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Monitoring Managers: Does it Matter?

Abstract

We test under what circumstances boards discipline managers and whether such interventions improve performance. We exploit exogenous variation due to the staggered adoption of corporate governance laws in formerly Communist countries coupled with detailed "soft" information about management actions, competence, and board interventions in 442 private-sector companies backed by private equity funds between 1993 and 2008. We find that when given the legal power to do so, boards fire CEOs in response to poor performance, especially if they hold the CEO responsible for the poor performance or have come to view him as incompetent. Consistent with agency models, we find that CEOs are not fired when performance deteriorates due to factors deemed explicitly to be beyond their control. Following forced management turnover, companies see performance improvements and their investors are considerably more likely to eventually sell them at a profit. Shareholder-friendly laws appear to lead to improved performance only indirectly, by making it easier for boards to fire underperforming or incompetent managers, rather than by increasing the threat of dismissal.

Key words: Corporate governance, large shareholders, boards of directors, CEO turnover, legal reforms, transition economies, private equity.

JEL classification: G34, G24, G32, K22, O16, P21.

Over the past two decades, regulators and stock exchanges in many developed countries have introduced corporate governance codes on the assumption that an active board of directors can help improve a company's performance. (For relevant surveys, see Shleifer and Vishny (1997) and Becht, Bolton, and Roëll (2003).) This assumption is a mainstay of the theoretical principal-agent literature on large shareholders and management incentives (surveyed in Tirole (2006)), and it finds indirect support in a large number of empirical studies. For example, stock markets tend to react favorably when activist investors buy stakes in a company (see Gillan and Starks (2000) and Brav et al. (2008)), presumably because investors foresee an improvement in performance.

We test the main predictions of principal-agent theory by examining under what circumstances boards discipline managers and whether such interventions improve performance. In broad strokes, the predictions are as follows. Suppose performance is a function of the agent's ability a, his costly effort e, and a stochastic term ε. A principal who cannot directly observe effort e should fire the agent following bad performance, even if the agent wasn't to blame (i.e., a bad draw ε). The principal can improve on this situation by actively monitoring the agent (see Tirole (2006) for a survey). This enables him to better distinguish between effort and bad luck. An active monitor will fire the agent only if poor performance is likely to have been caused by the agent's actions, e. This, in turn, improves the agent's incentive to expend costly effort. Active monitoring may also enable the principal to learn the agent's ability, a, enabling him to replace an incompetent manager.

Testing these predictions requires detailed data on the principal's information, his actions, and the motivation behind them. Typically, we observe none of these. Indeed, a recurring problem in the literature on CEO turnover is that we usually cannot distinguish between forced and voluntary departures, let alone know why a CEO was fired or how the board viewed his ability and effort. Using a unique dataset obtained from the European Bank of Reconstruction and Development (EBRD), we are able to overcome these data problems. Our results suggest that principal-agent theory describes the behavior of boards quite well, at least in our dataset.

The natural follow-on question is whether monitoring matters, that is, whether intervention by the principal improves performance. While it may seem obvious that it would, actually showing this empirically is quite challenging. We cannot observe the counterfactual, that is, what performance would have been had the principal not intervened. To identify the "treatment effect", researchers instead typically compare the performance of companies with interventions to that of those without. This raises a potential selection problem: Boards presumably intervene in badly managed or poorly performing companies. Thus, a comparison of companies with and without interventions likely picks up unobserved differences in the quality of management or the company, not just the effect of an intervention. In the cross-section, we might even find that companies in which boards have intervened still underperform their peers, but of course such underperformance would not have been *caused* by the intervention.

Our evidence shows that board interventions have a substantial, positive effect on performance. To solve the identification problem, we exploit a natural experiment that occurred as a result of the transition from centrally-planned to market economies after the fall of the Soviet Union: Since 1991, most transition countries have replaced Soviet-era with Western-style corporate law, in the process strengthening corporate governance and, especially, the powers of the board relative to management. We use the staggered adoption of such laws across countries as an instrument for board intervention.[1] One benefit of focusing on transition economies is that they experienced dramatic variation in their laws. In developed countries, in contrast, there is relatively little variation in the laws governing board actions.[2]

Our sample consists of 442 privately-held companies in 17 countries in Central and Eastern Europe and the former Soviet republics in Central Asia which between 1993 and 2005 received

[1] Staggered law changes are widely used as an instrument in empirical research. For an example that exploits variation in the adoption of anti-takeover legislation across U.S. states, see Bertrand and Mullainathan (2003).

[2] In other words, we exploit variation along the extensive margin as transition economies adopt corporate governance laws for the first time. In developed countries, by contrast, empirical work focuses on variation along the intensive margin. The 2002 Sarbanes-Oxley Act, for example, made boards in the U.S. somewhat more independent.

equity funding from 43 private equity funds backed by the EBRD.[3] The funds take (large) minority stakes in their portfolio firms, similar to venture capitalists but unlike buyout funds in the U.S. Thus, while they are large shareholders, and so well placed to perform an active monitoring role, as predicted by principal-agent theory, they cannot dismiss management at will. Instead, fund managers exercise their influence mainly through their presence on the board of directors.

The EBRD "audits" each fund's portfolio once or twice a year. For each portfolio company, EBRD staff collect detailed information about performance, material events, board decisions, management quality, etc. from fund reports, audited company financials, and site visits, and through the EBRD's representation on the funds' advisory boards or investment committees. Audits result in so called monitoring reports which we have been given unrestricted access to.

The EBRD's "soft" information collection helps mitigate the data problems discussed earlier. For example, we know if a fund manager blamed a firm's poor performance on actions taken by management or on external developments beyond management's control ("bad luck"); whether the board viewed a CEO as incompetent; and when and why a CEO was fired. Moreover, the EBRD records a portfolio firm's performance relative to the budget negotiated between management and the board at the beginning of each year. This gives us a measure of the board's expectations of a firm's performance for the following year.

Our identification strategy assumes that boards are more likely to intervene after the adoption of a law empowering the board to dismiss a manager than before. Empirically, we find that CEO turnover increases substantially, from close to zero to nearly 10% a year, when corporate governance laws are adopted. This not only supports our instrument but is interesting per se. It suggests that a legal framework that supports shareholders can make active monitoring more effective. This is in line with the law and finance literature, but while that literature has mainly

[3] The EBRD was established to assist countries in transitioning to a market economy. Accordingly, the EBRD invested in most commercial private equity funds with a focus on the region, reducing sample selection concerns. Sample funds' other investors are institutional investors (such as endowments), so funds have standard profit-maximizing objectives.

argued this point through cross-country comparisons, we can exploit within-country variation to identify the channel directly.

As predicted, we find that boards are more likely to fire senior (but not junior) managers following poor performance relative to the board's expectations. This echoes prior findings in the CEO turnover literature.[4] Furthermore, we find that managers are especially likely to be fired when evidence has mounted that they are incompetent or they are blamed for poor performance, consistent with active monitoring. However, as suggested by Holmström (1982), CEOs are not fired for simply being unlucky. These findings provide direct evidence in support of agency theory.

Does monitoring matter? Because our sample companies are private, we cannot measure performance using share prices. Instead, we follow the VC literature (e.g., Gompers and Lerner (1998, 2000)) which measures performance using "exit" events, i.e., the sale of a portfolio firm, either to a strategic acquirer or through an IPO. Exits are viewed as successful investment outcomes (the alternative is a write-off). Our data allow us to refine this performance measure by conditioning on the profitability of the exit: A sale is an exit, but not necessarily a profitable one. Thus, we also define as a successful outcome an exit at a positive internal rate of return (IRR).

When we naïvely relate performance to board intervention in the cross-section, we find that intervention is significantly *negatively* related to the probability that an investment is exited. This provides evidence of the selection bias discussed earlier. However, once we use the law changes as an instrument, the coefficient flips sign. The point estimate is not only statistically significant, it is economically large. Firing the CEO increases the probability of exiting over the next two years by 52.8 *percentage points* from the unconditional probability of 43%, while the probability of a profitable exit improves from 31.3% to 95.5%. Similarly, using the EBRD's annual performance evaluation data, we find that CEO turnover is followed by a 1.41-point improvement in

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[4] See Coughlan and Schmidt (1985), Warner, Watts, and Wruck (1988), Weisbach (1988), Kim (1996), and Fee and Hadlock (2003), who relate CEO turnover to prior stock price performance, and Denis and Denis (1995) and Huson, Malatesta, and Parrino (2004), who relate CEO turnover to operating performance.

performance relative to target the next year, on a scale from 1 ("severe underperformance relative to expectations") to 5 ("performance greatly above expectations"). Thus, interventions improve both short-run performance and the chances of a successful investment outcome.

What is the channel? In principle, the law changes could operate on both the extensive and the intensive margins: Legal reform makes it easier for the board to dismiss a low-quality manager, thus improving performance (the extensive margin); and they may increase the *threat* of dismissal, causing managers to raise their game regardless of the board's actual actions (the intensive margin). Our evidence suggests that the intensive margin is much less important in our data.

For our identification strategy to work, law changes must trigger intervention but have no direct effect on performance. There are three obvious ways in which this exclusion restriction might be violated. First, the law changes may have coincided with other beneficial country-level shocks that in turn affected performance. The staggered adoption of the laws across countries mitigates this concern, though we also include controls for macroeconomic conditions and progress towards a market economy to control for other contemporaneous shocks. Moreover, we can show that it is corporate governance reforms specifically rather than the adoption of Western-style commercial law more generally that matters. Finally, we find no evidence that company-level performance affected the timing of reforms, which casts doubt on lobbying and other reverse-causality stories.

Our results are robust across a wide range of alternative specifications. They are similar whether we focus on the CEO or top management more generally and when we allow for heterogeneous treatment effects across countries and unobserved time-invariant portfolio-firm effects (which help control for the fact that we do not observe CEO ownership or compensation contracts). The results also hold for sub-periods, sub-groups of countries, and for alternative exit horizons.

This paper contributes to the economic literatures on agency, corporate governance, law and finance, VC, and economic development. Our results show that the three main variables in principal-agent models – ability, effort, and bad luck – play their expected roles in management turnover. They also illustrate that monitoring matters, in the sense that it helps boards learn about managers' ability or observe their effort, rather than relying solely on a noisy output measure (i.e., performance). This supports models of active monitoring.

Our results provide rare direct evidence of the impact active boards can have on corporate performance. In our sample, performance improves dramatically when boards are given the legal power to monitor and discipline management, and it appears that the channel is actual intervention rather than the mere threat of it. This suggests that a shareholder-friendly legal framework is necessary but not sufficient to ensure good corporate governance; what is also needed is a board that is prepared to take action.

Much of the VC literature focuses on the positive side of investor activism. Bottazzi, Da Rin, and Hellmann (2008), for example, show that VCs improve the performance of their portfolio firms by helping to recruit board members or arranging follow-on funding. Puri and Hellmann (2000, 2002) and Acharya, Hahn, and Kehoe (2008) provide related evidence. Kortum and Lerner (2000) study VCs' beneficial effect on innovation, addressing causality concerns using a 1979 policy change that made it easier for pension funds to invest in venture capital. We add to this literature by focusing on the *disciplinary* role of boards, which is the focus of much of the theoretical principal-agent literature.

The development literature has demonstrated the importance of the law and institutions for a country's macroeconomic growth. We provide parallel microeconomic evidence by showing that corporate governance laws have a strong influence on corporate performance, at least in the presence of sophisticated foreign investors, such as the private equity funds in our dataset.

1. Empirical Model

1.1 Identification and Estimation

We seek to estimate the conditions under which a board (the principal) intervenes in a company by firing a manager (the agent) as well as the effect such an intervention has on the company's performance. We observe companies annually, from the year they first receive funding from a sample fund to the year they are exited or written off. We also observe whether and when the board intervenes. Thus, we relate intervention in year *t* to the company's lagged performance and the board's assessment of the manager's actions and competence and the influence of bad luck:

$$intervention_t = \delta_1' x_{1,t-1} + \varphi_1'(\lambda_k + \lambda_t + \lambda_j) + u_{1,t} \tag{1}$$

where the *x*'s include additional exogenous controls and the λ's are fixed effects for country *k*, calendar year *t*, and industry *j*. (In some specifications, we also include portfolio-firm effects.) To test whether intervention in turn improves performance, we estimate the following equation:

$$performance_t = \beta_2 intervention_t + \delta_2' x_{2,t-1} + \varphi_2'(\lambda_k + \lambda_t + \lambda_j) + u_{2,t} \tag{2}$$

While equation (1) is straightforward to estimate as long as we have good data for the key principal-agent variables, equation (2) is trickier. It is likely that the disturbances of the intervention and performance equations are correlated. Presumably, boards intervene when companies are badly managed or perform poorly and such companies will presumably underperform better-managed companies in future (even if they perform better than they would have done otherwise). Unless we have perfect controls for the quality of management and for performance, the disturbances are likely negatively correlated, so a naïve regression of performance on intervention will result in a downward biased estimate of β_2.

If so, identification requires that x_1 contain an instrument, i.e., at least one variable not included in x_2. The binary nature of both dependent variables (we observe whether or not a board intervenes and whether the company is exited rather than written off) poses no particular problem (see Maddala (1983, p. 118)). As long as it is identified, Greene (1998) shows that equation (2) can be consistently and efficiently estimated using a seemingly unrelated bivariate probit.

1.2 Instrument

Our instrument exploits plausibly exogenous variation in the timing of the introduction of laws governing the relationship between management and the board in transition countries. The identifying assumption is that boards are more likely to intervene after the adoption of a law empowering them to dismiss a manager than before, and that the law change affects performance only via board intervention. Table 1 provides an overview of the applicable legal changes in our sample countries. The table also lists the source texts we consulted.

Because we focus on transition economies, the legal change we exploit is not a subtle one. The notion that the CEO serves at the pleasure of the board doesn't exist in Soviet-era laws. Following the collapse of the Soviet Union, the formerly communist countries of Central and Eastern Europe and Central Asia reformed their legal systems, and (with the exception of Azerbaijan) all countries in our sample eventually adopted Western-style corporate laws empowering the board to monitor and dismiss management.[5] Thus, when we follow a given firm over time, we expect the likelihood of intervention to jump once the country it is incorporated in strengthens the board's powers, all else equal. Figure 1 suggests that it does, which supports the validity of our instrument.

The three graphs in Figure 1 show the annual incidence of CEO, top-management, and any-management turnover, respectively, relative to the year the country in question changed its corporate governance laws. Top management refers to the chief executive (CEO) or chief financial officer (CFO), while any management includes other named executives. Prior to a law change, managers are rarely fired. CEOs, for example, are fired in between 0.9% and 3.7% of firms in the five years preceding a law change, and there is no obvious time trend. In the year of a law change, CEO turnover jumps to 8.2%, rising to 9.7% the year after, and staying above 6% a year for the next few years. The graphs for top-management and any-management turnover look similar.

Law changes thus appear to correlate with intervention, but do they also satisfy the exclusion

[5] In a 2005 report on Azerbaijan, the World Bank commented: "There are no detailed guidelines for the roles, responsibilities, operation, qualifications or structure of supervisory boards. In practice, boards tend to be dominated by the controlling shareholder, have not assumed an independent oversight function, and are considered to play a relatively minor role in providing strategic guidance for corporations. Regular supervisory board meetings are not held in most companies." See http://www.worldbank.org/ifa/rosc_cg.html.

restriction? In Section 4.2, we investigate three main ways in which law changes might affect performance directly (rather than via the board) but find no compelling evidence that they do.

2. Sample and Data

Since the collapse of communism, the EBRD has fostered the emergence of a professional private equity industry by investing in funds with an investment focus on transition economies that meet minimum due diligence standards. We have detailed data for 43 such funds, which have invested in 442 privately-held companies across 17 countries in Central and Eastern Europe and the republics of the former Soviet Union.[6] We estimate that the 43 funds the EBRD invested in account for around two-thirds of funds focusing on transition economies over our sample period.

Sample funds are private-sector investment partnerships with standard profit-maximizing objectives. They differ from those studied in the private equity literature only in that they have a regional focus on transition economies. Here is a typical fund description:

> "[The fund] provides capital for private, medium-sized companies with strong prospects for growth and profitability. Specifically, the fund will target companies that have: (i) a leading or prominent position within their industry, (ii) a good management team, [and] (iii) a large and growing market for their products and services."

Funds were raised between 1992 and 2004 and began investing in 1993. We ignore investments made after 2005, as their performance cannot yet be measured accurately. Table 2 provides a break down of the sample by country and year of initial investment. The five most active countries are Poland (with 166 investments), Russia (65), Hungary (52), the Czech Republic (44), and Romania (27). Seven countries – Azerbaijan, Georgia, Kazakhstan, Latvia, Moldova, Serbia-Montenegro, and Turkmenistan – are home to fewer than 10 investments each. Across all countries, the number of deals increases from 15 in 1993 to 62 in 2000 and then falls to a low of 14 in 2005.

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[6] The EBRD invests in funds meeting the minimum due-diligence requirements established by a firm of investment consultants, Cambridge Associates. For an overview of these requirements for funds targeting emerging economies, see http://www.ifc.org/ifcext/cfn.nsf/AttachmentsByTitle/Patricia+Dinneen/$FILE/Panel4-PatriciaDinneen.pdf.

Unlike most studies of VCs or private equity funds, we have detailed information on the status of portfolio firms. We thus know whether an investment has been written off or is still alive, something that can be hard to infer if only cash flows are observed. We follow each investment from inception to the earlier of the final outcome or Dec. 2008, when our data end. Of the 442 sample investments, 310 (70.1%) were fully exited through an IPO or a sale, 83 (18.8%) were written off, and 49 (11.1%) were still alive, or had only been partially exited, as of 2008.

Tracking each investment across time gives us an unbalanced panel. Accounting for the right-censoring caused by the 49 investments that remain alive as of 2008, the average (median) company spends 5.5 (5) years in a sample fund's portfolio before being exited or written off. This is comparable to holding periods in U.S. VC funds (see Gompers (1995)).

Our data include precisely dated cash flows to and from portfolio firms, including the fund's initial (and any subsequent) investment, dividends, and the proceeds (if any) from the company's IPO, sale, or liquidation. This allows us to compute the profitability of each investment. We measure profitability as the time-weighted return on investment, i.e., as the internal rate of return (IRR).[7] This averages 15.6%. Among the five most active countries, the highest average IRRs are in the Czech Republic (+28.2%), Poland (+22.8%), and Romania (+18.8%). Investments in Hungary (-0.7%) and Russia (0.7%) barely broke even on average. Interestingly, as Table 2 shows, Poland and Russia have similar exit and write-off rates while differing substantially in terms of IRRs. Thus, exits and IRRs do not correlate perfectly. The main factor contributing to this pattern is the skew towards higher IRRs ("home runs") in the distribution of Polish IRRs.

Sample investments come from a wide range of industries. The EBRD classifies firms into 11 industries: Telecoms and media (85 firms), manufacturing (68), high-tech, electronics, and internet (61), services, hotels, and restaurants (56), retail (46), food & beverages (43), construction (25),

[7] IRRs are computed before the fees a private equity fund charges its investors. In the 49 unexited cases, the IRRs are in part based on unrealized capital gains. In all other cases, they are based solely on cash flows.

financial services (19), oil, gas, and mining (19), pharmaceuticals and medical (15), and energy (5).

In the remainder of this section, we discuss the definitions of our variables.

2.1 Intervention Measures

Once or twice a year, EBRD personnel review the performance of each portfolio firm along both quantitative and qualitative dimensions. Reviews are recorded in monitoring reports which follow a standardized template. The reviews contain both 'hard' information, mainly in the form of accounting data, and 'soft' information concerning the company's progress relative to plan, key events and developments, the quality of the management team, perceived challenges over the next year, etc.[8] Here are two examples of soft information:

> "Amid the market decline, … producers dramatically increased their production capacity last year, changing the competitive dynamics of the … industry. This posed challenges that proved too big for [the] previous management team. The new management team … introduced at the beginning of 2006 prepared an action plan that addresses the problem head-on and should bring improvement in this year's financial performance. In our opinion, the newly appointed CEO … is moving the company in the right direction."
>
> "[The fund manager] continues to believe that [the company] has developed a track record and unique capabilities that should translate into growth through adding new projects. At the same time [the fund manager] has lost confidence in the ability of the current CEO to make this growth happen and to extract full value from the current business. [The fund manager] has identified a new CEO candidate from the power engineering sector who has strong sales skills and management experience in a variety of sales, project and general management positions. The new CEO is scheduled to start work mid-December."

Based on the monitoring reports, we identify as board interventions all cases where over the course of the reporting year, the board dismissed: a) the CEO; b) the CFO; c) a member of top management (i.e., either the CEO or the CFO, or both); d) any member of the management team including the CEO and CFO; or e) a junior member of the management team (anyone other than the CEO or CFO). We expect the strongest results in case a) and the weakest results in case e).

[8] While monitoring reports are standardized, reviewers vary in the level of detail they record. As long as assignments of companies to reviewers do not correlate with performance, this generates noise, not bias. This appears to be so. Each reviewer is assigned a group of funds (rather than a subset of a fund's investments, say the under-performing ones), and the main assignment criterion is that, if possible, she speak the language of the country the fund invests in.

Over our sample period, there were 131 instances of CEO dismissals (case a) and 55 CFO dismissals (case b). In 33 of these cases, both the CEO and the CFO were replaced at the same time, giving a net number of 153 cases where a member of top management was dismissed (case c). There were a total of 189 cases of management change (case d), including 74 dismissals of managers below the level of CEO or CFO (case e). The average (median) CEO dismissal takes place 3.4 (3) years after a fund first invested in a portfolio company. Most companies (344) have no forced CEO turnover; those that do typically fire only one CEO over our sample period (84 cases), though some fire two CEOs (11 cases) or even three (3 cases), a few years apart.

Furthermore, the monitoring reports record whether the board took actions to strengthen the management team, by hiring additional senior managers (say, a manager in charge of exports). There are 166 such cases in our data. We use this information to examine the plausibility of our instrument. We expect that CEOs rarely resist hiring, while they do resist being fired, so our instrument should correlate with CEO dismissals but less so with management strengthening.

2.2 Firm Performance Measures

We use the exit information shown in Table 2 to construct our first performance measure. Specifically, within the context of our unbalanced panel, we code an indicator equal to one in year t if the company is fully exited in years t through t+2. Exits are a popular performance measure in the academic literature; see, for instance, Gompers and Lerner (1998, 2000), Brander, Amit, and Antweiler (2002), Sorensen (2008), and Hochberg, Ljungqvist, and Lu (2007). We look up to two years out because the effect of intervention on the probability of exit need not be instantaneous; robustness results for other horizons are reported in Section 5.

As we saw in Table 2, exit rates and IRRs do not correlate perfectly. The average IRR among the 83 write-offs is -80.8%, with a range from -100% to -2.5%, while among exited investments, IRRs average 43.8% (median: 16.4%). Thus, write-offs are clearly a worse outcome than exits. However, 79 of the 310 exited investments have negative IRRs, indicating that some sales are fire sales. Thus, our second performance measure refines the simple exit indicator by coding as a successful exit only those fully-realized investments that were exited at a positive IRR.

Why not use the IRRs to measure performance directly? Essentially, in a private equity setting, there is only one meaningful IRR: The one computed when an investment is exited or written off. Positive interim cash flows (e.g. from dividends) are rare in private equity investments, so the IRR is typically constant at minus 100% until a company is sold. So, if we were to model IRRs, our panel would collapse into a single cross-section and we could not use our instrument, which relies on time variation in the legal environment relative to the date of the board intervention.

While IRRs generally do not vary over a company's spell in a private equity fund's portfolio, we can derive an interim performance score from the EBRD's monitoring reports. The reports contain information about a company's financial and strategic plans for the year and the EBRD reviewer's subsequent evaluation of performance relative to these plans. We use this information to score portfolio firms on a scale of 1 to 5, where 3 denotes performance in line with expectations (as set out in the company's budget for the year); 4 and 5 denote performance above and greatly above expectations; and 2 and 1 denote underperformance and severe underperformance relative to expectations. Here is an example of a company coded as a 2 (i.e., performing below expectations):

> "The company made major progress … but the prices negotiated … are lower than expected. … As a result, the company will not meet its 2002 budgeted sales and profitability."

Across the sample and period, the average score is 2.9 and the distribution looks fairly normal: 6.6% of investments score a 1, 34.7% score a 2, 31% score a 3, 22.3% score a 4, and 5.4% score a 5. Thus, boards appear to have realistic, achievable expectations which in turn form a suitable benchmark against which to judge a company's performance.

In Section 4.5, we use the interim performance scores as a third performance measure. We also include them, in lagged form, in the intervention equation, to test whether boards fire managers in response to poor firm-level performance.

2.3 Monitoring Proxies

The EBRD audit reports also contain information about the management team's overall competence and the fund manager's perception of what, if anything, may have caused poor performance. We use this information to code three time-varying monitoring proxies. The first is an indicator set equal to one if in a given year the fund manager views management as incompetent. The other two capture the fund manager's opinion about the causes of poor performance. Fund managers can either blame management or bad luck. Examples of the former are cases in which the CEO underestimated the time to completion of a certain project or focused on cutting costs at the expense of marketing. Examples of the latter are a fire destroying the firm's inventory or the firm's export earnings being hit by the 1998 Russian debt crisis. We expect boards to be more likely to dismiss incompetent managers and managers whom they view as responsible for poor performance. Bad luck, on the other hand, should not necessarily lead to dismissal.

Do the monitoring proxies correlate with true ability and effort, or do fund managers record concerns only when they have already decided to fire a manager? In Figure 2, we graph the annual incidence of fund managers blaming management for poor performance or calling management incompetent, where time is measured relative to the year in which the country in question reformed its corporate governance laws. If concerns are recorded strategically, we expect to see a structural break when laws are introduced that give boards the power to dismiss managers.

On average, managers are blamed for underperformance in 3.5% of portfolio firms and viewed as incompetent in 5.4%. Importantly, we see no structural breaks around the law changes, which suggests that fund managers form and record their views of management quality regardless of whether they have the legal power to act on them. In that sense, the monitoring proxies are likely to correlate with true ability and effort. It also suggests that by the time the law is changed, there is pent-up pressure to fire management, consistent with the jump in management turnover we saw in

Figure 1. This, of course, is the identifying assumption behind our instrument.

2.4 Deal Characteristics

We control for three deal characteristics. The first is the cost (i.e., size) of the investment. Converted using historical exchange rates, the average and median costs are €6.0 million and €4.0 million, respectively, ranging up to €43.4 million. The average and median "post-money" valuations are €63.5 million and €15.5 million, respectively.

The majority of our investments fund either expansion at young private-sector firms (233 out of 442) or start-ups (124 deals). Perhaps surprisingly, only 32 of the 442 portfolio firms are privatizations, in the sense that the fund acquired its stake from the government. Our sample is thus quite different from the types of companies studied in the transition-economics literature (see Djankov and Murrell (2002) and references therein), though we will control for privatizations.

It is important to control for risk because riskier companies likely require more intervention and may have systematically different performance. Traditional empirical risk proxies, such as the volatility of equity returns or operating cash flows, cannot be computed in our sample as portfolio firms are privately owned and because the accounting data contained in the monitoring reports have many gaps. Instead, we (crudely) proxy for risk based on the fund's investment behavior. Specifically, we code whether the fund "staged" an investment, that is, whether the fund made continued funding dependent on the firm's performance, as opposed to providing the entire investment capital upfront. Gompers (1995) argues that VCs stage investments to maintain the option to discontinue funding if performance disappoints. This option is more valuable, the riskier the project.[9] Of the 442 investments, 117 were staged and one has insufficient data to determine for sure and so is dropped from the analysis.

While we have data on a fund's ownership and the presence of other blockholders, we do not include these in our empirical model. Ownership is negotiated and so reflects not only observed but

[9] Of course, staging is endogenous so we will not interpret it causally. Instead, we treat it simply as proxying for risk.

also unobserved firm characteristics, which would necessitate an additional instrument.[10] Likewise, the presence of other blockholders is likely endogenous (see Hermalin and Weisbach (2003)).

2.5 Macroeconomic and Institutional Conditions

To rule out other influences that may happen to be contemporaneous with our instrument, we control for macroeconomic and institutional conditions. We use real GDP growth to proxy for macro conditions in a company's home country.[11] The data come from the EBRD.[12] Compared to most developed countries, transition economies have highly variable GDP growth rates. During the 1998 Russian crisis, for example, Russia's real GDP fell by 5.3%.

The second control measures a country's institutional conditions using the EBRD transition indicator.[13] The indicator measures overall reform progress towards a market economy in a range of categories, including privatization, restructuring, price liberalization, financial sector development, infrastructure, and competition. It varies from 1 (centrally planned economy) to 4.33 (fully functioning market economy) and is updated annually.[14]

3. Determinants of Board interventions

We can now model the determinants of board interventions. As in Figure 1, discussed earlier, we define an intervention as the dismissal of the CEO, top management, or any member of management. We also model three other interventions: Dismissal of a CFO or of a junior manager (i.e., below the level of CEO or CFO), and actions designed to strengthen the management team through new hiring. Corporate governance reform is not a necessary condition for the latter three types of intervention, so we expect the instrument to have a weaker or no effect.

To test the main predictions of principal-agent theory, we relate each of these intervention

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[10] Moreover, the EBRD's data on ownership is undated. This could induce a spurious correlation between performance and ownership if funds acquire more ownership as they inject capital into underperforming firms. To get around this, we would need ownership at the start of each investment, which is not systematically available.

[11] Our results are robust to using a set of crisis indicators along the lines of Frankel and Rose (1996) or using the Hodrick-Prescott (1997) filter to isolate business cycles from GDP data.

[12] See http://www.ebrd.com/country/sector/econo/stats/sei.xls, accessed February 2009.

[13] See http://www.ebrd.com/country/sector/econo/stats/sci.xls, accessed February 2009.

[14] Glaeser, Johnson, and Shleifer (2001) use the EBRD indicator to measure reform in Poland and the Czech Republic.

measures in year *t* to lagged firm performance and to our monitoring proxies. As outlined in Section 2, we control for deal characteristics and macroeconomic conditions (each as of year *t*-1) and a limited set of country effects[15] and a full set of year and industry effects. We also include the instrumental variable. The IV equals one if, in year *t*, there is a law in place empowering the board to dismiss management, and zero otherwise. (We will explore other timing conventions later.)

Given the binary nature of the dependent variable, we estimate probit models. Recall that we track each investment in an annual panel from inception to the earlier of exit, write-off, or December 2008. As a result, the disturbances may be correlated within firm, so we cluster the standard errors at the level of each investment.[16]

3.1 Baseline results

Column 1 of Table 3, Panel A shows the results for CEO dismissals. They broadly support the main predictions of principal-agent theory. We find that boards are significantly more likely to fire CEOs after a year of poor performance. This echoes prior results in the literature and is consistent with principal-agent theory: If the agent's effort is unobservable, the principal should fire him following poor performance, even if the agent's action didn't cause it. Theory suggests that principals can improve on this situation by actively monitoring the agent, and our results confirm that they do. We find that a board is significantly more likely to fire a CEO if it blamed him for the poor performance,[17] but not if it blamed bad luck. In this way, active monitoring allows boards to take more accurate disciplinary action, thereby increasing efficiency as well as the agent's incentives to expend costly effort. Boards are also more likely to remove CEOs they have come to view as incompetent, suggesting that active monitoring enables principals to learn about the agent's

[15] As Table 2 makes clear, a full set of country dummies would over-determine our equations. Instead, we control for the five most active countries (Poland, Russia, Hungary, the Czech Republic, and Romania). Our results are not sensitive to reasonable alternatives.
[16] Alternatively, we could ignore within-firm correlations and instead cluster at the country-year level, to capture the fact that the instrument varies across countries and time. This has no material effect on our inferences.
[17] Recall that the monitoring proxies are lagged by one year. Our results thus suggest that boards act this year on beliefs they formed last year, rather than merely recording certain opinions this year to justify their current actions.

skill.

Economically, the effects are large. The unconditional probability that a CEO is fired in a given year is 5.9%.[18] A unit drop in the lagged interim performance score (say, from performing in line with expectations to performing somewhat below expectations) increases the probability of CEO dismissal by 30.4% (=1.8/5.9). CEOs who are blamed for poor performance are 111% more likely to be fired the next year. And if the board has come to view the CEO as incompetent, the conditional likelihood of subsequent dismissal increases by 120%.

We also find that CEO turnover increases after macroeconomic shocks. This echoes Jenter and Kanaan's (2008) and Kaplan and Minton's (2006) finding that CEOs at large, publicly traded U.S. firms are punished for bad performance caused by events beyond their control. However, economically this effect is relatively small in our data: A one-standard deviation fall in real GDP increases the likelihood of dismissal by around 16%.

The effect of the instrument is positive, confirming the non-parametric result in Figure 1 that a board is more likely to fire the CEO after the adoption of a law empowering it to do so. The *Z*-statistic of 3.41 (p=0.001) is large enough for the instrument to pass the Staiger-Stock (1997) test for weak instruments, which has a critical value of 10 for an *F*-test of the null that the instrument is uncorrelated with the troublesome regressor.[19] The instrument thus appears to be strong. Its economic effect is also large. Holding the other covariates constant, boards are 3.9 percentage points or 66.1% more likely to fire CEOs after corporate governance laws have been enacted than before. Thus, even sophisticated investors, such as the private equity funds in our sample, appear to need a supportive legal environment to exercise their monitoring role. Columns 2 and 3 report similar results for top-management and any-management turnover, respectively.

By contrast, when we focus on the likelihood that a CFO is fired (see column 4), none of our

[18] This is about twice the probability observed in studies of U.S. CEO turnover. See for example Kaplan (1999).
[19] Squaring the *Z*-statistic of 3.41 gives a χ^2 statistic of 11.62 with one degree of freedom. For our N, $\chi^2 \approx F$.

covariates is significant (apart from prior performance, at the 8% significance level). This includes the instrument. The same pattern is repeated in column 5, where we find that the instrument is unrelated to turnover among junior managers,[20] and in column 6, which focuses on board actions to strengthen management through hiring. We interpret these non-results as supporting our identifying assumption. Without the legal power to dismiss management, a board finds it hard to remove an obstinate CEO. By contrast, a CFO or a junior manager can be removed at any time by the CEO, regardless of the legal powers the board has at the time. Similarly, there is no a priori reason to believe that "friendly" board actions should become more effective after a law change. Thus, it is not surprising that the instrument has little effect in columns 4 through 6.

From here on, our tests focus exclusively on CEO, top-, and any-management turnover, for which corporate governance reform appears to be a necessary condition.

3.2 Alternative Specifications

Identification comes from within-country law changes. However, some countries adopted Western-style corporate governance laws before sample funds made investments and so lack within-country variation. For example, Bulgaria reformed its laws in 1991, prior to our sample period. Thus, none of the 15 Bulgarian investments in our sample experienced a legal change. Figure 3 shows the annual rate of CEO turnover broken down by country and by before vs. after a law change. (Results for top- and any-management turnover look similar.) There are eight countries with in-sample variation in corporate governance laws. Except in Turkmenistan, where apparently CEOs are never fired, CEO turnover increased markedly in every country after a law change.[21] In columns 1-3 of Table 3, Panel B, we use only the eight countries with in-sample law

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[20] Consistent with principal-agent models, we find that *junior* managers are not fired simply because the company underperforms, though they are fired when they are deemed incompetent or held responsible for the poor performance.
[21] Figure 3 suggests that law changes have a heterogeneous effect on interventions. The two extremes are Serbia Montenegro, where CEO turnover increases from 0 to 12.5% following a law change, and Croatia, where it increases from 3.8% before to 4.7% after. In Section 5, we show robustness to allowing for country-level heterogeneity.

changes.[22] This reduces the sample size from 1,817 to 1,483 firm-years but does not otherwise affect the results (except that the instrument becomes stronger still in the Staiger-Stock test).

Our baseline models include only a limited set of country effects due to sample size concerns. In columns 4-6 of Panel B, we restrict the sample to the five countries with the most investments (Poland, Russia, Hungary, the Czech Republic, and Romania) for which we can include fixed effects. This again has no material effect on the results, except to strengthen the instrument. We thus view our baseline specifications in Panel A, which use the whole sample, as conservative.

3.3 Instrument Validity Tests

Table 3, Panel C reports a test of the exogeneity of our instrument. If the instrument behaves as we hypothesize, the probability of board intervention should jump in the year the law was changed and then stay higher for a while. Moreover, if law changes provide an exogenous source of variation in board interventions, *future* law changes should not be driving *current* interventions. To test this, we replace the IV with a set of five indicator variables which capture the timing of the law changes relative to the firm-year in question. Specifically, we estimate the effect on the probability of intervention of a law change that took place two or more years earlier; one year earlier; in the same year; one year later; or two years later. (The omitted category is law changes that took place more than two years later.)

The results support our hypothesis, for each of the three intervention variables. All else equal, we find no significant increase in intervention in the year before or two years before a law change, either economically or statistically. The year a law changes, however, the likelihood of intervention increases significantly, by 127%, 129%, and 115% in columns 1-3, respectively. It remains significantly higher the following year and (except for the any-management turnover specification in column 3) thereafter. These patterns – which confirm the nonparametric evidence shown in

[22] Though not tabulated, we could further refine the sample by removing *firms* (rather than *countries*) that did not experience an in-sample law change, that is, those portfolio firms not experiencing a law change while being in a sample fund's portfolio. This cuts the sample to 927 firm-years but again does not affect the results.

Figure 1 – support our identifying assumption.

To further validate the instrument, we estimate placebo regressions as suggested by Bertrand, Duflo, and Mullainathan (2004). We randomly generate a placebo law-change date for each country, estimate the intervention equation as per Table 3, and record the law-change coefficient along with the size of a test of the null hypothesis that intervention is unrelated to the law change. We repeat this 1,000 times. Since the dates we use in the placebo regressions are random, we expect to incorrectly reject the null at the α% level in α% of the trials. This is indeed what we find. We falsely reject the null that the placebo law changes are unrelated to CEO dismissals at the 1% level in 1.1% of the simulations; at the 5% level in 6.4% of the simulations; and at the 10% level in 11.7% of the simulations.[23] These results suggest that the standard errors reported in Table 3 are very close to unbiased, which supports the validity of the instrument.

4. Effect of Board Intervention on Performance

4.1 Naïve Performance Models

Does board intervention improve performance? We first estimate a naïve version of equation (2), treating intervention as exogenous. As before, we focus on the dismissal of the CEO, top management, or any management. In columns 1 through 3 of Table 4, performance is measured as an indicator variable set equal to one if the portfolio firm is exited through an IPO or a sale over the years t to t+2, and zero otherwise. In columns 4 through 6, we additionally require that the IRR is strictly positive in the event of an exit.

In each specification, we find a *negative* and highly statistically significant relation between intervention and performance. To illustrate, all else equal, firing the CEO "results" in a 13.5 percentage point reduction in the probability of exiting over the next two years (from the unconditional probability of 43%). The negative sign suggests we have an endogeneity problem:

[23] The Table 3, Panel A point estimate of 0.520 for the effect of law changes on the likelihood of CEO turnover exceeds 998 of the 1,000 simulated coefficients, giving a simulated p-value of 0.002. The estimated p-value in Table 3, Panel A, based on standard errors clustered on portfolio firm, is nearly identical, at 0.001.

Boards intervene in badly managed or poorly performing companies, so when we compare the exit rates of companies with and without intervention, we are likely picking up unobserved differences in the quality of management or the company rather than the effect of active boards.

The solution to the endogeneity problem is to use an instrument. Before discussing the IV results, we first check whether our instrument has a *reduced-form* effect on performance, and if so, whether the exclusion restriction is likely to hold in our data.

4.2 Reduced-form Performance Models and the Exclusion Restriction

Given the evidence in Section 3 that intervention becomes more likely after changes in corporate governance laws, there should be a positive reduced-form relation between law changes and performance, as long as intervention improves performance. Table 5, Panel A tests if this is so.

We estimate four reduced-form models, namely for exits (columns 1-2) and exits at a positive IRR (columns 3-4), either without (columns 1 and 3) or with portfolio-firm effects (columns 2 and 4). In each case, we find a strong, statistically significant reduced-form relation between the instrument and performance. Importantly, it has the expected positive sign: Law changes strengthening the power of the board over managers are associated with improved exit performance. Economically, the effect is about twice as large when we include firm effects.

The other variables behave as expected. Firms with higher interim performance scores are significantly more likely to be exited and to be exited at a positive IRR. Staged deals are less likely to be exited, consistent with the interpretation that they are riskier. Favorable macroeconomic conditions improve a firm's exit chances. There is also some evidence that privatizations are exited more frequently.[24] The three sets of country, year, and industry effects are each statistically significant. The portfolio-firm effects in columns 2 and 4 are similarly significant. Their inclusion boosts the pseudo-R^2 by around 16 percentage points and affects the coefficient estimates of some

[24] Interestingly, there is some evidence in Table 5 that companies run by managers the board deemed incompetent are *more* likely to succeed. This counter-intuitive "result" suggests an omitted variable bias. Intervention correlates with incompetence (see Table 3), but of course the reduced-form models discussed here do not control for intervention.

of the controls, especially the macro and staging variables.

A reduced-form relation between law changes and performance is reassuring, but for our identification strategy to work, law changes must affect performance through their effect on board decisions rather than directly. There are three main reasons why this may not be the case.

Changes in corporate governance laws may have coincided with other beneficial economic shocks which in turn affected performance. An obvious concern stems from the fact that corporate governance reforms were often part of broader reforms of commercial law affecting contracts, employment, intellectual property, etc., not just the power of the board relative to management. It is therefore possible that replacing Soviet with Western commercial law affects performance independently of the role of the board. To shed light on this, we exploit a convenient feature of our data. Five sample countries (Croatia, the Czech Republic, Estonia, Lithuania, and Russia) amended the articles pertaining to the powers of the board some years after enacting Western-style commercial laws. (As Table 1 shows, the instrument uses the amendment date for these countries.) For example, under the 1996 Federal Law on Joint Stock Companies, Russian boards were originally given few powers, and Stepanov (2002) notes that until the law was amended in 2002, managers could stay in power even if a majority of shareholders voted against them.

If it is the adoption of commercial law that affects performance, rather than the strengthening of the board, then corporate governance *amendments* should have no effect on performance in the reduced-form models. In Panel B of Table 5, we restrict the sample to the five countries with such amendments.[25] In each of these, a Western-style corporate law was already in place at the time a sample fund invested in the country, so the law-change instrument isolates the effect of corporate governance amendments on performance. The point estimates in the four specifications confirm that strengthening corporate governance laws affects performance independently of the adoption of

[25] Alternatively, we could use the whole sample and run a horse race between the adoption of Western-style corporate law and the adoption of laws strengthening the board. When we do so, we find that the former has no significant effect on exit performance (p=0.424) while the latter does (p=0.003).

Western-style commercial law. Each coefficient is positive and statistically significantly different from zero, but not significantly different from the point estimates shown in Panel A of Table 5 where we include all 17 countries. This lends credibility to the exclusion restriction.

Second, there could be other contemporaneous shocks besides reform of commercial law. However, the staggered adoption of corporate governance laws mitigates this source of bias to a large extent. To violate the exclusion restriction, each country would have to strengthen board powers at exactly the same time as some other beneficial economic shock hit, which is somewhat unlikely. To further reduce the chances that our instrument correlates with unobserved economic shocks, we explicitly control for macroeconomic conditions and a country's reform progress.

A third concern is that the timing of corporate governance reforms may have been influenced by lobbying which in turn may correlate with performance. This also seems unlikely. Legal reform in transition countries was aided by the World Bank and the European Commission. These bodies provided technical assistance on their own timetables and according to their own resource constraints, and they are unlikely to be swayed by the relatively small companies in our sample.

We can test lobbying and other reverse-causality stories by replacing the IV with the set of five indicators we used in Panel C of Table 3. Though not tabulated, we find no evidence that *future* law changes affect performance, while *past* law changes do. This is consistent with the claim that the timing of law changes is orthogonal to the performance of portfolio firms in our sample.

4.3 IV Estimates of the Effect of Intervention on Performance

We now estimate the structural effect of intervention on performance using law changes as an instrument. As explained earlier, this involves estimating the intervention and performance equations jointly using a bivariate probit. The results are shown in Table 6, Panel A. In each specification, a likelihood ratio tests rejects the null that the disturbances in the two equations are uncorrelated.[26] This confirms that intervention is endogenous, as suspected, and needs to be instrumented. It also implies that our controls for management and firm quality are imperfect[27] (or that there is another omitted variable altogether that affects both intervention and performance).

Importantly, the sign of the intervention variable flips in every specification, compared to the naïve models in Table 4. Each point estimate is statistically significant at the 1% level or better. This provides direct evidence of the monitoring role of boards: Once we instrument it, intervention does improve performance. The economic effects are large. Holding all covariates at their sample means, firing the CEO increases the probability of exiting over the next two years by 52.8 *percentage points* from the unconditional probability of 43%, while the probability of exiting at a positive IRR improves from 31.3% to 95.5%. The effects of firing either a top member or any member of management are only a little smaller. In sum, even though boards intervene in lower-quality companies, doing so makes a successful investment outcome highly likely.

Interestingly, poor managerial skill or effort has no effect on performance in Table 6. This suggests that through intervening, boards can prevent managerial problems from having a lasting impact on performance, precisely as corporate governance models predict.

4.4 Adding Portfolio-Firm Effects

Having a panel allows us to include portfolio-firm effects to remove possible omitted variables biases that are due to unobserved firm-specific variables that are constant over time. Two key variables that we do not have data for are the CEO's ownership and his compensation contract. Some CEOs may have majority ownership, making it near impossible to fire them; such entrenched CEOs may also be associated with worse performance. Alternatively, CEOs whose compensation contracts provide optimal incentives are never fired and their firms will likely perform well.

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[26] Monfardini and Radice (2008) provide Monte Carlo evidence favoring a likelihood ratio test of the null that the disturbances are uncorrelated. This test performs well even when the distribution of the errors is misspecified. (A probit model assumes, of course, normality.) The same is not true of a *t*-test on the correlation coefficient. As expected, the correlation coefficient is negative, suggesting that boards intervene in lower-quality companies.

[27] E.g., being binary, our controls do not capture quality differences within the group of incompetent managers.

Formally, we rewrite equations (1) and (2) by adding firm-specific effects as follows:

$$intervention_{it} = \delta_1 x_{1it-1} + \nu_{1i} + \varphi_1^{'}(\lambda_k + \lambda_t + \lambda_j) + \varepsilon_{1it} \tag{3}$$

$$performance_{it} = \beta_2 intervention_{it} + \delta_2 x_{2it-1} + \nu_{2i} + \varphi_2^{'}(\lambda_k + \lambda_t + \lambda_j) + \varepsilon_{2it} \tag{4}$$

If the unobserved firm-specific effects ν_{1i} and ν_{2i} are correlated across equations, they will affect both intervention and performance, so their omission could cause the estimate of β_2 to be biased.

Table 6, Panel B shows that the bias is minimal. Including firm effects reduces the point estimates for the intervention variables by very little compared to those shown in Panel A, economically and statistically, leaving our conclusions unchanged.[28] Interestingly, the firm effects are negatively correlated across equations, consistent, for example, with an omitted management quality or ownership factor: Low-quality or entrenched management increases the likelihood of intervention and decreases the likelihood of good performance, resulting in a negative correlation.

4.5 Modeling Interim Performance

So far, we have measured performance as successful exits. We now use the EBRD's interim performance score discussed in Section 2.2 as an alternative measure. Recall that the score is defined as performance relative to expectation (i.e., the firm's budget). We treat the score as a continuous variable, which allows us to estimate standard linear regressions rather than probits. Specifically, we relate the performance score in year *t*+1 to an indicator that equals 1 if the board intervened in year *t* and zero otherwise and to a set of control variables dated *t*-1.

When we estimate naïve OLS regressions, shown in columns 1-3 of Table 7, we find no evidence that interventions have any significant effect on subsequent performance, whether we focus on CEO, top-management, or any-management turnover.

Recognizing that the treatment variable – board intervention – is endogenous, we next estimate

[28] As in Table 5, the portfolio-firm effects have a large effect on the coefficient estimates of some controls (namely, the macroeconomic conditions and staging variables), but not on the coefficient of interest (namely, intervention).

Heckman (1978) treatment models, shown in columns 4-6.[29] As in the probit models, we use law changes in year *t* as an instrument. The resulting point estimates are not only highly statistically significant, they are also economically large. For example, firing the CEO is followed by a 1.41-point increase in the performance score (which ranges from 1 to 5 with a mean of 2.9).

Interestingly, companies whose performance in *t*-1 was poor due to "bad luck" subsequently perform better. This suggests that bad luck is not persistent, allowing firms to bounce back later.

4.6 Extensive Versus Intensive Margin

The interim performance data allow us to shed light on the margins through which corporate governance reforms matter. Our evidence so far suggests that the reforms matter on the extensive margin: When the law changes, boards can more easily dismiss managers, and such interventions have a large impact on performance. The reforms could also operate on the intensive margin: By increasing the *threat* of dismissal, the reforms could induce managers to raise their game regardless of a board's actions. However, this effect appears to be considerably weaker in the data. Figure 4 shows that the average performance score is flat around law changes. Similarly, when we regress the performance score on the law change instrument (not tabulated), we find no significant contemporaneous effect and only a weakly positive effect at a one-year lag (p=0.087). Thus, managers do not appear to raise their game simply because the law has changed.[30] Instead, company performance improves when a board actually removes an underperforming manager.

4.7 Discussion

In all models, we get the same result: As long as we instrument it, intervention has a large, positive, and significant effect on performance, whether we measure performance as exit or relative to plan. Importantly, we find evidence that there *is* simultaneity bias, in the sense that the naïve

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[29] The normality assumption underlying the Heckman (1978) model may be violated, since the performance score has support on a bounded interval. Applying a standard logistic transform and re-estimating the Heckman models does not materially change our results (not tabulated), suggesting that normality is an acceptable approximation for these data.

[30] Of course, our data do not permit us to rule out that boards raise their expectations as soon as the law changes and managers simultaneously raise their game.

probit estimates are negative and significant. This appears due to a negative correlation between the disturbances in the intervention and performance equations, indicating that boards intervene in badly managed or poorly performing firms. Our instrument appears to do a good job breaking the simultaneity. Finally, the performance effect appears to require an actual intervention; an increased threat of dismissal appears to have little effect on performance.

5. Robustness

In this section, we report variations on the specifications reported in Table 6. First, we explore whether the results are sensitive to the horizon over which exit is measured. Previously, we related exit in years *t* through *t*+2 to intervention in year *t*. (Every intervention precedes an exit or a write-off, so there is no problem including exits in year *t*.) In Panel A of Table 8, we use two alternative horizons, namely *t* through *t*+1 and simply *t*. Columns 1-3 focus on exit alone while columns 4-6 additionally require that the IRR be positive. To save space, we report only the intervention coefficients. All other covariates shown in Table 6 are included in the estimation but not shown.

For each exit horizon, performance measure, and intervention target, we continue to find that management turnover improves the probability of exit significantly, both economically and statistically. For example, firing top management increases the probability of exit by year *t*+1 by 60 percentage points from the unconditional probability of 31.9%. The economic magnitudes in the other specifications are similarly large.

The ultimate fate of 49 of the 442 investments is unknown as of the end of our sample period, December 2008. The models shown so far implicitly assume that these investments will never be exited (they are coded as zeros), or at least not at a positive IRR. While this may be a plausible assumption given the poor state of the exit markets in 2009, it might bias our results. Specifically, if the 49 right-censored investments later were to turn out successful without having experienced interventions during our sample period, our estimates would be biased in favor of finding an effect of intervention on performance. (In fact, the right-censored investments are significantly *more* likely to experience interventions than are the successfully exited ones, so this story is unlikely.) Panel B restricts the sample to investments made before 2000, which largely avoids the problem of right-censoring as only six of the 244 pre-2000 investments remain alive as of 2008.[31] This has no material effect on our results, qualitatively or quantitatively.

Panels C and D restrict the sample to the set of countries with in-sample variation in corporate governance laws and the five countries with the most investments, respectively. This mirrors the first-stage models shown in Table 3, Panel B. In either sub-sample, we continue to find a significant and large effect of board intervention on performance.

So far, we have constrained the effect of law changes on intervention to be constant across time and across countries, by using a simple intercept shift in the law-change year. In Panel E, we allow the effect to vary over time by replacing the instrumental variable with a set of five indicator variables. These relate board interventions to whether a law change took place two or more years earlier, one year earlier, in the same year, one year later, or two years later. (In other words, we estimate a bivariate probit with the specification shown in Table 3, Panel C as the intervention equation.) In Panel F, we separately allow the effect to vary across countries by including country-level law change indicators. In either case, the coefficients estimated for the effect of intervention on performance are again barely changed.

6. Conclusions

We test the main predictions of principal-agent theory by testing under what circumstances boards of directors discipline managers and whether such interventions improve performance. Our identification strategy exploits plausibly exogenous variation in the timing of the introduction of laws empowering boards to dismiss managers in 17 post-Communist countries that transitioned from centrally-planned to market economies between 1991 and 2005. We show that these legal

[31] An alternative solution to the problem of right-censoring is to estimate a duration model, which models the hazard of exiting next period. The problem with this approach is that it cannot (currently) accommodate instruments.

reforms had a large effect on forced management turnover without apparently affecting performance other than through their effect on board actions.

Our main findings can be summarized as follows. When given the legal power to do so, boards fire CEOs in response to poor performance relative to plan, especially if they hold the CEO responsible for the poor performance or have come to view him as incompetent. CEOs are not fired when performance deteriorates due to factors beyond their control, though we do see more management turnover in tougher macroeconomic environments.

Given our evidence that boards intervene in underperforming companies, it is not surprising that a naïve model, which treats intervention as exogenous, spuriously suggests intervention "hurts" performance. Instrumenting intervention using corporate governance law changes leads to the opposite conclusion: Following forced management turnover, companies see performance improvements and their investors are considerably more likely to eventually sell them at a profit. These results suggest that active monitoring can be quite beneficial.

While our source of variation in principle affects all companies in our sample countries, the nature of our data is such that our results may only pertain to companies with large, sophisticated shareholders. Thus, we cannot say whether companies with dispersed ownership (see Berle and Means (1933)), or those with a majority of independent directors who may only nominally be accountable to shareholders (see Kumar and Sivaramakrishnan (2008)), would and could benefit from similar performance improvements. Likewise, we cannot say to what extent our results carry over to developed countries (such as the U.S.) whose comparatively more stable legal systems provide fewer natural experiments.

Our paper focuses mostly on the disciplinary role of the board. The reason why we do not model board activities designed to assist management is that our instrument doesn't apply to them: The board doesn't need the legal power to dismiss the CEO in order to offer strategic advice, help recruit talent, or make introductions to potential customers or suppliers.[32] We leave this interesting topic for future research.

[32] Facing the same problem, Bottazzi et al. (2008) argue that the supply of VC capital is local and hence use the local availability of experienced investors as an instrument. This argument would not work in our context, because we observe many private equity funds investing in multiple countries, and some that invest from afar (say, from London). In extremis, there is only one local market – Europe – and so we cannot use Bottazzi et al.'s identification strategy.

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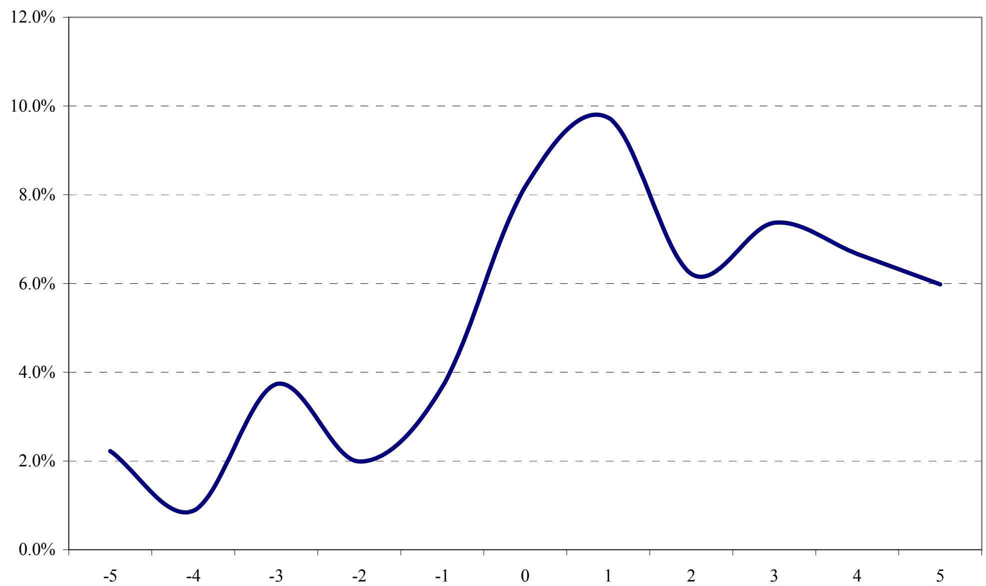
Figure 1. Management Turnover Around Law Changes.

The graphs show the annual incidence of CEO, top-management, and any-management turnover, respectively. Top management refers to the CEO or CFO; any management also includes dismissals of other named executive. All turnover is forced rather than voluntary. Time on the horizontal axis is measured relative to the year in which the country in question reformed its corporate governance laws, denoted year 0; see Table 1 for a list of these dates. Year -5 includes prior years; year 5 includes later years.

CEO change



Top management change

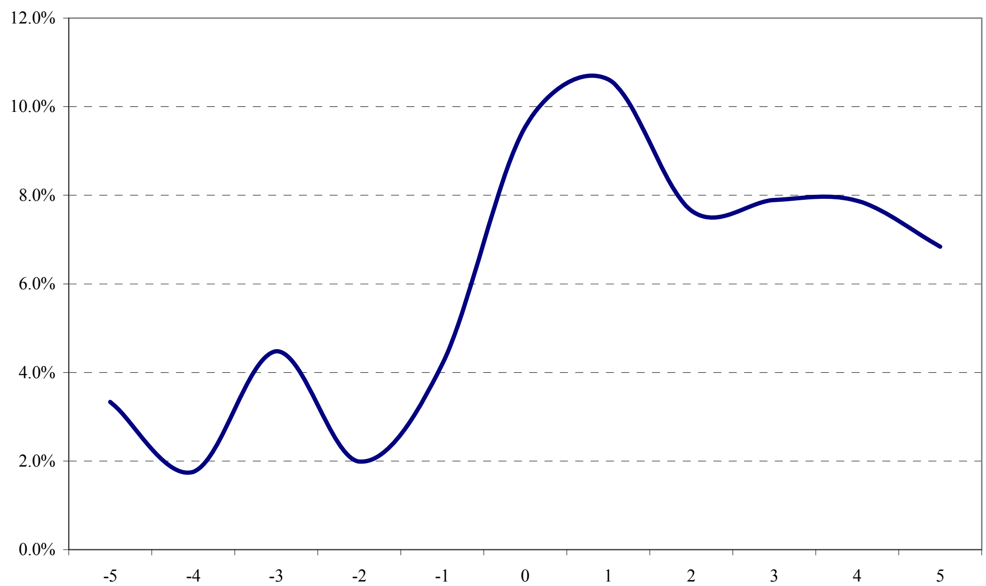
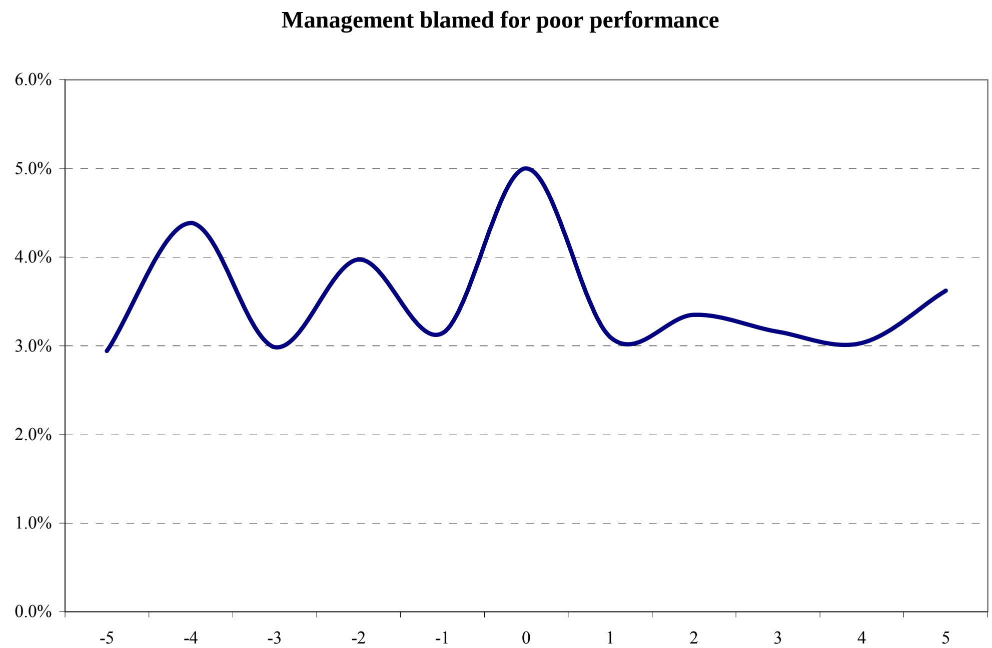


Figure 1. Continued.

Figure 2. Management Quality Around Law Changes.

The EBRD regularly reviews the performance of each portfolio firm, resulting in a monitoring report based on both quantitative and qualitative criteria. The graphs show the annual incidence of a monitoring report mentioning that a fund manager blamed management for a company's poor performance or that he viewed management as incompetent. Time on the horizontal axis is measured relative to the year in which the country in question reformed its corporate governance laws, denoted year 0; see Table 1 for a list of these dates. Year -5 includes prior years; year 5 includes later years.



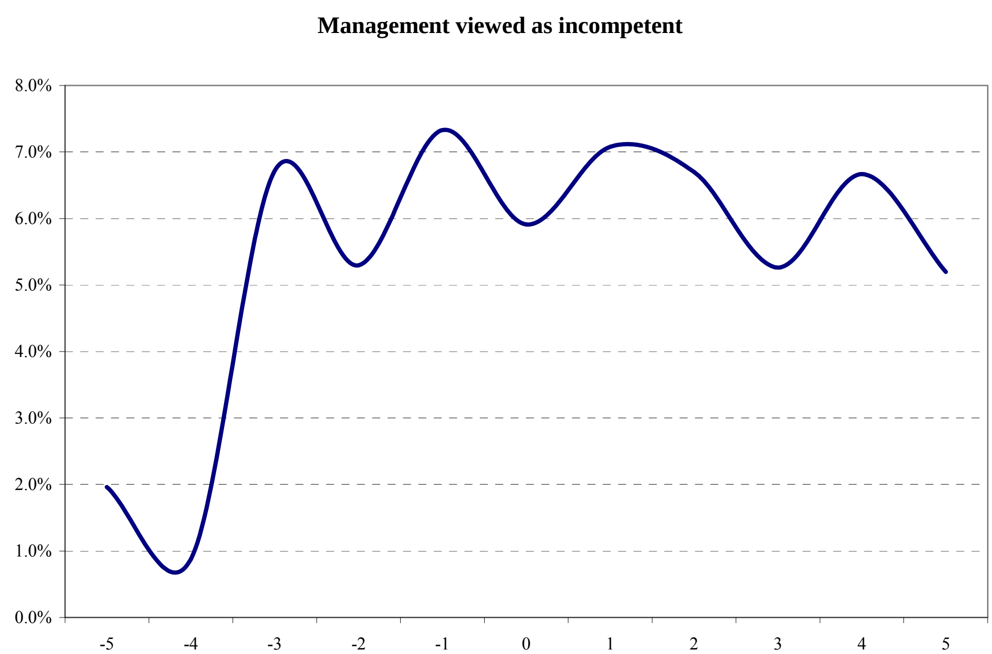


Figure 3. Management Turnover By Country Before and After Law Changes.
The figure shows the annual rate of CEO turnover broken down by country and by before vs. after a law change, using the law change dates listed in Table 1. (Results for top-management turnover and any-management turnover look similar and are not shown.) The number of sample firms per country is shown in parentheses. Recall that we have repeated observations for each sample firm over time. Thus, the unit of observation in the figure is a firm-year rather than a sample firm, and the height of each bar gives the *annual* incidence of CEO turnover in the sample. Absence of observations in either the before or the after period in a given country is indicated by a blank. For example, all Bulgarian sample investments were undertaken after the 1991 Commercial Act came in effect, so we cannot compute CEO turnover for the period before the law change.

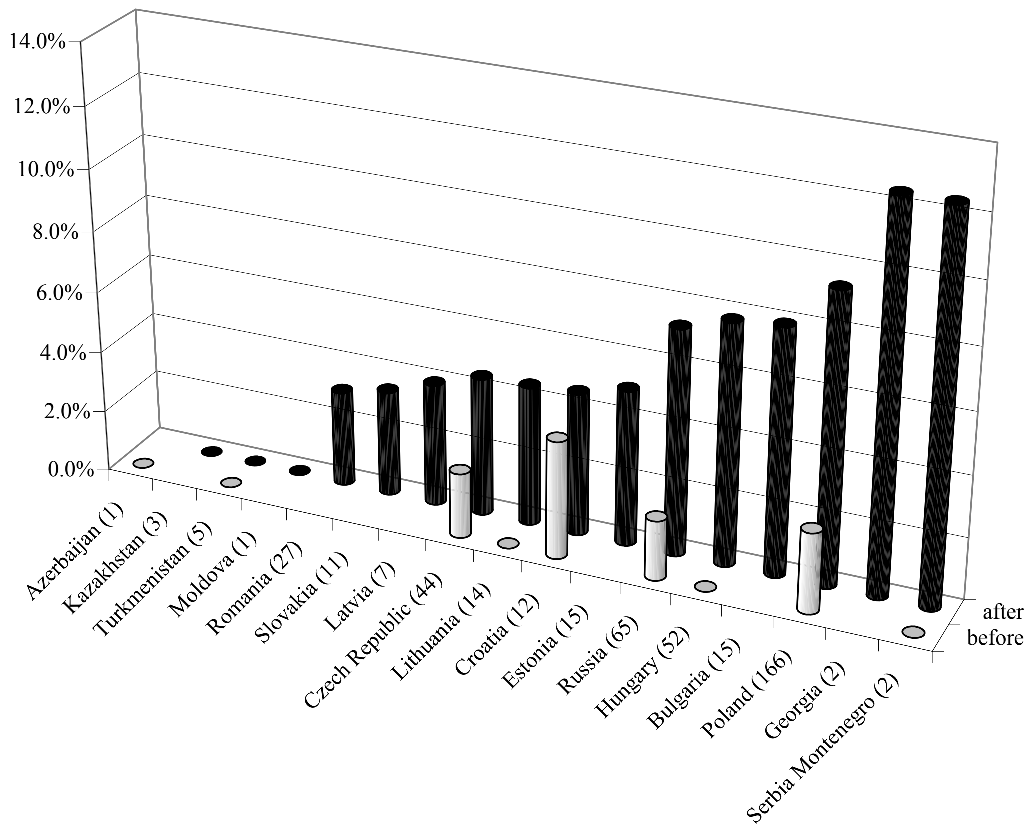


Figure 4. Interim Performance Scores Around Law Changes.

The EBRD regularly reviews the performance of each portfolio firm, resulting in a monitoring report based on both quantitative and qualitative criteria. We use the monitoring reports to construct a variable that captures the performance of each company each year relative to its annual budget. Portfolio firms are scored from 1 to 5, where 3 denotes performance in line with expectations (as set out in the company's budget for the year); 4 and 5 denote performance above and greatly above expectations; and 2 and 1 denote underperformance and severe underperformance relative to expectations. The graph shows the average interim performance score for each year around law changes. Time on the horizontal axis is measured relative to the year in which the country in question reformed its corporate governance laws, denoted year 0; see Table 1 for a list of these dates. Year -5 includes prior years; year 5 includes later years.

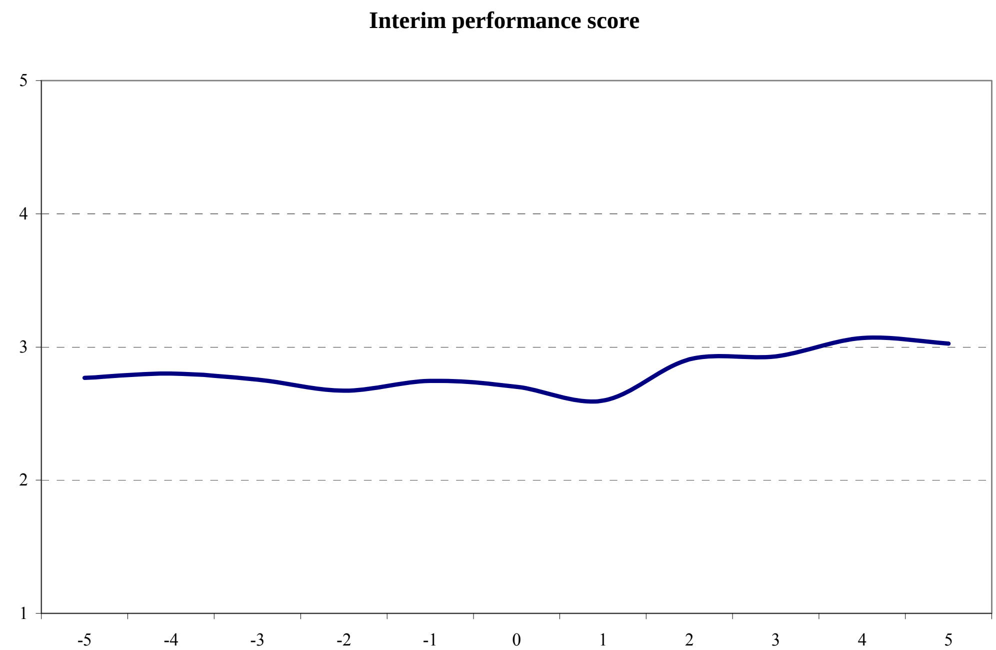


Table 1. Overview of Legal Changes Strengthening Supervisory Boards in Central and Eastern Europe and in Central Asia.
The table lists legal changes strengthening supervisory boards relative to executive boards. Most countries in our sample adopted the German system of a dual board structure. In the text, we use the terms "management" and "board" as a short-hand for the executive and supervisory boards, respectively. In addition to the laws themselves, we have consulted the following sources: EBRD Corporate Governance Legislation Assessment Project, 2007 (http://www.ebrd.com/country/sector/law/corpgov/assess/index.htm); EBRD Country Strategy Overview (http://www.ebrd.com/country/index.htm); World Bank Reports on the Observance of Standards & Codes (http://www.worldbank.org/ifa/rosc_cg.html); International Law Office (http://www.internationallawoffice.com); International Financial Law Review (http://www.iflr.com/Countries.aspx); OECD Corporate Governance Regional Roundtables (www.oecd.org/daf/corporateaffairs/roundtables); Doing Business country reports (http://www.doingbusiness.org); Federation of Euro-Asian Stock Exchanges (http://www.feas.org/MemberIndex.cfm); "New Joint Stock Company Law in Kazakhstan" by V.V. Markov (http://rusenergylaw.ru/2-2003/kaz13.html); the EBRD's legal journal, Law in Transition (http://www.ebrd.com/pubs/legal/series/lit.htm); "Corporate Governance and Securities Market Legislation in Transition" by G.P. Cigna (Journal of International Banking and Financial Law 21:11); and "Guide to the Russian Law on Joint Stock Companies" by B. Black, R. Kraakman, and A. Tarassova (Kluwer Law International, 1998, The Hague). We are grateful to Gian Piero Cigna, Senior Counsel of the EBRD, for expert advice.

Country	Year	Synopsis of law changes affecting corporate governance
Azerbaijan	-	-
Bulgaria	1991	"Commercial Act" in effect empowering the supervisory board to monitor the executive board and to dismiss members of the executive board (Article 241)
Croatia	2003	"Law on Commercial Companies and Bankruptcy" amended to empower the supervisory board to monitor the executive board and to dismiss members of the executive board (Articles 240, 241)
Czech Republic	2000	"Commercial Law" amended to empower the supervisory board to dismiss members of the executive board if provided for in the company's statutes (Section 194)
Estonia	1996	"1995 Commercial Code" amended to empower the supervisory board to monitor the executive board and to dismiss members of the executive board (Sections 308, 309)
Georgia	1995	"Law on Entrepreneurs" in effect empowering the supervisory board to monitor the executive board and to dismiss members of the executive board (Article 55.8)
Hungary	1998	"Act on Company Law No. 144/1997" in effect giving the supervisory board limited decision-making powers to complement their existing supervisory role (Sections 33, 35)
Kazakhstan	1998	"Law on Joint Stock Companies" in effect empowering the supervisory board to monitor the executive board and to dismiss members of the executive board (Article 53.2)
Latvia	2001	"Commercial Law" in effect empowering the supervisory board to monitor the executive board and to dismiss members of the executive board (Section 292) and to veto major executive decisions (Section 294)
Lithuania	2000	"Company Law" amended to empower the supervisory board to dismiss members of the executive board (Article 32)
Moldova	1997	"Joint Stock Company Law" in effect empowering the supervisory board to monitor the executive board and to oversee major capital expenditures (Article 65). Power to dismiss members of the executive board can be delegated to the supervisory board by corporate statute (Article 50.4)
Poland	2001	"Code of Commercial Companies" in effect empowering the supervisory board to suspend members of the executive board (Art. 368)
Romania	1990	"Company Law" in effect empowering the supervisory board to monitor the executive board and to dismiss members of the executive board (Article 153)
Russia	2002	"Federal Law on Joint Stock Companies" amended to empower the supervisory board to suspend members of the executive board without recourse to a shareholders' meeting (Articles 48, 65)
Serbia Montenegro	2004	"Companies Act" in effect empowering independent directors to dismiss executive directors with a majority vote (Article 317)
Slovakia	1992	"Commercial Code" in effect empowering the supervisory board to monitor the executive board and to oversee major capital expenditures (Section 197)
Turkmenistan	1999	"Law on Joint-Stock Companies" in effect empowering the supervisory board to monitor the executive board and to dismiss members of the executive board if provided for in the corporate charter (Article 48)

Table 2. Sample Overview.

The sample consists of 442 investments by 43 so called institutional-quality private equity funds investing in Central and Eastern Europe and the Central Asian republics of the former Soviet Union. The private equity funds were raised between 1992 and 2004 and made investments between 1993 and 2005. We track each investment through the earlier of the final outcome or the end of 2008 and record whether it has been fully realized (i.e., exited through an IPO or a sale), written off, or is still alive, as of 2008. "Alive" investments include those where an IPO has taken place but the fund has yet to sell its stake. Tracking each investment across time gives us an unbalanced panel. We compute the profitability of each investment as the time-weighted return on investment (i.e., as the IRR) using precisely dated cash flows to and from portfolio firms. All IRRs are calculated from cash flows denominated in the fund's home currency. Results are robust to converting cash flows into euros or dollars before computing IRRs.

	Number of investments by year of initial investment														Percent of sample that are			Mean
country	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	Total	fully realized	written off	still alive	IRR (%)
Azerbaijan							1							1	0.0	100.0	0.0	-100.0
Bulgaria				1	3		2		2		1	6		15	93.3	6.7	0.0	45.8
Croatia						1	1	5	1	2		2		12	66.7	8.3	25.0	-18.1
Czech Republic	3	5	5	1	5	4	8	9	2	2				44	77.3	20.5	2.3	28.2
Estonia				2		4	1	1	1		3	2	1	15	73.3	0.0	26.7	31.7
Georgia				1						1				2	100.0	0.0	0.0	15.3
Hungary	1	2	3	3	6	11	4	8	6	2	2	4		52	59.6	17.3	23.1	-0.7
Kazakhstan						1	2							3	100.0	0.0	0.0	54.8
Latvia										1	1	3	2	7	28.6	14.3	57.1	10.1
Lithuania				1	1	2	4	1	2			1	2	14	57.1	35.7	7.1	-15.0
Moldova												1		1	100.0	0.0	0.0	158.5
Poland	11	12	14	13	18	12	16	27	14	8	11	5	5	166	69.3	24.1	6.6	22.8
Romania				5		6	8	2	2			1	3	27	88.9	3.7	7.4	18.8
Russia		4	12	9	7	4		4	11	6	3	4	1	65	64.6	21.5	13.8	0.7
Serbia Montenegro											2			2	0.0	0.0	100.0	-24.2
Slovakia						1		4	5			1		11	90.9	9.1	0.0	4.6
Turkmenistan			1			1	1	1	1					5	100.0	0.0	0.0	44.6
All countries	15	23	35	36	40	47	48	62	47	22	23	30	14	442	70.1	18.8	11.1	15.6

Table 3.
Panel A: Determinants of Board Intervention.

The sample consists of an unbalanced annual panel of 442 companies which we observe from the year of the initial investment to the year of the final outcome (exit or write-off) or 2008, whichever is earlier. Panel A focuses on the determinants of board intervention, defined as the removal of an executive (columns 1-5) or actions to strengthen the management team by hiring additional senior managers (say, a manager in charge of exports). Top management refers to the CEO or CFO; junior management refers to any other named executive. Estimation uses probit. The explanatory variables are listed in the table and defined further in Section 2. We include an instrumental variable and so estimate the reduced form of the board intervention equation (1) in Section 1. The instrument equals one if in calendar year *t* the corporate law in country *k* allows the company's supervisory board to dismiss a member of the executive board, and zero otherwise. Table 1 provides details of the staggered adoption of such laws in the 17 countries in our sample. The Staiger-Stock test is a Wald test of the null hypothesis that the instrument does not correlate with board interventions. The critical value is 10. All specifications include country, industry, and time effects. The number of observations is 1,817. Heteroskedasticity-consistent standard errors, clustered on portfolio firm, are reported in italics beneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Dependent variable =1 if …					
	CEO change	top mgmt change	any mgmt change	CFO change	junior mgmt change	mgmt streng-thening
	(1)	(2)	(3)	(4)	(5)	(6)
Firm and mgmt performance						
interim performance score in *t*-1	-0.123**	-0.124***	-0.105**	-0.120*	-0.025	-0.099*
	0.050	*0.048*	*0.044*	*0.069*	*0.061*	*0.054*
=1 if poor performance blamed on management in *t*-1	0.486**	0.407**	0.638***	0.067	0.721***	0.593***
	0.205	*0.200*	*0.173*	*0.317*	*0.180*	*0.180*
=1 if poor performance blamed on bad luck in *t*-1	-0.018	0.056	0.042	-0.080	-0.090	0.134
	0.148	*0.144*	*0.128*	*0.209*	*0.166*	*0.144*
=1 if management viewed as incompetent in *t*-1	0.525***	0.514***	0.495***	0.338	0.398**	0.096
	0.158	*0.153*	*0.149*	*0.228*	*0.189*	*0.182*
Deal characteristics						
log investment cost through *t*-1	0.055	0.090**	0.002	0.108	-0.060**	0.019
	0.040	*0.044*	*0.034*	*0.067*	*0.026*	*0.039*
=1 if privatization	0.375**	0.281*	0.299*	-0.052	0.342*	0.498**
	0.171	*0.165*	*0.161*	*0.325*	*0.193*	*0.198*
=1 if deal is staged	0.161	0.135	0.120	0.156	0.232**	0.443***
	0.103	*0.101*	*0.091*	*0.128*	*0.112*	*0.095*
Macroeconomic conditions						
lagged EBRD transition indicator	-0.168	-0.148	-0.077	-0.306	-0.005	-0.555***
	0.245	*0.230*	*0.212*	*0.251*	*0.257*	*0.168*
lagged real GDP growth	-0.032**	-0.027*	-0.010	-0.018	0.008	0.010
	0.016	*0.016*	*0.015*	*0.019*	*0.024*	*0.016*
Instrument						
=1 if corp governance law enacted	0.520***	0.503***	0.418***	0.320	0.153	0.020
	0.152	*0.147*	*0.127*	*0.197*	*0.183*	*0.132*
Diagnostics						
Wald test: all coeff. = 0 (χ^2)	79.6***	83.9***	94.9***	31.2*	78.2***	91.8***
Staiger-Stock (1997) instrument strength test (χ^2)	11.6***	11.7***	10.9***	2.7	0.7	0.0
Pseudo-R^2	10.7%	9.7%	8.8%	6.8%	10.4%	10.2%

Table 3.
Panel B: Excluding Countries Without In-sample Legal Variation or With Few Investments.

To show robustness, Panel B re-estimates the probit models of Panel A in two sub-samples. In columns 1-3, we restrict the sample to the eight countries with in-sample-period variation in corporate governance laws shown in Figure 3. For example, Bulgaria adopted its corporate governance laws in 1991, before the beginning of our sample period. Thus, none of the 15 Bulgarian investments in our sample experiences a *change* in corporate governance laws and these are consequently dropped in columns 1-3. In columns 4-6, we restrict the sample to the five countries with the most investments, namely Poland (with 166 investments), Russia (65), Hungary (52), the Czech Republic (44), and Romania (27). The Staiger-Stock test is a Wald test of the null hypothesis that the instrument does not correlate with board interventions. The critical value is 10. All specifications include country, industry, and time effects. The number of observations is 1,483 in columns 1-3 and 1,489 in columns 4-6. Heteroskedasticity-consistent standard errors, clustered on portfolio firm, are reported in italics beneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Countries with law changes affecting sample companies during the sample period			**Five most active countries (Poland, Russia, Hungary, Czech Republic, and Romania)**		
	Dependent variable = 1 if			Dependent variable = 1 if		
	CEO change	top mgmt change	any mgmt change	CEO change	top mgmt change	any mgmt change
	(1)	(2)	(3)	(4)	(5)	(6)
Firm and mgmt performance						
interim performance score in *t*-1	-0.101*	-0.096*	-0.056	-0.115**	-0.110**	-0.072
	0.055	*0.053*	*0.048*	*0.053*	*0.052*	*0.047*
=1 if poor performance blamed on management in *t*-1	0.656***	0.572***	0.740***	0.619***	0.539**	0.675***
	0.221	*0.216*	*0.193*	*0.222*	*0.219*	*0.198*
=1 if poor performance blamed on bad luck in *t*-1	0.055	0.141	0.150	0.027	0.060	0.060
	0.154	*0.152*	*0.136*	*0.155*	*0.153*	*0.137*
=1 if management viewed as incompetent in *t*-1	0.491***	0.502***	0.459***	0.608***	0.612***	0.556***
	0.178	*0.170*	*0.166*	*0.177*	*0.171*	*0.166*
Deal characteristics						
log investment cost through *t*-1	0.041	0.068	-0.006	0.040	0.062	-0.001
	0.037	*0.043*	*0.030*	*0.038*	*0.042*	*0.031*
=1 if privatization	0.165	0.102	0.161	0.127	0.074	0.146
	0.181	*0.177*	*0.172*	*0.185*	*0.180*	*0.178*
=1 if deal is staged	0.191*	0.174	0.161*	0.184	0.172	0.145
	0.109	*0.108*	*0.098*	*0.114*	*0.112*	*0.102*
Macroeconomic conditions						
lagged EBRD transition indicator	-0.215	-0.201	-0.257	-0.824	-0.911*	-1.258***
	0.218	*0.202*	*0.193*	*0.503*	*0.480*	*0.418*
lagged real GDP growth	-0.045**	-0.038**	-0.032**	-0.041**	-0.036*	-0.028*
	0.020	*0.019*	*0.016*	*0.020*	*0.020*	*0.017*
Instrument						
=1 if corp governance law enacted	0.560***	0.549***	0.485***	0.671***	0.730***	0.719***
	0.156	*0.149*	*0.127*	*0.194*	*0.187*	*0.163*
Diagnostics						
Wald test: all coeff. = 0 (χ^2)	61.2***	69.1***	64.4***	76.1***	82.3***	85.4***
Staiger-Stock (1997) instrument strength test (χ^2)	12.8***	13.6***	14.6***	12.0***	15.2***	19.5***
Pseudo-R^2	10.6%	10.1%	8.5%	12.0%	11.3%	10.2%

Table 3.
Panel C: Time Profile of the Effect of Law Changes on Board Interventions.

This panel reports a test of the validity of the law-change instrument. We replace the instrumental variable of Panel A with a set of five indicator variables to capture the timing of the law changes relative to the panel year in question. Specifically, we estimate the effect on the probability of board intervention of a law change that took place two or more years earlier, one year earlier, in the same year, one year later, or two years later. (The omitted category is law changes that took place more than two years later.) The probit models are estimated in the full sample. All specifications include country, industry, and time effects. The number of observations is 1,817. Heteroskedasticity-consistent standard errors, clustered on portfolio firm, are reported in italics beneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Dependent variable =1 if …		
	CEO change	top mgmt change	any mgmt change
	(1)	(2)	(3)
Firm and management performance			
interim performance score in *t*-1	-0.119**	-0.121**	-0.102**
	0.052	*0.054*	*0.049*
=1 if poor performance blamed on management in *t*-1	0.479**	0.397*	0.629***
	0.211	*0.206*	*0.178*
=1 if poor performance blamed on bad luck in *t*-1	-0.024	0.050	0.043
	0.181	*0.184*	*0.144*
=1 if management viewed as incompetent in *t*-1	0.539***	0.525***	0.494***
	0.155	*0.156*	*0.161*
Deal characteristics			
log investment cost through *t*-1	0.068*	0.107**	0.008
	0.041	*0.047*	*0.039*
=1 if privatization	0.373	0.283	0.310
	0.256	*0.256*	*0.228*
=1 if deal is staged	0.162*	0.138	0.122
	0.092	*0.086*	*0.082*
Macroeconomic conditions			
lagged EBRD transition indicator	-0.074	-0.029	0.050
	0.240	*0.236*	*0.251*
lagged real GDP growth	-0.027*	-0.021	-0.008
	0.015	*0.018*	*0.017*
Instrument			
=1 if corp governance law enacted in calendar year *t*+2	-0.296	-0.373	0.001
	0.341	*0.338*	*0.203*
=1 if corp governance law enacted in calendar year *t*+1	0.085	0.104	-0.127
	0.224	*0.208*	*0.287*
=1 if corp governance law enacted in calendar year *t*	0.558***	0.577***	0.542***
	0.180	*0.221*	*0.204*
=1 if corp governance law enacted in calendar year *t*-1	0.610***	0.582***	0.366**
	0.160	*0.158*	*0.171*
=1 if corp governance law enacted in calendar year *t*-2 or earlier	0.354**	0.314*	0.276
	0.163	*0.190*	*0.202*
Diagnostics			
Wald test: all coeff. = 0 (χ^2)	326.4***	361.0***	390.4***
Pseudo-R^2	11.2%	10.4%	9.1%

Table 4. Naïve Performance Models.

To establish a baseline, we relate performance to board intervention ignoring the potential endogeneity of intervention. In other words, we estimate equation (2) in Section 1 without reference to equation (1). We expect the probit coefficient on intervention to be downward biased. To allow for lags in interventions affecting performance, we measure performance over the years *t* to *t*+2. (Results are robust to using shorter windows.) In columns 1 through 3, we measure performance using an indicator variable set equal to one if the portfolio firm is exited through an IPO or a sale, and zero otherwise. In columns 4 through 6, we additionally require that the IRR is strictly positive in the event of an exit. All specifications include country, industry, and time effects. The number of observations in columns 1-3 is 1,817. As we lack IRR data for one firm with three firm-years, the number of observations in columns 4-6 is 1,814. Heteroskedasticity-consistent standard errors, clustered on portfolio firm, are reported in italics beneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Dependent variable =1 if …					
	… exit in years *t* to *t*+2			… exit at positive IRR in years *t* to *t*+2		
Target of intervention:	CEO	top mgt	any mgt	CEO	top mgt	any mgt
	(1)	(2)	(3)	(4)	(5)	(6)
Board intervention						
=1 if management turnover at *t*	-0.363***	-0.373***	-0.430***	-0.334**	-0.284**	-0.402***
	0.135	*0.129*	*0.118*	*0.149*	*0.142*	*0.132*
Firm and mgmt performance						
interim performance score in *t*-1	0.164***	0.164***	0.163***	0.361***	0.361***	0.359***
	0.043	*0.043*	*0.043*	*0.044*	*0.044*	*0.044*
=1 if poor performance blamed on management in *t*-1	-0.031	-0.032	-0.014	-0.339	-0.341	-0.333
	0.208	*0.208*	*0.209*	*0.227*	*0.227*	*0.227*
=1 if poor performance blamed on bad luck in *t*-1	0.007	0.010	0.006	0.136	0.139	0.137
	0.112	*0.112*	*0.113*	*0.122*	*0.122*	*0.122*
=1 if management viewed as incompetent in *t*-1	0.347***	0.352***	0.362***	0.202	0.200	0.214*
	0.128	*0.128*	*0.128*	*0.126*	*0.125*	*0.126*
Deal characteristics						
log investment cost through *t*-1	0.051**	0.051**	0.050**	0.055*	0.055*	0.055*
	0.024	*0.024*	*0.024*	*0.031*	*0.031*	*0.031*
=1 if privatization	0.230	0.227	0.233	0.450**	0.445**	0.454**
	0.166	*0.166*	*0.165*	*0.206*	*0.206*	*0.205*
=1 if deal is staged	-0.368***	-0.366***	-0.365***	-0.231**	-0.231**	-0.228**
	0.095	*0.095*	*0.095*	*0.103*	*0.103*	*0.103*
Macroeconomic conditions						
lagged EBRD transition indicator	0.809***	0.812***	0.813***	0.375*	0.378*	0.381*
	0.202	*0.203*	*0.202*	*0.204*	*0.205*	*0.205*
lagged real GDP growth	0.042***	0.043***	0.043***	0.051***	0.052***	0.052***
	0.014	*0.014*	*0.014*	*0.016*	*0.016*	*0.016*
Diagnostics						
Wald test: all coeff. = 0 (χ^2)	146.3***	148.8***	150.9***	254.2***	251.8***	258.1***
Pseudo-R^2	9.4%	9.5%	9.7%	14.9%	14.9%	15.1%

Table 5.
Panel A: Reduced-form Performance Models.

To validate our instrument, we estimate reduced-form probit models of the performance equation, that is, we estimate the effect of law changes on performance controlling for our set of explanatory variables. Given that board intervention becomes more likely after changes in corporate governance laws, then if interventions improve performance, we should find a positive reduced-form relation between law changes and performance. To allow for lags in interventions affecting performance, we measure performance over the years *t* to *t*+2. (Results are robust to using shorter windows.) In columns 1 and 2, we measure performance using an indicator variable set equal to one if the portfolio firm is exited through an IPO or a sale, and zero otherwise. In columns 3 and 4, we additionally require that the IRR is strictly positive in the event of an exit. All specifications include country, industry, and time effects. The number of observations in columns 1 and 2 is 1,817. As we lack IRR data for one firm with three firm-years, the number of observations in columns 3 and 4 is 1,814. In columns 1 and 3, heteroskedasticity-consistent standard errors, clustered on portfolio firm, are reported in italics beneath the coefficient estimates. In columns 2 and 4, we include random portfolio-firm effects which rules out clustering. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Dependent variable =1 if …			
	… exit in years *t* to *t*+2		… exit at positive IRR in years *t* to *t*+2	
	(1)	(2)	(3)	(4)
Firm and mgmt performance				
interim performance score in *t*-1	0.165***	0.127**	0.361***	0.240***
	0.044	*0.058*	*0.044*	*0.068*
=1 if poor performance blamed on management in *t*-1	-0.036	-0.111	-0.349	-0.363
	0.203	*0.270*	*0.225*	*0.364*
=1 if poor performance blamed on bad luck in *t*-1	0.018	0.136	0.143	0.210
	0.112	*0.166*	*0.122*	*0.200*
=1 if management viewed as incompetent in *t*-1	0.284**	0.324*	0.124	0.308
	0.132	*0.195*	*0.126*	*0.238*
Deal characteristics				
log investment cost through *t*-1	0.033	0.124**	0.041	0.129**
	0.024	*0.049*	*0.030*	*0.054*
=1 if privatization	0.216	0.795*	0.452**	1.476***
	0.187	*0.438*	*0.218*	*0.542*
=1 if deal is staged	-0.413***	-1.093***	-0.268**	-0.978***
	0.098	*0.245*	*0.105*	*0.296*
Macroeconomic conditions				
lagged EBRD transition indicator	0.596***	4.168***	0.225	3.879***
	0.194	*0.354*	*0.200*	*0.433*
lagged real GDP growth	0.044***	0.177***	0.054***	0.217***
	0.014	*0.022*	*0.016*	*0.027*
Instrument				
=1 if corp governance law enacted	0.652***	1.678***	0.479***	1.540***
	0.123	*0.193*	*0.139*	*0.222*
Portfolio firms effects?	No	Yes	No	Yes
Diagnostics				
Wald test: all coeff. = 0 (χ^2)	164.2***	383.4***	261.2***	275.5***
Likelihood ratio test: portfolio-firm effects = 0 (χ^2)	n.a.	386.0***	n.a.	390.3***
Pseudo-R^2	10.9%	26.4%	15.6%	33.0%

Table 5.

Panel B: Reduced-form Performance Models in Amendment Countries Only.

In this panel, we test whether changes in corporate governance laws affect company performance independently of broader reforms of corporate law. We do so by re-estimating the reduced-form performance models of Panel A in the five countries that strengthened the power of the board relative to management through an amendment to a commercial law enacted some years earlier. As per Table 1, the five countries are Croatia, the Czech Republic, Estonia, Lithuania, and Russia. Each of these countries replaced its Soviet-era corporate law with Western-style corporate law *before* sample funds made any investments. Thus, the instrument isolates the effect of changes in corporate governance law on performance. All specifications include country, industry, and time effects. The number of observations is 605 in columns 1 and 2 and 602 in columns 3 and 4. In columns 1 and 3, heteroskedasticity-consistent standard errors, clustered on portfolio firm, are reported in italics beneath the coefficient estimates. In columns 2 and 4, we include random portfolio-firm effects which rules out clustering. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Dependent variable =1 if …			
	… exit in years *t* to *t*+2		… exit at positive IRR in years *t* to *t*+2	
	(1)	(2)	(3)	(4)
Firm and mgmt performance				
interim performance score in *t*-1	0.138*	0.020	0.365***	0.124
	0.076	*0.109*	*0.080*	*0.129*
=1 if poor performance blamed on management in *t*-1	0.573	0.771	0.734	1.118
	0.410	*0.644*	*0.465*	*0.879*
=1 if poor performance blamed on bad luck in *t*-1	-0.207	-0.329	-0.228	-0.585
	0.230	*0.372*	*0.266*	*0.462*
=1 if management viewed as incompetent in *t*-1	0.706***	0.400	-0.116	0.079
	0.275	*0.396*	*0.268*	*0.479*
Deal characteristics				
log investment cost through *t*-1	0.049	0.173**	0.018	0.141*
	0.038	*0.079*	*0.052*	*0.082*
=1 if privatization	1.094***	2.809**	1.757***	4.923***
	0.399	*1.323*	*0.420*	*1.460*
=1 if deal is staged	-0.603***	-1.727***	-0.265	-1.232**
	0.168	*0.443*	*0.174*	*0.513*
Macroeconomic conditions				
lagged EBRD transition indicator	0.859*	4.737***	0.730	4.781***
	0.516	*0.784*	*0.585*	*0.935*
lagged real GDP growth	0.049**	0.191***	0.052**	0.232***
	0.020	*0.036*	*0.026*	*0.046*
Instrument				
=1 if corp governance strengthened by amendment	0.680***	1.496***	0.586**	1.288***
	0.207	*0.351*	*0.229*	*0.421*
Portfolio firms effects?	No	Yes	No	Yes
Diagnostics				
Wald test: all coeff. = 0 (χ^2)	128.9***	138.5***	146.0***	109.6***
Likelihood ratio test: portfolio-firm effects = 0 (χ^2)	n.a.	118.8***	n.a.	115.1***
Pseudo-R^2	19.0%	33.4%	22.6%	38.3%

Table 6.
Panel A: Structural Performance Model Using Law Changes as an Instrument.

In columns 1 through 3, we measure performance using an indicator variable set equal to one if the portfolio firm is exited through an IPO or a sale, and zero otherwise. In columns 4 through 6, we additionally require that the IRR is strictly positive in the event of an exit. The models are estimated using a seemingly unrelated bivariate probit model that treats intervention as endogenous using the models shown in Table 3, Panel A. All specifications include country, industry, and time effects. The number of observations is 1,817 in columns 1-3 and 1,814 in columns 4-6. The Staiger-Stock test is a Wald test of the null hypothesis that the instrument does not correlate with board interventions. The critical value is 10. Heteroskedasticity-consistent standard errors, clustered on portfolio firm, are reported in italics beneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Dependent variable =1 if …					
	… exit in years t to t+2			… exit at positive IRR in years t to t+2		
Target of intervention:	CEO	top mgt	any mgt	CEO	top mgt	any mgt
	(1)	(2)	(3)	(4)	(5)	(6)
Board interventions						
=1 if management turnover at t	1.741***	1.613***	1.379***	1.967***	1.853***	1.648***
	0.226	*0.170*	*0.173*	*0.440*	*0.191*	*0.198*
Firm and mgmt performance						
interim performance score in t-1	0.167***	0.165***	0.165***	0.346***	0.345***	0.342***
	0.041	*0.041*	*0.040*	*0.042*	*0.040*	*0.040*
=1 if poor performance blamed on management in t-1	-0.020	-0.015	-0.049	-0.313	-0.294	-0.320
	0.192	*0.192*	*0.191*	*0.220*	*0.210*	*0.210*
=1 if poor performance blamed on bad luck in t-1	-0.007	-0.022	-0.010	0.106	0.087	0.102
	0.108	*0.108*	*0.108*	*0.120*	*0.119*	*0.118*
=1 if management viewed as incompetent in t-1	0.097	0.092	0.108	-0.037	-0.046	-0.029
	0.129	*0.126*	*0.125*	*0.133*	*0.117*	*0.114*
Deal characteristics						
log investment cost through t-1	0.043*	0.041*	0.046*	0.048*	0.046	0.052*
	0.023	*0.023*	*0.024*	*0.029*	*0.028*	*0.030*
=1 if privatization	0.109	0.136	0.130	0.325*	0.347*	0.323*
	0.165	*0.161*	*0.164*	*0.191*	*0.189*	*0.195*
=1 if deal is staged	-0.357***	-0.359***	-0.361***	-0.217**	-0.220**	-0.225**
	0.092	*0.090*	*0.090*	*0.099*	*0.096*	*0.096*
Macroeconomic conditions						
lagged EBRD transition indicator	0.755***	0.756***	0.747***	0.330	0.347*	0.348*
	0.196	*0.193*	*0.193*	*0.208*	*0.196*	*0.194*
lagged real GDP growth	0.043***	0.042***	0.041***	0.049***	0.050***	0.050***
	0.014	*0.014*	*0.014*	*0.017*	*0.015*	*0.015*
Diagnostics						
Wald test: all coeff. = 0 (χ^2)	949.5***	815.8***	619.9***	1072.3***	1143.5***	1022.2***
Exogeneity test (LR test)	23.8***	24.3***	19.9***	30.0***	29.0***	22.9***
Staiger-Stock (1997) test (χ^2)	15.5***	15.3***	18.0***	15.5***	13.5***	9.6***
Pseudo-R^2	10.4%	10.2%	9.9%	14.7%	14.3%	13.7%

Table 6.
Panel B: Structural Performance Model with Portfolio-firm Effects.

We repeat the analysis of Table 6, Panel A after including random portfolio-firm effects to control for time-invariant firm-level omitted variables which we allow to be correlated across the performance and intervention equations. See equations (3) and (4) in Section 4.4. In columns 1 through 3, we measure performance using an indicator variable set equal to one if the portfolio firm is exited through an IPO or a sale, and zero otherwise. In columns 4 through 6, we additionally require that the IRR is strictly positive in the event of an exit. The models are estimated using a seemingly unrelated bivariate probit model that treats intervention as endogenous using the models shown in Table 3, Panel A. All specifications include country, industry, and time effects. The number of observations is 1,817 in columns 1-3 and 1,814 in columns 4-6. The Staiger-Stock test is a Wald test of the null hypothesis that the instrument does not correlate with board interventions. The critical value is 10. Standard errors are reported in italics beneath the coefficient estimates. (Limdep does not support clustered standard errors for this model.) We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Dependent variable =1 if …					
	… exit in years t to t+2			… exit at positive IRR in years t to t+2		
Target of intervention:	CEO	top mgt	any mgt	CEO	top mgt	any mgt
	(1)	(2)	(3)	(4)	(5)	(6)
Board interventions						
=1 if management turnover at t	1.668***	1.602***	1.317***	1.582***	1.592***	1.597***
	0.132	*0.092*	*0.256*	*0.158*	*0.146*	*0.140*
Firm and mgmt performance						
interim performance score in t-1	0.134***	0.130***	0.124***	0.131**	0.150***	0.123**
	0.046	*0.045*	*0.048*	*0.057*	*0.058*	*0.054*
=1 if poor performance blamed on	-0.017	0.003	-0.043	-0.301	-0.332	-0.328
management in t-1	*0.216*	*0.233*	*0.266*	*0.309*	*0.410*	*0.287*
=1 if poor performance blamed on	0.127	0.074	0.146	0.184	0.174	0.182
bad luck in t-1	*0.140*	*0.137*	*0.148*	*0.169*	*0.179*	*0.173*
=1 if management viewed as	0.196	0.177	0.222	0.140	0.024	0.072
incompetent in t-1	*0.148*	*0.151*	*0.198*	*0.204*	*0.247*	*0.202*
Deal characteristics						
log investment cost through t-1	0.392***	0.312***	0.382***	0.445***	0.405***	0.400***
	0.038	*0.035*	*0.041*	*0.048*	*0.045*	*0.043*
=1 if privatization	1.066***	0.947***	1.419***	2.885***	3.341***	2.259***
	0.188	*0.176*	*0.208*	*0.252*	*0.269*	*0.224*
=1 if deal is staged	-1.531***	-0.892***	-1.654***	-2.074***	-2.029***	-1.998***
	0.112	*0.098*	*0.135*	*0.154*	*0.158*	*0.150*
Macroeconomic conditions						
lagged EBRD transition indicator	9.094***	9.076***	9.209***	10.418***	10.501***	10.406***
	0.411	*0.395*	*0.550*	*0.551*	*0.584*	*0.570*
lagged real GDP growth	0.227***	0.224***	0.226***	0.314***	0.307***	0.298***
	0.020	*0.019*	*0.023*	*0.027*	*0.027*	*0.027*
Diagnostics						
Wald test: all coeff. = 0 (χ^2)	828.5***	1226.5***	740.8**	802.9***	701.7***	732.2***
Exogeneity test (LR test)	402.6***	415.8***	399.2***	419.2***	421.4***	420.0***
Staiger-Stock (1997) test (χ^2)	22.0***	27.9***	8.9***	8.7***	10.2***	9.5***
Pseudo-R^2	21.8%	21.8%	20.6%	27.4%	26.7%	25.7%

Table 7. Effect of Board Intervention on Interim Performance Scores.

The EBRD regularly reviews the performance of each portfolio firm, resulting in a monitoring report based on both quantitative and qualitative criteria. We use the monitoring reports to construct a variable that captures the performance of each company each year relative to its annual budget. Specifically, portfolio firms are scored from 1 to 5, where 3 denotes performance in line with expectations (as set out in the company's budget for the year); 4 and 5 denote performance above and greatly above expectations; and 2 and 1 denote underperformance and severe underperformance relative to expectations. We estimate linear least-squares models with an integer dependent variable; results are somewhat stronger if we use a logistic transform of the dependent variable instead. Columns 4-6 are the second stage of Heckman (1978) treatment models that treat intervention as endogenous using the models shown in Table 3, Panel A as the first stage. All specifications include country, industry, and time effects. Due to the lagging structure, the number of observations is 1,154. The Staiger-Stock test is a Wald test of the null hypothesis that the instrument does not correlate with board interventions. The critical value is 10. Heteroskedasticity-consistent standard errors, clustered on portfolio firm, are reported in italics beneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Dependent variable: Interim performance score in year t+1					
	Naïve OLS			Heckman treatment model		
	CEO change	Top mgmt change	Any mgmt change	CEO change	Top mgmt change	Any mgmt change
	(1)	(2)	(3)	(4)	(5)	(6)
Board interventions						
=1 if management turnover at t	0.078	-0.045	-0.033	1.410***	1.037**	0.877*
	0.112	*0.108*	*0.093*	*0.367*	*0.462*	*0.490*
Firm and mgmt performance						
interim performance score in t-1	0.231***	0.229***	0.229***	0.258***	0.254***	0.254***
	0.036	*0.036*	*0.036*	*0.037*	*0.038*	*0.040*
=1 if poor performance blamed on management in t-1	-0.152	-0.152	-0.151	-0.155	-0.152	-0.185
	0.151	*0.153*	*0.152*	*0.145*	*0.147*	*0.148*
=1 if poor performance blamed on bad luck in t-1	0.241**	0.240**	0.240**	0.256**	0.234**	0.250**
	0.101	*0.101*	*0.100*	*0.104*	*0.102*	*0.104*
=1 if management viewed as incompetent in t-1	0.048	0.063	0.061	-0.108	-0.064	-0.056
	0.105	*0.105*	*0.105*	*0.123*	*0.121*	*0.123*
Deal characteristics						
log investment cost through t-1	0.040**	0.040**	0.040**	0.036**	0.035*	0.042**
	0.018	*0.018*	*0.018*	*0.018*	*0.018*	*0.019*
=1 if privatization	0.251*	0.251*	0.252*	0.245	0.257*	0.237
	0.143	*0.142*	*0.142*	*0.155*	*0.150*	*0.145*
=1 if deal is staged	-0.074	-0.073	-0.073	-0.090	-0.086	-0.086
	0.071	*0.071*	*0.071*	*0.074*	*0.073*	*0.072*
Macroeconomic conditions						
lagged EBRD transition indicator	-0.160	-0.157	-0.157	-0.188	-0.189	-0.202
	0.139	*0.139*	*0.139*	*0.139*	*0.139*	*0.143*
lagged real GDP growth	0.003	0.003	0.003	0.006	0.004	0.002
	0.012	*0.012*	*0.012*	*0.012*	*0.012*	*0.012*
Diagnostics						
Wald test: all coeff. = 0 (χ^2)	7.2***	7.2***	7.1***	228.9***	233.2***	242.5***
Wald test: independent equations	n.a.	n.a.	n.a.	9.4***	4.7**	3.2*
Staiger-Stock (1997) test (χ^2)	n.a.	n.a.	n.a.	17.6***	16.4***	15.6***
Adjusted R^2	12.9%	12.8%	12.8%	n.a.	n.a.	n.a.

Table 8. Robustness Tests.

This table reports variations on the bivariate probit specifications reported in Table 6. In Panel A, we redefine the horizon over which exit is measured. Panel B restricts the sample to investments made before 2000 to investigate robustness to right-censoring. Only six of the 244 pre-2000 investments are right-censored (in the sense of remaining alive at the end of our sample period, December 2008), compared to 49 such investments in the sample as a whole. Panels C and D restrict the sample to the set of countries with in-sample variation in corporate governance laws and the five countries with the most investments, respectively. See Table 3, Panel B for further details. In Panel E, we replace the instrumental variable with a set of five indicator variables to capture the timing of the law changes relative to the panel year in question. Specifically, the board intervention equation now relates intervention to whether a law change took place two or more years earlier, one year earlier, in the same year, one year later, or two years later. (The omitted category is law changes that took place more than two years later.) In Panel F, we allow for country-level heterogeneity in the effect of law changes on management turnover by including country-specific law change indicators in the intervention equation. To save space, each panel reports only the coefficient estimates and standard errors for the intervention variable in the performance equation. All other covariates shown in Table 6 are included in the estimation but not shown. In Panels A, E, and F, the number of observations is 1,817 in columns 1-3 and 1,814 in columns 4-6. In Panel B, the number of observations is 1,133 in all columns. In Panel C, the number of observations is 1,483 in columns 1-3 and 1,480 in columns 4-6. In Panel D, the number of observations is 1,489 in all columns. Heteroskedasticity-consistent standard errors, clustered on portfolio firm, are reported in italics beneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Dependent variable = 1 if exit			Dependent variable = 1 if exit at positive IRR		
Target of intervention:	CEO	top mgt	any mgt	CEO	top mgt	any mgt
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Alternative exit horizons						
exit in years *t* to *t*+1	1.953***	1.751***	1.353***	1.936***	1.850***	1.614***
	0.629	*0.276*	*0.239*	*0.305*	*0.317*	*0.225*
exit in year *t*	1.676***	1.360***	1.194***	1.610***	1.413***	1.286***
	0.645	*0.510*	*0.459*	*0.547*	*0.479*	*0.417*
Panel B: Sub-period results						
investments made before 2000	1.558***	1.547***	1.499***	1.957***	1.937***	1.898***
	0.246	*0.210*	*0.170*	*0.134*	*0.136*	*0.127*
Panel C: Country exclusions						
exclude countries without within-sample-period law changes	1.756***	1.629***	1.353***	1.958***	1.860***	1.661***
	0.434	*0.214*	*0.179*	*0.217*	*0.192*	*0.190*
Panel D: Most active countries						
five most active countries	1.553***	1.468***	1.206***	1.918***	1.856***	1.571***
	0.277	*0.243*	*0.245*	*0.335*	*0.217*	*0.238*
Panel E: Time-varying law changes						
IV uses indicators for law change in year *t-i*, *i* = –2, –1, 0, +1, +2	1.789***	1.663***	1.442***	1.917***	1.858***	1.662***
	0.384	*0.201*	*0.182*	*0.241*	*0.213*	*0.215*
Panel F: Heterogeneous effects						
use country-level law change indicators	1.690***	1.594***	1.406***	1.926***	1.840***	1.646***
	0.174	*0.151*	*0.160*	*0.267*	*0.194*	*0.199*