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ABSTRACT

Monitoring Managers: Does it Matter?

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 ‘hard’ information about the board’s performance expectations and ‘soft’ information about board and CEO actions and the board’s beliefs about CEO competence in 473 mostly private-sector companies backed by private equity funds between 1993 and 2008. We find that CEOs are fired when the company underperforms relative to the board’s expectations, suggesting that boards use performance to update their beliefs. CEOs are especially likely to be fired when evidence has mounted that they are incompetent and when board power has increased following corporate governance reforms. In contrast, CEOs are not fired when performance deteriorates due to factors deemed explicitly to be beyond their control, nor are they fired for making 'honest mistakes.' Following forced CEO turnover, companies see performance improvements and their investors are considerably more likely to eventually sell them at a profit.

JEL Classification: G24, G32, G34, K22, O16 and P21
Keywords: boards of directors, CEO turnover, corporate governance, large shareholders, legal reforms, private equity and transition economies

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Over the past two decades, regulators and stock exchanges in many developed countries have strengthened corporate governance codes on the assumption that an active board of directors can help improve a firm's performance. (For relevant surveys, see Shleifer and Vishny (1997) and Becht, Bolton, and Roëll (2003).) This assumption is a mainstay of the theory literature on large shareholders and boards (surveyed in Tirole (2006) and Hermalin and Weisbach (2003)), 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 firm (see Gillan and Starks (2000) and Brav et al. (2008)), presumably because investors foresee an improvement in performance.

How well do boards of directors discharge their duties to monitor and hold managers to account? Answering this straightforward question has proved challenging, for two reasons. The first is a data problem: To any outside observer, corporate governance is essentially a black box. While the board may learn the CEO's ability over time and observes many of his actions, researchers can at best infer the board's beliefs from occasional interventions, such as firing the CEO. And even then we usually cannot be certain that the CEO was indeed fired, as opposed to leaving voluntarily.

Even if we had access to the board's information set, it would still be challenging to show empirically that board interventions, such as firing the CEO, improve performance. The problem is that we do not observe the counterfactual, that is, what performance would have been had the board not intervened. To identify the 'treatment effect' of board interventions, researchers instead typically compare the performance of firms with interventions to that of those without. But this leads to a selection problem: Boards presumably intervene in poorly performing firms. Thus, a comparison of firms with and without interventions likely picks up unobserved differences in the quality of management or the firm, not just the effect of an intervention. In the cross-section, we might even find that firms whose boards have fired the CEO continue to underperform their peers, but of course such underperformance would not have been *caused* by the intervention.

In this paper, we examine under what circumstances boards discipline CEOs and whether such interventions improve performance. We exploit confidential data obtained from the European Bank for Reconstruction and Development (EBRD) for 473 companies backed by 43 private equity funds in which the EBRD was an investor. The unique features of these data are that we know exactly what the board does and that we can closely approximate the board's information set.

We show that boards update their beliefs about a CEO's ability both from 'hard' information, specifically, how the company has performed relative to the board's expectations, and from 'soft' (i.e., unverifiable) information obtained, for example, through interacting with the CEO. Moreover, boards act on their beliefs: Poor performance relative to expectations or the realization that the CEO is incompetent leads to firing, and usually does so fairly rapidly. However, boards take no action in response to observations that are uninformative about the CEO's ability: CEOs are not fired if, in the board's view, poor performance was the result of bad luck or of a managerial decision that was wrong ex post but reasonable ex ante. Finally, we find little evidence that CEOs engage in moral-hazard behavior such as shirking, failure to control costs ('enjoying the quiet life'; Bertrand and Mullainathan (2003)), or 'building empires,' and none that boards fire CEOs in response.

These findings suggest that active monitoring by the board serves, in large part, to solve an adverse selection problem. When a CEO is hired, his true ability and the match between his skills and the company's needs are not perfectly known. Over time, the board receives soft and hard information and, once it has learnt his true ability, takes corrective action if necessary.

This view of the board's role is consistent with theory models that stress the importance of CEO ability rather than moral hazard (for a prominent example, see Hermalin and Weisbach (1998)). In a pure moral-hazard model à la Holmström (1979), by contrast, there is no uncertainty about the CEO's ability and the board's role is instead to induce optimal effort. If the board is unhappy with the CEO's effort, or his actions more generally, it simply adjusts his incentives. A pre-commitment to fire the CEO following poor performance would strengthen incentives. But if pre-commitment is impossible, firing would never be optimal ex post in the presence of firing or replacement costs. In fact, it would be pointless, since his replacement would behave the same way when given the same incentives (since all CEOs are equally able). Even if pre-commitment were possible, knowing he would be fired, the CEO would never shirk and hence in equilibrium we would still not observe any firing. In short, firing in equilibrium requires an element of initial uncertainty about the CEO's type and subsequent learning by the board. Our finding that CEOs are fired for incompetence rather than moral-hazard behavior, which anyway appears to be rare in our data, is consistent with this view.

Does board intervention improve performance? When we naïvely relate performance to CEO turnover in the cross-section, we find that it is significantly *negatively* related to performance. This provides evidence of the selection bias discussed earlier. 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, many transition economies have replaced Soviet-era with Western-style corporate law, in the process strengthening corporate governance and, especially, the powers of the board relative to the CEO. We use the staggered adoption of such laws across 19 countries as an instrument for board intervention.[1] The benefit of focusing on transition economies is that they experienced dramatic variation in the laws governing board actions, which greatly improves identification. In developed countries such as the U.S., in contrast, there is relatively little variation in corporate governance rules over time.[2]

Our identification strategy assumes that boards are more likely to intervene after the adoption of a law empowering the board to dismiss the CEO than before. Empirically, we find that CEO turnover increases substantially, from around 3% to more than 13% a year, when the law changes. Identification also requires that changes in board power affect performance only through their effect on CEO turnover. A leading concern is that greater board power makes it harder for CEOs to get away with moral-hazard behavior such as shirking. The law changes might thus induce better

[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.

performance directly, as CEOs react to an increased threat of being fired, violating the exclusion restriction for identification. However, this seems unlikely. As discussed, our results indicate that boards fire CEOs for being incompetent, not for behavior that could be altered through better incentives. Moreover, boards rarely even complain about moral-hazard behavior in our data.

Our data also do not support three other potential violations of the exclusion restriction. First, the staggered adoption of the laws across countries mitigates the concern that the law changes may have coincided with other beneficial country-level shocks that in turn affected performance. (In addition, we include controls for macroeconomic conditions and a country's progress towards a market economy to control directly for contemporaneous shocks.) Second, 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 firm-level performance affected the timing of reforms, which casts doubt on lobbying and other reverse-causality stories.

Once we use the natural experiment, the effect of CEO turnover on performance flips sign and becomes positive. The point estimates are not only statistically significant, they are also economically large. To illustrate, firing the CEO nearly doubles the probability that a sample firm will be successful on one performance measure and on another helps a firm move from underperforming the board's expectations to meeting its targets the following year.

A limitation of our analysis is that we cannot say whether and to what extent our results generalize to developed countries. Similarly, the nature of our data is such that our results may only pertain to firms with large, sophisticated shareholders. Thus, we cannot say whether firms 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.

This paper contributes to the economic literatures on corporate governance, law and finance, VC, and economic development. Our results illustrate that monitoring matters, in the sense that it helps boards learn a CEO's ability, rather than relying solely on a noisy output measure (i.e., performance). They also 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 discipline the CEO. This suggests that a legal framework that supports shareholders can make active monitoring more effective and is in line with the law and finance literature. However, while that literature has mainly argued this point through cross-country comparisons, we can exploit within-country variation to identify the channel more directly.

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.[3] Hellmann and Puri (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 literature on boards.

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 can have a strong influence on corporate performance, at least in the presence of sophisticated investors, such as the private equity funds in our dataset.

1. Sample and Data

The EBRD was established to assist formerly Communist countries in transitioning to a market economy. As a part of this mission, the EBRD has sought to foster the emergence of a professional private equity industry by investing in funds with a focus on transition economies. We have detailed data for 43 such funds, which in turn made (mostly minority) investments in 473 private-sector

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[3] These authors argue that the supply of VC capital is local and hence use the local availability of experienced investors as an instrument for supportive actions taken by their sample funds. This identification strategy would not work in our context, because most sample funds invest in multiple countries and some invest from afar (say, from London), rather than locally.

companies across 19 transition economies in Central and Eastern Europe and the former Soviet republics in Central Asia. We estimate that sample funds account for around two-thirds of all private equity funds focusing on transition economies over our sample period.[4]

Sample funds are private-sector investment partnerships with standard profit-maximizing objectives. They differ from those studied in the venture capital literature only in that they have a regional focus on transition economies, as the following fund description illustrates:

> *"[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."*

The 43 sample funds were raised between 1992 and 2004. Their sizes range from €15.3 million to €300.8 million, with an average of €93.1 million – comparable to VC funds in the U.S. (see Hochberg, Ljungqvist, and Yu (2007)). Thirty-three of the funds were sponsored by Western (mostly U.S. or U.K.) fund managers; the remaining ten were sponsored by local managers.

The earliest portfolio investment dates from 1993. We ignore investments made after 2005 as it is too soon to measure their performance. Table 1 provides a breakdown of the sample by country and year of initial investment. The five most active countries are Poland (with 171 investments), Russia (75), Hungary (59), the Czech Republic (44), and Romania (30). Nine countries – Azerbaijan, Georgia, Kazakhstan, Latvia, Moldova, Serbia-Montenegro, Slovenia, Turkmenistan, and the Ukraine – are home to fewer than 10 investments each. Across all countries, the number of deals increases from 14 in 1993 to 59 in 2000 and then falls to 20 in 2005.

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 December 2008, when our data end. Of the 473 sample investments, 319 (67.4%) were 'exited' (i.e., sold to a strategic acquirer or through an IPO),

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[4] The EBRD invested in almost all funds meeting the minimum due-diligence requirements established by a firm of investment consultants, Cambridge Associates, alleviating sample selection concerns. For an overview of these requirements, see http://www.ifc.org/ifcext/cfn.nsf/AttachmentsByTitle/Patricia+Dinneen/$FILE/Panel4-PatriciaDinneen.pdf.

95 (20.1%) were written off, and 59 (12.5%) remained in the funds' portfolios as of 2008.

Tracking each investment across time gives us an unbalanced panel of 2,616 firm-years. Accounting for the right-censoring caused by the 59 investments that remain alive as of December 2008, the average (median) firm 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 firm's IPO, sale, or liquidation. This allows us to compute the lifetime profitability of each investment. We measure profitability as the time-weighted return on investment, i.e., as the internal rate of return (IRR), calculated before the fees funds charge their investors. This averages 7.5% p.a.

Sample investments come from a wide range of industries. The EBRD classifies firms into 11 industries: Telecoms and media (90 firms), manufacturing (71), high-tech, electronics, and internet (69), services, hotels, and restaurants (57), retail (51), food & beverages (49), construction (27), oil, gas, and mining (20), financial services (18), pharmaceuticals and medical (16), and energy (5).

Funds typically make minority investments. Their average (median) equity position is 33.7% (26%), and 372 of the 473 deals (78.6%) are minority investments.[5] Systematic data on CEO ownership or their incentive contracts are not available.

In order to foster best investment practice, the EBRD 'audits' the portfolio firms of each fund it has invested in, usually twice a year. This involves EBRD staff collecting detailed information about performance, material events, board decisions, etc. from fund managers' quarterly reports to their investors, interviews with fund managers and CEOs, the confidential notes of the funds' representatives on portfolio company boards, audited and internal financials, and site visits, and through the EBRD's own representation on the funds' advisory boards or investment committees.

[5] We do not attempt to model the determinants of fund managers' investment strategies, or the causal effects of ownership on performance. Ownership is endogenous, reflecting in part firm characteristics that we do not observe. Thus, modeling ownership would require another instrument.

Audits result in candid, confidential, and standardized 'monitoring reports' to which we have been given unrestricted access.[6] Monitoring reports contain both 'hard' information, such as accounting data, and 'soft' information concerning key developments, the quality of the CEO, etc.

2. Empirical Model

2.1 Identification and Estimation

We seek to estimate the conditions under which a board intervenes in a firm by firing the CEO as well as the effect such an intervention has on the firm's subsequent performance. Specifically, we relate intervention in year *t* to the board's information set dated *t*–1, denoted I_{t-1}, which includes the firm's lagged performance relative to the board's expectations and the board's assessment of the CEO's actions and competence and the influence of bad luck, if any, on performance:

$$intervention_t = \gamma_1' I_{t-1} + \delta_1' x_{1,t-1} + \varphi_1'(\lambda_k + \lambda_t + \lambda_j) + u_{1,t} \quad (1)$$

The *x*'s include other exogenous controls while the λ's are fixed effects for country *k*, year *t*, and industry *j*. We estimate this equation both with and without portfolio-firm effects. Firm effects help control possible omitted-variable biases due to unobserved firm heterogeneity, caused for example by lack of data on CEO ownership or compensation. To the extent that such omitted variables are constant over time (the CEO owns a majority of the equity in every panel year) they can effectively be removed using firm effects. However, their inclusion makes little difference in practice.

To test if intervention in turn improves performance, we estimate the following equation:

$$performance_t = \beta_2 intervention_t + \gamma_2' I_{t-1} + \delta_2' x_{2,t-1} + \varphi_2'(\lambda_k + \lambda_t + \lambda_j) + u_{2,t} \quad (2)$$

While eq. (1) is straightforward to estimate as long as we have good data on the board's information set, estimating eq. (2) is trickier. It is likely that the disturbances of eqs. (1) and (2) are correlated. Presumably, boards intervene when firms are badly managed or perform poorly and such firms will presumably underperform better-managed firms in future (even if they perform better than

[6] While monitoring reports are standardized, reviewers vary in the level of detail they record. As long as assignments of firms 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 particular fund's investments, say the underperforming ones), and the main assignment criterion is that, if possible, she speak the language of the country the fund invests in.

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 that does not belong in x_2.

2.2 Instrument

Our instrument exploits plausibly exogenous variation in the timing of the introduction of laws governing the relationship between the CEO and the board in transition economies. The identifying assumption is that a CEO is more likely to be fired after the adoption of a law empowering the board to dismiss management than before. The exclusion restriction requires that the law change affect performance only via board intervention. We investigate the validity of the instrument in Section 4.2 and the plausibility of the exclusion restriction in Sections 5.3 and 5.4.

Because we focus on transition economies, the legal change we exploit is not a subtle one. The notions that the CEO serves at the pleasure of the board, and that the board's role is to monitor the CEO, do not exist in Soviet-era laws. Reviewing the state of corporate governance in Azerbaijan, for example, the World Bank commented in 2005 that:

> *"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."*[7]

Table 2 provides an overview of the applicable legal changes and lists the source texts we consulted. All but one of our sample countries – Azerbaijan – adopted corporate governance reforms. There are two types of reform. Boards can either be given statutory power to dismiss the CEO or the law can reserve this power for the shareholders' meeting but allow for it to be delegated to the board by an amendment to the corporate charter. Our instrument is based solely on reforms that increase the board's statutory power. This is the type of reform adopted in 13 of the 19 sample countries, accounting for 351 of the 473 sample firms.

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[7] Quoted from http://www.worldbank.org/ifa/rosc_cg.html, accessed August 2008.

The reason we do not include reforms requiring a charter amendment in the construction of the instrument is as follows. To change the charter post-reform, a sample fund needs a majority of the votes in the annual shareholders' meeting. To dismiss the CEO pre-reform, it also needs a majority of the votes in the shareholders' meeting. Thus, reforms that require a charter amendment do not affect the distribution of power within the firm and so should have no effect on CEO turnover. In Section 4.2, we test this prediction.

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. It shows the annual rate of CEO turnover relative to the year boards in the country in question were given the statutory power to dismiss CEOs. In the five years prior to reform, CEO turnover is low: CEOs are fired on average in only 3.2% of firm-years. In the year the law changes, CEO turnover jumps to 13.3% and then stays on average at 8.2% a year for the next five years.

2.3 Further Testable Implications

Before a law change, firing the CEO requires a majority of shareholders to vote in favor of a resolution dismissing the CEO at the shareholders' meeting, so control requires majority ownership. After a law change, the CEO can be dismissed by a majority vote of the board, so a minority shareholder can effect CEO turnover if he can persuade a majority of the board to vote with him. The following two examples, taken from the EBRD's monitoring reports of two different Polish companies, illustrate how reform redistributes power from the shareholders' meeting to the board:

> *Pre-law change: "The fund manager's efforts to [dismiss management] were unsuccessful so far, as other shareholders rejected the Fund's motion in this respect put forward at the Shareholders Meeting."*

> *Post-law change: "[The fund manager] continues to be disappointed with … management, but has been unable to convince a sufficient number of the Directors of the Company to replace them. [The fund manager] has made its displeasure of managerial and financial issues known to the Company … [and] has put the entire Board … on notice as to certain issues that [the fund manager] believe[s] would give cause for the Board to remove the CEO."*

One testable implication of power shifting from the shareholders' meeting to the board is that the importance of soft information in CEO firing decisions should increase following reform, compared to that of hard information. By its very nature, soft information is unverifiable and so can more easily be communicated to board members (a majority of whom can fire a CEO after a law change) than to shareholders at large (a majority of whom have to vote in favor of firing the CEO before a law change). Hard information, on the other hand, could be effective in either case.[8]

Furthermore, we predict that soft information becomes more important after a law change *only if* the fund is a minority shareholder. A majority shareholder can fire a CEO at will, so changes in the board's statutory powers should not affect whether a CEO is fired for negative soft information.

3. Variable Definitions and Descriptive Statistics

3.1 Board Intervention Measures

Unlike the extant CEO turnover literature, we are able to distinguish unambiguously between voluntary and forced departures, based on the monitoring reports. Here are examples of each:

> *Voluntary: "The general manager of four years, who was deemed highly capable …, has left and a replacement has just been hired."*
>
> *Forced: "At the year end the board of the company decided to change the CEO."*

It is clear from the EBRD's monitoring reports that boards typically complain of management problems some time before firing, rather than after the fact. For example:

> *"The relationship with the company's Managing Director … remains difficult. [He] is unable to adjust to the needs and opportunities now arising ... Going forward, [he] would need to be replaced as a General Manager."*
>
> *"It has been agreed that [the CEO] would have to leave in the near future. [He] has disappointed with his lack of leadership, exaggerated and biased involvement in … debates … and overall lack of discipline and political skill."*

Our data contain 178 instances of forced CEO departures, only 42 of which (24%) occurred prior to corporate governance reform (see Table 2). The average (median) CEO dismissal takes place 3.4 (3) years after a fund first invested in a portfolio firm. Most firms (330) have no forced CEO turnover; those that do typically fire only one CEO over the sample period (116 firms), though some fire two CEOs (19 firms) or even three (eight firms), usually a few years apart. Including voluntary CEO departures, the sample contains 659 separate CEOs.

The monitoring reports also record 169 dismissals of managers below the rank of CEO and 201 instances of boards authorizing the hiring of additional managers (referred to as 'management

[8] Note that we lack data on the composition of the board and the number of board seats a fund manager controls.

strengthening'). In a start-up firm, the latter might involve hiring a CFO; in a later-stage investment, it could be hiring an export manager. We use this information to examine the plausibility of our instrument. We expect that CEOs rarely resist new hiring, while they do resist being fired, so our instrument should correlate with CEO dismissals but less so with management strengthening.

3.2 Hard Information: Board Performance Expectations

Our data allow us to measure each firm's performance relative to the board's expectations. At the beginning of each year, the board and management negotiate a budget for the year ahead which contains sales and profit targets as well as strategic and investment plans. At the end of each year, the EBRD records how the firm has performed relative to budget. For about a quarter of the firm-years, the EBRD monitoring reports mention both budgeted and actual numbers, and in around 80% of firm-years, the EBRD reviewer (who has access to both sets of numbers but doesn't necessarily mention them in the monitoring report) provides at least a qualitative assessment. To maximize the useable sample size, we use both pieces of information to score portfolio firms on a five-point scale, where 3 denotes performance in line with budget; 4 and 5 denote performance above and greatly above expectations; and 2 and 1 denote underperformance and severe underperformance relative to expectations. Here are two examples coded as 2s:

> *"Sales were 9% below budget; EBITDA was negative as opposed to US$2.8m budgeted profit."*

> *"Sales and earnings were below expectations due to a market slump that followed a big increase in VAT."*

The scores average 2.8 and are distributed fairly normally: 9.5% of investments score a 1, 33.4% a 2, 31.2% a 3, 20.7% a 4, and 5.2% score a 5. Thus, boards appear to have realistic, achievable expectations which in turn form a suitable benchmark against which we can judge a firm's performance. The following quote illustrates that boards do indeed aim for realistic budgets:

> *"The budgeted numbers prepared by management for … 2004 are sales of $242 million and EBITDA of $32 million. [H]owever, the … budget was challenged by the Board … and not approved due to concerns [about] how achievable these targets are. Currently management is reworking the operational budget for 2004."*

3.3 Soft Information

The EBRD monitoring reports also contain fund managers' confidential comments regarding the

CEO's ability and the fund manager's perception of what, if anything, may have caused poor performance. We capture this soft information using three time-varying indicators. The first equals one if in a given year the fund manager views the CEO as incompetent or thinks that his skills are a poor match for the firm's needs, and zero otherwise. Representative examples include:

> *Competence: "The top management team is strong."*
>
> *Incompetence: "It is now evident that the CEO lacks sufficient skills in some areas and we are searching for a suitable candidate to complement the current CEO in the senior management team."*
>
> *Incompetence: "Given the more competitive environment on the Polish post-Accession market, the Fund Manager sees the need for a more efficient sales and marketing strategy. The CEO is being replaced with someone more competent in these areas effective January 1, 2006."*

On average, CEOs are viewed as incompetent or a poor match in 6.7% of firm-years. In total, boards complain about the competence of 132 of the 659 CEOs in the sample (20%). Of these, 82 (62.1%) are eventually fired and 17 (12.0%) lead their firms into bankruptcy. The remaining 33 are never fired, perhaps because the board lacked the statutory power to do so (15 cases) or, more speculatively, because the fund manager couldn't muster the necessary number of votes.

Does this proxy correlate with true ability, or do fund managers record concerns only when they have already decided to fire a CEO? If concerns are recorded strategically, we expect to see more complaints once board power has increased. In Figure 2, we graph the annual rate of fund managers rating the CEO as incompetent, where time is measured relative to the year in which the country in question reformed its corporate governance laws. Boards complain just as frequently after a law change as before (namely, 7% of the time), suggesting that fund managers record their views of CEO competence regardless of whether they have the statutory power to act on them. In that sense, this piece of soft information is likely to correlate with true ability. It also suggests that by the time the law is changed, there is pent-up pressure to fire CEOs, consistent with the jump in CEO turnover we saw in Figure 1. This, of course, is the identifying assumption behind our instrument.

Our second indicator captures cases in which a fund manager is critical of a CEO's actions or decisions which he blames for the firm's underperformance. The main complaint concerns honest mistakes, as the following examples illustrate:

> *"Management made a serious mistake and signed FX options to hedge against the strengthening PLN [Polish zloty] shortly before the currency substantially weakened."*

> *"[This food manufacturer] had a bad year and will end 2003 below both last year's exceptionally good performance and this year's budget. The company allocated insufficient funds for marketing and sales support, focusing instead on better management of the existing freezer network through a team of temporary merchandisers. This proved a flawed strategy."*

Relative to the sample size of 2,616 firm-years, we find few complaints about traditional moral-hazard behavior, such as CEOs not working hard enough (seven firms), enjoying the quiet life (12 firms), engaging in self-dealing (seven firms), or building empires (one firm). The small number of such complaints could suggest that CEO compensation contracts and board monitoring are, on the whole, effective at controlling moral hazard problems, at least in the presence of private equity funds such as those in our dataset. Categorizing these complaints necessarily involves some subjective judgment on our part. Among these rare cases, the following examples are typical:

> *Shirking: "The CEO has not been properly managing the business."*

> *Enjoying the quiet life: "Management was unable to control wages and salaries which were 8% higher than in the first half of 98, despite the fact that the employment was reduced by approx. 20% in 4Qtr 98."*

> *Self-dealing: "… management was discovered to produce false invoices to inflate the 1997 results."*

> *Empire-building: "The company made four acquisitions in 1998 with one additional acquisition in 1Q99. The fund admits it gave [the CEO] excessive free-hand in the acquisitions."*

The third indicator captures 326 cases where, in the board's opinion, underperformance was caused by factors beyond management's control ('bad luck'). For example:

> *"On 10 September the finished goods warehouse … caught fire. The fire completely destroyed the company's warehouses as well as the main [production] facility."*

3.4 Deal Characteristics

Converted using historical exchange rates, the average and median investment costs (i.e., size) are €6.1 million and €4.1 million, respectively, ranging up to €43.4 million. These numbers are comparable to investments made by U.S. venture capitalists. The average and median 'post-money' valuations are €65.7 million and €15.7 million, respectively. Most investments fund expansion at private-sector firms (255 out of 473) or go into start-ups (124 deals). Only 40 of the 473 portfolio firms are privatizations. Our sample is thus quite different from the types of firms studied in the transition-economics literature (see Djankov and Murrell (2002) and references therein).

Riskier firms likely require more intervention and have systematically different performance. Traditional risk proxies, such as the volatility of equity returns or operating cash flows, cannot be computed in our sample as few portfolio firms are stock-market listed and the accounting data mentioned in the monitoring reports have many gaps. Instead, we (crudely) proxy for risk based on the fund's investment strategy. 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 473 investments, 125 were staged.

3.5 Macroeconomic and Institutional Conditions

To rule out confounding influences that are contemporaneous with the instrument, we control for macroeconomic and institutional conditions using real GDP growth[10] and the EBRD transition indicator.[11] The latter measures progress towards a market economy in a range of categories, such as 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.

4. Determinants of Board Interventions

We model the determinants of three types of board interventions: The dismissal of the CEO, the firing of a junior manager (anyone below the rank of CEO), and actions designed to strengthen the management team through new hiring. Corporate governance reform is not a necessary condition for the last two types of intervention, so we expect the instrument to have no effect.

We relate intervention in year *t* to the board's information set as of *t*-1, consisting of hard

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

[10] The data come from the EBRD (http://www.ebrd.com/country/sector/econo/stats/sei.xls, accessed February 2009). 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.

[11] See http://www.ebrd.com/country/sector/econo/stats/sci.xls, accessed February 2009. Glaeser, Johnson, and Shleifer (2001) use the EBRD indicator to measure reform in Poland and the Czech Republic.

information (performance relative to the board's expectations) and soft information (the board's view of CEO competence, CEO decisions, and bad luck). We control for deal characteristics, lagged macroeconomic conditions, a limited set of country effects,[12] and a full set of year and industry effects. We also include the instrument which equals one if, at the beginning of year *t*, there is a law in place empowering the board to dismiss the CEO, and zero otherwise. (We will explore other timing conventions later.) 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 firm level.[13]

4.1 Results

Table 3 reports the results. Column 1 shows that a board is more likely to fire the CEO after a year of poor performance relative to the board's expectations.[14] The effect is large, both economically and statistically: To illustrate, a unit drop in lagged performance (say, from meeting expectations to performing below expectations) increases the probability of CEO dismissal by 12.9 percentage points, or 189%, from the unconditional probability of 6.8% (p<0.001). Further lags (not tabulated) are not significant, suggesting that boards take disciplinary action quickly. Virtually identical results obtain when we include portfolio-firm effects, which are in fact not statistically significant (p=0.192; see column 2).

We interpret this finding as evidence that boards use performance signals to update their beliefs about CEO type. Moreover, they appear to do so in a considered way: The next two coefficient estimates in column 1 suggest that boards do *not* fire a CEO if they attributed the previous year's

[12] As Table 1 makes clear, a full set of country dummies would over-determine our equations. Instead, we control for the five most active countries. Our results are not sensitive to reasonable alternatives.
[13] 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.
[14] For related findings, 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.

poor performance to bad luck or to managerial decisions.[15] The absence of sanctions for making mistakes suggests that boards, on average, avoid punishing CEOs ex post for decisions that were reasonable ex ante. If we control separately for the small number of cases where boards complain about CEO shirking, enjoying the quiet life, self-dealing, or empire-building (shown in column 3), we find no significant effect on CEO turnover (p=0.539). This is consistent with the view that boards tend to take some action other than firing in response to moral hazard problems.[16]

Boards do, however, remove CEOs they have come to view as incompetent.[17] Indeed, the point estimate reported in column 1 suggests that they are 408% more likely to do so than if they did not complain about the CEO's competence in the previous year (p<0.001). This effect is independent of the performance effect, indicating that boards update their beliefs not just from hard data (here, past performance relative to expectations) but also from soft information. Indeed, if we partition the sample by prior-year performance (see column 4), we find that the lagged incompetence indicator predicts CEO dismissal even among firms that perform at or above plan (p<0.001).

Another way to see this is to ask what prompts a board to complain about CEO incompetence. Column 9 shows that such complaints are persistent: A board is four times more likely to complain about the CEO's incompetence if it complained about it in the previous year. Boards also update their beliefs based on prior-year performance (consistent with our interpretation of the performance-sensitivity of CEO dismissals above) but not in response to honest mistakes, and they discount bad luck. Given the relatively low pseudo-R^2 of 13%, boards clearly update partly based on soft-information signals that we do not observe, such as their interactions with the CEO.[18]

Returning to column 1, we find that the effect of the instrument on CEO turnover is positive,

[15] Jenter and Kanaan's (2008) and Kaplan and Minton (2006) document that CEOs at large U.S. firms are punished for industry shocks. While we have no data on industry shocks, CEOs in our sample are *not* fired for bad performance caused by events beyond their control. Neither do we find that CEO turnover increases after macroeconomic shocks.

[16] Not even CEOs suspected of self-dealing are necessarily fired. Of the seven such CEOs in our sample, two are fired, one leads his firm into bankruptcy, three were majority shareholders and so could not be dismissed, and one was subjected to intense board scrutiny which eventually absolved him of wrong-doing.

[17] Recall that the soft information 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 negative opinions this year to justify their current actions.

[18] We find no significant change in board complaints about CEO competence before and after corporate governance reform. This mirrors the non-parametric results of Figure 2 and supports our claim that such complaints are not recorded strategically.

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 effect is easily significant enough to reject the Staiger-Stock (1997) null of a weak instrument. The instrument thus appears to be strong. Its economic effect is also large. Holding the other covariates constant, CEOs are 99% more likely to be fired after boards acquire the statutory power to dismiss them than before. Thus, even sophisticated investors, such as the fund managers in our sample, appear to benefit from a supportive legal environment when performing their monitoring roles.

Column 5 tests if the importance of soft information in explaining CEO turnover increases following an increase in board power, compared to that of hard information, by interacting the performance-relative-to-expectations and CEO incompetence variables with the instrument. Unlike before, this specification is estimated as a linear probability model since probits with interaction terms are problematic (see Ai and Norton (2003)). The results suggest that a law change makes it easier for the board to act on soft information but has no effect on the board's ability to act on hard information. This is consistent with our prediction that fund managers can communicate soft information more easily to fellow board members (whose majority consent is sufficient to fire a CEO after a law change) than to shareholders at large (whose majority consent is necessary to fire a CEO before a law change), while hard information is equally effective in either case.

Column 6 tests the follow-on prediction that soft information becomes more important after an increase in board power *only if* the fund is a minority shareholder. This triple-difference estimate requires us to include a full set of interaction terms involving incompetence, law changes, and ownership. As in column 5, boards are more likely to fire CEOs they deem incompetent, especially once given the statutory powers to do so. The additional interaction terms support our prediction. Absent statutory powers, majority ownership significantly increases the sensitivity of CEO turnover to soft information (p=0.001). After a law change, majority ownership no longer makes a significant difference (summing the relevant coefficients, the p-value is 0.281). Thus, law changes affect the importance of soft information in firing decisions only if the fund is a minority shareholder.

As expected, the law change instrument has no effect on the likelihood that a junior manager is fired (column 7) or that the board takes 'friendly' actions, such as strengthening management through new hiring (column 8). These non-results are consistent with our identifying assumption. Without the statutory power to dismiss management, a board finds it hard to remove an obstinate CEO. By contrast, a junior manager can be removed at any time, not least by the CEO himself, regardless of the statutory powers the board has at the time. Similarly, there is no a priori reason to believe that friendly board actions should become more frequent after a law change.

4.2 Instrument Validity Tests

This section provides four tests of the validity of our instrument. Recall from the discussion in Section 2.2 that corporate governance reforms that require a corporate charter amendment should have no effect on CEO turnover, because they do not alter the fact that the fund manager needs the support of a majority of shareholders to remove a CEO. The sample contains five countries which adopted this weaker form of corporate governance law (see Table 2). Column 1 of Table 4 shows that this type of reform indeed has no significant effect on CEO turnover, as predicted (p=0.893).

Second, if the instrument behaves as we hypothesize, the probability of board intervention should jump in the year board power increases and then stay higher for a while. If it increased any earlier, our argument would be quite implausible: For boards to fire the CEO, according to our argument, they need statutory powers; the mere prospect of such powers should not be sufficient. Figure 1, discussed earlier, investigated this hypothesis non-parametrically. We now provide a formal test. Specifically, 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 estimate the effect on the probability that a CEO is fired in year t 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, reported in column 2 of Table 4, support our hypothesis. 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 the board's statutory power increases, however, the likelihood of intervention increases significantly, by 192%. In the following years, it remains significantly higher. This pattern – which mirrors Figure 1 – supports our identifying assumption.

A third way to validate the instrument is to test whether it is speculation about possible corporate governance reform or, as our argument implies, its actual enactment that affects boards' propensity to fire a CEO. To do so, we search local newspapers in each country for the first mention of reform. In Russia, for example, the local press reported on March 16, 2000, that a special committee of parliament was preparing reforms to the 1996 Federal Law on Joint Stock Companies with a view to empowering boards to dismiss management. The reforms were passed in 2001 and came into force on January 1, 2002. Our next test asks whether the probability of a CEO being fired increased in 2000, when reform was first mooted, or in 2002, when it became law.[19] The results are shown in column 3 of Table 4. While we continue to find a significant increase in CEO turnover when the laws come into force (p=0.001), we find no effect – statistically or economically – when reform is first mooted (p=0.38). This too supports our identifying assumption.

To further validate the instrument, we estimate placebo models (see Bertrand, Duflo, and Mullainathan (2004)). We randomly generate a placebo law-change date for each country, estimate the intervention equation as per column 1 in 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 models are random, we expect to incorrectly reject the true 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 1.8% of the simulations; at the 5% level in 6.0% of the simulations; and at the 10% level in

[19] Reform was faster in some countries than in others. The time between first press reports of reform and final enactment ranges from zero years (in Croatia) to three (in Latvia), with most countries taking one or two years.

12.0% of the simulations. These results suggest that the standard errors reported in Table 3 are reasonably close to unbiased, which supports the validity of the instrument.[20]

4.3 Alternative Specifications

Identification comes from within-country law changes, but our sample does not cover both the pre-law change and post-law change periods in every country. Bulgaria, for example, reformed its laws in 1991 so that none of the 17 Bulgarian investments in our sample experienced a law change. Table 2 reports the sample coverage relative to the year of each country's law change. There are eight countries with in-sample variation in corporate governance laws, and boards were given statutory powers to fire the CEO in five of these (Croatia, Poland, Romania, Russia, and Serbia Montenegro). In column 4 of Table 4, we include only these five countries. This reduces the sample size from 2,058 to 1,323 firm-years but does not otherwise affect the results.[21]

As noted earlier, our baseline models include only a limited set of country effects due to sample size concerns. In column 5 of Table 4, 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 individual country-level fixed effects. This again has no material effect on the results.

5. Effect of Board Intervention on Investment Success and Performance

Does board intervention improve performance? As our sample firms are privately-held, we cannot measure performance using share prices. Instead, we measure performance using the exit information shown in Table 1. Specifically, within the context of our unbalanced panel, we code an indicator equal to one in year t if the firm is exited in years t through $t+2$.[22] Exits are a popular performance measure in the academic literature; see, for instance, Gompers and Lerner (1998, 2000), Brander, Amit, and Antweiler (2002), Sorensen (2007), and Hochberg, Ljungqvist, and Lu

[20] The Table 3, column 1 point estimate of 0.610 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. This is nearly identical to the estimated *p*-value of 0.001 in Table 3, based on standard errors clustered on portfolio firm.

[21] 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 810 firm-years but again does not affect the results.

[22] By construction, every intervention must precede an exit or write-off, so there is no problem including exits in year *t*.

(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 shorter horizons are reported in Section 6.

Exit rates and IRRs do not correlate perfectly. While write-offs have lower mean IRRs than exited investments (-81.6% versus 32.3%), 82 of the 311 exits have negative IRRs, indicating that some sales are fire sales. To capture this, we alternatively refine the simple exit indicator by coding as a successful exit only those fully-realized investments that were exited at a positive IRR.[23]

5.1 Naïve Performance Models

Table 5 reports the results of estimating a naïve version of equation (2), i.e., treating CEO turnover as exogenous. We estimate four probit 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).[24] In all cases, we find a *negative* and mostly marginally statistically significant relation between intervention and performance. To illustrate, in column 1, firing the CEO 'results' in a 7.8 percentage point reduction in the probability of exiting over the next two years (from the unconditional probability of 40.3%), all else equal. The negative sign is consistent with the expected endogeneity problem: Boards intervene in badly managed or underperforming firms, so when we compare the exit rates of firms with and without intervention, we are likely picking up unobserved differences in the quality of management or the firm rather than the effect of intervention.

The solution to the endogeneity problem is to employ an instrument. Before discussing the IV results, we check whether our instrument has a *reduced-form* effect on performance. Given the evidence in Section 4 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. As Angrist and Krueger (2001) note, if we do not see the causal relation of interest in the reduced form, it is probably not there.

[23] Why not use the IRRs to measure performance directly? In a private equity setting, a firm's IRR does not vary annually – it can be computed only upon exit. If we were to model IRRs, the panel would hence 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 intervention.
[24] The firm effects are specified as random effects rather than as fixed effects, since fixed-effects probit suffers from a well-known incidental-parameters problem.

5.2 Reduced-form Performance Models

The four specifications reported in Table 6, Panel A point to a strong, statistically significant reduced-form relation between the instrument and performance, for either exit measure and whether or not we include firm effects. Importantly, it has the expected positive sign: Law changes strengthening the power of the board over the CEO are associated with improved exit performance.

The other variables behave as expected. Firms that perform better relative to expectations are more likely to be exited and to be exited at a positive IRR, as are larger deals. 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, both in terms of a country's GDP growth and its transition to a market economy. The three sets of country, year, and industry effects are each statistically significant, as are the portfolio-firm effects in columns 2 and 4.

5.3 Exclusion Restriction: Adverse Selection or Moral Hazard?

A reduced-form relation between law changes and performance is reassuring, but for our identification strategy to work, law changes must affect performance only through their effect on board decisions rather than directly. A first-order concern is that merely by increasing the *threat* of dismissal, the reforms could induce CEOs to raise their game. If so, law changes would affect performance directly, violating the exclusion restriction. This story presupposes that the agency problem is moral hazard, not adverse selection, since the CEO can change only his actions, not his type, in response to an increased threat of dismissal. But our formal econometric evidence in Table 3 does not suggest that moral hazard plays a role in boards' intervention decisions; instead, we find strong evidence that boards fire CEOs they have come to view as incompetent. Moreover, there are few complaints about moral hazard in the first place, perhaps because the incentives contracts offered by private equity funds generally work quite well, at least in our sample.

If CEOs *could* raise their game, it is reasonable to expect that they would begin doing so as soon as it is clear that corporate governance laws *will* be changed, rather than wait for the new laws to come into effect. In terms of our reduced-form models, we would thus expect performance to improve *before* the laws come into force. In Table 6, Panel B we run a horse race between the actual law-change date and the date when the law changes were first mooted in the local press, using the same data as in Section 4.2. The results, shown in columns 1 and 3, are unambiguous: Performance only improves once the law *has* changed, not when the press first reports that reform is on its way. This evidence is hard to reconcile with a moral-hazard argument.

Another way to address the moral-hazard concern is to test directly whether CEOs do, in fact, raise their game in response to law changes. The top graph in Figure 3 shows that average performance relative to expectations is flat around law changes. The bottom graph asks whether the prospect of reform, rather than its enactment, affects performance, and again we find no effect. Thus, CEOs do not appear to raise their game simply because the law changes, or is about to change, which is consistent with adverse selection being the main driver of interventions.[25,26]

Finally, if boards were seriously concerned about moral hazard, rather than adverse selection, we might expect them to restructure CEO incentive contracts when board power increases. After all, if corporate governance mainly solves a moral hazard problem, greater board power should reduce the incentive pay a CEO needs to be offered (see Core et al. (1999)). However, only six sample firms make changes to CEO incentive contracts, and the timing of these changes does not coincide with the law changes. This suggests that the tradeoff between board power and incentive contracts is far from first order in our data, possibly because moral hazard is only a second-order concern.

5.4 Exclusion Restriction: Other Concerns

The exclusion restriction could be violated in three other ways. First, 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

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[25] These nonparametric results are robust to estimating a regression to control for confounding effects.

[26] Boards might raise their expectations as soon as the law changes while CEOs might simultaneously raise their game, leading us to see no significant changes in performance relative to expectations around law changes. However, the budget data available in around a quarter of the monitoring reports do not support such a pattern. In fact, annual percentage changes in expected profits or expected sales are extremely volatile and do not tick up around law changes.

part of broader reforms of commercial law affecting contracts, employment, intellectual property, etc., not just board power. It is therefore possible that replacing Soviet with Western commercial law affects performance independently of any changes in board power. To shed light on this, we exploit a convenient feature of our data. Three sample countries amended the articles pertaining to the powers of the board some years after enacting Western-style commercial laws. We have already mentioned the 2002 amendment to Russia's 1996 Federal Law on Joint Stock Companies. The other two countries are Croatia and Romania; see Table 2.

If it is the adoption of commercial law that affects performance, rather than changes in board power, then corporate governance *amendments* should have no effect on performance in the reduced-form models. In Panel B of Table 6, we restrict the sample to the three countries with such amendments.[27] 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, shown in columns 2 and 4, confirm that strengthening board power affects performance independently of the adoption of Western-style commercial law. They are positive and statistically significantly different from zero, and if anything somewhat larger than the equivalent point estimates shown in Panel A of Table 6 where we include all 19 countries. This lends further 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 potential bias to a large extent. For the exclusion restriction to be violated, each country would have to strengthen board power at exactly the same time as some other beneficial economic shock hit, which is unlikely. To further reduce the chances that the instrument correlates with unobserved economic shocks, we explicitly control for macroeconomic conditions and a country's reform progress.

[27] 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=0.861) while the latter does (p=0.01).

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 have been swayed by the relatively small firms in our sample. We can test lobbying and other reverse-causality stories by replacing the instrument with the set of five time-varying indicators we used in Table 4. 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.

5.5 IV Estimates of the Effect of Intervention on Performance

We now turn to estimating the structural effect of intervention on performance using law changes as an instrument. The binary nature of both dependent variables (we observe whether or not a board intervenes and whether the investment is exited rather than written off) poses no particular problem (Maddala (1983, p. 118)). As long as it is identified, Greene (1998) shows that a system of two equations with binary dependent variables can be consistently and efficiently estimated using a seemingly unrelated bivariate probit. The results for our two performance measures, exit and exit at a positive IRR, are shown in columns 1 and 3 of Table 7. In each specification, a likelihood ratio test rejects the null that the disturbances in the two equations are uncorrelated. This confirms that intervention is endogenous, as suspected, and needs to be instrumented. As expected, the correlation is negative, suggesting that boards intervene in lower-quality companies.

Importantly, the sign of the intervention variable flips in both specifications, compared to the naïve models in Table 5. Each point estimate is statistically significant at the 2.5% 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 exit over the next two years by 36.9 percentage points from the unconditional probability of 40.3% (column 1), while the probability of exiting at a positive IRR improves by 48.7 percentage points from the unconditional probability of 29.3% (column 3). In sum, even though boards intervene in lower-quality firms, doing so makes a successful investment outcome significantly more likely.

Not surprisingly, poor managerial skill has a negative and at least marginally significant effect on performance. The presence of a CEO the board deems incompetent reduces the chances of a successful investment outcome from 40.3% to 27.1% in column 1. Since incompetence often leads to firing, according to Table 3, this suggests that failure (or inability) to get rid of a bad CEO increases the chances that the investment will have to be written off.

5.6 Adding Portfolio-Firm Effects

Are these findings robust to including portfolio-firm effects to control for unobserved heterogeneity? A standard result in IV estimation holds that the equation of interest can be estimated consistently even if the equation modeling the troublesome covariate (here: intervention) is not estimated consistently. Thus, omitting firm effects from the intervention equation should not affect the consistency of the estimate of intervention on performance. This general result will cease to hold, however, if the unobserved firm effects are correlated across the two equations. If they are, they will affect both intervention and performance, so their omission could cause bias in the estimated effect of intervention on performance. For example, some CEOs may have majority ownership, making it near impossible to fire them; such entrenched CEOs may also be associated with worse performance. We thus estimate a seemingly unrelated bivariate probit model with random portfolio-firm effects that are allowed to be correlated across the two equations.

Columns 2 and 4 in Table 7 show that the bias is minimal. Compared to columns 1 and 3, including firm effects increases the point estimate of the intervention variable a little in column 2 and reduces it a little in column 4, leaving our conclusions unchanged.

5.7 Modeling Performance Relative to Expectations

So far, we have measured performance as successful exits. We now use performance relative to expectations as an alternative measure. We treat the five-point performance 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 without or with portfolio-firm effects, shown in columns 1 and 2 of Table 8, we do find evidence that CEO turnover has a positive and, in column 2, significant effect on subsequent performance. This contrasts with the naïve exit models in Table 5, where we found negative and significant effects on exit. Thus, the Table 8 models appear to be less biased. Still, the estimated magnitude may be too small. The coefficients suggest that intervention improves performance by between 0.166 and 0.227 on a five-point scale whose mean is 2.8.

Columns 3 and 4 report IV models that treat intervention as endogenous, instrumented using the law changes. Because intervention is a binary variable, column 3 is estimated as a Heckman (1978) treatment model. This increases the estimated effect of intervention eight-fold relative to the naïve model in column 1. Specifically, forced CEO turnover now leads to a 1.382-point improvement on the five-point scale (p<0.001).[28] Column 4 includes random portfolio-firm effects. Because the Heckman model cannot accommodate firm effects, we estimate the model using generalized 2SLS. This too yields a larger point estimate than in the naïve model, of 1.879 (p=0.039).

Firms whose performance in t-1 was poor due to 'bad luck' subsequently perform better. This adds credence to this variable: Bad luck should not be persistent, so firms should bounce back later. Finally, firms run by managers who are deemed incompetent perform significantly worse.

[28] 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.

5.8 Discussion

Whether we measure performance as exit or relative to plan, we get the same result: As long as we instrument it, intervention has a large, positive, and significant effect on performance. This is echoed in the way fund managers comment on the beneficial effects of CEO turnover. For example:

> *"The turnaround under new management has been extremely impressive. From a disastrous 2002, when under former management there was a period of liquidation risk, [the company] has [re]turned to profit."*

There is strong evidence of simultaneity bias, in the sense that the naïve 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.

6. Robustness

Table 9 reports variations on the Table 7 specifications. As before, columns 1 and 2 focus on exits while columns 3 and 4 focus on exits at a positive IRR; portfolio-firm effects are included in columns 2 and 4. To save space, we report only the intervention coefficients. All other Table 7 covariates are, of course, included in the estimation.

Panel A explores 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. When we shorten the horizon to t through t+1, we continue to find that CEO turnover improves the probability of exit. When we shorten the horizon further, to simply t, the effect disappears (not tabulated). This suggests that it takes time for a new CEO to affect a firm's performance.

Panels B and C 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 columns 4 and 5 of Table 4, respectively. 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 D, we allow the effect to vary over time by replacing the instrumental variable with the set of five indicator variables introduced in Table 4, column 2. In Panel E, we 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.

7. Conclusions

This paper attempts to open up the black box that is corporate governance by investigating under what circumstances boards of directors fire CEOs. We use unique and very detailed data on boards' actions, expectations, and beliefs about CEO ability. We find that CEOs are fired when the company underperforms the board's expectations, suggesting that boards use performance signals to update their beliefs. CEOs are especially likely to be fired when evidence has mounted that they are incompetent and when board power has increased following corporate governance reforms. In contrast, CEOs are not fired when performance deteriorates due to factors deemed explicitly to be beyond their control, nor are they fired for making 'honest mistakes.'

Given our evidence that boards intervene in underperforming firms, it is not surprising that a naïve model, which treats intervention as exogenous, spuriously suggests intervention 'hurts' performance. Instrumenting board interventions using corporate governance law changes leads to the opposite conclusion: Following forced CEO turnover, firms 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.

This 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. We leave this interesting topic for future research.

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Figure 1. Forced CEO Turnover Around Law Changes.
The graph shows the annual incidence of forced CEO turnover. Time on the horizontal axis is measured relative to the year in which the country in question reformed its corporate governance laws by giving boards the statutory power to dismiss CEOs, denoted year 0; see Table 2 for a list of these dates. Countries without a law change, or where the law change only empowered the shareholders' meeting to amend the corporate charter to delegate the power to dismiss the CEO, are excluded. Year -5 includes prior years; year 5 includes later years.

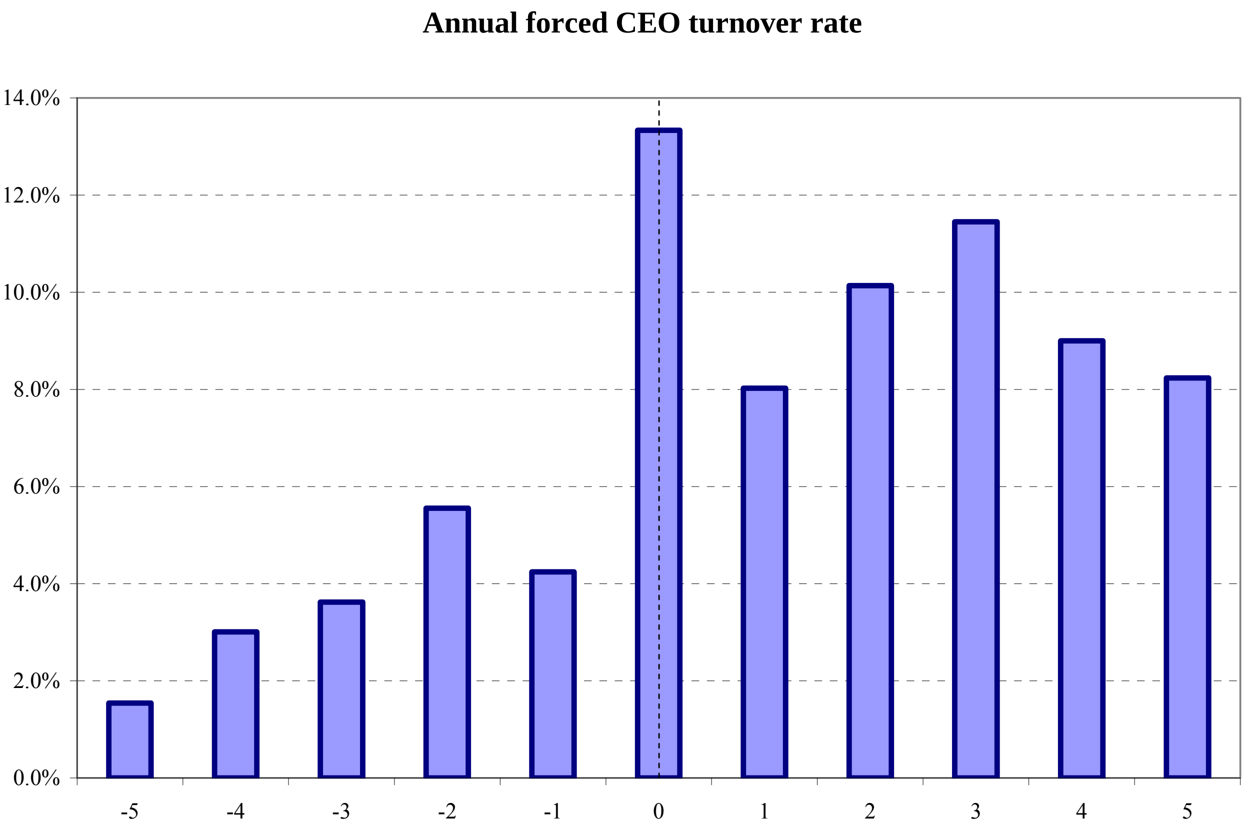


Figure 2. CEO Competence Around Law Changes.
The EBRD regularly reviews the performance of each portfolio firm, resulting in a monitoring report based on both quantitative and qualitative criteria. The graph shows the annual incidence of a monitoring report mentioning that a fund manager privately viewed the CEO as incompetent. Time on the horizontal axis is measured relative to the year in which the country in question reformed its corporate governance laws by giving boards the statutory power to dismiss CEOs, denoted year 0; see Table 2 for a list of these dates. Countries without a law change, or where the law change only empowered the shareholders' meeting to amend the corporate charter to delegate the power to dismiss the CEO, are excluded. Year -5 includes prior years; year 5 includes later years.

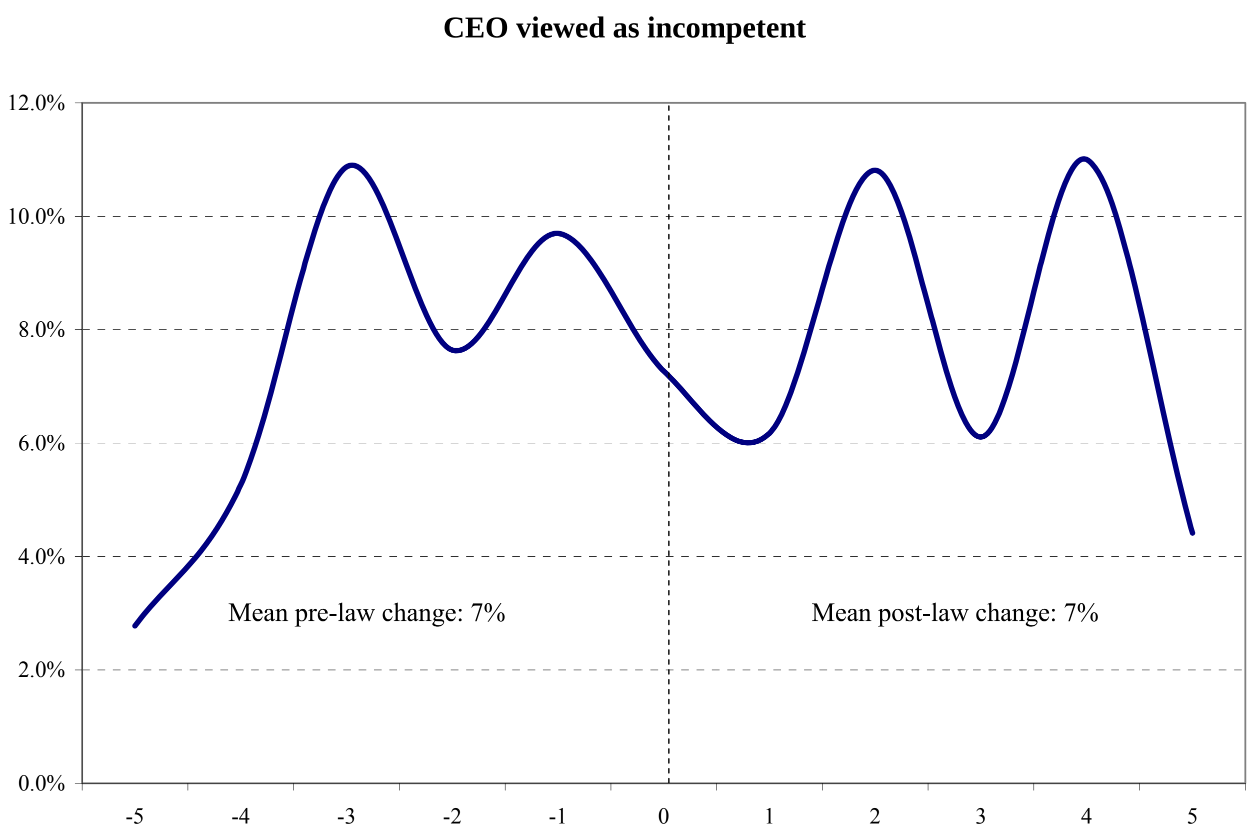
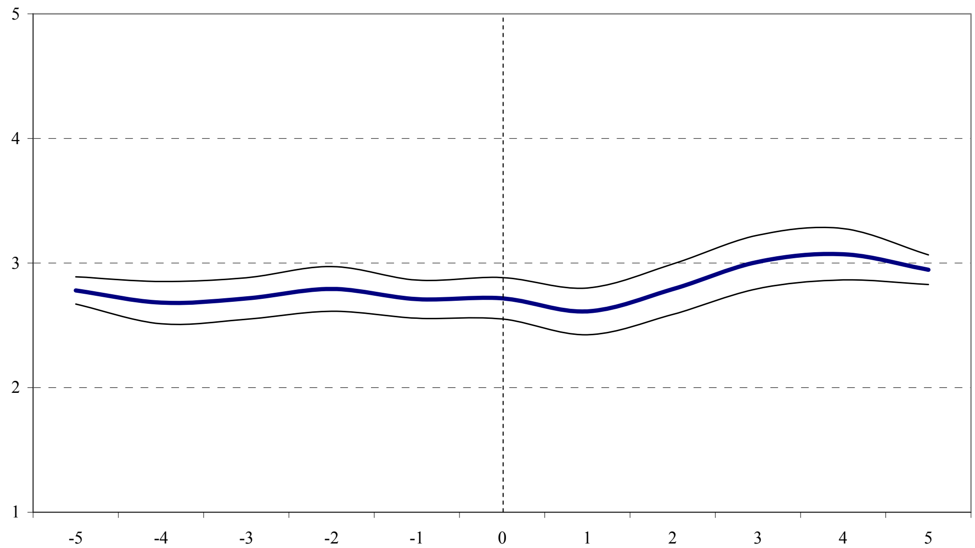


Figure 3. Performance Relative to Expectations Around Law Changes.
The EBRD regularly reviews the performance of each portfolio firm, resulting in a monitoring report based on both quantitative and qualitative criteria. We use the monitoring reports to construct a variable that captures the performance of each firm 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 firm'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 graphs show the average performance score for each year around the date the law changed (year 0 in the upper graph) or the date legislation was first mooted in the local media (year 0 in the lower graph), along with cross-sectional 95% confidence bounds. Countries without a law change, or where the law change only empowered the shareholders' meeting to amend the corporate charter to delegate the power to dismiss the CEO, are excluded. Year -5 includes prior years; year 5 includes later years.

Performance score
(year 0 = legislation enacted)



Performance score
(year 0 = legislation first mooted in the local media)

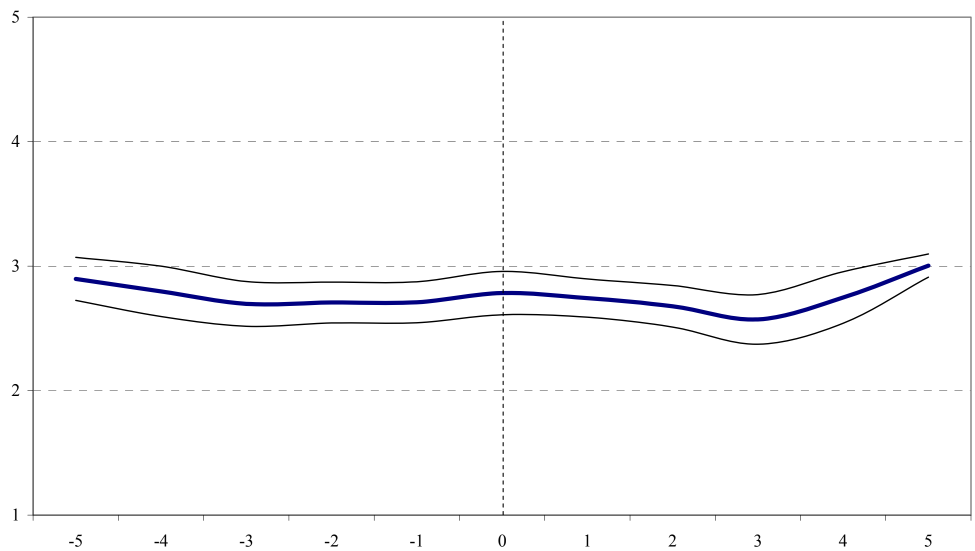


Table 1. Sample Overview.

The sample consists of 473 investments by 43 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 ‘exited’ through an IPO or a sale, written off, or is still alive, as of 2008. 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. In the 59 still-alive cases, the IRRs are in part based on unrealized capital gains. In all other cases, they are based solely on cash flows. 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	exited	written off	still alive	IRR (%)
Azerbaijan							1							1	0.0	100.0	0.0	-100.0
Bulgaria				1	3		2		2		1	8		17	94.1	5.9	0.0	54.1
Croatia						1	1	2	3	2		3		12	66.7	8.3	25.0	-7.8
Czech Republic	3	5	5	1	4	4	8	9	2	2			1	44	72.7	22.7	4.5	29.9
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	6	7	11	6	8	6	3	2	4		59	57.6	20.3	22.0	6.8
Kazakhstan						1	2							3	100.0	0.0	0.0	54.8
Latvia										1	1	3	2	7	14.3	0.0	85.7	10.1
Lithuania				2	1	2	4	1	2			1	2	15	53.3	40.0	6.7	-15.0
Moldova												1		1	100.0	0.0	0.0	158.5
Poland	10	12	14	14	20	12	16	26	14	8	12	5	8	171	69.0	22.8	8.2	-3.2
Romania				6		6	8	2	2		1	1	4	30	86.7	3.3	10.0	23.0
Russia		9	18	11	6	5		4	9	5	3	4	1	75	56.0	30.7	13.3	-2.8
Serbia Montenegro											2			2	0.0	0.0	100.0	-24.2
Slovakia						1		4	5			1	1	12	83.3	8.3	8.3	7.2
Slovenia								1						1	100.0	0.0	0.0	23.3
Turkmenistan			1			1	1	1	1					5	100.0	0.0	0.0	44.6
Ukraine					1									1	100.0	0.0	0.0	11.7
All countries	14	28	41	44	42	48	50	59	47	22	25	33	20	473	67.4	20.1	12.5	7.5

Table 2. 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); "Guide to the Russian Law on Joint Stock Companies" by B. Black, R. Kraakman, and A. Tarassova (Kluwer Law International, 1998, The Hague); and Yefymenko (2009). We are grateful to Gian Piero Cigna, Senior Counsel of the EBRD, for expert advice.

Country	Year board acquired power to dismiss CEO	Name of law	Part of law pertaining to board powers	Board is given statutory power to dismiss CEO?	Shareholder meeting can amend corporate charter to delegate power to dismiss CEO to the board?	Power to dismiss CEO brought in by amendment to previously enacted corporate law?	Number of firms	Maximum number of firm-years	Sample coverage relative to year of law change	Number of CEO dismissals	Percent of which occurred post-law change
Azerbaijan	-	-	-	No	No		1	4	only before	1	0
Bulgaria	1991	Commercial Act	Article 241	Yes			17	80	only after	8	100
Croatia	2004	Companies Act	Section 204	Yes		Yes	12	63	both	3	67
Czech Rep.	(2001)	Commercial Law	Article 194	No	Yes		44	247	both	14	50
Estonia	1995	Commercial Code	Sections 308, 309	Yes			15	79	only after	4	100
Georgia	1995	Law on Entrepreneurs	Article 55.8	Yes			2	8	only after	1	100
Hungary	(1998)	Act on Company Law	Section 37.1	No	Yes		59	322	both	30	83
Kazakhstan	1998	Law on Joint Stock Companies	Article 62	Yes			3	12	only after	0	
Latvia	2002	Commercial Law	Section 292	Yes			7	34	only after	3	100
Lithuania	1990	Law on Stock Corporations	Article 25.1	Yes			15	62	only after	4	100
Moldova	(1997)	Joint Stock Company Law	Article 50.4	No	Yes		1	4	only after	0	
Poland	2001	Code of Commercial Companies	Article 368.4	Yes			171	977	both	74	81
Romania	2006	Company Law	Article 153	Yes		Yes	30	174	both	8	0
Russia	2002	Federal Law on Joint Stock Cos.	Articles 48, 65	Yes		Yes	75	440	both	24	75
Serbia-MN	2004	Companies Act	Article 322	Yes			2	10	both	1	100
Slovakia	(1992)	Commercial Code	Article 187(1)	No	Yes		12	62	only after	2	100
Slovenia	1993	Companies Act	Article 250	Yes			1	7	only after	1	100
Turkmenistan	(1999)	Joint-Stock Companies Law	Article 48	No	Yes		5	27	both	0	
Ukraine	2009	Law on Joint-Stock Companies	Paragraph 52	Yes			1	4	only before	0	
All countries							473	2,616		178	76

Table 3. Determinants of Board Intervention.

The sample consists of an unbalanced annual panel of 473 firms which we observe from the year of the initial investment to the year of exit or write-off or to 2008, whichever is earlier. The tests reported in this table focus on the determinants of board intervention, i.e., the removal of an executive (columns 1-7) or actions to strengthen the management team by hiring additional senior managers (column 8). Column 9 studies the evolution of the board's beliefs about the CEO's competence. A junior manager in column 7 is any named executive below the level of the CEO. Estimation uses probit except in columns 5 and 6, which are estimated as linear probability models due to the presence of interaction effects (see Ai and Norton (2003) for why probits with interaction terms are problematic). "n.m." in columns 5 and 6 denotes "not meaningful." The explanatory variables are listed in the table and defined further in Section 3. Columns 1 through 7 include an instrumental variable and so estimate the reduced form of the board intervention equation (1) in Section 2. The instrument equals one if at the beginning of year *t*, the corporate law in country *k* allows the firm's board to dismiss the CEO. Table 2 provides details of the staggered adoption of such laws in the 19 countries in our sample. The Staiger-Stock (1997) test is a Wald test of the null hypothesis that the instrument does not correlate with board interventions. It has a critical value of 10 in an *F*-test. While we report a χ^2 statistic with one degree of freedom instead of an *F*-test, for our sample size, $\chi^2 \approx F$. All specifications include country, industry, and time effects. Column 2 includes random portfolio-firm effects to control for unobserved heterogeneity. These are not statistically significant (p=0.192). The number of observations is 2,058 except in column 4, which restricts the sample to the 1,196 firm-years with lagged performance at or above plan. 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.

Table 3. Continued.

	Dependent variable =1 if fire …							=1 if streng-then mgt	=1 if CEO viewed as incompetent
	CEO	CEO	CEO	CEO	CEO	CEO	junior manager		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Board's information set and beliefs									
performance relative to board's expectations in t-1	-0.311***	-0.314***	-0.310***	-0.206**	-0.039***	-0.036***	-0.130***	-0.027	-0.153***
	0.050	*0.057*	*0.050*	*0.100*	*0.007*	*0.006*	*0.050*	*0.046*	*0.049*
… x (=1 if corp governance law enacted)					0.008				
					0.010				
=1 if poor perf. blamed on manager's decisions in t-1	0.128	0.125	0.097		0.031	0.041	0.303	0.343*	0.126
	0.181	*0.216*	*0.233*		*0.034*	*0.035*	*0.211*	*0.205*	*0.207*
=1 if board complained about moral hazard probs in t-1			0.059						
			0.331						
=1 if poor performance blamed on bad luck in t-1	-0.030	-0.028	-0.029		-0.007	-0.014	0.005	0.263**	-0.240*
	0.129	*0.132*	*0.129*		*0.021*	*0.020*	*0.126*	*0.127*	*0.123*
=1 if manager viewed as incompetent in t-1	1.264***	1.294***	1.264***	1.510***	0.205***	0.189***	0.760***	0.380***	1.149***
	0.133	*0.132*	*0.133*	*0.296*	*0.052*	*0.051*	*0.130*	*0.136*	*0.124*
… x (=1 if corp governance law enacted)					0.255***	0.313***			
					0.085	*0.097*			
… x (=1 if fund has majority ownership)						0.464**			
						0.227			
… x (=1 if corp governance law enacted)						-0.614**			
						0.244			
Deal characteristics									
log investment cost through t-1	0.002	0.002	0.002	0.089	0.001	0.001	0.014	0.025	0.086***
	0.037	*0.030*	*0.037*	*0.074*	*0.004*	*0.004*	*0.039*	*0.034*	*0.033*
=1 if privatization	0.348**	0.360**	0.349**	0.389	0.038	0.036	0.147	0.498***	0.380***
	0.173	*0.172*	*0.174*	*0.259*	*0.025*	*0.026*	*0.154*	*0.173*	*0.141*
=1 if deal is staged	0.193**	0.198**	0.193**	0.051	0.018	0.018	0.116	0.467***	-0.028
	0.093	*0.100*	*0.093*	*0.157*	*0.012*	*0.012*	*0.093*	*0.092*	*0.086*
=1 if fund has majority ownership						-0.001			
						0.023			
… x (=1 if corp governance law enacted)						0.018			
						0.030			
Macroeconomic conditions									
lagged EBRD transition indicator	0.089	0.082	0.091	-0.233	0.012	0.007	-0.067	-0.499***	-0.117
	0.168	*0.248*	*0.169*	*0.437*	*0.015*	*0.015*	*0.229*	*0.163*	*0.157*
lagged real GDP growth	-0.010	-0.010	-0.010	-0.035	0.000	-0.001	0.009	-0.004	-0.009
	0.014	*0.016*	*0.014*	*0.025*	*0.001*	*0.001*	*0.017*	*0.013*	*0.012*
Instrument									
=1 if corp governance law enacted	0.610***	0.623***	0.611***	0.991***	0.028	0.049***	0.115	0.053	0.024
	0.132	*0.141*	*0.132*	*0.253*	*0.035*	*0.014*	*0.138*	*0.128*	*0.127*
Diagnostics									
Wald test: all coeff. = 0 (χ^2)	264.5***	182.9***	264.8***	99.4***	204.4***	209.4***	99.2***	115.1***	152.5***
Staiger-Stock (1997) test (χ^2)	21.3***	19.6***	21.4***	15.3***	n.m.	n.m.	0.7	0.2	0.0
Pseudo R^2 (adjusted R^2 in columns 5 and 6)	19.3%	19.3%	19.3%	20.7%	14.4%	15.1%	9.5%	10.8%	13.0%

Table 4. Instrument Validity Tests and Alternative Specifications.

The table reports three instrument validity tests and two alternative specifications. There are two types of corporate governance reforms. Boards can either be given statutory power to dismiss the CEO, or the law can reserve this power for the shareholders' meeting but allow for such power to be delegated to the board through an amendment to the corporate charter. Our instrument is based solely on changes in the board's statutory power, as identified in Table 2. For the instrument to be valid, it has to be the case that law changes that require a corporate charter amendment have no effect on CEO turnover. To change the charter post-law change, a sample fund needs a majority of the votes in the shareholders' meeting. To dismiss the CEO pre-law change, it also needs a majority of the votes in the shareholders' meeting. Thus, this type of law change should not affect the distribution of power within the firm. Column 1 tests this prediction. Second, 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. If it increased any earlier, our argument would be quite implausible: For boards to fire the CEO, according to our argument, they need statutory powers; the mere prospect of such powers should not be sufficient. Accordingly, column 2 investigates the time profile of the effect of law changes empowering boards to dismiss the CEO on the probability of board intervention. Third, we test whether it is speculation about possible corporate governance reform or, as our argument implies, its actual enactment that affects boards' propensity to fire a CEO. To test this, column 3 distinguishes between the year corporate governance reform was first mooted in the local press and the year it was enacted. The two alternative specifications in columns 4 and 5 explore two subsamples. Column 4 restricts the sample to the five countries (Croatia, Poland, Romania, Russia, and Serbia Montenegro) in which the law was changed to give boards the statutory power to dismiss the CEO and our data cover both the pre-law change and the post-law change regime (for details, see Table 2). Column 5 restricts the sample to the five countries with the most investments (Poland, Russia, Hungary, the Czech Republic, and Romania). All specifications include country, industry, and time effects. Estimation uses probit. The number of observations is 2,058 in columns 1 and 2, 1,323 in column 3, and 1,705 in column 4. 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.

Table 4. Continued.

	Dependent variable = 1 if CEO is fired in year *t*				
	(1)	(2)	(3)	(4)	(5)
Board's information set and beliefs					
performance rel. to expectations in *t*-1	-0.310***	-0.307***	-0.310***	-0.291***	-0.284***
	0.050	*0.050*	*0.050*	*0.067*	*0.056*
=1 if poor performance blamed on CEO's decisions in *t*-1	0.131	0.118	0.132	0.099	0.222
	0.180	*0.181*	*0.180*	*0.212*	*0.196*
=1 if poor performance blamed on bad luck in *t*-1	-0.029	-0.032	-0.035	-0.057	0.067
	0.130	*0.130*	*0.129*	*0.165*	*0.139*
=1 if CEO viewed as incompetent in *t*-1	1.263***	1.252***	1.258***	1.424***	1.262***
	0.133	*0.131*	*0.132*	*0.175*	*0.142*
Deal characteristics					
log investment cost through *t*-1	0.003	0.005	0.002	0.079	-0.020
	0.036	*0.038*	*0.037*	*0.061*	*0.032*
=1 if privatization	0.347**	0.351**	0.344**	-0.085	0.095
	0.174	*0.174*	*0.174*	*0.221*	*0.203*
=1 if deal is staged	0.193**	0.199**	0.192**	0.118	0.189*
	0.093	*0.092*	*0.092*	*0.129*	*0.106*
Macroeconomic conditions					
lagged EBRD transition indicator	0.094	0.167	0.071	-0.428	0.086
	0.170	*0.186*	*0.176*	*0.393*	*0.412*
lagged real GDP growth	-0.009	-0.015	-0.014	-0.028	-0.006
	0.014	*0.014*	*0.014*	*0.017*	*0.017*
Instrument					
=1 if corp gov. law enacted in *t*+2		0.066			
		0.223			
=1 if corp gov. law enacted in *t*+1		0.089			
		0.247			
=1 if corp gov. law enacted in *t*		0.767***			
		0.194			
=1 if corp gov. law enacted in *t*-1		0.470**			
		0.211			
=1 if corp gov. law enacted in *t*-2 or earlier		0.564***			
		0.172			
=1 if corp gov. reform mooted			0.157		
			0.179		
=1 if corp gov. law enacted	0.605***		0.529***	0.726***	0.676***
	0.135		*0.164*	*0.178*	*0.169*
=1 if change in board power requires charter change	-0.030				
	0.221				
Diagnostics					
Wald test: all coeff. = 0 (χ^2)	265.3***	269.5***	263.2***	1999.0***	250.5***
Staiger-Stock (1997) instrument strength test (χ^2)	20.0***	17.1***	10.4***	16.6***	16.0***
Pseudo-R^2	19.3%	19.3%	19.4%	23.1%	19.8%

Table 5. 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 2 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 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 is 2,058. 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)
Board's information set and beliefs				
=1 if CEO dismissed in year *t*	-0.210*	-0.092	-0.334**	-0.394*
	0.127	*0.174*	*0.148*	*0.228*
Hard and soft information				
performance rel. to expectations in *t*-1	0.121***	0.083	0.306***	0.238***
	0.041	*0.053*	*0.043*	*0.064*
=1 if poor performance blamed on CEO's decisions in *t*-1	0.227	0.058	0.086	0.457
	0.179	*0.236*	*0.185*	*0.307*
=1 if poor performance blamed on bad luck in *t*-1	-0.059	-0.105	0.072	0.162
	0.106	*0.139*	*0.112*	*0.169*
=1 if CEO viewed as incompetent in *t*-1	-0.005	-0.108	0.026	0.306
	0.150	*0.200*	*0.170*	*0.267*
Deal characteristics				
log investment cost through *t*-1	0.062**	0.213***	0.096***	0.311***
	0.025	*0.051*	*0.037*	*0.072*
=1 if privatization	0.188	0.680*	0.272	1.185**
	0.144	*0.390*	*0.180*	*0.484*
=1 if deal is staged	-0.368***	-0.922***	-0.281***	-0.854***
	0.085	*0.228*	*0.098*	*0.274*
Macroeconomic conditions				
lagged EBRD transition indicator	0.487***	0.378***	0.154	0.349***
	0.162	*0.025*	*0.167*	*0.029*
lagged real GDP growth	0.036***	0.133***	0.044***	0.165***
	0.012	*0.017*	*0.013*	*0.022*
Portfolio firms effects?	No	Yes	No	Yes
Diagnostics				
Wald test: all coeff. = 0 (χ^2)	146.7***	310.9***	250.9***	248.4***
Likelihood ratio test: portfolio-firm effects = 0 (χ^2)	n.a.	337.7***	n.a.	392.1***
Pseudo-R^2	7.9%	20.1%	13.7%	29.5%

Table 6. 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 empowering the board to dismiss the CEO 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 is 2,058. 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)
Board's information set and beliefs				
performance rel. to expectations in *t*-1	0.137***	0.068	0.324***	0.234***
	0.043	*0.055*	*0.044*	*0.066*
=1 if poor performance blamed on CEO's decisions in *t*-1	0.226	0.435	0.075	0.389
	0.178	*0.246*	*0.186*	*0.321*
=1 if poor performance blamed on bad luck in *t*-1	-0.020	-0.081	0.112	0.140
	0.107	*0.146*	*0.111*	*0.178*
=1 if CEO viewed as incompetent in *t*-1	-0.059	-0.155	-0.069	0.109
	0.144	*0.197*	*0.164*	*0.257*
Deal characteristics				
log investment cost through *t*-1	0.069***	0.212***	0.096**	0.267***
	0.026	*0.051*	*0.038*	*0.065*
=1 if privatization	0.156	0.059	0.243	0.106**
	0.161	*0.038*	*0.191*	*0.047*
=1 if deal is staged	-0.396***	-0.100***	-0.301***	-0.094***
	0.090	*0.023*	*0.102*	*0.028*
Macroeconomic conditions				
lagged EBRD transition indicator	0.361**	0.311***	0.033	0.284***
	0.178	*0.026*	*0.182*	*0.031*
lagged real GDP growth	0.045***	0.169***	0.053***	0.203***
	0.012	*0.019*	*0.014*	*0.024*
Instrument				
=1 if corp governance law enacted	0.513***	1.686***	0.428***	1.586***
	0.119	*0.176*	*0.127*	*0.205*
Portfolio firms effects?	No	Yes	No	Yes
Diagnostics				
Wald test: all coeff. = 0 (χ^2)	160.3***	425.9***	236.4***	310.7***
Likelihood ratio test: portfolio-firm effects = 0 (χ^2)	n.a.	420.9***	n.a.	446.1***
Pseudo-R^2	9.9%	25.1%	14.8%	32.7%

Table 6. Panel B: Tests of the Exclusion Restriction.

In this panel, we explore two ways in which the exclusion restriction might be violated: Managers might raise their game in response to an increase in the threat of dismissal; and changes in corporate governance laws might capture the performance-improving effects of broader reforms of corporate law, which they were often introduced as part of. To test the first story, we run a horse race between actual law changes empowering the board to dismiss the CEO and the date when such changes were first mooted in the local press. The results are reported in columns 1 and 3. To test the second story, we restrict the sample to the three countries that strengthened the power of the board relative to the CEO through an amendment to a commercial law enacted some years earlier. As per Table 2, the three countries are Croatia, Romania, and Russia. Note that each of these countries replaced its Soviet-era corporate law with Western-style corporate law *before* sample funds made any investments. Thus, there is no need to include separate indicators for commercial law reform and subsequent corporate governance amendments; the instrument isolates the effect of changes in corporate governance law on performance. The results are reported in columns 2 and 4. All specifications include country, industry, and time effects. Estimation uses probit. The number of observations is 2,058 in columns 1 and 3 and 546 in columns 2 and 4. 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	
	(1)	(2)	(3)	(4)
Board's information set and beliefs				
performance rel. to expectations in t-1	0.138***	0.224**	0.327***	0.383***
	0.043	*0.092*	*0.044*	*0.096*
=1 if poor performance blamed on CEO's decisions in t-1	0.236	0.247	0.094	0.530
	0.178	*0.338*	*0.184*	*0.340*
=1 if poor performance blamed on bad luck in t-1	-0.026	0.358	0.103	0.326
	0.107	*0.226*	*0.112*	*0.244*
=1 if CEO viewed as incompetent in t-1	-0.067	-0.069	-0.077	-0.387
	0.145	*0.320*	*0.164*	*0.291*
Deal characteristics				
log investment cost through t-1	0.069***	0.061	0.096**	0.030
	0.026	*0.062*	*0.039*	*0.070*
=1 if privatization	0.153	1.731**	0.240	1.627**
	0.161	*0.717*	*0.192*	*0.786*
=1 if deal is staged	-0.397***	-0.154	-0.303***	-0.124
	0.090	*0.186*	*0.101*	*0.205*
Macroeconomic conditions				
lagged EBRD transition indicator	0.334*	1.643***	-0.009	1.027**
	0.184	*0.436*	*0.189*	*0.522*
lagged real GDP growth	0.042***	0.058***	0.048***	0.052**
	0.013	*0.020*	*0.014*	*0.025*
Instrument				
=1 if corp gov. reform mooted	0.138		0.194	
	0.134		*0.137*	
=1 if corp gov. law enacted	0.431***		0.317**	
	0.135		*0.144*	
=1 if corp governance strengthened by amendment		1.036***		0.707***
		0.220		*0.204*
Diagnostics				
Wald test: all coeff. = 0 (χ^2)	159.4***	165.5***	233.1***	127.7***
Pseudo-R^2	10.0%	23.9%	14.9%	23.4%

Table 7. Structural Performance Model Using Law Changes as an Instrument.

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. The models are estimated using a seemingly unrelated bivariate probit model that treats intervention as endogenous using the model shown in column 1, Table 3. Columns 1 and 3 of this table are estimated without random portfolio-firm effects while columns 2 and 4 include random portfolio-firm effects to control for time-invariant firm-level omitted variables which we allow to be correlated across the intervention and performance equations. All specifications include country, industry, and time effects. The number of observations is 2,058. The exogeneity test is a likelihood ratio test of the null that the disturbances in equations (1) and (2) are uncorrelated. Based on Monte Carlo evidence, Monfardini and Radice (2008) conclude that 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 simple *t*-test on the correlation coefficient, so we report the LR test. The Staiger-Stock test is a Wald test of the null hypothesis that the instrument does not correlate with board interventions. It has a critical value of 10. 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, where we include random portfolio-firm effects, it is impossible to cluster. 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)
Board interventions				
=1 if CEO dismissed in year *t*	0.971**	1.611***	1.309***	1.124**
	0.432	*0.253*	*0.342*	*0.472*
Board's information set and beliefs				
performance rel. to expectations in *t*-1	0.160***	0.112**	0.333***	0.200***
	0.042	*0.046*	*0.042*	*0.062*
=1 if poor performance blamed on CEO's decisions in *t*-1	0.218	0.609**	0.074	0.756**
	0.167	*0.238*	*0.179*	*0.326*
=1 if poor performance blamed on bad luck in *t*-1	-0.020	-0.078	0.102	0.238
	0.103	*0.127*	*0.107*	*0.174*
=1 if CEO viewed as incompetent in *t*-1	-0.362*	-0.732***	-0.420**	-0.343
	0.220	*0.194*	*0.208*	*0.356*
Deal characteristics				
log investment cost through *t*-1	0.067***	0.295***	0.101***	0.900***
	0.025	*0.038*	*0.038*	*0.075*
=1 if privatization	0.116	0.087***	0.189	0.273***
	0.147	*0.017*	*0.175*	*0.027*
=1 if deal is staged	-0.367***	-0.139***	-0.277***	-0.135***
	0.085	*0.011*	*0.096*	*0.015*
Macroeconomic conditions				
lagged EBRD transition indicator	0.564***	0.706***	0.212	0.980***
	0.170	*0.034*	*0.171*	*0.057*
lagged real GDP growth	0.045***	0.192***	0.051***	0.277***
	0.012	*0.016*	*0.013*	*0.026*
Portfolio firms effects?	No	Yes	No	Yes
Diagnostics				
Wald test: all coeff. = 0 (χ^2)	681.9***	903.6***	907.5***	630.1***
Exogeneity test (LR test)	7.4***	407.7***	11.8***	480.8***
Staiger-Stock (1997) test (χ^2)	20.5***	14.0***	18.5***	10.8***
Pseudo-R^2	12.4%	22.6%	16.5%	29.5%

Table 8. Effect of Board Intervention on Performance Relative to Expectation.

The dependent variable measures a portfolio firm's performance relative to expectations on a five-point scale. Specifically, firms are scored from 1 to 5, where 3 denotes performance in line with expectations (as set out in the firm'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 1 and 2 are naïve models that ignore the potential endogeneity of intervention. Column 1 is estimated using OLS. Column 2 includes random portfolio-firm effects and is estimated using GLS. Columns 3 and 4 are the second stage of instrumental-variable models that treat intervention as endogenous (see Table 3 for the first stage). Because the intervention variable is binary, column 3 is estimated as a Heckman (1978) treatment model. In column 4, we include random portfolio-firm effects. Because Heckman's model cannot accommodate firm effects, this model is estimated using generalized two-stage least-squares. All specifications include country, industry, and time effects. Due to the lagging structure, the number of observations is 1,418. The Staiger-Stock test is a Wald test of the null hypothesis that the instrument does not correlate with board interventions. It has a critical value of 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.

	Dep. variable: Performance rel. to expectations in year *t*+1			
	Naïve model		IV model	
	(1)	(2)	(3)	(4)
Board interventions				
=1 if CEO dismissed in year *t*	0.166	0.227**	1.382***	1.897**
	0.103	*0.100*	*0.361*	*0.921*
Board's information set and beliefs				
performance rel. to expectations in *t*-1	0.311***	0.142***	0.372***	0.297***
	0.039	*0.037*	*0.044*	*0.055*
=1 if poor performance blamed on CEO's decisions in *t*-1	0.272	0.333*	0.196	0.220
	0.181	*0.171*	*0.178*	*0.166*
=1 if poor performance blamed on bad luck in *t*-1	0.294***	0.273***	0.327***	0.335***
	0.093	*0.090*	*0.100*	*0.094*
=1 if CEO viewed as incompetent in *t*-1	-0.209*	-0.135	-0.660***	-0.824**
	0.119	*0.113*	*0.171*	*0.375*
Deal characteristics				
log investment cost through *t*-1	0.037***	0.026**	0.041***	0.034**
	0.014	*0.013*	*0.015*	*0.016*
=1 if privatization	0.197	0.233	0.156	0.153
	0.138	*0.165*	*0.152*	*0.148*
=1 if deal is staged	-0.079	-0.095	-0.119*	-0.138*
	0.067	*0.081*	*0.070*	*0.078*
Macroeconomic conditions				
lagged EBRD transition indicator	-0.194	-0.222	-0.199*	-0.215
	0.118	*0.142*	*0.105*	*0.139*
lagged real GDP growth	0.007	0.006	0.015	0.015*
	0.009	*0.009*	*0.009*	*0.009*
Portfolio firms effects?	No	Yes	No	Yes
Diagnostics				
Wald test: all coeff. = 0 (χ^2)	277.5***	176.5***	249.2***	147.4***
Wald test: independent equations	n.a.	n.a.	9.2***	n.a.
Staiger-Stock (1997) test (χ^2)	n.a.	n.a.	15.6***	19.3***
R^2	17.1%	14.4%	11.4%	7.9%

Table 9. Robustness Tests.

This table reports variations on the bivariate probit specifications reported in Table 7. In Panel A, we redefine the horizon over which exit is measured. Panels B and C 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 4 for further details. In Panel D, 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 E, we allow for country-level heterogeneity in the effect of law changes on CEO 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 7 are included in the estimation but not reported. The number of observations is 2,058 in Panels A, D, and E; 1,323 in Panel B; and 1,705 in Panel C. 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	
Firm effects?	No	Yes	No	Yes
	(1)	(2)	(3)	(4)
Panel A: Alternative exit horizons				
exit in years t to t+1	0.744*	1.136***	1.552***	1.402***
	0.394	*0.269*	*0.413*	*0.255*
Panel B: Country exclusions				
exclude countries without within-sample-period law changes	1.452***	1.708***	1.024*	1.243***
	0.395	*0.298*	*0.592*	*0.476*
Panel C: Most active countries				
five most active countries	0.885**	1.632***	1.190**	1.316***
	0.424	*0.266*	*0.600*	*0.490*
Panel D: Time-varying law changes				
IV uses indicators for law change in year t-i, i = –2, –1, 0, +1, +2	1.121**	1.724***	1.417***	1.712***
	0.538	*0.219*	*0.335*	*0.158*
Panel E: Heterogeneous country effects				
use country-level law change indicators	0.950**	1.626***	1.284***	1.111**
	0.421	*0.249*	*0.355*	*0.475*