Cobb-Douglas imposing CRS				
$\hat{\delta}$	0.204** (0.022)	0.292** (0.025)	-0.060** (0.017)	0.061** (0.020)
$\hat{\delta}_f$	0.535** (0.048)	0.452** (0.095)	0.437** (0.161)	0.493** (0.158)
$\hat{\delta}_d$	0.168** (0.023)	0.281** (0.025)	-0.061** (0.017)	0.062** (0.020)
Translog				
$\hat{\delta}$	0.218** (0.023)	0.245** (0.024)	-0.046** (0.016)	0.052** (0.019)
$\hat{\delta}_f$	0.518** (0.049)	0.394** (0.088)	0.403** (0.154)	0.396** (0.151)
$\hat{\delta}_d$	0.186** (0.023)	0.234** (0.024)	-0.047** (0.016)	0.051** (0.019)
Assuming $\theta = 0.5$				
$\hat{\delta}$	0.262** (0.026)	0.329** (0.030)	-0.029 (0.019)	0.039 (0.021)
$\hat{\delta}_f$	0.492** (0.051)	0.414** (0.099)	0.359* (0.164)	0.422** (0.170)
$\hat{\delta}_d$	0.239** (0.027)	0.323** (0.031)	-0.031 (0.019)	0.039 (0.022)
Assuming $\theta = 0.3$				
$\hat{\delta}$	0.282** (0.031)	0.340** (0.034)	-0.016 (0.021)	0.031 (0.022)
$\hat{\delta}_f$	0.471** (0.058)	0.396** (0.107)	0.329* (0.170)	0.397* (0.176)
$\hat{\delta}_d$	0.263** (0.032)	0.337** (0.034)	-0.019 (0.021)	0.032 (0.032)
Assuming $\theta = 0.0$				
$\hat{\delta}$	0.310** (0.040)	0.357** (0.040)	0.002 (0.025)	0.019 (0.024)
$\hat{\delta}_f$	0.439** (0.073)	0.369** (0.123)	0.282 (0.183)	0.359 (0.189)
$\hat{\delta}_d$	0.299** (0.041)	0.356** (0.041)	-0.001 (0.025)	0.023 (0.025)

Note: ** = significant at 1-percent level. * = significant at 5-percent level. Standard Errors are shown in parentheses.

Table A3: Effect of Domestic Privatization, by Industry

Industry	Hungary				Romania				Russia				Ukraine			
	CD-OLS	CD-IV	OP	Translog	CD-OLS	CD-IV	OP	Translog	CD-OLS	CD-IV	OP	Translog	CD-OLS	CD-IV	OP	Translog
1	0.235**	0.154**	0.174**	0.235**	0.402**	0.371**	0.327**	0.382**	-0.002	-0.006	-0.031	-0.006	0.010	-0.004	-0.001	0.011
2	0.176*	0.112	0.107	0.181*	0.257**	0.234**	0.216*	0.250**	0.111	0.091**	0.051	0.130*	0.163	0.091	0.097	0.161
3	0.065	-0.017	-0.006	0.062	0.066	0.107	0.110	0.071	-0.275**	-0.279**	-0.328**	-0.290**	0.011	0.004	0.003	0.010
4	0.200**	0.116**	0.123*	0.193**	0.256**	0.275**	0.255**	0.240**	-0.240**	-0.222**	-0.273**	-0.234**	-0.123	-0.099*	-0.086	-0.088
5	0.219*	0.184**	0.161	0.188*	0.316*	0.410**	0.407*	0.335*	-0.036	-0.057	-0.088	-0.021	0.040	0.030	0.033	0.052
6	0.076	0.059	0.038	0.077	0.209**	0.147*	0.099	0.143	0.009	-0.021	-0.054	0.023	0.235*	0.231**	0.233*	0.207*
7	0.247**	0.149*	0.248*	0.233**	0.096*	0.095	0.079	0.113*	0.113**	0.111**	0.084*	0.099**	0.158**	0.115**	0.126**	0.190**
8	0.238**	0.196**	0.194**	0.242**	0.226**	0.253**	0.197**	0.231**	0.193*	0.162**	0.140	0.198*	0.006	-0.017	-0.018	0.009
9	0.225**	0.127**	0.133*	0.220**	0.131**	0.121**	0.077*	0.140**	-0.095*	-0.046	-0.068	-0.090*	0.051	0.055	0.058	0.065
10	0.150	0.038	0.036	0.160*	0.200*	0.151	0.050	0.229*	-0.168*	-0.148**	-0.173*	-0.171**	0.088	0.096	0.099	0.082

Note: 1=Food, Beverages, and Tobacco; 2=Textiles; 3=Apparel, Fur, and Leather; 4=Wood, Pulp, Paper, Furniture, and Manufacturing NEC; 5=Publishing, Printing, and Recorded Media; 6=_fuels, Chemicals, Rubber, and Plastic; 7=Other Non-Metallic Mineral Products; 8=Basic Metals, and Fabricated Metals; 9=Machinery and Equipment NEC; 10=Electrical and Optical Equipment. ** = significant at 1-percent level. * = significant at 5-percent level.