

International Cross-Listing, Firm Performance, and Top Management Turnover: A Test of the Bonding Hypothesis

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ABSTRACT

We examine a primary outcome of corporate governance, namely, the ability to identify and terminate poorly performing CEOs, to test the effectiveness of U.S. investor protections in improving the corporate governance of cross-listed firms. We find that firms from weak investor protection regimes that are cross-listed on a major U.S. Exchange are more likely to terminate poorly performing CEOs than non-cross-listed firms. Cross-listings on exchanges that do not require the adoption of stringent investor protections (OTC, private placements, and London listings) are not associated with a higher propensity to remove poorly performing CEOs.

DOES CROSS-LISTING IN THE UNITED STATES IMPROVE the corporate governance of foreign firms? The “bonding hypothesis” proposed by Coffee (1999, 2002) and Stulz (1999) predicts that after listing on a major U.S. stock exchange, foreign firms become subject to stringent U.S. investor protections that constrain insiders from expropriating minority shareholders. Because this hypothesis has important implications for the effectiveness of U.S. laws and enforcement as well as the efficacy of market-based approaches in improving global corporate governance, it has attracted the recent attention of academics and practitioners alike.

To date, empirical support for the bonding hypothesis is principally drawn from the large literature that examines the economic consequences of cross-listing in the United States.¹ However, as Leuz (2006) notes, the evidence in many of these studies is fairly indirect, as it is difficult to attribute the economic consequences of cross-listing directly to the bonding hypothesis because many

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¹ Karolyi (1998, 2006) and Benos and Weisbach (2004) provide comprehensive surveys. We also discuss the literature in Section II of this paper.

theories of cross-listing have similar economic predictions.² Moreover, the validity of the bonding hypothesis has been called into question by a number of recent studies that document cross-listed firms' lack of compliance with certain U.S. laws and the low number of enforcement actions by U.S. legal institutions (see, for example, Siegel (2005) and Lang, Raedy, and Wilson (2006)). Therefore, whether U.S. securities laws and regulations improve the corporate governance of cross-listed firms is under debate as the nascent empirical evidence is predominantly indirect and yields mixed results.

In this paper we pursue a different approach in testing the bonding hypothesis and examine a direct *outcome* of corporate governance: the propensity to replace poorly performing CEOs. We argue that if cross-listing actually results in increased shareholder protections, we should be able to observe specific outcomes that are consistent with improved corporate governance. We focus on the sensitivity of top executive turnover to performance since an extensive body of international research shows that a necessary component of effective corporate governance is the ability to identify and replace poorly performing CEOs (see, for example, Kaplan (1994), Coffee (1999), Murphy (1999), Volpin (2002), Dahya, McConnell, and Travlos (2002), Gibson (2003), DeFond and Hung (2004)). We compile a database of 70,976 firm-year observations from 47 countries from 1992 to 2003 to test the hypothesis that CEOs of cross-listed firms are more likely to face termination when firm performance is poor. We find that the relation between CEO turnover and poor performance is stronger for cross-listed firms than non-cross-listed firms, and that the stronger turnover to poor performance relation for cross-listed firms is concentrated in firms listed on major U.S. exchanges (for example, Level 2 and 3 American depositary receipts (ADRs)). Firms that list in the over-the-counter (OTC) market (Level 1), conduct private placements (Rule 144a), or even list in London do not have a significantly different relation between CEO turnover and performance from non-cross-listed firms. Further, we find that the increased relation between CEO turnover and poor performance for cross-listed firms is strongest in countries with weak investor protections. Overall, our results are consistent with the hypothesis that U.S. securities laws and regulations improve the corporate governance of cross-listed firms.

We also investigate several alternative explanations for our results, including the potential endogeneities that arise in a study of cross-listing and governance due to the nonrandom nature of the decision to list in the United States. For example, we investigate if our results are due to the notion that better-governed firms are the ones that self-select to cross-list. To do so, we examine several specifications that measure the sensitivity of CEO turnover to performance for cross-listed firms *prior* to cross-listing. These tests show that the relation of turnover to performance is insignificant (significant) in the pre-cross-listed (post-cross-listed) period, which suggests that our results are not an artifact of the pre-cross-listed governance status of our sample firms. We

² For further details on other theories that have been argued to generate similar predictions (e.g., market segmentation, investor recognition, increased liquidity, and better information) see the discussion in Doidge, Karolyi, and Stulz (2004a) and Hail and Leuz (2004).

also examine if other potential control changes around cross-listing such as privatizations, changes in ownership, M&A events, and changes in the composition of board of directors can explain our results. We find that our results remain robust to these events. Further, we examine if cross-listing induces top management to leave their jobs to pursue employment in the United States where they are likely to be more highly compensated or if cross-listed firms terminate poorly performing management because they are able to access a more international pool of top-management candidates. We find that firms that change CEOs tend to replace them with managers from their domestic labor pool and that departing CEOs most often get jobs in the local market. Therefore, shifts in the labor market also do not appear to explain our results.

We subject our tests to a battery of firm- and county-level robustness tests as well. We find our results are robust to country, industry, and year fixed effects in addition to the possible entrenchment effects of concentrated ownership structures. Our findings are also robust when we exclude countries that contain the largest portion of our sample, remove observations surrounding the Asian financial crises, and omit financial and regulated industries. An important methodological note is that all of our analyses control for the recently recognized difficulty in implementing and interpreting interaction effects in nonlinear models (see, for example, Ai and Norton, (2003)).

Our results advance the literature in several ways. First, our findings add to the debate on whether U.S. securities laws and enforcement are effective in reaching non-U.S. firms. Second, by showing that CEOs of cross-listed firms are more likely to face termination when firm performance is poor, our results also contribute to the literature by documenting a specific channel through which cross-listing improves corporate behavior, something that is not well-documented in the literature.³ Finally, our findings also have implications for the growing literature that examines how global corporate governance can be improved (see, for example, LaPorta et al. (2000) and Coffee (2002)). This research stems from a large number of studies that show that the economic consequences for firms located in countries with poor investor protections are severe.⁴ Given the economic impact of poor investor protections and the corresponding difficulty in changing a country's legal structure (i.e., legal convergence), an important question is whether market-based approaches (i.e., functional convergence), such as opting-in to a better legal system via cross-listing, can improve corporate governance. Our finding that cross-listing in the United States is associated with improved corporate governance is consistent with the hypothesis that the functional convergence of legal systems to a higher global standard is possible.

The remainder of the paper proceeds as follows. Section I discusses related literature. Section II describes the data. Section III presents the research design. Section IV shows the results and Section V presents robustness tests. Section VI concludes.

³ See, for example, the discussion in Leuz (2006).

⁴ See, for example, LLSV (1997, 1998) as well as the survey by Beck and Levine (2004).

I. Related Literature

The bonding hypothesis of Coffee (1999) and Stulz (1999) posits that firms cross-listed on a major U.S. stock exchange have better corporate governance than non-cross-listed firms from the same country, *ceteris paribus*, since cross-listed firms are subject to strong U.S. investor protections.⁵ For example, cross-listed firms on U.S. exchanges must adhere to U.S. disclosure practices, which require them to reconcile their net income and shareholder's equity to U.S. generally accepted accounting principle (GAAP), disclose the identity of majority shareholders (10% or greater), and follow detailed procedures during tender offers and going private transactions. These firms are also subject to far-reaching U.S. investor protection laws such as the Foreign Corrupt Practices Act and, more recently, the Sarbanes Oxley Act. Cross-listed firms are also subject to punishment by U.S. law enforcement, both by the Securities and Exchange Commission (SEC) as well as private investor law suits, and to increased scrutiny from intermediaries such as financial analysts and debt-rating agencies.⁶ In contrast, listing on the OTC market or conducting a private placement allows substantial exemptions from these laws and regulations.⁷ Specifically, the bonding hypothesis predicts that, *ceteris paribus*: (1) cross-listed firms will have better corporate governance than non-cross-listed firms, (2) the difference in governance between cross-listed firms and non-cross-listed firms will be greatest in the countries with the weakest investor protections, and (3) cross-listings that require the most stringent U.S. investor protections (i.e., on the NYSE, AMEX, or NASDAQ) will have the largest differences in corporate governance. In this way, cross-listing in the United States represents a market-based approach to increased investor protection.

While in theory a cross-listing in the United States should lead to more effective corporate governance, the ability of a cross-listing to serve as a bonding mechanism is under debate. On the one hand, several empirical studies examine the economic impact of cross-listing in the United States and find evidence that is consistent with the bonding hypothesis. This line of research finds that cross-listed firms from weak investor protection countries have larger stock-price reactions (Foerster and Karolyi (1999), Miller (1999)), higher valuation (Mitton (2002), Doidge, Karolyi, and Stulz (2004a)), more scrutiny by financial analysts (Baker, Nofsinger, and Weaver (2002), Lang, Lins, and Miller (2003)), lower cost of capital (Errunza and Miller (2000), Hail and Leuz (2004)), better information environments (Bailey, Karolyi, and Salva (2005)), lower voting premiums (Doidge (2004)), and more access to external finance (Reese and Weisbach (2002), Lins, Strickland, and Zenner (2005)). However, ascribing the evidence contained in many of these studies directly to the bonding hypothesis

⁵ It is important to note that while firms may choose to cross-list for a variety of reasons, once they are listed they become subject to U.S. laws and regulations.

⁶ Coffee (2002) calls these intermediaries "financial watchdogs."

⁷ For example, these firms are not required to register under the Exchange or Securities acts and are therefore exempt from most civil liability provisions and do not have to follow U.S. disclosure practices (Doidge (2004)).

is difficult given the well-known challenge in distinguishing among the various theories of cross-listing and the endogeneity issues inherent to this literature.⁸

On the other hand, the evidence in several recent studies suggests bonding via cross-listing in the United States is ineffective. For example, Siegel (2005) finds that the SEC and minority shareholders have rarely enforced U.S. laws against cross-listed firms and Lang, Raedy, and Wilson (2006) find that the accounting data of cross-listed firms from weak investor protection environments are of lower quality even though cross-listed firms are required to follow nominally similar accounting standards as U.S. firms. However, the approaches in these papers are not without their drawbacks, as Coffee (2002) and Benos and Weisbach (2004) suggest that measuring the incidence of legal actions may understate the deterrent benefit of laws, and Leuz (2006) argues that disclosure quality differences between cross-listed and U.S. firms may not be clear evidence against bonding as cross-listed firms are allowed considerable discretion in preparing their financial statements to U.S. GAAP.

Another challenge researchers face when testing the bonding hypothesis is that it is often difficult to assess the *quality* of governance from observed *mechanisms* of governance because governance mechanisms often substitute or complement one another, a finding that Doidge, Karolyi, and Stulz (2004b) emphasize is dependant on the extent of a country's investor protections. Further, this issue is likely to be exacerbated for cross-listed firms, given the many financial and regulatory changes that take place around a listing (see, for example, Lang, Lins, and Miller (2003, 2004)).

In this paper, rather than calculating the stock-price consequences, legal enforcement incidents, or changes in governance mechanisms around a cross-listing to infer improvements in investor protections, we measure a direct outcome of corporate governance: the propensity to replace poorly performing CEOs. Why CEO turnover? Replacing poorly performing CEOs is argued to be a necessary condition for good corporate governance (Shleifer and Vishny (1989, 1997)) and the sensitivity of top executive turnover to performance as a measure of the quality of corporate governance has been supported by a large number of studies in the United States and abroad, including recent research by Dahya et. al (2002), DeFond and Hung (2004), Gibson (2003), and Volpin (2002).⁹

II. Sample Selection and Descriptive Statistics

A. Sample Selection

Our empirical analysis consists of three main parts. First, we investigate whether the sensitivity of top executive turnover to poor firm performance is higher for cross-listed firms. In these tests we differentiate cross-listings by type in order to test whether cross-listings on major U.S. exchanges, which require the strongest governance provisions, have the largest effect. Second, we

⁸ For example, Sarkissian and Schill (2006) argue that valuation gains to cross-listing are transitory.

⁹ For U.S.-based studies see Hermalin and Weisbach (2003) and citations contained therein.

test if the effect of bonding is greatest for firms that are located in the countries with the weakest investor protection laws. We do so by examining the sensitivity of top executive turnover to poor firm performance across legal origins and investor protection laws. Finally, we conduct a battery of tests designed to gauge the robustness of our results by examining the sensitivity of top executive turnover to poor firm performance in the pre-cross-listing period, the effect of other potential governance changes concurrent with cross-listing, the exclusion of turnover that occurs in the list year, and the departing and entering CEOs' work history. We also rerun our tests excluding countries that contain the largest portion of our sample firms, omitting firms with large block ownership, removing observations surrounding the Asian financial crisis, and excluding financial and regulated firms.

To execute this analysis, we gather data on executive turnover and firm performance between 1992 and 2003 from the Worldscope database.¹⁰ The initial sample consists of approximately 38,000 firms from 59 countries. We exclude firms with missing firm-specific financial and executive data, firms with no identifiable top manager, and firms located in countries with missing legal environment data. We also exclude U.S. firms as the bonding hypothesis predicts differences between cross-listed and non-cross-listed firms, rather than differences between cross-listed and U.S. firms (see, for example, Leuz (2006)). Finally, we exclude firms that are reported in the Worldscope database only once because we need at least two consecutive years of nonmissing data on company officers and their titles to compute CEO turnover. The resulting sample includes 70,976 firm-year observations of 19,091 firms from 47 countries over 1992–2003. Every country in our final sample except Zimbabwe has at least one cross-listed firm in the United States. A breakdown of the sample distribution across countries, cross-listing status, and years is reported in Table I.

We obtain the list of cross-listed firms using several sources including the Bank of New York, Citibank, NYSE, and Nasdaq and verify the listing dates using Lexis–Nexis searches, Form 20-F, etc. Exchange-traded cross-listings are denoted as *Level 2/3*, over-the-counter cross-listings as *Level 1*, and private placements as *Rule 144a*. The data set also takes into account ADR program upgrades, such as from a Level 1 to a Level 2 program, and delistings from the U.S. market. We also include direct listings. Most notably, Canadian firms list their shares on U.S. exchanges directly without issuing ADRs. Given that the increased disclosure and securities law provisions required in listing on a major U.S. exchange are functionally equivalent for ADRs and direct listings, we classify Canadian firms that are traded on both a Canadian and a major U.S. exchange as Level 2/3 ADRs. However, the exclusion of Canadian firms from the sample does not change our conclusions.

We follow DeFond and Hung (2004) and use the titles CEO, Chief Executive Officer, and Chief Executive to identify the top manager in each firm. However,

¹⁰ We use a total of 37 Worldscope CDROMs during our sample period. Because of delays by firms in releasing information and Worldscope's backfilling procedure, Worldscope indicated to us that multiple CDROMs from each year should be used as they often contain different numbers of firms.

Table I
Descriptive Statistics

This table presents the distribution of the sample used in the regression analysis by country, cross-listing status, and year, and descriptive statistics for the main firm-level variables. Panel A describes the number of observations, number of firms, and CEO turnover percentage across countries. Panel B presents the distribution of the sample over time. Panel C displays the distribution of the sample by cross-listing status. Panel D presents the summary statistics for the sample used in the regression analysis. The last column in Panel D reports the median differences of the firm performance variables between the CEO turnover and nonturnover observations and the related results from a nonparametric test on the equality of medians. *CL* dummy is 1 if the firm cross-lists in the United States, 0 otherwise. *Level 2/3* dummy is 1 if the firm has a Level 2 or Level 3 ADR program, 0 otherwise. *Level 1* dummy is 1 if the firm has a Level 1 ADR program, 0 otherwise. *Rule 144A* dummy is 1 if the firm has a *Rule 144A* issuance, 0 otherwise. *Lagged Earnings Ratio* is the 1-year lagged ratio of earnings before interest and taxes to total assets. *Lagged Excess Returns* is the 1-year lagged total stock returns in excess of the country average. Total Assets is measured in million \$U.S. *** indicates significance at the 1% level.

Panel A: By Country			
Country	No of Obs.	No of Firms	CEO Turnover %
Argentina	36	18	25.00
Australia	2,463	1,001	15.79
Austria	530	160	15.09
Belgium	711	187	14.21
Brazil	348	155	11.78
Canada	3,455	1,011	19.28
Chile	473	152	18.39
China	276	170	19.57
Colombia	3	3	33.33
Czech Republic	83	42	15.66
Denmark	1,146	283	14.22
Finland	766	212	13.71
France	3,200	1,019	12.25
Germany	3,692	1,047	17.39
Greece	517	216	15.28
Hong Kong	2,028	797	16.18
Hungary	114	36	19.30
India	1,268	358	13.17
Indonesia	1,090	324	24.13
Ireland	418	93	14.83
Israel	217	90	21.66
Italy	1,192	318	18.04
Japan	21,009	3,776	14.15
Korea	1,704	675	38.40
Luxembourg	59	18	15.25
Malaysia	2,185	687	13.27
Mexico	304	117	17.76
Netherlands	1,034	305	19.05
New Zealand	294	90	18.71
Norway	356	110	18.82
Pakistan	459	111	12.64
Peru	87	43	24.14
Philippines	674	205	20.47
Poland	239	83	19.67

(continued)

Table I—Continued

Panel A: By Country						
Country	No of Obs.	No of Firms		CEO Turnover %		
Portugal	152	62		9.21		
Singapore	1,304	429		17.02		
South Africa	1,210	450		16.28		
Spain	590	163		16.95		
Sri Lanka	37	14		8.11		
Sweden	1,359	420		18.91		
Switzerland	1,413	360		20.74		
Taiwan	1,241	423		25.56		
Thailand	898	269		18.04		
Turkey	316	124		15.19		
United Kingdom	9,981	2,448		14.59		
Venezuela	25	11		4.00		
Zimbabwe	20	6		25.00		

Panel B: By Year						
Year	No of Obs.	No of Firms		CEO Turnover %		
1992	1,602	—		17.85		
1993	4,081	—		17.74		
1994	4,721	—		13.85		
1995	5,369	—		11.51		
1996	6,083	—		12.94		
1997	6,790	—		13.78		
1998	6,359	—		15.85		
1999	6,893	—		14.84		
2000	6,813	—		23.16		
2001	7,712	—		19.85		
2002	9,962	—		17.51		
2003	4,591	—		14.75		

Panel C: By Cross-listing Status						
Cross-listing Status	No of Obs.	No of Firms		CEO Turnover %		
Non-CL firms	65,563	17,729		16.06		
CL firms	5,413	1,362		19.18		
Level 2/3	2,088	609		21.50		
Level 1	2,494	565		16.92		
Rule 144A	801	188		20.22		
Total	70,976	19,091		16.30		

Panel D: Summary Statistics						
	N	Mean	Median	5 th Percentile	95 th Percentile	Turnover vs. Nonturnover (Medians)
Lagged Earnings Ratio	70,976	0.218	0.052	−0.133	0.202	−0.006***
Lagged Excess Returns	62,333	−0.011	−0.079	−0.724	0.909	−0.030***
Total Assets	70,976	259,278	372.651	15.534	494,872	—

many countries use other titles for top managers, which vary across and within countries. We use two sources to determine the top manager in the rest of the sample. When available, we use the top manager titles used by DeFond and Hung (2004) and Gibson (2003). For example, the titles CEO, Chief Executive Officer, Chief Executive, and President are used to identify the top manager in Argentina. We exclude firms in which the top manager title is shared by two officers to prevent a split turnover (Gibson (2003)). For the remaining 15 countries not covered in DeFond and Hung (2004) or Gibson (2003) (4.68% of our sample), we use press accounts, country experts' opinions, and manual data inspections of manager titles in each country to determine the top manager title. A list of top manager titles used in each country is displayed in the Appendix.¹¹ After the top manager in the firm is identified, we first compare the last names and the first letter of first names of top managers of the firm over time to determine whether there was a top manager replacement in any given year. We next hand-check CEO turnover events for the entire sample given that Defond and Hung (2004) find that first names of managers do not consistently precede their last names in several Asian countries such as Korea and Japan, and *Worldscope* infrequently contains typos on executive names for foreign firms.

As in DeFond and Hung (2004) and Gibson (2003), we do not know whether a CEO turnover event is voluntary (for example, due to retirement) because the *Worldscope* does not provide information on CEO age and tenure, and media coverage in English for the sample firms varies substantially across countries. Hermalin and Weisbach (2003) argue that voluntary turnover is unlikely to be related to performance, and hence not distinguishing between voluntary and forced turnovers events leads to additional noise in the dependent variable, which only affects standard errors. Consistent with their assertion, the empirical evidence suggests a similar or more sensitive relationship between CEO turnover and performance for involuntary (forced) replacements (see, for example, Huson, Parrino, and Starks (2001), Dahya et al. (2002), and Kaplan and Minton (1994)). Therefore, we do not expect this data limitation to alter our conclusions.

B. Descriptive Statistics

Panel A of Table I provides summary statistics for the sample based on a firm's country of domicile. Turnover ranges from a low of 4% in Venezuela to a high of 38.4% in Korea, with an average of 16.30%. For comparison, over our sample period the U.S. turnover rate was 12.86%. Similar to other studies that employ the *Worldscope* database, there is a clustering of observations in Japan and United Kingdom. Although our analysis is based on fixed country effects to ensure we are comparing CEO turnover differences within countries, in robustness tests reported later in the paper we remove observations from Japan and the United Kingdom and find that our conclusions are unaffected.

¹¹ We use the terms CEO turnover and top manager turnover interchangeably.

Panel B of Table I shows turnover by year, which ranges from a low of 11.51% in 1995 to a high of 23.16% in 2000. Panel C of Table I presents turnover by cross-listing status. The panel indicates that cross-listed firms have higher CEO turnover than non-cross-listed firms (19.18% vs. 16.06%). Of the cross-listed firms, CEO turnover is greatest for Level 2/3 firms, followed by Rule 144a and then Level 1 companies (21.50%, 16.92%, and 20.22%, respectively). Panel C of Table I also shows that 1,362 foreign firms are identified as cross-listed in our sample, of which 609 are exchange-traded cross-listings (*Level 2/3*), 565 are OTC cross-listings (*Level 1*), and 188 are private placements via Rule 144A issuance (*Rule 144a*).

We consider various measures of firm performance, including both operating performance measures and stock-price-based measures. We augment stock price data from Worldscope with data from Datastream International where possible. However, we expect the operating performance measures to be a better proxy in our international setting, as both Volpin (2002) and DeFond and Hung (2004) find that stock returns are not related to CEO turnover in countries whose markets are characterized by high stock-price synchronicity and illiquidity, attributes that make stock-price-based measures less informative.¹²

For our main tests, we focus on the ratio of accounting earnings before interest and taxes (EBIT) to book value of assets (*earnings ratio*), and the total stock returns in excess of the country average (*excess returns*). We follow DeFond and Hung (2004) and Volpin (2002) and use EBIT among accounting-based firm performance measures because it is not influenced by firms' capital structure policies or by differential country-level tax regimes. Similar forms of both variables are used extensively to proxy for firm performance in studies examining the sensitivity of CEO turnover to firm performance.¹³ We lag both performance variables by 1 year to prevent a possible overlap of the replaced CEO's performance with that of the new CEO. Panel D of Table 1 reports sample statistics of the main performance measures and shows that the lagged performance measures are significantly lower in firm-years with CEO turnover than in non-turnover years. In terms of the depth of the sample, the mean (median) number of years a firm is in our regression analysis is 3.84 (3) years.

We also use sales growth and the change in EBIT to total assets as alternative accounting-based measures of firm performance and obtain qualitatively similar results. In addition, we recompute our firm performance measures in which industry-adjusted performance is calculated as firm performance minus the median value of the corresponding two-digit SIC global industry and obtain similar results.

¹² Harvey (1995) shows that first-order autocorrelations in emerging markets are positive and significant and Lesmond (2005) finds that liquidity-related transactions costs in countries with weak legal institutions are higher than in markets with strong legal systems.

¹³ See Huson et al. (2001), Mikkelsen and Partch (1997), Gibson (2003), and Kang and Shivdasani (1995) for the accounting-based measure and Weisbach (1988), Kang and Shivdasani (1995), Defond and Hung (2004), Huson, Parrino, and Starks (2001), and Hadlock and Lumer (1997) for the stock-market-based measure.

III. Research Design

A. Empirical Model

To test our hypothesis that CEO turnover is more sensitive to poor performance for exchange-traded cross-listed firms than non-cross-listed firms, we estimate a series of probit models that take the form:

$$\begin{aligned} \Pr(\text{Turnover}) = & \phi[\alpha + \beta_1(\text{FirmPerformance})] + \beta_2(L23) \\ & + \beta_{12}(L23 * \text{FirmPerformance}) + \beta_3(L1) \\ & + \beta_{13}(L1 * \text{FirmPerformance}) + \beta_4(R144A) \\ & + \beta_{14}(R144A * \text{FirmPerformance}) + \delta X] \end{aligned} \quad (1)$$

where ϕ is the standard normal cumulative distribution, *L23* refers to exchange-traded cross-listings, *L1* refers to OTC cross-listings, *R144A* refers to private placements, and *X* is a set of firm control variables, country controls, industry controls, and year controls. Note that the cross-listed dummies are time-varying in that they take the value of 1 in the cross-listing year and can switch back to 0 if the firm delists or changes level of cross-listing.

We follow previous research and measure *turnover* as a binary variable that takes the value 1 if the top manager is changed in that year. We include firm size measured as the natural logarithm of the book value of total assets in millions of U.S. dollars. In the regression analysis, we winsorize the continuous variables at the 1% level for each country.

It is also important to note that throughout our analysis, we include country fixed effects that ensure we are measuring within-country differences between cross-listed and non-cross-listed firms as well as controlling for unobserved country effects. In addition, we include industry dummies using the two-digit SIC code to control for global industry-wide factors that may affect CEO turnover and firm performance. Finally, our regressions include indicator variables for each year. Our regressions also correct the standard errors for possible serial correlation and heteroskedasticity by clustering at the firm level.

We test our second hypothesis, which posits that the difference in the sensitivity of top management turnover to performance between cross-listed firms and non-cross-listed firms is greatest in the countries with the weakest corporate governance, by classifying countries into strong and weak investor protection regimes and comparing coefficients across samples. Alternatively, a random country effects specification could be employed with interaction effects, but in our sample this is inappropriate as it fails the Hausman specification test. We focus on three country-level measures of investor protection. The first measure, from La Porta, Lopez-De-Silanes, Shleifer, and Vishny (LLSV) (1997, 1998), is whether the home country has an English legal origin, which is an overall measure of strong investor protections. Following Djankov et al. (2005) we use the anti-director rights index (ADRI), which is a revised version of the original ADRI index from LLSV (1997) that addresses the coding concerns expressed

in Pagano and Volpin (2005) and Spamann (2006). The ADRI represents the degree of minority shareholder protection. We also use the anti-self-dealing index from Djankov et al. (2005), which measures how difficult it is for minority shareholders to thwart the consumption of private benefits by controlling shareholders. Djankov et al. (2005) argue that self-dealing is the central problem of corporate governance in most countries. In unreported tests, we also examine other country-level measures of investor protection from LLSV (1998) and La Porta, Lopez-De-Silanes, and Shleifer (LLS) (2006), such as the rule of law, burden of proof, disclosure, and private law enforcement indexes. In all instances, our results are consistent across every measure of high versus low investor protection. Further, the results are robust to using Spamann's (2006) ADRI.

B. Interpretation of Interactions in Probit Models

Recent research by Ai and Norton (2003) and Powers (2005) emphasizes the difficulty present in interpreting interactions in nonlinear models. Strikingly, the interaction effect cannot be evaluated by looking at the sign, magnitude, or statistical significance of the coefficient on the conventional interaction term. Ai and Norton (2003) show that the interaction effect is conditional on the independent variable, and therefore both the magnitude and statistical significance of the interaction term can vary across observations. For example, in our probit specification the correct marginal effect of a change in the interaction variable between the *L23* dummy and *firm performance* is

$$\frac{\Delta \frac{\partial F(u)}{\partial \text{FirmPerformance}}}{\Delta L23} = (\beta_1 + \beta_{12}) * \phi[(\beta_1 + \beta_{12}) * \text{FirmPerformance} + \beta_2 + X\delta] - \beta_1 * \phi[\beta_1 * \text{FirmPerformance} + \delta X] \quad (2)$$

where $F(u) = \text{Pr}(\text{Turnover})$, which is given by equation (1) and u denotes the regression specification. Equation (2) shows that the marginal effect of the interaction variable may not be zero even when β_{12} is zero. Thus, the standard coefficient on the interaction term may have an incorrect magnitude, standard error, and even sign relative to the true interaction effect.

To ensure our inferences are correct, we use the methodology developed by Norton, Wang, and Ai (2004) to compute the correct marginal effect of a change in the interaction variable between the respective cross-listed dummy and firm performance. We report both the marginal effects and their standard errors and display the graphs of the distribution of marginal effects and the associated z -statistics over the entire range of predicted probabilities for our main models. In tests where our inferences are unambiguous, we also summarize the range of corrected interactions by the mean interaction effect and its significance.

IV. The Effect of Cross-Listing on CEO Turnover

A. By Cross-Listing Type

Table II presents a series of probit regressions that include interactions between firm performance and cross-listing type to test the hypothesis that cross-listed firms have a higher performance to turnover sensitivity than non-cross-listed firms. Model 1 reports the results for the accounting-based performance measure and Model 2 presents the results for the stock-price-based performance measure. All regression models include country, industry, and year fixed effects as well as control for firm size.

Model 1 shows that the interaction between *Level 2/3* and *lagged earnings ratio* is negative and significant (-0.332 , t -statistic $= -2.087$). In contrast, OTC or Rule 144a cross-listings do not have a significantly higher propensity to terminate poorly performing CEOs than non-cross-listed firms' interaction (coefficient $= -0.120$, t -statistic $= -0.452$ and -0.350 , t -statistic $= -0.675$, respectively). This finding is consistent with the hypothesis that non-U.S. firms adopting the strongest governance and reporting requirements by cross-listing in the United States observe outcomes consistent with improved governance over similar firms that are not cross-listed in the United States. However, given the aforementioned problems with interpreting simple interaction terms in discrete choice models, we follow Ai and Norton (2003) and evaluate the corrected marginal effects and their significance at every predicted probability. Figure 1a shows that for major exchange-traded cross-listings, the corrected interaction effects are overwhelmingly negative across the predicted probabilities, while Figure 1b shows that these interaction effects are also significant (less than -1.96) for most probabilities. We summarize the corrected interactive effect and its significance in the last row of Table II by reporting the mean interaction effect and its significance (-0.084 , t -statistic $= -2.082$). In terms of economic significance, the absolute probability of replacing the CEO increases by 1.34% for Level 2/3 ADRs when we move from the top quartile to the bottom quartile of firm performance. For OTC (Rule 144a) cross-listings, the corrected interactive effects, presented in Figures 1c–f and summarized in the bottom rows of Table II, further confirm that the interaction effect is rarely significant across the range of predicted probabilities (the mean-corrected effect is -0.030 , t -statistic $= -0.452$, and -0.086 , t -statistic $= -0.667$, respectively).

For the control variables, we find that firm size is positively related to CEO turnover.¹⁴ Firm performance (*lagged earnings ratio*) is negative yet statistically insignificant, a finding that is the result of pooling countries where firm performance is unlikely to be used to evaluate management.¹⁵ The coefficient on *L2/3* is positive, indicating that exchange cross-listed firms also have higher

¹⁴ In the United States, size is generally thought to capture the effects of CEO and institutional stock ownership, board composition, managerial depth, and formal succession processes (see, e.g., Huson et al. (2001)). Gibson (2003) and DeFond and Hung (2004) also find that firm size is positively related to CEO turnover internationally.

¹⁵ When we split by investor protection regimes, the coefficient on *lagged earning ratio* is negative and significant in high protection countries.

Table II
CEO Turnover and Cross-Listing

This table presents the probit estimates of the relationship between the probability of CEO turnover and firm performance measured by *Lagged Earnings Ratio* (1-year lagged ratio of earnings before interest and taxes to total assets) or *Lagged Excess Returns* (1-year lagged total stock returns in excess of the country average returns). *Level 2/3* dummy is 1 if the firm has a Level 2 or Level 3 ADR program, 0 otherwise. *Level 1* dummy is 1 if the firm has a Level 1 ADR program, 0 otherwise. *Rule 144A* dummy is 1 if the firm has a Rule 144A issuance, 0 otherwise. Log Assets is the natural log of total assets measured in million \$U.S. The continuous variables are winsorized at the 1% level for each country. The interaction effect is defined as the change in the predicted probability of CEO turnover for a change in both the firm performance and the respective cross-listed dummy using the methodology of Norton, Wang, and Ai (2004). The z-statistics appear in parentheses below parameter estimates. Robust standard errors are estimated using the Rogers method of clustering by firm. *** and ** indicate significance at the 1% and 5% level, respectively.

Variable	(1)	(2)
Log assets	0.023*** [9.939]	0.024*** [9.433]
Firm performance: <i>Lagged earnings ratio</i>	-0.002 [-1.528]	-
Firm Performance: <i>Lagged excess returns</i>	-	-0.012 [-1.317]
<i>L2/3</i>	0.077** [2.226]	0.029 [0.860]
<i>L2/3</i> * firm performance	-0.332** [-2.087]	-0.092** [-2.115]
<i>L1</i>	0.043 [1.274]	0.076** [2.491]
<i>L1</i> * Firm performance	-0.120 [-0.452]	0.001 [0.107]
<i>R144A</i>	0.049 [0.704]	-0.012 [-0.202]
<i>R144A</i> * Firm performance	-0.350 [-0.675]	0.015 [0.330]
Constant	-0.905*** [-3.245]	-0.966*** [-3.302]
Country effects	Yes	Yes
Industry effects (two-digit SIC)	Yes	Yes
Year effects	Yes	Yes
Observations	70,976	62,333
Pseudo- R^2	0.026	0.028
Mean interaction effect for <i>L23</i> * Firm performance	-0.084** [-2.082]	-0.022** [-2.009]
Mean interaction effect for <i>L1</i> * Firm performance	-0.030 [-0.452]	0.0002 [0.067]
Mean interaction effect for <i>R144</i> * firm performance	-0.086 [-0.667]	0.003 [0.273]

Panel A. Lagged Earnings Ratio

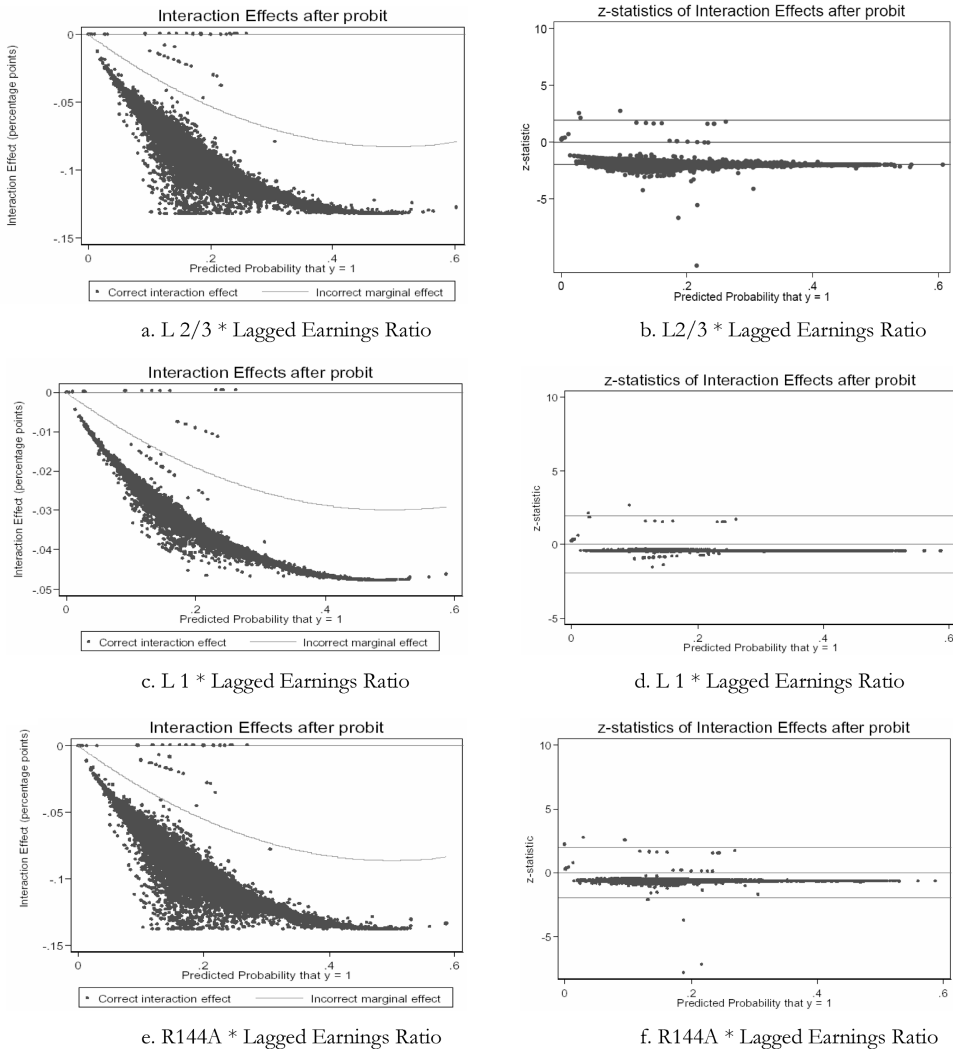


Figure 1. The economic significance of the impact of cross-listing on the relationship between CEO turnover and firm performance. The following graphs display the interaction effects and corresponding z-statistics on the interaction variable between the respective cross-listed dummy and firm performance measure reported in Table II, estimated using Norton, Wang, and Ai (2004). The interaction effect is defined as the change in the predicted probability of CEO turnover for a change in both firm performance and the respective cross-listed dummy. Panel A plots the graphs associated with the lagged earnings ratio measure and Panel B depicts the graphs for the lagged excess returns measure. The lines above and below 0 on the figures located on the right side represent the 5% significance levels (± 1.96).

Panel B. Lagged Excess Returns

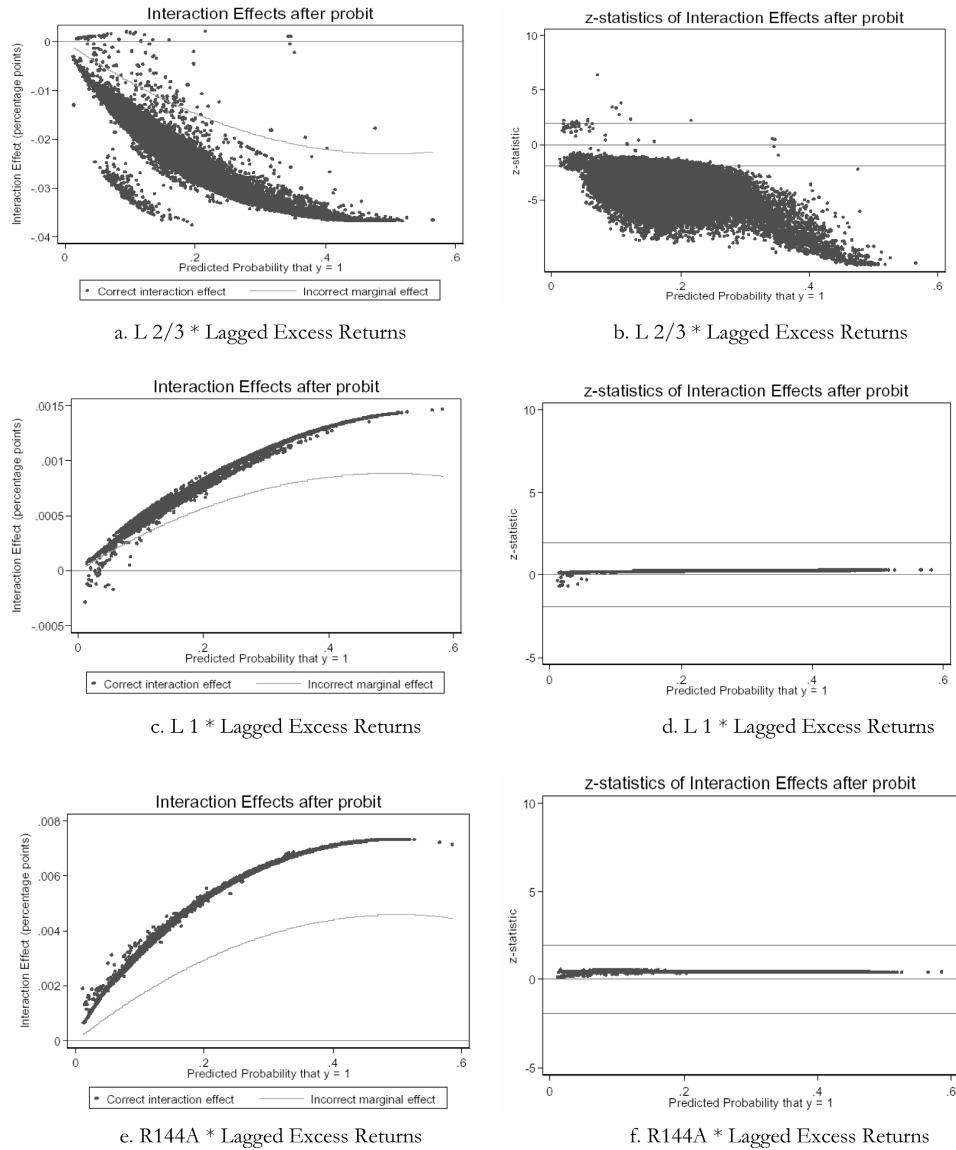


Figure 1—Continued.

absolute turnover, a result that is primarily driven by firms from the United Kingdom.¹⁶

Model 2 of Table II examines the sensitivity of CEO turnover to performance employing our alternative firm performance measure, 1-year lagged excess

¹⁶ Dahya et al. (2002) show that after the Cadbury Act was passed in 1992, CEO turnover increased for U.K. firms.

stock market returns. We find that the interaction between *L2/3* and stock-price-based performance is negative and significant, while the interactions between *L1* or *R144a* and stock-price-based performance are insignificant. Therefore, with this alternative performance measure we continue to find that cross-listed firms that are associated with the most stringent U.S. investor protections are more likely to terminate poorly performing CEOs.

Overall, the results contained in Table II provide support for the hypothesis that cross-listing on a major U.S. exchange, which requires the adoption of stringent U.S. investor protection laws, results in a significantly higher propensity to terminate poorly performing CEOs than their non-cross-listed counterparts. In addition, firms that cross-list via Level 1 or Rule 144a ADRs do not have an increased association between CEO turnover and poor firm performance.

In untabulated results, we also split our sample into countries with high and low stock-price informativeness to examine if the CEO turnover to performance sensitivity is higher in countries where stock prices are more informative about firm-specific performance. Prior research by DeFond and Hung (2004) and Volpin (2002) argues that only in countries where stock prices are informative is CEO turnover related to stock market performance.¹⁷ However, it is important to note that a significant relation between CEO turnover and performance in low informativeness countries is possible if stock prices become more informative about performance due to cross-listing, something that Fernandes and Ferreira (2006) and Dasgupta, Gan, Ning, (2005) suggest occurs. Consistent with DeFond and Hung (2004) and Volpin (2002), we find that the interaction between cross-listing types and firm performance is insignificant in countries that have below-median stock-price informativeness, while in countries where stock prices are informative, cross-listing on a major U.S. exchange results in a higher propensity to shed poorly performing CEOs. These results also suggest that the increased CEO turnover to performance sensitivity for cross-listed firms is not driven purely by stock prices becoming more informative for cross-listed firms in certain countries.

Overall, the results in Table II provide support for the bonding hypothesis. We find that cross-listed firms have outcomes that are consistent with better corporate governance systems than similar non-cross-listed firms. Further, the findings suggest that governance outcome differences are only significant for those firms that adopt the strongest U.S. investor protections by listing on a major U.S. exchange, rather than an OTC listing or private placement. These results provide support for the hypothesis that by cross-listing in the United States, firms are able to opt-in to superior corporate governance.

B. The Strength of Bonding for Firms in Low Investor Protection Countries

Table III tests the third prediction of the bonding hypothesis that the effect of bonding will be greatest for firms domiciled in the countries with the weakest investor protections. We test this hypothesis by splitting the sample

¹⁷ Bushman, Piotroski, and Smith (2004) show that corporate transparency is low in poor investor protection countries.

Table III
CEO Turnover, Cross-Listing, and Legal Environment

This table presents the probit estimates of the relationship between the probability of CEO turnover and firm performance under various measures of a country's legal environment. Firm performance is measured by *Lagged Earnings Ratio*, which is the 1-year lagged ratio of earnings before interest and taxes to total assets, or *Lagged Excess Returns*, which is the 1-year lagged total stock returns in excess of the country average returns. The Civil Law sample includes firms located in countries with a French, German, or Scandinavian legal system. The Common Law sample refers to firms located in countries with the English legal origin. Anti-director rights index measures the degree of minority shareholder protection. Anti-self-dealing is an index of the strength of minority shareholder protection against self-dealing by the controlling shareholder. All these country-level indices are obtained from Djankov et al. (2005). The medians of 3.5 for anti-director rights and 0.42 for anti self-dealing index used in Djankov et al. (2005) are used to group firms into high versus low investor protection regimes (lower than or equal to the median refers to low governance subsamples). *Level 2/3* dummy is 1 if the firm has a Level 2 or Level 3 ADR program, 0 otherwise. *Level 1* dummy is 1 if the firm has a Level 1 ADR program, 0 otherwise. *Rule 144A* dummy is 1 if the firm has a Rule 144A issuance, 0 otherwise. Log Assets is the natural log of total assets measured in million \$U.S. The continuous variables are winsorized at the 1% level for each country. The interaction effect is defined as the change in the predicted probability of CEO turnover for a change in both the firm performance and the respective cross-listed dummy using the methodology of Norton, Wang, and Ai (2004). The z-statistics appear in parentheses below parameter estimates. Robust standard errors are estimated using the Rogers method of clustering by firm. *** and ** indicate significance at the 1% and 5% level, respectively.

Panel A: One-Year Lagged Earnings Ratio						
Variable	Civil Law (1)	Common Law (2)	Low Anti-Director Rights (3)	High Anti-Director Rights (4)	Low Anti-Self-Dealing (5)	High Anti-Self-Dealing (6)
Firm, country, industry, year controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	44,735	26,237	41,007	29,967	15,936	55,020
Pseudo- R^2	0.033	0.022	0.034	0.023	0.031	0.029
Mean interaction effect for <i>L23 * lagged earnings ratio</i>	-0.341***	0.019	-0.333***	0.018	-0.328***	-0.045
	[-3.435]	[0.418]	[-3.339]	[0.386]	[-3.121]	[-1.023]
Mean interaction effect for <i>L1 * lagged earnings ratio</i>	-0.112	0.040	-0.104	0.038	-0.023	-0.031
	[-0.850]	[0.540]	[-0.763]	[0.519]	[-0.154]	[-0.440]
Mean interaction effect for <i>R144 * lagged earnings ratio</i>	-0.164	0.129	-0.183	0.058	-0.121	-0.076
	[-1.071]	[0.502]	[-0.975]	[0.331]	[-0.535]	[-0.497]

Panel B: One-Year Lagged Excess Returns						
Variable	Civil Law (1)	Common Law (2)	Low Anti-Director Rights (3)	High Anti-Director Rights (4)	Low Anti-Self-Dealing (5)	High Anti-Self-Dealing (6)
Firm, country, industry, year controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	39,809	22,519	36,552	25,771	13,455	48,854
Pseudo- R^2	0.033	0.023	0.034	0.024	0.032	0.030
Mean interaction effect for $L23 * lagged\ excess\ returns$	-0.044***	0.013	-0.054**	0.007	-0.035**	-0.009
$L23 * lagged\ excess\ returns$	[-3.256]	[0.801]	[-2.077]	[0.500]	[-2.492]	[-0.655]
Mean interaction effect for $L1 * lagged\ excess\ returns$	0.0004	0.002	-0.009	0.003	-0.0002	-0.003
$L1 * lagged\ excess\ returns$	[0.148]	[0.110]	[-0.250]	[0.751]	[-0.077]	[-0.200]
Mean interaction effect for $R144 * lagged\ excess\ returns$	-0.0002	0.026	0.004	0.004	-0.004	0.014
$R144 * lagged\ excess\ returns$	[-0.015]	[0.931]	[0.267]	[0.250]	[-0.253]	[1.148]

by investor protection regimes and examining the interactions between cross-listing types and firm performance, for both accounting- and stock-market-based performance measures (Panels A and B, respectively). Firm, country, industry, and year controls are included in all regressions.

The first two columns in Panel A of Table III split the sample by legal origin, a classification that proxies for the overall protection of minority shareholders in a country (see, for example, LLSV (1998)). Model 1 shows that in countries with Civil Law tradition, where investor protection is weakest, the corrected interaction between *Level 2/3* and *Lagged Earnings Ratio* is negative and significant (-0.341 , t -statistic = -3.435). Model 1 also shows that the corrected interactions between firm performance and *L1* or *R144a* are insignificant (-0.112 , t -statistic = -0.850 , and -0.164 , t -statistic = -1.071 , respectively). Therefore, in countries with poor investor protections, cross-listing on a major U.S. exchange is associated with an increased CEO turnover to poor firm performance sensitivity. In terms of economic significance, the probability of replacing the CEO increases by 4.36% in absolute terms for Level 2/3 ADRs when we move from the top quartile to the bottom quartile of firm performance measured in Civil Law countries.¹⁸

Model 2 of Panel A presents the results for Common Law countries, where investor protection is strongest. In these countries, we find that all the interactions between cross-listing type and the *Lagged Earnings Ratio* are statistically insignificant. Further, the difference in the interaction terms between Civil and Common Law countries is significant (p -value of lower than 0.01). Therefore, the results indicate that the effect of U.S. investor protections is most significant when the firm's home country investor protections are weakest. Although not reported, we find that the *Lagged Earnings Ratio* coefficient is negative and significant, which is consistent with previous research that finds accounting-based performance is used for managerial performance evaluation.

To further test if the extent of bonding is dependent on the category of protections investors are afforded in a particular country, we also investigate alternative investor protection indices from Djankov et al. (2005). Models 3 and 5 report results for countries classified as having weak protection of minority shareholders and poor safeguards against corporate tunneling. In both models, we find the interactions (both standard and corrected) between *Level 2/3* and *Lagged Earnings Ratio* are negative and significant, indicating that in these low investor protection countries, cross-listing on a major U.S. exchange is associated with increased CEO turnover to poor firm performance sensitivity. Models 4 and 6 report results for the strong investor protection countries. In both these models, the interactions between cross-listing type (*Level 2/3*, *L1*, or *R144a*) are insignificant, indicating no difference in the CEO turnover to firm performance relation in countries that have strong investor protection.¹⁹

¹⁸ For comparison, Huson et al. (2001) show that going from the top quartile to the lowest quartile in EBIT/TA ratio increases the probability of CEO turnover by 2%.

¹⁹ Using alternative measures of investor protection laws from LLS (2006) and LLSV (1998), such as the burden of proof, investor protection, private law enforcement, disclosure, and rule of law indexes, produces similar conclusions. Further, the results are robust to using Spamann's (2006) ADRI.

Panel B provides the results for the stock-price-based firm performance measure. As in Panel A, we find across all weak investor protection subsamples, the interaction between firm performance and *Level 2/3* is negative and significant. For example, the mean-corrected interactive effect in Civil Law countries is -0.044 (t -statistic = -3.256). For other cross-listing types, the interaction coefficients are insignificant. Further, Models 2, 4, and 6 show that in strong investor protection countries, cross-listing does not increase the sensitivity of CEO turnover to poor firm performance.

Taken together, the results in Table III show that in countries where investor protections are weakest, adopting the strongest U.S. investor protection provisions results in significantly greater propensity to terminate poorly performing CEOs, while in countries that already have strong investor protections, cross-listing does not change the CEO turnover to performance sensitivity. Further, the results show that in weak investor protection countries, only cross-listings that require the most stringent of U.S. investor protections (exchange-listed cross-listings) are associated with better governance outcomes. Therefore, the results provide support for the second and third main predictions of the bonding hypothesis.

C. Sensitivity of CEO Turnover to Performance in the Pre-Cross-Listing Period

The previous analysis establishes that cross-listed firms on major U.S. exchanges have outcomes that are consistent with improved corporate governance over non-cross-listed firms. In this analysis, the comparison group is all non-cross-listed firms, which includes firms that may never cross-list as well as cross-listed firms in their pre-cross-listing period. However, an alternative explanation for our findings is that only the better-governed firms choose to cross-list on a major U.S. exchange, which would drive our results. While our previous tests employed time-varying cross-listing indicator variables that equal one only *after* the firm cross-listed, we further examine this issue by performing two additional tests in which we examine the sensitivity of CEO turnover to performance in the pre-cross-listing period.

In the first test, we examine how the sensitivity of CEO turnover to firm performance differs before and after cross-listing, where we restrict the sample to firms that will have a cross-listing of the same type during our sample period.²⁰ The advantage of this experiment is that the non-cross-listed comparison group consists of firms that will have a cross-listing of a similar type during the sample period (that is, the pre-cross-listing period of the cross-listed firms). If firms that pursue exchange-traded cross-listings have better governance prior to cross-listing, then we would expect to find little difference between cross-listed firms and “to-be” cross-listed firms. Due to space considerations, in this test we focus on the broad Civil versus Common Law classifications using the *lagged earnings ratio* performance measure, but other governance

²⁰ We also examine the comparison groups together with indicator variables for cross-listing types (i.e., *Level 2/3*, *Level 1*, and *Rule144a* dummies together) and obtain similar results.

classifications and performance measures produce consistent results.²¹ Model 1 in Table IV shows that the sensitivity of turnover to performance for exchange-traded cross-listings is significantly larger than that for non-cross-listed firms that will eventually cross-list on a major exchange in low shareholder protection countries. Therefore, the results suggest that the difference in governance outcomes is driven by the post-cross-listing period of cross-listed firms. The coefficient on $L2/3$ also shows that these firms have higher absolute turnover after cross-listing. For Level 1 and Rule 144a cross-listings, the interactions between cross-listing type and firm performance are not significantly different between cross-listed and non-cross-listed firms.

The second way we test if the pre-cross-listing governance of our sample firms is driving our results is to exclude all observations for cross-listed firms following the cross-listing year and compare the interactions of “to-be” cross-listed firms’ performance to the full sample of non-cross-listed firms. If the pre-cross-listing status of cross-listed firms is driving our results, we might expect to see results in this analysis similar to our full sample tests presented earlier. An advantage of this test is that we are able to use the full sample of non-cross-listed firms. The disadvantage is that there are relatively few observations in the prelisting period, which is likely to lower the power of the test. Table V shows that when the post-cross-listing observations are excluded, we no longer find that cross-listed firms are more likely to terminate poorly performing CEOs.²² We also find in Table V that *Before L1 * Lagged Earnings Ratio* is positive and significant, which suggests these firms see an improvement from a very low level of corporate governance, where well-performing managers are more likely to leave, to a low level where there is no relation between turnover and performance.

Overall, the results in Tables IV and V show that when compared to their pre-cross-listing status, cross-listing on a major U.S. exchange results in significantly higher CEO turnover to firm performance sensitivity. Therefore, the results indicate that it is not the pre-cross-listing governance of firms that is driving the results. Taken together, the evidence in the preceding sections suggests that in countries with poor investor protections, cross-listing on a major U.S. exchange increases the likelihood that firms will have outcomes consistent with improved corporate governance systems.

D. London Listings

Listing in London is an often-cited alternative for firms that do not wish to subject themselves to the stringent listing requirements in the United States.²³

²¹ For example, using the excess returns measure, the interaction between performance and *Level 2/3* is also statistically significant (insignificant) in Civil (Common) Law, weak (strong) anti-director rights, and low (high) anti-self-dealing subsamples.

²² We also estimate a two-stage model for self-selection that controls for the decision to list in the first stage and find that our results are robust to this specification.

²³ See, for example, “London Calling” in *Forbes Magazine* May 8, 2006.

Table IV
CEO Turnover and Cross-Listing: Firms with a Cross-Listing of the Same Type

This table presents the probit estimates of the relationship between the probability of CEO turnover and firm performance for firms that have or will have a similar type of ADR program during the sample period. The sample in columns 1–2 is limited to firms that have or will have listed on a major U.S. exchange during our sample period. Similarly, the samples in columns 3–4 and 5–6 restrict the sample to firms that list on the OTC market and issue private placements via Rule 144a anytime during our sample period, respectively. *Lagged Earnings Ratio* is the 1-year lagged ratio of earnings before interest and taxes to total assets. The Civil Law sample includes firms located in countries with a French, German, or Scandinavian legal system. The Common Law sample refers to firms located in countries with the English legal origin. This classification of legal regimes is obtained from Djankov et al. (2005). *Level 2/3* dummy is 1 if the firm has a Level 2 or Level 3 ADR program, 0 otherwise. *Level 1* dummy is 1 if the firm has a Level 1 ADR program, 0 otherwise. *Rule 144A* dummy is 1 if the firm has a Rule 144A issuance, 0 otherwise. *Log Assets* is the natural log of total assets measured in million \$U.S. The continuous variables are winsorized at the 1% level for each country. The interaction effect is defined as the change in the predicted probability of CEO turnover for a change in both the firm performance and the respective cross-listed dummy using the methodology of Norton, Wang, and Ai (2004). The *z*-statistics appear in parentheses below parameter estimates. Robust standard errors are estimated using the Rogers method of clustering by firm. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Variable	Exchange-Traded ADRs		OTC-Traded ADRs		Private Placements	
	Civil Law (1)	Common Law (2)	Civil Law (3)	Common Law (4)	Civil Law (5)	Common Law (6)
Log assets	−0.011 [−0.481]	0.028* [1.770]	−0.006 [0.299]	0.013 [0.769]	0.148*** [3.028]	0.122** [2.299]
Lagged earnings ratio	1.405 [0.964]	−0.134 [−0.509]	−0.027 [−0.837]	−0.122 [−0.829]	−0.888 [−0.705]	−2.495 [−0.668]
L2/3	0.631*** [3.219]	0.021 [0.203]	−	−	−	−
L2/3 * lagged earnings ratio	−2.852* [−1.903]	0.021 [0.067]	−	−	−	−
L1	−	−	0.194 [1.547]	−0.063 [−0.567]	−	−
L1 * lagged earnings ratio	−	−	−0.266 [−0.449]	0.094 [0.250]	−	−

(continued)

Table IV—Continued

Variable	Exchange-Traded ADRs		OTC-Traded ADRs		Private Placements	
	Civil Law (1)	Common Law (2)	Civil Law (3)	Common Law (4)	Civil Law (5)	Common Law (6)
<i>R144A</i>	—	—	—	—	0.196 [0.899]	−0.714 [−1.575]
<i>R144A</i> * <i>lagged earnings ratio</i>	—	—	—	—	0.472 [0.303]	3.144 [0.796]
Constant	0.473 [0.635] Yes	−6.433*** [−10.192] Yes	−11.514*** [−40.628] Yes	−6.577*** [−18.563] Yes	−0.769 [−0.828] Yes	−0.320 [−0.427] Yes
Country effects	Yes	Yes	Yes	Yes	Yes	Yes
industry effects (two-digit SIC)	Yes	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	737	1,567	1,496	1,656	544	367
Pseudo- <i>R</i> ²	0.147	0.061	0.059	0.067	0.242	0.171
Mean interaction effect	−0.565* [−1.702]	0.005 [0.061]	−0.092 [−0.633]	0.023 [0.266]	0.046 [0.084]	0.819 [0.748]

Table V
CEO Turnover Prior to Cross-Listing

This table presents the probit estimates of the relationship between the probability of CEO turnover and firm performance under various legal environments prior to cross-listing. Observations for cross-listed firms from the cross-listing year on are excluded. *Lagged Earnings Ratio* is the 1-year lagged ratio of earnings before interest and taxes to total assets. Civil Law sample includes firms located in countries with a French, German, or Scandinavian legal system. The Common Law sample refers to firms located in countries with the English legal origin. Anti-director rights index measures the degree of minority shareholder protection. Anti-self-dealing is an index of the strength of minority shareholder protection against self-dealing by the controlling shareholder. All these country-level indices are obtained from Djankov et al. (2005). The medians of 3.5 for anti-director rights and 0.42 for anti-self-dealing index used in Djankov et al. (2005) are used to group firms into high versus low investor protection regimes (lower than or equal to the median refers to low governance subsamples). *Before Level 2/3* dummy is 1 for the period before the firm cross-lists on a major stock exchange in the U.S., 0 otherwise. *Before Level 1* dummy is 1 for the period before the firm cross-lists in the OTC markets in the U.S., 0 otherwise. *Before Rule 144A* dummy is 1 for the period before the firm has a private placement in the U.S., 0 otherwise. *Log Assets* is the natural log of total assets measured in million \$U.S. The continuous variables are winsorized at the 1% level for each country. The interaction effect is defined as the change in the predicted probability of CEO turnover for a change in both the firm performance and the respective cross-listed dummy using the methodology of Norton, Wang, and Ai (2004). The z-statistics appear in parentheses below parameter estimates. Robust standard errors are estimated using the Rogers method of clustering by firm. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Variable	Civil Law (1)	Common Law (2)	Low Anti-Director Rights (3)	High Anti-Director Rights (4)	Low Anti-Self-Dealing (5)	High Anti-Self-Dealing (6)
Log assets	0.022*** [7.076]	0.029*** [7.211]	0.020*** [6.068]	0.029*** [7.890]	0.020*** [4.084]	0.025*** [8.877]
Lagged earnings ratio	-0.001** [-2.260]	-0.263*** [-6.550]	-0.001*** [-2.912]	-0.273*** [-6.978]	-0.005 [-1.065]	-0.001** [-2.334]
Before L2/3	-0.310 [-1.580]	0.042 [0.478]	-0.342 [-1.471]	0.038 [0.453]	-0.262 [-1.320]	0.027 [0.319]
Before L2/3 * lagged earnings ratio	2.499 [1.444]	0.014 [0.054]	2.472 [1.254]	0.020 [0.077]	2.978 [1.538]	-0.143 [-0.565]
Before L1	-0.181 [-1.509]	-0.042 [-0.626]	-0.270** [-1.994]	-0.030 [-0.471]	-0.131 [-0.862]	-0.055 [-0.883]
Before L1 * lagged earnings ratio	0.048** [2.536]	0.114 [0.817]	0.055*** [2.842]	0.108 [0.781]	0.058*** [2.755]	-0.117 [-0.864]
Before R144A	-0.281* [-1.835]	0.352 [1.125]	-0.354 [-1.558]	0.096 [0.577]	-0.493 [-1.517]	-0.224 [-0.940]

(continued)

Table V—Continued

Variable	Civil Law (1)	Common Law (2)	Low Anti-Director Rights (3)	High Anti-Director Rights (4)	Low Anti-Self-Dealing (5)	High Anti-Self-Dealing (6)
<i>Before R144A * lagged earnings ratio</i>	−0.001 [−1.046]	−1.171 [−0.482]	1.505 [0.694]	0.270*** [6.895]	−0.516 [−0.192]	2.282 [1.077]
Constant	−0.936** [−2.048]	−0.640* [−1.676]	−0.889* [−1.935]	−1.486*** [−2.822]	−1.528*** [−3.415]	−0.869* [−1.909]
Country effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry effects (two-digit SIC)	Yes	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	42,137	23,409	38,556	26,992	14,869	50,662
Pseudo- <i>R</i> ²	0.033	0.021	0.034	0.022	0.030	0.028
Mean interaction effect for before	0.494	0.001	0.466	0.002	0.660	−0.034
<i>L23 * lagged earnings ratio</i>	[1.421]	[0.013]	[1.268]	[0.038]	[1.406]	[−0.567]
Mean interaction effect for before	0.010***	0.028	0.010***	0.026	0.012***	−0.026
<i>L1 * lagged earnings ratio</i>	[2.916]	[0.896]	[3.509]	[0.842]	[3.100]	[−0.871]
Mean interaction effect for before	−0.0001	−0.353	0.265	0.063***	−0.068	0.479
<i>R144 * lagged earnings ratio</i>	[−0.296]	[−0.490]	[0.718]	[5.712]	[−0.187]	[1.134]

Consistent with the hypothesis that a London listing does not convey the same governance commitment as a U.S. listing, Doidge et al. (2006) find that firms with high private benefits of control are more likely to list in London rather than on a major U.S. exchange. Baker et al. (2002) also find that monitoring by financial analysts is lower in London Stock Exchange (LSE) cross-listings versus U.S. cross-listings and Seetharaman, Ferdinand, and Lynn (2002) find that U.K. auditors charge higher fees for client firms that are listed in the United States to compensate for the higher risk of litigation.

To test if cross-listing on the LSE also results in improved governance outcomes, we gather a sample of 688 firm-year observations (145 firms from 27 countries) that trade in London and rerun our models to examine the sensitivity of CEO turnover to performance between firms that cross-listed in London and those firms that did not.²⁴ Since we are examining London listings, we exclude observations from the United Kingdom. Table VI reports that the interaction between *LSE Listing* and firm performance is not significant in any specification. In addition, the results are robust when U.S. firms are included. In unreported tests, we also find that the difference between listing on a major U.S. exchange and on the LSE is statistically significant at the 5% level. Therefore, unlike a major U.S. exchange cross-listing, we do not find that listing on the LSE is associated with better corporate governance outcomes.

E. Labor Market Effects: The Departing and Replacement CEOs' Work History

A potential explanation for our results is that cross-listing also changes the labor market for top management. For example, one possibility is that cross-listing may induce top management to leave their jobs to pursue employment in the United States where they are likely to receive higher compensation. Another possibility is that by cross-listing, firms are able to tap a more international pool of top management candidates and therefore are more likely to fire poorly performing managers. To investigate these effects, we examine a subsample of 150 cross-listed firms that experienced a CEO turnover after cross-listing. Due to the labor intensiveness of this investigation, these CEO turnover events are randomly drawn from sample firms, with an equal number for Level 2/3 ADRs, Level 1 ADRs, and private placements.

We first search news articles as well as reexamine the CEO data on the WorldScope database to find out where the departing CEO found employment. We are able to find the post-turnover employment information for 110 CEOs. Of these, only eight went to a U.S. firm, and only five of these were from a major exchange cross-listed firm (Level 2/3). Further, only one of the five left voluntarily. Therefore, this evidence does not appear to be consistent with a large number of CEOs leaving to take higher paid jobs in the United States.

We next investigate the newly appointed CEOs' previous work experience to see if cross-listed firms may be more likely to terminate poorly performing management when the potential quality of the labor market is higher. For the

²⁴ A list of international firms trading on the LSE is available at www.londonstockexchange.com.

Table VI
CEO Turnover and Cross-Listing on the London Stock Exchange

This table presents the probit estimates of the relationship between the probability of CEO turnover and firm performance under various legal environments for non-U.K. firms cross-listed on the London Stock Exchange. The U.K. firms are excluded from the sample. The sample includes firms listed on the LSE, including 1 from Argentina, 3 from Australia, 8 from Canada, 1 from Denmark, 2 from France, 7 from Germany, 7 from Greece, 5 from Hong Kong, 2 from Hungary, 13 from India, 2 from Indonesia, 26 from Ireland, 7 from Israel, 19 from Japan, 2 from Korea, 2 from Luxembourg, 1 from Malaysia, 3 from Netherlands, 2 from Norway, 5 from Poland, 7 from South Africa, 2 from Spain, 4 from Sweden, 2 from Switzerland, 7 from Taiwan, 4 from Turkey, and 1 from Zimbabwe for a total of 688 firm-year observations. *LSE Listing* dummy is 1 if the firm's shares are traded on the London Stock Exchange, 0 otherwise. The Civil Law sample includes firms located in countries with a French, German, or Scandinavian legal system. The Common Law sample refers to firms located in countries with the English legal origin. Anti-director rights index measures the degree of minority shareholder protection. Anti-self-dealing is an index of the strength of minority shareholder protection against self-dealing by the controlling shareholder. All these country-level indices are obtained from Djankov et al. (2005). The medians of 3.5 for anti-director rights and 0.42 for anti-self-dealing index used in Djankov et al. (2005) are used to group firms into high versus low investor protection regimes (lower than or equal to the median refers to low governance subsamples). Log Assets is the natural log of total assets measured in million \$US. The continuous variables are winsorized at the 1% level for each country. The interaction effect is defined as the change in the predicted probability of CEO turnover for a change in both the firm performance and the respective cross-listed dummy using the methodology of Norton, Wang, and Ai (2004). The z-statistics appear in parentheses below parameter estimates. Robust standard errors are estimated using the Rogers method of clustering by firm. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Variable	Full Sample (1)	Civil Law (2)	Common Law (3)	Low Anti-Director Rights (4)	High Anti-Director Rights (5)	Low Anti-Self-Dealing (6)	High Anti-Self-Dealing (7)
Log Assets	0.022*** [8.555]	0.020*** [6.844]	0.026*** [5.099]	0.018*** [5.797]	0.027*** [6.015]	0.018*** [4.001]	0.024*** [7.590]
Lagged earnings ratio	-0.002* [-1.686]	-0.001** [-2.215]	-0.020*** [-4.692]	-0.001** [-2.387]	-0.236*** [-5.214]	-0.004 [1.073]	-0.001*** [-3.314]
LSE Listing	0.037 [0.484]	0.172 [1.386]	-0.036 [-0.323]	0.179 [1.425]	-0.021 [-0.194]	0.217 [1.086]	0.016 [0.186]
LSE Listing * lagged earnings ratio	-0.091 [-0.229]	-1.680 [-1.290]	0.290 [0.718]	-1.662 [-1.269]	0.079 [0.201]	-2.036 [-1.156]	0.04 [0.111]
Constant	-0.791*** [-2.820]	-0.784*** [-2.691]	-1.630*** [-3.240]	-0.763*** [-2.621]	-1.413*** [-2.970]	-2.224*** [-3.894]	-0.778*** [-2.699]
Country effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry effects (two-digit SIC)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	60,971	44,730	16,237	41,002	19,967	15,931	45,016
Pseudo-R ²	0.028	0.032	0.024	0.034	0.025	0.030	0.031
Mean interaction effect	-0.023 [-0.389]	-0.438 [-0.220]	0.069 [0.590]	-0.435 [-0.221]	0.019 [0.516]	-0.528 [-0.173]	0.009 [0.396]

127 turnover events for which we are able to find information about the new CEO's previous work experience, only six CEOs are recruited from a nondomestic labor market, three are recruited by firms with Level 2/3 ADRs, and the other three by other cross-listed firms.²⁵ The finding that most cross-listed firms still recruit from the domestic labor market suggests that our results are not explained by the possibility that the propensity to terminate poorly performing CEOs in cross-listed firms increases due to access to a new and more international labor market. Taken together, the results of these two tests suggest that potential labor market changes concurrent with cross-listing do not drive our results.²⁶

V. Robustness Tests

In this section, we perform variations of the tests we conduct in Section IV. The purpose of this analysis is to gauge the sensitivity of our results to the exclusion or inclusion of certain observations and to alternative specifications of the tests. Where tabulated, we report results for the accounting-based firm performance measure (*lagged earnings ratio*) and note that the results are robust in all cases for the stock-price-based measure (*lagged excess returns*).

A. Privatizations, Institutional Ownership, Board Composition, and Mergers around Cross-Listing

One potential concern is that another underlying governance change concurrent with cross-listing could be responsible for our results. To investigate this issue, we gather information on potential control changes for cross-listed companies including privatizations, changes in institutional ownership, M&A events, as well changes in the composition of board of directors. We focus on firms that cross-listed between 1992 and 2003 and had pre- and post-turnover data for testing purposes. For privatizations, we match our sample of cross-listed firms with the firms that were fully or partially privatized over our sample period and find that 24 of our sample firms over the period from 1992 to 2003 were privatized during the year before or after cross-listing.²⁷ For M&A events, we match our sample of cross-listed firms with Securities Data Company's Mergers and Acquisitions database to find cross-listed firms that either

²⁵ To be more specific, of 42 newly appointed CEOs in firms with Level 2/3 ADRs, 39 were recruited from domestic firms, 1 from a U.S. firm, 1 from a British firm, and 1 from a Dutch firm. For firms with Level 1 ADRs, 2 out of 43 newly appointed CEOs were recruited from international firms (1 U.S. and 1 French firm), and for firms with Rule 144A issues, only 1 CEO out of 42 newly appointed CEOs was recruited from international firms (a British firm).

²⁶ It is important to note that compensation differences between U.S. and foreign firms could also play a role in the willingness of CEOs to leave newly cross-listed firms. However, disaggregate compensation data are generally unavailable for cross-listed firms (see Bryan, Nash, and Patel (2005)).

²⁷ Fourteen are L2/3, 3 are L1, and 7 are R144 firms. See Megginson and Netter (2001) for a survey of the privatization literature.

bought or sold controlling stakes in targets during the year before and after their cross-listing. From 1992 to 2003, 44 firms cross-listed and also acquired or sold controlling stakes in other companies in the year before or after cross-listing: of these, 27 were Level 2/3, 12 were Level 1, and 5 were R144 firms. For data on potential ownership and board of director changes for cross-listed firms, we hand-collect data from various filings (13F/13D/13G), prospectuses, and annual reports for the year before and after cross-listing. We classify a blockholder as institutional if it was a pension fund, mutual fund, insurance firm, bank, investment bank, credit institution, private equity fund, or venture capital fund and as foreign if the blockholder (institutional or otherwise) was located in a country different from the cross-listed firm's home country. Of the major exchange firms that cross-listed between 1992 and 2003 and had pre- and post-turnover data, 15% had a new foreign or institutional blockholder. For Level 1 (R144a), the percentage of new foreign or institutional blockholders is 11% (5%). In tracking changes in the board of directors around cross-listing, we were able to gather data for 101 of the 139 Level 2/3 firms that cross-listed from 1992 to 2003 and had pre- and post-turnover data. Of these, 24 (10) had a new outside (foreign) director following the cross-listing.²⁸

Using these data, we conduct a series of robustness tests to examine the influence of these potential governance changes that are concurrent with cross-listing. For example, we create indicator variables that equal one if the cross-listing also had one of these potential governance changes and reran our models (both individually and for all events together). In all instances, we find that our results are robust. We summarize these results by creating an index in which the firm gets an additional point if during the years surrounding cross-listing it was involved in a privatization, M&A event, or change in the ownership or board of directors. Table VII includes this control variable and reexamines the analysis of Table IV, the sensitivity of CEO turnover to firm performance before and after cross-listing where we restrict the sample to firms that have a cross-listing of the same type during our sample period. We find that these potential governance changes are insignificantly related to turnover, except for exchange-traded firms from Civil Law countries where the coefficient on the index is negative and significant, a finding that is driven by privatizations. The bottom row of Table VII reports that the mean interaction effect for Level 2/3 cross-listings remains negative and significant, which suggests that these other potential control events do not drive our previous findings.²⁹

B. Exclusion of United Kingdom and Japan

Given that United Kingdom and Japan are the two countries with the greatest number of observations in our sample, an obvious concern is that our results

²⁸ Given the disclosure requirements and practices of these non-U.S. firms, board data are generally only available for the major exchange-traded firms (Level 2/3).

²⁹ Adding an interaction on this events index and the *lagged earnings ratio* in the regression does not alter our findings.

Table VII
CEO Turnover and Cross-Listing: Firms with a Cross-Listing of the Same Type and Concurrent Control Changes

This table presents the probit estimates of the relationship between the probability of CEO turnover and firm performance for firms that have or will have a similar type of ADR program during the sample period. The sample in columns 1–2 is limited to firms that have or will have listed on a major U.S. exchange during our sample period. Similarly, the samples in columns 3–4 and 5–6 restrict the sample to firms that list on the OTC market and issue private placements via Rule 144a anytime during our sample period, respectively. The *Related Events Index* is the number of the four events a cross-listed firm experiences within 1 year around the year of cross-listing. These events are privatizations, M&A events (where more than 50% of the firm's shares are acquired), a new institutional or foreign blockholder (measured at the 10% cutoff point), or a new independent or foreign director on the board of directors. This variable ranges between 0 and 4, with 4 being the maximum number of events. *Lagged Earnings Ratio* is the 1-year lagged ratio of earnings before interest and taxes to total assets. The Civil Law sample includes firms located in countries with a French, German, or Scandinavian legal system. The Common Law sample refers to firms located in countries with the English legal origin. This classification of legal regimes is obtained from Djankov et al. (2005). *Level 2/3* dummy is 1 if the firm has a Level 2 or Level 3 ADR program, 0 otherwise. *Level 1* dummy is 1 if the firm has a Level 1 ADR program, 0 otherwise. *Rule 144a* dummy is 1 if the firm has a Rule 144a issuance, 0 otherwise. Log Assets is the natural log of total assets measured in million \$U.S. The continuous variables are winsorized at the 1% level for each country. The interaction effect is defined as the change in the predicted probability of CEO turnover for a change in both the firm performance and the respective cross-listed dummy using the methodology of Norton, Wang, and Ai (2004). The z-statistics appear in parentheses below parameter estimates. Robust standard errors are estimated using the Rogers method of clustering by firm. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Variable	Exchange-Traded ADRs		OTC-Traded ADRs		Private Placements	
	Civil Law (1)	Common Law (2)	Civil Law (3)	Common Law (4)	Civil Law (5)	Common Law (6)
<i>Related events index</i>	−0.394** [−2.200]	0.090 [0.924]	0.089 [0.271]	−0.190 [−1.202]	−0.418 [−1.189]	−0.195 [−0.758]
Log assets	−0.002 [−0.059]	0.038** [2.059]	0.031 [1.039]	0.014 [0.745]	0.174*** [3.207]	0.236*** [2.819]
<i>Lagged earnings ratio</i>	2.496 [1.329]	−0.172 [−0.658]	−0.005*** [−4.932]	−0.144 [−0.972]	−1.198 [−0.889]	−3.531 [−0.808]
<i>L2/3</i>	0.617** [2.540]	0.053 [0.464]	—	—	—	—
<i>L2/3 * Lagged earnings ratio</i>	−4.267** [−2.227]	0.082 [0.249]	—	—	—	—

(continued)

Table VII—Continued

Variable	Exchange-Traded ADRs		OTC-Traded ADRs		Private Placements	
	Civil Law (1)	Common Law (2)	Civil Law (3)	Common Law (4)	Civil Law (5)	Common Law (6)
<i>L1</i>	—	—	0.230* [1.733]	0.049 [0.403]	—	—
<i>L1 * lagged earnings ratio</i>	—	—	−0.092 [−0.119]	0.216 [0.573]	—	—
<i>R144A</i>	—	—	—	—	0.124 [0.539]	−0.765 [−1.439]
<i>R144A * lagged earnings ratio</i>	—	—	—	—	0.904 [0.553]	2.955 [0.652]
Constant	0.245 [0.241] Yes	−6.877*** [−6.744] Yes	−0.016 [−0.036] Yes	−6.039*** [−12.339] Yes	4.634*** [4.147] Yes	0.216 [0.158] Yes
Country effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry effects (two-digit SIC)	Yes	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	594	1,319	603	1,355	520	336
Pseudo- <i>R</i> ²	0.190	0.062	0.173	0.060	0.248	0.195
Mean interaction effect	−1.066** [−2.338]	0.021 [0.646]	−0.022 [−0.549]	0.055 [1.635]	0.186 [0.144]	0.848 [1.388]

may be primarily or completely driven by observations in these countries. Panel A of Table VIII presents results when we omit these observations. We find that the mean-corrected interactive effect between $L2/3$ and *lagged Earnings Ratio* is negative and significant in low investor protection countries. We continue to find that the interactions for other cross-listing types are insignificant. Thus, our results are robust to the exclusion of observations from the United Kingdom and Japan.

C. Exclusion of Observations during the Cross-Listing Year

Given managers of cross-listed firms value control (see, for example, Doidge et al. 2004a), it seems unlikely that managers would choose to cross-list in order to make their positions more susceptible to termination. Consistent with this hypothesis, recent research by Doidge et al. (2006) shows that firms with high private benefits of control are less likely to cross-list on a major U.S. exchange. Doidge (2005) and Ayyagari (2004) find that cross-listed firms continue to have concentrated ownership after cross-listing, which suggests that ownership concentration diffusion after cross-listing is not driving our results. However, these studies do find that cross-listed firms often sell control blocks during the listing year. In order to ensure the turnover we document is not driven by changes in control around cross-listing, we reexamine our tests when observations of cross-listed firms in the year of cross-listing are omitted. Similar results obtain. For example, Panel B of Table VIII shows that the mean-corrected interactive effect between $L2/3$ and *Lagged Earnings Ratio* is negative and significant in low investor protection countries. This panel also shows that the interactions for other cross-listing types are insignificant. Therefore, CEO turnover changes in the cross-listing year do not explain our results. We also eliminate turnover in the 2 years surrounding cross-listing and continue to obtain similar results.

D. Relative Industry Performance Measures

Morck, Shleifer, and Vishny (1988), Gibbons and Murphy (1990), and Parrino (1997) suggest that internal monitors use relative industry performance measures to evaluate CEO performance. To examine the robustness of our results to this alternative definition of firm performance, we recompute our firm performance measures such that industry-adjusted performance is calculated as firm performance minus the median value of the corresponding two-digit SIC global industry. Panel C of Table VIII reports results for the industry-adjusted lagged earning ratio. We find that across all models, the corrected interaction effect between $L2/3$ and industry-adjusted firm performance is negative and statistically significant, while the interactions for OTC and private placements remain insignificant. In untabulated results, we find similar results for industry-adjusted excess stock market returns. The findings indicate that our results are robust to this alternative definition of firm performance.

Table VIII
CEO Turnover and Cross-Listing: Additional Robustness Tests

This table presents the probit estimates of the relationship between the probability of CEO turnover and firm performance under various legal environments. Panel A excludes observations from the United Kingdom and Japan, Panel B excludes observations from the cross-listing sample, and Panel C uses an industry-adjusted firm performance measure. *Lagged Earnings Ratio* is the 1-year lagged ratio of earnings before interest and taxes to total assets. *Adjusted Lagged Earnings Ratio* is the 1-year lagged ratio of earnings before interest and taxes to total assets minus the median value of the corresponding two-digit SIC global industry. The Civil Law sample includes firms located in countries with a French, German, or Scandinavian legal system. The Common Law sample refers to firms located in countries with the English legal origin. Anti-director rights index measures the degree of minority shareholder protection. Anti-self-dealing is an index of the strength of minority shareholder protection against self-dealing by the controlling shareholder. All these country-level indices are obtained from Djankov et al. (2005). The medians of 3.5 for anti-director rights and 0.42 for anti-self-dealing index used in Djankov et al. (2005) are used to group firms into high versus low investor protection regimes (lower than or equal to the median refers to low governance subsamples). *Level 2/3* dummy is 1 if the firm has a Level 2 or Level 3 ADR program, 0 otherwise. *Level 1* dummy is 1 if the firm has a Level 1 ADR program, 0 otherwise. *Rule 144A* dummy is 1 if the firm has a Rule 144A issuance, 0 otherwise. *Log Assets* is the natural log of total assets measured in million \$US. The continuous variables are winsorized at the 1% level for each country. The interaction effect is defined as the change in the predicted probability of CEO turnover for a change in both the firm performance and the respective cross-listed dummy using the methodology of Norton, Wang, and Ai (2004). The *z*-statistics appear in parentheses below parameter estimates. Robust standard errors are estimated using the Rogers method of clustering by firm. *** indicates significance at the 1% level.

Variable	Panel A: Excluding the U.K. and Japan					
	Civil Law (1)	Common Law (2)	Low Anti-Director Rights (3)	High Anti-Director Rights (4)	Low Anti-Self Dealing (5)	High Anti-Self Dealing (6)
Firm, country, industry, year controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	23,717	16,247	19,989	19,977	15,936	24,019
Pseudo- <i>R</i> ²	0.048	0.024	0.055	0.026	0.031	0.043
Mean interaction effect for <i>L23 * Lagged earnings ratio</i>	-0.343*** [-3.322]	-0.004 [-0.076]	-0.334*** [-3.206]	-0.005 [-0.110]	-0.328*** [-3.121]	-0.061 [-1.240]
Mean interaction effect for <i>L1 * Lagged earnings ratio</i>	-0.059 [-0.395]	0.094 [1.177]	-0.054 [-0.334]	0.089 [1.136]	-0.023 [-0.154]	0.042 [0.517]
Mean interaction effect for <i>R144 * Lagged earnings ratio</i>	-0.157 [-0.973]	-0.039 [-0.128]	-0.152 [-0.765]	-0.065 [-0.351]	-0.121 [-0.535]	-0.170 [-1.012]

Table VIII—Continued

Panel B: Excluding the Cross-Listing Year						
Variable	Civil Law (1)	Common Law (2)	Low Anti-Director Rights (3)	High Anti-Director Rights (4)	Low Anti-Self Dealing (5)	High Anti-Self Dealing (6)
Firm, country, industry, year controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	44,605	26,063	40,893	29,777	15,865	54,787
Pseudo- R^2	0.033	0.022	0.034	0.023	0.031	0.029
Mean interaction effect for $L23 * Lagged earnings ratio$	-0.333***	0.023	-0.324***	0.022	-0.313***	-0.043
Mean interaction effect for $L1 * Lagged earnings ratio$	[-3.380]	[0.491]	[-3.284]	[0.461]	[-3.026]	[-0.963]
Mean interaction effect for $R144 * Lagged earnings ratio$	-0.114	0.056	-0.097	0.051	-0.047	-0.014
	[-0.841]	[0.742]	[-0.706]	[0.749]	[-0.307]	[-0.214]
	-0.189	0.205	-0.201	0.079	-0.077	-0.081
	[-1.341]	[0.747]	[-1.148]	[0.430]	[-0.339]	[-0.535]
Panel C: Relative Industry Performance						
Variable	Civil Law (1)	Common Law (2)	Low Anti-Director Rights (3)	High Anti-Director Rights (4)	Low Anti-Self Dealing (5)	High Anti-Self Dealing (6)
Firm, country, industry, year controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	43,974	26,240	40,327	29,887	15,466	54,748
Pseudo- R^2	0.030	0.015	0.031	0.016	0.024	0.024
Mean interaction effect for $L23 * Adjusted lagged earnings ratio$	-0.395***	0.019	-0.386***	0.015	-0.378***	-0.052
Mean interaction effect for $L1 * Adjusted lagged earnings ratio$	[-3.700]	[0.381]	[-3.621]	[0.315]	[-3.443]	[-1.110]
Mean interaction effect for $R144 * Adjusted lagged earnings ratio$	-0.101	0.062	-0.096	0.060	0.002	-0.016
	[-0.747]	[0.755]	[-0.682]	[0.741]	[0.015]	[-0.208]
	-0.167	0.162	-0.191	0.101	-0.131	-0.066
	[-1.006]	[0.586]	[-0.923]	[0.549]	[-0.507]	[-0.405]

E. Exclusion of Firms with Large Blockholders

The corporate governance effects of large blockholders have been well documented in the literature (see, e.g., La Porta, Lopez-De-Silanes, and Shleifer (LLS) (1999)). Large blockholders could have a negative effect on corporate governance if they are aligned with management in nonvalue maximizing activities and insulate managers from being replaced. Gibson (2003) finds that the link between performance and top management turnover is weaker in emerging market firms with a large domestic shareholder. Further, Doidge et al. (2006) find that the control rights held by the largest blockholder are higher for firms not cross-listed on a U.S. exchange. This raises the possibility that the relation between CEO turnover and performance may be impacted by the relative influence of large blockholders in cross-listed and non-cross-listed firms.

To investigate the role of large shareholders in our results, we collect data from Worldscope on the percentage of shares held by large shareholders (*closely held shares*) and exclude observations with large shareholders. We follow DeFond and Hung (2004) and exclude observations for which blockholders have direct holdings greater than 20%.³⁰ The results of this test (un-tabulated) indicate that our previous results remain qualitatively unchanged. Further, we also obtain similar results when the percentage of closely held shares is employed as a control variable in our full sample regressions. Therefore, our results continue to hold after excluding the influence of large blockholders.

F. Excluding Financial and Regulated Firms

In our previous analysis, we examine the broadest possible sample of firms and include them as part of our controls industry dummies. We are motivated by the fact that across countries, the set of industries and firms that are controlled by government regulation are likely to differ substantially. However, to ensure that our results are not being driven by the inclusion of financial and regulated firms, we omit firms with two-digit SIC codes of 60–69, 48, and 49. In untabulated results, we find that the results are robust to the exclusion of these firms.

G. Excluding the Asian Financial Crisis Period

To ensure our findings are not dependent on the inclusion of the firm-years surrounding the Asian financial crisis, we reestimate our models excluding various windows of observations surrounding the event, including: (1) 1997 and 1998, (2) 1996, 1997, and 1998, and (3) 1996, 1997, 1998, and 1999. In untabulated results, we find that our results are robust to the exclusion of

³⁰ Because of data availability, we are not able to measure indirect ownership acquired along ownership chains.

these years. We also estimate our models excluding years 2002 and 2003 to gauge the effect of Sarbanes–Oxley on our results. Our results are robust to excluding these observations.

H. Excluding Small Firms

While all our regressions control for firm size, we also conduct our tests in which we omit smaller firms and obtain similar results. For example, if we eliminate firms with total assets less than 15 million U.S. dollars, we obtain similar results for the interaction between *L2/3* and *lagged earnings ratio* in Models 1 and 2 of Table III (Civil and Common Law).

VI. Conclusions

Despite the large number of studies examining the costs and benefits of cross-listing in the United States, the effectiveness of a U.S. cross-listing as a bonding mechanism is under debate as it is often difficult to distinguish among the various theories for international cross-listings. In this paper, we argue that if cross-listing in the United States improves investor protection, then we should be able to detect *outcomes* that are consistent with improved corporate governance. We test our hypotheses by examining the relative propensity for cross-listed firms to terminate poorly performing CEOs. We construct a database of over 70,000 firm-year observations from 47 countries and find that cross-listed firms are more likely to shed poorly performing CEOs than non-cross-listed firms. Further, we find that this effect is concentrated in cross-listings on major U.S. exchanges with the strongest investor protections, rather than OTC, private placement, or London listings. Finally, we find that the difference between cross-listed and non-cross-listed firms in CEO turnover sensitivity to firm performance is greatest in the countries with the weakest investor protections. Taken together, our results provide support for the major tenets of the bonding hypothesis and for the ability of cross-listings to improve global corporate governance.

Appendix: Titles Used to Identify the Top Manager in Addition to Chief Executive Officer

This table presents the top manager title for each country in the sample other than “CEO,” “Chief Executive Officer,” and “Chief Executive.” When available, we use the top manager titles used by DeFond and Hung (2004) and Gibson (2003) to identify the top manager. For the remaining countries, we use press accounts and country experts’ opinions, and we also visually inspect executive titles of firms in each country to determine the top manager title. We exclude the top manager titles that contain deputy, vice, or assistant. † refers to the top manager title classification in either DeFond and Hung (2004) or Gibson (2003).

Country	Top Manager Title
Argentina	President
Australia	Managing director†
Austria	Chairman, board of management†
Belgium	Managing director†
Brazil	President†
Canada	None†
Chile	General manager†
China	General manager
Colombia	General manager
Czech Republic	Managing director
Denmark	Managing director†
Finland	Managing director†
France	None†
Germany	Chairman, board of management†
Greece	Managing director†
Hong Kong	Managing director†
Hungary	Managing director
India	Managing director†
Indonesia	President director†
Ireland	Managing director
Israel	President
Italy	Managing director†
Japan	President†
Korea	President†
Luxembourg	None
Malaysia	Managing director†
Mexico	President†
Netherlands	Chairman, board of management†
New Zealand	Managing director
Norway	President†
Pakistan	Managing director†
Peru	General manager
Philippines	President†
Poland	President
Portugal	President†
Singapore	Managing director†
South Africa	Managing director†
Spain	Managing director†
Sri Lanka	Managing director
Sweden	Managing director†
Switzerland	President
Taiwan	President†
Thailand	President†
Turkey	General manager†
United Kingdom	Managing director†
United States	None†
Venezuela	President
Zimbabwe	Managing director

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