Privatization*

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Abstract

In this chapter, we replicate, update, and extend our earlier work on manufacturing enterprise privatization and productivity in Russia. Our results suggest a more nuanced view of Russian privatization than that offered by either its critics or its defenders. We confirm earlier findings that the average impact on productivity of privatization to domestic owners is around -3 to -5 percent, though some regions show productivity gains similar to those in Central Europe (an increase of 10 to 20 percent). The regional variation is strongly positively associated with the size of the regional bureaucracy. Notwithstanding the average negative effect, our updated results through 2005 (the most recent year for which comparable data are available) show a pronounced change after 2002 as the productivity effects of Russian privatization have begun to approach those seen elsewhere much earlier. Privatization became most effective west of the Urals, in areas with greater market access. Initially an outlier, by 2005 Russia appeared to be becoming more of a normal country, at least in the narrow sense of the impact of private ownership on firm productivity.

1 Introduction

Of all the contentious aspects of Russia's "transition," perhaps none has been as controversial as the privatization of industrial enterprises. Extreme positions on Russian privatization were staked out early on, in some cases even while the program was still being designed and in many cases before a significant number of firms had even been privatized. In one corner were those claiming the Russian privatization policies to be purely cosmetic: at best meaningless, at worst contributing to the "criminalization" of the Russian economy (e.g., Arrow et al., 1996; Black, Kraakman and Tarassova, 2000; Freedland, 2000; Goldman, 1997; Intriligator, 1994; Miller and Tenev, 2007; Roland, 2001; Stiglitz, 2000). Besides the blatant corruption

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associated with a number of transactions, one aspect of the program singled out for particular criticism was the massive advantages given to enterprise managers and other employees in acquiring shares in their employers. Although Russia was hardly alone among transition economies in the degree of insider participation (Blasi, Kroumova and Kruse, 1997; Earle and Estrin, 1996), a program that so actively facilitated inside ownership and discouraged the entry of outsiders hardly seemed the cure that the doctor would have ordered, given the need for extensive and painful restructuring of most industrial enterprises.

From the other corner, the Russian program has been defended as the best that was politically and administratively feasible, or even hailed as an outright success (Åslund, 1995, 2007; Blanchard et al., 1993; Boycko, Shleifer and Vishny, 1995; Shleifer and Treisman, 2000; Lieberman, 2008). Under the conditions of the continued Communist domination of the Duma and the lack of an organized lobby for outsider privatization through sales to core investors or a Czech-style voucher scheme (McFaul, 1995), the only alternative to continued state ownership may have been ceding majority control to insiders, as the program at least initially did. According to this view, the program at least opened the way to improved corporate governance and restructuring by "depoliticizing" the enterprise-state relationship, even under insider ownership, while creating some possibilities for outside investors to enter.[1]

Systematic analysis of Russian privatization, or indeed any privatization in the transition economies, has greatly lagged these judgments.[2] There was little relevant previous experience to support the early pro-privatization enthusiasm, and there had been little systematic research by the late 1990s to corroborate the negative views of the critics. What evidence was available was limited to either macroeconomic performance indicators or detailed observations of a few firms. The first surge of statistical studies of privatized firms appeared just as the critics' position seemed to become dominant around 2000. As summarized in reviews by Megginson and Netter (2001) and Djankov and Murrell (2002), these studies tended to find positive effects of privatization on measures of firm performance in many countries, including Russia. But the research suffered from methodological weaknesses, and it was either ignored or ineffective in persuading most skeptics.

The major weakness in these early studies stems from data that were generally based on small samples of enterprises observed for short periods of time, in some cases not much more than a collection of case studies and usually with a great deal of missing information. To pick an example from a study in which one of us was involved, Earle and Estrin (1997) analyze a survey of 439 Russian enterprises, but most of their regressions contain little more than 200 firms with information for only two points in time: 1990 and the first quarter of 1994, when privatization was still underway.[3] The small sample in this and other studies greatly limits the use of econometric methods that can identify a privatization effect distinct from heterogeneity or selection bias in the privatization process. For instance, firms

[1]International views of the program seem to have seesawed over time, from relatively approving during most of the 1990s (the EBRD raised its index of large-scale privatization rapidly to 3.0 in 1993—the top rating of any transition economy that year, where a score of 1.0 indicates central planning and 4.3 indicates a complete transition—and further to 3.3 in 1997), to sharply negative as the "Washington consensus" (Rodrik, 2006) evaporated after about 2000.

[2]Some of our review of the literature follows Brown, Earle and Gehlbach (2009).

[3]Other early surveys of privatized firms in Russia include Webster et al. (1994), Pistor (1995), Earle, Estrin and Leshchenko (1996), and Radygin (1996).

in certain industries may be more likely to be privatized than those in others, but with small samples of firms from many industries, researchers cannot control for either detailed industry fixed effects or for time-varying patterns such as industry-specific shocks or trends. Moreover, with only a cross-section, it is impossible to identify changes to the private-state performance differential resulting from privatization. And with only one or two years of both pre- and post-privatization information (as in Earle and Estrin, 1997, and many other studies), the identifying variation is still very limited, with any estimates pertaining only to the very short run and conditioning on the particular pre- and post-privatization years available. Some studies used data only on privatized firms, thus failing to exploit the possibility of a state-enterprise comparison group that may have had its own evolution over time. Finally, many analyzed various outcomes only loosely connected to firm performance or restructuring (e.g., sales, wage arrears, debt default, and a number of qualitative indicators based on managers' reports on whether they have restructured, introduced new products, had successful transactions, etc.).

We have grappled with these difficulties in previous work, gradually building up much larger databases that cover near-universes of manufacturing enterprises over a long time period. Focusing on multi-factor productivity as arguably the most appropriate measure of firm performance, our previous research exploits the cross-sectional coverage and size of the data by estimating privatization effects within industry-year cells, thus controlling for any heterogeneity across industries and any industry-level shocks or mis-measurements (for instance, in prices). It also takes advantage of the longitudinal dimension of the data to include not only firm fixed effects, but also firm-specific trends that permit each firm to have its own productivity growth rate, such that privatization effects refer to deviations from these trends. The long time series allow us to estimate privatization effects conditional on more than just one pre-privatization year, and they also facilitate the examination of longer-term effects of privatization.

Brown, Earle, and Telegdy (2006), hereafter BET (2006), was the first paper to use these data and methods. The results reported there raise some puzzles about the effects of privatization in Russia, as compared to Hungary, Romania, and Ukraine. The broad pattern supports the conclusion of the earlier survey by Djankov and Murrell (2002) that the effect of privatization is considerably stronger in Eastern Europe than in the CIS. Moreover, as many had suspected, BET (2006) report estimated productivity effects of privatization to foreign investors that are uniformly positive and usually large (generally 20–40 percent, depending on whether the specification allows for firm-specific time trends or merely includes firm fixed effects). However, the estimated effects of privatization to domestic owners are largest in Romania (14–24 percent), followed by Hungary (5–15 percent) and Ukraine (2–4 percent), and they are actually negative for Russia, where although small in magnitude (-3 to -5 percent), they are always statistically significantly different from zero. Analysis of the dynamics of these effects raises further questions. Whereas in Hungary, Romania, and Ukraine the impact of privatization tended to be fairly immediate, i.e., in the first post-privatization year (with a slight lag in Ukraine), in Russia the initial effect was sharply negative and increasingly worse through the first four post-privatization years, with a modest positive impact appearing only after about seven years.

More recent work (Brown, Earle and Telegdy, 2010) has raised some new puzzles in the cross-country comparison of privatization effects on employment and wages. In the same

four countries, we find that the estimated employment and wage effects of privatization are small, lying between -3 percent and +3 percent, and frequently indistinguishable from zero. But the data again suggest Russia is a bit of an outlier, with some evidence for a positive employment effect (about 5 percent) and a somewhat stronger negative wage effect (about -4 percent).

In Brown, Earle, and Gehlbach (2009), hereafter BEG (2009), we investigate regional variation in the estimated productivity effect of privatization in Russia. When the privatization effect is permitted to vary by oblast, the estimates display substantial variation, with the estimates in many oblasts similar to our results for Hungary and Romania. We examine various explanations for the cross-sectional variation in privatization effectiveness. Relative to cross-country comparisons, a key advantage of this approach is the ability to hold constant privatization design and the general macroeconomic environment to a degree not possible in multi-country studies. The primary finding that emerges from this analysis is a strong, robust association of privatization effectiveness with the size of the regional bureaucracy, which varies for reasons that appear to be exogenous to the economic transition. This result could be interpreted as reflecting either increased capacity of helpful bureaucrats or greater competition among corrupt bureaucrats in regions where bureaucrats are relatively numerous.

In this chapter, we replicate, update, and extend our earlier work on manufacturing enterprise privatization and productivity in Russia. Our replication sets the groundwork for the rest of the chapter, and we use new—and we believe, improved—versions of the data. Our updating uses data beginning in 1985 (as before) and running through 2005, three years more than in BET (2006). Unfortunately, 2005 is the last year for which somewhat comparable data are available, and the 2005 database is already different in some serious ways from the earlier time series, as we discuss below. We therefore limit most of the analysis to data through 2004, though we include data from 2005 when discussing dynamics, given some intriguing developments in the last years of our sample.

We extend our previous work in two broad directions. The first is to consider some additional issues at the aggregate Russian level, including the calendar-year dynamics of the privatization effect for both domestic and foreign privatization and differences in the effect associated with 100-percent versus partial privatization. The second is to extend our analysis of regional variation in privatization effectiveness, including changes in regional patterns that are evident in the updated data.

We should be clear about the many topics this chapter does not cover. For data availability reasons, we examine only manufacturing firms, with no attention to the service or agricultural sectors or to other types of assets, including land and housing, which would also involve a different set of issues. Our focus remains on the multi-factor productivity effects of privatization, with only passing reference to employment and wages, and no attention to a variety of other possible outcomes one might investigate. Another serious omission is any direct analysis of different methods of privatization: vouchers vs. cash, insiders vs. outsiders, and the mixtures of methods that varied across firms. Again, the reason for this omission is our lack of reliable firm-level data on privatization method and ownership structure—aside from the foreign vs. domestic and 100-percent vs. partial distinctions—rather than from any lack of interest. We are also unable to follow the evolution of the ownership structure after privatization, for instance as employees sold shares to outsiders, and our analysis instead

focuses on the impact of the initial change in ownership from 100 percent state to (in most cases) majority private.

Further, because nearly all Russian privatization in manufacturing happened astonishingly quickly, in 1993–94, our estimates are identified almost entirely from this early phase of the process, and the subsequent sales in the later 1990s provide little additional information for our estimates. Our data contain few lease buyouts from the *perestroika* period, at least ones that we can identify clearly, although this was an important phase in Russia's privatization (Earle and Estrin, 1997). We also have nothing to say about the dozen or so firms privatized through "loans for shares," which have attracted much more attention than the 16,000-odd firms in the mass privatization, with the latter frequently indicted for the abuses associated with the former. We do not discuss the broader economic effects of privatization, for instance any externalities for the business environment, entrepreneurship and start-ups, or economic inequality. Finally, while much of the debate over Russian privatization has concerned political-economy issues such as the usefulness of privatization in making the transition from socialism irreversible, the relationship with democratization, or the role of privatization policies in broader transition strategies, we focus entirely on the direct, measurable effects of privatization on firm performance.

2 Data and Methods

Our analysis in this chapter relies upon updated and revised versions of the data sets we have used in our earlier research described above. The source for the firm-level data we use to estimate the productivity effects of privatization is the Federal State Statistics Service (Rosstat), the Russian successor to the corresponding Soviet agency (Goskomstat). The basic statistical methodologies and data-collection procedures have remained unchanged throughout this period. Our basic data source is industrial-enterprise registries, which we supplement with joint-venture registry data and balance-sheet data.

The key aspects of these firm-level data are the coverage and size of the cross-section and the length of the time series. The data include every firm meeting certain criteria (listed below) rather than coming from a sample survey, such as those used in earlier research on privatization in transition economies. Universal coverage means there are no concerns about sample selection, which might otherwise be biased towards firms willing to answer survey questionnaires. Because of the large number of observations, resulting from universal coverage and from the size of Russia, we can control for industry-specific shocks, thus removing a potentially large source of heterogeneity that plagued previous studies, and we are also able to estimate separate privatization effects by region and correlate them with regional characteristics (described in Section 4 below).

The length of the time series is equally important. The 20-year span from 1985 to 2004 (or 2005 in some of our analysis) permits us to track the productivity performance of a firm, relative to its peers in the same industry and time period, for at least several years before and after it is privatized. It also enables us to examine changes over time in the estimated productivity effects of privatization, and the relationships between regional privatization effectiveness and other time-varying regional characteristics (also described in Section 4 below).

The industrial registry coverage is defined by the Federal State Statistics Service as including 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. The actual practice seems to be that once firms enter the registries, they continue to report even when the original conditions for inclusion are no longer satisfied. The data may therefore be taken as comprehensive for the "old" sector of firms (and their successors) inherited from the Soviet system. The firms in the Russian industrial registry accounted for 91 percent of officially reported total industrial employment at the beginning of the transition process in 1992.

We exclude non-profit organizations and non-manufacturing sectors . Focusing on the effects of privatization with a relatively homogeneous comparison group, we also include only firms that are state-owned on entry into the database. Finally, we retain only those firm-years containing complete information, which has little effect on sample size. The resulting sample of initially state-owned firms, some of which were then privatized, contains 283,250 firm-year observations over the 1985–2004 period for 26,521 enterprises. In some analyses, we also make use of a file with information for 2005, which we have linked to the earlier data. This file is much smaller and lacks information on end-of-year capital stock; in this case we therefore impute a constant within-industry evolution of the capital stock from its end-2004 level for each firm, as explained further in Section 3 below.

Besides the addition of recent years, this database differs in some ways from those we studied in earlier papers. Most important are several aspects of the data cleaning. In this paper we remove extreme outliers, defined as follows: any variable x in year t for which x_t/x_{t-1} and x_t/x_{t+1} are both greater than 5, or any x_t when $x_t/x_{t+1} > 10$ and t is the entry year of the firm in the data, or any x_t when $x_t/x_{t+1} > 10$ and $t+1$ is the year of exit of the firm from the data. In addition, we have cleaned the ownership information to create consistent time series: missing values for ownership variable x_t are filled in when $x_{t-1} = x_{t+1}$, and no reversals of privatization status are permitted, so that whenever $x_{t-1} = x_{t+1}$, then x_t is set equal. Although we would not expect these changes to have large effects on the results, it is reassuring, as we report below, that in fact they do not.

Although the cross-section and longitudinal size of this database has important advantages over alternative sources (chiefly sample surveys), we hasten to list its shortcomings. First of all, the number of variables in the database is quite limited. We lack information on material costs and value-added, which would be valuable in estimating total factor productivity. As long as—conditional on labor and capital—material costs do not vary within an industry-year cell, this is not a problem; but if, for instance, privatized firms use more expensive materials compared to those of state-owned enterprises in the same industry-year and with the same employment and capital stock, then the productivity advantage associated with privatization would be biased upward.

We also lack detailed information on ownership, which as discussed in the introduction prevents us from estimating the productivity effects of different ownership structures, such as the extent of outside participation, the concentration of shareholdings, and the relative importance of managers and workers. Our data do, however, permit us to compare the effects of foreign versus domestic privatizations. They also distinguish two types of domestic privatizations: one resulting in mixed state-private ownership and the second resulting in 100-percent private. Because the nature of the privatization programs (Boycko, Shleifer and

Vishny, 1995; Frydman, Rapaczynski and Earle, 1993) as well as early survey evidence (e.g., Blasi, Kroumova and Kruse, 1997; Earle and Estrin, 1997) both strongly suggest that most privatizations involved a transfer of a majority of shares, our earlier research combined these two categories into a single category. In this chapter, we present some results with these categories disaggregated.

The data are also limited in coverage, as emphasized in Section 1, excluding all privatization outside the manufacturing sector. The criteria for inclusion in the registries, discussed above, imply that state-owned firms (literally, if the state or any other firm in the registry owns at least 25 percent) are covered regardless of size. But there may be some exclusion of small firms (those with fewer than 100 employees) once their ownership by the state or other registry entities falls below 25 percent, although our impression is that firms remain in the registry once they enter. For instance, the registries contain many 100-percent privately owned firms with fewer than 100 employees; those that were once state-owned contribute to the identification of privatization effects.

Our methods for estimating these effects also build on our earlier research. At the aggregate level, that is when we estimate a single average effect of privatization on firm-level productivity, we follow the basic methods of BET (2006). At the regional level, when permitting the productivity effect of privatization to vary across regions, we rely on BEG (2009).

Our estimating equation for multi-factor productivity in the aggregate analysis is

$$x_{jt} = \mathbf{f}\left(k_{jt}, l_{jt}\right) + \mathbf{Y}\boldsymbol{\gamma} + \mathbf{w}_t\boldsymbol{\alpha}_j + F_{jt}\phi + D_{jt}\delta + \eta_{jt}, \tag{1}$$

where j indexes firms and t indexes 20 time periods (years 1985 to 2004). The variable x_{jt} is output, $\mathbf{f}$ is a 1×10 vector of industry-specific production functions, k_{jt} is capital stock, l_{jt} is employment, $\mathbf{Y}$ is a 1×200 vector of industry-year interaction dummies, $\boldsymbol{\gamma}$ is the associated 200×1 vector of coefficients, $\mathbf{w}_t$ is a vector of aggregate time variables, $\boldsymbol{\alpha}_j$ is the vector of associated individual-specific slopes, and F_{jt} is an indicator of whether the firm was foreign-owned at the end of year $t-1$ and ϕ the associated coefficient. The variable D_{jt} is an indicator for domestic private ownership at the end of year $t-1$, and δ is the coefficient of interest: the average productivity effect of domestic privatization. Finally, η_{jt} is an idiosyncratic error.

Regarding the functional form of $\mathbf{f}$, we again draw upon results from BET (2006), who show that estimated privatization effects are insensitive to changes in functional form (including translogs and a variety of assumed factor shares); therefore we use a simple Cobb-Douglas. However, we allow the estimated functions to vary across industries, and a full set of unrestricted industry-year interactions, the $\mathbf{Y}$, permits different productivity levels for each industry in each year, controlling for any time- and industry-varying factors such as unmeasured factors of production, price changes not captured by deflators, , and quality differences across industry-year cells.

The alternative specifications of $\mathbf{w}_t$ embody our methods of controlling for selection bias. The OLS model has $\mathbf{w}_t \equiv (0, 0)$, the FE model has $\mathbf{w}_t \equiv (1, 0)$, and the FE&FT model with firm fixed effects and firm-specific trends has $\mathbf{w}_t \equiv (1, t)$, so that $\boldsymbol{\alpha}_j \equiv (\alpha_{1j}, \alpha_{2j})$, where α_{1j} is a fixed unobserved effect and α_{2j} is the specific trend for firm j. We estimate the FE&FT model in two steps, detrending all variables for each firm separately before estimating the

model on the detrended data. We correct the standard errors for correlation of error terms across observations for the same firm.

Most of the equations we estimate in this chapter are extensions of this basic equation. In the aggregate analysis, the extensions include permitting the average domestic and foreign privatization effects δ and ϕ to vary by calendar year. In another extension, we permit the effect to vary with the domestic private share—whether 100 percent or less than 100 percent. Finally, in the regional analysis, we allow variation in the domestic privatization effect both across regions and over time, as discussed further in Section 4 below.

3 Aggregate Effects

We begin by replicating earlier results first reported in BET (2006). Table 1 shows the results from that earlier paper using three estimation methods (OLS, FE, and FE&FT, as described above) for four countries (Hungary, Romania, Russia, and Ukraine) in columns (1)-(4), and it adds for comparison our new estimates with the new database, including information through 2004 and incorporating some other refinements, as discussed above, in column (5). Before focusing on the new Russian estimates, it is worthwhile to emphasize some of the basic patterns in these results.

The OLS coefficients imply large positive correlations between private ownership and multifactor productivity within industry-year cells. The strong positive relationship in the OLS estimates holds for both types of private ownership—domestic and foreign—and for all four countries. But there is also significant variation in the magnitudes across type—with the foreign coefficient always substantially larger than the domestic—and across countries. In particular, the magnitude of the domestic privatization effect is largest in Romania, followed in order by Hungary, Ukraine, and Russia. Adding firm fixed effects (FE) reduces the estimated coefficients considerably, suggesting some combination of measurement error and selection bias in the privatization process, as higher productivity firms are more likely to be privatized. Adding firm-specific trends (FT) usually reduces the results further, consistent with a positive association between firm-level productivity growth and the probability of privatization. Although the magnitudes vary substantially across these specifications, some of the basic patterns remain consistent, particularly the tendency for a relatively large foreign privatization coefficient in all countries, and the cross-country variation in the domestic coefficient, with the same ranking in all specifications.

The new results for Russia, despite using an extended time series (through 2004) and somewhat different cleaning procedures compared with BET (2006), are nevertheless qualitatively quite similar. While the domestic coefficient is positive and statistically significant in the OLS specification, in both the FE and FE&FT models the estimate is negative and statistically significant. This result cannot be explained away as measurement error of the classical sort in the ownership variable, which would lead to attenuation bias rather than a statistically significant reversal of sign. Some attenuation is observed from the addition of firm-specific trends, but the difference is slight, especially in the new, extended data. The general implication of this evidence is that privatization produced an average fall in multifactor productivity of about 5 percent in privatized Russian firms, which is the basic puzzle we explore in this chapter.

A potentially important aspect of the analysis in BET (2006) is that their ownership measure pools together 100-percent private with mixed state-private firms. Their justification for pooling is to construct an approximate indicator for majority privatized, given earlier research (Earle and Estrin, 1997) showing that in nearly all Russian firms with any privatization, more than 50 percent of the shares were privatized. In addition, the goal in BET (2006) is to achieve comparability with the estimates from the other countries in the study, for which majority privatization indicators are computed. That said, the Russian data contain separate variables for 100 percent private and mixed state-private. We analyze these variables to check whether the smaller estimated Domestic coefficients in Russia might be explained by the inclusion of minority privatizations, so that any privatization effects would be diluted (relative to the other countries, where majority is clearly measured). Other purposes of this analysis include a general robustness check on the earlier results and a test of the degree to which a greater degree of privatization has a larger impact on productivity.

Estimates from regressions using the "Mixed" and "100%" variables, but otherwise the same data and methods as in Column (5) of Table 1, are shown in Table 2. The results for foreign ownership and other variables are very similar to those in Table 1. The FE and FE&FT estimates show the relative magnitudes in the expected order—that is, higher for 100-percent private than for mixed ownership following privatization. But the differences are very small and at best marginally significant. Most importantly, in both the FE and FE&FT specifications the estimated productivity effect of each ownership category is negative and statistically significantly different from zero. Thus, it does not appear that the overall negative coefficient of domestic privatization is substantially influenced by the degree of share transfer, and the negative coefficient that distinguishes Russia from the other countries is robust to this alternative specification of ownership.

One interpretation of the reduced productivity of privatized firms relative to state-owned enterprises in Russia is that the Russian state has been active in supporting its remaining assets.We thank Michael Alexeev for this point. If, for instance, the state supplies inputs at preferential prices, restricts competition, or provides direct subsidies to state-owned firms, these actions could result in higher output per units of labor and capital. In this case, the measured productivity of state firms would rise even if privatization was generating improved performance through better management and restructuring. Essentially, the outcomes for the control sample (state ownership) would be affected by the treatment (privatization), and the coefficients would not reflect the impact of treatment. Although this interpretation cannot be ruled out, it raises the question why the post-privatization private-state productivity gap is so different in Russia from Ukraine, where the state also maintained active support for its enterprises. And it also seems inconsistent with the strong effect of foreign privatization estimated in our data: one might expect an even greater attenuation of the foreign privatization effect, as the state is less likely to have supported foreign-owned firms than either state- or domestic private-owned firms.

Another set of interpretation and measurement issues revolves around the possible tunneling and hiding of cash flows in Russia. A well-known method for managers of all types of companies to extract rents and avoid taxes is to set up or spin off a trading company that functions as an intermediary with customers. Through transfer pricing, the output value of the original company appears to fall, resulting in lower measured productivity even in the absence of any change in technical efficiency. On the other hand, such tunneling is

also possible in state-owned enterprises, and indeed represents a kind of spontaneous privatization that provided a major impetus for rapid formal privatization policies, under the premise that new owners would monitor their firms to prevent asset-stripping of this and other types. Interpreting the estimated privatization coefficient in Russia along these lines also begs the question why this practice would be so much more prevalent among privatized firms in Russia compared to those in other countries, particularly Ukraine. In any case, the tunneling interpretation implies that privatization led to resource withdrawal in Russia to a much greater degree than in other countries, and the question remains why Russia would be so different.

A related concern is that the outcome variable productivity is mismeasured even taking firm boundaries as given. Zhuravskaya (2007) suggests that the estimated effect of privatization in Russia may be biased by a greater tendency for privatized relative to state-owned enterprises to hide their cash flow from the tax collectors. This hypothesis would require large differences in patterns of tax evasion across countries to account for the different estimated coefficients; for instance, if state firms all hide cash flow at the same rate, then privatized firms in Russia would have to hide 25 percent more revenue than in Romania to account for the estimated impact of privatization. Moreover, if hiding revenue is correlated with understating inputs, including capital purchases and numbers of employees, then the revenue hiding differential would have to be even greater.

The tax-evasion hypothesis is intrinsically difficult to test, since anything hidden for tax purposes is unlikely to be directly revealed in a survey. However, recent work attempts to assess the evidence by using a question on the Business Environment and Economic Policy Survey (BEEPS): "What percentage of the sales of a typical firm in your area of activity would you estimate is reported to the tax authorities, bearing in mind difficulties with complying with taxes and other regulations?" The phrasing of this question, while asking about a "typical firm in your area," is actually intended to elicit information about the respondent. Assuming it does so, or at least provides information about other firms with the same ownership type as the respondent, enables a comparison of the degree of revenue hiding across ownership types and countries.

Gehlbach (2006, 2008*a*) previously analyzed this variable, regressing self-reported tax compliance on ownership, employment, industry, number of competitors, whether a firm belongs to a business association, and country and town-size dummies. Consistent with the hypothesis in Zhuravskaya (2007), he finds that state-owned enterprises are typically more tax-compliant, but the estimated effect is small (1.9 percentage points in the CIS, 1.1 percentage points in Eastern Europe and the Baltics) and statistically insignificant. Focusing on the possible implications of tax evasion for estimates of privatization effectiveness, Brown, Earle and Telegdy (2011) report qualitatively similar results when restricting the sample to Russia, Hungary, Lithuania, Romania, and Ukraine: privatized firms tend to report less of their revenue compared to state-owned enterprises of the same industry and size, but the gaps are generally small. Moreover, the variation of these differences across countries does not support a role for tax evasion in accounting for the estimated productivity effects of privatization: In Hungary and Lithuania the privatized firms understate revenue 7 percent more than do the state firms, in Romania the difference is 1.3 percent, in Ukraine there is no difference, and in Russia the difference is 3 percent. Thus, the gap is actually bigger in Hungary and Lithuania, and the Romanian and Russian gaps are similar in size.

These results bolster the earlier findings of lower estimated productivity effects of privatization in Russia, relative to other transition economies where these effects can be estimated. A final question is whether the pattern has changed recently. To address this question, we re-estimate Equation 1, permitting the coefficients on D_{jt} and F_{jt} to vary by calendar-year. For this purpose, we stretch the data to cover 2005, assuming no variation within industries in capital-stock growth that year. The results, which are similar if we use labor productivity rather than multi-factor productivity as the outcome variable, are shown in Figure 1.

The first part of the top panel of Figure 1, the time-varying estimates of domestic privatization effects, is very similar to results in BET (2006); although the latter are in event time (normalized around the privatization year) rather than calendar time, in practice such a large fraction of Russian privatization happened so quickly (in 1993-1994) that calendar and event time are highly correlated. The productivity effect of domestic privatization is estimated at about zero in 1994 and 1995, but then it plunges in 1996 to about -0.15, statistically significantly different from zero. After remaining at about the same level through 1998, the estimates rise back to zero from 1999 to 2002, which was the last year of data available for the analysis in BET (2006).

With the additional years of information, we are able to track the further progress of these estimates, which as the top panel of Figure 1 shows, tend to continue to increase. The estimate for 2003 is already 0.1, statistically significantly different from zero, and it rises to about 0.2 in 2004 and 0.3 in 2005. While the upward trend in the privatization effects in other countries starts much earlier, as shown in BET (2006), the most recently available data suggest that Russia may have become somewhat less of an outlier, relative to other countries for which comparable estimates are possible.

For comparison, the bottom panel of Figure 1 shows analogous results for the foreign privatization effect. The basic shape of the estimated productivity effects mimics the domestic graph, but the estimated foreign effect is never statistically significantly less than zero, and it is always positive and statistically significant after 1997. From 1999, it hovers around 0.3–0.4, which is typical of the other transition economies studied by BET (2006). One interesting detail is the drop in 2005, which could be attributable to the change in the nature of the data, as discussed above, or to the worsening of the business environment for foreign investment associated with the Khodorkovsky trial and related developments. Together with the strong contemporaneous improvement in the domestic effect, there is essentially no difference in the estimated effects of privatization to domestic and foreign owners by 2005.

4 Regional Variation

The generally poor performance of Russian privatization relative to that in many other countries, as well as the strong improvement we have estimated a decade after program implementation, are striking examples of the large variation in privatization performance. What could account for this variation? In principle, a number of factors might be at work simultaneously, including program design, macroeconomic environment, the presence or absence of complementary public goods, and the underlying institutional environment. Understanding the determinants of privatization performance is important not only to our understanding of the postcommunist transition, but also to future reform efforts, in Russia and elsewhere.

Cross-country comparative analysis is one approach to analyzing these determinants, but the possibility to draw strong inferences from country-level results is constrained by both measurement and identification issues. Reliable estimates of privatization effectiveness are available only for the relatively few countries for which high-quality firm-level data are available. In addition, many plausible determinants of privatization effectiveness (e.g., institutional quality) are measured with considerable noise at the country level. Finally, in terms of identification, the transition setting implies simultaneous change along numerous dimensions, with substantial unobserved heterogeneity across countries.

One solution to these problems is to explore variation in privatization *within* countries. Russia provides a particularly good setting for this exercise, as both its size and federal structure imply substantial variation across regions along various dimensions—the obvious downside is that some factors of potential importance (e.g., macroeconomic environment) vary little across regions. The large number of firm-year observations in our data set allow for estimation of privatization effects at the region or even region-year level, though as we will show, the precision of these estimates is sensitive to the number of observations available for a given region, with implications for our econometric strategy.

To begin, we follow BEG (2009) in disaggregating the estimated productivity effect of domestic privatization into region-level effects; given the relatively small number of foreign privatizations in our data set, we assume a constant foreign-privatization effect. We will sometimes refer to "regional privatization effects," with "domestic" implied. Our estimating equation is

$$x_{jt} = \mathbf{f}\left(k_{jt}, l_{jt}\right) + \mathbf{Y}\boldsymbol{\gamma} + \mathbf{w}_t\boldsymbol{\alpha}_j + F_{jt}\phi + D_{jt}\mathbf{R}\boldsymbol{\delta} + D_{jt}\mathbf{I}\boldsymbol{\vartheta} + \eta_{jt}, \tag{2}$$

which differs from Equation 1 in interacting the indicator for domestic private ownership (D_{jt}) with vectors of region ($\mathbf{R}$) and industry ($\mathbf{I}$) dummies. The vector $\boldsymbol{\delta}$ contains our parameters of interest: the region-level productivity effect of privatization to domestic owners. The interaction of industry dummies with the domestic-privatization indicator controls for variation across regions in industrial composition. Thus, for example, any difference in the estimated effect of privatization on firm performance between Krasnoyarsk and Krasnodar should not be driven by the greater presence of metallurgy in the former region. We concentrate here on results from a model with firm fixed effects and firm-specific trends (i.e., $\mathbf{w}_t = (1, t)$), given the discussion of selection bias above.

Figure 2 illustrates the distribution of these estimated regional privatization effects. Consistent with the aggregate results reported in the previous section, the distribution is centered around a moderately negative effect. There is, however, considerable variation across regions, with heterogeneity at least as large as that observed across countries. The largest (most positive) effect is in Kamchatka, where privatization to domestic owners is estimated to have increased productivity by 38 percent; the smallest (most negative) is in North Osetia, with an estimated effect of -54 percent. As these examples illustrate, some of the largest-magnitude effects are in regions with relatively few firm-year observations, where estimation is relatively imprecise. Figure 3 demonstrates the relationship between the standard errors of elements of δ and the number of firm-year observations in our data set.

As mentioned above, this estimation error poses a slight complication for our attempt to identify determinants of region-level privatization performance. Denoting by $\hat{\delta}_r$ the estimated

privatization effect in region r, our second-stage estimating equation is

$$\hat{\delta}_r = \mathbf{Z}_r\mu + \varepsilon_r, \tag{3}$$

where $\mathbf{Z}_r$ is a vector of determinants of δ_r, the *true* region-level privatization effect, with μ the associated parameter vector. In general, the variance of ε_r is smaller in regions with more firm-year observations, given the greater precision of estimates from our firm-level equation (2). As described in BEG (2009), we correct for this heteroskedasticity in two ways. First, we simply estimate Equation 3 by OLS and calculate heteroskedasticity-robust standard errors. Second, we estimate Equation 3 by FGLS, using an estimator first suggested by Hanushek (1974) that incorporates the standard errors from first-stage estimation of δ.

Table 3 reports these results, using various definitions of the determinant vector Z_r. In columns (1) and (2), we include numerous factors that might influence privatization effectiveness either through program design (e.g., if privatization was implemented differently in regions with embedded semi-autonomous regions) or the post-privatization business environment (e.g., if the business environment is systematically different in regions with relatively large public administrations). We use values from the beginning of the transition period (1991), or as close to that as possible.[4] In columns (3) and (4), we add various measures of the local political environment. In general (i.e., except for the vote for Yeltsin in 1991), these are measured during the late 1990s or early 2000s and are more likely to suffer from endogeneity problems than the variables introduced in columns (1) and (2). Finally, in columns (5) and (6) we include only those variables that are robust determinants across the first four columns.

The first two of these robust determinants—population and urbanization—may reflect agglomeration effects, such that the incentives of private owners to invest in productivity-enhancing improvements are greater when input and output markets are deeper. The size of the regional executive-branch bureaucracy, in turn, is also positively associated with privatization effectiveness. As discussed in BEG (2009), survey evidence suggests that the post-privatization business environment is substantially better in regions with relatively large bureaucracies, with less queuing for licenses and smaller bribes paid to public officials; this may reflect either reduced congestion or greater competition among bureaucrats providing the same services.[5] Finally, privatization to domestic owners has a systematically larger effect in Russia's ethnic republics. Although we do not have strong priors with respect to this variable, BEG (2009) present evidence that this effect may be driven in part by systematically worse performance of state-owned enterprises in ethnic republics, rather than systematically better performance of privatized enterprises.

Also notable are the variables that appear not to be related to privatization effectiveness. Perhaps surprisingly, regional income per capita as measured in 1991 is uncorrelated with the impact of privatization on firm productivity. Although productivity may benefit from the availability of a well-educated workforce, there does not appear to be any differential impact on privatized enterprises. Similarly, the quality of transportation infrastructure, while

[4]Gross regional product, which we use in the panel regressions reported further below, is unavailable before 1994.

[5]BEG (2009) show that there is no relationship between privatization effectiveness and components of the bureaucracy with little direct control over the local business environment.

certainly measured with error, has no significant relationship with privatization effectiveness. Various factors that might influence the regional political economy—the share of output in resource extraction, employment and output concentration—are not significantly associated with the impact of privatization on firm productivity; neither are political and institutional outcomes correlated with privatization effectiveness. There is no evidence of a "Siberian Curse" (Hill and Gaddy, 2003) on privatization, at least as any such effect would be reflected in average January temperatures. More generally, the mere physical location of a firm is uncorrelated with privatization effectiveness in these average results, though, as we will show, such an effect emerges with time.

Thus far, we have focused on variation in the average effect of privatization at the regional level. It is straightforward to extend our methodology to account for time-varying regional effects. To do so, we first estimate a variant of Equation 2 where $\mathbf{R}$ and $\mathbf{I}$ are vectors of region-year and industry-year interactions, respectively. As before, we therefore control for composition effects, here by allowing the privatization effect to vary by industry-year.

Figure 4 illustrates the evolution of regional privatization effects over time. At the beginning of the transition period, regions with the most positive privatization effects are generally located away from Russia's European core. By 2004, the pattern has reversed, with privatization most effective west of the Urals. Our sense is that reflects the increasing importance of market access as the economic transition takes root. As Hill and Gaddy (2003) discuss, socialist firms were often located in inhospitable environments far other firms or consumers. At the beginning of the transition period, this may have mattered little to the relative incentives of private owners. With the development of private markets, however, the incentive effect of private ownership likely strengthened for private firms in the western part of Russia, with its relatively dense road networks and population centers. This effect may have been compounded by the tendency of new firms to locate in regions with relatively good market access (Brown et al., 2008).[6]

We can track this evolution more systematically, if still somewhat crudely, by estimating a panel version of Equation 3—regressing the estimated region-year privatization effects on the interaction of distance from Moscow and a linear time trend, together with region and year fixed effects—for the years 1994–2004. Table 4 presents results from this exercise, using both OLS and the FGLS estimator described above. With each passing year of transition, being located 1000 kilometers closer to Moscow increases the estimated effect of privatization on firm productivity by a full percentage point.

Columns (3) and (4) investigate two other potential determinants of privatization effectiveness in a panel setting: improvements in the general economic environment, as captured by gross regional product, and in the capacity of the state, as proxied by regional public administration employment. We include population among the regressors to account for economies of scale in public administration (Gehlbach, 2008*b*). As both GRP and public-administration employment may be endogenous to regional privatization effectiveness, these results should be viewed with caution.[7] Nonetheless, the associations are suggestive: privatization is more strongly associated with productivity improvements as the regional economic

[6]Using similar firm-level data for 2004, Brown et al. (2008) show that market access is relatively more important for private firms, but they do not address the change in regional variation that we establish here.

[7]BEG (2009) provide instruments for regional bureaucracy size in cross-sectional regressions similar to those in Table 3. Finding analogous time-varying instruments is an important task for future research.

environment and state capacity improve. Notably, the market-access effect identified in columns (1) and (2) is largely unaffected by the inclusion of these variables.

5 Conclusion

From the many possible questions that could be (and to varying extents have been) asked about Russian privatization, we have focused in this chapter on one rather narrow issue: the direct firm-level productivity effects of privatization in the manufacturing sector. Early studies of this issue, including a number of our own, were plagued with data problems that prevented the use of econometric methods to carry out apples-to-apples comparisons and to control for firm-level productivity differences and trends that could be correlated with privatization. Although empirical work is never absolute, we believe the recent availability of far superior data that facilitate the use of better methods allows greater confidence in results.

A robust finding of this chapter and our other recent papers is that Russia is an outlier in the measured impact of privatization on manufacturing firm productivity. While other countries for which comparable data are available show clear positive effects for privatization both to new domestic owners and to foreign investors, the Russian estimates for domestic privatization are negative in any specification that compares post- to pre-privatization performance (i.e., includes firm fixed effects). The Russian coefficients are small, but they are statistically significant, suggesting an approximate 5-percentage-point negative impact on multi-factor productivity. By contrast, the estimated effect of foreign privatization in Russia is positive, and although smaller than in other countries, it is qualitatively much more similar than the estimated domestic effects.

Besides replicating these basic findings with more recent data, we also report new tests of the possible dependency of the productivity effects on whether a firm has 100 percent of its shares privatized or remains partially state-owned. Our results are consistent with the hypothesis that full privatization yields better effects, but the difference is slight, and even for full privatization the estimated effect is negative and statistically significant. We also report an investigation into the question of whether privatized firms in Russia tend to systematically under-report sales relative to state-owned enterprises (and to a greater degree than in other countries), which could provide a measurement-based explanation for the outlier result in Russia. The survey data show only small differences in the relative under-reporting of sales by privatized firms and no evidence for an especially large gap in Russia. Comparing the results to other countries implies no role for the under-reporting hypothesis in accounting for cross-country differences.

In extending the data through 2005 (the last year for which the data necessary for estimation are available), we are able to document the somewhat more recent trends in privatization effectiveness in Russia. We find a pronounced upward trend in estimated productivity effects of domestic privatization in each year after 2002. According to this analysis, Russias domestic privatization effectiveness in 2005 is similar to those of the comparator countries in the first few years after privatization (typically 5 to 10 years earlier). At the same time, the 2005 results hint at a decline in foreign privatization effectiveness, perhaps associated with the increased uncertainty of property rights in recent years.

Our efforts to understand Russias privatization performance lead us to analyze regional variation, where we find a large range in the estimated domestic privatization effects. A number of regions have estimated effects on productivity similar to those in Central Europe, but many others exhibit negative estimated effects. This cross-sectional variation is strongly and robustly associated with the number of personnel in the regional executive bureaucracy, which are the parts of the Russian state most involved in regulation, licensing, permits, and infrastructure—areas of particular interest to restructuring firms. Population size and degree of urbanization are also strongly correlated with privatization effectiveness, but almost no other variables are. In particular, the data provide no support for privatization effectiveness varying significantly with per-capita income, share of output from resource extraction, climate, education, transport, border location, concentration of output and employment, and a number of political measures.

East-west location within Russia is also uncorrelated with privatization in these results, which are based on regional privatization effectiveness estimated as an average over the entire post-privatization period. But when we permit the regional effect to vary over time, we discover an interesting shift: regions in western Russia display increasingly positive privatization effectiveness. Examining this visual result econometrically, we demonstrate that proximity to Moscow is an increasingly important determinant of regional privatization effectiveness. Remarkably, even though these panel regressions include region fixed effects, the estimated bureaucracy coefficient exhibits a magnitude and significance similar to that in our cross-sectional results.

Taken together, our results suggest a more nuanced view of Russian privatization than that offered by either its critics or its defenders. The initial impact of privatization to domestic owners was disappointing, though there were parts of the country in which privatized firms showed the expected productivity gains. With time, however, the productivity effects of Russian privatization have begun to approach those seen much earlier elsewhere, with privatization most effective west of the Urals, where market access is greatest. Initially an outlier, Russia has become more of a "normal country" (Shleifer and Treisman, 2005), at least in the narrow sense of the effectiveness of privatization.

Of course, the controversy over Russian privatization will not disappear with these results. Some may judge a decade too long for privatization to live up to its potential, given its more rapid success elsewhere and the alleged damage to democratic institutions as Russia's economic transition was delayed. Others will argue that the difference between, say, three and ten years is of little account in grand-historical terms and that privatization did have its intended effect when all was said and done. Although empirical work on privatization cannot resolve these normative debates, one can hope that it at least provides a sounder evidentiary basis for whatever claims are made.

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Table 1: Estimated productivity effects of domestic and foreign privatization in Russia and other transition economies

	(1)	(2)	(3)	(4)	(5)
	Hungary	Romania	Ukraine	Russia	Russia
OLS estimates					
Domestic	0.176***	0.488***	0.166***	0.135***	0.165***
	(0.035)	(0.031)	(0.027)	(0.018)	(0.015)
Foreign	0.761***	0.725***	0.451***	0.670***	0.753***
	(0.061)	(0.098)	(0.182)	(0.135)	(0.079)
FE estimates					
Domestic	0.150***	0.240***	0.044**	−0.050***	−0.069***
	(0.022)	(0.022)	(0.019)	(0.015)	(0.013)
Foreign	0.554***	0.399***	0.408***	0.362***	0.434***
	(0.052)	(0.088)	(0.149)	(0.143)	(0.077)
FE&FT estimates					
Domestic	0.053**	0.136***	0.015	−0.027***	−0.053***
	(0.024)	(0.020)	(0.017)	(0.010)	0.009
Foreign	0.226***	0.303***	0.355***	0.183	0.151***
	(0.053)	(0.105)	(0.143)	(0.115)	(0.059)

Columns (1)-(4) are taken from BET (2006), and Column (5) is computed using the data (through 2004) and methods described in the text. Estimated coefficients (and their corresponding standard errors) are shown for Domestic (= 1 if the firm is majority private but not majority foreign-owned at the end of year $t-1$) and Foreign (= 1 if the firm is majority foreign-owned at the end of year t-1), respectively, relative to 50% or greater State (the residual category). The dependent variable is log(Output). Independent variables include log(Capital) and log(Employment), with coefficients permitted to vary across industries, as well as full sets of unrestricted industry-year interaction dummies. OLS = ordinary least squares; FE = firm fixed effects; FT = firm-specific trends. Standard errors are adjusted for clustering on firms. Significance levels: *** = .01, ** = .05.

Table 2: Estimated productivity effects by degree of privatization

	Mixed	100%
OLS		
	0.169^{***}	0.161^{***}
	(0.016)	(0.016)
FE		
	-0.080^{***}	-0.056^{***}
	(0.014)	(0.015)
FE&FT		
	-0.058^{***}	-0.040^{***}
	(0.009)	(0.011)

Notes: Samples and methods are the same as in column (5) of Table 1. As described in the text, Mixed refers to privatized firms with partly state, partly private ownership, whereas "100%" refers to privatized firms with no residual state share. Significance levels: *** = .01.

Table 3: Determinants of average regional privatization effectiveness

Dependent variable: Estimated regional privatization effectiveness from firm-level regression.

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	FGLS	OLS	FGLS	OLS	FGLS
Log population	0.120***	0.095**	0.102***	0.091**	0.086**	0.085***
	[0.045]	[0.037]	[0.034]	[0.035]	[0.041]	[0.026]
Urbanization	0.620***	0.513**	0.477**	0.405*	0.468***	0.455***
	[0.174]	[0.203]	[0.189]	[0.213]	[0.150]	[0.133]
Log regional executive	0.207*	0.150	0.272**	0.208**	0.186*	0.178**
bureaucracy per capita	[0.120]	[0.105]	[0.115]	[0.100]	[0.099]	[0.074]
Ethnic republic	0.075**	0.078*	0.105***	0.104**	0.055	0.076**
	[0.036]	[0.041]	[0.032]	[0.041]	[0.037]	[0.033]
Log income per capita	−0.057	−0.020	0.032	0.047		
	[0.107]	[0.102]	[0.101]	[0.098]		
Share of output in resource	−0.075	0.026	−0.198	−0.062		
extraction (%)	[0.211]	[0.136]	[0.182]	[0.132]		
Mean January temperature	0.005	0.004	0.000	0.000		
	[0.005]	[0.003]	[0.004]	[0.003]		
Share of population with higher	−0.300	−0.296	−0.806	−0.670		
education (%)	[0.652]	[0.737]	[0.710]	[0.724]		
Distance from Moscow	0.009	0.005	0.008	0.005		
(1000 km)	[0.012]	[0.008]	[0.011]	[0.008]		
Transportation infrastructure	0.095	0.047	−0.037	−0.080		
	[0.174]	[0.173]	[0.172]	[0.166]		
Border region	−0.015	−0.011	0.004	−0.003		
	[0.026]	[0.029]	[0.026]	[0.029]		
Presence of semi-autonomous	0.090	0.072	0.085	0.063		
region	[0.080]	[0.049]	[0.068]	[0.048]		
Regional output concentration	0.995	0.938	0.716	0.688		
	[0.634]	[0.643]	[0.668]	[0.644]		
Regional employment	−0.297	−0.249	−0.496	−0.413		
concentration	[0.353]	[0.308]	[0.316]	[0.306]		
Vote for Yeltsin, 1991 (%)			0.004*	0.002		
			[0.002]	[0.001]		
Democracy			0.001	0.002		
			[0.002]	[0.003]		
Government transparency			0.017*	0.012		
			[0.009]	[0.007]		
Media freedom			−0.001	−0.001		
			[0.002]	[0.001]		
Strength of political parties			0.513	0.610***		
			[0.308]	[0.221]		
Constant	−1.747***	−1.366**	−1.782***	−1.482***	−1.314***	−1.282***
	[0.579]	[0.559]	[0.549]	[0.528]	[0.430]	[0.309]
Observations	77	77	77	77	77	77
R-squared	0.346		0.484		0.241	

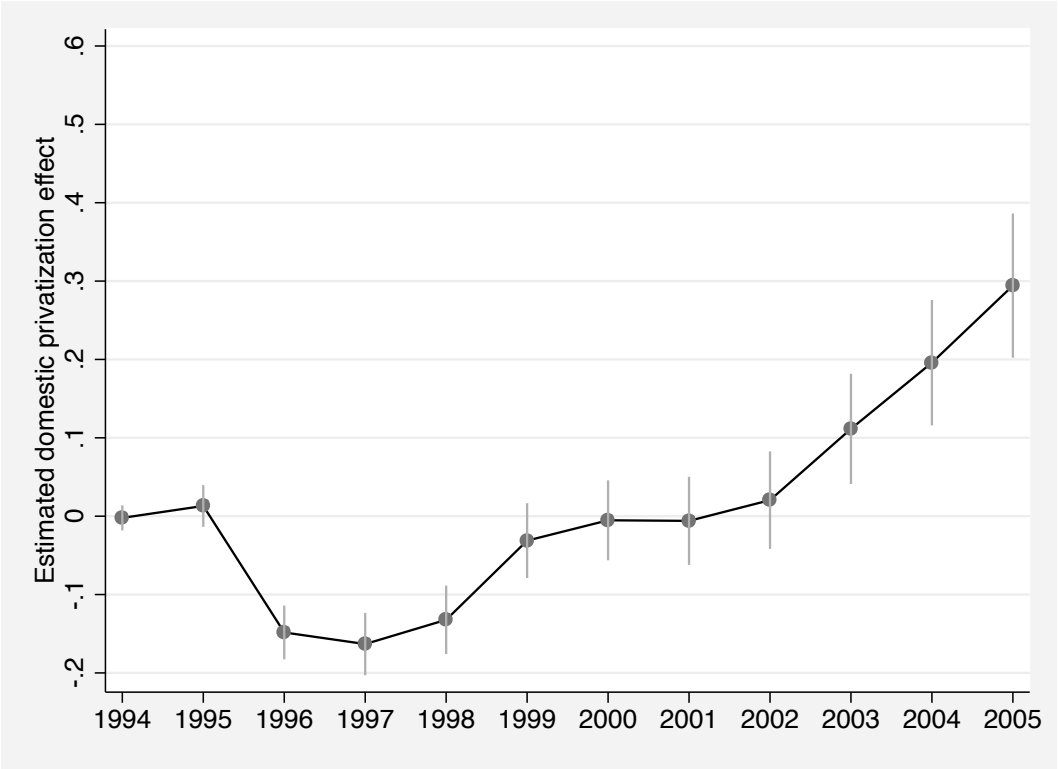
Notes: OLS regressions in Columns 1, 3, and 5, with heteroskedasticity-robust standard errors in brackets. FGLS regressions in Columns 2, 4, and 6; see text for details. Significance levels: *** = .01, ** = .05, * = .10.

Table 4: Determinants of annual regional privatization effectiveness

Dependent variable: Estimated annual regional privatization effectiveness from firm-level regression.

	(1)	(2)	(3)	(4)
	OLS	FGLS	OLS	FGLS
Year × distance from	-0.011^{***}	-0.012^{***}	-0.010^{***}	-0.011^{***}
Moscow (1000 km)	[0.002]	[0.001]	[0.001]	[0.001]
Log GRP			0.266^{***}	0.218^{***}
			[0.064]	[0.044]
Log population			1.120^{***}	0.526^{**}
			[0.291]	[0.216]
Log regional executive			0.504^{***}	0.228^{***}
bureaucracy per capita			[0.090]	[0.066]
Region fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	847	847	847	847
Regions	77	77	77	77

Notes: OLS regressions in Columns 1 and 3, with heteroskedasticity-robust standard errors corrected to allow for clustering within regions in brackets. FGLS regressions in Columns 2, 4, and 6; see text for details. Significance levels: *** = .01, ** = .05, * = .10.



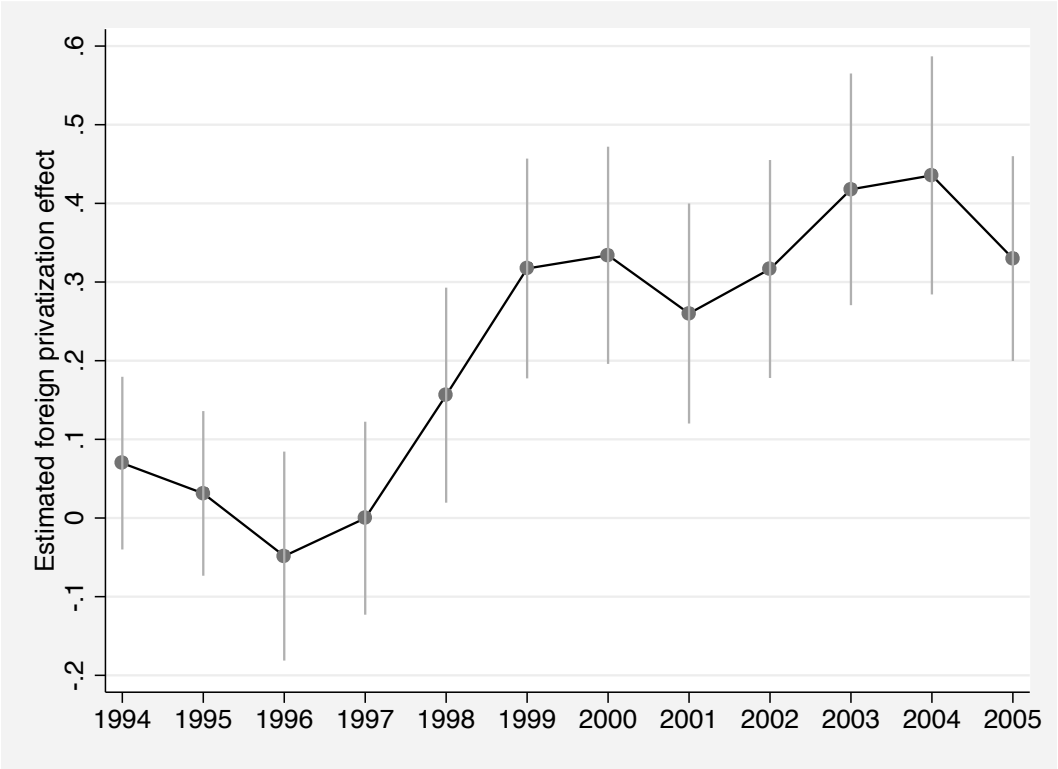


Figure 1: Dynamics of domestic and foreign privatization effects.

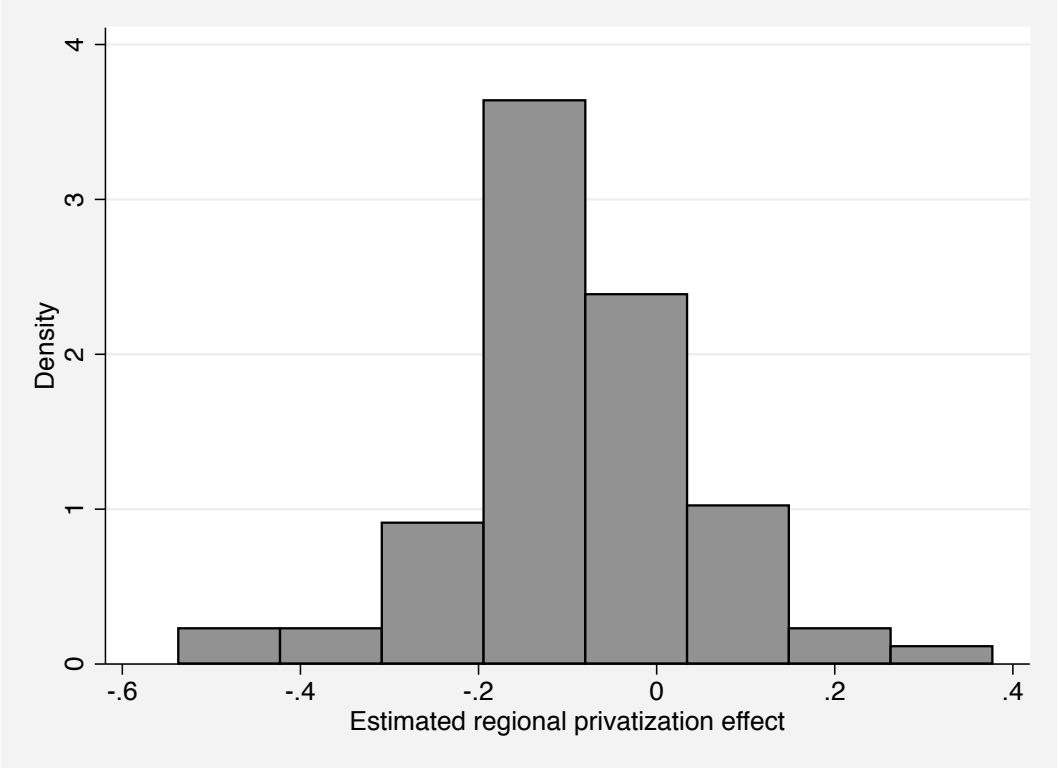


Figure 2: Frequency distribution of estimated regional privatization effects.

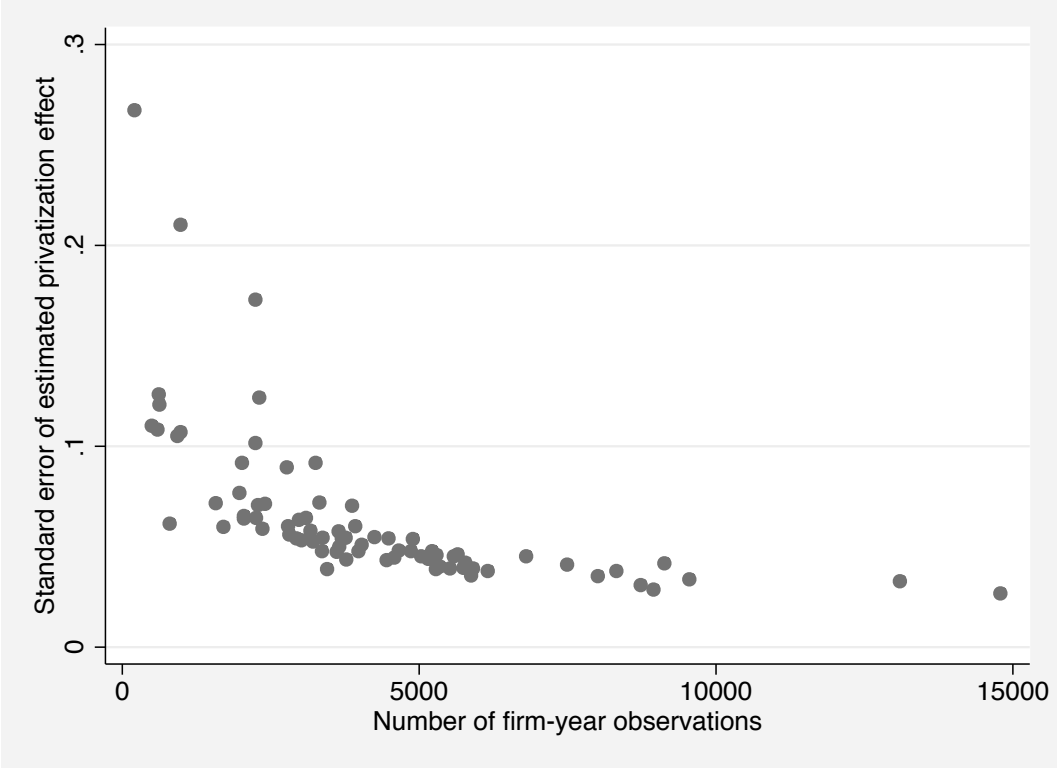


Figure 3: Precision of estimated regional privatization effects.

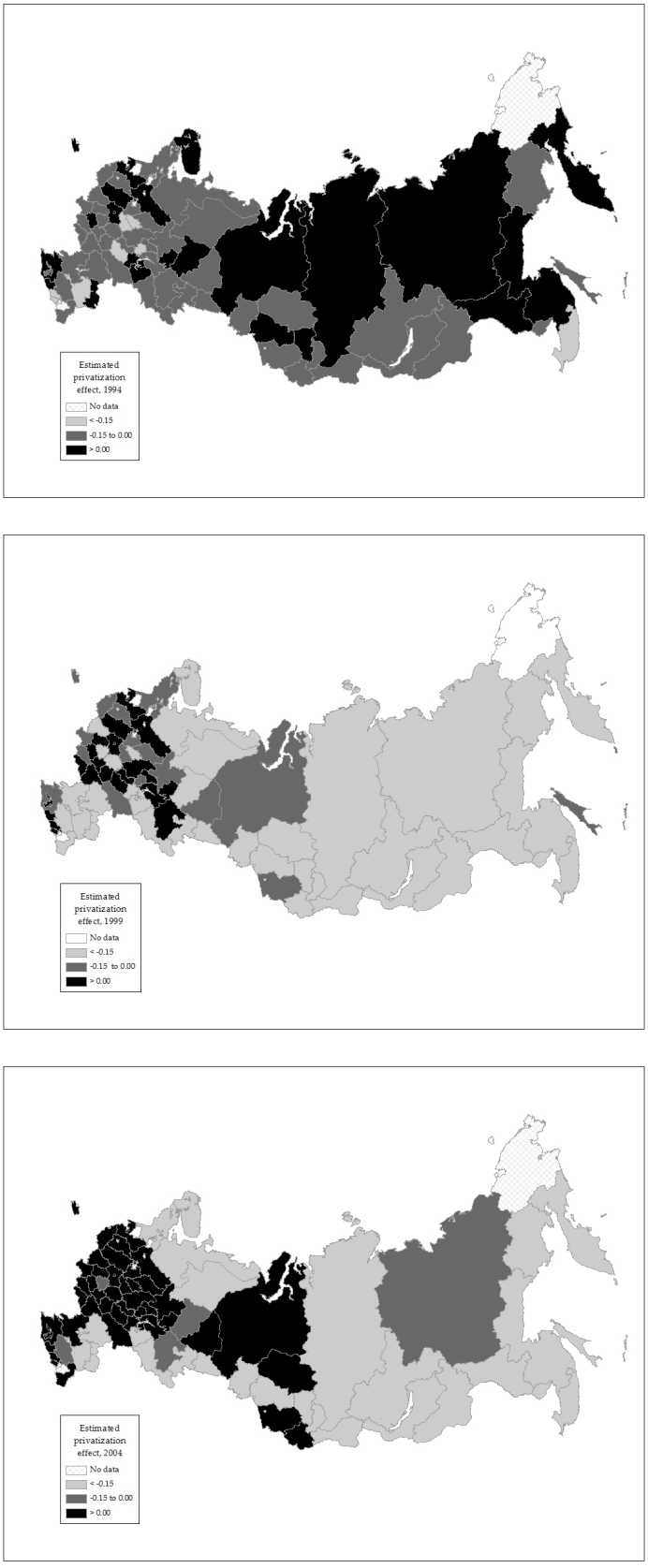


Figure 4: The evolution of regional privatization effectiveness: 1994 (top), 1999 (middle), and 2004 (bottom).