Enterprise Restructuring in Transition:
A Quantitative Survey

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Abstract.

We review the voluminous empirical literature analyzing the process of enterprise restructuring in transition economies, synthesizing the results of papers using meta-analysis. We provide new insights into the relative effectiveness of different reform policies, and into how this effectiveness varies across regions. We address new and enduring questions of economics, such as the effects of privatization, the importance of different types of owners, the role of managerial incentives versus managerial human capital, the consequences of soft budgets, the effects of competition, and the role of institutions.

Many shall run to and fro, and knowledge shall be increased.
Daniel 12:4

1. Introduction

Over the last decade, more than one hundred and fifty thousand large enterprises in twenty-seven 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 netherworld of barter and ersatz money. 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 for 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 very 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 blockholder ownership? Which private owners are most effective, managers, workers, banks, or investment funds? 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? Does competition promote productivity change? Which institutions are necessary to complement other mechanisms of change?

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, and the ramifications for the world economy and polity. Analysis of the determinants of enterprise restructuring is central in any effort to develop an understanding of the effects of reform measures in transition countries. With enterprise restructuring apparently more successful in some countries than others, the natural question that arises is whether relative success is systematically related to policy. In this paper, we address this question by examining how the effects of policy have varied between transition countries.

The enduring questions of economics and the immediate policy concerns overlap when examining the issue of ownership. While the role of state versus private ownership has been at issue for more than a century, privatization has been the pre-eminent policy reform of the 1990's. At the beginning of the transition, the speed and character of privatization was one of the most intensely debated issues (Lipton and Sachs 1990, Murrell 1992). Now, the early emphasis on fast privatization is subject to intense criticism (Stiglitz 1999, Black, Kraakman and Tarassova 2000). But the formulation of this criticism has not taken full advantage of the available evidence on the effects of privatization. A comprehensive analysis of the evidence, which we provide below, is necessary to assess these privatization debates.

Like bees in a newly discovered field of clover, economists have gathered an enormous amount of information on enterprise restructuring in transition countries. The literature that will undergird this review is voluminous, but not as easily digestible as honey. The relevant papers appear in a wide range of outlets and, given long publication lags, many significant contributions are still in working paper form. Even scholars preoccupied with the transition process are finding it difficult to keep abreast of developments. Important results of potentially widespread interest (e.g. on the effects of ownership change) are buried within papers that focus on more narrow transition-related themes, escaping the attention of the broader economics profession. Thus, only a focused effort at canvassing and synthesizing this literature would suffice to bring out the central lessons of the large variety of available empirical evidence.

The objective of the present paper is to survey and to synthesize the evidence on the determinants of enterprise restructuring in transition. To date, there has not been a broad synthesis of the literature that has focused on the hard empirical evidence.[1] We provide such a synthesis, summarizing the composite conclusions emanating from more than one hundred studies. Where possible, we compare the results from the transition literature with those from studies of mature 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 soon result in a hard-to-remember list. An interpretative summary presents its own dangers. Experimental evidence shows that reviewers are not reliable when synthesizing the statistical results of any more than a few papers (Hunter and Schmidt 1990, Rosenthal, 1984). 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; Pohl, Anderson, Claessens, and Djankov, 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 (Hunt, 1997).[2] Apart from making our methods of synthesis transparent, application of meta-analysis has several other advantages. First, it provides the ability to use the results of many studies on a similar topic, combining many tests with weak power to produce a single

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[1.] Previous survey papers in this area (for example World Bank 1996, Brada 1996, EBRD 1998, Havrylyshyn and McGettigan 1999) used quite limited empirical evidence, which came almost exclusively from the Central Europe and China. Now studies of other countries (the former Soviet Union, Mongolia, and Vietnam) are beginning to be numerous, providing a much wider variety of evidence. Nellis (1999) does cover the full range of countries, but we go beyond this by providing a more systematic summary of the evidence and by focusing on a wider set of determinants of enterprise restructuring.

[2.] Examples of recent use of meta-analysis in economics are Smith and Kaoru (1990), Smith and Huang (1995), Neumark and Wascher (1998), Phillips and Goss (1995), and Stanley (1998).

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 more sound.

The paper is organized as follows. Section 2 lays out the methodology. 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 on the restructuring process. Section 5 documents the role of managers, focusing on management turnover and manager incentives. Section 6 analyses the role of soft-budget constraints in delaying or limiting productivity enhancements. Section 7 links product market competition and enterprise restructuring efforts. Section 8 examines the importance of the institutional and legal framework for enterprise restructuring. We conclude with some reflections on directions for future research.

We find that, on aggregate, privatization is strongly associated with more enterprise restructuring. 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 quality of analyses and other methodological factors. The privatization effect is, however, ambiguous and variable in the countries of the Commonwealth of Independent States (CIS). For those countries, the judicious conclusion is that there is no strong evidence of either positive or negative effects of privatization on restructuring.

The survey also documents the effects of different types of owners on enterprise restructuring. The most effective owners (investment funds, foreigners, and blockholders) produce amounts of restructuring that are much greater than produced by the worst owners (diffuse individuals and workers), who are statistically indistinguishable from traditional state ownership. However, state ownership within

partially-privatized firms is surprisingly effective, always producing more restructuring than enterprise insiders (as a whole), workers, and diffuse individual ownership, and matching the restructuring produced by managers and banks.

The effects of different owners varies between regions. Workers and outsiders are relatively better owners in Eastern Europe than in the CIS, while banks, concentrated individual ownership, and managers are relatively more effective in the CIS than elsewhere. Indirect evidence suggests that these differences are at least in part due to less well-functioning institutions of corporate governance in the CIS countries. When those institutions are weak, the effect of diffuse owners, outsiders, and workers is greatly diminished.

One mechanism through which private ownership affects performance is in the selection of managers who can run the firm efficiently. We test the hypothesis that management turnover – or more broadly, bringing in new human capital – is associated with improved enterprise performance. Statistical analyses show that this is the case. We do not find evidence that the strengthening of managerial incentives leads to a larger amount of 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. The effect is strong in Eastern Europe. The results are more ambiguous for the CIS, suggesting that hardened budgets work in that region too, but that their effect is weaker than elsewhere.

Product market competition has a significant effect in improving enterprise performance. The sources of improvement differ between regions, however. In Eastern Europe, import competition has a large effect. In contrast, in the CIS, domestic competition, through new entry or de-monopolization, is statistically significant in explaining restructuring, while import competition matters much less (and might even have a negative effect on enterprise restructuring).

Finally, we examine the role of institutions in restructuring. There is a relatively small amount of empirical evidence on this topic. Moreover, the methodologies used and the hypotheses tested vary so much between papers that it is not possible for us to synthesize results in the way that we do for the other issues examined here. The empirical literature suggests that when effective institutions are lacking, costly substitutes emerge in their place. This, in turn, implies that benefits could flow from second-best measures in other policy areas. For example, if corporate governance institutions are weak, it might not be beneficial to privatize to those owners who would be most effective were they operating in a world of well-functioning institutions. Similarly, the strength of contract enforcement institutions can influence the effectiveness of different owners, again suggesting greater benefits from a second-best privatization policy. Institutional development can foster progress in two ways: helping to moderate the deleterious effects of sub-optimal policies and creating fertile territory for the implementation of first-best policies.

Before proceeding further, we mention three important topics relevant to the study of the microeconomics of transition, which we do not examine. First, we do not survey the burgeoning literature on entrepreneurship in transition (see Johnson, McMillan, and Woodruff (1999b) and Bratowski, Grosfeld, and Rostowski (2000)). Second, we do not examine activity in the informal sector and the reasons for informality (Johnson, Kaufmann, and Shleifer (1997)). Third, we do not examine the effect of restructuring policies (e.g. ownership changes) on the broader institutional environment (except in the section on hardened budget constraints). On this topic, see Murrell (2001) and Shleifer and Treisman (2000).

2. Methodological Prologue

What is enterprise restructuring and what changes might induce it in transition countries? The answer to this question lies in the 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 here. (See Berliner 1976, Murrell 1990, and Kornai 1992 for details.)

The classical socialist enterprise received a plan on output levels and on inputs to be used in the production process. Meeting this plan was of prime importance and the plan was normally an ambitious one. 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 responded to a complex mix of monetary and career-based incentives, which were a function of fulfillment of the plan, enterprise performance, and political loyalty. The crucial point here is simply that enterprise profits and enterprise efficiency were much less important to a socialist manager than to any manager of a capitalist firm, even one fortunate enough to be the full beneficiary of Berle and Means' separation of ownership and control.

A labyrinthine bureaucracy replaced the institutions and the markets of capitalism. It found customers and determined prices, with bureaucratic pressure substituting for competition. The state interceded between producer and buyer, most notably in isolating enterprises from domestic consumers and foreign markets. The bureaucracy acted as a contract generating and a contract enforcement agency. Its one-year plans were an immediate guarantee of short-term working capital. A centrally-determined investment project would automatically receive long-term credits. Given the ubiquitous role of the state, much would be decided by negotiations, which were a major concern of top managers and a key element of their expertise. One consequence of the frequency of these negotiations was the universal presence of easy financing, which further turned manager's attention away from profits and efficiency.

Internally, the enterprise was organized along very hierarchical lines. One-person rule was in place, and that one person was surrounded by process engineers, not by marketing personnel or developers of new products. Workers had virtually no role in enterprise decision-making, except in the limited sphere of personnel policy, where a variety of factors led to firing rates that were extremely low by any standard

(Granick 1987). One such factor was the role that the enterprise played as provider of social welfare, which resulted in the paradoxical situation that social welfare provision was more decentralized under central planning than in a capitalist welfare-state. Hence, efficiency considerations were often secondary in determining the size of an enterprise's workforce.

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

Restructuring, then, is change in the above described enterprise behaviors, particularly in levels of enterprise efficiency. We examine how restructuring responds to the removal of the central features of the socialist economy, such as ownership by the state, soft budgets, managers who focused on physical production rather than on monetary incentives, etc.. Thus, the typical study that we review presents estimates of an equation of the form:

$$Y = \alpha + X\beta + \gamma P + \epsilon \tag{1}$$

where the enterprise is the unit of observation, Y is some measure of enterprise restructuring, P is some measure 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 variables measuring enterprise characteristics that are pertinent to the determination of Y, and ϵ is an error term. γ is the parameter of direct interest.

Nearly every study that we examine uses data solely from medium-large and large enterprises. The reason for the dominant focus in the literature on larger enterprises is straightforward. These enterprises were the core of the socialist economy and when they were privatized they were transferred as going

concerns, leading to some degree of continuity in their operations and in their personnel. Data collection and observation of the process of restructuring were facilitated by this continuity. In contrast, smaller enterprises were notoriously weak under socialism and soon were swamped by new entrants. In the process of change, they often vanished, with their assets resurfacing in a completely new activity used by new personnel. Data collection under such circumstances faces enormous difficulties, leading to few studies that examine the progress under reforms of the small enterprises that existed before the transition began. Interpretation of the results of studies on small enterprises would also be difficult, since these enterprises go through a process that is more akin to rebirth than to restructuring.

The studies analyzed in this paper vary greatly in methodology and it is our intention to ensure that our composite results do not simply reflect deficiencies in methodology. To this end, we have collected data summarizing every paper's methodology, which we describe in the ensuing paragraphs. In Section 3, in context, we discuss how we use these data.

The papers use many different forms of the variable Y, but there is one distinction that is easily recorded and worth emphasizing. One category of Y comprises quantitative indicators that are based on accounting information and that measure actual enterprise performance. Other indicators of restructuring are somewhat softer, perhaps derived from survey questions on economic performance that are posed to managers (e.g. forecasts of sales in the surveyed year) or from information collected about reorganization (e.g. whether the enterprise has introduced new products) or perhaps reflecting operational factors farther removed from current performance (e.g. the extent of wage arrears). These two types of indicators will be referred to as quantitative and qualitative.

The prevailing sentiment in the literature is that the quantitative variables are to be trusted more (despite the mis-reporting and accounting difficulties that are rife in transition countries). They certainly do measure directly the prime objective of enterprise restructuring, an improvement in economic performance. On the other hand, there is also the view that quantitative performance might suffer when

an enterprise is undertaking fundamental efforts to reorganize and that these efforts might be observed earliest in the qualitative variables.[3] We focus primarily on the quantitative indicators in this paper, deeming them more reliable. However, where sufficient analyses are available, we examine both types.

We also adopt an alternative method of taking into account the fact that reforms take several years to show their effects. We collect information from each study on the number of years of reform that is reflected in the study's data. When we use a single study in more than one section of our paper the number used for years of reform will vary, simply because different reforms occurred at different times. In sections 3 and 4, the appropriate time period is time since privatization, information on which is taken from the papers in question. In sections 5, 6, and 7, the pertinent time is the number of years since price liberalization, the date of which is given in EBRD (1999).[4] Price liberalization normally marked the beginning of decentralization of the state enterprise sector, giving scope for managerial incentives, managerial turnover, the hardening of budgets, and competition to affect enterprise performance.

Perhaps the thorniest methodological problem encountered in estimating γ is selection bias. This occurs when P (e.g. hardness of budgets or level of private ownership) is systematically related to some enterprise characteristic that also affects Y. If that characteristic is unobserved, and therefore not an element of X, the estimate of γ will be biased. This problem has been thoroughly recognized in the literature, but solutions are not always easy to obtain. Thus, for example, only 53% of the estimates of γ used in Section 3, which examines private versus state ownership, employ methods that might counter such bias and only 30% use methods that we regard as wholly satisfactory.

The prevailing evidence suggests that selection bias is a real possibility. For example, van Wijnbergen and Marcinin (1997) show that selection into Czechoslovakia's voucher program was

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[3.] 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.

[4.] Mongolia and China are not in the EBRD's domain. Information for these countries is taken from the pertinent papers.

non-random and that it is necessary to take this into account in ascertaining the effects on outcomes of inclusion in this program. OLS and instrumental variables estimates appearing in the same papers differ, quite often considerably, suggesting bias. But the sign of bias is not uniform.

Is there any pattern to the observations on the direction of bias in studies of privatization, the area of investigation where there are a number of studies that give the pertinent information? As a rough rule, there is negative selection bias in the estimate of the effect of private owners when examining ownership that arose from mass privatization, while the bias is in the opposite direction where mass privatization was not used. (See for example Claessens and Djankov (1998), Grigorian (2000), Perevalov et al. (2000), and Earle (1998). Anderson et al. (2000) provide an exception.) This is in accord with expectations, given that mass privatization was often viewed as a mechanism to rid the state of unsaleable enterprises, while other mechanisms would offer buyers more avenues to select the best ones.

The fact that some studies have identified non-trivial selection bias suggests that we must be sensitive to its presence when synthesizing results. However, since the sign of the bias varies across different contexts, composite results might be less affected than those within individual studies. In order to investigate systematically whether selection bias does affect our results, we have rated papers on a scale of 1 to 3, reflecting the amount of attention to the problem of selectivity, a 1 indicating no attempt, 2 an indirect attempt (e.g. including an initial level of Y within X), and 3 a direct attempt, most usually employing an instrumental variables approach. This rating applies to a paper's attempts to counteract bias in the estimates of the effect of the policy variable, P, that is of particular interest in each section of this paper. For example, in the section on soft-budgets, the rating reflects the quality of the methods used to counteract econometric problems arising from the fact that recipients of soft-budgets are a non-random group of enterprises. In the section on competition, the problem addressed is a slightly different form of endogeneity, that arising from two-way causation between measured concentration and firm

productivity. Since it is easier to discuss in context how we use these ratings to gauge the sensitivity of the results to selection effects, we postpone that discussion to the next section.

There is also variability between studies in the comprehensiveness of the vector X. The number and appropriateness of the variables used in X is an indicator of how likely are problems of omitted-variable bias. For example, sector, region, and size are likely to covary with both performance and ownership. Thus, we rate on a scale of 1 to 3 the extent to which each paper uses an adequate set of control variables in vector X.

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 might inappropriately reflect work of poorer quality. Some scholars prefer to focus reviews of empirical literature on the high points, ignoring papers that fall short methodologically. 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. In the preceding paragraphs, we have discussed several easily ascertainable measures of methodological quality and we will use them in producing composite results below. However, as everyone who has ever produced a referee's report knows, a rote checking of the fulfilment of objective criteria usually does not capture the full picture of a paper's quality. We therefore add one more measure of methodological quality, rating each paper on a scale of one to ten on overall quality of the empirical evidence. This quality rating reflects the objective factors discussed in the paragraphs above, our own subjective view of the strength of the analysis and the data that is used, and the relative standing of the journal in which the paper is published, if it has been published. Because this measure partially reflects our own judgements, we use it primarily as a final check on the robustness of the conclusions reached, rather than in providing core results.

In sum, for each paper, we have the following indicators of methodology employed:[5]

1. The nature of the dependent variable, whether quantitative or qualitative.

2. The length of time (for the pertinent reform) that is embodied in the estimates.

3. The number and appropriateness of the variables used in X, on a scale of 1 to 3.

4. The attention paid to selection bias (or more generally, endogeneity), on a scale of 1 to 3.

5. This paper's overall rating of study quality, on a scale of 1 to 10.

The use of these indicator variables is most easily described in context. We do this in the following section, which examines perhaps the most prominent policy aimed at enterprise restructuring, the change from state to private ownership.

3. State Versus Private Ownership

State ownership is the staple of a traditional socialist economy and private ownership is the essence of capitalism. In the early debates on transition policy, there was no disagreement about the desirability of creating an economy dominated by private ownership, but rather conflicting views on the best strategy to accomplish this, through fast privatization (Lipton and Sachs 1990, Boycko, Shleifer, and Vishny 1995) or through concentrating on building a nascent private sector (Kornai 1990, Murrell 1992). The relative emphasis on the differing strategies has waxed and waned with events. With Eastern Europe in deep crisis in the early 1990's, fast privatization seemed to gain urgency. However, with the recovery of Poland, a relatively slow privatizer, that perceived urgency declined somewhat (Pinto et al. 1993, Aghion, Blanchard, and Burgess 1994, and Brada 1996). But Poland is only one of many transition countries, an outlier at that. The latter half of the 1990's has offered examples of fast privatizers performing well and fast privatizers performing badly, with similar variation across slow privatizers,

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[5.] The data compiled for this study, except for our assessments of the overall quality of papers, are available from the authors on request.

giving sustenance for a variety of opinions about the results of privatization (Pohl et al. 1997, Havrylyshyn and McGettigan 1999, Stiglitz 1999, Black et al. 2000). Since anecdotal cross-country evidence provides little basis for strong conclusions, one must turn to the microeconomic empirical literature on the relationship between ownership change and enterprise restructuring.

Studies examining whether private enterprises perform better than state owned enterprises use equation (1), where P is some measure of the degree of private ownership of the enterprise. A large variety of variables takes the place of Y, X, or P. For example, Y might be output (measured variously by sales, total revenues, value added, etc.), while X might contain capital, labor, and regional and industry dummies, with the basic equation then representing a production function and the estimate of γ capturing the effect on total factor productivity of a change in ownership. Alternatively, Y might be output growth and through X the study controls for the effect of sector, region, or size. Similarly, P might be a dummy variable indicating non-zero private ownership versus 100% state ownership, or it might be the percentage of shares held by private owners, or the percentage of shares held privately over some threshold level, or one of myriad other choices. One could fill a complete paper simply listing the different Y's, P's, and X's that have been used.

The similarities and differences between two papers (Frydman, Gray, Hessel, and Rapaczynski, 1999a, 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 causing sample sizes to be fairly small. Each study examines the effects of

privatization as a whole and the differing effects of a variety of owners. We discuss the latter in the next section.

Both studies wrestle with decisions on specification of the dependent variable, how to measure ownership, which control variables 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.[6] Their ownership variable is a dummy, equal to one if the firm is privatized. As controls, they use initial levels (not growth rates) of the four performance measures, accompanied by sectoral, country, and time dummies. The possibility of selection bias is examined in a number of different ways, employing methods developed for the analysis of treatment effects in a panel data context. 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 cumulative effect of these analyses is to convince the reader that the privatization effects are real, rather than an artefact of selection for privatization.

Enterprise record-keeping during the chaos of the early transition years in Mongolia was so poor that there was no possibility for Anderson, Lee, and Murrell 2000 to obtain panel data. Hence, they focus on performance in one year, using three different dependent variables, gross output (within a Cobb-Douglas production function framework), sales per employee, and value-added per employee. The equations for the latter two variables include a lagged dependent variable, nesting a specification that uses growth as 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

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[6.] A major aspect of Frydman et al. (1999a) is the effect of privatization on different dependent variables, e.g., changes of cost of goods sold, revenue growth, productivity growth. For reasons of space, we cannot examine this interesting issue here.

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 differences in the incentives of different types of owners during privatization. Comparison of ordinary least squares and instrumental variables results suggests that the estimated effect of privatization is upwardly biased when using OLS.

Frydman et al. and Anderson et al. obtain strikingly different results. In Central Europe, privatization improves revenue growth by approximately 7% a year; in Central Asia, wholly private firms are 30% to 70% 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 more attention to possible sources of statistical bias than most of the papers in this literature. One obvious candidate to explain the differences in results is the countries studied, the most advanced transition countries versus one of the most backward. We will examine this issue later in this section. Another possibility is the difference in privatization programs, particularly in the types of owners generated by the programs, which is the subject of Section 4.

Having given the reader a flavor of the literature, we now turn to the composite results. We are interested in the size and statistical significance of the estimate of γ ($\hat{\gamma}$) in equation (1), which capture the relevant information on the effects of privatization on performance. What is immediately apparent, however, is that the $\hat{\gamma}$'s of different studies are not directly comparable because of the large variations in the way in which Y and P are measured. Therefore, we seek a method of combining the results of different studies. We begin with a simple method that combines t-statistics, answering a limited, but important, range of questions. But, as we will see below, if the aim is to compare the strength of the

effects of privatization in different regions or to contrast the effects of different types of owners, we must use methods that examine more than t-statistics.

In this section, we combine the results of 35 distinct studies.[7] The theory justifying the methodology of aggregating results is analogous to that used when conducting tests on the mean of a sample. Collect several independent observations that come from the same distribution, find their mean, and take advantage of existing theoretical results that relate the distribution of the mean to the distribution of the underlying observations. The variance of the sample mean will be less than that of individual observations, implying that the power of statistical tests based on the mean will be greater than that of tests based on individual observations.

Within the 35 studies, we have identified 89 $\hat{\gamma}$'s together with their corresponding t-statistics. We use more than one estimate 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). Of course, most of the studies contain many $\hat{\gamma}$'s (quite often as many as ten or more), usually because the authors have presented many different formulations of the same basic equations by varying the content of X. Where different $\hat{\gamma}$'s are obtained in such a way, we use only one, relying on indications in the paper concerning the author's preferred estimates or, lacking those, using our own judgment.

Together with each $\hat{\gamma}$ and t-statistic combination, we collected information on sample size, country under study, and the five indicators of methodology discussed in the previous section. Each $\hat{\gamma}$ and its

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[7] The studies are Anderson, Korsun, and Murrell (2000), Anderson, Lee, and Murrell (2000), Brown and Earle (1999), Brown and Brown (1999), Claessens and Djankov (2001), Djankov (1999b, 1999c), Earle (1998), Earle and Estrin (1997), Earle and Estrin (1998), Earle and Sabirainova (1999), Earle, Estrin, and Leshchenko (1996), Earle and Rose (1997), Estrin and Rosevear (1999a, 1999b), Evans-Klock and Samorodov (1998), Frydman, Gray, Hessel, and Rapaczynski (1999a, 199b), Glennerster (2000), Grigorian (2000), Grosfeld and Nivet (1997), Hendley, Murrell, and Ryterman (2001), Jones (1998), Jones and Mygind (1999a), Konings (1997), Lehmann, Wadsworth, and Acquisti (1999), Linz and Krueger (1998), Major (1999), Perevalov, Gimadi, and Dobrodey (2000), Pohl, Anderson, Claessens, and Djankov (1997), Roberts, Gorkov, and Madigan (1999), Smith, Cin, and Vodopivec (1997), Warzynski (2000), Xu and Wang (1999), and Zemplinerova, Lastovicka, and Marcincin (1995).

accompanying information is a unit of observation for this paper. We will refer to each observation as an

"analysis" indicating that it summarizes one regression analysis. Finally, we add a last indicator

variable, the number of analyses in our data set that are derived from the same paper and that are on the

same country, which will allow us to ensure that our results are not distorted by the use of a large number

of analyses from one paper. Of course, our task is to understand the composite implications of the 89

analyses on which we have information.

The data set comprises t-statistics on $\hat{\gamma}$'s from M analyses, denoted $t_1,...,t_M$. Form the following

statistic:

$$\sum_{k=1}^{M} t_k \bigg/ \sqrt{M} \tag{2}$$

This statistic has a normal distribution, allowing the application of standard tests.[8] M, which is the

number of analyses, plays an analogous role to that of size of sample in the standard test of the mean of a

sample of observations, with which all readers will be familiar. It is readily apparent that a set of studies

with small positive t-statistics could be significant in the aggregate despite the non-significance of each

individual study. As it happens, less than one-half of the t-statistics examined in this section show a

statistically significant effect of privatization, but collectively they are highly significant, as we will see

below.

Our synthesis of results relies on tests of (2), whereas the usual method of combining results in

literature reviews is the method of vote-counting (Hunter and Schmidt, 1990). Vote-counting concludes

that there is statistical non-significance in the aggregate when a set of studies has a median t-statistic that

is insignificant. This method produces misleading conclusions, since it combines probability

information erroneously. This point can be illustrated with a simple example. Researcher A obtains a

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[8.] Hunter and Schmidt 1990. This statement assumes that the sample sizes in the individual analyses are sufficiently large, which is the case for all papers that we have examined here.

t-statistic of 2.0 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 1.0 in each study. Researcher B triumphantly announces that A has been mistaken, for the vote is now 2 studies to 1 for non-significance of the effect. 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 1 contains the results obtained by applying equation (2). Two different ways of grouping observations lead to the rows of the table. First, there is the quantitative-qualitative division of dependent variables. 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 of the transition countries. 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. The non-CIS group comprises Eastern Europe and the Baltics (with one study of China). Interestingly, once we seek a criterion that corresponds to our split of countries, we find that the criterion is the length of time that the countries labored under communism, seventy years for each CIS country and less than fifty years in the non-CIS grouping.[9] 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 of the statistics appearing in column (1), with one exception. Thus, the first conclusion from this table is that the aggregate effects of privatization are positive. This also applies when both types of indicators, quantitative and qualitative, are examined separately. The one case where the effects of privatization are not significantly positive is the case of

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

quantitative indicators for the CIS. Thus, a second conclusion from the table is that the effects of privatization in the CIS countries are limited.

How robust are these conclusions? The papers that contribute analyses to our data vary a great deal in characteristics, not least in the amount of attention paid to reducing selection bias and controlling for extraneous factors. Therefore, due caution suggests that we examine whether the above conclusions are spurious, reflecting methodological deficiencies in the papers under review rather than real economic phenomena. The simplest way to undertake such an examination is to weight the various t-statistics when forming a composite statistic, using weights reflecting the methodological differences.[10]

Suppose that there are weights, $w_1,...,w_M$ for each t-statistic. Then the following statistic has a normal distribution:

$$\sum_{k=1}^{M} w_k t_k \left/ \sqrt{\sum_{k=1}^{M} w_k^2} \right. \tag{3}$$

Any such weighting procedure discounts those studies with smaller weights, effectively producing an aggregate statistic that appears to be based on fewer studies.[11]

We use each of the indicators of methodology individually as weights and present the results in Table 1. The main purpose behind this weighting exercise is to see if there is any reason to doubt the broad outlines of the conclusions derived from column (1). The use of the different weights does not change the overall picture for the non-CIS grouping. For the CIS, the conclusion on the quantitative indicators is strengthened if anything: some of the pertinent statistics are now negative and significant.

Although it is tempting to do so, one cannot immediately conclude from Table 1 that the effect of

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[10.] Note that the t-statistics implicitly weight according to sample size and therefore we do not use sample size as a weight here.

[11.] For example, when all studies have the same t-statistics, 10 studies, 5 with weights of 1 and 5 with weights of ½, would produce a composite statistic equal to that produced by 9 equally-weighted studies.

privatization on the quantitative variables in the non-CIS countries is greater than the effect in the CIS.

Table 1 provides information only on the statistical significance of an effect relative to a γ of 0. It is

quite possible that an effect can be numerically stronger in economic terms but weaker in statistical

terms. To compare directly the size of the two economic effects, it is necessary first to develop our

methodology a little further, identifying a statistic that is comparable across a heterogeneous group of

studies and that captures effect size.

In order to describe the methods to be used in the most straightforward terms, we first use the

simplest linear model:

$$Y = \alpha + \gamma P + \epsilon \tag{4}$$

where all variables and parameters are as defined in equation (1). Variances of the pertinent variables

(and their estimates, since there is no ambiguity here) are denoted by σ_Y^2, σ_P^2, and σ_ϵ^2, where

$\sigma_Y^2 = \gamma^2 \sigma_P^2 + \sigma_\epsilon^2$. The t-statistic corresponding to the kth study's $\hat{\gamma}$ for equation (4) is then:

$$\tag{5}$$
$$t_k = \hat{\gamma}_k \, n_k^{1/2} \, \sigma_{Pk} / \sigma_{\epsilon k}$$

n_k is degrees of freedom in the kth study. We assume throughout that sample size is large relative to the

number of parameters estimated, so that sample size approximates degrees of freedom. This assumption

is necessary since many studies do not indicate precisely how many parameters are estimated, leaving

degrees of freedom unknown.[12]

On inspection of equation (5), it is readily apparent that the presence of sample size in the t-statistic

renders it inappropriate for cross-study comparisons that focus on the relative size of privatization

effects. But (5) emphasizes a very important property of t-statistics: they are invariant to changes in the

units of Y or P. While we seek a statistic that does not reflect sample size, the invariance to changes in

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

units is a property that must be maintained when comparing estimates across a heterogeneous collection

of studies.

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:

$$r_k = \hat{\gamma}_k \, \sigma_{Pk} \Big/ \sqrt{\left(\sigma_{Pk}^2 \gamma^2 + \sigma_{\varepsilon k}^2\right)} = \hat{\gamma}_k \, \sigma_{Pk} \Big/ \sigma_{Yk} \qquad (6)$$

It is now simple to make the adaption to the case where, as in (1), other variables (X) are also

present. In this case, we simply use partial correlation coefficients, where the σ_{Yk} and σ_{Pk} that appear in

equation (6) are now the standard deviation 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 that can be

explained by those in X: P and Y controlling for X. There is a similar adaptation in the t-statistic:

equation (5) 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 (5) and (6) imply that there is a

simple relation between published t-statistics and the corresponding correlations:

$$r_k^2 = t_k^2 \Big/ \left(t_k^2 + n_k\right) \qquad (7)$$

where the formula applies equally to partial correlations (Greene 2000).[13]   Therefore, the typical study

presents information that is sufficient to compare the estimated size of the privatization effect across a

heterogeneous collection of studies.

Of course, as (6) makes clear, the partial correlation coefficients are still not a pure privatization

effect. For example, if the amount of variation in the dependent variable is lower in one analysis than

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[13.]   The sign of the r_k is obtained from the sign of the estimated γ.

another, then the former analysis will estimate a larger privatization effect, *ceteris paribus*. Nevertheless, we do examine the effect of factors such as years since privatization, controls used, and study quality, which might lead to differences across analyses in the amount of error and dependent variable variance. When weighting the analyses with variables reflecting these factors, the general conclusions of our tests are hardly moderated, suggesting that it is a pure privatization effect that we are capturing. Of course, we cannot dismiss completely the possibility that our test results reflect a larger variability of outcomes in one group of countries (σ_{Yk} greater), rather than the pure economic effect of privatization (γ greater). The reader should keep this point in mind in the ensuing discussion.

When conducting tests on the values of correlation coefficients, the standard recommendation is to use "Fisher's Z_r", which equals $\ln[(1+r)/(1-r)]/2$ and is, to a close approximation, normally distributed. Its variance is $[1/(n-3)]$, where n is the sample size used to calculate the correlation coefficient (Shadish and Haddock, 1994). Thus, the tests that we use are simply the standard ones on the differences of the means of two sets of normally distributed variables. Analogously to the method used for Table 1, weighting procedures are employed to find weighted-mean partial correlations and to conduct tests based on Fisher's Z_r, so that one can investigate the effects of such factors as study-quality.

Table 2 presents the test results. The focus of the tests is on whether the strength of the privatization effect in the CIS is significantly less than that for the non-CIS countries. The first rows of the table give the pertinent information when all analyses are included. The remainder of the table examines the results for the quantitative and qualitative variables separately. As in Table 1, the columns reflect the use of different weights.

The results are in accordance with those expected from Table 1. When examining the test statistics for qualitative and quantitative variables together or when the quantitative variables are examined alone, every test statistic affirms (at the 1% significance level) that the privatization effect is stronger in the non-CIS than in the CIS. In most cases, the privatization effect in the non-CIS countries is more than

twice the size of that in the CIS countries. The results are similar, but somewhat weaker statistically, for the qualitative variables, perhaps because there are so few studies for the non-CIS countries that fit into this category.

There remains the issue of the economic size of the privatization effect. One immediate reaction of readers might be that the r_k's are rather low (the highest in the first row of Table 2 being 0.090). Such a judgment is in the eye of the beholder, but an example might make the values of partial correlations more intuitive.[14] Suppose that Y is a dummy variable, with a value of 1 when the enterprise grows and 0 when the enterprise declines. P is also a dummy, with value 1 when the enterprise is private and 0 when state-owned. Assume that 50% of firms in each of two regions are growing and 50% of firms in each region are private. Now use the quantitative unweighted r_k's from Table 2 (0.016 for the CIS and 0.078 for the non-CIS) and assume that they were obtained from simple regressions.   Then, given the previous assumptions, the data on the dependent variable for the CIS must be such that 50.5% of private firms were growing and 49.5% of state firms were growing.[15] The corresponding percentages in non-CIS would be 54% and 46%. The implication is that complete privatization in the non-CIS countries would result in 8% more firms growing, while complete privatization in the CIS would result in only 1% more firms growing.

In order to give more flavor of the size of the privatization effect, Table 3 presents $\hat{\gamma}$'s from a variety of studies. A quick perusal will convince the reader that the estimates of the economic effects of privatization in individual studies are quite high (some in a negative direction). Thus, the apparently low levels of aggregate (or average) partial correlation coefficients appearing in the previous tables is indicative of high σ_ϵ^2's: there is much variation in enterprise outcomes that cannot be explained by the

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[14.] The methodology of this example is borrowed from Rosenthal (1984, pp. 129-132).

[15.] To clarify the nature of this exercise. We are making up data for the independent variable, that on ownership, and making up data on the sample means of the dependent variable. Then, once the values of the correlation coefficients are assumed, the proportion of each type of enterprise that must be growing is determined.

standard control variables available to the econometrician. For the CIS, there is another factor, a significant percentage (36%) of the $\hat{\gamma}$'s are negative, so that the composite estimate of the effect of privatization combines both positive and negative outcomes. (In the non-CIS, all but one of the 36 $\hat{\gamma}$'s are positive.) Table 3 suggests that the effects of privatization are economically large.

4. The Effects of Different Types of Owners

One of the reasons that changes of ownership might have had different effects across regions is that differences in the privatization processes resulted in different mixes of owners across countries. The hoped-for quick re-trading of shares to the most effective owners has not happened (Anderson, Korsun, and Murrell 1999 and Blasi and Shleifer 1996). Therefore, the owners created initially by the privatization process will have more than a short-term effect on enterprise performance. This is important, of course, only if the type of ownership makes a difference. As it happens, transition experience offers unusually comprehensive evidence on this score.

Theorizing on the link between types of ownership and corporate restructuring dates back at least to Berle and Means (1933), who contended that diffuse ownership yields significant power to managers whose interests do not coincide with those of shareholders. This contention has generally been supported empirically in market economies (e.g. Roll, 1986; Johnson et al.1985). Morck, Shleifer, and Vishny (1988) find that some managerial ownership can ameliorate the problems identified by Berle and Means. In the transition context, incumbent managers will have intimate knowledge of an enterprise, which might be necessary to take the dramatic measures needed for restructuring. However, these managers were hardly selected for entrepreneurship and risk-taking.

Insider ownership more generally has been a concern of the comparative systems literature. An important empirical question is whether the well known theoretical pathologies of labor-managed firms outweigh the motivational effects from worker ownership. The evidence is mixed. The essays in Blinder

(1990) suggest beneficial effects of small amounts of worker ownership, while the survey of Bonin, Jones, and Putterman (1993) finds mixed evidence when comparing the efficiency of producer cooperatives to capitalist firms, as do Kruse and Blasi (1995).

Shleifer and Vishny (1986) argue that individual block-owners have a strong incentive to monitor management because of their non-diversifiable holding in the corporation. Different types of blockholders might have special characteristics that make them more suited to the task of restructuring. For example, foreigners will have superior knowledge of world markets and better technology. Financial institutions might have more incentive to monitor their own customers. They also can give the credit that is often a pre-requisite for restructuring. But the properties of large outside owners do not always lead automatically to improved performance. Large owners have opportunities to expropriate value, particularly when the minority shareholders are not well protected (La Porta et al. 2000a). Commercial banks face conflicts when they are large creditors of firms in which they hold equity stakes. Thus, which type of outside owner is most advantageous for enterprise restructuring is very much an empirical issue.

Coase (1988) and Demsetz and Lehn (1985) argue that the evidence from mature market economies on the relation between types of owners and firm restructuring may be spurious. If the transaction costs of taking value-maximizing positions in firms are low, each firm would have the "right" ownership structure: there might not be a relationship between ownership type and restructuring. This observation raises the perceived contribution of evidence from transition countries. In transition economies, the structure of ownership was not endogenously determined in markets with low transactions costs, but quite often emerged in political and administrative processes. In many cases, ownership structure is exogenous and in others it is easy to obtain reliable instruments to counter endogeneity bias. The results for transition countries might give much better information on the true characteristics of different owners than has been generated in previous studies.

A further examination of the papers by Frydman, Gray, Hessel, and Rapaczynski, 1999a, and Anderson, Lee, and Murrell 2000 exhibits the methodological decisions to be made when examining the effects of different owners, and the variations in results that can be obtained. When examining state versus private ownership, both papers estimated a version of equation (1) $(Y = \alpha + X\beta + \gamma P + \epsilon)$ with P a scalar measure of private ownership. When estimating the effects of different owners, both papers use exactly the same methodology as when they examined state versus private, but now P is a vector, capturing the ownership held by different entities. Frydman et al. examine outsiders, insiders, and the state in one analysis, and foreigners, domestic financial firms, domestic non-financial firms, domestic individuals, the state (in a privatized firm), the state (in a non-privatized firm), managers, and workers in another analysis. Their ownership variables are dummies capturing whether the given owner type is the largest shareholder. Measuring ownership in this way follows immediately from a somewhat unusual feature of their data, the fact that all privatized firms had highly concentrated ownership.

In Mongolia, examined by Anderson et al., the variety of owners after privatization is narrower. Managerial and worker ownership are highly correlated due to the nature of the privatization scheme and therefore there is no value in differentiating between different types of insiders. Individuals with small stakes are numerically dominant among outsider owners. Hence, the analysis of the effects of different owners in Anderson et al. only contrasts the state (in a privatized firm) versus outsiders versus insiders. Ownership is measured by the percentage of shares held by each type of owner.

Frydman et al. find that foreigners and domestic financial firms produce the largest positive effects, while outsider owners outperform insiders. Results are mixed on insiders versus state ownership in non-privatized enterprises. 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 and 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 blockholders, than in Central Asia, where they are individuals, usually with tiny share ownership. Anderson et al. examine whether diffusion of ownership can explain their results and do find better performance in those few enterprises in which blockholders have obtained a seat on the board of directors.[16]

Finally, the results on state ownership repay particularly close examination. Anderson et al. sampled privatized enterprises, many of which had large state ownership shares. Thus their study includes no firms that are 100% state owned. Frydman et al. sampled both privatized and non-privatized firms, thus having two state ownership variables. The Anderson et al. results on state ownership are comparable to those of Frydman et al. on state ownership in privatized firms. When this is understood, the results on state ownership now appear much more consistent between studies, since Frydman et al. find that state ownership in a privatized firm performs at least as well as the median type of private owner. However, the two papers have very different interpretations of their results. Frydman et al. view the state as passive, with other (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 therefore more willing than insiders to push for efficiency and more able to do so than outsiders were.

In the way that they have provided 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. The details matter crucially, for example which study used the privatization agency for its sampling frame or which study's data reflect concentrated ownership. These facts are often not emphasized in papers. To ascertain them one must often delve into subtext. Yet without knowing them,

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

we could not, for example, bring any coherence to the results of the two papers that we have just examined.

We now move onto the methodology for combining the results of many papers that examine the effects of different types of owners. The typical study will present estimates of an equation which, in its simplest version, is:

$$Y = \alpha + X\beta + \delta O + \theta I + \epsilon \tag{8}$$

O and I are measures of the amount of ownership held by two different types of owners. δ and θ are the parameters of interest and all other variables are as defined before. For estimates of equation (8) 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 the units of δ and θ are comparable. Usually, studies use a number of different ownership shares (e.g., those of workers, managers, investment funds, banks, etc). When we turn to the results, we will examine the more general case of many types of owners. To ease the discussion of methodology, however, we will use two types and refer to O as outsider ownership while I is insider ownership. Whatever is not owned by these two is owned by a third entity, the state. The state share is omitted from the equation given linear dependence of the three ownership variables.

In papers estimating (8), 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). The latter comparison invariably appears directly in papers, in the standard presentation of coefficients and t-statistics. But obtaining all information pertinent to the comparison of δ and θ usually presents some difficulties. Papers generally provide t-statistics for estimates of δ and θ ($\hat{\delta}$ and $\hat{\theta}$), which can then be immediately converted into estimates of the partial correlation coefficients that show the effect of changing ownership from state to outsider or state to insider. But how does one find an estimate of the effect of changing from insider to outsider ownership? Papers usually do not provide all necessary information for an exact estimate.

To apply the methodology developed above, we require a t-statistic for a test of the null hypothesis that $\delta = \theta$. Calculation of this statistic requires $\hat{\delta}$ 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:

$$\text{Variance}(\hat{\delta} - \hat{\theta}) = \text{Variance}(\hat{\delta}) + \text{Variance}(\hat{\theta}) - 2\text{Covariance}(\hat{\delta}, \hat{\theta}) \tag{9}$$

We sought a reasonable, 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. This means that the covariance of the estimates of the parameters attached to O and I will almost certainly be positive. We have verified this point in three ways. First, take a simple theoretical case. Assume that outsiders have 100% ownership in ⅓ of enterprises, insiders 100% of another ⅓, and the state completely owns the last ⅓. In this case, 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.[17] Third, we have investigated the size of the variance of $(\hat{\delta} - \hat{\theta})$ in two data sets to which we have access, those used in Anderson, Lee, 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% and 122% 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 $(\hat{\delta} - \hat{\theta})$. We use the following:

[17.] 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.

$$\mathrm{S.E.}(\hat{\delta}-\hat{\theta})=\max\left\{1.25\sqrt{[\mathrm{Var.}(\hat{\delta})+\mathrm{Var.}(\hat{\theta})]/2}\,,\,\max\left(\mathrm{S.E.}(\hat{\delta}),\mathrm{S.E.}(\hat{\theta})\right)\right\} \qquad (10)$$

If the first term within the braces on the right-hand side of (10) applies, the estimate of the 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 inserted as a conservative adjustment, corresponding to the assumption that the covariance will usually be somewhat less than ½ of the mean of the two variances. (Alternatively, if the covariance were zero, which is unlikely, this factor would need to be 1.41 [= $2^{\frac{1}{2}}$].) The second term within the braces is again a conservative adjustment, for a scenario in which the variances of $\hat{\delta}$ and $\hat{\theta}$ are of quite different sizes, which they are in only 25% of the analyses used in this paper. That element of (10) 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 using only the information contained in papers. We believe that this procedure is a reasonable second-best, without which there would be no possibility of exploiting the vast amount of information in the literature on the effects of different types of owners. In formulating (10), we have used conservative assumptions, designed to ensure that we do not over-estimate differences between owners. However, we can test whether these assumptions are too conservative. We explain this test below, after developing the procedures used to combine the results of many studies on the effects of different owners.

We follow the empirical literature in identifying the ownership categories that we analyze. To settle on a list of ownership types, we first reviewed the literature and identified those ownership types that recurred consistently across studies, appearing in at least five papers. Eleven ownership categories satisfied this criterion, some being subsets of others. We should emphasize that we were not free to construct our own ideal set of ownership categories, since this synthesis uses the results of others. Moreover, 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 only in a small number of cases. In fact, our review of the literature

reveals a surprising amount of consistency between papers and we are confident that the results we

present are not materially affected by variation in ownership definitions across papers. This process

resulted in eleven categories:

1. traditional state ownership: state ownership in enterprises that are 100% state and that have not been

    part of a privatization program.

2. commercialized (or corporatized) state enterprises: state ownership in enterprises that have been

    legally separated from the state, that are treated as private enterprises under the corporate

    governance laws and that have, usually, been part of a privatization program.[18]

3. enterprise insiders: a composite group, where workers and managers were not differentiated.

4. outsiders: a composite group consisting of all non-employee, non-state owners.

5. workers (non-management employees).

6. managers (managerial employees).

7. banks.

8. investment funds (if the investment fund is identified as owned by a bank or by the state then the

    ownership is classified in either 7 or 2).

9. foreign owners.

10. blockholders: outsider, non-state ownership that has been concentrated in the hands of large

    individual owners e.g. individual entrepreneurs, domestic firms, etc. If the blockholder is

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[18.] 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% state ownership. Where a study did not provide enough information for us to know which of these two categories of state ownership applied or where the study mixed the two types, we assigned the results to the traditional state ownership category.

identified as a manager, bank, investment fund or foreigner then the ownership is classified in

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, and 10. This category is dominated by individual outsider ownership

that remains diffused across large number of individual owners. 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.

The reader will immediately notice that some of these categories overlap: for example, workers and

managers together are insiders. However, one should not assume that the aggregate category is the same

as the sum of its parts. It is crucial to note that 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 is

not, however, 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.

From 23 studies, we have compiled a data set of 331 observations on the effects of different types of

owners on quantitative enterprise outcomes.[19] Since each study usually contains several types of owners,

each contributes several different pair-wise comparisons to our data-set.[20] The central variables of

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[19.] The studies are Anderson, Lee, and Murrell (2000), Brown and Earle (1999), Claessens and Djankov (1999a, 1999b), Claessens, Djankov, and Pohl (1997), Cull, Matesova, and Shirley (2000), Djankov (1999b, 1999c), Earle (1998), Earle and Estrin (1997), Estrin and Rosevear (1999a), Frydman, Gray, Hessel, and Rapaczynski (1999a), Grosfeld and Nivet (1997), Jones (1998), Jones, Klinedinst, and Rock (1998), Jones and Mygind (1999a, 1999b), Konings (1997), Lee (1999), Lehmann, Wadsworth, and Acquisti (1999), Roberts, Gorkov, and Madigan (1999), Smith, Cin, and Vodopivec (1997), and Weiss and Nikitin (1998).

[20.] As in the previous section, we include two or more estimates from the same paper of the same pairwise comparison only if the estimates are derived from conceptually different regressions.

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 (10). Application of (7) immediately leads

to estimates of partial correlation coefficients, r_{ijk}, where $i,j = 1,...,11$ (the number of categories of

owners) and $k = 1,...,K_{ij}$, 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 of the 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 the previous section 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.[21] This is accomplished using a simple dummy-variable regression framework:

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

$$D_{ijk}^m = \begin{cases} 1 & \text{if} \quad m = i \\ -1 & \text{if} \quad m = j \\ 0 & \text{otherwise} \end{cases} \tag{11}$$

Since we have information on only the relative performance of different owners, it is not possible to

estimate all 11 λ_m. Thus, we adopt the natural convention that owner type 1 is traditional state ownership

and focus on $\hat{\lambda}_m - \hat{\lambda}_1$ (for $m = 2,...11$), a composite estimate of the partial correlation coefficient

measuring the effect of switching ownership from traditional state ownership to owner m.

[21.] For example, if we have information on owners A and B versus owner C and information on A and B versus D, then we can estimate the effect of C versus D without having access to any study that matches C versus D.

The individual r_{ijk} reflect many enterprise observations and it is important to use this fact in estimation of (11) and interpretation of its results. If the underlying data are normally distributed, then the variance of the correlation coefficient is approximately (Shadish and Haddock, 1994):

$$var\left(r_{ijk}\left|ijk\right.\right) = \left(1 - r_{ijk}^{2}\right)^{2} \Big/ n_{ijk} \qquad (12)$$

Since $var(r_{ijk}|\ ijk) = var(\epsilon_{ijk})$, we can use this information on variances within a generalized least squares procedure, when obtaining the $\hat{\lambda}_{m}$ - $\hat{\lambda}_{1}$ and their standard errors.[22]

The results appear in the Figure 1's first bar, labeled 'basic results'. Before discussing results, however, we further refine the estimates. First, we examine whether the pragmatic approach to estimating variances, which hinges on the application of (10), is appropriate. Second, we ask whether selection bias might be distorting the estimates of the effect of different types of owners.

For 30% of our 331 observations, it is not necessary to apply (10) 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% of observations and those found with the remainder of the observations, which use (10). This question is addressed by estimating the following equation using generalized non-linear least squares:

$$r_{ijk} = \sum_{m=1}^{11} \lambda_{m} D_{ijk}^{m} + \gamma D \sum_{m=1}^{11} \lambda_{m} D_{ijk}^{m} + \varepsilon_{ijk} \qquad (13)$$

D is a dummy variable equal to 1 when the observation on r_{ijk} was derived using (10). The estimate of γ provides the pertinent information on whether we have been too conservative when formulating (10). In fact, we did find this to be the case, $\hat{\gamma}$ implying that application of (10) leads to r_{ijk} that are 32% smaller

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[22.] 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 a GLS routine in any of the standard econometrics computer packages. However, the standard output must be re-interpreted to take into account the assumptions on error variances. See Hedges (1994).

than appropriate.[23] The $\hat{\lambda}_m$ - $\hat{\lambda}_1$ derived from the application of (13) appear in the second bar of Figure 1. The reader will immediately notice that this adjustment leads to slightly larger differences in the effects of different owners than those found when estimating (11). This is to be expected given that $\hat{\gamma}$ implies that (10) leads to r_{ijk} that are smaller than appropriate and that estimation of (13) compensates for this. However, the ordering of the effectiveness of the different owners is unchanged.

In the previous section, examining state versus private ownership, we conducted sensitivity tests weighting the results from different studies according to five methodological characteristics. In this section, with the larger amount of information to be presented, it seems appropriate to concentrate on just one of these characteristics, the one that will be of most concern to readers when thinking about different owners, selection bias. Suspicions about selection 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. To examine whether such suspicions are justified, we examine the degree to which our results change when we discount observations from papers that pay less attention to the problem of selection bias.

As in the previous section, we classified papers into three groups reflecting the attempts to counter the problem of selectivity bias: no attempt, an indirect attempt, or direct application of a statistical procedure. Then in estimating (13), we applied weights to the observations, one-third if the pertinent paper had not addressed the problem of selectivity bias, two-thirds if the paper had addressed the issue in some indirect way, and unity if a direct statistical procedure, such as instrumental variables, had been used. Using these weights requires a trivial adaptation of the generalized non-linear least squares procedure already employed in estimating (13). The results are added to Figure 1, appearing in the third and final bar.

[23.] Formula (10) 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 (13). The tests strongly endorse the hypothesis that the two biases are equal.

There is a large amount of consistency between the overall pictures derived from the three sets of estimates, inspiring confidence in the methods employed in this section. Nevertheless, the results for some owners, especially managers and workers, do change substantially with the selection bias correction. Therefore, the remainder of this section focuses on the estimates derived from this third method of estimation.

Figure 1 concentrates on the numerical size of the ownership effect, but does not address the issue of significance. Table 4 examines this issue, reporting the results of standard tests of the null hypotheses that $\lambda_m - \lambda_h = 0$ for every m-h combination.[24] A first inspection of Table 4 immediately reveals an unusually large number of highly significant t-statistics. This is partially a consequence of employing a methodology that combines results and embodies the precision of estimates from individual studies. Just as in the previous section, we have been able to generate an unusual amount of statistical power through this methodology. 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, for which information is less precise since they are included in few studies, are significantly different than the effects of less than half the other owners.

Figure 1 suggests that differences between owners are of great economic importance. Privatization to workers is detrimental; privatization to diffuse individual owners has no effect, and privatization to 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 blockholders is three times as productive as privatization to insiders. Foreigners were expected to make productive changes and they are unsurprisingly one of the best owners. But it is notable that investment funds are significantly better

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

than foreigners and that two other ownership types, banks and blockholders, are not significantly

different from foreigners. Similarly, diffuse individual ownership was not expected to be very effective,

but it is perhaps surprising that it is statistically indistinguishable from traditional state ownership.

Perhaps the most notable and unexpected result is the place of state ownership in commercialized

enterprises. One must remember of course that this result is not for economies in which real ownership

has been developed organically for decades, but rather for a situation 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, it is not surprising that state ownership can 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. 1999a) or because the very act of commercialization changes the

incentives facing the state when it intervenes into enterprise affairs (Shleifer and Vishny 1994). Once

one takes these points into account, the result for state ownership in commercialized enterprises is less

surprising.

One 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 owners and those of blockholders, but it is also implicit in the effects of foreigners, funds, and

banks since these entities will usually concentrate their shareholdings. Claessens and Djankov (1999b)

focus on the effects of concentration in their study of Czech privatization. They show that a 10%

increase in the percentage of shares held by the largest five shareholders will increase labor productivity

by 5%. They also find diminishing returns to concentration, the marginal effect decreasing as

concentration increases. These results are echoed in Brown and Earle's (1999) study of Russia.

Comparing Owners Across Regions

We have found that privatization has stronger effects in non-CIS countries than in the CIS and that different types of owners have different effects. This immediately raises the question of whether the latter could explain the former. One could directly address this question by using data on ownership in different countries, but there is no systematic collection of such data. Nevertheless, the papers used for this study do contain some evidence on ownership. The strong impression gained from this evidence is that worker and diffuse individual ownership is more prevalent in the CIS than in non-CIS, while foreign, investment fund, concentrated individual, and bank ownership is less prevalent. Thus, since the CIS has an ownership portfolio that contains a greater share of less effective owners, structure of ownership is a strong candidate to explain differences in the effects of privatization between regions.

The effects of different types of owners could also vary between regions because different types of owners require different levels of institutional support and institutional quality varies across countries.[25] Of the 331 data points used in the previous section, 48% are in the CIS, giving sufficient data in each region for inter-regional comparisons. The natural first line of inquiry is to see whether the effects of all owners are completely different between the regions, using the standard Chow test. The data reject the null hypothesis that the $(\lambda_m - \lambda_l)$ in the CIS are the same as those in the non-CIS at the 1% significance level.

Figure 2 presents estimates of the effects of the different types of owners in the two regions, using the methods employed in obtaining the third bar of Figure 1. In all cases except one (workers), the

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[25.] In this paper, we follow North (1990) in defining institutions as the rules that constrain economic agents together with the incentives to follow these rules. In the present context, we particularly refer to the set of institutions pertinent to the governance of large enterprises. See Pistor (2001) for a discussion of which of these institutions are most pertinent in a transition economy context.

effects (relative to traditional state ownership) for the CIS are greater than for the non-CIS.[26] How can this be consistent with the results of Section 3, showing that privatization had stronger effects in the non-CIS than in the CIS? There are two plausible reasons. First, workers own a large share of privatized enterprises in the CIS and their effects are negative. Second, commercialized state ownership is separated from traditional state ownership in this section but not in the previous one. Since many studies obtain their data from the privatization process itself, the results for Section 3 are quite strongly affected by observations on commercialized state enterprises, which are clearly very different from traditional state enterprises.

One can extract the germ of an institutional story from Figure 2. For some owners, it is important that the mechanisms of corporate governance function well and function continuously, while other owners are not so dependent on these mechanisms. When the institutions of corporate governance are weak, the effectiveness of manager-owners and powerful blockholders would not be so greatly diminished because of their direct access to power, blockholders quickly installing their own managers (Barberis et al. 1996). The owners dependent on institutional help are diffuse individual owners, outsiders where there are a number of different blockholders, and perhaps even workers. History is important here. In some Eastern European countries, most notably Poland and Hungary, workers were much closer to the exercise of managerial power and therefore might have less need of formal institutions. In other countries, Russia for example, managers excluded workers from all decision-making and in these countries workers had some of the same problems of exercising their ownership rights as did outsiders. Given these observations, the pattern of ownership effects in Figure 2 is broadly consistent with the argument, most forcefully proposed by Fox and Heller (1999) and Coffee (1999), that corporate governance institutions functioned less well in the CIS than elsewhere.

[26]   It is worth repeating that these results do not show that the various owners are more productive in the CIS than in the non-CIS 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.

This interpretation is considerably reinforced when one examines the degree of variation of ownership effects in the two regions. The argument in the previous paragraph implies that there will be greater variation in the size of ownership effects where corporate governance institutions are weak. A quick glance at Figure 2 immediately suggests that there is much more variation in ownership effects in the CIS than elsewhere. This is verified by examining the within-region variance of the partial correlation coefficients reported in Figure 2. The variance is five times higher in the CIS and the coefficient of variation is 50% higher. Evidence is also found in the R-squares for the regressions that provide the data for Figure 2: the R-squared for the CIS is 0.73 while that for the non-CIS is 0.56. Hence, differences between owners are much more stark and more consistent in the CIS than elsewhere. This suggests that institutions have been more effective in non-CIS countries in providing the help that is essential to some types of owners (e.g. outsiders), but that is not needed by others (e.g. managers).

Thus, we conclude that the effectiveness of privatization in the CIS, relative to non-CIS, has been diminished by two factors. First, ownership in the CIS is higher amongst those types of owners who are less effective everywhere. Second, the types of owners that need institutional help have received less assistance from institutions in the CIS than elsewhere.

These two effects resonate when one considers the case of worker ownership. Worker ownership is much higher in the CIS and much less effective there. This worker ownership came about in a region where workers were a weak force within the enterprise and had virtually no historical experience of ownership or management. Managers in the CIS were all powerful within the enterprise and after privatization they retained the reigns of power.[27] When power within the enterprise does not flow from ownership, the incentives to disregard efficiency are great. Thus, a critical factor in explaining the smaller effect of privatization in the CIS is the large share of worker ownership, arising in an

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[27.] See for example the data on board membership in Blasi and Shleifer 1996 and Anderson, Korsun, and Murrell 1999.

environment where neither history nor institutions provided fertile ground for worker-owners to exercise their power.

But the amount of worker ownership itself was a direct result of institutional weaknesses. The political bargain that led to employee-dominated privatization in Russia (Shleifer and Treisman, 2000) was neither necessary nor feasible in countries such as Poland and Czechoslovakia, where democratic institutions were already functioning much more effectively. In Mongolia, where there were no concessions to employees, worker-ownership was still very large, simply because of the lack of trustworthy alternatives in this institution-poor environment (Korsun and Murrell, 1995). Therefore, one should resist the temptation to conclude from Tables 1 and 2 that things might have been different in the CIS and that privatization could have had the level of effectiveness that it has exhibited elsewhere. Such a conclusion would rest on the assumption that the CIS could have matched the quality of East European political and economic institutions at the very beginning of the transition process.

5. The Role of Managers in Enterprise Restructuring

The previous two sections have documented the benefits of privatization in enhancing enterprise restructuring. They have shed less light, however, on the precise mechanisms by which privatization yields greater efficiency. One explanation is that private owners are better at selecting managers who can run the firm efficiently. Managers of state-owned enterprises are often selected because of their political skills, rather than their business abilities. In contrast, managers of privatized enterprises are more likely to be chosen for their ability to operate in a market environment. The hypothesis that management turnover – or more broadly, bringing in new human capital – is important in improving enterprise performance was first put forward and tested by Barberis et al. (1996) for a sample of privatized Russian shops. The analysis showed that the presence of new managers raised the likelihood of restructuring, whereas equity holdings of old managers were less important for restructuring.

An alternative hypothesis states that what matters for the performance of managers is the correct incentive structure. This includes both "sticks" and "carrots": if managers do not perform well they are dismissed, if they run the firm well they receive better remuneration. A corollary to this hypothesis is that management turnover is not necessary to enhance restructuring efforts, except as a signaling device to managers who may want to shirk. Theoretical models on the early transition, e.g., Aghion et al. (1994) and Aghion and Blanchard (1996), suggest that the incentives of top managers to behave in a profit-maximizing manner improve once they hold a stake in the performance of their firms. Furthermore, career concerns may lead managers of state-owned enterprises to restructure if privatization in the economy is imminent or under way, since privatization induces competition among managers (Roland, 1994; Roland and Sekkat, 2000). This hypothesis has been illustrated in the case of Poland where managers of state-owned enterprises initiated restructuring efforts in the early transition period once a private sector emerged (Pinto et al., 1993). Sections 3 and 4 present equivocal evidence on this hypothesis, since traditional state-owned firms have performed poorly, but commercialized firms have performed somewhat better.

Testing the effect of managerial turnover on enterprise restructuring is not easy. Studies of management changes in market economies often suffer from selection bias as new managers may be better suited than existing managers to manage the firm. The improvement in corporate performance associated with management changes may occur not just because old managers are entrenched in their way of doing business, but rather because their skills-mix has become outdated. The literature on the effects of changes in managers in emerging economies suffers from a different problem: often 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 Barberis et al. study covers retail shops which, because of relatively low capitalization, can be majority management

owned. Hence, this study's findings may not generalize to larger firms, where management ownership is likely to remain small.

Testing the managerial incentives hypothesis is equally difficult. While data on equity ownership and salaries of managers do exist and have been used in a number of studies, data on bonuses, managerial perks, and stock options are not readily available. Yet these types of incentives may account for a large part of the manager's decision to stay with or join a particular firm and work hard on improving performance. Also, managerial incentives may be used in some countries as a substitute for other policies, e.g., prior to 1993 performance contracts in China were used instead of privatization and full price liberalization (Qian et al., 1999). In such cases, the effect of incentives might be distorted due to the incompleteness of other reform efforts.

The data set in Claessens and Djankov (1999a) 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. Second, there were few managers with skills suited to a market economy in the Czech Republic at the start of transition, therefore reducing the likelihood that a new-manager effect is simply proxying for skill-updating. The Czech liberalization process started only in 1990 and prior to that few Czechs could obtain education and skills in the west and then return as superior managers for the new environment.[28] The Czech experience thus allows one to address the question of the effect of new top management on enterprise restructuring more definitively.

Using a sample of 706 large privatized firms, Claessens and Djankov find that profitability and labor productivity are both positively related to appointments of new managers, especially those appointed by private owners. The appointment of new managers shortly before privatization also yields

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[28.] The 1968 exodus of Czech professionals and their return in the early 1990s provided some managers who had significant experience in Western management, but these were isolated cases.

better enterprise restructuring, even though the selection was made by State Ownership Fund officials, i.e., management turnover enhances performance even when undertaken in the state sector. (Note the consistency here with the results on state ownership in commercialized enterprises in the previous section.) Equity ownership of general managers has a small positive effect on corporate performance. Hence, enterprise restructuring in transition economies requires new human capital, which can best occur through management changes.

In a similar study, Frydman, Hessel, and Rapaczynski (1998) use a sample of 413 state-owned and newly privatized enterprises in the Czech Republic, Hungary, and Poland to study management turnover and its effect on subsequent enterprise restructuring. They find that the rate of replacement of old managers in outsider-controlled privatized firms was not statistically different from that in state-owned enterprises. The turnover rate was extremely high: during 1990-1994, nearly two-thirds (64%) of managers were dismissed or moved voluntarily. (Claessens and Djankov also find high turnover rates: 35.6% in privately owned enterprises and 42.1% in those under the State Ownership Fund.) Frydman, Hessel, and Rapaczynski (1998) find that management turnover leads to positive gains in both state-owned and privatized firms, but the effect is only significant in the latter case. They also find that insider-dominated firms were the worst performers, and attribute this finding in part to the lack of management turnover. But it is not clear a priori why any owner would not resort to hiring suitable management if the owner lacks the appropriate expertise.

In a study of 300 large Ukrainian firms, Warzynski (2000) finds that management turnover does not improve productivity and profitability on its own, but displays positive effects when coupled with privatization. The study provides ample evidence that most of the management changes, 62 percent, were associated with skills-updating as managers retired, left voluntarily for better-suited jobs, or acknowledged that their replacement was due to the need for a manager with better skills. While turnover was higher in state-owned enterprises, where 60.7 percent of top management positions changed

hands in 1993-1997, than in privatized and new private enterprises, with 47.5 percent management

turnover, state enterprises did not become more efficient after the change was made. This suggests that

privatization was a factor in determining whether the new manager was one who was able to improve the

performance of the enterprise.

Groves et al. (1995), using a sample of 769 Chinese state-owned companies, provide support for the

alternative hypothesis - the role of incentives. They show that the ability of provincial state officials to

tailor managerial contracts and auction them competitively resulted in large improvements in labor

productivity and profitability. The contracts generally allowed for bonus payments linked to the amount

of profit taxes the provincial government could collect. Groves et al. show that prior to such contracts

managers were more interested in revenue growth, while the contracts provided them with incentives to

maximize profits. Profits increased in those enterprises with auctioned management contracts more than

in other enterprises. Li (1997) also finds bonuses to be effective in China. In contrast, Broadman and

Xiao (1997) and Shirley and Xu (2001) document a negative relation between manager performance

contracts and enterprise restructuring in Chinese state enterprises. Their results are consistent with other

cross-country studies on developing countries that find little evidence for the beneficial effects of

incentive contracts for managers (e.g., Shirley and Xu, 1998).

The findings reviewed so far show that management turnover is effective (Barberis et al., Claessens

and Djankov, Frydman, Hessel, and Rapaczynski) and manager incentives sometimes works (Pinto et al,

Groves et al.; Li) and sometimes does not (Barberis et al., Broadman and Xiao, Shirley and Xu, 2001).

To understand the composite implications of these studies, we apply the methods of Section 3. Panel A

of Table 5 combines the regression results from 8 studies with 29 separate analyses, testing the

importance of both managerial turnover and managerial incentives in restructuring.[29] The construction of

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[29.] Barberis et al. (1996), Broadman and Xiao (1997), Claessens and Djankov (1999a), Frydman, Hessel, and Rapaczynski (1998), Groves et al. (1995), Li (1997), Shirley and Xu (2001), and Warzynski (2000). The Pinto et al. (1993) study does not contain regression results that can be used in the table.

Panel A employs the methods used for Table 1. We use the same weighting factors designed to examine the robustness of the composite conclusions, with two small modifications. Years since privatization is now replaced by a measure of the number of years of liberalization. The selection bias with which we are now concerned is that due to the non-randomness of the set of enterprises that institute new incentive schemes or turnover managers.

We find that in all cases turnover and incentives, considered together, are an important determinant of restructuring. The pertinent t-statistics have values between 3.21 and 6.77. Management turnover on its own also has a significant effect on restructuring, no matter what procedure is used to weight studies. Manager incentives have a negative sign, with the effect being significant in two cases. This perverse effect might be a proxy for asset-stripping, i.e., if managers run the company without much oversight, they are more likely to divert resources for their own benefit (Cull et al., 2000).

Panel B directly compares the two hypotheses, using the methods developed for Table 2. We find a statistically significant difference in every specification, with partial correlation coefficients indicating that management turnover can be seven to nine times as effective as manager incentives for enterprise restructuring.

 What explains the large importance of management changes? Barberis et al. interpret their findings as establishing the importance of human capital that is new to the enterprise. This interpretation is further bolstered by the findings in Claessens and Djankov and Frydman, Hessel, and Rapaczynski that management turnover also contributes to enterprise restructuring in state-owned enterprises, i.e., it is not solely dependent on the strong monetary incentives that come with private ownership. These findings are consistent, however, with another interpretation. Having witnessed the fate of their predecessors, new managers may be afraid of being demoted or dismissed and this fear may drive them to perform better. Under this interpretation, it is not that management change brings better-qualified people, but that managerial slack is reduced.

Yet another hypothesis, and one consistent with the Barberis et al. story, is that the change of managers severs links with politicians and other firms (suppliers and customers alike), whose continued existence limits the growth opportunities of the enterprise. This hypothesis has not been empirically tested to-date.

What is the economic significance of management turnover and management incentives? Barberis et al. find that management turnover more than doubles the likelihood of renovation and the extent of shedding of excess labor in Russian retail shops. It increases the amount of extra hours worked by 80%, and induces 50% more change in suppliers. Claessens and Djankov find that management turnover in state-owned and privatized enterprises results in 1.9% and 6.2% higher labor productivity. Frydman, Hessel, and Rapaczynski (1998) find an even larger effect on labor productivity, 7.3%, in their sample of Central European firms.

Manager incentives can also have large economic effects. Groves et al. show a 7.3% increase in profitability in enterprises with incentive schemes in place. Li finds that a 10 percentage point increase in bonus payments results in 0.74 percentage points increase in total factor productivity (TFP) growth, i.e., if the bonus doubles, there is a 7.4% increase in TFP. In contrast, Shirley and Xu (2001) show that performance contracts of managers in Chinese enterprises reduced TFP growth by 48 percent during 1986 to 1989.

6. Enterprise Restructuring and Hardened Budgets

Three alternative theories exploring the causes of soft budget constraints have been suggested in the transition literature. Janos Kornai (1979, 1998) relates the softness of budget constraints to the paternalistic attitude of the government in socialist economies which results in the accommodation of enterprise requests for extra finance. Firms are financed even when the expected return is below the real interest rate. The government's goal is to maximize employment opportunities and provide auxiliary

services (kindergartens, schools, hospitals, recreation facilities) at the enterprise level, i.e., soft budgets are a substitute for a functioning social safety net.

A reason for the existence of soft budgets has been advanced in Shleifer and Vishny (1994). They model the bargaining between politicians and managers, which leads to equilibria with subsidies to firms and, possibly, bribes to politicians. Politicians pursue non-economic objectives in order to enlarge their political constituency, e.g., by keeping enterprise employment high. An important result of the Shleifer-Vishny analysis is that the hardening of budget constraints, defined as tighter credit policy, induces restructuring and raises efficiency only when bribes are not possible. Thus, hardening budget constraints is not sufficient to raise efficiency.

A third analysis views soft budgets as the continued extension of credit even when the substandard performance of an already-financed investment project has been revealed (Dewatripont and 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 maximizes the expected value of the funds that can be eventually recovered. Projects that are ex ante unprofitable are completed because they are ex post profitable once some costs are sunk. Asymmetric information is not necessary to explain the refinancing of firms that have problems in servicing old debts, because the marginal return on refinancing loans can be large enough to compensate partially for the losses on the old debt. Whatever the source of soft budget constraints, the interesting aspect of this phenomenon is that the possibility of refinancing may exert adverse effects on the behavior of the prospective debtor, leading to a sub-optimal equilibrium.

These three theories of the causes of soft budgets differ significantly. The first explains accommodating lending behavior determined by a benign government's paternalism, while the second suggests that soft budgets arise from politicians' self-interest. In both, soft budgets compensate the enterprise for keeping surplus employment. The predicted effect on enterprise restructuring from soft

budgets is the same in both cases: lack of productivity improvements and continuation of unprofitable

production (and non-production) activities. The third explains an undesirable outcome of optimal

decisions by a financial institution in a situation of imperfect (and asymmetric) information. The

prediction on enterprise restructuring is improved performance over time as the investment enters the

production process.

The predictions on the channels of soft budgets also differ among the three theories. The first theory

suggests that the central government will be the main source of soft financing. The second supports the

notion that local politicians provide soft budgets through direct subsidies, tax exemptions or arrears.

Finally, the third hypothesis identifies banks (or financial intermediaries more generally) and suppliers of

trade credit as the main channel of soft financing.

Most of the literature that documents the use of different channels of soft budgets during early

transition supports the first hypothesis. Schaffer (1998) finds that bank lending is the primary source of

soft budgets in transition countries, where the banking sector is in central state hands. Tax arrears to the

central government are the main source of soft financing in Hungary and Poland. Anderson, Korsun, and

Murrell (2000) use a survey of 250 Mongolian enterprises, asking whether state aid was expected when

financial difficulties arose. One quarter of enterprises, a large proportion of which had central

government ownership, expected soft budgets. Other explanatory variables do not matter significantly.

For example, less profitable or less productive enterprises do not seem to perceive soft budgets any more

strongly than do other enterprises. Local government ownership has a much weaker effect than does

central ownership.

In contrast, McKinsey Global Institute (1999) shows that tax exemptions by the local government

are the main channel of soft financing in Russia. Similarly, Alfandari et al. (1996) show that the share of

local government in financing unviable firms in Russia increased from 0 to 13% in 1992-1994. Claessens

and Djankov (1998) use a sample of over 6,000 enterprises in seven Central and East European countries

to show that the availability of bank credit to non-viable enterprises is associated with the importance of politicians in regulating the particular industry and the corruptibility of politicians. They conclude that the evidence provides significant support for the Shleifer-Vishny model.

Transition experience provides little evidence that points specifically to the third hypothesis. Schaffer's (1998) evidence on bank lending and soft budgets suggests that the critical factor is whether the banking sector is predominantly state-owned. He finds that trade arrears are not a major channel of soft financing, since on average they are equal only to three months worth of a firm's payables. This compares favorably to the level of trade arrears in mature market economies. Product specific subsidies are not a major source of soft budgets, first because they shrank significantly during the transition period, and second because they are often the result of price controls. McMillan and Woodruff (1999a) show that trade creditors in Vietnam stop financing enterprises once their payments are two months in arrears.

Most of the empirical studies of soft-budgets to-date focus on causes and the channels of transfer. There is less focus on the question of whether hardening budget constraints would entail improvements in enterprise performance and what types of restructuring would be most likely. In this section, we discuss the results of 10 papers that use regression analysis to answer these questions. The data come from Bulgaria (Claessens and Peters, 1997; Djankov and Hoekman, 2000), Kazakhstan (Nenova and Djankov, 2000), Lithuania (Grigorian, 2000), Romania (Abdelati and Claessens, 1996; Coricelli and Djankov, 2000; Djankov, 1999a), Mongolia (Anderson, Lee, and Murrell 2000), Russia (Earle and Estrin, 1998), and a cross-country study of the seven Central and East European countries (Claessens and Djankov, 1998 and 2001). They generally cover the period between 1992 and 1999, and contain 28 separate analyses.

Using the methodology developed in Section 3, Table 6, Panel A, shows that the effect of hardened budgets on enterprise restructuring (defined as sales growth, TFP growth, or labor productivity growth) is very significant in non-CIS countries and generally significant in CIS countries. For non-CIS

countries, the t-statistics vary between 10.70 and 15.24, while for CIS countries the statistics are in the interval from 1.52 to 4.76. The positive signs imply that hardened budgets have a beneficial effect on restructuring. Panel B compares the size of the hardened budget effect across the two regions. The studies on non-CIS and CIS countries show effects of similar magnitude, which are not significantly different from each other.

What is the economic significance of soft budgets on enterprise restructuring? Li and Liang (1998) use the employment of non-production workers, investment with below-average rates of return, and distribution of bonuses in excess of those regulated by the government as proxies for the existence of soft budgets in Chinese state owned enterprises. They find that enterprises do not respond to financial losses by reducing one or more of these three factors. If all non-production workers were eliminated, the average enterprise would avoid 38% of its financial losses. If all inefficient investment (below the median industry rate of return) projects were eliminated, 126% of losses would be avoided, i.e., their profit margin (percentage of sales) would change from -8.7% to 2.3%. Finally, the amount of excessive bonuses accounted for 39% of the average enterprise's losses.

Claessens and Peters (1997) find that the presence of soft budgets in Romanian enterprises results in a reduction of labor shedding by 4% annually during 1992-1994. Coricelli and Djankov show that labor shedding was reduced by 4.6% during 1993-1995. Claessens and Djankov (1998) find a 2.7% unrealized TFP-growth as a result of continued soft financing in the Eastern European countries. Djankov and Hoekman (2000) document an unrealized annual gain of 3 percentage points in Bulgaria over the 1992-1995 period. Earle and Estrin (1998) find a 5.7% unrealized labor productivity growth. Djankov (1999a) finds an unrealized labor shedding of 8.9 percentage points on average. Alfandari et al. (1996) show that recipients of soft financing record labor productivity growth that is a 6% 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% unrealized labor productivity growth per annum.

One alternative to soft budgets is to offer generous severance pay and buy workers support. This was tried on a limited scale in Romania, where employees of non-viable enterprises were offered up to 12 months of wages as severance (Djankov, 1999a). However, the restructuring agency continued the flow of soft financing to the isolated enterprises, prolonging their loss-making production. Tornell (1999) also suggests that the offer of generous severance packages can be an effective substitute for soft budgets in countries with strong labor unions and a weak safety net. He draws on experience in the United Kingdom during the privatization of British Steel and British Coal in the mid-1980s and in the Mexican coal sector during the early 1990s.

Short of a special program, could anything else have been done to reduce the risk of soft financing? The reduction in state ownership of enterprises seems to be a main determinant. Anderson, Korsun, and Murrell (2000) show that a 10% larger share of central government ownership increases by 9% the probability of receiving soft financing. Their estimates suggest that privatization reduced the percentage of enterprises with soft budgets from 78% to 23%. Similarly, Alfandari et al. (1996) show that the probability of receiving state support is more than doubled if the enterprise is state-owned.

Privatizing the banks also brings about a reduction in soft financing. Claessens and Djankov show that bank credit was restricted to profitable projects once the Hungarian banking system was largely privatized in 1995. A recent study of Kazakhstan shows that privatized banks are 38% less likely to serve as a channel that provides soft budgets to the ailing enterprise sector (Nenova and Djankov, 2000). A study of 92 economies, including 11 transition economies, shows that government ownership of banks is associated with lower growth of productivity in the corporate sector, stemming from inefficient allocation of resources across enterprises (La Porta, Lopez-de-Silanes, and Shleifer, 2000b).

These findings identify another indirect channel through which privatization is beneficial for enterprise restructuring. In particular, they suggest that privatization (of both enterprises and banks) helps reduce the ability or the incentives of governments to continue to finance unviable enterprises. The empirical literature also shows that larger enterprises (in terms of employment) 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. There are other reasons why an enterprise gets soft financing. Those have to do with location in economically depressed regions (one-company towns being the extreme example) or with support for the economic and social infrastructure (e.g., power companies). Those may require continued subsidization, as happens frequently in mature market economies. But such financing has more to do with the social welfare policies of a responsible government than with soft budgets.

7. Product Market Competition

There is a substantial theoretical literature that studies the relationship between competition and corporate efficiency. The general hypothesis is that increased competition stimulates improvements in productivity. Two lines of argument have been developed in support of this hypothesis. The first is derived from the literature on X-inefficiency (internal to the firm); the second centers on industry rationalization. The X-inefficiency literature assumes that managerial effort is under-supplied in the absence of vigorous competition. Horn, Lang and 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 (Holstrom, 1982; Nalebuff and Stiglitz, 1983) argue that incentive schemes for managers will generate better results the greater the number of players (firms) involved. This arises because of greater opportunities for performance

comparison. Hart (1983) builds an explicit model to show the link between increased competition and improved manager performance.

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 (Schmidt, 1997). Resource reallocation occurs across firms within and between sectors. Although the shake-out may result in a transitional decline in measured efficiency as firms with increasing returns to scale lose domestic market share, over time this may be offset by greater output as the size of the market expands due to exit and access to export markets. Much depends on the existence of scale economies and the ease of entry and exit. Since competition raises the probability of bankruptcy and hence job losses, it also generates stronger incentives for workers to improve productivity and higher labor turnover across firms within sectors (Dickens and Katz, 1987).

Both strands of the theoretical literature lead to the same prediction: greater competition leads, possibly with a lag, to productivity improvements in imperfectly competitive industries. Few empirical investigations using firm-level data have, however, established a strong link between greater competition and subsequent improvements in enterprise performance. Two studies of British manufacturing firms (Nickell, Wadhwani and Wall, 1992; Nickell, 1996) use a panel framework to show that market concentration has had an adverse effect on the level of total factor productivity. In contrast, Blanchflower and Machin (1996) find no effect of changes in domestic market structure on the productivity of UK plants. Most studies exploring the effect of increased import competition on enterprise behavior focus on the impact on relative changes in TFP across sectors. Harrison (1994) finds that the reduction in tariffs and the subsequent increase in import penetration in Cote d'Ivoire following the 1982 trade liberalization had a positive, although not statistically significant, effect on TFP-growth. Van Wijnbergen and Venables (1993) find a strong positive effect of increased import penetration on labor productivity in a large sample of Mexican manufacturing firms.

The initial period of transition provides a unique opportunity to test the importance of product market competition on the subsequent performance of enterprises. This is because the majority of transition economies liberalized their trade regimes relatively fast. Some went on to de-monopolize their industrial sectors through break-ups of conglomerates, spin-offs of individual production units, and by allowing entry of new private firms (Lizal et al., 1995). The short period in which these changes took place allows the researcher to identify the timing of the policy change and control for other economic or firm-specific variables.

We find 17 studies that explicitly investigate the effect of product market competition on enterprise restructuring. Among those, 10 studies focus on non-CIS countries (Claessens and Djankov (2001); Djankov and Hoekman (2000); Djankov and Hoekman (1998); Grigorian (2000); Hersch, Kemme, and Bhandari (1994); Halpern and Korosi (1998); Konings (1997); Konings (1998); Li (1997); Shirley and Xu (2001)) and 7 use data for either Russia (Brown and Brown, 1999; Earle and Estrin, 1998; Brown and Earle, 2000; Perevalov et al., 2000), Georgia (Kreacic, 1998), Ukraine (Warzynski, 2000), or Mongolia (Anderson, Lee, and Murrell, 2000). We are able to distinguish 67 separate analyses, where the authors use either different measures of enterprise restructuring (total factor productivity growth, labor productivity growth, sales growth, and qualitative variables like renovation of facilities) or different indicators of competitive pressures. Twenty-three analyses use import competition as the main explanatory variable, while forty-two studies focus on the effects of domestic market structure (Table 7). The two analyses derived from Li (1997) depend both on foreign and local competition and cannot be classified into one of the two categories.

The analyses discussed in this section are quite homogeneous. In most cases, the dependent variable is quantitative: 29 analyses use TFP growth as the indicator of enterprise restructuring, 18 use labor productivity growth, while the remaining 20 use changes in price-cost margins. Eight analyses, all derived from Kreacic (1998), use qualitative indicators of restructuring (facilities renovation, change of

suppliers, longer work week, and computerization of the accounting function). Import competition is proxied by the import penetration ratio, the number of foreign firms that enterprise managers consider as their rivals in the domestic market, or the industry-level tariff rate. Domestic market competition is measured by either the Herfindahl index, the percentage of sales revenues of the top 3 (sometimes 2 or 4) firms in the respective industry, or the number of local competitors that enterprise managers perceive as rivals.

Overall, the analyses indicate that product market competition has been a major force behind improvements in enterprise productivity in transition economies. Table 7, again based on the methods employed in producing Table 1, has t-statistics varying between 8.19 and 11.40 for the effect of all types of competition in all countries. When we divide the sample into analyses based on import competition versus domestic market structure (for all countries together), we find that both are significant in explaining enterprise performance.

Examining the effects of competition in each of the regions, Table 7 shows that the effects are strong for non-CIS countries, where the t-statistics are highly significant in all cases. For CIS countries, increased competitive pressures are associated with enhanced restructuring, but the effect is not always statistically significant.

The case of competition in CIS countries is one of the few in this paper where it matters whether one treats the different analyses equally, or whether one weights the analyses according to methodology used. The years of reform covered by the data and the attention to selection bias matter (and these are reflected in the overall assessment of quality). Anderson, Lee, and Murrell (2000) suggest reasons why this might be so. First, competition has two opposing effects on measured enterprise productivity, spurring real productivity and reducing prices. (It is virtually impossible in a cross-sectional setting to purge the productivity measures of enterprise-specific price variations.) The price effect will occur much quicker than the productivity effect and probably will dominate during the very early years of transition.

Similarly, a high level of enterprise productivity might lead to high concentration in an enterprise's sector, leading to downwardly biased estimates of the effect of competition on productivity when a study uses enterprise-level data and does not address the issue of the endogeneity of competition. These methodological problems are more likely to appear in studies of the CIS, where enterprise-independent data on competition is scarce and where fewer years have elapsed since reforms began.

A further sub-division of the sample shows an interesting pattern: import competition in the CIS countries does not have a significant effect on enterprise restructuring. In contrast, import competition is always very significant in explaining enterprise restructuring in the non-CIS countries. Since Brown and Brown (1999) and Earle and Estrin (1998) devise 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? EBRD (1998) shows that, on average, non-CIS countries are twice as open to competition from abroad as CIS countries. This makes the effect of import competition much more palpable. Joskow, Schmalensee, and Tsukanova (1994) attribute the relative closedness of Russian markets to the underdeveloped transport infrastructure. An alternative explanation is that regional governments shield producers from foreign competition (Shleifer and Treisman, 2000). Putting barriers on import competition is a cheap way for regional governors to subsidize inefficient local producers. Finally, a number of CIS countries, particularly in Central Asia and the Caucasus, have an industrial sector geared towards extracting and processing industries, while imports comprise the majority of consumer goods. In such countries, while the average import penetration may be high, there is little direct competition within many industries.

Changes in domestic market structure are important in explaining enterprise restructuring in both the CIS and non-CIS samples. The significant effect of changes in domestic market structure on enterprise restructuring in the CIS is surprising given recent evidence on barriers to entry in transition

economies. Djankov, La Porta, Lopez-de-Silanes, and Shleifer (2000) document the number of procedures, and the associated time and cost, for starting a new business in 75 countries around the world, including 20 transition economies. They find that establishing a new business in Russia, which dominates the CIS sample here, and Ukraine takes twice as much effort, time, and money than a start-up in Eastern Europe. It also takes 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, Broadman (2000) documents the presence of significant geographic segmentation in Russia and a striking lack of competition at the regional level. Further research is needed to reconcile the results of these studies with the findings in Table 7.

An important contribution of the studies surveyed here is that several of them do not focus on product market competition as the sole explanation for changes in enterprise performance. Instead, they recognize that several policies are being implemented at the same time and attempt to control for other policies, as well as look for interaction effects between policies.

Li (1997) studies the effects of increased competition, improved managerial incentives, and factor reallocation across industries and firms on enterprise productivity. He finds that factor reallocation accounts for about 60% of the total improvement in efficiency, while increased competition accounts for about 30%, with better managerial incentives accounting for the rest.

Djankov and Hoekman (2000) investigate the relationship between firm productivity and increased competition in Bulgaria during the 1991-95 period, focusing on two major changes in policies-opening to international trade and the de-monopolization of state-owned industry. They find that changes in import competition and domestic market structure (industry concentration) have a positive impact on subsequent total factor productivity growth. This finding is only robust when they control for the availability of soft budget constraints at the enterprise level. The same result obtains in Anderson, Lee, and Murrell (2000), this time on the effect of domestic market structure and soft budgets. The analyses illustrate the

importance of accounting for changes in other policies that may enhance or attenuate the effect of greater competition.

Table 8, based on the methodology used for Table 2, examines the statistical significance of differences in the effects of product market competition across regions and across types of competition. In the first panel, we show that competition from local producers has a stronger effect than import competition but the difference is only statistically significant in column 4, and marginally significant in columns 5 and 6. The second panel shows that competition has a stronger effect in explaining enterprise restructuring in non-CIS countries than in CIS countries and that this difference is statistically significant. The third panel compares the relative importance of foreign competition in the CIS and non-CIS countries, suggesting that foreign competition has a larger effect in Eastern Europe and the Baltics than 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 non-CIS countries.

Economic effects of competition are large. The magnitude of the coefficients in the studies we survey imply that in CIS countries firms that face near perfect competition are 40-60% more efficient than enterprises that operate in near monopoly markets. In contrast, increased competition in non-CIS countries results in 30% higher efficiency for firms that operate in near perfectly competitive markets. This difference may be due to the fact that changes in enterprise restructuring in response to changes in market structure exhibit diminishing returns. Since the non-CIS countries started the transition process earlier, the effects of additional changes in competitive pressures may be smaller.

8. The Role of Institutions in Enterprise Restructuring

The beginning of transition coincided with the publication of North's (1990) influential book, with its central message that institutions provided a crucial underpinning to market-capitalism and that the process of building these institutions was fraught with difficulties. This message was not at the forefront

of policy discussions during the early years of transition. Stabilization, privatization, and liberalization

dominated the agenda.[30] Gradually the focus has changed, spurred by studies showing the hefty costs of

inefficient state administrations and corruption (Kaufmann1994) and by the recognition that the

relatively poor performance of the CIS countries was not easily explained by differences in the standard

reforms. Some scholars have also ascribed the disappointing Czech economic performance to a lack of

attention to corporate governance and the financial system during mass privatization (Coffee, 1996).

Now, in contrast to the early neglect, institutions are in vogue (Johnson, Kaufmann, and Shleifer, 1997,

Blanchard and Kremer, 1997, Stiglitz, 1999).

Restricting ourselves to enterprise-level empirical studies of the determinants of enterprise

restructuring, as we do in this paper, there is a relatively small amount of evidence on the importance of

institutions. One reason for this is that research has tended to follow policy, focusing on privatization,

competition, and soft-budgets, rather than institutions. Moreover, whereas competition, privatization,

and hardening of budgets can vary greatly between enterprises, the institutional framework is often the

same for all enterprises, leading to conceptual problems in designing tests. Thus, our review of the

evidence on institutions necessarily examines only a small number of studies. Since these studies vary

widely in methodology and focus, we cannot synthesize the results using the methods of previous

sections. The findings in this section are less emphatic: the enterprise level evidence on the link between

institutional reform and enterprise restructuring is still thin.[31]

An influential paper by Blanchard and Kremer (1997) has claimed that the absence of contract

enforcement mechanisms was a primary factor causing the dramatic fall in output in early transition in

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

[31]. In making this conclusion, we must emphasize that we refer only to enterprise level evidence. Cross-sectoral or cross-county empirical results (e.g. Johnson, Kaufmann, and Shleifer, 1997, Blanchard and Kremer, 1997) are outside the frame of reference for this paper.

the CIS. They hypothesize that weak contract enforcement will be more critical for those enterprises whose input-supply relationships are more complex, a prediction that 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). There are several papers that test this hypothesis using enterprise-level data. Konings and Walsh (1999) and Konings (1998) show statistically significant evidence supporting this prediction for Bulgaria, an insignificant coefficient with the predicted sign for Estonia, and a coefficient with the wrong sign for the Ukraine. Marin and Schnitzer (1999) provide evidence in support of the hypothesis for the Ukraine, while Recanatini and Ryterman (2000) fail to support it for Russia. Application of the methods outlined in Section 3 leads to ambiguous results, best characterized as providing only weak support for the Blanchard-Kremer hypothesis. Using a somewhat less conventional explanatory variable, the number of products produced, Konings and Walsh (1999) find support for the Blanchard-Kremer hypothesis for a sample of old firms, whereas the effect is not present in de novo firms. This result suggests that weak institutions are not the central problem, since these institutions apply to enterprises old and new. Instead, the breakdown of old relationships and the destruction of information might be the critical factors that produce these results. Recanatini and Ryterman (2000) present evidence in support of this interpretation.

Institutional reform can lead to improved enterprise efficiency when legal rules are effective in structuring economic transactions and resolving disputes. Economic agents can then turn to public bodies, such as the courts and the police, to enforce those rules. Although a large proportion of transactions everywhere in the world are enforced through private mechanisms, such as reputation, these mechanisms are sometimes costly, especially if the parties feel the need to resort to private force (Hay, Shleifer, and Vishny, 1996). Institutional reforms may therefore enhance enterprise restructuring if the legal system replaces more costly private mechanisms of supporting transactions.

Focusing on private Vietnamese firms, McMillan and Woodruff (1999a,b) document the nature of enforcement of trading relations when formal institutions are virtually non-existent.[32] Trading relations depend on prior reputation, built using information from business networks or prior experience, with networks used to sanction defaulting customers. But these private mechanisms may lead to inefficiency. Reliance on private sources of information requires frequent visits to the trading partner to gain information, wasting managerial time, and limiting the geographic scope of transactions. Moreover, continuing to deal with customary trading partners means refusing to deal with new entrants, and consequently less restructuring in procurement activities.

Formal business associations and informal networks can also serve as repositories of information and disposers of sanctions, supporting transactional activities (Greif 1993, 1994). Such associations have emerged spontaneously during the transition process, and have been investigated empirically in the case of the early transition in Russia (Ickes, Ryterman, and Tenev, 1995; Recanatini and Ryterman, 2000). These studies show that members of business associations are more likely to be successful in restructuring than are non-member firms: affiliation with a business association reduces the probability of output decline by 47 percent. But there are several reasons why such a relationship might exist, for example supplying information (Recanatini and Ryterman 2000) or facilitating the supply of credit (Perotti and Gelfer 1999). Hendley, Murrell, and Ryterman (2000) find that formal associations do not play a large role in enforcing contracts in Russia, although informal networks of older enterprises might be important. Similarly, McMillan and Woodruff (1999b) find only a relatively small role for business associations in dispute resolution in Vietnam.

Some have argued that the absence of institutions can lead to a reliance on criminals as contract enforcement agents, perhaps even spurring the rise of such groups (Leitzel, Gaddy, and Alexeev, 1995).

[32.] Only 9 percent of Vietnamese managers thought the courts could enforce contracts, in contrast to 58 percent of Russian managers and 55 percent of Ukrainian firms (Johnson, McMillan, and Woodruff, 1999a).

McMillan and Woodruff (1999b) do not find criminal groups to be an important feature of business activity in Vietnam (only 2 percent of managers admit to having used "bounty hunters" to collect payments). Koford and Miller (1998) document a similarly low usage of criminal enforcement in Bulgaria, as do Johnson, McMillan, and Woodruff (1999a) for Poland and Romania. Evidence is mixed on Russia. Johnson, McMillan, and Woodruff (1999a) show nearly one half of small Russian firms resorting to the help of organized crime in their dealings with suppliers and customers. In contrast, Hendley, Murrell, and Ryterman (2000) find little evidence of large Russian enterprises using private security firms (e.g. the mafia) for contract enforcement.

The overall picture, then, does not suggest the heavy reliance on extra-legal methods of enforcement that had sometimes been suggested during the early transition (Greif and Kandel, 1995). This suggestion arose from the supposition that there was an extreme failure of formal contract enforcement institutions, which now seems incorrect. Enterprises (excepting the Vietnamese ones) use the courts frequently and rate their effectiveness quite highly. Johnson, McMillan, and Woodruff (2000) find 74% of the respondents in small firms in five transition countries viewing the courts as able to enforce contracts. Hendley, Murrell, and Ryterman (2001) find large Russian enterprises rating the legal system relatively highly compared to other institutions, using the courts frequently, and regarding the courts as effective in enforcing contracts.

Johnson, McMillan, and Woodruff (2000) address the crucial question of the circumstances under which the formal institutions are useful. They find that confidence in the courts particularly affects new relationships, allowing firms to undertake transactions that would otherwise not be consummated. Since new relationships are associated with new entry and restructuring, this suggests that formal contract enforcement institutions are crucial to the process of growth and development. The results in Hendley, Murrell, and Ryterman (2001), however, suggest a paradox. They find that the amount of legal human capital possessed by enterprise employees is an important determinant of success in transactions in

Russia. But this capital is most likely to be present in larger, established enterprises, since legal

knowledge takes time and resources to accumulate and there are economies of scale in the use of law.

Evidently the new enterprises that could make most use of formal enforcement institutions are the ones

that have a comparative disadvantage in using these institutions. These facts pose a challenge for

institutional design in the future.

Even if criminal groups do not have much of a role in contract enforcement, they do affect

businesses when wielding their comparative advantage, running protection rackets, stealing goods and

cash, etc. Such criminal activity certainly represents a failure of institutional reform, in this case of law

enforcement institutions. Johnson, McMillan, and Woodruff (1999b) find remarkable variation in such

activity across Eastern Europe: while less than 1% of Romanian firms make payments for protection,

more than 90% of Russian firms do so. But these direct costs are only part of the picture, since criminal

activity also reduces the incentive for enterprise restructuring. For example, in examining the

determinants of renovations in Warsaw and Moscow shops, Frye (2001) finds that the quality of police

services is a critical factor. Using the opinion of managers on whether courts can enforce contracts as

the principal measure of property rights enforcement, Johnson, McMillan, and Woodruff (1999b)

estimate that firms perceiving property rights to be insecure invest nearly 40 percent less than firms that

perceive the security of property rights to be adequate. These studies suggest that, at low levels of

institutional development, lack of enforceable property rights might be more important than the absence

of external financing in determining investment in new projects or expanded capacity.[33]

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[33]. Several studies that fall outside the scope of our review because they are conducted at a more aggregate level than enterprises, offer similar results. Pistor (2001) argues convincingly that securities market laws and regulations, rather than corporate law, have been crucial elements in explaining the superior performance of the Polish (and perhaps Hungarian) corporate sector, relative to that in the Czech Republic. Glaeser, Johnson, and Shleifer (2001) similarly argue that capital market regulations in Poland have led many new firms to go public and raise capital by issuing equity. Slavova (1999) uses an aggregate measure of bank credit extended to private firms in 16 transition economies to show that the extensiveness and effectiveness of institutions that support external financing (pledge law, bankruptcy law, the court system) affect the flow of bank financing to the private sector. The study also finds that stock market capitalization in these countries depends on the enforcement of regulations on public disclosure of information and penalties of managers who breach their duty to minority shareholders. In a companion paper, Slavova (2000) finds that the strength and enforcement of the pledge and bankruptcy laws

Establishing effective corporate governance should be at the heart of the institutional reforms aimed at the firms on which this paper is focused, the large firms beginning the transition in the state sector. Surprisingly, however, there has been little systematic empirical work at the enterprise level on the effects of corporate governance institutions. While Black, Kraakman, and Tarassova (2000) and Fox and Heller (1999) for Russia, and Stiglitz (1999) more generally, claim that the failure of corporate governance institutions has been of great importance, their evidence is anecdotal. Anderson, Korsun, and Murrell (1999) do use systematic survey evidence 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. Similarly, the evidence that we present in Section 4, on the effects of different owners in the CIS and Eastern Europe, is consistent with greater dysfunction of corporate governance institutions in the CIS, but the argument is indirect. Further enterprise level work on the effects of corporate governance institutions is certainly of some urgency, given the present policy importance of the topic and the paucity of existing evidence.

The above paragraphs have focused on the direct effects of institutional reform on enterprises. But indirect effects might be just as important. When good institutions are lacking, costly substitutes might be needed, perhaps necessitating second-best measures in other policy areas.[34] Those owners who are most effective in a world of perfectly functioning institutions might be relatively less effective when corporate governance institutions do not function well or contract enforcement is weak. Hendley, Murrell, and Ryterman (2001) find that increases in both state ownership and employee control raise the effectiveness of enterprise transactions. A decrease in competition increases the success of transactions.

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in transition economies has a significant effect on the flow of foreign direct investment. This, in turn, enhances enterprise restructuring efforts, as documented in Section 4.

[34]. Murrell (1992) suggests that otherwise unpalatable old institutions might be temporarily useful for this reason. Intriligator (1994) and Stiglitz (1999) argue for delays in privatization to give time for the reform of legal institutions. In contrast, Boycko, Shleifer, and Vishny (1995) contend that political pressure for legal reform appears only after privatization.

The explanation for these results is that alternative mechanisms substitute for weak institutions. In the dire economic conditions of Russia, the probability that the enterprise will survive and the probability that enterprise personnel will be around to implement long-term agreements are greater the smaller is non-state outsider ownership. Similarly, when contracts are poorly enforced, increases in competition expand the opportunities for hold-up.

This analysis suggests that institutional weaknesses can reduce the potency of policies that previous sections have shown to be effective. Weak contract-enforcement institutions can be more of a problem for outsider owners than for state ownership. Weak corporate governance results in a greater need for ownership concentration (Claessens and Djankov, 1999b), which then limits the sources of outside finance. Increasing competition can initially have deleterious effects (Blanchard and Kremer 1997). Bilateral monopoly might be beneficial, as Hendley et al. (2001) find in Russia, echoing Kranton's (1996) theoretical results. Similarly, Jin and Qian (1998) and Che and Qian (1998) show in theory and practice (in China) that local government ownership might be superior to private ownership, when the legal system has no power to control a predatory central government.

Conversely, institutional innovations can help to moderate the deleterious effects of less-than optimal policies. Prasnikar and Svejnar (1998) use data on 458 Slovenian firms that have not gone through privatization and show that workers appropriate depreciation funds less than other funds, because of a rule that these must be used for investment. Hence a crude institution, a rule and its enforcement, can counter deficiencies in polices elsewhere, for example where workers might be tempted to decapitalize state-owned firms. Prasnikar and Svejnar (1998) also show that state-enterprise managers who have their own private firms do not siphon off cash flows to those firms. The authors interpret this as evidence of a well-functioning system of penalties for breach of management contracts. However, seemingly sensible second-best institutions can fail as well, as Djankov (1999a) shows for the enterprise isolation program in Romania.

Informal institutions, although playing an important role in North's overview, have been little examined in the transition context. One exception is the work of Earle and Sabirainova (1999), who argue that regional pockets of large-scale wage arrears in Russia can be explained by the existence of equilibria at the local level. The workers' willingness to accept wage arrears is sustained by the widespread presence of arrears on the local market and in turn the workers' acceptance contributes to the amount of arrears. At the national level, the wage arrears were first precipitated by the government itself, which, reacting to a variety of pressures to balance the budget, sequestered scheduled payments. With the government itself a major non-payer, a culture of non-payment developed across the economy. With local equilibria locked-in and with cultural norms playing a role, this thesis can be placed squarely within institutional theory, even though it refers to informal norms rather than formal rules and even though the institution is detrimental to economic performance.

This section is ample testament to the disjointedness and the many holes in the enterprise-level evidence on the effect of institutions on restructuring. Thus, the major difference between this and the preceding sections, the absence of tables synthesizing the major results, reflects the state of the literature. Evidently, if institutions are to deserve the prominence in policy deliberations that they presently have, empirical work at the enterprise level is a matter of some urgency. For such work, the above paragraphs have identified interesting themes. A number of studies suggest that, in the absence of credible institutions, some otherwise-sensible economic policies do not work well and in fact might worsen incentives to restructure. In such cases, second-best solutions (for example, some state ownership) may yield better results. The nature of the complementarity between institutional reforms and the other reforms that we have examined in Sections 3-7 certainly is a major item on the research agenda.

9. Conclusions

This study documents and synthesizes the empirical evidence on the determinants of enterprise restructuring in the early years of transition from central planning to a market economy. It identifies the main areas of existing research, as well as a number of topics where further research is needed. Since we have listed the main conclusions of the paper already in the introduction, we close this paper by emphasizing one empirical result that runs throughout this paper and by highlighting those areas of research that require future emphasis.

A central finding of the paper is that transition policies have had similar effects on the restructuring process in CIS and non-CIS countries in terms of direction, but not in terms of economic or statistical significance. In particular, privatization, hardened budget constraints, and product market competition all appear to be important determinants of enterprise restructuring in non-CIS countries, while they are less effective in the CIS. We hypothesize, but cannot document with rigor, that the difference in impact is due to the varying degree of institutional development between the regions.

This observation highlights the most critical area where further research effort is needed in order to understand more fully the role of economic reforms in generating enterprise restructuring. Scholars of the transition process should examine more closely the nature and type of institutions necessary to enhance the restructuring process. While a number of studies on the role of institutions have been identified in this survey, they do not provide a systematic body of evidence that can be useful in guiding economic policy and especially the trade-offs in choosing between policies. Especially important in this regard is the link between the nature of institutional development and the types of owners who are most productive. If this link could be more fully understood, then policymakers would be able to use this information productively to design more effective methods of privatization and to implement institutional reforms that are targeted to the structure of ownership that is present in a country.

In surveying the literature, we have also found relatively little evidence on the role of manager incentives, particularly on the effect of (credible) penalties imposed on managers for lack of effort, for expropriation of other stakeholders, or for outright theft. Anecdotal evidence has emerged to suggest that the relative success of Chinese micro-economic reform depends as much on the severe punishments for poor managers, as it does on rewarding superior performance. This may explain why the restructuring process has been slower in the CIS, where decentralization of government and privatization amidst weak institutions have left few mechanisms to punish managers who engage in inefficient practices, or worse. To date, there has been no concerted effort to subject these hypotheses to rigorous empirical tests.

A third area for emphasis in future research is the link between enterprise restructuring in existing firms and new entrepreneurship. There could be a complementary effect, whereby enterprise restructuring frees productive resources that move into the new private sector. Alternatively, the two activities might be substitutes for each other: enterprise restructuring makes existing firms more productive, and hence more difficult to challenge. These alternatives have been discussed in several studies surveyed here. Yet, there has been no systematic empirical evidence to support either view.

Beyond their significance in identifying the main results of existing research and the gaps in that research, the findings of this study are pertinent for policy makers and advisers in transition economies that are just starting the implementation of economic reforms, e.g., Belarus, Tajikistan, Turkmenistan, or that have implemented only partial reforms, e.g., China, Romania, Uzbekistan, and Vietnam, or that may enter the transition process in the years to come, e.g., Cuba and North Korea. Policies in these countries can be guided by the successes and failures of the leading reformers, which have been so intensively studied in the research work that we have synthesized above.

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Table 1: The Effects of Private versus State Ownership on Enterprise Restructuring
     Composite statistics derived from 89 analyses appearing in 35 studies

			Weighting Method Used to Obtain the Composite Test Statistic					
			(1)	(2)	(3)	(4)	(5)	(6)
Regions	Type of dependent variable in the analyses	Number of analyses contributing to the composite test statistic	None	Extent of controls (vector X)	Attempts to address selection bias	Inverse of number of analyses used from same study	Years of privatization covered by the analysis	Overall rating of quality
			Normally distributed composite test statistics					
All countries	Both	89	12.48	10.92	12.07	11.85	12.29	10.00
Non-CIS	Both	36	14.83	15.14	13.94	15.00	13.98	14.13
CIS	Both	53	3.95	3.55	2.84	1.69	3.10	0.07
All countries	Quantitative	61	10.82	9.31	9.76	11.17	10.82	8.43
Non-CIS	Quantitative	33	14.37	14.69	13.58	14.61	13.66	13.87
CIS	Quantitative	28	0.37	-0.08	-2.28	-0.98	-1.03	-3.73
All countries	Qualitative	28	6.27	5.83	7.34	4.56	5.84	5.53
Non-CIS	Qualitative	3	3.71	3.71	3.71	3.60	3.71	3.49
CIS	Qualitative	25	5.35	5.11	6.76	3.37	5.32	5.06

Table 2: Comparing the Size of Privatization Effects Across Regions
   Partial correlation coefficients and tests statistics for regional differences derived from 89 analyses from 35 studies

Type of dependent variable in the analyses		Number of analyses used for composite partial correlation	Weighting Method Used					
			(1) None	(2) Extent of controls (vector X)	(3) Attempts to address selection bias	(4) Inverse of number of analyses in same study	(5) Years of privatiz-ation	(6) Overall rating of quality
			Partial correlation coefficients and test statistics on their difference					
Both	Partial correlation for Non-CIS	36	0.084	0.084	0.073	0.080	0.090	0.076
Both	Partial correlation for the CIS	53	0.038	0.035	0.041	0.036	0.037	0.016
Both	Test statistic for difference between partial correlations		3.92	4.24	2.62	3.77	4.23	4.83
Quantitative	Partial correlation for Non-CIS	33	0.078	0.079	0.070	0.073	0.088	0.074
Quantitative	Partial correlation for the CIS	28	0.016	0.012	-0.001	0.023	0.007	-0.019
Quantitative	Test statistic for difference between partial correlations		4.15	4.26	4.51	3.62	5.20	5.42
Qualitative	Partial correlation for Non-CIS	3	0.146	0.146	0.146	0.147	0.146	0.145
Qualitative	Partial correlation for the CIS	25	0.063	0.059	0.089	0.051	0.068	0.064
Qualitative	Test statistic for difference between partial correlations		1.99	2.09	1.36	2.23	1.86	1.84

Table 3: The Gains or Losses Due to Ownership Change: A Sampling of Estimates

The table below shows estimates of the performance of a 100% private firm relative to a 100% state firm.
For the growth measures, relative performance is private firm yearly growth rate minus state firm yearly growth rate: estimates greater than zero indicate a positive privatization effect.
For the levels measures, relative performance is private firm productivity as a percent of state firm productivity: estimates greater than 100 indicate a positive privatization effect.

Growth of Output or Sales		Growth of Productivity		Levels of Productivity	
% Private minus % State	Country	% Private minus % State	Country	Private as a % of State	Country
8.7	Poland	4.3	Central Europe	162	Russia
7.3	Central Europe	3.1	Eastern Europe	29	Mongolia
-9.7	Russia	3.46	Russia	70	Mongolia
10.9	Bulgaria	10.6	Kyrgyz	35	Mongolia
2.5	Czech Republic	3.6	Georgia/Moldova	57	Russia
2.1	Hungary	0.9	Georgia/Moldova		
5.4	Poland				
7.7	Romania				
6.2	Slovakia				
6.8	Slovenia				
18.0	Kyrgyz				

Figure 1: How Ownership Affects Firm Performance after Privatization
Estimated Effects of Changing from Traditional State Ownership to Different Private Owners
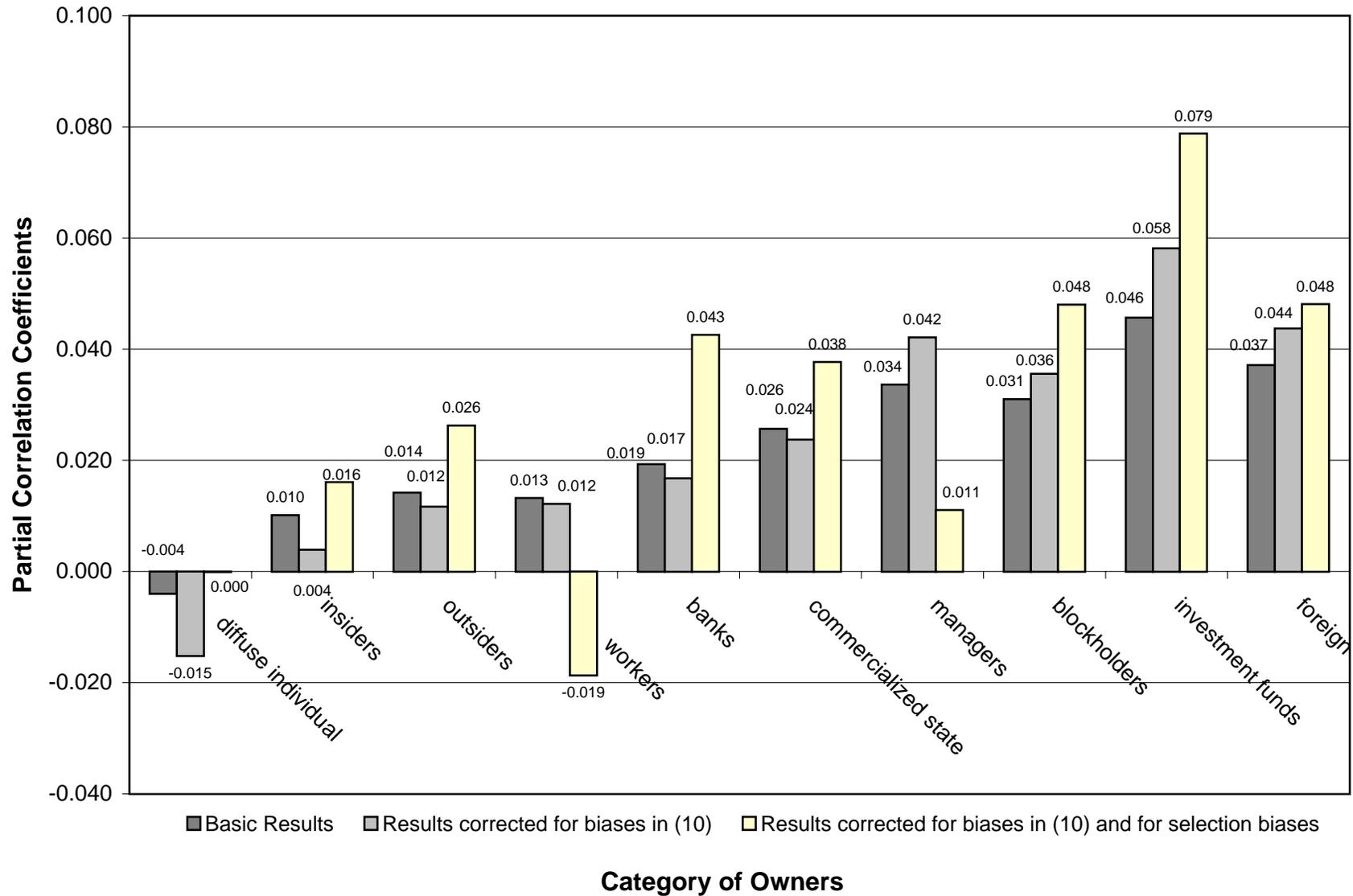
Table 4: Testing Differences between the Effects of a Variety of Owners
     Statistics testing differences between owners derived from 331 pairwise comparisons of owners taken from 23 studies

	Workers	Diffuse Individual	Traditional State	Managers	Insiders	Outsiders	Commercial-ized State	Banks	Blockholders	Foreign
			t-statistic for {(effect of owner listed on row) minus (effect of owner listed on column)}							
Diffuse Individual	1.50									
Traditional State	1.37	0.01								
Managers	1.93	0.92	0.89							
Insiders	2.73	3.64	2.53	0.42						
Outsiders	3.31	4.23	3.67	1.21	2.05					
Commercialized State	4.33	8.13	6.12	2.22	5.99	2.55				
Banks	3.53	3.28	3.41	1.92	2.11	1.26	0.40			
Blockholders	4.81	7.74	7.48	2.97	6.39	3.82	2.56	0.44		
Foreign	4.81	7.79	7.80	2.98	6.54	3.87	2.61	0.45	0.02	
Investment Funds	5.56	6.17	7.05	4.28	5.32	4.48	3.71	2.47	2.96	2.95

Interpreting the table: 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 non-empty, 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.

Figure 2: Regional Variations in the Effects of Different Types of Owners
Comparing Ownership Effects in the CIS to those in non-CIS
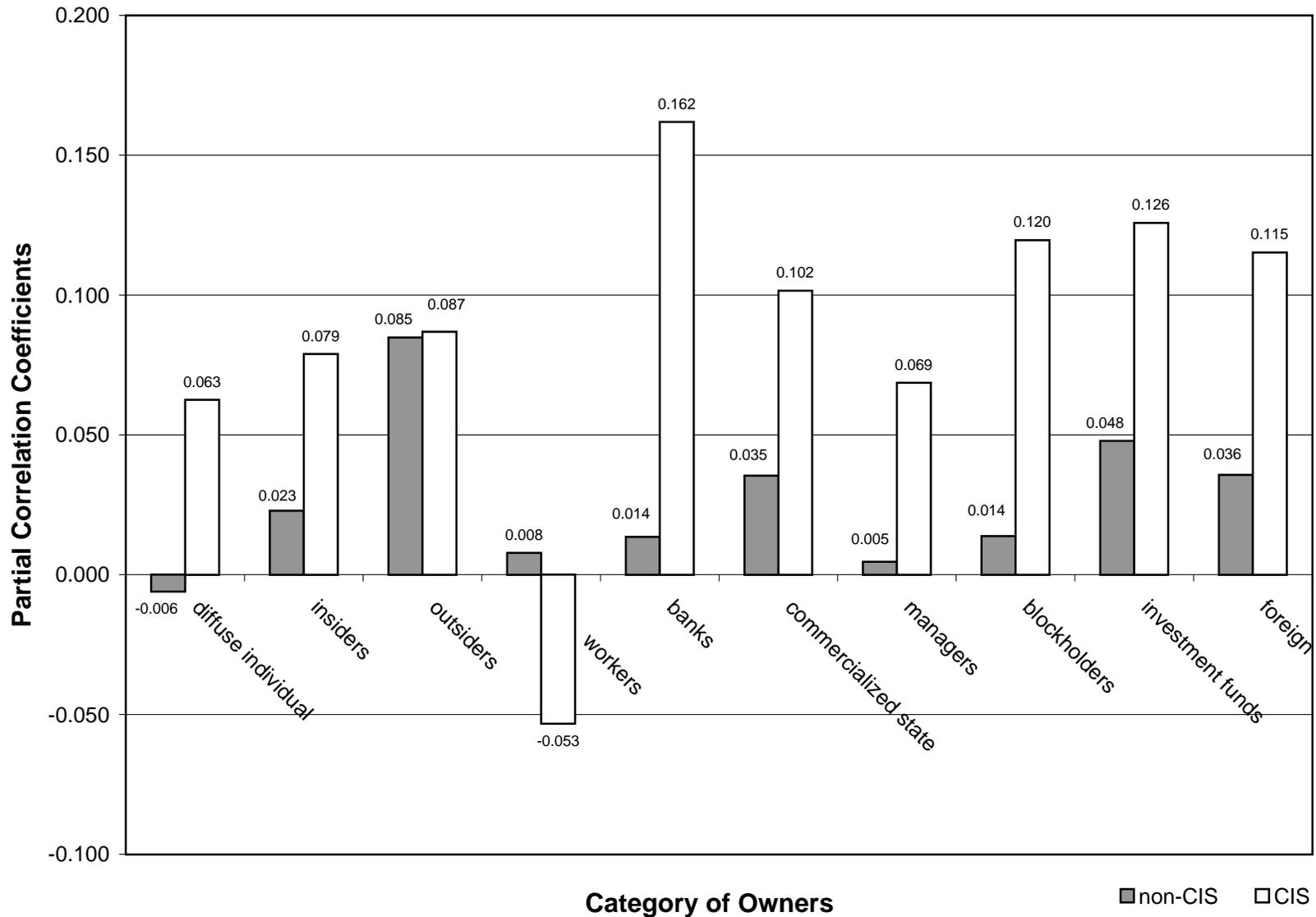
Table 5: The Role of Managers in Enterprise Restructuring
        Statistics derived from 29 analyses appearing in 8 studies

	Number of analyses contributing to the composite effect	Weighting method used					
		(1)	(2)	(3)	(4)	(5)	(6)
		None	Extent of controls (vector X)	Attempts to address selection bias	Inverse of number of analyses in same study	Years of reform covered by the analysis	Overall rating of quality
A. Do Managers Matter?							
		Composite normally distributed statistics					
Both turnover and incentives	29	6.77	3.21	6.36	4.37	5.41	6.41
Management turnover	17	8.92	9.04	9.13	7.13	7.17	9.45
Management incentives	12	-2.28	-4.18	-1.42	-1.91	-0.55	-1.52
B. Comparison of Significance							
		Partial correlation coefficients and test statistics on their difference					
Management turnover	17	0.08	0.08	0.08	0.07	0.08	0.09
Management incentives	12	-0.01	-0.02	0.01	0.01	0.01	0.01
test of difference		4.68	5.74	4.25	3.61	3.73	4.56

Table 6: The Importance of Hardening Budget Constraints in Enterprise Restructuring
      Statistics derived from 28 analyses appearing in 10 studies

Regions	Number of analyses contributing to the composite test statistic	Weighting Method Used					
		(1)	(2)	(3)	(4)	(5)	(6)
		None	Extent of controls (vector X)	Attempts to address selection bias	Inverse of number of analyses used from same study	Years of reform covered by the analysis	Overall rating of quality
A. Testing the effects of hardening budgets		Composite normally distributed statistics					
All countries	28	15.24	15.04	14.75	10.70	14.51	12.22
non-CIS	21	15.50	16.33	15.68	9.73	14.03	13.99
CIS	7	3.63	2.18	1.93	4.49	4.76	1.52
B. Comparing the effects between regions		Partial correlation coefficients and test statistics on their difference					
non-CIS	21	0.06	0.06	0.06	0.05	0.05	0.05
CIS	7	0.04	0.03	0.03	0.04	0.04	0.03
test of difference		0.81	0.94	0.82	0.42	0.73	0.67

Table 7: The Effect of Competition on Enterprise Restructuring
Statistics derived from 67 analyses appearing in 17 studies

Sample	Number of analyses contributing to the composite test statistic	Weighting Method Used to Obtain the Composite Test Statistic					
		(1) None	(2) Extent of controls (vector X)	(3) Attempts to address selection bias	(4) Inverse of number of analyses used from same study	(5) Years of reform covered by the analysis	(6) Overall rating of quality
		Composite normally-distributed statistics					
All countries, all competition	67	8.19	8.43	8.33	11.40	8.58	10.58
All countries, import competition	23	3.31	3.15	2.55	4.03	4.38	4.11
All countries, domestic market structure	42	5.43	5.81	6.03	8.02	4.72	6.27
Non-CIS countries, all competition	46	8.63	9.06	8.79	10.77	8.05	10.07
CIS countries, all competition	21	1.85	1.69	2.61	5.14	3.42	3.68
Import competition, non-CIS	15	4.77	4.77	3.94	4.21	5.12	4.65
Import competition, CIS	8	-0.93	-1.33	1.41	0.87	0.23	-0.19
Domestic market structure, non-CIS	29	4.46	4.94	4.81	6.24	2.89	4.54
Domestic market structure, CIS	13	3.09	3.08	3.66	5.16	4.08	4.47

Table 8: The Relative Effect of Different Types of Competition in Different Regions
Statistics derived from 67 analyses appearing in 17 studies

Sub-samples	Number of analyses contributing to the composite test statistic	Weighting Method Used					
		(1)	(2)	(3)	(4)	(5)	(6)
		None	Extent of controls (vector X)	Attempts to address selection bias	Inverse of number of analyses used from same study	Years of reform covered by the analysis	Overall rating of quality
		Partial correlation coefficients and test statistics on their difference					
Import competition	23	0.01	0.01	0.01	0.01	0.01	0.01
Domestic market structure	42	0.03	0.04	0.04	0.07	0.04	0.04
test of difference		-1.37	-1.54	-1.56	-3.19	-1.73	-1.71
All competition, non-CIS	46	0.06	0.06	0.07	0.08	0.06	0.07
All competition, CIS	21	-0.01	-0.01	-0.01	0.01	0.02	0.01
test of difference		3.67	3.41	3.49	3.24	3.31	2.86
Import competition, non-CIS	15	0.05	0.05	0.06	0.04	0.05	0.04
Import competition, CIS	8	-0.08	-0.09	-0.07	-0.05	-0.06	-0.07
test of difference		3.64	3.65	3.56	2.97	3.59	3.54
Domestic market structure, non-CIS	29	0.04	0.04	0.04	0.04	0.03	0.03
Domestic market structure, CIS	13	0.02	0.03	0.03	0.10	0.04	0.07
test of difference		0.32	0.26	0.12	-2.44	-0.32	-1.28