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J. David Brown
John S. Earle
Scott Gehlbach

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J. David Brown
Heriot-Watt University and IZA

John S. Earle
Upjohn Institute for Employment Research, Central European University and IZA

Scott Gehlbach
University of Wisconsin, Madison and CEFIR

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IZA

P.O. Box 7240
53072 Bonn
Germany

Phone: +49-228-3894-0
Fax: +49-228-3894-180
E-mail: iza@iza.org

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ABSTRACT

Helping Hand or Grabbing Hand? State Bureaucracy and Privatization Effectiveness*

Why have economic reforms aimed at reducing the role of the state been successful in some cases but not others? Are reform failures the consequence of leviathan states that hinder private economic activity, or of weak states unable to implement policies effectively and provide a supportive institutional environment? We explore these questions in a study of privatization in postcommunist Russia. Taking advantage of large regional variation in the size of public administrations, and employing a multilevel research design that controls for pre-privatization selection in the estimation of regional privatization effects, we examine the relationship between state bureaucracy and the impact of privatization on firm productivity. We find that privatization is more effective in regions with relatively large bureaucracies. Our analysis suggests that this effect is driven by the impact of bureaucracy on the post-privatization business environment, with better institutional support and less corruption when bureaucracies are large.

JEL Classification: H11, L33, P23, P26, P37, P48

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Corresponding author:

John S. Earle
Upjohn Institute for Employment Research
300 South Westnedge Avenue
Kalamazoo, Michigan 49007
USA
E-mail: earle@upjohn.org

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1 Introduction

In recent decades economic reforms aimed at reducing the role of the state in the economy have swept the developing and formerly communist worlds. Yet despite high hopes, the effect of these reforms has been sharply uneven. Why have similar economic reforms been successful in some environments but not others? Are reform failures the consequence of leviathan states that hinder private economic activity, or of weak states unable to implement policies effectively and provide a supportive institutional environment? We explore these questions in a study of privatization in postcommunist Russia. Taking advantage of large variation across Russia's regions in the size of public administrations, and employing a multilevel research design that controls for pre-privatization selection in the estimation of regional privatization effects, we examine the relationship between state bureaucracy and the impact of privatization on firm productivity.

The role of the state in economic reforms has been central to the study of postcommunist transitions. In response to the sometimes disappointing outcome of these reforms, especially in the countries of the former Soviet Union, many have stressed what seems to be the particularly dysfunctional nature of postsocialist bureaucracies (e.g., Frye and Zhuravskaya, 2000; Hellman, Jones and Kaufmann, 2000). Beyond this general consensus, however, there is little agreement on whether the problem is too much of the wrong kind of state, one that projects a "grabbing hand," or not enough of the right kind of state, one unable to offer a "helping hand" (Frye and Shleifer, 1997). Certainly rent-seeking bureaucrats raise the cost of doing business in postcommunist countries, a fact established in various cross-national enterprise surveys (e.g., EBRD, 2005). That said, the bureaucracies of Eastern Europe and the former Soviet Union are not large by world standards, and some—most notably Russia's—are in fact quite small (Brym and Gimpelson, 2004).[1] Together with the relative difficulty of extracting tax revenues in many postcommunist countries (e.g., Treisman, 1999; Gehlbach, 2008), this has led some observers to characterize postsocialist states as more "incapable" than "grabbing" (Easter, 2002), unable to enforce contracts, implement antitrust and bankruptcy laws, and generally provide an environment conducive to private economic activity (Grzymala-Busse and Jones Luong, 2002).

In principle, bureaucracy size could affect reform outcomes for a number of distinct reasons. Larger bureaucracies might have greater *capacity*, which could be used for good or ill. Post-communist governments were asked to perform tasks fundamentally different from those of their communist predecessors, and those with larger bureaucracies may have found it easier to implement reforms effectively and provide the necessary legal and regulatory infrastructure. Alternatively, more capacious states may have found it easier to interfere, predating and erecting barriers that negated the intended effects of reform. The size of the bureaucracy could also affect the *incentives* of bureaucrats, an argument associated especially with Shleifer and Vishny (1993). On the one hand, the power of any individual bureaucrat to

[1] Goldsmith (1999) makes a similar point with respect to African bureaucracies. Schiavo-Campo, de Tommaso and Mukherjee (1997) report that 0.8 percent of the Russian population is employed in government administration, versus an unweighted average across postcommunist countries of 1.9 percent and across OECD countries of 4.3 percent.

extract rents might be less when there are many competing bureaucrats able to provide the same service (e.g., license or permit), improving the institutional environment for private economic activity. On the other hand, if a larger bureaucracy implies that functions are subdivided into various services that are complementary from a private actor's point of view, then each additional bureaucrat may act as a "toll taker" who fails to internalize the impact of his "toll" (e.g., bribe) on the demand for other bureaucrats' services.[2] In sum, there are reasons to expect either a positive or negative impact of bureaucracy size on reform outcomes, though the actual direction of the effect is an empirical rather than a theoretical question.

These concerns are of particular importance to postcommunist privatization, the central policy in the effort to transform economies oriented around public ownership and bureaucratic coordination into ones where private economic activity and market coordination predominate. The premise was that the profit-motivated owners of privatized enterprises would engage in active restructuring to improve their firms' performance. Unlike passive state-owned enterprises, privatized firms were expected to invest in new technology and equipment, to lay off surplus labor or restructure their workforces, and to develop improved or completely new products and markets. In each of these cases, the active participation and cooperation of the bureaucracy would be essential. A bureaucracy geared toward grabbing, or one that was simply incapable, could raise the costs of any actions that the new private owners might want to carry out, and in the limit might prevent any improvements from taking place. In contrast, a bureaucracy oriented toward helping would issue the necessary permits and approvals and provide the complementary infrastructure that would reduce the costs of restructuring. The opportunities for bureaucratic influence—both predatory and supportive—are therefore especially important for privatized firms, such that an examination of the relationship between state bureaucracy and privatization effectiveness offers an important test of the role of the state in market-oriented economic reforms.

To explore the relationship between state bureaucracy and privatization effectiveness, we exploit a unique panel data set on the performance of nearly 25,000 state-owned and privatized firms in 77 Russian regions, taking advantage of large regional variation in bureaucracy size for reasons exogenous to the postcommunist transition. Using statistical methods originally developed to control for selection bias in labor-market programs, we estimate the impact of privatization on firm performance at the regional level while controlling for the possibilities that better-performing firms and firms with better prospects were more likely to be selected for privatization. We find large variation in the impact of privatization on multifactor productivity. We then examine region-level determinants of estimated regional privatization effectiveness, focusing especially on the size of state bureaucracy, measured as the number of employees in public administration (*not* public employment more generally), controlling for population. This focus on within-country variation allows us to hold constant many features of the macroeconomic environment and privatization-policy design that vary across countries, while focusing on institutional variation as an explanation for variation in privatization

[2] If bureaucrats collude, then the total bribe is independent of the number of bureaucrats. The perfect collusion cited by Shleifer and Vishny in the cases of monarchies and "old-time Communist regimes" may, however, be harder to maintain when the number of bureaucrats is large.

outcomes.

We find that privatization has a larger (more positive) effect on firm performance in regions with relatively large state bureaucracies. The estimated effect—which controls for regional differences in industrial structure—is quite large and robust to model specification (including estimation by OLS, FGLS, and 2SLS), choice of sample, and the inclusion of numerous controls. Differences in the predicted effectiveness of privatization in regions with large and small bureaucracies are comparable in magnitude to observed differences in privatization effectiveness across postcommunist countries. Our estimates suggest that this effect is driven by increases in the performance of privatized firms in regions with relatively large bureaucracies rather than decreases in the performance of state-owned enterprises. Furthermore, we find that of the three categories of bureaucrats for which we have disaggregated data, the impact on privatization effectiveness is limited to variation in the size of regional executive-branch bureaucracies, which plausibly have the greatest influence over the regional business environment. We observe little evidence that state bureaucracy influences privatization effectiveness through privatization-program implementation, as might be the case if more effective owners were selected in regions with relatively large bureaucracies. In contrast, we find some evidence using data from two recent surveys of Russian firms that the size of regional bureaucracies affects the post-privatization business environment, with private firms in regions with relatively large bureaucracies reporting shorter waiting times and fewer bribes when dealing with bureaucrats.

Beyond their role in helping to understand the role of the state in economic reform, the results in this paper contribute more generally to the literature on state administration as a key factor in explaining economic development. Cross-national studies have borne out Weber's insight that the character of the bureaucracy matters for growth (e.g., Knack and Keefer, 1995; Mauro, 1995; Evans and Rauch, 1999). Somewhat surprisingly, however, given the central role that the state plays in theories of economic development, relatively little is known about the empirical relationship between the size of the bureaucracy and economic performance.[3] As we show below, state-owned as well as privatized enterprises appear to be better off in regions with relatively large bureaucracies, implying that economic performance overall may be positively related to the size of the bureaucracy. Extending the analysis to other aspects of economic performance and other countries is an important agenda for future research.

The paper proceeds as follows. Section 2 reviews the literature on postcommunist privatization and firm performance, emphasizing the puzzles that have been raised by this research and how our paper helps to address them. In Section 3 we introduce our data, and in Section 4 we discuss our empirical strategy. Our main findings are presented in Section 5, where we establish a positive relationship between state bureaucracy and privatization effectiveness. We explore two alternative mechanisms through which this relationship might operate in Section 6. Section 7 concludes.

[3]There has been some theoretical investigation. See, e.g., Acemoglu and Verdier (2000).

2 Research on postcommunist privatization and firm performance

The design of privatization policies and their consequences for firm performance have been among the most discussed and researched issues in postcommunist Eastern Europe and the former Soviet Union. From the early 1990s, many policymakers and observers viewed privatization as the linchpin of a strategy to improve managerial incentives, encourage firm restructuring, and generally effect a shift to a "private property regime" (Frydman and Rapaczynski, 1994, p. 169). The initial enthusiasm for ownership change led in most countries to rapid divestment through programs of "mass privatization," which generally used vouchers for citizens to acquire shares (Blanchard et al., 1993; Boycko, Shleifer and Vishny, 1995). Some of these programs also gave special preferences to employees of the companies concerned, leading to large-scale ownership by managers and workers, in degrees varying with the design of the program and with the particular outcomes for each company. The emphasis on privatization became decidedly less fashionable later in the 1990s, as many critics argued that the programs had either done little good and resulted in misplaced priorities—for instance, by neglecting institutional change—or had actually caused damage—for example, by facilitating asset-stripping (e.g., Stiglitz, 1999; Roland, 2001).

Yet until very recently the empirical support for either of these positions was quite weak. The pro-privatization enthusiasm at the beginning of transition had little or no relevant previous experience that could serve as its basis, and there had been few systematic studies by the late 1990s to corroborate the negative views of the critics. The little evidence available to support such evaluations was limited to either macroeconomic performance indicators or detailed observations of a few firms. Just as the critics' position, which was part of a broader attack on the "Washington consensus," seemed to become dominant, a surge of statistical studies of privatized firms appeared. These studies, as summarized in reviews by Megginson and Netter (2001) and Djankov and Murrell (2002), tended to find positive effects of privatization on measures of firm performance in many countries. But the studies suffered from enough methodological weaknesses to make them ineffective in persuading most skeptics.

Among the important methodological problems in most research on the firm-performance effects of privatization are small sample size and short time series, with concomitant inability to conduct meaningful comparisons and control for selection bias in the privatization process. Short of a randomized experiment, an analysis of privatization effects requires detailed panel data with a large enough number of privatized and state-owned firms within industries and a long time series of observations on each firm before and after the privatization process. Yet nearly all studies until the early 2000s used survey samples of at most a few hundred firms, and the data were either cross-sectional or at best had only a few years of data. The small samples made it difficult to carry out appropriate comparisons, so that privatized firms in some industries were evaluated relative to state-owned firms in others. The short time series made it difficult to take into account possible biases in the selection of firms to be privatized. For example, if firms with inherently higher productivity or more rapidly growing productivity have a higher probability of privatization, then a simple comparison of pre- and post-privatization performance would result in a positively biased estimate of the true privatization effect.

These methodological issues have been addressed in more recent research, in particular by Brown, Earle and Telegdy (2006). Their results for Hungary, Romania, Russia, and Ukraine provide much stronger support for the earlier conclusion of the Djankov and Murrell (2002) survey that the effect of privatization is considerably stronger in Eastern Europe than the CIS.[4] The pattern across countries raises some puzzles, however: while the effect of privatization to foreign investors on productivity is uniformly positive and large (generally 20–40 percent, depending on the precise specification), the effect of privatization with new domestic owners is largest in Romania (14–24 percent), followed by Hungary (5–15 percent) and Ukraine (2–4 percent).[5] The estimates are actually negative for Russia, and although small in magnitude (-3 to -5 percent), they are always statistically significantly different from zero. Further analysis of the dynamics of these effects shows that while 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 distinctly negative and progressively worse through the first four post-privatization years, with a modest positive impact appearing only after about seven years.

Why does the effectiveness of privatization vary so much, and why is the Russian effect zero or even negative? A leading hypothesis is that institutional variation across countries plays a role in producing these divergent outcomes—Miller and Tenev (2007) write, for example, that Russian privatization was "implemented in an environment of a weak state, which did not have the capacity to protect its ownership rights and coordinate reforms"—but this explanation alone is inconsistent with the finding that Romania's privatization impact is larger than Hungary's, and Ukraine's larger than Russia's.[6] The much debated methods of privatization might also be part of the answer, and that explanation is certainly consistent with the much larger and more uniform effect of ownership by foreign investors, but it does not fully account for the cross-country differences in the estimates. Among other candidate explanations, Brown, Earle and Telegdy (2006) find little systematic variation in effectiveness of privatization by cohort (which could arise due to different methods being used across time or learning about how to implement privatization more effectively), calendar year (different macroeconomic conditions or business environments across time), growth in the firm's industry, or industrial composition.

A major difficulty in these attempts to identify the source of variation in privatization effectiveness is that the very small number of countries for which careful estimates of priva-

[4] 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.).

[5] The ranges given here depend on whether the specification allows for firm-specific time trends or merely includes firm fixed effects, a methodological issue discussed in Section 4 below.

[6] 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.

tization effectiveness are available precludes reliable statistical analysis. Moreover, there is substantial variation across postcommunist countries in privatization design and macroeconomic performance, both of which may affect institutional development and privatization performance, and thus much heterogeneity that is difficult to hold constant in cross-national comparisons. Our focus on Russia in this paper allows us to take advantage of institutional variation across a comparatively large number of regions, as well as variation in the impact of privatization on firm performance. We are thus able to more carefully investigate one candidate explanation for cross-sectional variation in privatization effectiveness, even while holding privatization design and the macroeconomic environment constant. Our particular emphasis on state bureaucracy addresses the fundamental question about postcommunist institutions raised above: is the problem too much of the wrong state, or not enough of the right one?

3 Data

We use panel data on manufacturing enterprises in Russia to estimate the effect of privatization on firm-level total factor productivity by region. The firm-level data are collected by the Federal State Statistics Service (Rosstat), the Russian successor to the corresponding agency in the USSR. The basic statistical methodologies and data collection mechanisms have remained unchanged through this period. We combine information from industrial-enterprise registries with joint-venture registries and balance-sheet data to construct comprehensive data and fill in missing values.

According to the Federal State Statistics Service, 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 of officially reported total industrial employment. The Russian data are available as an unbalanced panel from 1985 to 2004.

We exclude non-manufacturing sectors and non-profit organizations from this data set. To focus 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 firm-years in the sample only when they contain complete information, which does not reduce the sample appreciably. The resulting sample contains information on 24,684 enterprises, with 269,390 firm-year observations.

Table 1 presents summary statistics for the firm-level variables used in this analysis. The data include measures of *Output*, *Employment*, and *Capital stock*, as well as industry affiliation and regional location. The data do not contain an ownership variable prior to 1993, nor do they distinguish between minority and majority shares, instead containing codes for 100 percent state, mixed state-private, 100 percent (domestic) private, joint ventures, and 100 percent foreign. 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). Therefore we classify all mixed firms as private, together with the other three private categories. We classify all joint ventures as *Foreign private*, combining them with the 100-percent-foreign category, but together they comprise only about one percent of the sample by the end of the period of observation. The residual category—the difference between private and foreign—we label *Domestic private*.

To measure bureaucracy size in each region, we use employment in federal, regional, and local public administration per 1000 residents, which we term *Bureaucracy* (with "per 1000 residents" implied), provided for the years 1995–2004 by the Federal State Statistics Service. This category is far narrower than all public employment, excluding teachers, doctors, and other state employees not part of the bureaucracy. Included are employees of the executive, legislative, and judicial branches of government, though personnel of the Ministries of Interior (i.e., police) and Defense are excluded. Approximately three quarters of these employees are civil servants, with the remainder support staff such as secretaries and drivers (Brym and Gimpelson, 2004); data for 2004 suggest little regional variation in the ratio between the two. For some exercises we disaggregate public-administration employment into three broad categories for which data are available for the entire period 1995–2004: executive-branch employees formally subordinated to the federal government (*Federal executive bureaucracy*); executive-branch employees formally subordinated to regional and local governments (*Regional executive bureaucracy*); and public-administration employees in other branches of government (*Bureaucracy, other branches*), the vast majority of whom work either in the court system or the procuracy.

Because we estimate an average regional privatization effect—not a separate effect for each region-year—for most regressions we average public-administration employment per 1000 residents over the period 1995–2004; we discuss possible endogeneity concerns associated with this treatment just below. We use the log of this measure, and in all regressions control for the log of regional *Population* in the appropriate period (e.g., log of average population over 1995–2004). This specification accounts for economies of scale in public administration (e.g., Alesina and Spolaore, 2003), which imply that per-capita bureaucracy will be smaller on average in more populous regions,[7] and controls for the direct impact of population on privatization effectiveness (e.g., through attractiveness to investors, given fixed costs of investment in a region).

As Table 2 and Figure 1 illustrate, there is substantial and systematic variation across regions in our measure of public-administration employment. This variation appears to be driven in part by historical experience and other idiosyncratic factors, as it is remarkably stable over time—the bivariate correlation between any two adjacent years is never below 0.99—and not fully explained by other regional characteristics. In particular, though there is a general increase in the size of bureaucracies over this period, this affects all regions more or less equally, as would be the case if employment in any given year were benchmarked against

[7] The pairwise correlation between log bureaucracy and log population is -0.85.

previous-year staffing levels. Moreover, as Gimpelson and Treisman (2002) report, any variation over time appears not to be driven by changes in regional revenues, as might be the case if variation in economic performance produced variation in public-administration employment. Nonetheless, to assure that our use of average public-administration employment does not drive our results, we reran all regressions reported below using public-administration employment in 1995 rather than average employment; the results are qualitatively similar.

The stability of regional variation in public-administration employment suggests little reason to be concerned that bureaucracy is endogenous to regional privatization effectiveness. Nonetheless, our estimates of the impact of state bureaucracy on privatization effectiveness could be biased by the omission of some regional characteristic correlated with both public-administration employment and privatization effectiveness. Although our results are robust to the inclusion of numerous covariates, as a further robustness check we employ instrumental-variables techniques, identifying the impact of state bureaucracy from plausibly exogenous regional variation in public-administration employment. In particular, we instrument (log) bureaucracy on log *Population density* and number of *Jurisdictions*. These variables affect the size of bureaucracy through two different types of scale economies, the former because less densely populated regions may require relatively more bureaucrats, and the latter because regions with more jurisdictions require more bureaucrats.[8] As a consequence, any effect of bureaucracy on privatization effectiveness identified through this instrumentation strategy is likely to operate through incentives within the bureaucracy rather than through capacity, as bureaucracies that are larger for only these reasons may not be physically more capable of producing public goods or intervening in the economy (e.g., because any individual bureaucrat would spend more time transporting himself across a Siberian region than across Moscow). Below we report Hansen J statistics from such two-stage least squares regressions to test the assumptions that these instruments have been properly excluded.

In addition to log population, in all of our region-level regressions we control for a number of characteristics that may be correlated with both regional privatization effectiveness and our measure of state bureaucracy. We include (log) *Income per capita* (adjusted for regional CPI) and *Urbanization*, as both variables may affect product and input markets and so potentially the gains from privatization, and they could also reflect the demand for shares in the privatization process. Because these variables may in turn be affected by the success of privatization, we use data from 1992 (income per capita) and 1991 (urbanization), both before the initiation of mass privatization in Russia, though as we discuss below our results are robust to using values from later years. We also include a dummy variable (*Autonomous region*) equal to one if the region is an ethnic republic (nineteen regions) or an autonomous oblast (one region), as the legal and institutional environment might differ in regions granted more autonomy.[9] In further robustness checks, we also control for a number

[8] Bureaucracies might also be larger in less densely populated regions if Soviet planners made disproportionate investments in administrative capacity in underdeveloped and sparsely populated regions east of the Urals (e.g., Hill and Gaddy, 2003) and if those elites persisted from the communist to the postcommunist period (Szelényi and Glass, 2003). Note that because log population density can be rewritten as log population minus log territory, we implicitly control for both population and territory when regressing bureaucracy on number of jurisdictions.

[9] Our results are nearly identical if we instead define autonomous region as a republic only. Russia's

of other geographic, economic, and political characteristics of regions, as described in Section 5 below.

4 Empirical strategy

We employ a multilevel research design that first estimates regional privatization effectiveness from firm-level data and then regresses those region-level estimates on various regional characteristics. For the first stage of this two-stage procedure, we follow the estimation approach of Brown, Earle and Telegdy (2006) in using program-evaluation techniques to assess and control for selection bias in the privatization process. Brown, Earle, and Telegdy show that estimated effects of privatization are extremely sensitive to the specification of underlying heterogeneity across firms, and they demonstrate that the data imply that firms selected for privatization not only tend to be more productive on average than firms remaining state-owned, but also exhibit faster productivity growth. Once these two idiosyncratic factors are taken into account, however, no discernable selection bias remains. A number of specification tests imply that the specification containing both a firm fixed effect (FE) and a firm-specific trend (FT) is preferred to specifications omitting these components, and we thus employ this specification (FE&FT) to estimate regionally varying privatization effects in this paper. We also permit the technology parameters to vary across industries, and we include a full set of industry-year effects to control for time-varying industry characteristics and shocks that may be correlated with both ownership and productivity.

Our estimating equation for the first stage is

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

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, γ is the associated 200×1 vector of coefficients, $\mathbf{w}_t$ is a vector of aggregate time variables, α_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, $\mathbf{I}$ is a 1×10 vector of industry dummies with ϑ the associated vector of coefficients, $\mathbf{R}$ is a 1×77 vector of region dummies, and δ is the vector of coefficients of interest in this paper: the region-level productivity effect of domestic privatization. Finally, η_{jt} is an idiosyncratic error.

Concerning the functional form of $\mathbf{f}$, we focus on the Cobb-Douglas function in this paper, as Brown, Earle and Telegdy (2006) show that estimated privatization effects are robust to alternative functional forms (including translogs and a variety of assumed factor shares). The estimated functions are permitted to vary across industries, and the inclusion of interactions

federal system includes a number of semi-autonomous regions that are embedded within larger regions. Unfortunately, our firm data identify only the broader region in which the firm resides, though as we discuss below our results are robust to controlling for presence of a semi-autonomous region in the broader region. Missing data for some of the regional variables reduce the number of regions in the data set from 80 to 77.

between domestic private ownership and industry dummies removes the effect of regional variation in industrial composition. We always include a full set of unrestricted industry-year interactions, the $\mathbf{Y}$, which allows different productivity levels for each industry in each year, controlling for any time- and industry-varying factors such as price changes not captured by deflators, unmeasured factors of production, and quality differences across industry-year cells.

Our methods of controlling for selection bias are embodied in the specification of $\mathbf{w}_t$. The FE&FT model with firm fixed effects and firm-specific trends has $\mathbf{w}_t \equiv (1, t)$, so that $\alpha_j \equiv (\alpha_{1j}, \alpha_{2j})$, where α_{1j} is a fixed unobserved effect and α_{2j} is the specific trend for firm j. In practice, the FE&FT model is estimated in two steps, the first detrending all variables for each firm separately and the second estimating the model on the detrended data. Standard errors are corrected for correlation of error terms across observations for the same firm.[10]

Given the very small number of foreign-privatized firms in our data set, we do not attempt to estimate a separate foreign-privatization effect for each region, but assume an effect ϕ that is constant across regions. In fact, as discussed above, Brown, Earle and Telegdy (2006) find uniformly positive and large effects of foreign privatization across the countries in their study, in contrast to the quite different effects of domestic privatization that are our focus. Nonetheless, to assure that our results are not driven by this treatment, we re-estimated Equation 1, excluding all firms ever under foreign ownership. The estimated regional privatization effects for this subsample are nearly perfectly correlated with those for the full sample, and the estimated relationship between state bureaucracy and privatization effectiveness is almost identical. For conciseness, in what follows we often refer simply to estimated privatization effects, omitting the qualifier "domestic."

For the second stage of our multilevel procedure, we wish to estimate parameters of the model $\delta_r = \theta + B_r\zeta + \mathbf{Z}_r\mu + u_r$, where δ_r is the productivity effect of domestic privatization for region r; θ is a constant; B_r is log bureaucracy and ζ the associated coefficient; $\mathbf{Z}_r$ is a vector of control variables described below, including log population, with μ the associated vector of coefficients; and u_r is an error term with spherical variance σ^2. We do not, however, observe δ_r directly, but instead have an estimate $\hat{\delta}_r = \delta_r + v_r$ from estimation of Equation 1. Our estimating equation for the second stage is therefore

$$\hat{\delta}_r = \theta + B_r\zeta + \mathbf{Z}_r\mu + (u_r + v_r) = \theta + B_r\zeta + \mathbf{Z}_r\mu + \varepsilon_r, \tag{2}$$

where we define $\varepsilon_r \equiv u_r + v_r$ and assume that u_r and v_r are independent. The difficulty in estimating Equation 2 is that the precision of first-stage estimates of δ_r will generally be greater in regions with more firm-year observations, implying that ε_r will have smaller variance in such regions. Estimation of Equation 2 by ordinary least squares would therefore be inefficient and could produce inconsistent standard errors.

We address this problem of second-stage heteroskedasticity in two ways. First, we simply estimate Equation 2 by ordinary least squares but calculate heteroskedasticity-robust standard errors. This produces consistent parameter estimates and standard errors, but does

[10] Our analysis of serial correlation in the residuals suggests that the process is not a simple AR(1), and the lagged residuals are sometimes significant (with varying signs) up to 4 lags, with patterns differing across specifications.

not exploit the known variance structure arising from the first-stage estimation procedure. Second, we employ a feasible generalized least squares (FGLS) estimator first suggested by Hanushek (1974; see Lewis and Linzer, 2005 and Jusko and Shively, 2005 for recent treatments).[11] The logic of this estimator is that more weight should be given to regions where δ_r has been estimated relatively precisely, but only to the degree that υ_r is an important component of ε_r.

Formally, denote the standard error of $\hat{\delta}_r$ from first-stage (firm-level) estimation as $\hat{\omega}_r$, and the $N \times N$ estimated variance matrix (where N is the number of regions) associated with the vector $\hat{\delta}$ as $\hat{\mathbf{G}}$. Further, denote the residual for region r from OLS estimation of Equation 2 as e_r. An unbiased estimate of σ^2 is thus $\hat{\sigma}^2 = \left[\sum_r e_r^2 - \sum_r \hat{\omega}_r^2 + \mathrm{tr}\left(\mathbf{X}'\mathbf{X}\right)^{-1}\mathbf{X}'\hat{\mathbf{G}}\mathbf{X}\right]/(N-K)$, where K is the number of regressors in the second-stage model, and $\mathbf{X}$ is the $N \times K$ matrix of regressors. We then define the estimated variance matrix $\hat{\mathbf{O}} \equiv \hat{\mathbf{G}} + \hat{\sigma}^2\mathbf{I}$ for use in FGLS estimation.

As discussed in the previous section, we check the robustness of our results by instrumenting bureaucracy on population density and the number of jurisdictions at the regional level. We present results from two-stage least squares regressions, calculating heteroskedasticity-robust standard errors to account for heteroskedasticity arising from estimation of regional privatization effects.

5 Results

We begin by discussing results from the first stage of our multilevel procedure, where we estimate the region-level productivity effect of domestic privatization from firm-level data. Because we include both firm fixed effects and firm-specific trends in Equation 1, these estimates are based on deviations resulting from privatization from the productivity trend for each individual firm, controlling for industry-year shocks.[12] Further, because Equation 1 allows the privatization effect to vary across sectors as well as regions, our estimates of regional privatization effectiveness control for the composition of regional industry (e.g., that Tyumen has more natural-resource firms than St. Petersburg).

Variation in these regional estimates is large, ranging from a 46-percent reduction in multifactor productivity in the Jewish Autonomous Oblast to a 40-percent increase in Kamchatka,[13] with an unweighted mean across regions of minus 9 percent. Figure 2 illustrates the frequency distribution of these estimates, and Figure 3 provides a map.

[11] Lewis and Linzer (2005) and Jusko and Shively (2005) each focus on the special case in which the first-stage sampling errors are independent of each other. That is not the case in our model, given that the δ_r are estimated in a single equation. Our FGLS estimator therefore takes the more general form suggested by Hanushek (1974).

[12] Thus, for example, privatization would have the same impact on two firms—one poised to attract investment, the other burdened with excess employment and declining demand—if it increased productivity two percent above a positive trend in the first case and two percent above a negative trend in the second.

[13] Such outliers are usually smaller regions with fewer observations, implying that these estimates should be treated with relative caution. Our FGLS estimation does precisely this.

Table 3 summarizes the key result of this paper, the estimated impact of state bureaucracy on regional privatization effectiveness. The first two columns present OLS and FGLS regressions of our estimates of regional privatization effectiveness on log bureaucracy (per 1000 residents) and the covariates discussed above. In each regression, the estimated effect of state bureaucracy on regional privatization effectiveness is large and statistically significant. The estimated effect of bureaucracy is larger, and the associated standard error is smaller, in the FGLS regression, where greater weight is given to regions in which first-stage privatization effects are estimated with relative precision. Controlling for other regional characteristics, a one-standard deviation increase in log public-administration employment is associated with a 7–8 percentage-point increase in regional privatization effectiveness. To put this result in perspective, Brown, Earle and Telegdy (2006), using the same (FE&FT) estimation method that we employ in the first stage of our multilevel procedure, estimate the impact of privatization to domestic owners in four postcommunist countries to range from minus three percent in Russia to fourteen percent in Romania. Thus, variation in state bureaucracy within Russia is associated with differences in privatization effectiveness comparable to those observed across postcommunist countries.

This estimated relationship is robust to changes in specification and sample. As discussed above, we obtain a similar estimate of the relationship between state bureaucracy and privatization effectiveness if we substitute log bureaucracy in 1995 for the log of average bureaucracy from 1995 to 2004. We also replaced all of our regional characteristics with values for various years between 1995 and 2004, with no fundamental change in our results.[14] Our results are also essentially unchanged if we run the OLS model having excluded influential observations (i.e., those with large DFBETA statistics).[15] Moreover, we obtain nearly identical results when we include the firm's founding date in the first stage as a measure of capital obsolescence (late-developing regions with larger bureaucracies might have fewer such firms), and very similar results from a one-stage "interaction model," where we model the regional privatization effect as a deterministic function of regional characteristics. We have checked that our results are robust to exclusion of Moscow and St. Petersburg from the sample, and to inclusion of numerous other covariates, including share of regional output in resource extraction; regional output and employment concentration; quality of local transportation infrastructure; distance from Moscow; share of population in higher education; mean January temperature; border region; presence of a semi-autonomous region embedded within the larger region; proxies for democracy, government transparency, media freedom, party strength, and governor power; and vote for Yeltsin in 1991.[16]

[14] In particular, income per capita in 1992 might be a poor proxy for local purchasing power later in the transition period, though we obtain nearly identical results when we substitute values from later years.

[15] Checking for influential observations is particularly important in a multilevel context, where estimation errors in the first stage may produce outliers that drive results in the second. See, e.g., Achen (2005).

[16] Given that Equation 1 controls for industry-specific privatization effects, any effect of resource extraction would reflect the impact of regional industrial structure on all firms in a region, whether such firms operate in resource extraction or not. Stoner-Weiss (1997) and Hale (2003) focus on the incentive of regional Russian leaders to promote economic development, highlighting the role of industrial concentration and strength of governors' political machines, respectively. Mean January temperature might be correlated with regional privatization effectiveness if investors were less attracted to inhospitable regions populated by Soviet planners (Hill and Gaddy, 2003).

Our baseline results thus point to a strong effect of bureaucracy size on privatization effectiveness. In principle, this relationship could be spurious if large bureaucracies were also better-paid, since greater formal compensation may encourage bureaucrats to exert effort and refrain from rent seeking. To check that this is not driving our results, we constructed various measures of expenditures on government administration, adjusting for regional CPI and considering different aggregations across level of government. Although positively correlated with bureaucracy size after controlling for population, inclusion of (the log of) these measures produces little substantive change in our results, and the effect of the expense variables themselves is never significant.

Further evidence that bureaucracy size drives privatization effectiveness comes from our instrumental-variable regression, presented in the last column of Table 3. Both of our instruments—population density and number of jurisdictions—are strongly correlated with bureaucracy in the first stage of the 2SLS regression, in a direction consistent with scale economies in public administration, and the Hansen J-statistic (which allows a test of overidentification in the presence of heteroskedasticity) provides evidence that these variables are properly excluded.[17] The second-stage estimate of the effect of bureaucracy on privatization effectiveness is statistically significant and of a magnitude slightly larger than in the FGLS regression. This is suggestive of an effect of bureaucracy size on incentives within the bureaucracy, rather than on physical capacity to implement economic reforms or provide public goods, as bureaucracies that are larger solely because of scale effects may not have any greater capacity. Consistent with this interpretation, Shetinin et al. (2005) report that regional officials in Russia often issue permits in areas where they have no formal licensing authority, with the cost of such permits less than that of the corresponding licenses from federal authorities. If firms are able to choose among such approvals, then the cost of acquiring licenses to engage in economic activity could be negatively associated with the number of regional officials. We present further evidence of this effect in the following section.

In principle, a positive estimated relationship between state bureaucracy and regional privatization effectiveness could reflect two different effects. First and perhaps most obviously, it is possible that larger bureaucracies improve the performance of privatized firms, while having less impact on state-owned enterprises. Second, it may be that large state bureaucracies find it easier to meddle in the affairs of state-owned enterprises, thus worsening their performance; to the extent that bureaucrats are unable or disinclined to similarly interfere with the operation of privatized firms, the estimated impact of privatization on firm performance would be greater in regions with relatively large bureaucracies. Combinations of both effects are also possible.

Unfortunately, we cannot evaluate these possibilities in any specification that includes firm fixed effects, as in any such specification only the relative magnitude of firm characteristics can be estimated. The only way to address this issue is in a pooled OLS context, in which

[17] If we include both instruments as regressors, then the estimated coefficient on bureaucracy is 0.259 (significant at $p = 0.155$), and the estimated coefficients on both log population density and number of jurisdictions are small and imprecisely estimated. Analogous results obtain if we perform the same exercise with disaggregated bureaucracy, discussed below, with an estimated coefficient on regional executive bureaucracy of 0.218 (significant at $p = 0.072$).

it is possible to estimate separate regional effects for state and private firms. We therefore estimate the following equation:

$$x_{jt} = \mathbf{f}\left(k_{jt}, l_{jt}\right) + \mathbf{Y}\gamma + F_{jt}\phi + S_{jt}\mathbf{I}\kappa + S_{jt}\mathbf{R}\chi + D_{jt}\mathbf{I}\lambda + D_{jt}\mathbf{R}\nu + \eta_{jt}. \quad (3)$$

This equation differs from Equation 1 in omitting the detrending term $\mathbf{w}_t\alpha_j$, and in including separate ownership-industry and ownership-region interactions for state-owned ($S_{jt} = 1$, $D_{jt} = 0$) and domestically owned private ($S_{jt} = 0$, $D_{jt} = 1$) firms. Given the issues associated with selection bias in the privatization process, the difference in estimated regional private (ν) and state (χ) effects is not our preferred estimate of regional privatization effectiveness. What is of interest is whether the estimated state effect is the same or larger in regions with relatively large bureaucracies. If that is the case, then the evidence would suggest that state bureaucracy improves privatization effectiveness by disproportionately improving the performance of privatized firms, rather than by worsening the performance of state-owned enterprises.

Table 4 presents results from regressions of the state effect estimated in Equation 3 on state bureaucracy and other regional characteristics, using the same three estimators as in Table 3. In each specification, the estimated coefficient on bureaucracy is positive and statistically significant. The evidence implies that state bureaucracy does not affect privatization performance by making state-owned enterprises worse off; on the contrary, bureaucracy is estimated to improve state-enterprise efficiency. But the estimated effect of bureaucracy on the productivity of privatized firms is even greater.[18] On balance, the hand of the state "helps" rather than "grabs."

But which hand? Table 5 reports results from regressions analogous to those in Equation 2, but with disaggregated state bureaucracy. (Because we have three bureaucracy categories but only two instruments, we cannot employ our instrumental-variables estimator here.) Of the three categories for which we have annual data from 1995–2004, only regional executive-branch bureaucracy is associated with privatization effectiveness. This is consistent with an interpretation of regional bureaucracies as having the most direct influence over the regional business environment. Alternatively, this result could be driven by variation in the power of regional governors vis-à-vis the center, if that variation is reflected in variation in the relative size of regional and federal bureaucracies. However, if we include the ratio of regional to federal employees together with (log) bureaucracy, then the estimated impact of bureaucracy is statistically significant as in Table 3 while that of the regional/federal ratio is not. Similar results hold with regressions where the dependent variable is the state effect estimated in Equation 3.

The evidence in this section is thus that bureaucracy size, and in particular the number of bureaucrats employed by the executive branch of regional governments, is associated with better privatization performance. In the following section we explore two alternative mechanisms through which this effect might have operated.

[18] To see whether regional characteristics might have affected selection bias in the privatization process, we regressed the difference in the estimated privatization effect from Equation 3 (the difference in the estimated regional state and private effects) and that from Equation 1 on the same regional characteristics as in our other specifications. None of the estimated regional effects in this model are statistically significantly different from zero.

6 State bureaucracy and privatization effectiveness: mechanisms

Why do privatized firms perform better in regions with relatively large state bureaucracies, and in particular, with relatively large regional executive-branch bureaucracies? In principle, state bureaucracy might affect privatization performance through two separate mechanisms, which are not mutually exclusive. First, it is possible that privatization was conducted differently in regions with more bureaucrats. For example, it is possible that privatization in regions with large regional bureaucracies resulted in higher-quality owners, i.e., those who were more willing or able to restructure. Second, it may be that the post-privatization business environment was different in regions with relatively large regional bureaucracies, such that privatized firms found public officials more supportive of their efforts to restructure. In this section we explore the "privatization implementation" and "post-privatization business environment" hypotheses in turn. In each case we face data constraints that prevent us from drawing strong conclusions, so the results we report should be viewed as suggestive but not definitive.

6.1 Privatization implementation

Our data—while comprehensive in terms of firm-year coverage—offer little detail about post-privatization ownership structure. (As discussed above, we observe only whether a firm was privatized to a domestic or foreign owner; given the paucity of foreign privatizations we focus on domestic privatization effects.) Nonetheless, we may explore the privatization-implementation hypothesis through other tests. In the results we report from these tests, we define bureaucracy as the number of public-administration employees in the relevant category in 1995, the first year for which we have data and the year most relevant for the privatization-implementation hypothesis, as mass privatization in Russia was conducted from late 1992 to mid-1994. However, all of our results are robust to defining bureaucracy as the average over 1995–2004, as in the previous section.

Our first exercise takes advantage of substantial variation across regions in the proportion of firms privatized. The Russian privatization program relied on managers and employees to organize the cumbersome processes of corporatization and initial share allocations, and the bureaucracy's primary role was to check that the proper procedures were followed (Frydman, Rapaczynski and Earle, 1993; Boycko, Shleifer and Vishny, 1995). The bureaucracy thus mainly performed a blocking function in the overall process, such that poorly operating and understaffed bureaucracies might have been less successful in preventing poorly designed privatizations from going forward, resulting in worse ownership structures and lower privatization effectiveness.

We test this hypothesis by controlling in our second-stage regressions for the fraction of firms in each region that were eventually privatized (*Privatization propensity*). If this variable at least partially drives the observed relationship between regional bureaucracy and privatization effectiveness, then the estimated effect of privatization propensity should be negative and the estimated effect of regional bureaucracy in Table 5 should decline when privatization propensity is added to the equation, in the limit becoming insignificantly different from zero. Our results, shown in the first two columns of Table 6, contradict this hypothesis. The estimated relationship between privatization probability and privatization effectiveness is not significantly different from zero, and the estimated coefficients on log regional executive bureaucracy are statistically indistinguishable from those in Table 3.

A second test of the privatization-implementation hypothesis is suggested by a particular feature of the Russian privatization program. According to Russian privatization legislation, regional governments retained control over privatization of small enterprises, defined as enterprises with up to 200 employees and one million rubles of fixed assets as of January 1, 1992; all other privatizations were governed from Moscow (Frydman, Rapaczynski and Earle, 1993). Consequently, if bureaucrats influenced the privatization process for firms in their regions, this was more likely the case for small than large firms. Evidence that state bureaucracy—and in particular, regional executive-branch bureaucracy—improves privatization effectiveness more for small than for large firms would therefore be consistent with the privatization-implementation hypothesis.

To explore this hypothesis, we estimate the following first-stage equation:

$$x_{jt} = \mathbf{f}\left(k_{jt}, l_{jt}\right) + \mathbf{Y}\gamma + \mathbf{w}_t\alpha_j + F_{jt}\phi + D_{jt}\mathbf{I}\vartheta + D_{jt}\mathbf{R}\delta + L_{jt}D_{jt}\mathbf{R}\psi + u_{jt}.$$

As in Equation 1, we control for selection bias in the privatization process through our specification of $\mathbf{w}_t$. The inclusion of the term $L_{jt}D_{jt}\mathbf{R}\psi$ captures the marginal privatization effect for small firms, measured at the regional level, where the dummy variable L_{jt} equals one if a firm meets the definition of small in the privatization legislation, and zero otherwise. Our interest is in the vector of coefficients ψ: a positive relationship between estimates of these effects and regional bureaucracy would be consistent with the hypothesis that bureaucracy influences privatization effectiveness, at least in part, through the process of privatization itself. As shown in the second two columns of Table 6, however, the effect of all of the bureaucracy variables, including regional executive-branch bureaucracy, is statistically indistinguishable from zero.

The evidence thus does not support the hypothesis that regional bureaucracies influence privatization effectiveness through the process of privatization itself, as any such influence would likely be reflected in regional differences in the propensity to privatize or in variation in the effect of privatization on small and large firms. In the following section we explore the alternative hypothesis that regional bureaucracies influenced privatization effectiveness through the post-privatization business environment.

6.2 Post-privatization business environment

Our firm-level data contain no direct measures of business-state relations, so we turn to two alternative data sets: a Centre for Economic and Financial Research (CEFIR) survey monitoring the regulatory burden of small enterprises conducted in the spring of 2002 (hereafter referred to as the "Monitoring" survey), and the Russia sample from the 2005 round of the

EBRD-World Bank Business Environment and Enterprise Performance Survey (BEEPS). Although cross-sectional in nature, late relative to the bulk of Russian privatization, and far less comprehensive in regional coverage than the firm data we use in the rest of this paper, each survey includes questions that can be used to evaluate the regional business environment. The Monitoring sample was constructed from random draws from lists of registered firms within each region, whereas the BEEPS survey employed quotas for employment, location, industry, ownership, and exporter status.[19]

We focus on measures of business-state relations that could plausibly be affected by the size of regional executive-branch bureaucracies, on the one hand, and affect the relative performance of privatized firms, on the other. For the Monitoring survey, we examine the relationship between bureaucracy and four "objective" measures of the firm's regulatory burden, each focusing on licenses received in the second half of 2001 and each measured in logs: *Average time elapsed to license*, *Maximum time elapsed to license*, *Firm's person-hours to get license*, and *Average license price* (including official payment, costs of using intermediaries and consultants, gifts, etc.). For the BEEPS, we examine measures of the magnitude of *Bribes to bureaucrats* and *Kickbacks to bureaucrats* (measured as percentage of sales and contract value, respectively) for firms in the respondent's "line of business," as well as categorical measures of the degree of *Contract/property-rights enforcement* and the firm's ability to *Appeal administrative violations* without recourse to unofficial payments (i.e., bribes). Table 7 presents summary statistics and complete wording for these measures.

For both data sets, we are forced to draw region-level inferences from a small number of regions: 20 for the Monitoring survey, and 14 for the BEEPS.[20] We therefore rely on parsimonious specifications, relating measures of business-state relationships to size of bureaucracy and regional population (measured the year prior to the survey), as well as to various firm-level characteristics. Consistent with our estimation strategy in the rest of the paper, we assume the presence of unobserved region-level effects. This is conservative, as the standard errors we calculate from these "random-effects" models are generally larger than the analogous heteroskedasticity-robust standard errors that correct for arbitrary correlation within clusters.[21]

Because we have too few state-owned enterprises in either data set to evaluate the interactive effect of ownership and regional characteristics, we drop these firms from the subsequent analysis. Our results should thus be viewed as reflective of the business environment for private firms. In principle, the regional business environment for state-owned enterprises might vary either in similar or quite different ways, but privatized firms likely had a larger motivation to undertake costly restructuring to improve their performance, and therefore

[19] We have access only to the first round of the Monitoring survey, conducted in 2002. Previous rounds of the BEEPS, conducted in 1999 and 2002, include no region identifier. Further information on the Monitoring survey is available at `http://www.cefir.ru/ezhuravskaya/policy/Round1_Monitoring_eng_report.pdf`. The 2005 BEEPS data set and related information are available at `http://www.ebrd.com/country/sector/econo/surveys/beeps.htm`.

[20] We obtain results qualitatively similar to those reported above when regressing estimated privatization effects on regional characteristics for these subsamples.

[21] Donald and Lang (2007) provide Monte Carlo evidence that "cluster-robust" standard errors are systematically too small when the number of clusters is small.

they had the most to gain or lose from the performance of public officials.

Our primary interest is in the impact of regional executive bureaucracy—the only category of bureaucracy significantly associated with privatization effectiveness—on the business environment for private firms, though the effect of the other measures may be of some independent interest. Table 8 presents results from the Monitoring regressions. Regional executive bureaucracy is negatively associated with all four measures, implying a lower waiting time and cost to obtain a license in regions with more regional executive bureaucrats per capita.[22] With the exception of the third model, where the firm's person-hours to get a license is the dependent variable, the estimated coefficients on regional executive bureaucracy are statistically significant. We obtain similar results from other specifications, including OLS regressions with heteroskedasticity-robust standard errors that do and do not adjust for regional clustering, between-effects regressions, and regressions that include other regional controls (including income per capita, urbanization, and autonomous region). In all cases, the estimated coefficient on regional executive bureaucracy is negative, and it is always significant in regressions where the dependent variable is average or maximum time elapsed to license.

Table 9 presents results from the BEEPS regressions, including marginal effects for ordered-probit regressions where categorical measures are the dependent variable. Regional executive bureaucracy has a large and statistically significant effect on kickbacks to bureaucrats, with a one-standard deviation increase in log regional executive bureaucracy predicted to reduce kickbacks by close to one percent of the contract value. This result is robust to the same alternative specifications as with the Monitoring data: only for the between-effects regression does the estimated effect of regional executive bureaucracy lose significance, though the magnitude is still sizeable. The estimated effect of regional executive bureaucracy in the other three models is not statistically significant, though always with the expected sign.

Thus, within the limits imposed by the very small number of regions represented in these data sets, the results here provide some evidence that larger regional executive bureaucracies—which the regressions in Table 5 identified as that portion of the bureaucracy responsible for better privatization performance—contributed positively to the post-privatization business environment, reducing wait times and lowering costs for licenses and government contracts. In principle, lower wait times could be driven either by increased capacity or better incentives (i.e., competition among bureaucrats) in larger bureaucracies. In contrast, the relationship between bureaucracy size and the cost of licenses and government contracts seems more consistent with improved incentives resulting from bureaucratic competition. Without discounting the numerous frustrations of private firms in Russia in dealing with public officials, it appears that those problems may be smaller in regions where there are more bureaucrats with whom to work, with positive consequences for the relative performance of privatized firms.

[22] In a regression of the number of licenses and authorizations possessed by the respondent on the same regional and firm characteristics, the estimated coefficient on log regional executive bureaucracy is negative and statistically insignificant. Thus, the lower average waiting time and cost of obtaining a license in regions with large regional executive bureaucracies does not appear to be offset by a need to obtain more licenses.

7 Conclusion

In principle, large bureaucracies may work for or against economic reforms designed to reduce the role of the state. Taking advantage of large variation across Russian regions in the size of public administrations, we find strong evidence that privatization had a larger (more positive) impact on firm performance where regional executive bureaucracies were large. Our analysis suggests that this effect was driven primarily by the impact of state bureaucracy on the post-privatization business environment, rather than on the process of privatization itself.

Scholars accustomed to viewing bureaucrats as inefficient and venal may be surprised by our finding of a positive relationship between bureaucracy size and privatization effectiveness. Yet inefficiency does not imply that bureaucrats are incapable of providing institutional support for reform, but merely that bureaucratic capacity may need to be augmented. With respect to venality, our results point especially to an incentive effect of bureaucracy size, where rent seeking by any individual bureaucrat is discouraged when bureaucracies are large. One normative implication of this analysis is that the recent emphasis on providing better compensation to fewer bureaucrats may be misplaced. Making more officials available to individuals with business before the state may be the best way to assure that state authority is not misused.

Strong policy recommendations await further research, however, with a need to examine the impact of bureaucracy size in other policy and political contexts. Moreover, greater attention must be given to the direct cost of building a large bureaucracy: this paper has focused only on the impact of bureaucracy size on reform outcomes. The results of this research will help to illuminate the tradeoffs involved in choosing the optimal size of the state.

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Table 1: **Firm-level variables and summary statistics**

	1985	1992	1998	2004
Output	497	617	360	584
	(1,464)	(5,783)	(4,546)	(6,083)
Employment	781	779	438	452
	(2,563)	(2,537)	(1,728)	(1,798)
Capital stock	339	562	664	999
	(1,399)	(3,218)	(15,599)	(21,611)
Domestic private	0.000	0.000	0.693	0.659
Foreign private	0.000	0.000	0.009	0.012

Notes: Means and standard deviations. Capital and output are expressed in constant 2004 mln rubles. Output equals the value of gross output net of VAT and excise taxes. Employment equals the average number of registered industrial production personnel, which includes non-production workers, but excludes "nonindustrial" employees who mainly provide employee benefits. Capital equals the average book value of fixed assets used in the main activity of the enterprise, adjusted for revaluations. The domestic and foreign private dummies are based on Rosstat ownership classifications as of December 31st of the previous year. The nonmonotonicity of the mean of the domestic private dummy is due to split-ups of state-owned firms that are subject to later privatization.

Table 2: Variable definitions, sources, and summary statistics for region-level variables

Panel A: Definition and sources for regressors

Variable	Definition and/or source
Bureaucracy	Public administration employees per 1000 residents, *Regiony Rossii*, Rosstat
Population	*Regiony Rossii*, Rosstat
Income per capita	*Regiony Rossii*, Rosstat
Urbanization	Urban population as proportion of total population, *Regiony Rossii*, Rosstat
Autonomous region	Dummy variable = 1 if region is republic or autonomous oblast
Population density	Residents per square kilometer, *Regiony Rossii*, Rosstat
Jurisdictions	100s of *raiony* + *goroda*, *Regiony Rossii*, Rosstat

Panel B: Summary statistics

	Mean	Std. Dev.	Min	Max
Regressors				
Log bureaucracy	2.175	0.243	1.514	2.883
Log federal executive bureaucracy	1.126	0.259	0.588	2.049
Log regional executive bureaucracy	1.512	0.253	0.578	2.246
Log bureaucracy, other branches	0.119	0.319	−0.653	1.359
Log population	7.258	0.786	5.303	9.094
Log income per capita	−1.987	0.330	−2.555	−0.982
Urbanization	0.698	0.124	0.270	1.000
Autonomous region	0.260	0.441	0	1
Log population density	2.950	1.581	−1.029	9.010
Jurisdictions	0.379	0.198	0.020	1.130
Estimated effects				
Privatization effect	−0.089	0.148	−0.463	0.401
State-ownership effect	1.321	0.176	0.683	1.608
Marginal privatization effect for small firms	0.080	0.333	−0.551	1.927

Note: Privatization effect, state-ownership effect, and marginal privatization effect for small firms estimated from firm-level data summarized in Table 1; see text for details.

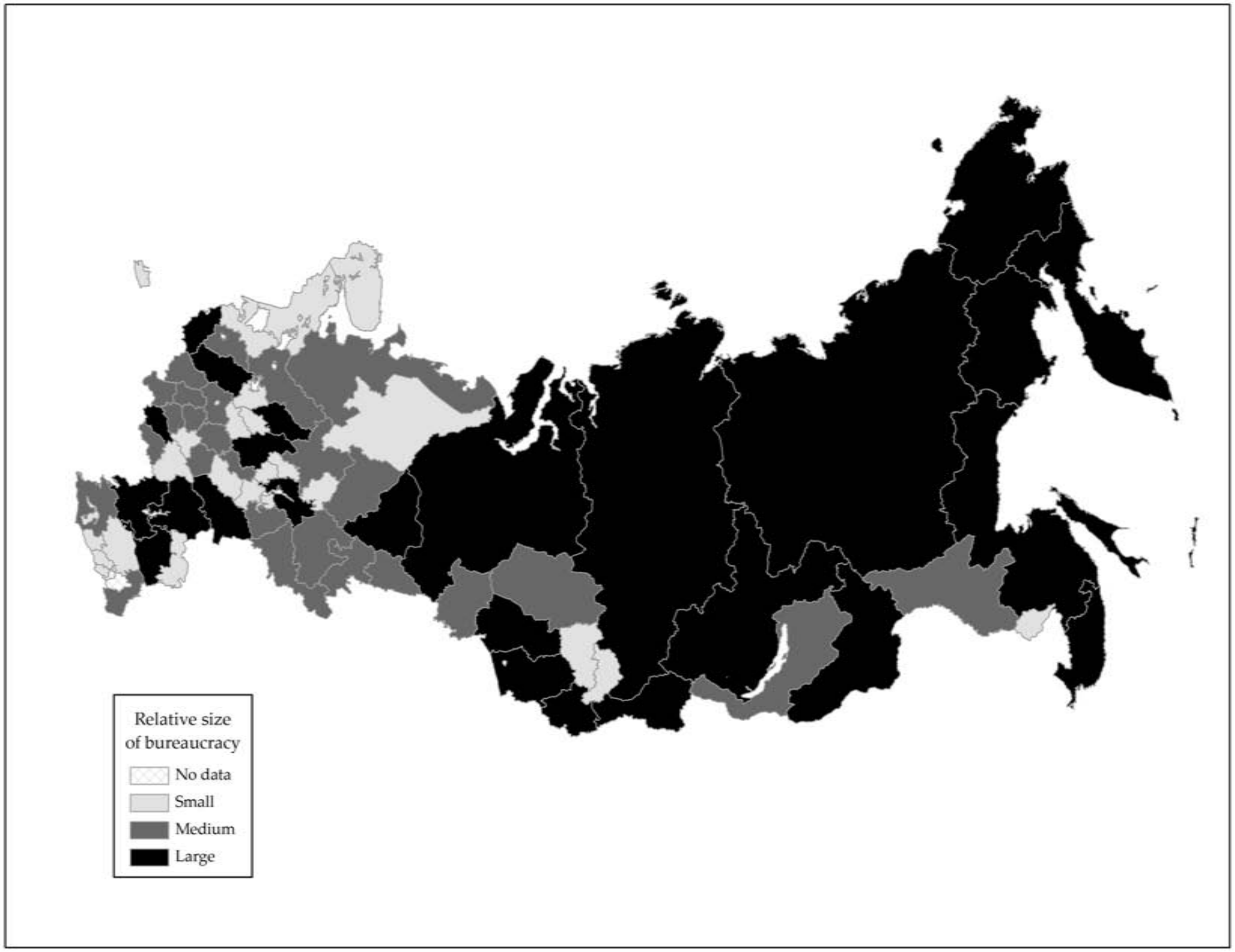

Figure 1: Regional variation in state bureaucracy

Notes: "Small," "medium," and "large" are defined as the corresponding terciles of the residuals from a regression of log public-administration employment on log population.

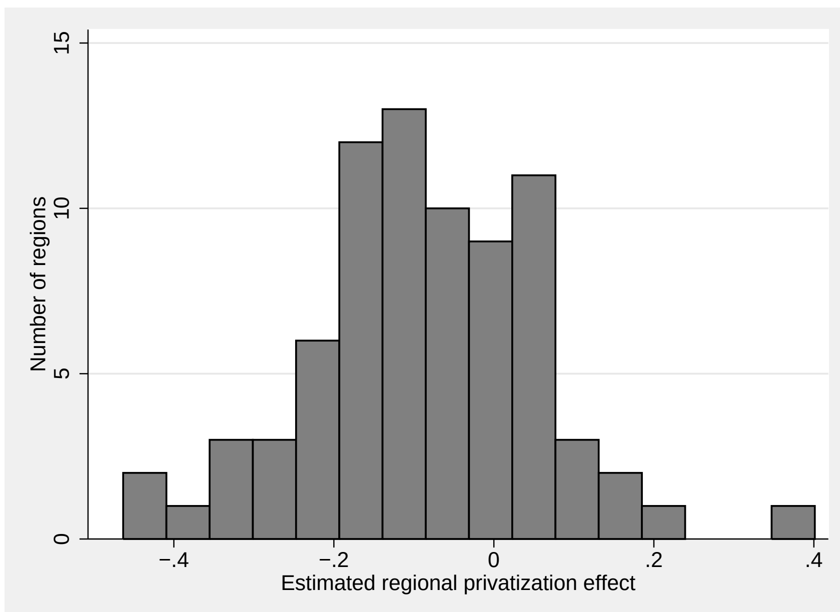

Figure 2: Frequency distribution of estimated privatization effects

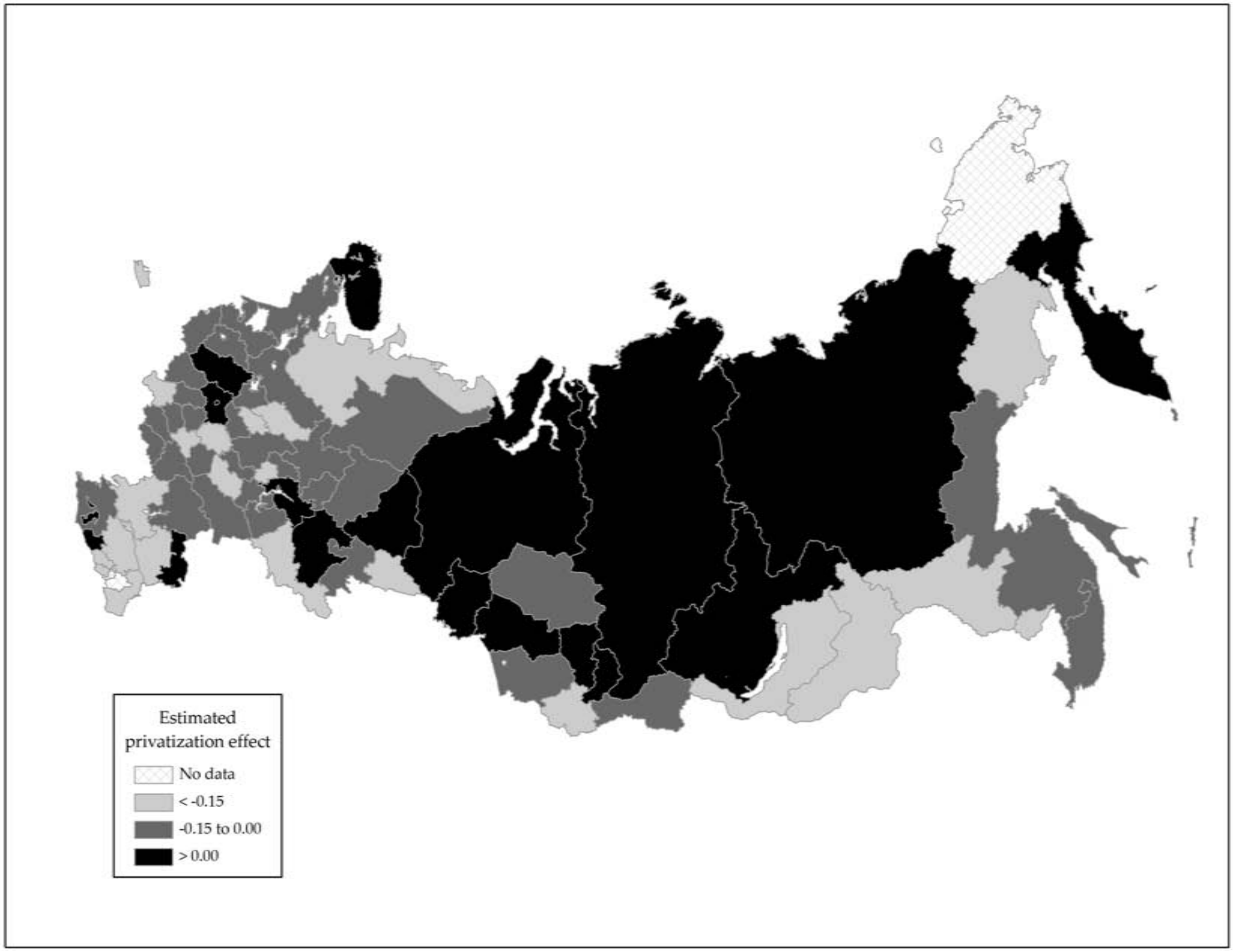

Figure 3: Geographic distribution of estimated privatization effects

Notes: Estimated productivity effect of domestic privatization from firm-level FE&FT regression. Estimation controls for industry-specific privatization effects, such that regional variation in industrial structure is not responsible for regional variation in privatization effects, as well as industry-year interactions.

Table 3: State bureaucracy and privatization effectiveness

	OLS		FGLS		2SLS	
	Estimated coefficient	Standard error	Estimated coefficient	Standard error	Estimated coefficient	Standard error
Log bureaucracy	0.296**	0.144	0.351***	0.124	0.424**	0.183
Log population	0.127***	0.045	0.141***	0.039	0.162***	0.052
Log income per capita	0.012	0.063	−0.001	0.051	−0.008	0.066
Urbanization	0.455**	0.216	0.458***	0.154	0.506**	0.231
Autonomous region	0.089**	0.038	0.110***	0.039	0.102***	0.036
Constant	−1.971***	0.717	−2.219***	0.611	−2.584***	0.907
Log population density (first stage)					−0.049***	0.008
Jurisdictions (first stage)					0.215**	0.098
F-statistic, excluded instruments (p-value)					22.80 (0.000)	
Hansen J statistic (p-value)					0.514 (0.437)	

Notes: Dependent variable is estimated privatization effect from firm-level FE&FT regression. Log population density and number of jurisdictions used as instruments in 2SLS regression. Heteroskedasticity-robust standard errors reported for OLS and 2SLS regressions. See text for details on FGLS estimator. Significance levels: *** = 0.01, ** = 0.05, * = 0.10.

Table 4: The impact of state bureaucracy on state-owned enterprises

	OLS		FGLS		2SLS	
	Estimated coefficient	Standard error	Estimated coefficient	Standard error	Estimated coefficient	Standard error
Log bureaucracy	0.310**	0.146	0.272**	0.124	0.347**	0.149
Log population	0.059	0.042	0.061	0.040	0.070	0.048
Log income per capita	0.058	0.054	0.061	0.051	0.052	0.056
Urbanization	0.781***	0.213	0.788***	0.155	0.796***	0.234
Autonomous region	−0.056	0.042	−0.056	0.039	−0.052	0.042
Constant	−0.198	0.609	−0.128	0.641	−0.378	0.743
Log population density (first stage)					−0.049***	0.008
Jurisdictions (first stage)					0.215**	0.098
F-statistic, excluded instruments (p-value)					22.80 (0.000)	
Hansen J statistic (p-value)					0.672 (0.412)	

Notes: Dependent variable is estimated state-ownership effect from firm-level OLS regression. Log population density and number of jurisdictions used as instruments in 2SLS regression. Heteroskedasticity-robust standard errors reported for OLS and 2SLS regressions. See text for details on FGLS estimator. Significance levels: *** = 0.01, ** = 0.05, * = 0.10.

Table 5: State bureaucracy and privatization effectiveness: branch and subordination

	OLS		FGLS	
	Estimated coefficient	Standard error	Estimated coefficient	Standard error
Log federal executive bureaucracy	−0.051	0.167	−0.006	0.144
Log regional executive bureaucracy	0.242***	0.089	0.256***	0.095
Log bureaucracy, other branches	0.062	0.099	0.051	0.111
Log population	0.105**	0.051	0.120***	0.043
Log income per capita	−0.003	0.063	−0.010	0.052
Urbanization	0.557**	0.225	0.551***	0.173
Autonomous region	0.076*	0.040	0.097**	0.042
Constant	−1.579**	0.615	−1.774***	0.516

Notes: Dependent variable is estimated privatization effect from firm-level FE&FT regression. Heteroskedasticity-robust standard errors reported for OLS regression. See text for details on FGLS estimator. Significance levels: *** = 0.01, ** = 0.05, * = 0.10.

Table 6: State bureaucracy and privatization effectiveness: privatization-implementation hypothesis

	Privatization propensity				Small vs. large			
	OLS		FGLS		OLS		FGLS	
	Estimated coefficient	Standard error	Estimated coefficient	Standard error	Estimated coefficient	Standard error	Estimated coefficient	Standard error
Log federal executive bureaucracy	0.123	0.159	0.068	0.137	−0.362	0.370	−0.174	0.325
Log regional executive bureaucracy	0.196*	0.099	0.205**	0.101	−0.196	0.243	−0.194	0.243
Log bureaucracy, other branches	−0.071	0.119	−0.036	0.128	0.599	0.328	0.438	0.304
Privatization propensity	0.154	0.229	0.036	0.167				
Log population	0.103**	0.044	0.108**	0.042	−0.150	0.108	−0.141	0.101
Log income per capita	0.019	0.070	0.007	0.054	0.056	0.141	0.055	0.127
Urbanization	0.561**	0.224	0.558***	0.186	−0.262	0.459	−0.208	0.429
Autonomous region	0.112*	0.056	0.106**	0.051	−0.231**	0.124	−0.187	0.119
Constant	−1.733***	0.555	−1.666***	0.562	2.288*	1.335	1.947	1.255

Notes: Dependent variable in first two models is estimated privatization effect from firm-level FE&FT regression. Dependent variable in second two models is estimated marginal privatization effect for small firms from firm-level FE&FT regression. Heteroskedasticity-robust standard errors reported for OLS regressions. See text for details on FGLS estimator. Significance levels: *** = 0.01, ** = 0.05, * = 0.10.

Table 7: Summary statistics for Monitoring and BEEPS data

Monitoring data:

	Mean	Std. Dev.	Min	Max
Log average time elapsed to license	3.330	1.176	0.000	5.900
Log maximum time elapsed to license	3.548	1.165	−0.693	7.090
Log firm's person-hours to get license	2.617	1.319	−2.996	6.397
Log average license price	8.786	1.768	1.609	19.742

BEEPS data:

	Mean	Std. Dev.	Min	Max		
Bribes to bureaucrats	1.099	1.702	0.000	10.000		
Kickbacks to bureaucrats	1.973	4.332	0.000	30.000		
	1	2	3	4	5	6
Contract/property-rights enforcement	81 (16.0%)	91 (18.0%)	154 (30.5%)	114 (22.6%)	46 (9.1%)	19 (3.8%)
Appeal administrative violations	100 (20.1%)	137 (27.5%)	142 (28.5%)	62 (12.5%)	40 (8.0%)	17 (3.4%)

Notes: Full wording of Monitoring questions: "On average, how many days elapsed / What is the maximum time [in days] elapsed between the start of the licensing or authorization procedure and the moment you were officially notified that your request had been approved or denied?" "On average, how much time [in person-days] was spent by you and personnel of your firm filling out forms and contacting licensing officials?" "On average, how much did you pay to receive a license or authorization (including official payments, payments to intermediaries and consultants, gifts, etc.)?" " Full wording of BEEPS questions: "On average, what percent of total annual sales do firms like yours typically pay in unofficial payments/gifts to public officials?" "When firms in your industry do business with the government, what percent of the contract value would be typically paid in additional or unofficial payments/gifts to secure the contract?" "I am confident that the legal system will uphold my contracts and property rights in business disputes." "If a government agent acts against the rules I can usually go to another official or to his superior and get the correct treatment without recourse to unofficial payments/gifts." Possible responses for contract/property-rights enforcement: "strongly disagree" (1), "disagree in most cases" (2), "tend to disagree" (3), "tend to agree" (4), "agree in most cases" (5), "strongly agree" (6). Possible responses for appeal administrative violations: "never" (1), "seldom" (2), "sometimes" (3), "frequently" (4), "usually" (5), "always" (6).

Table 8: State bureaucracy and post-privatization business environment: Monitoring data

	Log average time elapsed to license		Log maximum time elapsed to license		Log firm's person-hours to get license		Log average license price	
	Estimated coefficient	Standard error	Estimated coefficient	Standard error	Estimated coefficient	Standard error	Estimated coefficient	Standard error
Regional characteristics								
Log federal executive bureaucracy	−0.063	0.548	0.430	0.552	−0.649	0.826	0.464	0.928
Log regional executive bureaucracy	−1.324***	0.345	−1.077***	0.342	−0.437	0.491	−1.250**	0.576
Log bureaucracy, other branches	1.085**	0.493	0.731	0.494	0.740	0.728	−0.341	0.817
Log population	0.037	0.213	0.191	0.214	−0.060	0.302	−0.406	0.354
Firm characteristics								
Log employment	0.187***	0.058	0.213***	0.058	0.208***	0.068	0.175*	0.098
Industry (other than food processing)	−0.023	0.233	−0.058	0.233	0.110	0.280	0.187	0.383
Food processing	−0.500*	0.291	−0.590**	0.294	−0.018	0.352	−0.242	0.497
Services	0.037	0.218	−0.028	0.220	0.029	0.267	0.305	0.369
Trade	−0.424**	0.200	−0.541***	0.202	−0.277	0.244	−0.532	0.333
Agriculture	−0.190	0.362	−0.162	0.385	−0.274	0.439	0.819	0.602
Construction	0.194	0.243	−0.082	0.242	0.012	0.293	−0.029	0.393
Information technology	0.643**	0.275	0.495*	0.278	0.305	0.332	0.266	0.455
Constant	4.553**	2.053	2.787	2.068	3.780	2.922	12.885***	3.418
N	412		405		405		376	
R-squared between	0.633		0.515		0.249		0.381	

Notes: Random-effects regressions, with random effects specified at level of region (20 total). Excluded category is other sectors. Significance levels: *** = 0.01, ** = 0.05, * = 0.10.

Table 9: State bureaucracy and post-privatization business environment: BEEPS data

	Bribes to bureaucrats		Kickbacks to bureaucrats		Contract/property-rights enforcement			Appeal administrative violations		
	Est. coef.	Std. error	Est. coef.	Std. error	Est. coef.	Std. error	Marg. effect	Est. coef.	Std. error	Marg. effect
Regional characteristics										
Log federal executive bureaucracy	−0.674	1.266	−1.175	2.192	−0.276	0.510	−0.102	0.223	0.515	0.068
Log regional executive bureaucracy	−0.281	0.710	−3.030**	1.354	0.337	0.283	0.125	0.032	0.281	0.010
Log bureaucracy, other branches	−0.172	1.298	−2.531	2.266	−0.861	0.525	−0.319	0.949*	0.535	0.291
Log population	−0.506	0.543	−1.737*	0.975	−0.045	0.229	−0.017	0.245	0.235	0.075
Firm characteristics										
Log employment	−0.004	0.053	0.063	0.139	0.052	0.032	0.019	0.028	0.032	0.009
Mining	0.783	0.663	0.152	1.708	0.143	0.390	0.054	0.024	0.380	0.008
Construction	0.328	0.346	2.650***	0.911	−0.167	0.204	−0.060	−0.333	0.206	−0.093
Manufacturing	−0.004	0.310	0.211	0.841	−0.007	0.184	−0.003	−0.149	0.188	−0.044
Transport	0.374	0.418	0.390	1.134	−0.209	0.246	−0.074	−0.008	0.252	−0.002
Wholesale/retail trade	−0.128	0.294	0.794	0.789	−0.076	0.176	−0.028	−0.220	0.178	−0.065
Business services	−0.123	0.352	0.368	0.923	0.026	0.209	0.010	−0.060	0.218	−0.018
Hotels/restaurants	−0.033	0.493	−0.188	1.235	−0.201	0.304	−0.071	−0.534*	0.286	−0.133
Other parameters										
Constant/cutpoint 1	6.280	5.612	20.434**	10.016	−1.136	2.352		1.359	2.408	
Cutpoint 2					−0.541	2.350		2.153	2.408	
Cutpoint 3					0.254	2.350		2.935	2.408	
Cutpoint 4					1.022	2.352		3.435	2.410	
Cutpoint 5					1.674	2.355		4.061	2.415	
N	499		466		505			498		
R-squared between/log likelihood	0.101		0.228		-821.9			-795.1		

Notes: Random-effects (first two models) and random-effects ordered-probit (second two models, implemented with Stata package `gllamm`) regressions, with random effects specified at level of region (14 total). Marginal effects for ordered-probit models are changes (continuous or discrete) in Pr(outcome = 4) + Pr(outcome = 5) + Pr(outcome = 6). Excluded category is other services. Significance levels: *** = 0.01, ** = 0.05, * = 0.10.