Enterprise Restructuring in Transition: A Quantitative Survey

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1. *Introduction*

OVER THE LAST decade, more than 150,000 large enterprises in 27 transition countries have encountered revolutionary changes in every aspect of their political and economic environments. Some enterprises have responded to the challenge, entering world markets with great dynamism and becoming indistinguishable from their competitors in mature market economies. Many others remain mired in their past, undergoing protracted deaths, delayed at times by their slippage into a world of barter and subsidies. Thus the revolutionary changes in

transition countries have been matched by enormous variance in the degree to which enterprises have restructured their operations and responded successfully to events. With changes in the institutional and policy environment much faster and more encompassing than in virtually any other historical episode, this is as close to a policy laboratory as economics gets.

This mammoth quasi-experiment offers lessons of profound importance for economic studies and economic policy. Since the pace at which firms restructure is a fundamental determinant of economic growth, analysis of the determinants of restructuring in formerly socialist countries sheds light on the bases of economic progress. Such analysis addresses age-old questions and poses new ones. What are the relative productivities of state and private enterprises? Does mass privatization work? What is the efficiency cost of diffuse share ownership relative to block-holder ownership? Which private owners are most effective—managers, workers, banks, or investment funds? Does competition promote productivity change? Does it matter whether competitive pressure comes from foreign or domestic firms? To what degree do soft budgets dull enterprise performance? Is a strengthening of managerial incentives sufficient to inspire turnaround, or is replacement of managers necessary for revitalization?

Answers to these questions are obviously of vital significance for economic deliberations in general. But beyond this, the transition process is important in and of itself because of its geographical scope, the large changes in levels of economic well-being in the last decade, and the ramifications for the world economy and polity. Analysis of enterprise restructuring is central in any effort to understand the effects of the most important reform measures adopted in transition countries. With enterprise restructuring apparently more successful in some countries than others, the natural question that arises is whether such differences relate systematically to policy. In this paper, we address this question by examining how the effects of policy have varied between transition countries.

Our objective is to survey the evidence on the determinants of enterprise restructuring in transition.[2] We provide such a synthesis, summarizing the composite conclusions from more than one hundred empirical studies. Where possible, we compare the results from the transition literature with those from studies of market economies.

With such a large body of literature under review, it is necessary to pay special attention to the methodology of synthesis. Because there are so many results, verbal description alone would result in a hard-to-remember list. An interpretative summary presents its own dangers. Bayesian priors might come to weigh too heavily in the synthesis, a danger that is all too great in the transition arena, where the contentiousness of the subject has encouraged forthright statements. Indeed, we have made such statements, although the reader might be reassured to note that our priors to some extent cancel (Murrell 1992; Gerhard Pohl et al. 1997).

In view of these factors, we adopt more routinized methods of synthesizing the evidence, drawing on insights from meta-analysis, which has long been in use in other disciplines, particularly bio-medicine, psychology, and education (Morton Hunt 1997).[3] Apart from making our methods of synthesis transparent, application of meta-analysis has several other advantages. First, it provides tools to aggregate the results of many studies on a similar topic, combining many tests with weak power to produce a single one with larger power. Second, these methods allow one to test hypotheses across groups of studies. For example, we examine whether the replacement of managers is more effective than the addition of incentives, and we test whether privatization has stronger results in Eastern Europe than in the former Soviet Union. Third, the synthesis of results can address the thorny issue of differences in the quality of studies, allowing one to gauge the extent to which the conclusions change when one gives greater weight to those studies that are methodologically sounder.

We find that privatization is strongly associated with more enterprise restructuring. Economic effects are quite often very large, for example adding several percentage points to enterprise growth rates. The privatization effect is, however, statistically insignificant in the Commonwealth of Independent States (CIS). These results are robust. They hold when we vary the emphasis assigned to the results of different studies by using weights that reflect the differing attention paid to selection bias or the overall quality of analysis.

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[2] The previous survey papers in this area, Josef Brada (1996) and Oleh Havrylyshyn and Donal McGettigan (2000), used quite limited empirical evidence, which came almost exclusively from Central Europe and China. Now studies of other countries (the former Soviet Union, Mongolia, and Vietnam) are becoming numerous, providing a much wider variety of evidence. John Nellis (1999) covers the full range of countries; here we provide a more systematic summary of the evidence and focus on a wider set of determinants of enterprise restructuring.

[3] Examples of recent use of meta-analysis in economics are Kerry Smith and Ju Chin Huang (1995), Smith and Yoshiaki Kaoru (1990), David Neumark and William Wascher (1998), Joseph Phillips and Ernest Goss (1995), and Thomas Stanley (1998).

The survey also documents the effects of different types of owners on enterprise restructuring. We find that state ownership within traditional state firms is less effective than all other ownership types, except for worker-owners, who have a negative effect. Privatization to outsiders is associated with 50 percent more restructuring than privatization to insiders (managers and workers). Investment funds, foreigners, and other block-holders produce more than ten times as much restructuring as diffuse individual ownership. State ownership within partially privatized firms is surprisingly effective, producing more restructuring than enterprise insiders and non-block-holder outsiders. The effects of different owners vary between regions. Workers are better owners in Eastern Europe than in the CIS, while banks and concentrated individual ownership are significantly more effective in the CIS than elsewhere.

Product market competition has a significant effect in improving enterprise performance. The economic effects can be large, with a typical study indicating that enterprises in highly competitive sectors are 20–30 percent more productive than monopolies. The sources of improvement differ between regions, however. In Eastern Europe, the beneficial effect comes mainly through import competition but is also evident through domestic competition. In contrast, in the CIS domestic competition is sometimes statistically insignificant while import competition generally has a negative effect on enterprise restructuring.

We next explore the link between enterprise restructuring and the hardening of budget constraints. The evidence is consistent with the view that hardened budget constraints have had a beneficial effect on enterprise restructuring in Eastern Europe and the CIS. The effect in the CIS is economically larger than that in Eastern Europe.

We also examine whether management turnover—or more broadly, bringing in new human capital—is associated with improved enterprise performance. Statistical analyses show that this is the case in both Eastern European and CIS countries. We find evidence that the strengthening of managerial incentives leads to a greater amount of restructuring, in economic terms.

Our review of existing research then leads to suggestions on three priority areas for future research. The design of the studies we survey was not driven by attempts to address difficult methodological problems like selection bias or simultaneity, or by efforts to examine the effects of less easily quantifiable aspects of policy, such as the quality of institutional construction, or by considerations that viewed transition in the wider context of development. In the future, research should focus on these issues.

We conclude by comparing the strength of the effects of the different policy reforms examined in this paper. Privatization to outsiders is associated with the largest restructuring gains, while privatization to workers has no effect in Eastern Europe and is detrimental in the CIS. Hardened budget constraints are the second most important determinant in the CIS, while the establishment of competition is second in Eastern Europe.

The paper is organized as follows. Section 2 lays out the nature of the studies under review, in general terms. Section 3 investigates the empirical evidence on whether state-owned or privatized firms undertake more economic restructuring. Section 4 studies the effects of different types of owners. Section 5 links product market competition and enterprise restructuring efforts. Section 6 analyzes the role of soft-budget constraints in limiting productivity enhancements. Section 7 documents the role of managers, focusing on management turnover and manager incentives. Section 8 develops the future research agenda. Section 9 concludes.

2. *The Nature of Reviewed Studies: Enterprise Restructuring and Its Determinants*

What is enterprise restructuring and what changes might induce it in transition countries? The perspective taken here is found at the core of every paper that we survey. The enterprises initially functioned in socialist economies, and their behavior was a product of the institutions and policies of those economies. In the 1990s, those institutions and policies changed radically. These changes compelled enterprises to adapt their behavior in order to survive, and perhaps to succeed, in a new, liberalized, market environment. The term enterprise restructuring has come to denote the whole process undertaken by enterprises as they adapt for survival and success in a market economy.[4]

The search for possible determinants of enterprise restructuring therefore lies in those institutions and policies that changed most rapidly in the early years of transition. To identify these, it is useful to reflect on the central characteristics of the socialist economy and its enterprises. These have been widely discussed in the literature and we only need to reiterate a few central issues.[5]

The classical socialist enterprise was by definition state-owned and was oriented to an input-output plan rather than any market. Meeting the plan was of prime importance and the plan was normally very ambitious. Therefore, production issues dominated entrepreneurship, marketing, and cost minimization in managerial concerns. Consistently, the typical manager was a production engineer and not a businessman. Managers faced a mix of monetary and career-based incentives, which were a function of plan fulfillment, enterprise performance, and political loyalty.

Profits and efficiency were much less important than they are under capitalism.

The enterprise was organized along very hierarchical lines. Workers had virtually no role in enterprise decision making, except in personnel policy, leading to firing rates that were extremely low by any standard (David Granick 1987). The enterprise was not only a producer of goods, but also played an important role in implementing the state's social welfare policies. For these reasons, efficiency considerations were often secondary in determining the size of the workforce.

A labyrinthine bureaucracy replaced the institutions and markets of capitalism. Bureaucratic pressure substituted for competition. The bureaucracy found customers and determined prices, interceding between producer and buyer. It generated contracts and enforced them. Its one-year plans guaranteed short-term working capital, and its investment projects automatically received long-term credits. Given the ubiquitous role of the state, much was decided by negotiation. One consequence of the frequency of these negotiations was the universal presence of soft budgets, which further turned attention away from profits and efficiency.

Pre-transition reforms did change this standard picture in a few countries, notably Yugoslavia (which was centralized only for a few years), Hungary, and Poland (Leszek Balcerowicz 1995, and Kornai 1986). Decentralizing reforms reduced the scope of bureaucratic decision making. Markets and competition increased in importance. Enterprises came closer to ultimate consumers. Paradoxically, however, abandonment of formal planning led to increased bargaining between bureaucracy and enterprise, further softening budgets. Notably also, workers gained more power within enterprises, acquiring experience at being informal owners.

In sum, the pre-transition enterprises were state-owned, protected from competition, shielded from failure by soft budgets, and managed by production engineers with incentives oriented toward the plan or politics.

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[4] See, for example, Phillippe Aghion and Mark Schankerman (1999), Roman Frydman et al. (2000), and Lubomir Lizal, Miroslav Singer, and Jan Svejnar (2001).

[5] See Joseph Berliner (1976), Murrell (1990), and Janos Kornai (1990) for details.

The enterprises were embedded in a set of institutions, for contracting, for financing, for governance, that were far different from those of a market economy. Transition policies aimed to change all of this. Therefore, the literature with which we are concerned emphasizes the following determinants of restructuring: ownership, competition, soft budgets, managerial incentives and characteristics, and broader institutional changes.

The widespread liberalization meant that enterprises had to adapt in order to survive and prosper in markets that were increasingly contestable, the process of change that the literature conventionally defines as enterprise restructuring. In some studies, the measurement of restructuring focuses directly on enterprise decisions, for example, changes in the structure of corporate governance and management (Saul Estrin and Adam Rosevear 1999a) or renovation of factories (Djankov 1999a), or investment rates (Irena Grosfeld and Jean-Francois Nivet 1999). But more usually, the degree of enterprise restructuring has been captured by performance, with performance measured by variables that are objectives of companies in market economies and that were less important for socialist enterprises. Thus, productivity (e.g., Stephen Smith, Beom-Cheol Cin, and Milan Vodopivec 1997; and Young Lee 1999) or profits (e.g., Stijn Claessens and Simeon Djankov 1999a; and Estrin and Rosevear 1999a) are often used. But sales or revenue have also been used extensively (e.g., Frydman et al. 1999; and Derek Jones 1998) under the premise that the ability to hold on to customers or to find new ones is an indicator of successful change within the enterprise, especially when accomplished in the face of steep recessions (Frydman et al. 1999).[6] There seems to be no consensus in the literature concerning which variables are the best measures of restructuring, apart from a greater preference for measures of performance than for indexes of internal decisions.[7]

The typical study relates these restructuring measures to their determinants, estimating an equation of the form:

$$Y = \alpha + X\,\beta + \gamma\,P + \varepsilon \qquad (1)$$

where the enterprise is the unit of observation, Y is the restructuring measure, P captures some aspect of the reforms to which the enterprise is subject, e.g., ownership change, degree of hardness of the budget, etc., X is a vector of other enterprise characteristics, and ε is an error term. γ is the parameter of direct interest. The overwhelming majority of papers justifies their exact choice for (1) pragmatically, rather than presenting a structural derivation.

The studies we examine use data from medium-large and large enterprises. These enterprises were the core of the socialist economy, and when liberalization came they remained going concerns, leading to some continuity in operations and personnel. This continuity facilitated data collection. In contrast, smaller enterprises were notoriously weak under socialism and soon were swamped by new entrants. They often vanished, with their assets resurfacing in new activities, with new personnel. Data collection for smaller enterprises, therefore, faced enormous difficulties, leading to few studies examining the progress of the small enterprises established before transition.

The paragraphs above emphasize the common elements of the studies under review. We now turn to important ways in which studies differ. The cataloging of these differences and the determination of their effect on results are important features of our survey. We focus on three such variations in the characteristics of studies:

[6] There is wide variation in whether levels or growth rates are used. This decision often depends on the availability of data and also reflects whether lagged dependent variables are included as explanatory variables in the equation. The differences between levels and growth specifications diminish greatly with the inclusion of lagged dependent variables.

[7] The lack of consensus probably results from the difficulties of obtaining data, with researchers using the best measure available.

measurement of Y, selection bias, and over-all quality.

Many varieties of Y appear in the studies, but one distinction, already suggested above, is worth emphasizing. One category of Y comprises quantitative indicators of enterprise performance, which are based on accounting information. Other Y's are somewhat softer, perhaps derived from survey questions on economic performance posed to managers, e.g., forecasts of sales in the surveyed year as in John Earle (1998), or using information collected about reorganization, e.g., the introduction of new products as in Wendy Carlin et al. (2001), or reflecting operational factors only indirectly related to current performance, e.g., the extent of wage arrears, as in Hartmut Lehmann, Jonathan Wadsworth, and Alessandro Acquisti (1999). We will refer to these two types of indicators as quantitative and qualitative.[8]

The prevailing sentiment in the literature is that the quantitative variables are to be trusted more (even with the misreporting and accounting difficulties that are rife in transition countries). They measure directly the prime output of enterprise restructuring: economic performance. On the other hand, there is also the view that quantitative performance might suffer when an enterprise is investing in large-scale reorganization and that the results of this process might be observed earliest in qualitative variables. We focus primarily on the quantitative indicators in this paper, deeming them more reliable. Where a sufficient number of studies is available, however, we examine both types.

A second crucial variation between papers lies in the degree of attention paid to possible biases in the estimates of γ due either to selection effects or simultaneous causation.[9] Typical cases of selection bias occur when either the level of ownership or the use of managerial incentives is systematically related to some unobserved enterprise characteristic that also affects Y. Examples of simultaneous causation are when an enterprise's market share is being used to measure competition or subsidies are being used to measure soft budgets: in each case, the measured determinant of restructuring reflects enterprise performance. These problems have been thoroughly recognized in the literature, but solutions are not always easy to obtain. Thus, for example, in the studies examining private versus state ownership in section 3, only 53 percent of the estimates of γ employ methods that might serve to counter selection bias.

The prevailing evidence suggests biases due to selection effects or simultaneous causation are a real possibility. For example, Sweder van Wijnbergen and Anton Marcinin (1997) show that selection into Czechoslovakia's voucher program was nonrandom and that this must be taken into account in ascertaining the effects of the voucher program on outcomes. Moreover, when there are OLS estimates and estimates employing some technique to counter bias in the same paper, they quite often differ considerably, suggesting the presence of bias. (See, for example, David Brown and John Earle 2000; Rachel Glennerster 2000; James Anderson, Lee, and Murrell 2000; and Earle 1998.)

The fact that some studies have identified nontrivial selection or simultaneity bias suggests that we must be sensitive to its presence. Hence, in every section of this paper, we present conclusions that allow the reader to understand how the synthesis of results might have been affected by these biases. We present sufficient information to allow each reader to judge which of the broad

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[8] We do not use indicators for which there is substantial disagreement in the literature on whether the sign of γ should be positive or negative. The most pertinent example is employment, whose direction of change would depend very much on the extent of excess labor under the old regime.

[9] Our use of the term selection bias might not be wholly consistent with the precise terminology of econometrics. We simply follow the literature.

conclusions emanating from this literature fall under the shadow of selection bias. How we do this is best explained in a practical context, in the next section. But it must be emphasized that we have directly confronted issues of selection or simultaneity biases in reaching our conclusions, that we draw back from stating strong conclusions where we feel the possibility of such bias leaves some residual doubt, and that nevertheless there are many strong conclusions that we can reach.

A third variation between papers on which we focus is quality of analysis. One of the primary objections to the application of meta-analysis hinges on the fact that the quality of empirical work varies greatly across papers, meaning that a simple aggregation of results might give undue weight to lesser quality research. Therefore, some scholars prefer to focus reviews of empirical literature on the high points, ignoring papers that are less convincing from a methodological standpoint. However, it is also possible to take a middle road, one that examines whether the composite results change when considerations of quality are taken into account. This is our approach. How we do this is, again, best left until our methods can be explained in context in the next section.[10]

3. *State versus Private Ownership*

In early transition debates, there was agreement on the goal of an economy dominated by private ownership, but conflicting views on how best to attain this, through fast privatization (Jeffrey Sachs 1992; Maxim

Boycko, Andrei Shleifer, and Robert Vishny 1995) or through stimulation of a nascent private sector (Kornai 1990; Murrell 1992, 1995). The relative emphasis on the differing strategies waxed and waned with events. With deep crisis in Eastern Europe in the early 1990's, fast privatization gained emphasis. However, after the recovery of Poland, a relatively slow privatizer, that emphasis declined (Brian Pinto, Marek Belka, and Stefan Krajewski 1993; Phillippe Aghion, Olivier Blanchard, and Robin Burgess 1994; and Brada 1996). But Poland is only one of many transition countries, an outlier at that. Now there are fast privatizers performing well (Estonia) and fast privatizers performing badly (Russia), with similar variation across slow privatizers (Poland versus Romania), giving sustenance to a variety of opinions about the results of privatization (Pohl et al. 1997; Havrylyshyn and McGettigan 2000; Joseph Stiglitz 1999; Bernard Black, Reinier Kraakman, and Anna Tarassova 2000). To referee this debate, it is necessary to turn to the microeconomic empirical literature.[11]

Studies examining whether privatized enterprises perform better than state-owned enterprises use equation (1), with P a measure of the degree of private ownership of an enterprise of pre-1989 vintage. A large variety of variables takes the place of Y, X, or P with no consensus on the nature of the equation to be estimated. For Y, a significant number of papers use measures of output levels, captured, for example, by sales (e.g., Jozef Konings 1997; and Earle 1998) or value added (e.g., Anderson, Georges Korsun, and Murrell 2000; and Smith, Cin, and Vodopivec 1997). Other papers use growth rates of the same variables (e.g., Frydman et al. 1999; and Jones 1998). Qualitative versions of Y are also extensively used, especially in studies of the former Soviet Union, where there are greater difficulties in obtaining accurate accounting data.

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[10] An earlier version of this paper (Djankov and Murrell 2000) examined other variations in the methodological characteristics of studies. It applied the methods that appear below (on quality and selection bias) also to the length of time (for the pertinent reform) that is embodied in the estimates and the number and appropriateness of the variables used in X. In the present paper, these dimensions of quality are simply absorbed into the overall measure of quality. The reader can refer to that earlier paper for the more detailed results. The data compiled for that study, except for our assessments of the overall quality of papers, are available from the authors on request.

[11] William Megginson and Jeffry Netter (2001) undertook an extensive survey of the effects of privatization worldwide. Their results are primarily germane to countries with long-established market economies.

One common form of *P* is the percentage of shares owned privately (e.g., Brown and Earle 2000). Other papers use a dummy variable capturing whether private ownership is above some threshold value, for example 0 percent in Estrin and Rosevear (1999a) and 50 percent in Derek Jones and Niels Mygind (2002). Frydman et al. (1999) and Claessens and Djankov (2000) form their dummy variables on the basis of corporate laws, the former defining a privatized enterprise as one in which private ownership is above the 33.3-percent threshold needed to block strategic company decisions, while the latter also uses the 66.7-percent level that guarantees that a state minority owner cannot veto such decisions.

Variety is also present in the *X*'s. Sectoral, regional, and (in the case of panels) time and firm dummies are common. Among the *X*'s used in various studies are measures of competition, levels of soft budgets, size of firms, access to finance, year of privatization, and many more. Pre-privatization levels of dependent variables are sometimes included under the premise that these might dampen the effects of selection bias. One common formulation includes capital and labor in the *X*'s and uses output for *Y*, with the basic equation then representing a production function and the estimate of γ capturing the effect of ownership on total factor productivity.

The variety in the formulation of estimating equations is a reflection of two factors. First, there is the absence of a single compelling theory that models the process of change within an enterprise.[12] Without such theory, specifications for estimating equations rely on ad hoc formulations. Second, the set of variables for which data are available varies greatly, with every study having deficiencies in some respect. Given these reasons for the variety of approaches, with none obviously superior to all others, it seems judicious to include a wide range of studies in drawing general conclusions.

The similarities and differences between two papers (Frydman et al. 1999; and Anderson, Lee, and Murrell 2000) exhibit the methodological decisions to be made when conducting such studies and the variations in results that can be obtained. Frydman et al. examine the performance from 1990 to 1993 of a panel of 218 privatized and state firms from the Czech Republic, Poland, and Hungary, while Anderson et al. focus on 1995 data for 211 privatized (including partially state-owned) Mongolian enterprises. Data collection, including sample design, was carried out specifically for each of these studies, raising the quality, extensiveness, and appropriateness of the information collected but resulting in small sample sizes. Each study examines the effects of privatization as a whole and the effects of different owners. We discuss the latter in the next section.

Both studies wrestle with the choice of the dependent variable, how to measure ownership, which *X*'s to include, and how to counter selection bias. Their decisions differ a great deal. Frydman et al. use four different dependent variables, rates of growth of revenues, employment, revenues per employee, and costs per unit of revenue.[13] Their ownership variable is a dummy. As controls, they use initial levels (not growth rates) of the four performance measures, accompanied by sectoral, country, and time dummies. Selection bias is parried in a number of ways, primarily by methods developed for the analysis of treatment effects in panels. In separate analyses, the authors use a dummy variable (in *X*) capturing pre-privatization differences between state firms and privatized firms; they employ a firm fixed-effects model, and they verify that performance of those firms slated for privatization, but not yet privatized, is closer to that of state firms than privatized ones. The

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[12] Additionally, there is also no agreed theory on how to model the selection of firms for privatization.

[13] A major aspect of Frydman et al. (1999) is the effect of privatization on different dependent variables. For reasons of space, we cannot examine this interesting issue here.

cumulative effect of these analyses is to convince the reader that the privatization effects are real, rather than an artifact of selection for privatization.

Enterprise record-keeping during Mongolia's early transition was so poor that Anderson et al. could not obtain panel data. Hence, they focus on performance in one year, using three different dependent variables, total factor productivity (within a Cobb-Douglas production function), sales per employee, and value-added per employee. The equations for the latter two variables include a lagged dependent variable, thus nesting a specification in which growth is the dependent variable. Since the estimated coefficients on the lagged dependent variables are significantly different from one, this suggests that growth measures are not suitable dependent variables. The ownership variable is the percentage of enterprise shares held by non-state owners. Controls include regional and sectoral dummies, levels of competition, and the presence of soft budgets. Selection bias is countered through the use of instrumental variables. Suitable instruments were available because of idiosyncratic features of the privatization program and due to the varying incentives of different types of owners during privatization. Comparison of OLS and instrumental variables results suggests that OLS estimates of the effect of privatization are upwardly biased.

Frydman et al. and Anderson et al. obtain strikingly different results. In Central Europe, privatization improves revenue growth by approximately 7 percent a year; in Mongolia, wholly private firms are 30 percent to 70 percent less efficient than completely state-owned firms. Although there are many differences between the analyses of these two papers, it is quite unlikely that methodological differences can explain the divergence in results, since both papers use conventional methods to solve the usual problems and pay close attention to possible biases. An obvious candidate to explain the

differences in results is the countries studied, the most advanced transition countries versus one of the most backward. We examine this issue later in this section. Another possibility is the types of owners generated during privatization, which is the subject of section 4.

One respect in which the two papers are similar is in their use of quantitative dependent variables derived from accounting information. In this respect, Carlin et al. (2001) provide an interesting contrast. They use data from a survey of over three thousand firms in 25 countries. Because of the logistical and compatibility issues involved in collecting accounting data from such a wide variety of countries, they rely for their dependent variables on responses to questionnaires. One set of questions focused on changes in real sales and employment, leading to a measure of productivity growth. Another set focused on internal restructuring, asking about upgrading of existing products, introduction of new products, opening of new plants, and quality accreditation. Using principal components to combine the answers on this latter set, the paper derives a measure of new product restructuring.

Carlin et al. do not find a direct effect of change in ownership on productivity or sales growth, but they do find a significant effect of privatization on new product restructuring. Moreover, new product restructuring directly increases sales and productivity growth, implying an indirect effect of ownership on enterprise growth. These results give some sustenance to the argument that qualitative variables provide useful information where accounting ones are not available. The authors nevertheless caution the reader that they were not able to implement procedures to counter selection bias on the ownership variable.

The three papers discussed above generate highly varying results, which is a characteristic of the literature as a whole. In table 1, we focus on the economic size of effects. Such information is usually not emphasized

TABLE 1
GAINS OR LOSSES DUE TO OWNERSHIP CHANGE: A SAMPLING OF ESTIMATES

Estimates of the performance of a 100% private firm relative to a 100% state firm. Positive signs indicate superior private performance. For the growth measures, relative performance is private-firm yearly growth rate minus state-firm yearly growth rate. For the levels measures, relative performance is private-firm productivity minus state-firm productivity expressed as a % of state productivity.

Output or Sales Growth		Productivity Growth		Levels of Productivity	
% private minus % state	country	% private minus % state	country	private minus state as % of state	country
8.7	Poland[1]	4.3	Central Europe[2]	−43	Russia[11]
7.3	Central Europe[2]	3.5	Russia[3]	62	Russia[12]
−9.7	Russia[3]	10.6	Kyrgyz[6]	14	Estonia[13]
10.9	Bulgaria[4]	3.6	Georgia/Moldova[7] (MBOs)	−30	Lithuania[14] (diffuse owners)
2.5	Czech Republic[4]	0.9	Georgia/Moldova[7] (voucher privatized)	49	Latvia[14] (individual owners)
2.1	Hungary[4]	16.0	Romania[8]	−65	Mongolia[15]
−4.5	Russia[5]	1.0	25 countries[9]	36	Romania[8]
18.0	Kyrgyz[6]	3.1	Eastern Europe[10]	140	Slovenia[16]

Notes: These estimates are based on the authors' calculations, derived from information presented in the following papers: [1] Grosfeld and Nivet (1999); [2] Frydman et al. (1999); [3] Jones (1998); [4] Pohl et al. (1997); [5] Perevalov, Gimadi, and Dobrodey (2000); [6] Roberts, Gorkov, and Madigan (1999); [7] Djankov (1999a); [8] Earle and Telgedy (2001); [9] Carlin et al. (2001); [10] Claessens and Djankov (2000); [11] Brown and Earle (2000); [12] Earle (1998); [13] Jones and Mygind (2002); [14] Jones and Mygind (2000); [15] Anderson, Lee, and Murrell (2000); [16] Smith, Cin, and Vodopivec (1997).

in the papers under review. Indeed, it is remarkable that in most cases we had to calculate the numbers in table 1 ourselves, sometimes using a considerable degree of interpretation. Moreover, a significant number of papers did not offer enough information for analogous estimates to be calculated. This suggests that the surveyed papers do not emphasize the economic interpretation of their estimates sufficiently.

The estimates in table 1 are sampled to reflect a variety of contexts and results. The first conclusion from the table is that privatization has economically significant effects. A second conclusion is that these effects are far from uniform. Privatization done in the right way, or under the right circumstances, can have huge positive effects, but privatization can also be hugely detrimental.

This large variation in results suggests that an overall judgment on the effects of privat-

ization can only be reached by employing methods that aggregate the results of the many papers under consideration. Moreover, to understand the reasons for variations between estimates, the aggregation methods should offer the possibility of assessing how methodological factors influence estimates and should allow for comparisons between sets of studies. It is to this aggregate analysis that we now turn.

3.1 *Statistical Significance: Combining t-Statistics*

We are interested in the size and statistical significance of the estimate of γ ($\hat{\gamma}$) in equation (1). The $\hat{\gamma}$'s of different studies are not directly comparable because Y and P are measured in many different ways. Therefore, we seek methods of combining the results of studies with different characteristics. We first begin by combining t-statistics.

In this section, we combine the results of 37 distinct studies. Table 2 lists these studies, the countries included in them, their *Y*'s, and their *P*'s. Within these studies, we have identified 93 distinct empirical analyses and extracted 93 $\hat{\gamma}$'s together with their corresponding t-statistics. We use more than one analysis from a single paper only in cases in which the estimates are derived from conceptually distinct analyses, e.g., from completely different forms of the dependent variable or from different countries.

The theory justifying the aggregation of t-statistics is analogous to that used when conducting tests on the mean of a sample. Collect several independent observations from the same distribution, find their mean, and deduce the distribution of the mean. The variance of the sample mean will be smaller than that of individual observations, implying that statistical tests based on the mean will be more powerful than tests on individual observations.

The data comprise *M* t-statistics on $\hat{\gamma}$'s, $t_1, \ldots, t_M$. The following is normally distributed:[14]

$$\sum_{k=1}^{M} t_k \Big/ \sqrt{M} \qquad (2)$$

M, which is the number of analyses, plays the role of sample size. It is readily apparent that a set of analyses with small positive t-statistics could be significant in the aggregate even with nonsignificance in each individual analysis. As it happens, less than one-half of the t-statistics examined in this section are statistically significant, but collectively they are highly significant.

Our synthesis of results relies on tests of (2), whereas the most common method of combining results in literature reviews is the method of vote-counting (John Hunter and Frank Schmidt 1990). Vote-counting concludes that there is nonsignificance in the aggregate when a set of studies has a median t-statistic that is insignificant. This method produces mislead-ing conclusions, since it combines probability information erroneously, a point that is best illustrated with a simple example.

Researcher A obtains a t-statistic of 2 in a study, pronouncing significance for the effect. Researcher B, not favorable to A's conclusions, conducts two separate studies of two separate countries and obtains t-statistics of 1 in each study. Researcher B triumphantly announces that A has been mistaken, for the vote is now 2 studies to 1 for nonsignificance. But the combined statistic obtained by applying (2) to all three studies is $4/\sqrt{3} = 2.31$. B has actually strengthened support for A's conclusions.

Column (1) of table 3 contains the results obtained by applying equation (2). The rows reflect two different ways of grouping estimates. First, there is the quantitative-qualitative division of dependent variables, discussed above. Second, there are regional groupings. Corresponding to much of the rest of the literature (e.g., EBRD 1999), the basic split is between the non-Baltic former Soviet Union (the CIS) and the rest.[15] The countries outside the CIS comprise Eastern Europe and the Baltics (with one study of China). For brevity, but at the cost of some inaccuracy, we will refer to the two groups as Eastern Europe (EE) and the CIS in the rest of the paper. Interestingly, once we seek a criterion that corresponds to our split of countries, we find that it is the length of time that the countries labored under communism, seventy years for each CIS country and less than fifty years in EE.[16] The reader therefore might like to think of our regional groups as "two generations" and "three generations," indicating the length of time under communism.

The significant effects of privatization show clearly in all but one of the statistics

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[14] Assuming sample sizes in all analyses are sufficiently large, which is the case here.

[15] In the set of papers under consideration, there are two studies of Mongolia. Since this country looks like a typical member of the CIS (Korsun and Murrell 1995), Mongolia is included in the CIS grouping.

[16] Moldova is an exception to this rule. This country contributes only a small amount of data to this paper.

TABLE 2
STUDIES OF PRIVATIZATION

Paper	Countries	Dependent variables	Levels or Growth	Privatization Measure
Anderson, Korsun, Murrell 2000	Mongolia	softness of budgets	levels	% ownership
Anderson, Lee, Murrell 2000	Mongolia	TFP, labor productivity	levels	% ownership
Brown and Earle 2000	Russia	TFP	levels	% ownership
Brown and Brown 1999	Russia	sectoral profitability	levels	% of sectoral output in private firms
Carlin et al. 2001	25 countries	productivity, creation of new products	growth, levels	dummy if privatized
Claessens and Djankov 2000	7 EE combined	TFP, labor productivity	growth	dummy if 33% private
Djankov 1999a	Georgia, Moldova	sales, renovations	growth	dummy if privatized
Djankov 1999c	Georgia, Moldova, Kazakstan, Kyrgyz, Russia, Ukraine	labor productivity, renovations	growth, levels	% ownership
Earle 1998	Russia	mid-year projection of labor productivity	levels	% ownership
Earle and Estrin 1997	Russia	qualitative restructuring	levels	% ownership
Earle and Estrin 1998	Russia	mid-year projection of labor productivity	levels	% ownership
Earle and Rose 1997	Russia	new goods production	levels	dummy if privatized
Earle and Sabirainova 1999	Russia	wage arrears	levels	dummy if privatized
Earle, Estrin, Leshchenko 1996	Russia	links to government, sales	levels	dummy if privatized
Earle and Telgedy 2001	Romania	TFP	levels	% ownership
Estrin and Rosevear 1999a	Ukraine	profits, sales	levels	dummy if privatized
Estrin and Rosevear 1999b	Ukraine	qualitative restructuring	levels	dummy if privatized
Evans-Klock and Samorodov 1998	Kyrgyz	excess employment	levels	dummy if privatized
Frydman, Gray, Hessel, Rapaczynski 1999	Hungary, Poland, Czech Republic	revenues, labor productivity	growth	dummy if 33% private
Frydman, Gray, Hessel, Rapaczynski 2000	Hungary, Poland, Czech Republic	default on debts	levels	dummy if 33% private
Glennerster 2000	Macedonia	profits per worker	growth	dummy if privatized
Grigorian 2000	Lithuania	TFP, labor productivity	levels	% ownership
Grosfeld and Nivet 1999	Poland	output	growth	dummy if privatized
Hendley, Murrell, Ryterman 2001	Russia	success in transactions	levels	% ownership
Jones 1998	Russia	revenue, labor productivity	growth	dummy if 50% private
Jones and Mygind 2002	Estonia	TFP	levels	dummy if 50% private
Konings 1997	Hungary, Romania Slovenia	revenues	levels	% ownership
Lehmann, Wadsworth, Acquisti 1999	Russia	wage arrears	levels	% ownership
Linz and Krueger 1998	Russia	labor productivity	growth	dummy if privatized
Major 1999	Hungary	capital productivity	levels	ownership scale
Perevalov, Gimadi, Dobrodey 2000	Russia	revenues, labor productivity	growth, levels	dummy if privatized
Pohl, Anderson, Claessens, Djankov 1997	Bulgaria, Czech Republic, Hungary, Poland, Romania, Slovenia, Slovakia	TFP	growth	dummy if privatized
Roberts, Gorkov, Madigan 1999	Kyrgyz Republic	sales, labor productivity, qualitative restructuring	growth, levels	dummy if privatized
Smith, Cin, Vodopivec 1997	Slovenia	TFP	levels	% ownership
Warzynski 2001b	Ukraine	productivity	growth	dummy if privatized
Xu and Wang 1999	China	return on equity and assets	levels	% ownership
Zemplinerova, Lastovicka, Marcincin 1995	Czech Republic	restructuring index	levels	dummy if state majority owner

TABLE 3
EFFECTS OF PRIVATE VERSUS STATE OWNERSHIP ON ENTERPRISE RESTRUCTURING

Composite statistics derived from 93 analyses appearing in 37 studies

Regions	Type of Dependent Variable in the Analyses	Number of Analyses Contributing to the Composite Test Statistic	Weighting Method Used		
			(1)	(2)	(3)
				attempts to handle selection bias	overall rating of quality
			none		
			normally distributed test statistics		
All countries	both	93	13.48	13.34	11.19
EE	both	37	15.71	15.49	15.18
CIS	both	53	3.95	2.84	0.07
All countries	quantitative	64	11.78	11.07	9.54
EE	quantitative	34	15.37	15.14	14.92
CIS	quantitative	28	0.37	−2.28	−3.73
All countries	qualitative	29	6.64	7.58	5.99
EE	qualitative	3	3.71	3.71	3.49
CIS	qualitative	25	5.35	6.76	5.06

appearing in column (1). Thus, the first conclusion from this table is that the aggregate effects of privatization are positive. This also applies when both quantitative and qualitative indicators are examined separately. The one case where the effects of privatization are not significantly positive is for quantitative indicators for the CIS.

How robust are these conclusions? The papers that contribute analyses to our data vary a great deal, in the quality of analysis in general, and the amount of attention paid to selection bias in particular. Therefore, due caution suggests that we examine whether the above conclusions could reflect methodological deficiencies in some of the surveyed papers, rather than real economic phenomena.

A straightforward way to undertake such an examination is to weight the various t-statistics, using weights reflecting methodological differences. Suppose that there are weights, $w_1, \ldots, w_M$, for each t-statistic. Then the following statistic has a normal distribution:

$$\sum_{k=1}^{M} w_k t_k \Big/ \sqrt{\sum_{k=1}^{M} w_k^2} \qquad (3)$$

This weighting procedure discounts studies with smaller weights. For example, if there are particular concerns about selection bias, the weights would reflect attention paid to this issue.

We have constructed two sets of weights. First, we rated papers on a scale of one to three, reflecting the amount of attention paid to the problem of selection bias. A score of one indicates no attempt at countering such bias, as in Djankov (1999a), while two suggests a less-than-complete attempt, for example, using fixed effects without any strong justification that this approach addresses the problem of bias (David Grigorian 2000). A three involves a convincing attack on the problem, as in Brown and Earle (2000), which uses both fixed effects and instrumental variables. Obviously the choice of scale is ad hoc, but it does fulfill our basic purpose, which is to check whether conclusions change noticeably when discounting results derived from weaker methodologies. This weighting plays a role analogous to the procedure of deemphasizing some papers in the standard literature review. But it approaches

the issue in a somewhat more transparent manner, since the reader can directly observe the consequences of the deemphasis.

We also constructed a rating of the overall quality of the papers. This rating reflects systematic information collected on specific issues such as selection bias, number of reform years reflected in the data, the nature of Y, and appropriateness of X, as well as our own subjective assessment of the strength of the analysis.[17] It also reflects the relative standing of the journal in which the paper is published, if it has been published. The rating is on a scale of one to ten. Again, the use of this rating is to examine whether conclusions change noticeably if the results from weaker methodologies are discounted.

Comparisons across the columns of table 3 suggest robustness in the conclusions for qualitative variables in both regions and for quantitative variables in EE. For example, the evidence in table 3 suggests quite clearly that the overall conclusions for EE are not vitiated by the possibility of selection bias. Table 3 serves to blunt the perception held by some that selection bias presents an insurmountable problem in assessing the effects of privatization.

There is one case where weighting by quality does change the aggregate result: the statistic for quantitative variables in the CIS changes from nonsignificance to significantly negative. Given this sensitivity to the emphasis placed on different results and given the differences in the results for qualitative and quantitative measures for the CIS, it is difficult to draw an overall conclusion on the effects of privatization in this region. If the results for the quantitative variables are deemed most reliable, then our analysis suggests that privatization has not had positive effects.

3.2 *Strength of Effect: Comparisons across Regions*

Although it is tempting to do so, one cannot immediately conclude from table 3 that the effect of privatization on the quantitative variables in EE is greater than the effect in the CIS. Table 3 provides information only on the statistical significance of an effect relative to zero. It is quite possible that an effect can be weaker in statistical terms but stronger in economic terms. To compare the size of the two economic effects, it is first necessary to identify a statistic that summarizes aggregate effect size for a heterogeneous set of studies. In order to focus on substantive results, the more formal discussion of that statistic is relegated to an appendix. We concentrate on intuition below.

The t-statistic is not suitable because its magnitude reflects sample size, which has nothing to do with the economic effect of privatization. $\hat{\gamma}$ is not suitable because it is dependent on the units of measurement of Y and P, which vary greatly between studies. On the positive side, the t-statistic is invariant to units of measurement, while $\hat{\gamma}$ does not depend on sample size. Thus, we seek a measure that combines the positive properties of the t-statistic and of the estimated parameter, while eliminating their negative characteristics.

The standard procedure in meta-analysis is to use the partial correlation coefficient (Robert Rosenthal 1984). Partial correlation coefficients are not usually published, but they are easily calculated from regression statistics that appear in all papers.[18] Since correlation coefficients are unit-free, they are comparable across a heterogeneous set of studies and therefore can be aggregated

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[17] An earlier version of this paper presented results for many sets of weights, reflecting each methodological variable on which we collected systematic information. The reader can examine these results in Djankov and Murrell (2000).

[18] William Greene (2000, pp. 233–36) proves this result within the classical regression framework, to which a majority of the studies reviewed conform. When applying this result outside this framework, we simply appeal to analogy. This suggests that more effort should be made in including information in published papers that allows the estimates to be more readily compared to estimates in other papers that examine the same economic phenomena.

to form a summary statistic on effect size for the whole set of studies.

The first two rows of column (1) of table 4 show the simple unweighted means of the estimates of the partial correlations derived from two groups of studies, those on EE and those on the CIS. As expected, the EE estimate is larger. As the appendix shows, it is straightforward to construct a normally distributed test statistic that reflects the null hypothesis that these two means are equal. This test statistic appears in the third row of table 4, providing evidence that the privatization effect is significantly larger in Eastern Europe than in the CIS.

One disadvantage of working with partial correlation coefficients is that their size is not obviously interpretable in terms of real economic phenomena. However, a few facts might make the numbers in table 4 more easily interpretable. For example, if a study had the same sample size and standard errors as the Frydman et al. result in table 1 (second row, first column), then a partial correlation coefficient of 0.05 would correspond to a percentage growth of sales of privatized enterprises that is 3.51 higher than that of state enterprises. Using the Brown and Earle (2000) result (table 1, first row, last column) in a similar fashion, a partial correlation coefficient of –0.05 would correspond to privatized enterprises being 38 percent less efficient than state enterprises.

Weighted means of the partial correlation coefficients can also be calculated and a normally distributed test statistic can be derived to examine the significance of the difference between the two weighted means. Columns (2) and (3) of table 4 present the results for the aggregate partial correlation coefficients calculated using the weights that were described in the context of table 3. Thus, differences between the columns of the table reflect the sensitivity of the overall results to varying judgments on the importance of methodological quality.

When examining the test statistics for qualitative and quantitative variables to-

gether or when the quantitative variables are examined alone, the test statistics affirm (at the 1-percent significance level) that the privatization effect is stronger in EE than in the CIS. For the quantitative variables, the use of the weights adjusting for methodological quality actually strengthens this conclusion, suggesting that selection bias, or other methodological deficiencies, do not play any role in leading us to this conclusion. The results are similar, but weaker statistically, for the qualitative variables, perhaps because there are so few studies for EE that fit into this category. In sum, there is strong evidence that the effect of privatization in Eastern Europe has been much greater in magnitude than in the CIS.

4. The Effects of Different Types of Owners

One reason that changes of ownership might have had varying effects across regions is that different privatization processes led to different mixes of owners. In the absence of quick re-trading of shares (Anderson, Korsun, and Murrell 1999; Joseph Blasi and Andrei Shleifer 1996), the owners created by the privatization process have had more than a temporary effect on enterprise behavior. This is important, of course, only if ownership makes a difference in restructuring. As it happens, transition experience offers unusually good evidence on this score.

The pre-transition literature offers a variety of disparate empirical results. There has been general support for Adolf Berle and Gardiner Means' (1933) contention that diffuse ownership yields power to managers with less interest than shareholders in restructuring (Bruce Johnson et al. 1985; Richard Roll 1986). Randall Morck, Andrei Shleifer, and Robert Vishny (1988) found that some managerial ownership can ameliorate such problems, while Shleifer and Vishny (1986) argue that block-holders have an incentive to monitor management. However, block-holders also have opportunities to

TABLE 4
COMPARING THE SIZE OF PRIVATIZATION EFFECTS ACROSS REGIONS

Partial correlation coefficients and test statistics derived from 90 analyses from 36 studies

Regions	Type of Dependent Variable in the Analyses	Number of Analyses Contributing to the Composite Statistic	Weighting Method Used		
			(1)	(2)	(3)
			none	attempts to handle selection bias	overall rating of quality
			partial correlation coefficients and test statistics on their differences		
EE	both	37	0.083	0.073	0.075
CIS	both	53	0.038	0.041	0.016
Test statistic for difference	both		*3.89*	*2.74*	*4.82*
EE	quantitative	34	0.078	0.070	0.073
CIS	quantitative	28	0.016	−0.001	−0.019
Test statistic for difference	quantitative		*4.14*	*4.63*	*5.41*
EE	qualitative	3	0.146	0.146	0.145
CIS	qualitative	25	0.063	0.089	0.064
Test statistic for difference	qualitative		*1.99*	*1.36*	*1.84*

expropriate value from minority shareholders. Quite separately, the comparative systems literature pursued the question of whether the pathologies predicted by the neoclassical model of the labor-managed firm outweigh the motivational effects of worker ownership. The evidence is mixed (Alan Blinder 1990; John Bonin, Derek Jones, and Louis Putterman 1993; Douglas Kruse and Joseph Blasi 1995).

The existing literature on market economies does not provide a clear picture of which type of owner is most advantageous for restructuring. Ronald Coase (1988) and Harold Demsetz and Kenneth Lehn (1985) suggest a reason for the lack of clarity. They argue that the evidence from mature market economies might be spurious. If the transaction costs of taking value-maximizing positions in firms are low, each firm would have the "right" ownership structure: there is no relationship between ownership type and restructuring.

Therefore, evidence from transition countries has a vital contribution to make. In transition economies, ownership was largely determined through political and administrative processes rather than endogenously in markets with low transaction costs. In many cases, ownership structure is exogenous. In others, it is relatively easy to obtain reliable instruments to counter endogeneity bias. The evidence from transition should offer much better information on the effects of different owners than has been generated previously.[19]

The results for transition countries are unique in another respect. Many different types of owners were created. At the time of privatization, government agencies and firms knew the structure of ownership and could impart this information to researchers.

[19] For example, Megginson and Netter's (2001) comprehensive survey of the effects of privatization in all types of economies does not reflect on the relative effectiveness of different types of private owners.

The resultant ownership information was at a level of detail that could not be obtained in mature market economies. Hence, there is a large set of studies examining the effects of different owners. By aggregating the results, we are able to compare the effects of a comprehensive set of owners, something not previously accomplished in the literature.

We begin by examining the characteristics of the pertinent papers. The methodologies used by the papers are essentially the same as those discussed in section 3. In fact, many papers begin with an analysis of state versus private and then turn to a disaggregation of private ownership, e.g., Brown and Earle (2000), Earle (1998), Frydman et. al. (1999), and Jones (1998). Such papers use the same methodology and independent variables in both parts of their analyses, simply making P in (1) a vector that captures the ownership held by different entities.

A further focus on Frydman et al. (1999) and Anderson, Lee, and Murrell (2000) exhibits the methodological decisions to be made when choosing the elements of P and the variations in results that can be obtained. Frydman et al. examine outsiders, insiders, and the state in one analysis, and foreigners, domestic financial firms, domestic nonfinancial firms, domestic individuals, the state (in a privatized firm), the state (in a nonprivatized firm), managers, and workers in another analysis. This is an unusually comprehensive list of owners to appear in one paper, and most papers use a much smaller set, e.g., Smith, Cin, and Vodopivec (1997) using foreigners, insiders, and the state, and Claessens and Djankov (1999a) using managers and outsiders. The ownership variables in Frydman et al. are dummies capturing whether the given owner is the largest shareholder, a common approach, e.g., Jones and Mygind (2000, 2002). Measurement of ownership in this way reflects a somewhat unusual feature of the Frydman et al. data, the highly concentrated ownership in all privatized firms.

In Mongolia, examined by Anderson, Lee, and Murrell (2000), the variety of owners after privatization is narrower and the degree of ownership concentration less. Managerial and worker ownership are highly correlated due to the nature of the privatization scheme and there is no value in differentiating between them. Individuals with small stakes are dominant among outsider owners. Hence, analysis of the effects of different owners in Anderson et al. only contrasts the state (in a privatized firm) versus outsiders versus insiders. In contrast to Frydman et al.'s dummy variables, ownership is measured by the percentage of shares held by each type of owner, perhaps the most usual approach, e.g., Brown and Earle (2000), Claessens and Djankov (1999a,b), Earle and Estrin (1997), and Smith, Cin, and Vodopivec (1997).

Frydman et al. find that foreign and domestic financial owners produce the largest positive effects, while outsider owners outperform insiders, results echoed later in our synthesis of many papers. Their results are mixed when comparing insider ownership to state ownership in enterprises with 100-percent state ownership. Anderson et al. find that outsiders and insiders perform less effectively than the state in privatized enterprises, while there is no significant difference between insiders and outsiders.

What explains the differences in these two sets of results? Selection bias or omitted variables seem unlikely candidates, given the attention paid by both studies to these issues. Rather, the outsider owners are very different in Central Europe, where they are block-holders, than in Central Asia, where they are individuals, usually with tiny amounts of ownership. Anderson et al. examine whether diffusion of ownership can explain their results and do find better performance in those few enterprises in which block-holders have obtained seats on the boards of directors.[20] The positive relation between ownership

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[20] However, because of lack of instruments, they are not able to control for selection bias on this variable.

concentration and performance is a direct focus of Claessens and Djankov's (1999b) study on the Czech Republic.

In reconciling the findings of Frydman et al. and Anderson et al., the results on state ownership warrant particularly close examination. Anderson et al. were able to sample only privatized enterprises (including ones with large state ownership). Thus their study includes no firms that remained under traditional state ownership. Frydman et al. sampled both privatized and nonprivatized firms, thus having two state ownership variables. The Anderson et al. results on state ownership are comparable only to those of Frydman et al. on state ownership in privatized firms. When this is understood, the two studies' results appear much more consistent, since Frydman et al. find that state ownership in a privatized firm performs at least as well as the median type of private owner.[21] However, the two studies provide very different interpretations of their results. Frydman et al. view the state as passive, with private owners dominating the decisions of partially state-owned privatized firms. Anderson et al. view the Mongolian state as active and enormously pressured by economic necessity. The government was more willing than insiders to push for efficiency and more able to do so than outsiders.

In providing a reconciliation of the results of the two studies, the above paragraphs provide an important warning to the harried reader trying to absorb a large number of results very quickly. Details matter crucially, for example which study used the privatization agency to obtain its sample or which study's data reflect concentrated ownership. Choice of estimation method can even influence the interpretation of which owners are reflected in results, fixed effects estimates reflecting only ownership that changes over the sample period. These facts are often not emphasized in papers.

4.1 *Quantifying the Differences between Owners*

We now turn to the quantitative synthesis of results. The objective of this synthesis is to find the relative sizes of the effects of different owners and to test which owners have significantly different effects from others. We follow the literature in identifying the ownership categories that we analyze, identifying those ownership types that recur consistently in a number of studies.[22] This resulted in eleven categories, some of which overlap:[23]

1) traditional state ownership: enterprises that are 100-percent state-owned and that have not been part of a privatization program;

2) the state in commercialized (or corporatized) enterprises: state ownership in enterprises that have been legally separated from the state bureaucracy, that are treated as private under the corporate governance laws, and, usually, that have been part of a privatization program;[24]

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[22] Since there is no source that provides a definition of a standard set of ownership categories, there are inevitable variations across the empirical papers in how different owners are defined. Where papers used ownership types that differed too much from the types that appeared in other papers, we did not use the results. This happened in only a small number of cases.

[23] For example, workers and managers together are insiders. One should not assume that an aggregate category necessarily produces the same result as the sum of its parts. For example, estimates of the effects of managers and workers usually appear in different studies than those for insiders as a whole. Which owners are included in a study, however, is not random. Researchers are more likely to collect data on a specific owner when the privatization process gives scope for that owner to be active in privatization procedures. Hence, the results for insiders probably reflect somewhat different circumstances than those for managers and workers on their own. We should not expect to see the insider effect simply equal to the weighted sum of the worker and managerial effect.

[24] In most countries commercialization (or corporatization) occurred as preparation for privatization. Therefore, our data on this ownership type is dominated by results for enterprises that have less than 100-percent state ownership. Where a study did not provide enough information for us to know which of the two categories of state ownership applied or where the study mixed the two types, we assigned the results to the traditional state ownership category.

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[21] As is clearly shown when we aggregate results below, this result is echoed in many other studies.

3) enterprise insiders: a composite group, including both workers and managers;

4) outsiders: a composite group consisting of all nonemployee, nonstate owners;

5) workers (nonmanagerial employees);

6) managers (managerial employees);

7) foreign owners of all types;

8) banks, except those included in 7;

9) investment funds, except if ownership of the fund is such that 2, 7, or 8 applies;

10) block-holders: outsider ownership concentrated in the hands of large individual owners, except where the block-holder could be classified in 2, 6, 7, 8, or 9;

11) diffuse outsider: the residual outsider ownership category, when outsider owners are not identified as belonging to 7, 8, 9, or 10; a category dominated by individual outsider owners, each of whom owns a miniscule portion of any specific enterprise.[25]

All papers that we review examine a subset of these eleven types of owners. For example, a paper might focus on the state versus insiders and outsiders and estimate:

$$Y = \alpha + X\beta + \delta O + \theta I + \varepsilon \qquad (4)$$

O and *I* are measures of the amount of ownership held by outsiders and insiders, state ownership is the omitted variable, δ and θ are the parameters of interest, and all other variables are as defined before. The information of prime interest is the comparison of δ to θ (outsiders versus insiders) and each of these to 0 (insiders or outsiders versus the state). We examine both statistical significance and the magnitude of the ownership effect.

The methods used here are extensions of those already introduced in section 3. However, a number of complications arise in developing the results. First, important information is often missing from papers (relevant to the statistical precision of the comparison between δ and θ). Therefore, it is necessary to investigate how to fill this gap.

Second, there is no longer a binary comparison between state and private, but a multilateral comparison between many types of owners. Therefore, a dummy-variable regression framework is required to combine results, rather than simply using differences between means. Third, to take advantage of information on the precision of estimates from different papers, it is necessary to use generalized least squares. Fourth, the weighting of results according to methodological quality also requires generalized least squares. A full consideration of these complications entails an extended discussion of methodology, in which the average reader is probably not interested. The discussion is therefore relegated to an appendix.

From 24 studies on twenty countries, we have compiled all usable information on the effects of different types of owners on quantitative enterprise outcomes. The pertinent papers, together with the countries and owners in each study, are listed in table 5.

From these studies, we derive the t-statistics and then the partial correlation coefficients corresponding to ownership effects. The data are then combined in a generalized least squares framework to obtain a composite estimate of the effect of each of the new owners (2–11 above) relative to traditional state ownership. These estimates of ownership effects are in the form of partial correlations, as in section 3.

The estimates of the partial correlations appear in figure 1. These estimates reflect a process that weights more heavily those studies that have paid more attention to the issue of selection bias.[26] We focus on this methodological issue because readers of previous versions of this paper have indicated that this is of most concern.[27] By comparing the results that are obtained when all

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[25] This category is used only when the study differentiates between various types of outsiders. When all outsiders are treated as one, owner group 4 applies.

[26] The weighting scale is the same as in the previous section.

[27] Suspicions about selection bias arise naturally. Perhaps the state kept the best enterprises during privatization, or managers fought harder to retain control when prospects were good, or foreigners were willing to pay for efficient enterprises only.

TABLE 5

STUDIES OF PRIVATIZATION BY OWNERSHIP TYPE

Paper	Countries	Ownership Types
Anderson, Lee, Murrell 2000	Mongolia	2, 3, and 4
Brown and Earle 1999	Russia	2, 3, 4, 7, and 11
Claessens and Djankov 1999a	Czech Republic	4 and 6
Claessens and Djankov 1999b	Czech Republic	2 and 7–10
Claessens, Djankov, Pohl 1997	Czech Republic	2 and 7–9
Cull, Matesova, Shirley 2002	Czech Republic	1 and 7–9
Djankov 1999b	Georgia and Moldova	1, 6, and 11
Djankov 1999c	Six CIS states	2, 5–7, 10, and 11
Earle 1998	Russia	1, 4–11
Earle and Estrin 1997	Russia	1, 4–11
Earle and Telgedy 2001	Romania	2, 3, 10, and 11
Estrin and Rosevear 1999a	Ukraine	1–6
Frydman, Gray, Hessel, Rapaczynski 1999	Central Europe	1–10
Grosfeld and Nivet 1999	Poland	1 and 2
Jones 1998	Russia	1, 4–6, and 8
Jones, Klinedinst, Rock 1998	Bulgaria	1 and 2
Jones and Mygind 2000	Baltics	1, 5–7, and 10
Jones and Mygind 2002	Estonia	2, 5–7, and 10
Konings 1997	Slovenia, Hungary, Romania	1 and 5
Lee 1999	China	1 and 2
Lehmann, Wadsworth, Acquisti 1999	Russia	1 and 7
Roberts, Gorkov, Madigan 1999	Kyrgyz Republic	2–4
Smith, Cin, Vodopivec 1997	Slovenia	1, 3, and 7
Weiss and Nikitin 1998	Czech Republic	2 and 8–10

Ownership types are coded as follows (see text for details):
  1) traditional state ownership
  2) the state in commercialized (or corporatized) enterprises
  3) enterprise insiders
  4) outsiders
  5) workers (nonmanagerial employees)
  6) managers (managerial employees)
  7) foreign owners of all types
  8) banks
  9) investment funds
10) block-holders
11) diffuse outsider

papers are treated equally (see appendix) to those in figure 1, one can examine whether the results seem particularly sensitive to selection bias. Three conclusions follow from this comparison. First, the relative position of most owners remains the same whatever the treatment of selection bias. Second, selection bias reduces the estimated magnitudes of most ownership effects. Third, only the results for managers and workers show a considerable degree of sensitivity to how selection bias is handled. Thus, our assessment of the results is that a high degree of confidence can be placed in the relative ranking that appears in figure 1, except for some residual doubt about the positions of managers and workers, especially the former.

Figure 1 suggests that differences between owners are of great economic importance. Privatization to workers is detrimental;
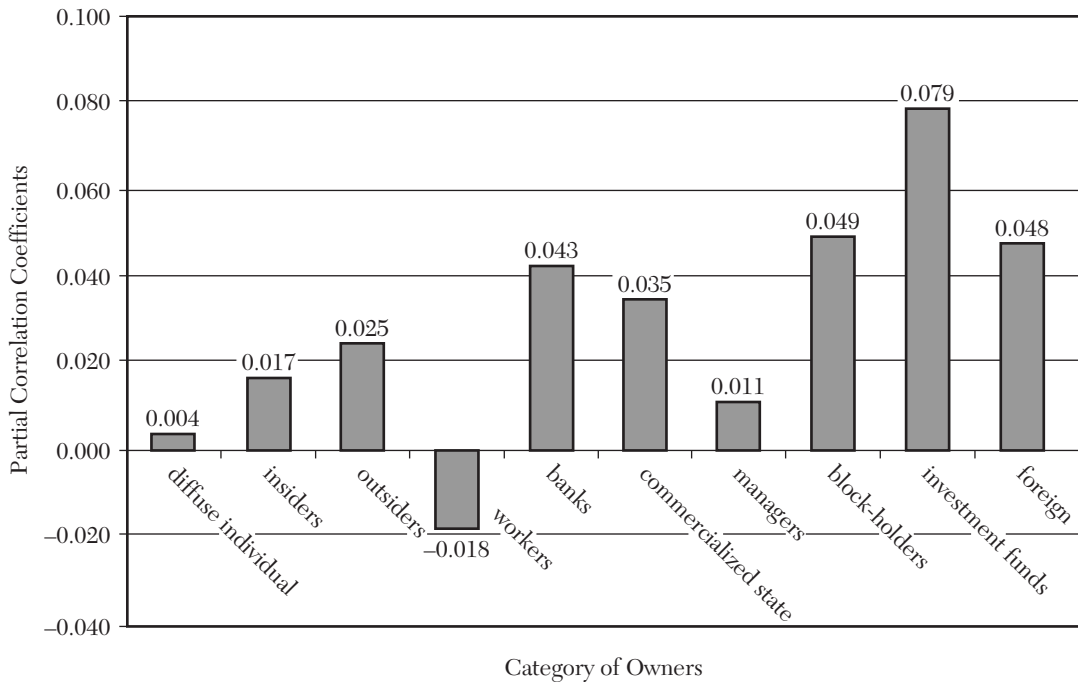
Figure 1. How Ownership Affects Firm Performance after Privatization.
Estimated Effects of Changing from Traditional State Ownership to Different Private Owners

privatization to diffuse individual owners has no effect, and privatization to investment funds or to foreigners has a large positive effect. Loosely speaking, privatization to funds is five times as productive as privatization to insiders, while privatization to foreigners or block-holders is three times as productive as privatization to insiders. Foreigners were expected to make productive changes, but it is notable that investment funds are significantly better than foreigners, and that banks and block-holders are close to foreigners. Similarly, diffuse individual ownership was not expected to be very effective, but it is surprising that it is indistinguishable from traditional state ownership.

A notable result is the difference between traditional state ownership and state ownership in commercialized enterprises, a result that recurs from Central Europe to China (Claessens, Djankov, and Pohl 1997; Lee 1999). Remember, of course, that this result is not for economies in which ownership has

been developed organically for decades, but rather where ownership has been artificially transferred, sometimes to private owners who are creatures of the state. Then, if corporate governance laws are weak, share re-trading is sluggish, and the state is focused on solving economic problems, state ownership could easily be superior to some types of ownership (Anderson, Lee, and Murrell 2000). The superiority of state ownership in commercialized enterprises over traditional state ownership might arise because the part-owners who are private are playing an important role in enterprise affairs (Frydman et al. 1999) or because the very act of commercialization changes the incentives facing the state when it intervenes into enterprise affairs (Shleifer and Vishny 1994).

A conclusion implicit in the results of figure 1 is that concentrated shareholding produces larger effects than diffuse shareholding. This is seen most clearly in the difference between the effects of individual

TABLE 6
TESTING DIFFERENCES BETWEEN THE EFFECTS OF A VARIETY OF OWNERS

Statistics derived from 341 pair-wise comparisons of owners taken from 24 studies

	Workers	Traditional State	Diffuse Individual	Managers
	t-statistic for {(effect of owner listed on row) minus (effect of owner listed on column)}			
Traditional State	1.36			
Diffuse Individual	1.78	0.50		
Managers	1.93	0.90	0.63	
Insiders	2.77	2.61	3.12	0.45
Outsiders	3.20	3.45	3.54	1.09
Commercialized State	4.12	5.69	7.21	1.99
Banks	3.53	3.42	3.04	1.93
Foreign	4.82	7.82	7.67	2.98
Block-holders	4.90	7.73	7.74	3.09
Investment Funds	5.55	7.05	5.98	4.28

Notes: To compare owners A and B: If the cell located at the intersection of A's row and B's column is blank, then B is more productive than A. If the cell corresponding to A's column and B's row is nonempty, then the number in that cell is the t-statistic for a test of the null hypothesis that B's effect minus A's effect is equal to zero.

owners and those of block-holders, but it is also implicit in the effects of foreigners, funds, and banks since these entities usually concentrate their shareholdings. Claessens and Djankov (1999b) focus directly on the effects of concentration in their study of the Czech Republic. They show that a 10-percent increase in the percentage of shares held by the largest five shareholders will increase labor productivity by 5 percent. They also find diminishing returns to concentration. These results are echoed in studies of Russia, e.g., Earle and Estrin (1997) and Brown and Earle (2000).

Table 6 examines statistical significance of the differences in figure 1, reporting the values of t-statistics for standard tests of null hypotheses that one owner has the same effect as another, for all pair-wise combinations of owners.[28] A first inspection of table

6 immediately reveals an unusually large number of significant t-statistics.[29] This is partially a consequence of employing a methodology that combines results and embodies the precision of estimates from many studies. Just as in the previous section, we have been able to generate an unusual amount of statistical power. However, it is not the case that all owners have significantly different effects from all others. For example, workers, diffuse individual owners, and traditional state ownership cluster at the bottom, with effects that do not differ significantly. The effects of banks are significantly different from the effects of less than half the other owners, partially reflecting the fact that banks are included in few studies.

4.2 *Comparing Owners across Regions*

We have found that privatization has stronger effects in Eastern Europe than in

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[28] These test statistics are easily calculated using standard generalized least squares procedures and implicitly use the information on the variances of estimates that is extracted from the original studies.

[29] The appendix suggests using a conservative benchmark for significance, but this statement holds even under this standard.

TABLE 6 *(Cont.)*

Statistics derived from 341 pair-wise comparisons of owners taken from 24 studies

Insiders	Outsiders	Commercialized State	Banks	Foreign	Block-holders
		t-statistic for {(effect of owner listed on row) minus (effect of owner listed on column)}			
1.67					
5.28	2.28				
2.09	1.39	0.63			
6.66	4.18	3.44	0.44		
6.71	4.36	3.71	0.56	0.38	
5.30	4.60	3.97	2.47	2.96	2.84

the CIS and that different types of owners have different effects. This immediately raises the question of whether the latter could explain the former. This question would be best addressed using data on the distribution of ownership in different countries, but there is no systematic collection of such data. The available evidence does suggest that worker and diffuse individual ownership are more prevalent in the CIS than in EE, while foreign, investment fund, concentrated individual, and bank ownership are less prevalent. (See, for example, Anderson, Lee, and Murrell 2000; Frydman et al. 1999; Brown and Earle 2000; and Claessens and Djankov 1999b.) Thus, since the CIS ownership portfolio contains a greater share of less effective owners, structure of ownership is a strong candidate to explain regional differences in the effects of privatization.

The effects of different types of owners could also vary between regions. To investigate this, the natural first step is to see whether the vectors of estimated ownership effects differ significantly between regions.

The data reject the null hypothesis that ownership effects in the CIS are all the same as those in EE, at the 1-percent significance level. The next step then is to redo figure 1, for each region separately.

In all cases except one (workers), the ownership effects for the CIS in figure 2 are greater than for EE.[30] How is this consistent with section 3, which showed that private ownership had stronger effects in EE than in the CIS? There are two obvious reasons. First, the effect of workers is strongly negative and they own a much larger share of privatized enterprises in the CIS than elsewhere. (In Frydman et al.'s 1999 Central European sample, workers are the dominant owner in 9 percent of firms, while they are dominant in 50 percent of Earle and Estrin's 1997 Russian sample.) Second, commercialized state own-

[30] It is worth emphasizing that these results do not show that the various owners are more productive in the CIS than Eastern Europe in absolute terms. Rather, these results could just as easily be due to the relative unproductiveness of traditional state enterprises in the CIS, which are the basis for comparison.
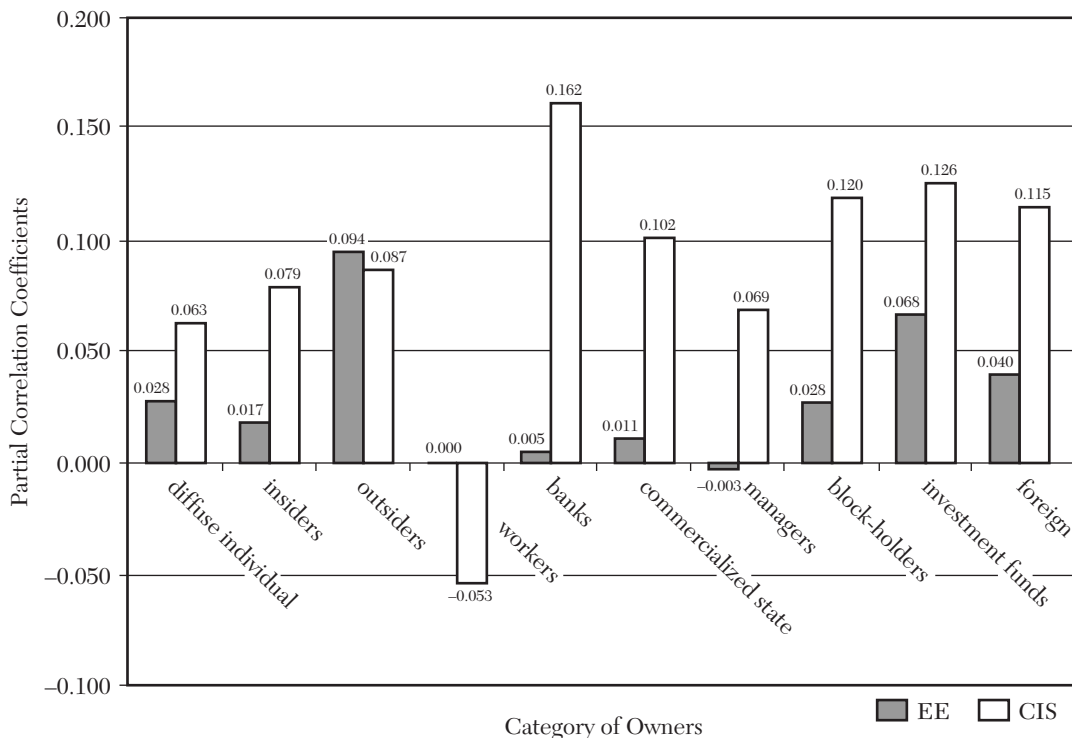
Figure 2. Regional Variations in the Effects of Different Types of Owners.
Comparing Ownership Effects in Eastern Europe to Those in the CIS

ership is separated from traditional state ownership in figure 1 but not in table 4. Commercialized state ownership has above average effects (relative to traditional state ownership) in the CIS and below average in EE. Thus, if one conceives of the privatization process as two steps, first the move away from *traditional* state ownership and then the move away from state ownership of all forms, figure 2 and table 1 together suggest that the first step alone was a important contribution of the privatization process in the CIS, while this was not so in Eastern Europe.[31]

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[31] Lest some readers find this statement confusing, it should be emphasized that it was sometimes possible for an enterprise to pass through a privatization program and still end up with 100-percent state ownership! Going through a privatization program and reduction of state ownership were not synonymous, although highly correlated.

While we have treated the two regions as individually homogenous in deriving figure 2, it should be noted that there are also large differences between the estimates for different countries within the same region. For example, Romania seems to be an outlier in Eastern Europe. John Earle and Almos Telgedy (2001) find that diffuse individual and insider ownership are surprisingly productive in Romania. Indeed, the studies for the CIS are somewhat more homogenous than those outside the CIS, as evidenced by the R-squares for the regressions that provide the data for figure 2: 0.73 in the CIS and 0.31 outside that region. This is perhaps a reflection of the greater diversity, in history and in reform programs, within EE. Hence, figure 2 is hardly a last answer, but rather a beginning, in a search for the reasons why different owners are more effective in some settings than others. In section 8, we provide some discus-

sion of this issue, examining an institutional interpretation of the patterns in figure 2.

5. *Product Market Competition*

There is a substantial theoretical literature analyzing the relationship between competition and enterprise efficiency. The general hypothesis is that increased competition stimulates improvements in productivity. Two lines of argument support this hypothesis. The first is derived from the literature on X-inefficiency, while the second centers on industry rationalization. The X-inefficiency literature argues that managerial effort is under-supplied in the absence of vigorous competition. Henrik Horn, Harold Lang, and Stefan Lundgren (1995) show that greater competition induces an expansion of output by incumbent firms through improved internal technical efficiency without any reallocation of resources across firms. Earlier studies (Bengt Holmström 1982; Barry Nalebuff and Joseph Stiglitz 1983) argue that incentive schemes for managers will generate better results the greater the number of players (firms) involved because of greater opportunities for performance comparison.

A second line of argument is that increased competition may lead to a rationalization of oligopolistic industries as firms are forced to compete for market share (Klaus Schmidt 1997). Resource reallocation occurs across firms, within and between sectors. Although the shake-out may result in a transitional decline in productivity as firms with increasing returns to scale lose domestic market share, over time this may be reversed as market share expands due to exit of competitors and access to export markets. Much depends on the existence of scale economies and the ease of entry and exit.

In transition economies, increased competition may have negative effects on efficiency. Competition creates incentives for breaking contracts when institutions are weak (Olivier Blanchard and Michael Kremer 1997). Barry Ickes, Randi Ryterman, and Stoyan Tenev (1995) suggest that excessive competition, especially from abroad, destroys network capital and harms enterprise performance in the early transition period. The former Soviet Union experienced significant losses in markets with the disintegration of the trade relations between Russia and the other former republics (Simeon Djankov and Caroline Freund 2002). As traditional markets collapsed and as countries were opened up to foreign competition, many firms found it difficult to restructure their product lines and at the same time reorient their sales towards new markets.

The empirical literature outside of transition economies yields a mixed picture on the relationship between competition and enterprise efficiency. Few empirical investigations using firm-level data have established a strong link between greater competition and subsequent changes in enterprise performance.[32] Two studies of British manufacturing firms (Stephen Nickell, Sushil Wadhwani, and Martin Wall 1992; Stephen Nickell 1996) use a panel to show that market concentration has an adverse effect on total factor productivity. In contrast, David Blanchflower and Stephen Machin (1996) find no effect of changes in domestic market structure on the productivity of British plants. Silke Januszewski, Jens Koke, and Joakim Winter (2001) use a sample of publicly listed German companies to show that neither import competition nor domestic market competition has beneficial effects on TFP growth. Studies on developing countries also find ambiguous results. Ann Harrison (1994) finds that a reduction of tariffs in Cote d'Ivoire and the subsequent increase in import penetration had an insignificant effect on TFP growth. In contrast, Sweder van Wijnbergen and Anthony Venables (1993) find a strong positive effect of increased import penetration on labor productivity in Mexican manufacturing firms.

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[32] On the macro level, Francisco Rodriguez and Dani Rodrik (1999) survey the literature on open trade policies and economic growth and conclude that the evidence that openness is associated with growth is very weak.

The initial period of transition provides a unique opportunity to test the importance of product market competition. A majority of transition economies rapidly liberalized their trade regimes. Many went on to demonopolize their industrial sectors by breaking up conglomerates, spinning off individual production units, and allowing entry of new private firms (Simeon Djankov and Bernard Hoekman 2000; Lizal, Singer, and Svejnar 2001). The short period in which these changes took place allows the researcher to identify the timing of the policy change and to control for other economy-wide and firm-specific factors that influence the change in productivity.

We find 23 studies that explicitly investigate the effect of product market competition on enterprise restructuring. Among those, thirteen focus on EE countries (Claessens and Djankov 2000; Rumen Dobrinsky et al. 2001; Djankov and Hoekman 1998, 2000; Grigorian 2000; Irena Grosfeld and Thierry Tressel 2001; Philip Hersch, David Kemme, and Sharad Bhandari 1994; Laszlo Halpern and Gabor Körösi 1998; Könings 1997, 1998; Nikolay Markov et al. 2000; Mary Shirley and Lixin Colin Xu 2001; Frederic Warzynski 2001a) and ten use data for Russia (Manuella Angelucci et al. 2001; Annette Brown and David Brown 1999; Earle and Estrin 1998; Brown and Earle 2000; Boris Kuznetsov et al. 2002; Yuri Perevalov et al. 2000), Georgia (Vladimir-Goran Kreacic 1998), Ukraine (Warzynski 2001b), Mongolia (Anderson, Lee, and Murrell 2000), or all transition economies (Carlin et al. 2001). We are able to distinguish 82 separate analyses, where the authors use either different measures of enterprise restructuring (total factor productivity levels or growth rate, labor productivity level or growth, sales growth, and qualitative variables like renovation of facilities) or different indicators of competitive pressures. Twenty-eight analyses use import competition as the main explanatory variable, while 52 focus on domestic market structure.

The analyses are fairly homogeneous. In most cases, the dependent variable is quantitative: 55 analyses use TFP as the indicator of enterprise restructuring, nineteen use labor productivity, and two use sales growth. Six analyses, all derived from Kreacic (1998), use qualitative indicators of restructuring (facilities renovation, establishment of a marketing department, and computerization of the accounting function). Import competition is proxied by the import penetration ratio (twenty analyses), or the industry-level tariff rate (eight analyses). Domestic market competition is measured by either the Herfindahl index (25 analyses), the percentage of sales revenues of the top two, three, or four firms in the respective industry (fifteen analyses), the number of local competitors that enterprise managers perceive as rivals (twelve analyses), or the change in the enterprise's own market share (two analyses).

Some explanatory variables raise the issue of endogeneity. In proxying for the level of domestic competition, Hersch, Kemme, and Bhandari (1994) asked enterprise managers to "categorize the number of other firms now producing in your main domestic market as either none, 1 to 10, 11 to 50, 51 to 100, or more than 100" (p. 358). Dobrinsky, Dochev, and Markov (2001) use lagged own market share as one of their proxies. Both variables are problematic since the number of competitors or one's market share may be endogenous to the performance of the enterprise. Accordingly, when we aggregate the empirical analyses using weights, we assign a weight of 1 to both studies for "attempts to handle endogeneity." Studies using the Herfindahl index, e.g., Könings (1998), avoid this problem and are given a rating of three.

The endogeneity problem is also present in studies that use manager perceptions of import competition or the import penetration ratio as a measure of competition from abroad. The former suffers from obvious endogeneity, while the second depends on the volume of imports, which is endogenous to the performance of the producers in the

TABLE 7
GAINS OR LOSSES DUE TO PRODUCT MARKET COMPETITION: A SAMPLING OF ESTIMATES

Study	Country	Dependent Variable	Explanatory Variable	Economic Effect[1]
Angelucci et al. 2001	Romania	Log TFP level	Herfindahl level	19.1%
Perevalov et al. 2000	Russia	Log TFP level	Number of competitors	2.4%
Grosfeld and Tressel 2001	Poland	TFP growth	Own market share	1.4 pp
Djankov and Hoekman 2000	Bulgaria	TFP growth	Herfindahl level	1.4 pp
Warzynski 2001b	Ukraine	TFP growth	Number of competitors	−3.8 pp
Brown and Earle 2000	Russia	Log TFP level	Herfindahl level	1.4%
Konings 1998	Bulgaria	TFP growth	Herfindahl level	4.4 pp
Konings 1998	Estonia	TFP growth	Herfindahl level	−1.0 pp
Halpern and Korosi 1998	Hungary	Log TFP level	Herfindahl level	−2.6%
Anderson, Lee, Murrell 2000	Mongolia	Log TFP level	Own market share	9.2%
Claessens and Djankov 2000	Hungary	Labor prod. growth	3-firm concentration	7.1 pp
Markov et al. 2000	Bulgaria	Log TFP level	Own market share	−23.7%
Markov et al. 2000	Bulgaria	Log TFP level	3-firm concentration	5.9%
Angelucci et al. 2001	Romania	Log TFP level	Import penetration	−6.0%
Markov et al. 2000	Bulgaria	Log TFP level	Import penetration	−2.1%
Brown and Earle 2000	Russia	Log TFP level	Import penetration	1.4%
Djankov and Hoekman 2000	Bulgaria	TFP growth	Import penetration	1.1 pp
Halpern and Korosi 1998	Hungary	Log TFP level	Import penetration	3.0%
Dobrinsky et al. 2001	Bulgaria	Log TFP level	Import penetration	−0.7%
Kreacic 1998	Georgia	Log TFP level	Import penetration	−5.8%
Claessens and Djankov 2000	Czech Rep.	Labor prod. growth	Tariff levels	4.3 pp

[1] The economic effect is calculated from the coefficient estimates assuming a 10-percentage-points change in the explanatory variable. pp means percentage points.

domestic sector. Accordingly, we assign a value of 1 for handling endogeneity to studies that use perception of import competition, e.g., Konings (1997); a 2 to all analyses that use import penetration, e.g., Angelucci et al. (2001); and a 3 for all analyses that use changes in the statutory tariff rate, e.g., Djankov and Hoekman (2000). The endogeneity ratings are also reflected in the overall quality rating.

Table 7 illustrates the variety of analyses in terms of country samples, the choice of dependent and explanatory variables, and the estimated economic effects of competition on enterprise efficiency. For example, Angelucci et al. (2001) study the effect of competitive pressure on firm performance in Romania, using panel data from 1997 to 1998.[33] The study finds that a 10-percent-

[33] The study also uses a large panel of Polish firms. As the data cover both small and large companies, they are not used in our analysis.

age-points reduction in the Herfindahl index results in an increase in TFP growth of 19.1 percent; a 10-percentage-points increase in import penetration results in a TFP growth loss of 6 percent. The negative effect of competition is particularly pronounced for state-owned enterprises, and is insignificant for privatized firms. The authors conclude that the technology gap between domestic firms and importing rivals may have been too wide and thus acted to discourage enterprise restructuring. In effect, domestic (state-owned) firms are priced out of their markets.

Grosfeld and Tressel (2001), using a panel of 153 Polish firms, find that the reduction in own market share by 10 percentage points results in 1.4-percentage-points increase in productivity growth. Again, this effect is stronger for privatized firms. The results are robust to the use of alternative estimation techniques, and the inclusion of controls for

ownership concentration and ownership types. The evidence supports the view that competition-enhancing policies and privatization should be pursued simultaneously.

The analyses indicate that product market competition has been a major force behind improvements in enterprise productivity in transition economies as a whole, as shown in table 8A. When we divide the sample into analyses based on import competition versus domestic market structure (for all countries together), we find that each is generally significant in explaining enterprise performance. The effect of domestic market structure is always significant. Import competition is insignificant in explaining enterprise restructuring when giving less weight to studies that do not confront endogeneity problems. Examining the results by region, the effects are strong for EE countries, where in all cases the t-statistics are significant at conventional levels. For CIS countries, increased competitive pressures are not associated with enhanced restructuring.

A further subdivision of the analyses shows an interesting pattern: import competition in the CIS countries does not have a significant effect on enterprise restructuring and has a negative sign. In contrast, import competition is significant in explaining enterprise restructuring in the EE sample, with t-statistics varying between 3.37 and 4.15. Since all the Russian studies use import penetration variables that are region specific, it would not be correct to conclude that this difference is due to the fact that Russia has a larger internal market and the effect of import competition may be muted or imprecisely measured. What explains this difference then? The evidence is consistent with the theoretical predictions in Blanchard and Kremer (1997) and Ickes, Ryterman, and Tenev (1995). In the CIS, the rapid opening to competition from abroad acted to deter domestic restructuring. Perhaps a gradual approach to trade liberalization is the preferred policy choice.

Changes in domestic market structure are important in explaining enterprise restructuring in both the CIS and EE samples. (The exception is in the CIS when we weight according to attention paid to endogeneity bias.) These results are upheld in a study of over 3,300 enterprises in 25 transition economies (Carlin et al. 2001) that shows strong positive effects of the reduction of market concentration on firm efficiency. The significant effect of changes in domestic market structure on enterprise restructuring in the CIS resonates with recent evidence on high barriers to entry in transition economies. Djankov et al. (2002a) document the number of procedures and the associated time and cost for starting a new business in 85 countries around the world, including 22 transition economies, to explain poor aggregate growth performance in the CIS. Establishing a new business in Russia or Ukraine takes twice as much effort, time, and money than a start-up in Eastern Europe, and three times longer than establishing a new business in Latvia or Lithuania. The authors argue that entry barriers serve to impede product market competition. Similarly, Andrei Shleifer and Daniel Treisman (2000) document the presence of significant geographic segmentation in Russia and lackluster competition at the regional level.

Table 8B examines the statistical significance of the variation in the effect of product market competition across regions and across types of competition. In the first panel, we show competition from local producers has a stronger effect than import competition, but the difference is not statistically significant. The second panel shows that competition has a stronger effect in explaining enterprise restructuring in EE countries than in CIS countries and that this difference is statistically significant when weighting for attempts to handle endogeneity, and when controlling for the overall quality of the paper. The third panel compares the relative importance of foreign

TABLE 8
IMPORT AND DOMESTIC COMPETITION AND ENTERPRISE RESTRUCTURING

Composite statistics derived from 82 analyses appearing in 23 studies

Category	Number of Analyses Contributing to the Composite Test Statistic	Weighting Method Used		
		(1) none	(2) attempts to handle endogeneity bias	(3) overall rating of quality
A. Testing the Effects of Competition		normally distributed test statistics		
All countries	82	5.13	4.14	5.01
Import competition	28	2.86	1.79	3.30
Domestic competition	54	4.27	3.78	3.79
EE, all competition	58	5.09	4.90	5.08
CIS, all competition	24	1.58	0.68	1.22
EE, import competition	20	3.97	3.37	4.15
CIS, import competition	8	−0.93	−1.12	−1.55
EE, domestic competition	38	3.41	3.65	3.16
CIS, domestic competition	16	2.59	1.56	2.10
B. Comparing Types of Competition and Regions		partial correlation coefficients and test statistics on their difference		
Import competition	28	0.003	−0.013	0.002
Domestic competition	54	0.025	0.015	0.021
Test statistic for difference		*−1.43*	*−1.80*	*−1.53*
EE countries	58	0.032	0.026	0.025
CIS countries	24	−0.018	−0.030	−0.014
Test statistic for difference		*2.68*	*3.05*	*2.08*
EE, import competition	20	0.036	0.029	0.028
CIS, import competition	8	−0.080	−0.087	−0.096
Test statistic for difference		*3.41*	*3.52*	*3.59*
EE, domestic competition	38	0.030	0.024	0.023
CIS, domestic competition	16	0.014	−0.002	0.017
Test statistic for difference		*0.46*	*1.21*	*0.30*

competition in the CIS and EE countries, suggesting that foreign competition has a large positive effect in Eastern Europe and the Baltics and a large negative effect in the CIS countries. The last panel shows that there are no discernible patterns in the way in which the effects of domestic market structure differ between the CIS and EE countries.

The findings on the effect of import competition deserve special attention. In the CIS, import competition has a large negative effect in economic terms, although this effect is statistically not robust. In Eastern Europe, import competition has a positive effect in economic terms, but the results of the individual studies are mixed, consistent with the literature on other developing economies.

6. *Soft Budgets*

The concept of soft budget constraints was coined by Kornai (1979, 1980) to explain shortages in socialist economies, especially in the market socialist system. Kornai describes the soft budget phenomenon as "firms are bailed out persistently by state agencies when revenues do not cover costs" (Kornai 1998, p. 12) and defines soft budgets as "the expectation of the decision-maker as to whether the firm will receive help in time of trouble . . . " (Kornai 1998, p. 14). Stiglitz further narrows the definition to situations when "enterprises believe that any losses they incur will be made good by the government" (Stiglitz 1994, p. 184).

Two alternative theories explore the causes of soft budgets.[34] Kornai (1979, 1980) relates the softness of budget constraints to the paternalistic attitude of the government, which results in the accommodation of enterprise requests for new finance. Firms are financed even when the expected new project's return is below the real interest rate. The government's goal is to provide economic security for enterprise employees and supply social services (kindergartens, schools, hospitals, recreation facilities) in the enterprise.

A different reason for the existence of soft budgets is advanced in Shleifer and Vishny

(1994) and Boycko, Shleifer, and Vishny (1996). These papers model bargaining between politicians and managers, which leads to equilibria with subsidies to firms and excess employment. Politicians use government subsidies to induce firms to maintain a higher-than-efficient level of employment in order to enlarge their own political constituency. Since the model predicts that subsidies increase when there is a decline in enterprise profits, these political decisions constitute a soft-budget policy regime.[35]

These two theories of the causes of soft budgets differ significantly. The first explains accommodating lending behavior determined by concerns of social stability, while the second suggests that soft budgets arise from politicians' narrower self-interest. In both, soft budgets compensate the enterprise for keeping surplus employment. The predicted effect on enterprise restructuring from soft budgets is lack of productivity improvements and continuation of unprofitable production (and nonproduction) activities.

The theoretical literature suggests that an empirical measure of the incidence of soft budgets needs to satisfy two criteria. First, it has to capture the expectations of enterprise managers of a future government bail-out, not the current policy of the government. Imprudent behavior of managers that results in poor enterprise performance can occur in enterprises that have not yet received government subsidies but expect to do so when they are in trouble. Also, enterprises that have received subsidies in the past may not expect those to continue in the future, perhaps as a result of new government policies. Second, the expectation of government support is contingent on the enterprise being in financial distress. Both Kornai's theory and the Shleifer-Vishny-Boycko model show that without the fear of imminent job losses,

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[34] A third theory, relevant for both transition and developed market economies, views soft budgets as the continued extension of credit even when the substandard performance of an already-financed investment project has been revealed (Michel Dewatripont and Eric Maskin 1995; Maskin 1999). Because of asymmetric information, even poor projects are initially financed. By the time creditors can observe project quality they will continue to lend, because refinancing may still maximize the expected value of funds that can be recovered. A fourth theory, advanced in Stiglitz (1994), suggests that soft budgets arise when financing institutions have an incentive to make large gambles. In this model, an insolvent bank is willing to invest in a risky project with low or even negative expected payoff, because the bank will become solvent if the gamble pays off; and if the gamble doesn't pay off, the bank will be no worse off than it was before it made the loan, i.e., it will still be insolvent. We do not discuss these two theories in the context of soft budget constraints in transition, because the source of soft budgets here is a (private) financial institution, not the government.

[35] In Shleifer and Vishny (1994) there are several types of equilibria, depending on whether bribes occur and on the nature of property rights. The effect of a decline in profits is either an increase or no change in subsidies, depending on which equilibrium pertains.

i.e., if the enterprise is solvent, there is no reason for the government to extend new financing to the enterprise.

Few empirical papers meet these criteria for measuring soft budgets. The study that comes closest to the prescribed measure is Anderson, Korsun, and Murrell (2000). They survey 249 enterprises that underwent Mongolia's mass-privatization program by mid-1995, asking enterprise managers what proportion of lost revenues the state would make up if losses threatened the enterprise's ability to maintain its employment level.[36] The resulting measure of soft budgets is expectational in nature, and the expectations are limited to the situation where the enterprise has lost revenues. Almost three-quarters of the surveyed enterprise managers, 73.1 percent, thought that the government would do nothing to help out, 2.4 percent of managers thought that the government would bail them out completely, and 13.6 percent of managers thought that the government would make up at least half of the decline in revenues. The perceived degree of budget softness was shown to be strongly correlated with the amount of central government ownership. Almost half of the enterprises with majority central government ownership expected some financial assistance, while 31.3 percent of the managers of these enterprises expected that the government would make up at least half the decline in revenues. The data on managers' expectations

is used in Anderson, Lee, and Murrell (2000) to study the effect of soft budgets on enterprise performance.

While theoretically most appropriate, the expectational measure used in Anderson et al. is not easily replicable in other studies since it relies on a survey instrument designed for studying the soft budgets phenomenon. In the absence of such data, other authors use quantitative variables that proxy for the expectations of enterprise managers, while different variables are used to control for the financial distress of the enterprise. The best example of this approach is David Li and Minsong Liang (1998), who construct indicators of soft budgets from survey data on employment of nonproduction workers, investment with below-average rates of return, and distribution of bonuses in excess of these regulated by the Chinese government. All three factors are shown to contribute to the financial distress of the enterprise. Using a sample of 681 enterprises for the period 1980 to 1994, Li and Liang find that if all of the non-production workers, defined as workers with zero marginal product (such as cleaners, political counselors, etc.), were eliminated, 38 percent of financial losses would be avoided. As another example, excessive bonuses accounted for 39 percent of total enterprise losses. The empirical question that the authors ask is whether enterprises respond to financial losses, defined as negative cash flows, by reducing any of the factors that cause these losses initially. The answer is No. All enterprises in financial distress expected to be bailed out by the government.

Several other papers illustrate the difficulties in constructing a soft budget proxy in the absence of survey data. These studies rely on data from the financial statements of enterprises. Wafa Abdelati and Stijn Claessens (1996) and Fabrizio Coricelli and Simeon Djankov (2002) use samples of over four thousand Romanian companies and distinguish among chronic loss-makers and adjusted enterprises. Chronic loss-makers are

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[36] The question was phrased as follows: "Suppose that unfortunate market conditions resulted in a sudden drop in your enterprise's revenues, so that you might have to lay off workers. How likely is it that the government (either national or local) would help your enterprise out, so that it would not be forced by its financial situation to lay off workers? Please indicate your expectation of the likely government reaction by choosing a point on the scale from 0 to 10—a "0" means that you think the government would do absolutely nothing to help out and a "10" means that you think the government would completely make up for the decline in revenues in some way, and a "5" means that the government would make up half the decline in revenues. Choose any number between 0 and 10, indicating your expectation concerning the extent to which the government would help out" Anderson, Korsun, and Murrell (2000, p. 222).

defined as enterprises with negative cash flows during the 1991 to 1994 period. Adjusted enterprises are defined as enterprises that had negative cash flows in 1991 or 1992 or both, but then recorded positive cash flows in 1993 and 1994. The proxy for soft budgets is the amount of new bank financing, net of accrued interest expenses. Since the banking system was fully in state hands in 1994, this proxy measure captures the major channel of government funding, as tax arrears and direct government subsidies are shown to be small relative to the flow of new credit. The authors seek to answer the question whether chronically loss-making enterprises received financial support in order to keep employment as high as that of profitable enterprises. The answer is Yes. In fact, during the sample period, loss-making enterprises received about 50-percent more bank credit as a share of revenues and shed less labor than did adjusted firms. Assuming that the government would continue its policy of keeping loss-making enterprises afloat, these studies provide empirical support for the presence of soft budgets. Stijn Claessens and Kyle Peters (1997) use a similar approach to study the effect of soft budget constraints in Bulgaria. They use trade arrears to other state enterprises, and the share of external financing in total assets, as alternative proxies for soft budgets. The dependent variable is the change in value added.

Earle and Estrin (1998) use data for about three hundred Russian enterprises during 1990 to 1994 and construct a measure of government assistance to enterprises, which they interpret as a proxy for soft budgets. The measure relies on flow-of-funds data and includes all possible channels of government support, such as federal subsidies and investments, tax benefits, preferential credits, extra-budgetary funds, tax exemptions and other benefits associated with foreign trade, and local government subsidies and tax arrears. In addition to relying on a current policy proxy, the analysis does not capture the financial distress of the enterprise, making the results difficult to compare with the theories of soft budgets. For example, product specific subsidies, often the result of price controls, can be given to both profitable and loss-making enterprises. Studies using such data document the effect of distortions due to price controls as much as the incentives of soft budgets.

Earle and Estrin are in good company. Claessens and Djankov (1998), Djankov and Hoekman (2000), Grigorian (2000), and Carlin et al. (2001) use similar variables as proxies for soft budgets. This pattern is in part explained by the fact that all five studies focus on the effects of ownership changes and use the soft budgets proxy only as a control variable. We include these studies in the survey since they provide valuable information on the incidence of government financing of loss-making enterprises in transition economies, and some of the effects that this financing has. If anything, our criticism of their approach reflects the dearth of research focused on the soft budgets issue.

The studies discussed so far document numerous channels of government support for ailing enterprises. While Abdelati and Claessens (1996) and Coricelli and Djankov (2002) identify new credit from the state-owned banking sector as the main conduit of soft budgets in Romania during the early transition period, and Claessens and Peters (1997) finds similar results for Bulgaria, Mark Schaffer (1998) shows that bank lending was not a major source of soft budgets in Hungary during the early transition period, even before the banking sector was privatized. Once the Hungarian banking system was largely privatized, by 1995, bank credit was restricted to profitable projects (Claessens and Djankov 1998). Tax and social security arrears to the central government were the main source of soft financing in Poland in 1993, while trade credit was not a source of soft budgets in either Hungary or

Poland.[37] These results are reflected also in Claessens and Djankov (1998) who find that, by the mid-1990s, financing of loss-making enterprises by the still state-owned banking sector was the primary channel of soft budgets in Bulgaria, the Czech Republic, Romania, Slovakia, and Slovenia, but not in Hungary or Poland. Not coincidentally, by the end of the 1990s all of these countries had experienced severe (Bulgaria and Romania) or at least significant (the Czech Republic, Slovakia, and Slovenia) banking crises.

Shleifer and Treisman (2000) show that tax exemptions by the local government are the main channel of soft financing in Russia, as do Gilles Alfandari, Qimiao Fan, and Lev Freinkman (1996) and Brown and Earle (2000). Direct subsidies from the central government are an important source of soft budgets in Kazakhstan (Simeon Djankov and Tatiana Nenova 2000) and Lithuania (Grigorian 2000). Finally, Shumei Gao and Mark Schaffer (1998) show that credit is always available to loss-making state enterprises in China.

What is the economic significance of hardened budgets on enterprise restructuring? In addition to the results already discussed, Li and Liang (1998) find that if all inefficient investment projects were eliminated, the average enterprise's profit margin would change from –8.7 percent to 2.3 percent. Claessens and Peters (1997) find that the presence of soft budgets in Romanian enterprises results in a reduction of labor shedding by 4 percent annually during 1992 to 1994. Coricelli and Djankov (2002) find that labor shedding was reduced by 4.6 percent during 1993–1995. Claessens and Djankov (1998) find a 2.7 percent unrealized TFP growth as a result of continued soft financing in the Eastern European countries. Djankov and Hoekman (2000) find an unrealized annual growth of 3 percentage points in Bulgaria over the 1992–95 period. Earle and Estrin (1998) find a 5.7 percent unrealized labor productivity growth. Alfandari, Fan, and Freinkman (1996) show that recipients of soft financing record labor productivity growth that is 6 percent less than that of non-recipients. Finally, Abdelati and Claessens (1996) find that a one-standard-deviation increase in the flow of financing from state banks in Romania is associated with a 14.2 percent unrealized labor productivity growth per annum. This evidence suggests that soft budgets are a major deterrent of enterprise restructuring.

We next turn to a discussion of the statistical significance of the results on soft budgets. We discuss the results of ten papers that use regression analysis to answer these questions. The data come from Bulgaria (Claessens and Peters 1997; Djankov and Hoekman 2000), Kazakhstan (Djankov and Nenova 2000), Lithuania (Grigorian 2000), Romania (Abdelati and Claessens 1996; Coricelli and Djankov 2002), Mongolia (Anderson, Lee, and Murrell 2000), Russia (Earle and Estrin 1998), and cross-country studies of the seven Central and East European countries (Claessens and Djankov 1998) and 25 transition economies (Carlin et al. 2001). They cover the period between 1992 and 1999, and contain 31 separate analyses.

Before discussing the aggregate results, however, we acknowledge the simultaneity problem that plagues most empirical work on soft budgets.[38] Theory suggests that poorly performing firms get bailed out, while good firms do not receive government financing since they don't need it. Researchers want to examine whether soft budgets cause poor performance, yet the data may be overwhelmed by the relationship operating in the other direction. Researchers have addressed this problem in various ways, but none is entirely satisfactory. Weighting papers by a

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[37] Overdue trade credit between private enterprises should not be considered as evidence of soft budgets. Also, John McMillan and Christopher Woodruff (1999) show that private trade creditors in Vietnam stop financing enterprises once their payments are two months in arrears.

[38] We owe this point to Mark Schaffer.

subjective measure of how the authors have handled the simultaneity bias and by the overall quality of the empirical analysis gives some perspective on the importance of the problem, but does not eliminate it.

Table 9A shows that the effect of hardened budgets on enterprise restructuring, defined as sales growth, TFP, or labor productivity, is very significant in EE countries and generally significant in CIS countries. The positive signs imply that hardened budgets have a beneficial effect on restructuring. Table 9B compares the size of the hardened budget effect across the two regions. The studies on EE and CIS countries show effects of similar magnitude, with the t-statistics for differences between partial correlations insignificant.

How can soft budgets be limited? Both Kornai and Shleifer-Vishny suggest, and the empirical literature shows, that privatization of enterprises is the main policy choice. Anderson, Korsun, and Murrell (2000) show that a 10-percent-larger share of central-government ownership increases by 9 percent the probability of receiving soft financing. Their estimates suggest that privatization reduced the percentage of enterprises with soft budgets from 78 percent to 23 percent. Similarly, Alfandari, Fan, and Freinkman (1996) show that the probability of receiving state support is reduced in half if the enterprise is private. Frydman, Hessel, and Rapaczynski (2000) find evidence to suggest that hardening budgets induces cost efficiency but not revenue restructuring, and that the only policy for the Czech, Hungarian, and Polish governments to commit to hard budgets was through privatization of enterprises. Without privatization, it is often rational for the government to extend assistance to loss-makers.[39]

[39] One example: A policy of offering generous severance pay for workers was tried in Romania in 1992 to 1994. Employees of large loss-making enterprises were offered twelve months of wages as severance. However, the government continued the flow of soft financing to large loss-making enterprises even after severance packages were offered, thus dooming the initiative to failure (Djankov 1999b).

The literature surveyed here also shows that larger state-owned enterprises are more likely to be the recipients of soft financing. This implies that policies that reduce the role of industry giants (through de-monopolization, split-ups, or spin-offs) will also reduce the presence of soft budgets (Lizal, Singer, and Svejnar 2001).

Other findings are less robust. Privatizing the banks may bring about a reduction in soft financing. A study of 92 economies, including eleven transition economies, shows that government ownership of banks is associated with lower TFP growth in the enterprise sector, stemming from inefficient allocation of resources across enterprises (Rafael La Porta, Florencio Lopez-de-Silanes, and Andrei Shleifer 2002). Beware of the Stiglitz (1994) model, however. There are many examples of private banks extending soft credits to enterprises as part of their own survival-through-gambling strategy. Finally, the decentralization of the government budget and decision making seems to have limited soft budgets in China (Yingyi Qian and Gérard Roland 1996) and Mongolia (Anderson, Korsun, and Murrell 2000) but served to increase the incidence of supporting failing enterprises in Russia (Blanchard and Shleifer 2000; Ekaterina Zhuravskaya 2000). Further empirical research is needed to be more definitive on the effects of bank privatization and fiscal decentralization.

7. *The Role of Managers*

Economic theorists have taken two broad approaches in assessing the role of managers in improving corporate performance. One focuses on manager incentives. Michael Jensen and William Meckling (1976) hold that managers want to pursue projects or consume perquisites that benefit them personally. This argument is based on the observation that in most modern corporations, as shown in Berle and Means (1933) and more recently in La Porta, Lopez-de-Silanes, and Shleifer (1999), ownership is separated from

TABLE 9
HARD BUDGET CONSTRAINTS AND ENTERPRISE RESTRUCTURING

Composite statistics derived from 31 analyses appearing in 11 studies

Region	Number of Analyses Contributing to the Composite Test Statistic	Weighting Method Used		
		(1)	(2)	(3)
		none	attempts to handle simultaneity bias	overall rating of quality
A. Testing the Effects of Hardening Budgets		composite normally distributed statistics		
All countries	31	8.44	6.81	7.63
EE	23	7.34	6.52	7.78
CIS	8	4.17	2.26	2.13
B. Comparing the Effects in the Two Regions		partial correlation coefficients and test statistics on their difference		
EE	23	0.027	0.025	0.029
CIS	8	0.065	0.045	0.045
Test statistic for difference		−1.53	−0.64	−0.50

control and hence managers (who have the control rights) do not bear the consequences of their actions. A variation of the agency problem, developed in Holmström (1979), studies unobservable managerial effort. When owners cannot judge how hard managers work, there is an incentive for managers to slack off. The implication of both theories is that when manager ownership is high, managers will act so as to increase enterprise efficiency.[40] Theoretical models on the early transition, e.g., Aghion, Blanchard, and Burgess (1994) and Aghion and Blanchard (1996), follow the incentive literature by showing how the incentives of managers to behave in a profit-maximizing manner improve once they hold a significant stake in the performance of their firms.[41]

A second theory focuses on management turnover (Sherwin Rosen 1992). The theory stipulates that if the manager is not capable or knowledgeable, no amount of incentives will do. In such instances, it is better to bring in a new manager. Barberis et al. (1996) apply this theory to data on Russian shops and find that the hiring of new managers brings about significant improvements in enterprise performance, while giving incentives to old managers does not.

In this section, we survey the literature that has studied government policy affecting the role of managers in improving enterprise performance in transition. Unlike the studies on western firms where corporate boards design manager incentives and fire managers for bad performance, the transition literature on managers to-date has focused primarily on government policies. Many of the studies discussed here use data on Chinese enterprises. There are few studies that test the theories of manager incentives and manager turnover in Central and Eastern Europe or the former Soviet Union. This disparity is a reflection of the policies taken

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[40] In both theories, the optimal level of managers' ownership is below 100-percent if managers are risk averse (Morck, Shleifer, and Vishny 1988).

[41] Furthermore, career concerns may lead managers of state-owned enterprises to restructure if privatization in the economy is imminent or under way, since privatization creates a market for managers and hence increases managers' incentives to outperform their peers (Roland 1994; Gérard Roland and Khalid Sekkat 2000).

by the Chinese government at the outset of economic reforms. Privatization was not seen as an option and instead the government sought to improve the performance of state-owned enterprises by giving their managers more autonomy and better incentives (Barry Naughton 1994).[42]

The decision to provide more autonomy and better incentives in China started in 1978 as an experiment in the agricultural sector (John McMillan, John Whalley, and Lijing Zhu 1989). It was quickly emulated in manufacturing (Keun Lee 1990). The evidence shows that managers responded to the new autonomy by strengthening the discipline imposed on workers, by increasing the proportion of workers' income paid in the form of bonuses, and by raising the fraction of workers on fixed-term contracts (McMillan and Naughton 1992). Enterprises also spent more on productive investment (Theodore Groves et al. 1994).

The incentive system let state enterprises retain profits, separated management from politics, and allowed managers to be treated as individual contractors, with their pay and bonuses to be linked to the performance of their units. These changes in incentives were so fundamental, that the process is sometimes called "privatization from below," e.g., Naughton (1994, p. 266). The policy was designed to give managers control rights, contingent on keeping government ownership intact.

The move to autonomy in decision making at the enterprise level may account for a large part of the estimated effect on enterprise restructuring. The autonomy effect needs to be controlled for, in order to estimate the effect of incentives per se. Fortunately, most of the studies that we survey use separate variables for the autonomy component of the responsibility system, e.g., Lee (1990), Groves et al. (1994), Roger Gordon and Wei Li (1995). The simulta-

neous introduction of several policies within the responsibility system also makes it difficult to differentiate the effect of manager incentives from that of manager turnover. Indeed, "stronger incentives and better managers are complementary changes. They might be so strongly complementary that neither change would be effective by itself. Some managers might be so inadequate as to be unable to respond to new incentives, no matter how well designed. Good managers might not work well under badly structured incentives. If so, restructuring is effective only if both changes—new managers and new incentives—are introduced together" (McMillan 1997, p. 7). We try to give the reader some sense of the relative importance of turnover versus incentives, but one should consider them two parts of a single policy.

The measurement of incentives creates some problems. While data on equity ownership and salaries of managers do exist and have been used in a number of studies, data on stock options and bonuses are less readily available. Yet these types of incentives could account for a large part of the manager's decision to stay with or join a particular firm and work hard on improving its performance. While liquid capital markets are rare in transition economies (Claessens, Djankov, and Daniela Klingebiel 2000) and stock options are not much used, bonuses or management pay linked to enterprise performance are used in all transition economies (Derek Jones and Takao Kato 1996). In China, bonuses or, in the case of enterprises with auctioned management positions, bonuses based on the bid price of the new manager are used in 97 percent of enterprises (Gordon and Li 1991). Outside of China, however, we could not find any empirical studies using such data.

The earliest study on management incentives and enterprise performance in China is Lee (1990), which constructs dummies for whether the enterprise has implemented a) all incentive reforms; or b) any one of these

[42] The Polish government followed a similar policy in the first years of transition (Pinto, Belka, and Krajewski 1993).

reforms. The results indicate that the adoption of all reforms yields about a 4 percent total factor productivity increase, while accounting for other firm characteristics such as the initial productivity level, export orientation, the ratio of nonmanagerial to managerial employees, and the ratio of temporary workers to total employment.

Groves et al. (1995) show that management incentives improved profitability in Chinese enterprises by 7.3 percentage points and that managers benefited significantly from this improvement: the average pay with new incentives was 37.4 percent higher than the previous average salary. Using the same data set to construct a ten-year panel, Groves et al. (1994) show that bonuses are positively associated with productivity in five manufacturing industries: textiles, chemicals, building materials, machinery, and electronics. In all but one, chemicals, this association is statistically significant. Gordon and Li (1995) and Li (1997) also find manager incentives to be effective in increasing total factor productivity in China. Both papers control for the timing of decentralization, as well as for non-linearities in the production function. Using data for 1983 to 1987, Gordon and Li find that firms implementing manager incentives experienced an additional 17 percent increase in productivity beyond that found for other firms. Li (1997) finds that an increase in bonuses by one percentage point raised the TFP growth rate by 0.089 percentage points between 1980 and 1984, and by 0.060 percentage points between 1985 and 1989. Similarly, Chun Chang, Brian McCall, and Yijiang Wang (2001) find manager incentives to be associated with a 4 percent increase in the return to equity, using a panel data set of eighty agro-processing enterprises from 1984 to 1993.

In contrast, Harry Broadman and Geng Xiao (1997) find a positive but generally insignificant effect of incentives on labor productivity in the early reform period, 1980 to 1985, and Shirley and Xu (2001) document a negative but generally insignificant relation between manager performance contracts and TFP growth in Chinese state enterprises. Claessens and Djankov (1999b) use data on stock ownership in publicly listed Czech companies and fail to find evidence of the effect of manager incentives on enterprise performance. However, all three papers lack good proxies for manager incentives. For example, Broadman and Xiao do not have data on managerial wages and bonuses and use the gross value of nonindustrial fixed capital as a proxy for in kind compensation. Shirley and Xu use a dummy variable for the presence of a performance contract as a proxy, but cannot differentiate among contracts that offer generous bonuses based on performance and contracts that are merely window dressing.

We turn to the effect of manager turnover next. The seminal study in the transition literature linking manager turnover and enterprise performance is Groves et al. (1995). They use a sample of Chinese state-owned companies to investigate the link between manager turnover and productivity growth. Poorly performing firms are more likely to have a new manager selected by auction,[43] and such managers are required to post a higher security deposit and are subject to more frequent performance reviews. Managers are frequently fired for poor performance and in such cases often lose part of their security deposit. The incidence of turnover during the sample period of 1980 to 1989 is very high. Only 11 percent of managers in place in 1980 remained managers by the end of the period, and almost half (44 percent) of managers were appointed after 1985. These changes are primarily a reflection of the functioning of the market for managers: only 25 percent of turnover is the result of retirement. The economic effect of replacing bad managers is high: new managers

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[43] Auctions for managerial jobs took place in 14 percent of the sample enterprises.

bring about a 16-percent increase in labor productivity.[44]

The literature on the effects of changes in managers in Central and Eastern Europe and the former Soviet Union suffers from an acute problem: management turnover often takes place as an endogenous decision after privatization. New owners pick new managers, and the effect of management change is confounded with ownership change, especially when the new owners themselves are the new managers. For example, the retail shops studied in Barberis et al. (1996) are in many cases manager-owned. Even for mid-size and larger enterprises, managers may be large owners (Kuznetsov, Mangiarotti, and Schaffer 2002) and hence lose their jobs when there is ownership change.

The data set in Claessens and Djankov (1999b) is well suited for empirical testing of the importance of management turnover. First, the privatization process in the Czech Republic prevented incumbent managers from obtaining significant ownership. As a result, management changes were separated from ownership change. The data display high turnover: 35.6 percent of managers in privately owned enterprises and 42.1 percent in those under state ownership. The authors construct dummy variables for enterprises without management changes, for enterprises where managers were replaced while under state ownership, and for privatized enterprises where the new owners hired new managers. These dummies are used alongside variables for the initial labor productivity level, the enterprise size, and manager's equity ownership to explain differences in annual labor productivity growth rates. Labor productivity growth increases by 4.2 percent with the appointment of a new manager in privatized enterprises. The appointment of new managers in state-owned enterprises yields a 3.5-percent increase in labor productivity growth.

In a similar study, Frydman, Hessel, and Rapaczynski (2000) use a sample of state-owned and newly privatized enterprises in the Czech Republic, Hungary, and Poland to study management turnover and its effect on the annual rate of revenue growth during the period 1991 to 1993. The turnover rate was extremely high: nearly two-thirds (64 percent) of managers were dismissed or moved voluntarily. Managers changed in 75 percent of state-owned enterprises, in 72 percent of enterprises with a dominant outside owner, and in 55 percent of insider-owned enterprises. In state-owned enterprises, management turnover is associated with a 3.1-percent increase in revenue growth, but this effect is statistically insignificant. In privatized enterprises, the effect is much larger, at 18.5 percent, and statistically significant. Using the same methodology in a study of large Ukrainian firms, Warzynski (2001b) finds that management turnover does not affect the change in productivity in state-owned enterprises, but has a small positive effect in privatized enterprises.[45]

The studies reviewed in this section show that manager incentives work and management turnover is almost always effective in improving enterprise performance. To understand the composite implications of these studies, we combine the regression results from twelve studies with 33 separate analyses, testing the importance of both managerial turnover and managerial incentives in restructuring (table 10).[46] We find that in all cases manager incentives and turnover, considered together, are an important determinant of restructuring. The pertinent t-statistics have values between 8.19 and 9.46. Separately, manager turnover and manager incentives also have significant effects on restructuring. Panel B directly compares the

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[44] Shirley and Xu (2001), using the same data set for 1986 to 1989, also find the economic effect of manager turnover to be quite high: a new manager brings about 4 percentage points higher TFP.

[45] The turnover was higher in state-owned enterprises, where 60.7 percent of management positions changed hands in 1993–97, than in privatized enterprises, with 47.5 percent of management turnover.

[46] The construction of panel A employs the method used in table 3, while the construction of panel B uses the method of table 4.

TABLE 10
THE ROLE OF MANAGERS IN ENTERPRISE RESTRUCTURING

Composite statistics derived from 33 analyses appearing in 12 studies

Region	Number of Analyses Contributing to the Composite Test Statistic	Weighting Method Used		
		(1)	(2)	(3)
		none	attempts to handle simultaneity bias	overall rating of quality
A. Do Managers Matter?		composite normally distributed statistics		
Both turnover and incentives	33	8.19	9.18	9.46
Management turnover	13	7.63	7.55	8.34
Management incentives	20	4.38	5.67	5.40
B. Comparison of Turnover and Incentives		partial correlation coefficients and test statistics on their difference		
Management turnover	13	0.034	0.032	0.038
Management incentives	20	0.039	0.047	0.040
Test statistic for difference		*−0.39*	*−1.01*	*−0.21*

size of the two economic effects. There is no statistically significant difference, indicating that management turnover can be as effective as manager incentives for enterprise restructuring. Note, however, that the evidence on incentives comes from China alone, and that this result seems to be much weaker in countries where incentive policies are not used as a substitute for ownership change (Qian 1999). In such countries, the evidence shows that privatization has a large and significant effect on performance, while manager-related variables usually do not have statistical significance (Warzynski 2001b).

Before concluding this section, we suggest one avenue for further research on manager incentives in transition. It is striking that the literature has not studied the presence of penalties (disincentives) for managers in the event of poor performance, asset-stripping, or spending enterprise resources for personal enrichment. While we have not been able to find any empirical literature on this topic, a multitude of press

reports on China suggests that the government has imposed harsh penalties, including the death penalty, for enterprise managers who abuse their power, e.g., *China Daily* (2000). In contrast, the literature on Russia suggests that the central government was unable to impose any such disciplines (Shleifer and Treisman 2000). The presence of such "sticks" may be an important determinant of the apparent positive effect of manager incentives in China (Chen Qingtai 2001). In an environment where negligent or fraudulent behavior by managers is severely punished when uncovered, managers have the choice of working hard and getting bonuses or slacking off and living off their salary alone. In contrast, where bad behavior goes unpunished, managers have the choice of stripping enterprise assets and getting a huge windfall now, as opposed to working hard through the years and receiving better compensation through bonuses. The evidence in Black, Kraakman, and Tarassova (2000) shows that many Russian managers pursue the former path.

8. *The Future Research Agenda*

In aggregating the results of the many papers that examine the determinants of enterprise restructuring, we have arrived at strong conclusions. Given the number of analyses being aggregated, almost six hundred in total, it is unlikely that any new study would modify these conclusions significantly unless its whole approach constituted a new departure.

Many of the studies under review were stimulated in part by the easy availability of new types of data, which opened up whole new avenues of enquiry. The design of the studies was not driven by attempts to address difficult methodological problems, by efforts to examine the effects of less easily quantifiable aspects of policy, such as the nature of institutions,[47] or by considerations that viewed transition in the wider context of development. In the future, the most productive research will focus directly on these three issues. In the ensuing subsections, we discuss each of the three.[48]

8.1 *Selection, Simultaneity, and Complementarities*

In designing a new research project that examines those determinants of restructuring that we have already reviewed, what should the central focus be? Our reading of the concerns of fellow researchers is that the most important source of skepticism about existing results centers on the possible biases in estimates that might arise either from simultaneous causation or from the selection of enterprises to be subjected to some policy. We have focused on this issue in the previous sections and we have argued that our conclusions are robust to such bias. However, we sense enough residual concern over this

issue to suggest that there is a large pay-off from any study that goes beyond the methods employed in the existing literature.

A study aimed at quelling all possible doubts about bias in estimates would focus, especially in the collection of data, on ways to diminish selection or simultaneity bias. Existing studies usually confronted these problems after data collection, then being forced to employ ad hoc methods. In contrast, it is with data collection that the most profitable route would begin. In the case of privatization and the effects of different owners, this would entail detailed, micro-institutional study of the process that generated private ownership. The hope would be that such study would uncover instrumental variables whose properties were so transparent that they left no concerns. In the case of competition or soft budgets, the research design would entail the construction of measures of competition or of budget hardness that were independent of enterprise performance. Again, detailed study of institutional processes would be highly beneficial, in leading the researcher to the types of data that have the desired exogeneity properties and in convincing the reader of this fact.

An examination of complementarities in policies provides a second area where new research might offer a significant possibility of modifying the conclusions reached above. In policy debates at the beginning of transition, there was great emphasis on complementarities. However, neither theory nor existing empirical studies provide strong guidance on where such complementarities exist or how to model or measure them.[49] Existing empirical work on complementarities relies mainly on testing the effects of simple interactions between policy variables, suggesting an ad hoc approach formulated after data collection. A more convincing analysis would focus on

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[47] This is a general characterization to which there are notable exceptions. For example, Joel Hellman, Geraint Jones, and Daniel Kaufmann (2000) and Carlin et al. (2001) sought measures of nonstandard enterprise determinants.

[48] When focusing on deficiencies in the literature, we should make it clear that our own papers are not exempted from such criticism.

[49] Angelucci et al. (2001), Grosfeld and Tressel (2001), Earle and Estrin (1998), Perevalov, Gimadi, and Dobrodey (2000), and Djankov and Hoekman (2000) all contain results on complementarities. However, the patterns in the results defy an easy summary.

complementarities at the beginning of research, designing data collection on the basis of both theory and detailed country-specific study. Using a structural approach to the estimation of complementarities, rather than trying different ad hoc interaction effects, would provide a more convincing analysis and would offer the possibility of policy implications of profound importance.

This is an appropriate juncture to make a more prosaic suggestion on future research. As we have become painfully aware while writing this paper, the existing literature has a number of deficiencies in the presentation of results and in the amount of information offered to the reader. All too often, papers obscure the type of quantitative information on economic effects that we presented in tables 1 and 7. Often, this information can only be extracted with great effort; indeed, in a significant number of papers, we could not deduce the pertinent magnitudes. However, the size of economic effects should arguably be the most significant results in papers that examine policy.[50]

Deficiencies in presentation of information become most significant when trying to compare or combine the results of papers on identical topics. For example, we would have been able to present more incisive conclusions in the previous sections, if authors of the surveyed papers had devoted more consideration to presenting results so that they could be accurately compared with the rest of the literature. In this respect, it seems that economics lags some other empirical fields (Hunt 1997).

8.2 *The Elusive Determinant: Institutions*

The beginning of transition coincided with the publication of North's (1990) influential book, with its central message that institutions were crucial to the success of market capitalism. This message was not at the forefront of policy discussions during the early years of transition.[51] Gradually the focus has changed, spurred by a recognition that the relatively poor performance of the CIS was not explained by differences in standard reforms. Institutions are now in vogue (Johnson, Kaufmann, and Shleifer 1997; Blanchard and Kremer 1997; Stiglitz 1999). It is no coincidence that the same emphasis is now present in the more general literature on growth and development (Robert Hall and Charles Jones 1999; Daron Acemoglu, Simon Johnson, and James Robinson 2001).

As Oliver Williamson (2000, p. 595) states, however, "we are still very ignorant about institutions." This is no more so than when relying on the evidence that is the focus here, the determinants of large-enterprise restructuring in transition. There is a dearth of studies including institutional variables. One reason for this is that research has followed policy, focusing on privatization, competition, and soft budgets. A second is that the institutional framework is often the same for all enterprises, leading to difficulties in designing tests. This has led to the use of indirect estimates to judge institutional effects.

One indirect approach uses variations across enterprises in the need for institutions. This has been employed to examine Blanchard and Kremer's (1997) theory that the erosion of contract enforcement mechanisms was a primary factor causing the large output fall in the CIS. They hypothesize that the need for contract enforcement is more critical for enterprises with more complex input requirements. Such enterprises would perform relatively worse in the period after the abandonment of contract enforcement through planning but before the creation of any effective alternative. This prediction

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[50] Other information is also frequently omitted. For example, the methodological appendix discusses how comparisons between different owners are made difficult because papers omit crucial statistical information.

[51] For a fuller history of the ebb and flow of policies in the first decade of transition, see Cynthia Clement and Peter Murrell (2001). The essays in Christopher Clague and Gordon Rausser (1992) constitute an exception to the early lack of focus on institutions.

also follows from the observation that the supply of information and the coordination of decisions was a central task of the now defunct planning apparatus (Murrell 1992).

Tests of this prediction relate measures of the complexity of enterprise relationships to economic performance, with complexity capturing the institutional effect indirectly by proxying the need for institutions. Konings (1998) measures complexity by the number of firms in the enterprise's sector and Jozef Konings and Patrick Paul Walsh (1999) use number of products produced by an enterprise, obtaining evidence that is generally supportive of the prediction in Bulgaria, Estonia, and Ukraine. Francesca Recanatini and Randi Ryterman (2000) use the measure suggested by Blanchard and Kremer (1997), the diversity of sources of inputs, with negative results. However, they do find that growth was lower in those enterprises that formerly received the strongest institutional support from central planning, one interpretation being that this variable is a proxy for some institutional need.

Johnson, McMillan, and Woodruff (2002) also examine contract enforcement, using a different, indirect approach. They exploit differences in managerial beliefs about the effectiveness of institutions, finding that managers who believe that courts can enforce contracts extend more trade credit than those who do not. These results suggest that effective courts can significantly enhance economic efficiency. In a similar study, Timothy Frye (2001) finds that the variations in the quality of police services, as measured in opinions, are important determinants of investment. Kathryn Hendley, Murrell, and Ryterman (2001) use the amount of legal human capital in an enterprise to assess the effects of law on Russian enterprises. They find that such capital is an important determinant of success in transactions, concluding that legal institutions affect enterprise performance even in supposedly lawless Russia. Although their measure of human capital is based on facts rather

than opinions, the conclusion that institutions affect performance is still only an indirect implication.

Corporate governance is another important element of the institutional infrastructure pertinent for large enterprises. But the evidence on this too is sparse and indirect. Using largely anecdotal evidence, Black, Kraakman, and Tarassova (2000) for Russia, and Stiglitz (1999) more generally, claim that the failure of corporate governance institutions has been of great importance. Anderson, Korsun, and Murrell (1999) do use systematic survey data to show that corporate governance laws work poorly in Mongolia, but they present no evidence on whether there is a cost in terms of foregone restructuring.

Black (2001) focuses on corporate governance practices in Russia enterprises. He estimates that a 600-fold increase in stock-market valuation would follow from implementing best-practice, rather than worst-practice, corporate governance. Although the differences in practices are not due to different institutions and although valuation is not the same as efficiency, these estimates are suggestive of the improvements that might be stimulated by more effective institutions.

Our analysis in section 4 also offers indirect evidence on the importance of corporate governance institutions. The owners most dependent on institutional support are diffuse individual owners, outsiders where there are a number of different block-holders, and perhaps even workers. Hence, the pattern of ownership effects in figure 2 is broadly consistent with the argument that corporate governance institutions functioned less well in the CIS than elsewhere (John Coffee 1999). This interpretation is reinforced when one examines the amount of variation of ownership effects in the two regions. There is much more variation in ownership effects in the CIS, suggesting that institutions have been more effective in Eastern Europe in providing the support

that is essential to some types of owners but not needed by others.[52]

Competition policy was a strong focus of early institutional reforms (Paul Joskow, Richard Schmalensee, and Natalia Tsukanova 1994). Our results in section 5 certainly show that competition is important, but existing research does not allow one to take the extra step to conclude that competition policy itself was a critical factor. Mark Dutz and Maria Vagliasindi (2000) aggregate data on individual firms to the country level and show that competition policy led to an increase in enterprise mobility. Brown and Earle (2001) show that competition-enhancing policies, such as price decontrol and infrastructure investment, increase the effect of competition on enterprise productivity in Russia. Again missing from this area of study is research that directly estimates the link between institutions and restructuring at the level of the enterprise.

This literature survey provides ample testimony to the fact that the evidence on the connection between institutions and enterprise restructuring is both scant and indirect.[53] Evidently, if institutions are to deserve the prominence in policy deliberations on enterprise restructuring that they presently have, empirical work at the enterprise level is a matter of great urgency.

8.3 *Transition as Development*

Over the last decade, the empirical research on enterprise restructuring in transition has developed as a separate field, with only sporadic cross-pollination with the more general literature on economic efficiency, growth, and development. Two factors explain this somewhat isolated stance. First, the research on transition partially emerged from the older comparative economics literature, which focused most of all on comparisons of socialism and capitalism. With the collapse of socialism and with capitalism constituting the only viable target, the subsequent rapid transformation in the former socialist economies led to a one-dimensional focus centering on the path from plan to market, when analyzing policy and the associated changes at the enterprise level. Second, the very specific characteristics of socialist enterprises caused researchers to view restructuring in transition as a singular task, rather separate from the issues of attaining corporate efficiency in market-capitalist economies. In section 2 we described a prototypical socialist enterprise and the many dimensions of the enormous changes that it faced in the 1990s. Many of these changes, e.g., selling shares for vouchers or the first contacts with foreign businesses, were unparalleled in other economies, both in character and in size.

In the years of transition, the former socialist countries have developed capitalist economies, albeit functioning quite differently from each other and differently from capitalist economies that traversed other historical paths. There is the odd country still clinging to a socialist-type economy, e.g., Belarus, and Uzbekistan, but even in such places there is privatization, entry of new private firms, import competition, and the establishment of some institutions characteristic of capitalism. With the most basic features of capitalism now established, the policy issues that China, Eastern Europe, and the CIS now encounter in their restructuring efforts are commonplace across the world's economies. For example, governments face the task of producing more effective corporate governance institutions and the need to resist the political pressures from industrial dinosaurs, while enterprises confront the challenges of entering new

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[52] Grigorian (2000) suggests an interesting institutional reason for the different results for banks in the CIS and Eastern Europe. Commercial banks in several Eastern European countries were reluctant to take large equity positions in privatized enterprises, in order to make their prudential practices consistent with those in the European Union. This led to less ability to impose restructuring.

[53] We should emphasize that we are restricting discussion to the focus of this paper, empirical work explaining restructuring in large enterprises.

markets and shaping corporate structures to attract finance. In short, the process of transition and the research on restructuring in transition have both matured to a point where future research efforts can integrate easily into a broader literature that studies differences between capitalist economies and between varieties of those institutions that support a vibrant corporate sector.

Such integration would greatly enrich the study of enterprise restructuring in transition, as it would bring to bear the findings of a voluminous literature on growth and development in capitalist economies. For example, this wider literature recognizes the fact that different capitalist economies evidence many different ways of solving the institutional problem of promoting enterprise efficiency. But the institutional solutions are not always, nor perhaps often, the result of direct efforts to improve efficiency. Rather, they are the product of the efforts of powerful interest groups that aim to enrich themselves. Hence, it is incorrect to consider a country's policy reforms and new institutions as efficient adaptations to a country's environment (Djankov et al. 2002b), as is quite often implicitly assumed in the literature that we have reviewed. For example, the difference between the effects of privatization and increased competition in Eastern Europe and the CIS, documented in this paper, would be profitably studied from the perspective of the political economy of the creation of institutions.

Scholars outside the field of transition will also benefit from the integration of research on restructuring in transition into the more general study of institutions and economic development. As evidenced in this paper, the transition experience provides multiple case studies of reform, exhibiting great successes and spectacular failures. Scholars of policy reform have much to learn from the study of the former socialist economies. The distinctive history of the transition economies, which still affects present structures, provides the extra exogenous variation in independent variables that is so important in the genera-

tion of incisive empirical results. But this variation is only useful if it is one of degree rather than kind, meaning that lessons can be drawn from country comparisons. Now with transition and transition research into its second decade, we conclude that this is the case.

9. *Concluding Comments*

We have surveyed the research on enterprise restructuring in transition to synthesize its main findings and to convey the evolving structure of this field of study. We find plentiful work examining the effects on restructuring of privatization, different types of new owners, competition, hardened budget constraints, and policies on managers. Using a methodology that enables us to combine the many results on each of these determinants, we analyze the effects of each separately, contrast the size of the various effects in Eastern Europe and in the CIS, and compare the results on different determinants to each other.

A capsule summary of the results appears in the Introduction and no reiteration is needed here. But we do leave the reader with one last perspective, in figure 3. The figure pulls together the comparable quantitative evidence on the effects of the main policy reforms pursued by transition economies in their efforts to restructure the enterprise sector. Although it is a somewhat hazardous exercise to compare partial correlation coefficients reflecting the effects of such disparate policies, such comparisons constitute the only concrete evidence that is available to judge which policies are most effective. In our view, this figure aptly summarizes the findings of the preceding sections.[54]

Privatization to outsiders is found to have the largest positive effects on enterprise restructuring, both in Eastern Europe and in the CIS. Hardened budgets are also economically significant in explaining restructuring. Increased competition is associated with positive results in Eastern Europe but

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[54] Indeed, it is based on the partial correlation coefficients reported in figure 2, and tables 8b and 9b.
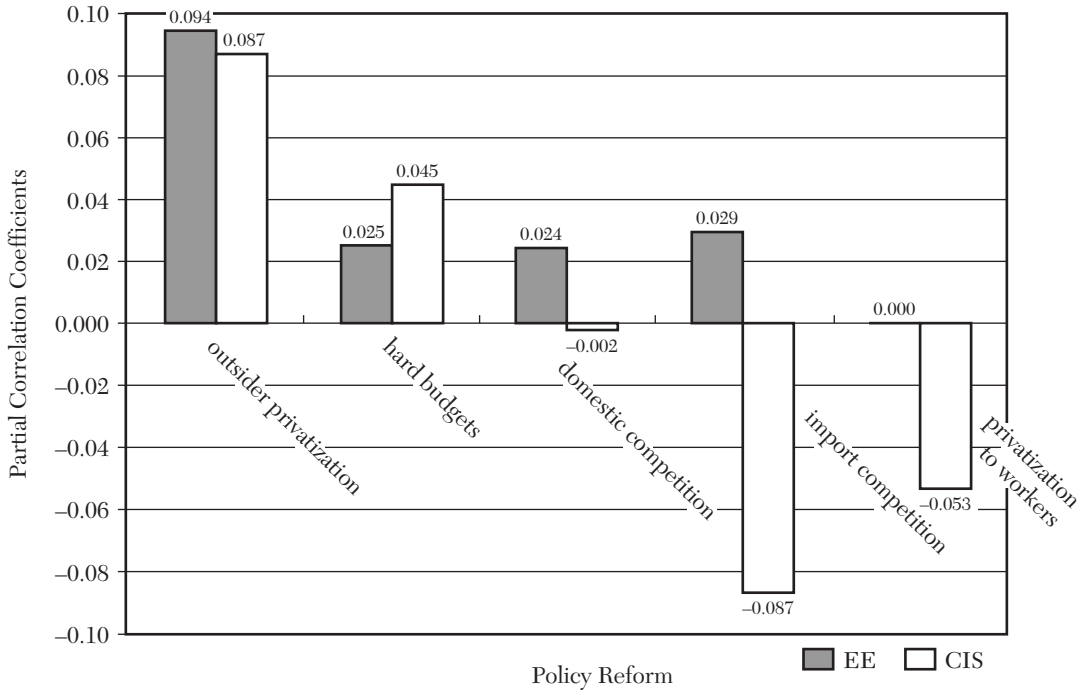
Figure 3. How Policy Reform Affects Enterprise Performance.
Comparing Economic Effects in Eastern Europe to Those in the CIS

not in the CIS. In fact, in the initial transition period, import competition is detrimental to enterprise restructuring in Russia, Ukraine, Kazakhstan, and the other CIS countries. Finally, privatization to workers has not enhanced restructuring in Eastern Europe and has had negative effects in the CIS.

In section 8, we suggested directions for future research. Large rewards await the researchers who venture into those areas, especially those who are able to isolate the effects of institutions on enterprise restructuring, and who do so in a manner facilitating precise comparisons between the effects of institutional reform and the effects of the policies examined above.

Methodological Appendix

This appendix develops the methods introduced in sections 3.2 and 4 and employed in sections 3–7. This appendix should be read in conjunction with sections 3 and 4.

(a) *Comparing Sets of Studies*

For expositional purposes, begin with the simplest linear model:

$$Y = \alpha + \gamma P + \varepsilon \qquad (A.1)$$

where all variables and parameters are as defined as in equation (1) of the text. Variances of the pertinent variables (and their estimates) are denoted by σ_Y^2, σ_P^2, and σ_ε^2 where $\sigma_Y^2 = \gamma^2 \sigma_P^2 + \sigma_\varepsilon^2$. The t-statistic corresponding to the kth study's $\hat{\gamma}$ for equation (A.1) is then:

$$t_k = \hat{\gamma}_k n_k^{1/2} \sigma_{Pk} / \sigma_{\varepsilon k} \qquad (A.2)$$

where n_k is degrees of freedom in the kth study. We assume that sample size is large relative to the number of parameters estimated, so that it approximates degrees of freedom. This is a pragmatic assumption, since many studies do not indicate precisely how many parameters are estimated, leaving degrees of freedom unknown.[55]

On inspection of equation (A.2), it is apparent that the presence of sample size in the t-statistic renders it inappropriate for cross-study comparisons

[55] For example, studies often include industry dummies without stating the number of sectors.

of economic size of effects (of ownership, competition, etc.). But (A.2) shows that t-statistics have an important property: they are invariant to changes in units of Y or P. When comparing estimates across a heterogeneous collection of studies, we seek a statistic that does not reflect sample size but is invariant to units.

The standard procedure in the meta-evaluation literature is to use a statistic that is intermediate between the t-statistic and $\hat{\gamma}$ (Rosenthal 1984). This is the correlation coefficient, which is scale free and does not depend on sample size:

$$r_k = \hat{\gamma}_k\, \sigma_{Pk} \Big/ \sqrt{(\sigma_{Pk}^2\, \gamma^2 + \sigma_{\varepsilon k}^2)} = \hat{\gamma}_k\, \sigma_{Pk} \big/ \sigma_{Yk} \qquad (A.3)$$

It is now simple to make an analogy to the case in which other variables (X) are present. We simply use partial correlation coefficients, where the σ_{Yk} and σ_{Pk} that appear in equation (A.3) are now the standard deviations of the errors in a regression of X on Y and the standard deviation of the errors in a regression of X on P. Loosely speaking, the variables that are correlated are those that capture variations in P and Y after P and Y have been purged of any variations due to X: P and Y controlling for X. There is a similar adaptation in the t-statistic: equation (A.2) should also use the standard deviations of P and Y controlling for X. Since partial correlation coefficients are usually not published, it is fortunate that (A.2) and (A.3) imply that there is a simple relation between t-statistics and the corresponding correlations:

$$r_k^2 = t_k^2 \big/ (t_k^2 + n_k) \qquad (A.4)$$

where the formula applies equally to partial correlations (Greene 2000).[56]

When testing correlation coefficients, it is usual to use a transformation, "Fisher's Z_r" $(\ln[(1 + r)/(1 - r)]/2)$, which is approximately normally distributed. Its variance is $[1/(n - 3)]$, where n is the sample size used to calculate the correlation coefficient (William Shadish and Keith Haddock 1994). Tests on the means of a set of correlations involve transforming each correlation and using the means of the transformations, also normally distributed. Moreover, weighting procedures can be employed to find weighted-mean partial correlations and to conduct tests on the weighted means of the Z_r transformations, so that one can investigate, just as in table 4, how sensitive are the results to such factors as study quality.

(b) *Combining the Results of Studies on the Effects of Different Types of Owners*

Two new problems arise when combining the results of studies using (4), instead of (1). First, important information is missing. Second, bilateral comparisons of ownership effects must be combined to produce a multilateral comparison. We address each problem in turn.

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[56] The sign of the rk is obtained from the sign of the estimated γ.

(b.1) *Missing Information on the Variance of Ownership Effects*

Here, there is no loss in generality in examining a case where the typical study estimates:

$$Y = \alpha + X\beta + \delta O + \theta I + \varepsilon \qquad (A.5)$$

O and I are the amounts of ownership held by two different owners, e.g., insiders and outsiders. (The generalization to more owners appears below.) Whatever is not owned by these two is owned by a third entity, the state, the omitted ownership share. δ and θ are the parameters of interest and all other variables are as defined before. For estimates of (A.5) to be usable in the present context, it is necessary that O and I be measured on the same scale within a single study (but not necessarily across studies), so that δ and θ are comparable.

In papers estimating (A.5), the information of prime interest is the comparison of δ to θ (outsiders versus insiders) and each of these to 0 (insiders or outsiders versus the state). Papers present the information to make the latter comparison, in the usual listing of coefficients and t-statistics, which can be converted into partial correlation coefficients showing the effect of changing ownership from state to outsider or state to insider. But obtaining all information pertinent to the comparison of δ and θ presents some difficulties.

We require a t-statistic for a test of the null hypothesis that $\delta = \theta$. This statistic is a function of $\hat{\delta}$ and $\hat{\theta}$ and estimates of the variances of $\hat{\delta}$ and $\hat{\theta}$, all of which usually appear in papers, plus an estimate of the covariance of $\hat{\delta}$ and $\hat{\theta}$, which is invariably omitted, since:

$$\text{Variance } (\hat{\delta} - \hat{\theta}) = \text{Variance } (\hat{\delta})$$
$$+ \text{Variance } (\hat{\theta}) - 2\text{Covariance } (\hat{\delta}, \hat{\theta}) \quad (A.6)$$

We sought a pragmatic method of estimating the variance of $(\hat{\delta} - \hat{\theta})$. O and I will almost always be negatively correlated because they are shares of ownership. Hence, the covariance of the estimates of the parameters attached to O and I will almost certainly be positive. Therefore assuming the covariance equals zero will lead to an overestimate of the variance of $(\hat{\delta} - \hat{\theta})$.

We have verified this point in three ways. First, take a simple theoretical case. Assume that outsiders have 100-percent ownership in a third of enterprises, insiders 100 percent of another third, and the state completely owns the last third. Then, the covariance of $\hat{\delta}$ and $\hat{\theta}$ is equal to one-half of the variance of either $\hat{\delta}$ or $\hat{\theta}$. Second, we have used simulated ownership data with five ownership types and have consistently found in these simulations that the estimated covariance of $\hat{\delta}$ and $\hat{\theta}$ is positive and at least half the size of the smaller of the variances of the two estimated parameters.[57] Third, we have investigated the size of the variance of $(\hat{\delta} - \hat{\theta})$ in two data sets, those used in Anderson, Lee,

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[57] Note that the comparisons between the relative sizes of variances and covariances does not depend on either the size of errors or the data on Y. As in the theoretical result, we assume that the equation does not contain X.

and Murrell (2000) and Claessens and Djankov (1999b). For those data sets (with various configurations of X's and ownership types), we found that the standard error of $(\hat{\delta} - \hat{\theta})$ varied between 75 percent and 122 percent of the standard errors of the $\hat{\delta}$'s and the $\hat{\theta}$'s.

Hence, the variance of $(\hat{\delta} - \hat{\theta})$ will almost certainly lie in the interval between the sum of the variances of $\hat{\delta}$ and $\hat{\theta}$ and the mean of the variances of $\hat{\delta}$ and $\hat{\theta}$. This suggests that we can take the t-statistics of $\hat{\delta}$ and $\hat{\theta}$, calculate the corresponding standard errors (S.E.'s) or variances (var.'s), and then form a crude estimate of the S.E. of $(\hat{\delta} - \hat{\theta})$:

$$\text{S.E.}(\hat{\delta} - \hat{\theta}) = \max\left\{1.25\sqrt{[\text{Var.}(\hat{\delta}) + \text{Var.}(\hat{\theta})]/2},\right.$$
$$\left.\max\left(\text{S.E.}(\hat{\delta}), \text{S.E.}(\hat{\theta})\right)\right\} \qquad \text{(A.7)}$$

If the first term within the braces on the right-hand side of (A.7) applies, the estimated variance will lie between the mean estimated variance of the individual parameters and the sum of the estimated variances of the individual parameters. The 1.25 factor is a conservative adjustment, corresponding to the assumption that the covariance will usually be somewhat less than one-half of the mean of the two variances. (Alternatively, if the covariance were zero, which is unlikely, this factor would need to be $2^{1/2}$). The second term within the braces is again a conservative adjustment, used when the variances of $\hat{\delta}$ and $\hat{\theta}$ are of quite different sizes (which is the case in only 25 percent of the analyses used in this paper). That element of (A.7) ensures that a very small variance does not influence the results too heavily.

Obviously, the procedure that we apply is not first best, but first best is not possible given the available information. Without a pragmatic second-best procedure it would not be possible to exploit the vast amount of information in the literature on the effects of different types of owners. In formulating (A.7), we have used conservative assumptions to ensure that we do not overestimate differences between owners. However, below, we do test whether these assumptions are too conservative.

(b.2) *Combining Many Bilateral Ownership Comparisons*

The present subsection examines how to aggregate information on the effects of different types of owners taken from many studies. Since each study usually contains several types of owners, each contributes several different pair-wise comparisons of owners (i.e., observations). The central variables of interest in each observation are the t-statistics and sample sizes, the latter denoted n_{ijk}. The t-statistics are obtained either directly from the studies or by using formula (A.7). Application of (A.4) gives partial correlation coefficients, r_{ijk}, where $i,j = 1, \ldots, 11$ (the number of categories of owners) and $k = 1, \ldots, K_{ij}$, with K_{ij} the number of studies that contribute information on the i,j-th ownership comparison. r_{ijk} estimates the effect of privatizing to ownership type j rather than to ownership type i.

For some comparisons, e.g., insiders versus managers, K_{ij} is zero because studies using enterprise data invariably make the sensible methodological decision not to use overlapping ownership categories within a single regression. For many other ownership comparisons, K_{ij} is quite small, since data on many types of owners (e.g., banks, funds, etc) are not available for many countries. Therefore applying the methods of section 3 in a straightforward manner does not get us very far. We seek a method that combines information from all data points when obtaining estimates of the effect of each type of owner.[58] This is accomplished using a dummy-variable regression framework:

$$r_{ijk} = \sum_{m=1}^{11} \lambda_m D_{ijk}^m + \varepsilon_{ijk}$$

$$D_{ijk}^m = \begin{cases} 1 & \text{if } m = i \\ -1 & \text{if } m = j \\ 0 & \text{otherwise} \end{cases} \qquad \text{(A.8)}$$

As we have information on only the relative performance of owners, it is not possible to estimate all eleven partial correlation coefficients (λ_m). Thus, we compare all owners to traditional state ownership and focus on $\hat{\lambda}_m - \hat{\lambda}_1$ ($m = 2, \ldots, 11$), the effect of switching from traditional state ownership to owner m.

The r_{ijk} reflect many observations and it is important to use this fact in estimating (A.8). If the underlying data are normally distributed, then the variance of r_{ijk} is approximately:

$$\text{var}(r_{ijk}|ijk) = (1 - r_{ijk}^2)^2 / n_{ijk} \qquad \text{(A.9)}$$

Given that $\text{var}(\varepsilon_{ijk}) = \text{var}(r_{ijk}|ijk)$, generalized least squares can be employed to use this variance information in estimating the $\hat{\lambda}_m - \hat{\lambda}_1$ and their standard errors.[59,60]

For 30 percent of the observations, it is not necessary to apply (A.7) since the pertinent t-statistic appears in the paper. We can therefore ask whether there are any systematic differences between the estimated ownership effects found from these 30 percent of observations and those found with the remainder of the

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[58] For example, if there is information on owners A and B versus owner C and information on A and B versus D, then the effect of C versus D can be estimated without having access to any study that matches C versus D.

[59] Since we have estimates of the actual error variances, our GLS procedure uses the assumption that the error variances in the regression are equal to these estimates. The numerical results are easily obtained using any standard GLS routine. However, the standard output must be reinterpreted to take into account the assumptions on error variances. See Larry Hedges (1994).

[60] Since each data point is a bilateral comparison and since the number of bilateral comparisons taken from a paper is greater than the number of owners in the paper, some underestimate of standard errors might occur leading to some overestimate of the t-statistics in table 6. This argues for a small amount of discounting of the t-statistics in table 6.
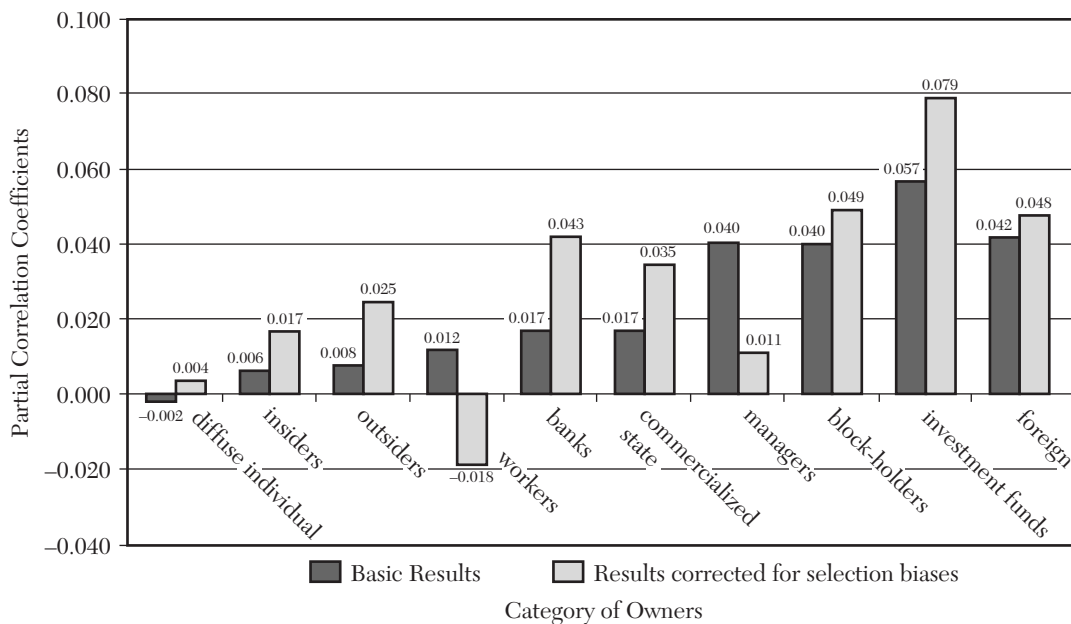
Figure A.1. Judging the Effects of Selection Bias on the Results

observations, which use (A.7). This question is addressed by estimating the following equation using generalized non-linear least squares:

$$r_{ijk} = \sum_{m=1}^{11} \lambda_m D_{ijk}^m + \eta D \sum_{m=1}^{11} \lambda_m D_{ijk}^m + \varepsilon_{ijk} \quad \text{(A.10)}$$

D is a dummy variable equal to 1 when the observation on r_{ijk} was derived using (A.7). The estimate of η provides the information on whether we have been too conservative in formulating (A.7). In fact, we did find this to be the case, $\hat{\eta}$ implying that application of (A.7) leads to r_{ijk} that are 32 percent smaller than appropriate.[61] When comparing the $\hat{\lambda}_m - \hat{\lambda}_1$ derived from (A.8) and those from (A.10), the latter leads to slightly larger differences in the effects of different owners than those found when estimating (A.8).[62] This is to be expected given that $\hat{\eta}$ implies that (A.7) creates r_{ijk} that are smaller than appropriate and that estimation of (A.10) compensates for this. However, the ordering of the effectiveness of the different owners is unchanged. Thus, we conclude that our pragmatic approach to fill-ing in missing information is warranted and does not distort the results. Given the information generated in estimating (A.10), we use the results from this formulation in the paper and in the remaining parts of this appendix.

As in section 3, we classified papers into three groups and generated corresponding weights reflecting the papers' attempts to counter the problem of selection bias. Then in estimating (A.10), we applied these weights to the observations. Thus, in this appendix we present two sets of estimates, ones that treat all studies equally and ones that more than proportionately reflect the results obtained in papers that pay greater attention to solving the problem of selection bias.

The two sets of estimates appear in figure A.1. There is a large amount of consistency between them, suggesting that the overall interpretation of the results is not vitiated by the possibility of selection bias. The correlation between the two sets of estimates is 0.70. Nevertheless, the results for some owners, especially managers and workers, do change substantially with the selection bias correction. With the estimates for workers and managers removed, the correlation between the two sets of estimates is 0.96. These facts provide the basis for using the selection-bias corrected estimates in section 4 and for general confidence in these estimates, except for some residual doubt about managers and workers.

[61] Formula (A.7) contains two elements, to be applied in different situations. We also tested whether these two elements led to systematically different biases in the estimated partial correlation coefficients, using a method analogous to that in (A.10). The tests strongly endorse the hypothesis that the two biases are equal.

[62] The relevant details are in Djankov and Murrell (2000).

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