Enterprise Restructuring in Transition:
A Quantitative Survey

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NOTE: We will make final revisions to this paper in July 2000, at which time we will make reference to all pertinent papers that have come to our attention by June 30, 2000. If anyone reading this survey knows of a pertinent paper not presently included in the list of references, please send a copy or a reference to one of us.

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Abstract.

There are now over 125 empirical papers that analyze the process of enterprise restructuring in transition economies. We synthesize the results of these papers, using insights from meta-analysis. This synthesis provides new insights into the relative effectiveness of different reform policies. It addresses 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 in the last decade, and the ramifications for the world economy and polity. Analysis of enterprise restructuring is central in any effort to gain an understanding of the effects of the most important reform measures adopted in transition countries. With enterprise restructuring apparently more successful in some countries than others, the natural question that arises is whether such differences relate systematically to policy. In this paper, we address this question by examining how the effects of policy have varied between transition countries.

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 1999). But the formulation of this criticism has not taken advantage of the enormous amount of 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 in this area of research. 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 125 empirical 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

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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 Huang (1995), Smith (1990), Neumark and Wascher (1998), Phillips and Goss (1995), and Stanley (1998).

of many studies on a similar topic, combining many tests with weak power to produce a single one with larger power. Second, these methods allow one to test hypotheses across groups of studies. For example, we examine whether the replacement of managers is more effective than the addition of incentives and we test whether privatization has stronger results in Eastern Europe than in the former Soviet Union. Third, the synthesis of results can address the thorny issue of differences in the quality of studies, allowing one to gauge the extent to which the conclusions change when one gives greater weight to those studies that are methodologically 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. The privatization effect is, however, statistically insignificant in the CIS countries. 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 survey also documents the effects of different types of owners on enterprise restructuring. We find that the most effective owners (foreign investors) produce ten times as much restructuring as the least effective private owners (diffuse individual ownership). Manager ownership leads to twice as much restructuring as that observed in companies privatized to outsiders (blockholders **and** individuals). Overall, however, privatization to outsiders is associated with 50% more restructuring than privatization

to insiders (managers **and** workers). State ownership within traditional state firms is less effective than all other ownership types. However, state ownership within partially-privatized firms is surprisingly effective, producing more restructuring than ownership by enterprise insiders, workers, (non-blockholder) individuals, and banks.

The effects of different owners varies between regions. Workers and outsiders are better owners in Eastern Europe than in the CIS, while banks and concentrated individual ownership are significantly more effective in the CIS than elsewhere. 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 in both CIS and non-CIS countries. 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 in Eastern Europe. The effect in the CIS is positive but statistically insignificant and significantly weaker than that in Eastern Europe.

Product market competition has a significant effect in improving enterprise performance. The sources of improvement differ between regions, however. In Eastern Europe, the beneficial effect comes primarily through import competition.   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. 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, our article does not survey the burgeoning literature on entrepreneurship in transition; for that, see Johnson, McMillan, and Woodruff (1999) and Bratowski, Grosfeld, and Rostowski (2000). Second, we do not include studies that investigate the size of the informal sector during the early years of transition and the reasons for informality; for that, see Johnson, Kaufmann, and Shleifer (1997). Third, we do not study the effects of ownership changes on the broader institutional environment (apart from hardened budget constraints). On this topic, see Hendley, Murrell, and Ryterman (2000) 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 do not need to reiterate anything but 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. These 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 benefits, 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 a secondary consideration 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 budget constraints. 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 factors that produced such behaviors, such as ownership by the state, soft budgets, managers who focused on physical production rather than profits, etc.. Thus, the typical study that we review presents estimates of an equation of the form:

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

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, 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.

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 a 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, finding that they generally lead to the same basic conclusions. Additionally, we adopt an alternative method of taking into account the fact reforms take several years to show their effects: we use the length of time that reforms have been in place as one of the properties of studies on which we collect data.

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 bias has been thoroughly recognized in the literature,

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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.

but solutions are not always easy to obtain. Thus, for example, only one-half of the estimates of γ used in Section 3, which examines private versus state ownership, employ a method that might counter such bias.

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 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 quite often differ. But the sign of bias is not uniform. While Earle (1998) finds the estimated effect of private ownership increasing when selection bias is countered for a sample of Russian firms, Anderson, Lee, and Murrell (2000a) find the opposite result for Mongolia. Claessens and Djankov (1998) find that better firms were privatized (a positive bias) in Poland and Slovenia while worse ones were privatized in the Czech Republic and Slovakia. They also find that selection bias is more important for small than for large firms.

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 affects 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. Since it is easier to discuss in context how we use this rating to gauge 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 the paper uses an adequate set of control variables in vector X, in order to judge the sensitivity of our conclusions on this score.

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. 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. We rate each paper on a scale of one to ten reflecting the quality of the empirical evidence contained in the paper. This quality rating reflects the 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.[4]

In sum, for each paper, we have the following indicators of the nature of the methodology employed:

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, on a scale of 1 to 3.

5. Our own subjective 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.

[4.] Although the quality ratings reflect our subjective assessments, they do not reflect only that. For example, whether a paper pays attention to selection bias is usually an objective fact. Also, quality of place of publication does not correlate perfectly with our subjective assessment of quality, but our quality score reflects the place of publication.

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 how this could be most effectively done, 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 emphasis on speedy privatization 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, 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. 1999). 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), with P some measure of the degree of private ownership of the enterprise. A large variety of variables take 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 a myriad other choices. One could fill a complete paper simply listing the

different Y's, P's, and X's that have been used.

We are interested in the size and statistical significance of the estimate of γ, which capture the

relevant information on the effects of privatization on performance. What is immediately apparent,

however, is that the γ's estimated in 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. This method

allows us to answer a limited, but important, range of questions. But, as we will see below, if the aim is

to make comparisons between the strength of the effects of privatization in different regions or between

different types of private owners, we must use methods that look at more than t-statistics.

In this section, we combine the results of 31 distinct studies.[5] Within these studies, we have

identified 82 conceptually distinct estimates of γ ($\hat{\gamma}$'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.

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

Together with each $\hat{\gamma}$ and t-statistic combination, we collected information on the $\hat{\gamma}$, the t-statistic, sample size, country under study, and the five indicators of methodology discussed in the previous section. Each $\hat{\gamma}$ and its 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 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 82 analyses on which we have information.

For a data set comprising M analyses, denote the t-statistics on the $\hat{\gamma}$'s by $t_1,...,t_M$. Then, form the following statistic:

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

This statistic has a standard normal distribution.[6] 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. This point shows the dangers of vote-counting (Hunter and Schmidt, 1990), a method of synthesis that is often implicitly adopted in reviews of the literature. Vote-counting assumes statistical non-significance in the aggregate when a set of studies has a median t-statistic that is insignificant.[7] 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 significant, as we will see below.

[6.] 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.

[7.] Consider the following example. Researcher A obtains a t-statistic of 2.0 in a study, pronouncing significance for the effect. Researcher B obtains a t-statistic of 1.0, announcing that A has been mistaken in pronouncing significance. But the combined statistic from both studies is $3/\sqrt{2} = 2.12$, and so B has actually strengthened the estimate of the significance of the effect.

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 exactly 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. 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 evidently positive is the case of quantitative indicators for the CIS. Thus, a second conclusion from the table is that the effects of privatization in the CIS countries are limited.[8]

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

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[8.] This pattern figured prominently in Nellis' (1999) review of studies on privatization in transition countries.

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.[9]

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

normal distribution:

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

Any such weighting procedure discounts those studies with smaller weights, effectively producing an

aggregate statistic that appears to be based on fewer studies.[10]

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: privatization has not had a significant positive effect. This

strengthening is reflected in the overall (quantitative and qualitative) weighted results for the CIS, which

unequivocally support the hypothesis of an insignificant effect of privatization.

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

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

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

[10.] 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.

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:

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

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

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

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.[11]

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

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

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

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:

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

where the formula applies equally to partial correlations (Greene 2000).[12] 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

another, then the former analysis will appear to have 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. Nevertheless, we cannot dismiss completely the possibility that our test results reflect a larger

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

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 greater 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 effect on the results of using different weights is examined.

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 fact, only one of the statistics fails to reach the 5% significance level. In all cases but one, 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 0.093). Such a judgment is in the eye of the beholder, but an example might make the values of partial correlations more

intuitive. 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 0.084 for the

non-CIS) and assume 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.[13] 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 16% more firms growing, while complete privatization in the CIS would result in only 2% more

firms growing.[14]

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_Y's: there is much variation in enterprise outcomes that cannot be explained by the

standard control variables available to the econometrician. For the CIS, there is another factor, a

significant percentage (33%) 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 31 $\hat{\gamma}$'s for the

quantitative Y's are positive.) Table 3 suggests that the effects of privatization are economically large.

[13.] 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.

[14.] And in fact, the unweighted statistics are the ones that are most favorable to the CIS. Use of the statistics in column (2) of Table 2, for example, would suggest that privatization resulted in 16% more firms growing in the non-CIS and 2% fewer firms growing in the CIS.

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) and 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 whether the type of private ownership matters.

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) and McConnell and Servaes (1990) 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) argued that individual block-owners have a strong incentive to monitor management because of their non-diversifiable holding in the corporation. Consistent evidence is

reported by Friend and Lang (1988). 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. 1999). 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 transaction costs inhibiting investors from 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.

When examining which types of owners are most productive, the typical study will present estimates of an equation which, in its simplest version, is:

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

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.

In general, many variables capture ownership shares (e.g., those of workers, managers, investment funds, banks, etc). When we turn to the results, we will use 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 the perfect collinearity between the three ownership variables.

The central objective in estimating (8) is to compare δ to θ (outsiders versus insiders) and each of these to 0 (the state versus insiders and outsiders). The latter comparison is simple, but the former presents some additional 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 partial correlation coefficients, showing the effect of changing ownership from state to outsider or from state to insider. But how does one find a scale-free estimate of the effect of changing from insider to outsider ownership? The problem here is that papers do not provide all necessary information to ascertain this estimate exactly.

To apply the methodology that has been developed above, we require a t-statistic on the test that $\delta = \theta$, which is not usually presented. To calculate this statistic, one needs $\hat{\delta}$ and $\hat{\theta}$ and estimates of the variances of $\hat{\delta}$ and $\hat{\theta}$, all of which we have, plus an estimate of the covariance $\hat{\delta}$ and $\hat{\theta}$, which papers invariably omit, since:

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

Fortunately, there is a reasonable, pragmatic method of obtaining an estimated 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, one can examine a simple theoretical case.[15] 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.[16] Third, we have investigated the size of the variance of $(\hat{\delta} - \hat{\theta})$ in two real-world data sets to which we have access, those used in the studies by 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:

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

If the first term within the braces on the right-hand side of (10) applies, the estimate of the variance will be between the average 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 average of the variances. (Alternatively, if the covariance were zero, which is unlikely, this factor would be 1.41 [= $2^{½}$].) The second term within the braces is again a conservative adjustment, for a scenario in which

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[15.] This assumes that there is no X in the equation.

[16.] 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.

the variances of $\hat{\delta}$ and $\hat{\theta}$ are of very different sizes (which they are usually not). That element of (10) ensures that a very small variance does not influence the results too heavily. Once (10) has been applied to obtain estimates of t-statistics, (7) is used to obtain estimates of partial correlation coefficients.[17] Obviously, the procedure that we apply is not first best, but first best cannot be obtained using 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.

We follow the empirical literature in identifying the ownership categories that we analyze. To settle on a list of ownership types to analyze in this paper, 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 the 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 owner categories:

1. traditional state ownership: state ownership in enterprises that are 100% state and that have not been part of a privatization program.

[17.] Of course, there is no need to apply (10) in those cases in which the appropriate t-statistic appears in the paper, that is for comparisons with the omitted ownership category.

2. state ownership in commercialized (or corporatized) 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 ownership share is identified as owned by a bank or by the state then the ownership is classified in 7 or 2).

9. foreign owners.

10. blockholders: outsider ownership that has been concentrated in the hands of large individual owners e.g. individual entrepreneurs, domestic firms, etc. (If the blockholder is 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 applies only when the study makes attempts to differentiate between various types of outsiders. When all outsiders are treated as one group, then owner 4 applies.

[18.] In most countries commercialization (or corporatization) occurred as a preparatory phase in the privatization process. 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, we assigned the results to the traditional state ownership category. This occurred only in a few cases.

The reader will immediately notice that some of these categories overlap: for example, workers and managers together are insiders.[19] However, one should not assume that the collective entity is the sum of its parts. We will examine this point when the results are presented.

From 23 studies, we have compiled a data set of 305 observations on the effects of different types of owners on quantitative enterprise outcomes.[20] Since each study usually contains several types of owners, each will contribute several different pair-wise comparisons to our data-set.[21] The central variables of interest in each observation are the t-statistics and sample sizes, the latter denoted n_{ijk}. These immediately lead 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. 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 brings to bear information from all of our data points when obtaining estimates of the effect of each type of owner.[22]

[19.] Even though individual studies do not compare overlapping categories, a synthesis of results can do so, given enough variation across studies in the types of owners examined.

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

[21.] 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.

[22.] 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.

This is accomplished using a simple dummy-variable regression framework, estimating the following

equation:

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

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

(11)

Of course, we only have information on the relative performance of different owners, so it is not possible

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

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

coefficient that measures the effect of switching ownership from traditional state ownership to owner

type m.

The individual r_{ijk} are obtained using many enterprise observations (sometimes thousands) and it is

important to use this fact in estimating the regression and interpreting its results. If the underlying data

are normally distributed, then the variance of the correlation coefficient is approximately (Shadish and

Haddock, 1994):

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

Since $var(r_{ijk} | ijk) = var(\varepsilon_{ijk})$, we can use a generalized least squares procedure and the information on

error variances to estimate the coefficients λ_m and their standard errors.[23]

The results are presented in Figure 1 and Table 4. Figure 1 concentrates on the numerical size of the

ownership effect, showing the size of the $\hat{\lambda}_m - \hat{\lambda}_1$, the effect of switching from traditional state ownership

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[23] Since we have estimates of the actual error variances, we embody in the GLS procedure the assumption that the error variances in the regression are equal to these estimates. The results can be easily obtained by using a standard generalized least squares procedure. However, the standard output must be interpreted to take into account the assumptions on error variances. See Hedges (1994).

to type m. Table 4 examines whether the privatization effects are significant, reporting the results of

standard tests of the null hypotheses that $\lambda_m - \lambda_h = 0$ for every m-h combination.[24]

On a first inspection of Table 4, one immediately notices the presence of 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. In fact, at a

rough approximation, there are three groupings of owners. At the bottom, traditional state ownership and

diffuse individual ownership do not have significantly different effects. In the middle, insiders,

outsiders, workers, banks, and commercialized state ownership are clustered. (The two ends of this

cluster, insiders and commercialized state are significantly different from each other.) The most effective

owners, none of which have statistically different effects from any other, are managers, concentrated

individual ownership, investment funds, and foreigners.

Figure 1 suggests that differences between owners are of great economic importance. Loosely

speaking, the most effective privatization (to foreigners) is ten times as productive as the least effective

privatization (to diffuse individual ownership). Managers are more than nine times as productive as

diffuse individual ownership.

Lest readers be concerned, we should explain one result that seems, on first glance, paradoxical, but

which, on reflection, reveals important information. Insiders are less productive owners than both

managers and workers, even though these two groups of employees constitute the full set of insiders. In

interpreting this result, it is crucial to note that the estimates for the effect of managers and workers

usually appear in different studies than those for insiders as a whole. Which owners are included is not,

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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 all the information on variances of coefficient estimates that is extracted from the original studies.

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. Thus, the studies that use variables on worker ownership will usually reflect instances where specific groups of workers have been able to make concerted efforts to obtain dominant ownership. Similarly, studies that identify managerial ownership will often reflect instances where there has been scope for managerial buyouts. These are very different situations from those in which an undifferentiated group of insiders has been given concessions.

Consider the following example: country A privatizes all of its enterprises by giving generalized concessions to insiders whereas country B privatizes half of its enterprises to managers and half of its enterprises to workers. In country A, privatization might be followed by struggles between workers and managers, who have diverse interests. Such struggles will delay adjustment. In country B, the worker-owned and manager-owned enterprises might each quickly implement their own forms of adjustment, since separately they each can reach decisions more quickly. Given the way that privatization studies are carried out, researchers on country A would be more likely to report results for the group of insiders, while those on B would show results separately for managers and workers. Then, our synthesis would show that undifferentiated insiders are not as good owners as managers and workers separately. Moreover, this would not be an artefact of the research process but would convey something very important about reality: privatizing to heterogeneous groups might be worse than privatizing to homogeneous groups. The whole is less than the aggregation of its separate parts.

By and large, the rest of the results are in accordance with expectations, but there are some surprises. Foreigners were expected to make productive changes and they are unsurprisingly the best owners. But it is notable that three other ownership types are very close indeed to foreigners. Certainly,

the estimated productiveness of managers and investment funds was not uniformly expected.[25] 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 only aspect of the result for state ownership in commercialized enterprises that still seems paradoxical is its position relative to banks. But, on realizing that many of the banks in transition countries are commercialized, formerly state-owned banks, still owned wholly or partially by the state, it is not surprising that bank ownership and commercialized state-ownership are statistically indistinguishable.

Lastly, it should be emphasized that nothing in the methodology that produces Figure 1 prevents the estimates of $\lambda_m - \lambda_1$ from being negative. Thus, Figure 1 shows that all owners are better than traditional state ownership (although some insignificantly so).

[25.] The point made above should be emphasized, however. Managers who made a concerted effort to buy-out the whole enterprise are probably over-represented in the evidence on managerial ownership.

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.[26] Of the 305 data points used in the previous section, 43% 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 results are on the borderline of statistical significance: the data reject the null hypothesis that all the $(\lambda_m - \lambda_1)$ are the same in the CIS and the non-CIS at the 5% significance level, but not at the 1% level.[27] This immediately suggests that some types of owners have different effects across regions and others do not.

[26.] 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 (2000) for a discussion of which of these institutions are most pertinent in a transition economy context.

[27.] All the papers used for this study examine the comparative effects of owners within a single country. Thus, it is not possible to produce results on the λ_m in one region versus the λ_m in another. Rather one has to focus on the effects compared to traditional state ownership (i.e., $\lambda_m - \lambda_1$).

Figure 2 presents estimates of the effects of the different types of owners in the two regions.[28] There are some dramatic and obvious divergences (e.g. banks and workers), but in a majority of cases the estimates are rather close (e.g. commercialized state, managers, and foreigners). Statistical tests identify workers and outsiders as significantly more effective in the non-CIS countries, while banks and concentrated individual ownership are significantly more effective in the CIS.[29] For the other six ownership types, there are no significant differences between regions.

There is the germ of an institutional story in these results, although our discussion here turns more interpretive than analytical and the reader should be cautioned that the next paragraphs are more conjectural than the ones that have gone before. 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

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[28.] It bears repeating that all these effects are relative to traditional state ownership.

[29.] These four owners constitute the only set that satisfy the following condition: imposing the restriction that all owners not in the set have the same effect in both regions, each owner in the set has a significantly different effect between regions.

proposed by Fox and Heller (1999) and Coffee (1999), that corporate governance institutions functioned less well in the CIS than elsewhere.

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 60% higher in the CIS even after the obvious outlier of banks is excluded. Evidence is also found by running regression (11) separately for the two regions. When this is done, the adjusted R-squared is 0.59 for the CIS and 0.12 for non-CIS: differences between the effectiveness of different types of owners are much more stark and much more consistent in the CIS than elsewhere.[30] 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

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[30.] Note that, for practical purposes, (11) was transformed so as to have a constant term when obtaining estimates. So the R-squares are meaningful ones.

privatization they retained the reigns of power.[31] 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 that occurs in an environment where workers would find it tough to challenge managers.

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, discussed in the transition literature, is that private owners are better at selecting managers who can run the firm efficiently. This is because managers of state-owned enterprises are chosen on the basis of their ability to lobby the government on behalf of the enterprise. In contrast, managers of privatized enterprises are selected for their ability to operate in a market environment. The hypothesis that management turnover—or more broadly, bringing in new human

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

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. Some theoretical models on the early transition suggested 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 (Aghion et al., 1994; Aghion and Blanchard, 1996). 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.

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 event study literature in the U.S. finds that the stock price of companies goes up on the news of the sudden death of a long-serving CEO (Johnson et al., 1985). The improvement in corporate performance associated with management changes may occur, however, 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 an analogous simultaneity 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 top management ownership is likely to remain small and where management turnover as a result of shareholders' action is large, even in the absence of mergers or acquisitions that may bring about a change in ownership. This in turn weakens the general applicability of this policy prescription.

The data set in Claessens and Djankov (1999a) is, in contrast, 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, as compared to developed countries, 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.[32] 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 better enterprise restructuring. This is interesting as the selection was made by State Ownership Fund officials,

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[32.] 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.

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 holdings of general managers have 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.

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 has 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) 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). 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 6 studies with 25 separate analyses, testing the importance of both

managerial turnover and managerial incentives in restructuring.[33] (Construction of Panel A employs the

methods used for Table 1.) Using different weighting factors, we find that in all cases turnover and

incentives, considered together, are an important determinant of restructuring. The pertinent t-statistics

have values between 2.30 and 6.55. Management turnover on its own also has a significant effect on

restructuring. But manager incentives are not significant.

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 ten 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

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[33.] The Pinto et al. (1993) study does not contain regression results that can be used in the table.

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 rather an application of the old adage that a hanging concentrates the mind.

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 sometimes 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.

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 may still maximize the expected value of funds that can be 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 for refinancing firms with problems in servicing old debts, as the marginal return to new loans may be large enough to compensate 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 effort of the 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 or high

marginal returns for new loans. 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) show 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 that the banking sector is in central state hands. He finds that trade arrears are not a major channel of soft financing, since on average they comprise a payment period of 2 months. 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 (1999) 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 7 papers that use regression analysis to answer these questions. The data come from Bulgaria (Claessens and Peters, 1997; Djankov and Hoekman, 2000), Romania (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 2000). They generally cover the period between 1992 and 1996, and contain 24 separate analyses.

Using the methodology developed in Section 2, Table 6, Panel A, shows that the effect of hardened budgets on enterprise restructuring (defined as sales growth, TFP growth, labor productivity growth) is

very significant in non-CIS countries and generally insignificant in CIS countries. For non-CIS countries, the t-statistics vary between 2.13 and 13.88, while for CIS countries the statistics are in the interval 0.95 to 2.12. The positive signs imply that hardened budgets have a beneficial effect on restructuring. A possible explanation for this result is documented by the McKinsey Global Institute: politicians often complement soft budgets with barriers to competition from imports or new local entry. In such circumstances, an enterprise can show higher, somewhat spuriously so, labor productivity as it captures or keeps a large share of the market. 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) will 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 find that labor shedding was reduced by 4.6% during 1993-1995. Claessens and Djankov find a 2.7% unrealized TFP-growth as a result of continued soft financing in the Eastern European countries. Djankov and Hoekman (2000) find 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. (1997) show that recipients of soft financing record labor productivity growth that is a 6% less than that of non-recipients. The results from the various studies are summarized in Table 7.

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. (1997) show that the probability of receiving state support is 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 almost 70% less likely to serve as a channel of soft budgets to the ailing enterprise sector (World Bank, 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, 2000).

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 of governments to continue financing 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 the 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.

Trade liberalization programs in developing countries in the 1980s and the early 1990s have generated enterprise-level data that allow tests of the imports-as-market-discipline hypothesis. 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.

A second group of studies uses the dynamics of entry and exit across sectors as a measure of the effect of imports-as-market-discipline. Roberts (1996) finds some evidence for a positive effect of import

competition on increased turnover in Colombia, but only in highly concentrated industries. Liu (1993) finds support for the theory in a sample of Chilean firms.

A third group of studies focuses on changes in industry mark-ups (price-cost margins) in countries that have undergone a recent trade liberalization. A reduction in mark-ups is interpreted as a sign that increased competitive pressure has induced managers to improve efficiency. Levinsohn (1993) finds weak support for the imports-as-market-discipline hypothesis in Turkey. MacDonald (1994) finds that import penetration has positive effects on productivity growth only in concentrated industries.

Finally, another group of studies focus on competition in the domestic product market as a determinant of corporate performance. Two studies of British manufacturing firms (Nickell, Wadhwani and Wall, 1992; Nickell, 1996) use a panel framework to show that market concentration has an adverse effect on the level of total factor productivity. By the nature of their estimation, this also implies that an increase in market concentration is followed by a fall in productivity. In contrast, Blanchflower and Machin (1996) find no effect of changes in domestic market structure on the productivity of UK plants.

The initial period of transition from central planning to market 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., 2000). 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 13 studies that explicitly investigate the effect of product market competition on enterprise restructuring. Among those, 8 studies focus on non-CIS countries (Claessens and Djankov (2000); Djankov and Hoekman (2000); Djankov and Hoekman (1998); Hersch, Kemme, and Bhandari (1994); Halpern and Korosi (1998); Konings (1997b); Konings (1998); Li (1997)) and 5 studies use data for

either Russia (Brown and Brown, 1998; Earle and Estrin, 1998; Brown and Earle, 2000), Georgia (Kreacic, 1998), or Mongolia (Anderson, Lee, and Murrell, 2000). We are able to distinguish 63 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 thirty-eight studies focus on the effects of domestic market structure (Table 8). 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: 25 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 by 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 8, again based on the methods employed in producing Table 1, has t-statistics varying between 6.98 and 10.60 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 each is significant in explaining enterprise performance.

Examining the effects of competition in each of the regions, Table 8 shows that the effects are strong for non-CIS countries, where in all cases the t-statistics are significant at conventional levels. For CIS countries, increased competitive pressures are associated with enhanced restructuring, but the effect is not generally statistically significant. In particular, the unweighted t-statistic and the t-statistic weighted by the indicator of the extent of control variables used in the regression analysis and the attempts to handle selection bias are insignificant, with values of 0.99, 0.82, and 1.29, respectively.

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.[34] In contrast, import competition is always very significant in explaining enterprise restructuring in the non-CIS sample. Since Brown and Brown (1998) 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 industries.

[34.] One possibility, as indicated by the results in column (4), is that successful handling of selection bias is necessary to obtain significant results for import competition in CIS countries. However, successful handling of selection bias does not seem to be a critical factor on other rows of the table.

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 8.

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 the remainder accounted for by better managerial incentives.

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 from imports.

Table 9, based on the methodology used for Table 2, examines the statistical significance of the variation in the effect of product market competition across regions and across types of competition. In the first panel, we show competition from local producers has a stronger effect than import competition but the difference is only statistically significant in column 5. 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 generally 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.[35] 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 more 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

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[35.] For a fuller history of the ebb and flow of policies in the first decade of transition, see Clement and Murrell (2000). The essays in Clague and Rausser (1992) constitute an exception to the early lack of focus on institutions.

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.[36]

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

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 (1998) and Konings (2000) 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 (2000) finds support for

the Blanchard-Kremer hypothesis for a sample of old firms, whereas the effect is not present in de novo

firms. This results suggests that weak institutions are not the central problem, since these institutions

apply to enterprises old and new. Instead, the breakdown of old relationships, the destruction of

information and relationships, might be the critical factor that produces 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

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[36]. 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.

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.[37] 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, 1999, 2000). These studies show that members of business associations are more likely to undertake restructuring activities 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 when credit markets function poorly (Perotti and Gelfer 1999). Hendley, Murrell, and Ryterman

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[37.] 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).

(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). 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 similar 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 same studies find enterprises (excepting the Vietnamese ones) using the courts frequently and rating their effectiveness quite highly. The overall picture, then, does not suggest the extreme failure of formal contract enforcement institutions and heavy reliance on extra-legal methods of enforcement that had sometimes been suggested during the early transition (Greif and Kandel, 1995).

The more usual way in which criminal groupings are expected to affect businesses is when such groupings wield 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 (1999c) 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 (2000) 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 (1999c) estimate that firms perceiving property rights to be insecure invest nearly 40 percent less than firms that perceive 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.[38]

The reform of corporate governance was at the heart of the early institutional reforms that were 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 (1999) 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

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[38]. 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 (2000) 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. Johnson and Shleifer (2000) 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 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.

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.[39] 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. In examining the implementation of enterprise transactions, Hendley, Murrell, and Ryterman (2000) 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. 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. find in Russia, echoing Kranton's (1996) theoretical results. Similarly, Jin and Qian (1998) and Che and Qian (1998) show in theory and practice

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[39] 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.

(in China) that local government ownership might be superior to private ownership, when the legal system has no power to control a predatory 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. This study also shows 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 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 the 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 much 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 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, 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 82 analyses appearing in 31 studies

Regions	Type of dependent variable in the analyses	Number of analyses contributing to the composite test statistic	Weighting Method Used to Obtain the Composite Test Statistic					
			(1)	(2)	(3)	(4)	(5)	(6)
			None	Subjective rating of quality	Extent of controls (vector X)	Attempts to handle selection bias	Inverse of number of analyses used from same study	Years of privatization covered by the analysis
			Normally distributed composite test statistics					
All countries	Both	82	12.79	9.38	12.23	12.26	11.72	11.76
Non-CIS	Both	34	15.42	14.13	16.32	14.23	15.44	14.10
CIS	Both	48	3.74	-0.33	3.50	2.95	0.06	1.91
All countries	Quantitative	55	11.49	7.96	11.16	10.04	11.58	10.45
Non-CIS	Quantitative	31	15.00	13.84	15.90	13.85	15.13	13.77
CIS	Quantitative	24	0.36	-3.96	0.20	-2.55	-1.82	-2.17
All countries	Qualitative	27	5.89	5.04	5.49	7.10	3.38	5.39
Non-CIS	Qualitative	3	3.71	3.49	3.71	3.71	3.39	3.71
CIS	Qualitative	24	4.93	4.56	4.75	6.51	2.06	4.84

Table 2: Comparing the Size of Privatization Effects Across Regions
  Partial correlation coefficients and tests statistics for regional differences derived from 82 analyses from 31 studies

Type of dependent variable in the analyses		Number of analyses used for composite partial correlation	Weighting Method Used					
			(1)	(2)	(3)	(4)	(5)	(6)
			None	Subjective rating of quality	Extent of controls (vector X)	Attempts to handle selection bias	Inverse of number of analyses in same study	Years of privatiz- ation
			Partial correlation coefficients and test statistics on their difference					
Both	Partial correlation for Non-CIS	34	0.090	0.083	0.090	0.083	0.084	0.093
Both	Partial correlation for the CIS	48	0.039	0.015	0.037	0.045	0.030	0.035
Both	Test statistic for difference between partial correlations		4.10	4.99	4.46	2.86	4.18	4.39
Quantitative	Partial correlation for Non-CIS	31	0.084	0.081	0.086	0.080	0.078	0.091
Quantitative	Partial correlation for the CIS	24	0.016	-0.024	0.016	-0.002	0.021	0.004
Quantitative	Test statistic for difference between partial correlations		4.11	5.36	4.23	4.51	3.64	5.14
Qualitative	Partial correlation for Non-CIS	3	0.146	0.145	0.146	0.146	0.147	0.146
Qualitative	Partial correlation for the CIS	24	0.062	0.062	0.059	0.089	0.041	0.065
Qualitative	Test statistic for difference between partial correlations		2.01	1.87	2.09	1.36	2.31	1.93

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
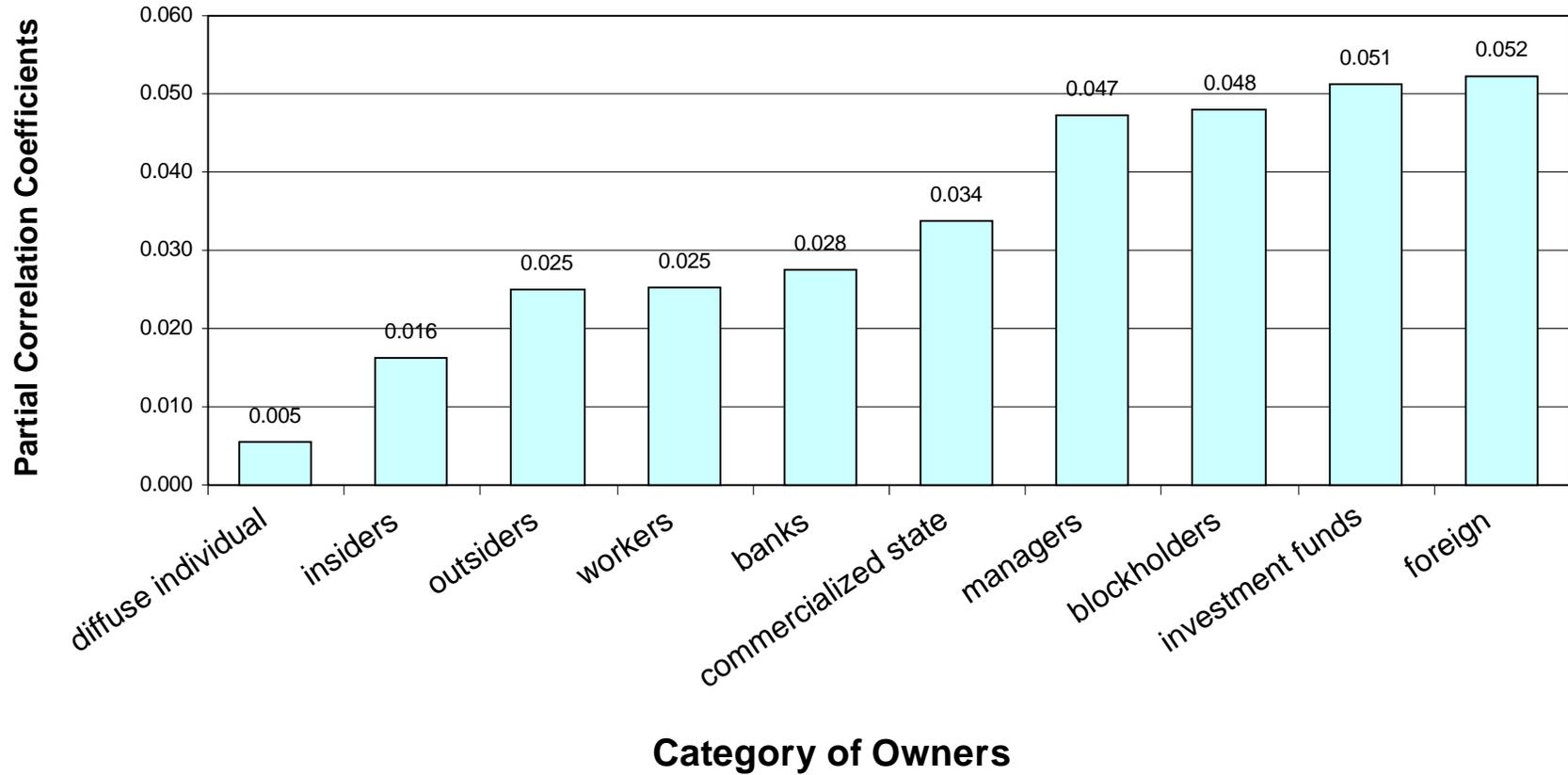
Table 4: Testing Differences between the Effects of a Variety of Owners
  Statistics testing differences between owners derived from 305 pairwise comparisons of owners taken from 23 studies

	Traditional State	Diffuse Individual	Insiders	Outsiders	Workers	Banks	Commercial-ized State	Managers	Concentrated Individual	Investment Funds
	t-statistic for {(effect of owner listed on row) minus (effect of owner listed on column)}									
Diffuse Individual	1.11									
Insiders	2.97	2.63								
Outsiders	3.19	4.17	1.85							
Workers	3.40	2.95	1.38	0.05						
Banks	4.05	3.36	1.86	0.36	0.28					
Commercialized State	6.29	6.02	5.48	1.69	1.33	1.08				
Managers	6.34	5.99	4.87	3.00	2.94	2.52	2.20			
Concentrated Individual	6.64	12.66	9.01	5.02	3.63	3.46	3.70	0.11		
Investment Funds	6.70	7.68	6.04	3.97	3.41	3.55	3.05	0.52	0.61	
Foreign	5.99	13.77	7.71	5.88	3.96	3.67	3.47	0.69	1.23	0.17

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 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 Eastern Europe and the Baltics
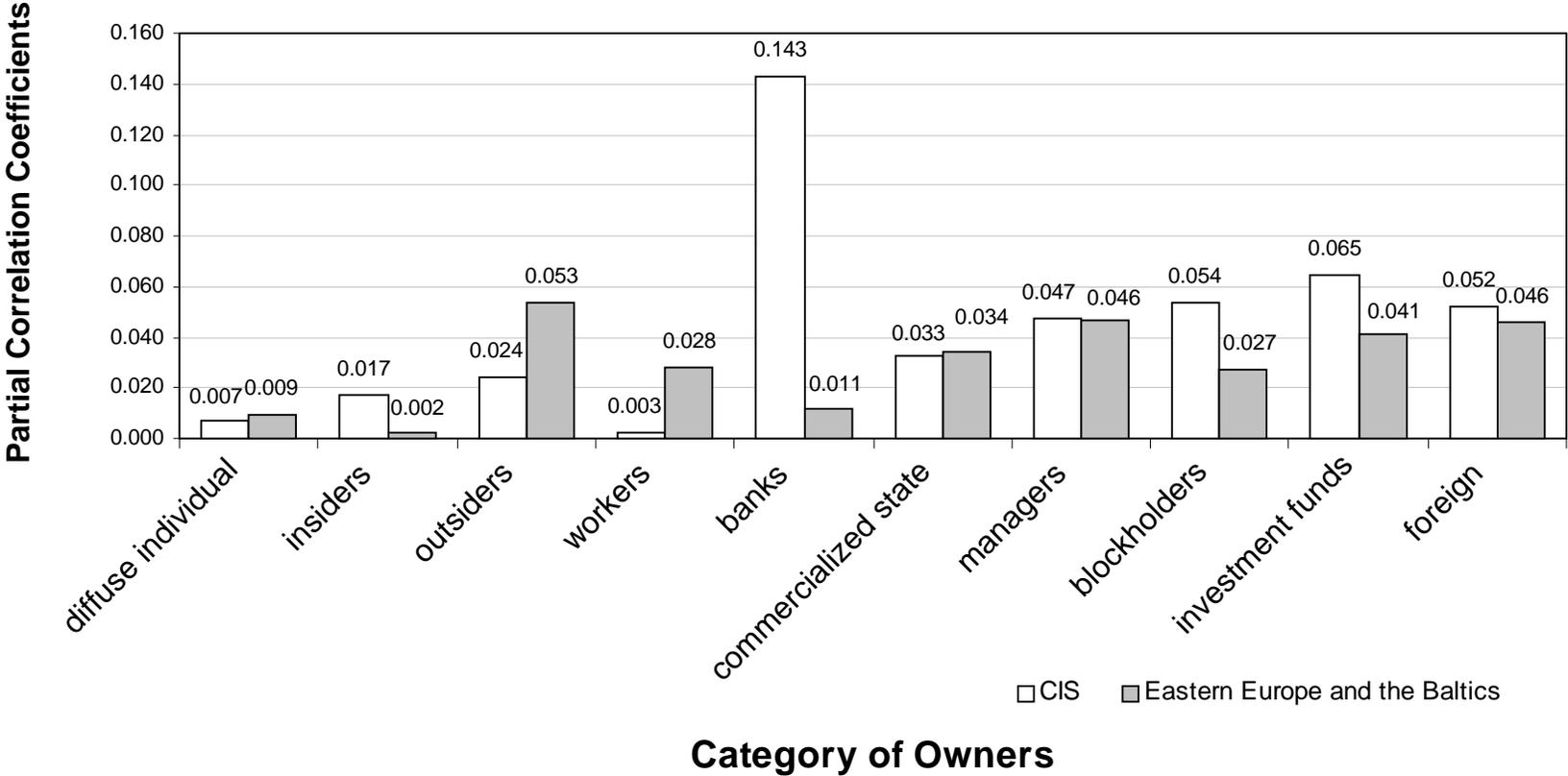
Table 5: The Role of Managerial Turnover and Managerial Incentives in Enterprise Restructuring
     Statistics derived from 25 analyses appearing in 6 studies

		Weighting method used					
		(1)	(2)	(3)	(4)	(5)	(6)
	Number of analyses contributing to the composite effect	None	Subjective rating of quality	Extent of controls (vector X)	Attempts to handle selection bias	Inverse of number of analyses in same study	Years of reform covered by the analysis
A. Do Managers Matter?				Composite normally distributed statistics			
Both turnover and incentives	25	5.14	5.86	2.69	6.55	3.79	2.30
Management turnover	14	8.38	8.73	8.23	8.58	6.54	5.02
Management incentives	11	-1.69	-1.09	-3.60	-0.46	-2.74	-2.46
B. Comparison of Significance				Partial correlation coefficients and test statistics on their difference			
Management turnover	14	0.09	0.10	0.10	0.10	0.08	0.08
Management incentives	11	0.00	0.00	-0.01	0.02	0.00	-0.01
test of difference		4.79	4.65	5.79	4.68	4.67	4.24

Table 6: The Importance of Hardening Budget Constraints in Enterprise Restructuring
      Statistics derived from 24 analyses appearing in 7 studies

Regions	Number of analyses contributing to the composite test statistic	Weighting Method Used					
		(1) None	(2) Subjective rating of quality	(3) Extent of controls (vector X)	(4) Attempts to handle selection bias	(5) Inverse of number of analyses used from same study	(6) Years of reform covered by the analysis
A. Testing the effects of hardening budgets		Composite normally distributed statistics					
All countries	24	10.19	6.23	12.46	11.83	2.97	4.14
non-CIS	19	10.70	6.76	13.83	12.87	2.13	4.07
CIS	5	1.48	1.21	1.24	0.95	2.12	1.10
B. Comparing the effects between regions		Partial correlation coefficients and test statistics on their difference					
non-CIS	19	0.04	0.03	0.06	0.05	0.01	0.02
CIS	5	0.06	0.05	0.05	0.04	0.00	0.04
test of difference		-0.43	-0.51	0.22	0.30	1.84	-0.63

Table 7: The Losses from Soft Budgets (per annum)

The table below shows the losses in performance of an enterprise with soft budgets compared to an enterprise with hard budgets.

Forgone Labor Shedding		Forgone Total Factor Productivity Growth		Forgone Labor Productivity Growth		Forgone Profits (as a % of sales)	
Estimate	Country	Estimate	Country	Estimate	Country	Estimate	Country
8.9%	Romania	2.7%	Eastern Europe	5.7%	Russia	14.4%	China
4.0%	Eastern Europe	3.0%	Bulgaria	6.2%	Russia	23.5%	Romania
4.6%	Romania	3.46	Russia				

Table 8: The Effect of Competition on Enterprise Restructuring
      Statistics derived from 63 analyses appearing in 13 studies

Sample	Number of analyses contributing to the composite test statistic	Weighting Method Used to Obtain the Composite Test Statistic					
		(1)	(2)	(3)	(4)	(5)	(6)
		None	Subjective rating of quality	Extent of controls (vector X)	Attempts to handle selection bias	Inverse of number of analyses used from same study	Years of reform covered by the analysis
		Composite normally-distributed statistics					
All countries, all competition	63	6.98	9.08	6.86	6.64	10.07	10.60
Non-CIS countries, all competition	44	7.70	8.81	7.72	7.30	9.60	10.89
CIS countries, all competition	19	0.99	2.71	0.82	1.29	4.29	2.04
All countries, import competition	23	3.30	4.11	3.16	2.42	4.04	3.55
All countries, domestic market structure	38	3.81	4.16	3.71	3.94	5.52	3.83
Import competition, non-CIS	15	4.77	4.66	4.77	3.95	4.22	4.57
Import competition, CIS	8	-0.93	-0.19	-1.33	3.68	0.87	-0.04
Domestic market structure, non-CIS	27	3.19	2.80	3.05	3.12	3.31	2.82
Domestic market structure, CIS	11	2.10	3.48	2.10	2.41	4.42	2.73

Table 9: The Relative Effect of Different Types of Competition in Different Regions
       Statistics derived from 61 analyses appearing in 13 studies

Sub-samples	Number of analyses contributing to the composite test statistic	Weighting Method Used					
		(1)	(2)	(3)	(4)	(5)	(6)
		None	Subjective rating of quality	Extent of controls (vector X)	Attempts to handle selection bias	Inverse of number of analyses used from same study	Years of reform covered by the analysis
		Partial correlation coefficients and test statistics on their difference					
Import competition	23	0.01	0.01	0.01	0.00	0.02	0.00
Domestic market structure	38	0.03	0.03	0.03	0.03	0.06	0.02
test of difference		-0.98	-0.16	-1.11	-1.19	-2.23	-1.51
All competition, non-CIS	44	0.06	0.07	0.06	0.06	0.08	0.10
All competition, CIS	19	-0.03	0.00	-0.03	-0.03	0.04	-0.15
test of difference		4.19	3.52	3.87	3.97	1.66	6.41
Import competition, non-CIS	15	0.05	0.04	0.05	0.06	0.04	0.04
Import competition, CIS	8	-0.08	-0.07	-0.09	-0.08	-0.04	-0.07
test of difference		3.65	3.55	3.64	3.58	2.98	3.65
Domestic market structure, non-CIS	27	0.03	0.03	0.04	0.04	0.03	0.03
Domestic market structure, CIS	11	0.00	0.04	0.01	0.01	0.09	0.00
test of difference		1.15	-0.29	0.80	0.83	-1.82	1.02