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Abstract

We examine how globalization of corporate governance practices influences the dismissal risk of European CEOs. It is hypothesized that the harsh monitoring of the American corporate governance system influences European CEOs' dismissal performance sensitivity, indirectly and directly. The former materializes via European firms cross-listing on U.S. exchanges, the latter results from European firms hiring American independent board members. Both influences are hypothesized to result in increased dismissal performance sensitivity. Based on data from the 250 largest European publicly traded firms we find a significant increase in the dismissal sensitivity in poorly performing companies with American board membership.

JEL classification: G15, G18, G32, M14, M16, M52

Keywords: CEO dismissal, performance sensitivity, globalization, corporate governance, foreign board membership, binary response models

Introduction

Fundamental differences exist between corporate governance systems around the world (e.g., Denis & McConnell (2003); La Porta, de Silanes & Shleifer (1999)). However, there is a trend towards a global harmonization of these systems (e.g. Hansmann & Kraakmann (2001); Perotti & von Thadden (2003)). The trend is visible both at the institutional level via, for instance, the proliferation of national corporate governance codes (starting with Cadbury in 1992) and at the firm level, via, for instance, the global use of executive stock options (starting in the US in the 1950s). There is currently no consensus about the features of an ultimate global corporate governance system, should one appear. There are many indications that the industrial world, at least, has embarked on a route towards a more harmonized corporate government system. Among the four main corporate governance systems – the Anglo-American system, the German system, the Latin system and the Japanese system (see e.g. Shleifer & Vishny (1997) and Goergen (1998)) – the Anglo-American system is commonly seen as the most demanding system (Lucier, Schuyt & Handa (2004). Some will even argue for the "superiority", in market performance terms, of this system (e.g. Economist (2001), p. 32). The strict information requirements imposed by the Security and Exchange Commission (SEC) provide further reasons for regarding the American system as a good proxy for the most demanding and costly corporate governance system. The implementation by the United States Congress of the Public Company Accounting reform and Investor Protection Act 2002 (Sarbanes-Oxley) further underpins this view. Part of the effort to achieve a global cost of capital is to comply with the rules of the "global" corporate governance system, i.e. the American system.

The globalization of the corporate governance systems may take two major routes: one via legislation and institutions and another via corporate actions (Coffee (2002)). In this study we focus on the latter route. We analyze non-American (European) companies and the influence on dismissal performance sensitivity following from their efforts to reap the benefits of compliance with the American corporate governance system. These benefits accrue as a result of firm specific strategies to achieve a global cost of capital (Stulz (1999)). We analyze the influence following from two kinds of effort to do this; to actually comply with the American system (cross-listing) and to signal compliance by recruiting at least one independent American board member.

Past research has highlighted how firms can internationalize their cost of capital by the process of cross-listing on the American markets; either directly (on Nasdaq, NYSE, or Amex) or through an American Depositary Receipt (ADR) (see e.g. Howe & Madura (1990); Sundaram & Logue (1996); Foerster & Karolyi (1999); Miller (1999); Pagano, Röel & Zechner (2002)). For example, Siegel (2005) finds that the beneficial effects that accrued to Mexican firms cross-listing at American exchanges were better explained by reputational bonding (to good corporate governance practices) than by legal bonding, or by forming a strategic alliance with a foreign multinational firm (Siegel (2009)). The author shows that Mexican firms with cross-listing in the US were not legally forced to protect minority shareholder interest, but acted in accordance with good corporate governance as they developed a reputational asset in the market for outside capital.

A second alternative to internationalize the cost of capital with great implications for corporate governance practices, is the recruiting of an independent representative for the American corporate governance system. As shown by Oxelheim & Randøy (2003) this may contribute to institutional contagion of corporate governance practices. They found that this recruitment produced a harsher performance monitoring of CEOs in Norway and Sweden.

It is commonly argued that globalization of the firm produces a more complex task environment for top management (e.g. Finkelstein & Boyd (1998), Sanders & Carpenter (1998)). We argue in this paper that the compliance with the American corporate governance system means a new monitoring regime for the complying firm with increased dismissal performance sensitivity. An indirect support for this is found in the higher compensation to CEOs of non-American firms with Anglo-American listing and/or board influence (Oxelheim & Randøy (2003)). The higher compensation reflects that the American corporate governance system is less tolerant of poor performance (Lucier, Schuyt & Handa (2004)). Since the CEOs anticipate that they may be more heavily penalized for a performance shortfall over their domestic peers, Oxelheim & Randøy (2005) argue that the higher compensation partly reflects a premium for greater risk of dismissal.

The greater CEO dismissal performance sensitivity generates a globalization cost that has been neglected in the literature. This cost reflects a reduced job security, an increased likelihood of shorter tenure, and a potentially negative impact on the reputation of the incumbent in case of dismissal. Countries with a relatively low level of CEO turnover can expect to face higher levels of CEO dismissal performance sensitivity as their firms become exposed to monitoring and regulations from financial markets with less turnover tolerance for poor performance (i.e., the case of American financial markets).

In the empirical part of the paper we study CEO turnover among the 250 largest European publicly traded firms in 2004. In line with the arguments of Hall & Gingerich (2009) and Pedersen & Thomsen (1997), we do not expect the effect of financial globalization to be uniform. With due attention to missing observations, we investigate 270 succession events over the period 2000–2004. We find a significant increase in the dismissal sensitivity in poorly performing companies with American board membership whereas no significant increase is found from cross-listing in the US.

The paper is organized in the following way. In Section 2 we review past studies on dismissal performance sensitivity, and present the research question. We pay special attention to the relevance of agency theory and managerial power. In Section 3 we propose two research hypotheses to be tested, followed by Section 4 in which we describe the European data and focus on key binary relationships. In Section 5, we specify the statistical model and test the hypothesized relationships in a multivariate setting. The results of our model diagnostic and model validation exercise are reflected in Section 6. Finally, we summarize the key findings and discuss managerial and policy implications in Section 7.

Dismissal performance sensitivity

The linkage between globalization and CEO pay has been identified by past studies. Sanders & Carpenter (1998) report a positive relationship between international sales and CEO compensation in U.S. firms, and Girma, Thompson & Wright (2002) found a similar result for large UK firms. Furthermore, Oxelheim & Randøy (2005) report a positive relationship between CEO pay and the globalization of sales, globalization of ownership and globalization of board membership of Scandinavian firms. The present study extends past studies to include the effect of globalization on CEO dismissal risk.

Whereas the direct linkage between performance and dismissal/turnover has been addressed (e.g. Jensen & Murphy (1990)), the moderating effect of firm level globalization has not. Specifically, our center of attention is on the dismissal performance sensitivity, and how this sensitivity of European firms is moderated by American corporate governance influence on European firms.

Agency theory provides a normative approach to the role of executive incentives (including pay and risk factors) as a corporate governance mechanism. Managerial incentives should bridge the gap between the interest of managers versus the interest of owners (e.g. Fama (1980); Fama & Jensen (1983)). The implication is that companies should be paying CEOs with more incentive-based pay than the supply and demand of executive talent would suggest (e.g. Jensen & Murphy (1990)). We argue here that agency theory also provides the underlying rationale for the increased CEO dismissal performance sensitivity related to the globalization of the firm. Past research has identified a specific pay premium for CEOs being exposed to Anglo-American board members and foreign regulatory authorities (Oxelheim & Randøy (2005)). We argue that part of this pay premium is a compensation for the stronger relationship between performance and dismissal among firms exposed to such international (American in our case) corporate governance monitoring.

The managerial power/managerial entrenchment literature suggests that the governance of corporate behavior may deviate from the normative perspective of agency theory. US-based research indicates that some CEOs have been able to build a power base that weakens – or even isolates the CEO – from shareholder demands (Boyd (1994); Zajac & Westphal (1996)). Specifically, Allgood & Farrell (2000) found that CEO tenure (a proxy for managerial power) moderates the relation between firm performance and turnover. They found that entrenched CEOs are less exposed to performance–forced dismissals. We argue that globalization of the firm affects the potential for CEO entrenchment.

Hypotheses

The point of departure of this paper is that the American corporate governance system, particularly during the 1990s and early 2000s, is to be seen as the most demanding and unforgiving corporate governance system in the world (Lucier, Schuyt & Handa (2004). This view is based on factors such as a high risk of dismissal, a focus on short-term (quarterly) results, and a high degree of transparency vis-á-vis investors. We argue that the more demanding the corporate governance system, the less the degree of freedom for the CEO. The form of corporate governance in most of the European markets is the so-called insider or control–oriented system (Berglöf (2000); La Porta, de Silanes & Shleifer (1999). In this corporate governance system the emphasis is on the ability of large shareholders to monitor corporate behavior (Angblad, Berglöf, Högfeldt & Svancar (2001)), whereas the American system puts more emphasis on monitoring by way of board independence, a market for corporate control, and institutional monitoring (e.g. SEC and stock exchanges). In line with this, we suggest that CEOs in firms in non-American countries that move closer to the American system of corporate governance, will face an increased dismissal performance sensitivity due to institutional contagion.

We emphasize two specific firm activities that signal to the international investor community compliance with an American standard of monitoring corporate behavior. Cross-listing in an American stock market is one of these activities. Recruitment of at least one independent board member representing a more demanding corporate governance system, i.e. the American system is the other. These activities open the way for an institutional spill-over effect that enhances the CEOs pay while simultaneously increases his/her risk of dismissal. Signals of this kind imply an upgrading of the corporate governance monitoring compared with that provided solely by the domestic system.

Cross-listing and dismissal performance sensitivity

International cross-listing is a generally recognized way of breaking away from a domestic capital market (e.g., Howe & Madura (1990); Sundaram & Logue (1996); Foerster & Karolyi (1999); Miller (1999); Doidge, Karolyi & Stulz (2004)). Cross-listing implies that the firm will be scrutinized by a new international investor clientele, it will be exposed to new regulatory authorities, and it will need to comply with new standards with regards to disclosure and accounting. We argue that cross-listing in American markets exposes the CEO to higher career risks and rewards. For most firms from semi-segmented capital markets, excluding the few companies that already enjoy an international cost of capital, an American stock exchange listing is a big step, for which the firms concerned are rewarded in terms of a higher market value (Modén & Oxelheim (1997), Stulz (1999)). Part of the value-creation arising from such an American cross-listing is captured by the CEO (rent-sharing) who possesses the scarce set of skills necessary for a successful cross-listing (Oxelheim & Randøy (2005)). We argue that the new regulatory environment and the new investor clientele envisaged at the listing on the US financial market will confront the CEOs with harsher monitoring implying increased dismissal performance sensitivity.

> **Hypothesis 1**: There is a positive relationship between European firms' cross-listing in the U.S. and their CEO's dismissal performance sensitivity.

Outsider American board members and dismissal performance sensitivity

Corporate governance research recognizes the essential role of the board of directors in sustaining an effective organization (OECD (1999); Jensen (1993)). Oxelheim & Randøy (2005) show that for non-American firms with one or more independent American board members the CEO of such a firm receives a significantly higher compensation than a CEO of a firm without a recruitment of that kind. One of their explanations of this finding is the signal these companies send out about being open for a harsher American styled monitoring. Consequently, CEOs in such a position will be exposed to a clash between two corporate governance cultures, and the reconciliation of the two systems will pose new challenges and tasks for them. Among other things this may call for a new corporate language (Oxelheim, Stonehill, Randøy, Vikkula, Dullum & Modén (1998)), new internal reporting requirements, new investor-relation activities (Useem (1998)) and a higher dismissal risk. When combined with the lower tolerance for poor performance, characterizing the American corporate governance system, the dismissal performance sensitivity will change.

> **Hypothesis 2**: There is a positive relationship between European firms' outsider American board membership and their CEO's dismissal performance sensitivity.

Data

Data sources

We have chosen to study large firms and CEO successions in these firms rather than focusing on all CEO successions. The reason for this is the access to relevant information. The crucial classification of successions into different categories heavily relies on the information published in newspapers and magazines. Only larger firms are in general covered by the media. Moreover, our explanatory variable, "cross-listing on US stock exchanges", is also dependent on published information. Since smaller firms may opt for ADR level-1 or over-the-counter listing, with less demanding financial reporting and disclosure requirement, we argue that the choice of large firms is further motivated. The choice of only larger firms comes at the expense of fewer observations. This forces us to household with our degrees of freedom by making a thorough analysis of missing values; a kind of analysis that is called for with the aim of achieving statistical rigor but which is regrettably omitted in most studies.

Data was collected for the 250 largest (by market capitalization in 2004) European publicly traded firms during the time period of 2000-2004. The study is based on the population of CEO succession events as reported in the financial media (e.g. Financial Times). Data on these events and linked performance data were collected by Booz Allen. Information regarding independent American board membership (defined as American citizens) and the cross-listing of European firms on American markets has either been collected from Annual Reports, company web pages, or solicited through direct contact with the firms. The definition of all variables used in the study is given in the Appendix A1.

Descriptive univariate statistics

The period of our study covers 270 succession events among the largest 250 European companies. As found in Table 1, the data set contains CEO succession events from fourteen EU countries. Only 7% of the succession cases reported involved firms from non-EU countries like Norway, Switzerland and the Russian Republic. Approximately 41% of the 270 CEO succession events occurred in British companies, while 14% of all succession cases happened in companies located in Germany and 10% in French companies. Observations from those three countries account for

65% of all observations.

Table 1: Country composition of succession events

i	Country	code	absolute frequency	relative frequency
1	Belgium	BEL	4	0.0148
2	Czech Republic	CZE	3	0.0111
3	Denmark	DEN	4	0.0148
4	Finland	FIN	6	0.0222
5	France	FRA	26	0.0963
6	Germany	GER	39	0.1444
7	Ireland	IRE	5	0.0185
8	Italy	ITA	18	0.0667
9	Luxembourg	LUX	2	0.0074
10	Netherlands	NLD	17	0.0630
11	Portugal	POR	0	0.0000
12	Spain	SPA	9	0.0333
13	Sweden	SWE	8	0.0296
14	United Kingdom	UK	110	0.4074
15	Norway	NOR	5	0.0185
16	Russian Republic	RUS	2	0.0074
17	Switzerland	SWI	12	0.0444
$\sum$			270	$\approx$ 1.0000

Table 2: Industry composition of succession events

Industry	numerical codes	absolute frequency	relative frequency
Energy	10	9	0.0333
Materials	15	27	0.1000
Industrials	20	37	0.1370
Consumer discretionary	25	36	0.1333
Consumer staples	30	20	0.0741
Health care	35	11	0.0407
Financial services	40	70	0.2593
Information technology	45	15	0.0556
Telecommunication services	50	21	0.0778
Utilities	55	24	0.0889
$\sum$		270	1.0000

Table 2 demonstrates that our events appear in a broad range of industries. The group "Industrials" is the biggest group whereas "Energy" is the smallest in terms of turnover events. However, in relation to how many firms there are in each industry in our material the probability changes substantially.

The circumstances surrounding each turnover case were researched and characterized by one of 11 categories listed in Table 3.

Table 3: Reasons for succession (2000–2004)

r	Reason	absolute frequency	relative frequency (%)	classification
1	Board/power struggle	26	9.63	forced
2	Move to lesser position	3	1.11	forced
3	Poor performance	72	26.67	forced
4	Death or illness	7	2.59	voluntary
5	Interim CEO	7	2.59	voluntary
6	Job demands	3	1.11	voluntary
7	Merger	66	24.44	merger
8	Planned succession	64	23.70	voluntary
9	Move to another company	16	5.93	voluntary
10	Earlier tenure	0	0.00	voluntary
11	Governance change	6	2.22	voluntary
	$\sum$	270	100	

Each category reflects a more or less precise reason for the CEO succession. The descriptive analysis shows that in about 27% of our cases outside observers interpreted the information available about the succession case as being indicative of a turnover due to "poor performance". This modal category contains cases in which a CEO is simply fired by the board or forced out of the position in a more subtle way. Almost twenty four percent of the recorded successions were characterized as planned successions, events which were known to happen (due to contractual arrangements or retirement). The third dominant category is constituted by succession events which happen in the course of a merger. A connection to takeover or merger activity was established in more 24% of the recorded events. The events associated with one of these three categories account for 75% of the total number of cases.

For the purpose of our CEO succession study we introduce a simple dichotomy based on the relatively fine grid of reasons exhibited in Table 3. We need to distinguish events in which a decision unit (integral part of the company, e.g. the board) decides to remove the CEO from his position due to motives (unplanned developments) related to the company and implements the decision in one way or another against the preferences of the CEO. Such circumstances are thought of as a *forced succession*. If a CEO leaves to take a comparable position in another company, she/he implements her/his preferences. Such cases are referred to as unforced (by an internal force) in the sequel. Note that the death of a CEO hardly constitutes voluntary change in the leadership. But since it is not forced by an exogenous force such events are classified as unforced. The distribution over succession events over the coarser scheme of categories is given in Table 4.

Table 4: Distribution of categories

i	Classification	reasons (r in Table 3)	absolute frequency	relative frequency (%)
1	Forced	1,2,3	103	38.15
2	Voluntary	4, 5, 6, 8, 9, 10, 11	101	37.41
3	Merger	7	66	24.44
	$\sum$		270	100.00

After selecting the succession cases which are *not related to merger activity*, we can rely on a total of 204 dismissal cases. Each of our succession cases can be classified as either a dismissal (forced succession) or a voluntary one. Since M&A's may be undertaken for disciplining or management performance reasons, they may sometimes appear to fit the criteria for a forced succession. However, mergers/takeovers are in the grey area between forced and voluntary dismissal. We will control for the robustness of the classification in Subsection 5.4

The basic descriptive statistics for all relevant variables are given in Table 5. We list the number of observations used in the computation of the respective estimate(s) for each variable in the second column of the table. An n less than 204 indicates the presence of missing values for the variable at hand.

We have also registered the age of CEOs involved in the succession events. We managed to collect CEO age from 266 of the 270 succession event - which vary between the ages of 28 and

75. The youngest CEO, Jonas Birgerson, was affiliated with a Swedish information technology firm (Framfab). He was removed from his position in the course of a merger (acquisition) episode in 2000. The two CEO successions which involved 75 year old individuals were both planned. In the case of the British hardware company Farnell PLC, Morton Mandel served as an interim CEO for six month, while in the other case Hans-Joachim Langmann retired from the CEO position, (Vorstandsvorsitzender) at the pharmaceutical company Merck after holding that position for 30 years.

Table 5: Basic descriptive statistics

Variable	n	$\hat{\mu}$	$\hat{\sigma}$	$x_{(1)}$ min.	$x_{(n)}$ max.
dismissal	204	.495	.501	.000	1.000
CEO age	194	56.263	7.463	28.000	75.000
CEO tenure	203	6.356	5.703	.100	43.000
market capitalization	202	10091.290	16221.120	1036.160	124282.700
total stock returns	190	-.024	.223	-.676	1.027
US exchange listing	204	.735	.442	.000	1.000
US board membership	185	.184	.388	.000	1.000
US board membership × return	171	-.011	.097	-.676	.293
US listing × return	190	-.025	.181	-.672	1.027

Based on our study we can expect a CEO involved in a succession event to be about 56 years old (standard deviation of 7.2). 50% of our age observations lie between 52 and 61, i.e. are distributed over 19% of the range. The skewness measure of -.54 indicates an asymmetrical age distribution, i.e. the age distribution is skewed to the left and the CEO variable is not normally distributed. CEO tenure varies between slightly more than a month (interim positions) and 43 years. The latter case is Calisto Tanzi the chairman and founder of the Italian company Parmalat who was dismissed at the age of 65 due to poor performance in 2003. Based on our sample we can expect a CEO to be in his ("her" in only one case!) position for 6 years before he (she) is replaced. The median duration on the job is 1.2 years lower: in 50% of all cases an individual served more than 4.8 years as CEO before the position was filled by a successor.

The missing data problem

Complete data records are available on only 165 of the 204 cases. There are no missing values for the dependent variable (dismissal). However, the same statement holds only for a subset of the independent variables. Only the indicator variable US exchange listing and the categorical variables country grouping and industry category are complete. All other variables have missing observations for certain cases. There is only one case in which we do not have an observation on CEO tenure. But the information about the presence of at least one US board member is not available on 19 (9%) of the records. In the case of the variable US board membership × return, which is associated with an interaction term, there are even 33 (16%) of the observations missing. A different perspective of the scope of our missing value problem is provided by Table 6, which shows that 165 (81%) of the records are complete.

Table 6: Distribution of the number of missing values per observed succession case

♯ missing values per record	f^m	$\hat{p}^m$
0	165	0.8088
1	5	0.0245
2	19	0.0931
3	11	0.0539
4	3	0.0147
5	1	0.0049
$\sum$	204	1.0000

With close to 20% of our succession cases being incomplete, we will work to clarify whether the apparent lack of data will have adverse effects when estimating models involving the variables given in Table 5. Three types of missing data are distinguished. If the dismissal cases for which we observe missing data form a *random subset* of the sample under scrutiny then the missing data are classified as *missing complete at random* (MCAR). We face the second type of "missingness" if the occurrence of the missing data is related to other observables, i.e. dependent or independent variables. We refer to them as *missing at random* (MAR). Frequently, the absence of data depends on unknown or unobservable information. The terminology *missing not at random*

(MNAR) is used to designate this case. In practice, cases with at least one missing value on it are often simply discarded. To see whether this so-called *complete case analysis* is admissible from a statistical point of view we need to establish the type of ”missingness” prevalent in our data. In the case of MAR and MNAR, a complete case analysis will imply *inefficient* and *biased* estimators of the model parameters. If one can establish the MCAR case, then the estimates obtained using *complete case analysis* are still going to be *inefficient* but *unbiased* (van der Heijden, Donders, Stijnen & Moons (2006), Little (1992)). With respect to likelihood based inference, Rubin (1976) has demonstrated that likelihood ratios are invariant to discarding of incomplete records in the MCAR case. Fortunately, it is possible to test if data are MCAR on the basis of the sample. If this test fails then the *imputation* of missing data would clearly be preferred to the complete case analysis.

Testing if missing data are ”missing completely at random”

For the purpose of describing the procedure we have used, let X denote any variable in our data set. Let C denote the set of indices of all complete records and $\bar{C}$ represents the index set of all records containing at least one missing value. Denote $\{X_c\}$, $c \in C$ the group of values recorded for variable X which were found on complete cases and let $\{X_i\}$, $i \in \bar{C}$ denote the set of all sample values of X which were found on the incomplete records. A test of the simple hypothesis: $H_0 : F_{X_c} = F_{X_i}$ versus $H_1 : F_{X_c} = F_{X_i}$, where F denotes a distribution function, is then carried out. In choosing the statistical test(s), we took the properties of the random variable X under scrutiny into account. This procedure is carried out for each variable in our study. If we fail to reject the null hypotheses for most of the variables involved then this is taken as evidence for the fact that our data are MCAR.

In our study, the situation is complicated by the fact that it involves three different types of variables. The observations on CEO age, CEO tenure, market capitalization and total stock returns (industry adjusted) must be viewed as realizations of continuous random variables while the remaining variates are of discrete nature: the values on the binary response variable (dismissal), as well as the realizations of the variable *US exchange listing*, and *US board membership* can be viewed as realizations of random variables associated by a Bernoulli type distribution. The remaining categorical variates country group and industry category follow a multinomial distribution. To test whether the two samples $\{X_c\}$ and $\{X_i\}$ come from the same population (can be characterized by the same distribution function) each type of random variable requires another type of test. To boost statistical validity, we chose two admissible non-parametric test procedures generating evidence against the null by focusing on different aspects of the underlying distribution. In the continuous cases, we use the Kolmogorov-Smirnov test for the equality of two distribution functions (Smirnov (1939), Doob (1949), Darling (1957)) along with the rank based Kruskal-Wallis procedure (Kruskal & Wallis (1952), Fligner (1985)).

Table 7: Properties of continuous variables for complete and incomplete records

Variable	f^m	complete records		incomplete records		p-values	
		$\hat{\mu}$	$\hat{\sigma}$	$\hat{\mu}$	$\hat{\sigma}$	KS-test	KW-test
CEO age	10	56.52	7.21	54.790	8.74	<u>0.515</u>	0.3780
CEO tenure	1	6.64	5.91	5.120	4.57	0.263	0.1003
market capitalization	2	11252.46	17568.19	4913.090	5416.68	0.131	0.1555
total stock returns	14	-.03	.22	-.004	.22	<u>0.914</u>	0.6877

Legend: f^m = absolute frequency for missing; $\hat{\mu}$ = mean; $\hat{\sigma}$ = standard deviation; KS: Kolmogoroff-Smirnov test; KW: Kruskal-Wallis test; ___ = exact values (Kim (1969))

As shown in Table 7, the smallest p-value observed for tests of our continuous variables equals 0.1003. The differences in the first two moments shown in the body of the table are not significant. Based on the p-values, we fail to reject the null hypothesis that the data come from the same population in each and every case. For each variable, the different tests imply the same decision. We therefore conclude that in the case of our continuous variables missing values will not constitute a problem.

In the case of simple binary data, H_0 can be expressed in terms of the equality relative frequencies for the event that the value 1 is realized. We choose the standard PR-test to test this hypothesis. According to the summary of results exhibited in Table 8, we find no missing values on the dependent variables (dismissal). There are 83 complete records on which the variable dismissal takes the value 1, i.e. is indicating that the observed succession case was classified as a dismissal (forced). Among the complete cases, a forced succession occurs with a probability being slightly higher than .5. In comparison, the relative frequency of a dismissal equals 0.4615 given that the data on the case was incomplete. We clearly fail to reject the hypothesis that the probability of the event *dismissal* is the same in both groups, since the p-value amounts to 0.641. The same result is obtained for the variables *US exchange listing* and *US board membership*. Hence, missing data on indicator variables will not constitute a problem when a complete case analysis is executed.

Table 8: Properties of indicator variables for complete and incomplete records

Variable		complete records		incomplete records		p-value
	f^m	$f(1)$	$\hat{p}(1)$	$f(1)$	$\hat{p}(1)$	PR-test
dismissal	0	83	0.5030	18	0.4615	0.6410
US exchange listing	0	123	0.7455	27	0.6923	0.4987
US board member	19	32	0.1939	2	0.1000	0.3057

Legend: f^m = absolute frequency for missing; $f(1)$ absolute frequency for value 1; $\hat{p}(1)$ relative frequency for value 1; PR-test: proportion test

Finally, the strategy outlined above is applied to the country and industry groupings, i.e. to our categorical variables country group and industry category. Given the discrete nature of the data, the use of the Kolmogorov-Smirnov or the Kruskal-Wallis test is not advisable since those tests require data from an absolutely continuous distribution. As argued in Conover (1980, pp. 368–376), those tests tend to be conservative when applied to discrete data. Again, the differences in the multinomial densities reflected as differences in medians and interquartile ranges do not appear to be significant in the light of the appropriate χ^2 test described in Mood, Graybill & Boes (1974, pp. 448–452). We fail to reject the hypothesis that the realizations in the complete and incomplete groups come from the same distribution.

Table 9: Properties of categorical variables for complete and incomplete records

Variable	f^m	complete records		incomplete records		p-values
		median	$\hat{iqr}$	median	$\hat{iqr}$	IMD-test
country group	0	1	2	1	3	0.2737
industry indicator	0	2	3	2	2	0.1367

Legend: f^m = absolute frequency for missing; $\hat{iqr}$ = inter quartile range; IDM independent multinomial distribution;

In the light of the evidence exhibited in Tables 6 to 9 we conclude that our data are of the MCAR. Using complete case analysis will not trigger a bias in our estimators.

Model and estimation results

To provide a framework which allows us to test the hypotheses motivated in Section 3, we chose a *binary response* type model. In our context, the probability for the event that a CEO succession is forced as opposed to voluntary is modeled as a non-linear function of risk factors. After stating the model explicitly, we discuss some aspects concerning statistical inference. Two alternative specifications of the model are introduced and the estimation results are presented.

The binary response model

We consider the dichotomous random variable Y taking the value 1 if a succession case is a dismissal. If the succession is voluntary then the variable assumes the value 0. It is assumed that the discrete random variable Y follows a *Bernoulli* distribution depending on the parameter $p \in (0, 1)$, the probability of dismissal. Moreover, we assume that there exists a non-linear functional relationship between the dismissal probability and a set of independent variables represented by the elements of the vector z, i.e. $p = F(\theta' z)$, where F is monotonically increasing function and θ denotes a parameter vector of appropriate dimension. Under these assumptions the binary random variable Y follows the Bernoulli distribution

$$Y \sim B(F(\theta' z)) = F(\theta' z)^y \, (1 - F(\theta' z))^{1-y} \, I_{\{0,1\}}(y)$$

where $I_{\{0,1\}}$ denotes the indicator function. Hence

$$E[Y \mid Z = z] = p = F(\theta' z) \tag{1}$$

represents the (non-linear) regression of the random variable Y on the explanatory variables listed in z.

Estimation approach

To obtain estimates of the parameter vector θ, i.e. $\hat{\theta}$, for the binary response model we rely on the maximum likelihood (ML) procedure. Standard arguments demonstrating the optimality properties of the ML procedure rely crucially on the assumption of statistical independence of the observations. In our sample, we have a few succession cases from within the same company. This fact, most likely, constitutes a violation of this independence assumption. Fortunately, it has been shown in the literature on adaptive designs that the independence assumption can be relaxed. Chang (1999), for instance, provide a strong consistency result allowing for certain dependencies in time. In our situation we should therefore benefit from implementing the ML approach.

Specification of the model

Two specifications of a the binary response model (1) are discussed below: the *logistic* specification and the *probit* specification. [1] In our case, there is no substantive information available which would let us choose between the two alternatives *a priori*. We chose to consider both to see whether our results are invariant under alternative specifications.

We employ the specifications of the binary response model (1) to test the hypotheses concerning the relationship between the dismissal probability p and *US exchange listing* (Hypothesis 1), as well as the relation between p and *US board membership* (Hypothesis 2). Since one can only expect to obtain reasonable statistical tests if the coefficients θ are estimated with precision,

[1] The logistic specification follows from choosing the CDF of the standard logistic distribution as a specification for F. Therefore

$$P(Y=1) = F(\theta' z) = \int_{-\infty}^{\theta' z} l(t)\, dt = L(\theta' z) = [1 + e^{-\theta' z}]^{-1}$$

generates the so-called logistic model. Let $\phi(t)$ ($\Phi(\bullet)$) denote the density function (CDF) of a standard normal random variable. Then

$$P(Y=1) = F(\theta' z) = \int_{-\infty}^{\theta' z} \phi(t)\, dt = \Phi(\theta' z)$$

is referred to as the probit specification of the binary response model.

we have to avoid the specification of a statistical model which might produce unreliable estimates. Based on a theoretical analysis Harrel, Lee, Matchar & Reichert (1985) conclude that such problems can be avoided if the ratio of the number of outcome events to the number of explanatory variables (EVP-ratio) lies between 10 and 20. These results were clearly supported by in an extensive simulation study due to Peduzzi, Concato, Kemper, Holford & Feinstein (1996). Following their guidelines and taking our data situation into consideration, we should not include more than ten explanatory variables when modeling the probability of the event *forced succession*.

As a result of a structured exploratory modeling phase [2] involving all potential explanatory variables introduced in Section 3, it became clear that the variables CEO tenure and market value could be eliminated from the variable pool. Moreover, controlling for all other important factors the industry categories were never found to be significant. Honoring the EVP rule, we finally specify our binary response model as

$$p = F(\theta' z) = F(\theta_0 + \sum_{i=1}^{10} \theta_i \, z_i) \tag{2}$$

where the independent variables are identified as z_1 = CEO age, z_2 = total stock return, z_3 = first country-group dummy, z_4 = second country-group dummy, z_5 = third country-group dummy, z_6 = fourth country-group dummy, z_7 = US exchange listing, z_8 = US board membership, z_9 = US listing $\times$ return, z_{10} = US board membership $\times$ return. The observations on the dichotomous dependent variable are stored in the variable *dismissal*.

It is possible that industry adjusted stock performance does not have a significant *main effect* on the probability of dismissal, but the effect of the total stock return varies with some characteristics of the succession case or the nature of the environment in which the case is embedded. To account for such *interaction effects* we introduce the two interaction terms US exchange listing $\times$ return and US board membership $\times$ return into the model (a term over and above the first order term). These interaction terms constitute the variables z_9 and z_{10}. The resulting logit and probit version of model (2) is estimated using the maximum likelihood approach.

[2] The authors have carefully documented the specifics of the procedures run and the results obtained. The material can be obtained from the authors upon request.

Estimation Results

Prior to performing the ML estimation procedure we assessed whether the sufficient condition for the existence, finiteness and uniqueness of estimates as given by Albert & Anderson (1984, Theorem 3,p.7) is fulfilled. A numerical check confirmed that our data exhibits the pattern referred to as *overlap*, implying that the said sufficient condition holds. As could be expected in such a case, we observed a rapid convergence of the algorithm locating the maximum of the likelihood function under both specifications.

Table 10: Summary table for logit and probit models

	Logit			Probit		
Variables	Estimate	p-value	S-Ind.	Estimate	p-value	S-Ind.
Const.	11.22	0.000	***	6.56	0.000	***
CEO age	-0.19	0.000	***	-0.11	0.000	***
total stock return	-0.85	0.610		-0.57	0.581	
country-group dummy 1	-0.96	0.037	**	-0.60	0.030	**
country-group dummy 2	-2.11	0.013	**	-1.27	0.007	**
country-group dummy 3	0.63	0.387		0.36	0.391	
country-group dummy 4	-1.41	0.060	*	-0.86	0.058	*
US exchange listing	0.61	0.187		0.36	0.187	
US board membership	-0.74	0.224		-0.41	0.232	
US listing × return	0.49	0.805		0.33	0.789	
US board membership × return	-11.94	0.021	**	-7.11	0.020	**
sample size	165			165		
Log-lik.	-83.68			-83.59		
pseudo R^2	0.27			0.27		

Legend: S-ind. ≡ significance indicator: * if $p < 0.10$, ** if $p < 0.05$, *** if $p < 0.001$

Our ML estimation results are summarized in Table 10. At a first glance both models are valid and from a qualitative point of view the models produce identical results. There are no differences in the signs of the estimated coefficients. With respect to the statistical significance of the ML estimates, the findings are identical for the two specifications. In both cases we do not find main effects for the variables *US exchange listing* and *US board membership* but a clearly significant interaction term *US membership* $\times$ *return* indicating a moderator effect of *US board membership* on the relationship between the dismissal risk and the performance variable *total stock return.* [3]

Model diagnostics and validation

Prior to discussing the substantive conclusions which can be drawn from our statistical results, we turn to model diagnosis and model validation. The diagnostics for the logit model and the probit model yield similar results. To avoid redundancy, only the results for the logistic model are reported. The same holds for the outcome of our approach to internal model validation.

Assessing goodness-of-fit

The logistic model reported above was found to be significant. To assess the fit of the model, we draw on various standard statistical tests. Being aware of the uncertainties surrounding the operational characteristics of these tests in our specific situation, we augment the analysis by employing a graphical method, so called *marginal model plots.* Both approaches fail to produce evidence against the model. As a result we attest convincing explanatory power to the binary response models at hand.

The standard likelihood-ratio-test of the hypothesis

$$H_0: \quad \theta_1 = \theta_2 = \ldots = \theta_i = \ldots = \theta_{10} = 0$$
$$H_1: \quad \theta_i \neq 0 \text{ for at least one } i$$

[3] Following up on our discussion of alternative classification schemes for CEO dismissals in Section 4.2, we reclassified the CEO succession cases occurring in the context of mergers as dismissals and estimated the model using the same set of independent variables. Compared to the original classification the Pseudo R^2 decreases by 0.0534. We observe no sign changes and very similar numerical values. Given the new dependent variable, the coefficients for the country dummy (cg1) and the interaction term "US board membership x return" are no longer significant at the five percent level. Moreover, our dummy for the introduction of the "Sarbanes-Oxley Act of 2002" did not turn out to be significant on the five percent level. This holds for both classification schemes considered.

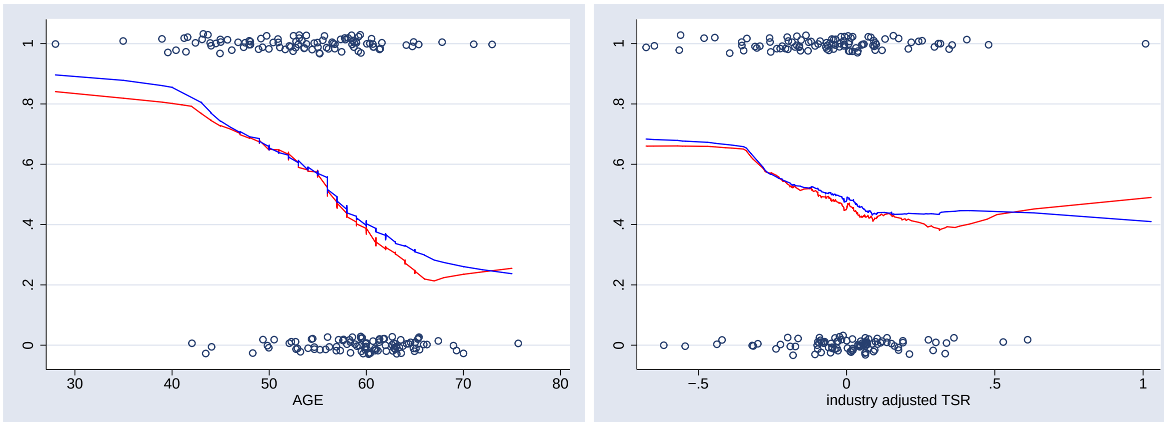
results in a realization of the test statistic $\chi^2(10) = 61.36$ (p =0.0000). We clearly reject H_0. At least one of the explanatory variables contributes significantly to the model. In addition, the value of McFadden's pseudo R^2 (likelihood-ratio index) of 0.27 indicates that the inclusion of our predictors have the desired significant effect on the likelihood function.

Having identified a *reasonable model*, say $M_\theta(y \mid z)$ for the true but unknown conditional distribution of binary succession events y represented as $F(y \mid z)$, we can now turn to more refined aspects of the model. To assess its goodness of fit, we set out to test the hypothesis $H_0 : F = M_\theta$ versus $H_1 : F \neq M_\theta$. Several omnibus tests of this hypothesis are used in practice. The operational characteristics of those tests are known if *all* explanatory variables are either (i) discrete (with repeated measurements) or (ii) continuous. Our model relies on a mixture of discrete and continuous independent variables. If statistical validity matters, a naive implementation of those standard tests appears to be unwarranted. Apart from reporting the outcome of a standard test of fit, we assess the fit of the model by a non-parametric technique.

Hosmer & Lemeshow (1980) devised a test procedure for the case of *continuous explanatory variables*. The test pools observations according to the model probabilities. Carrying out this test, we obtain a test statistic of $\chi^2(154) = 152.88$ ($p = 0.5103$). At standard significance levels, we clearly fail to reject the null. The model seems to fit reasonably well. [4]

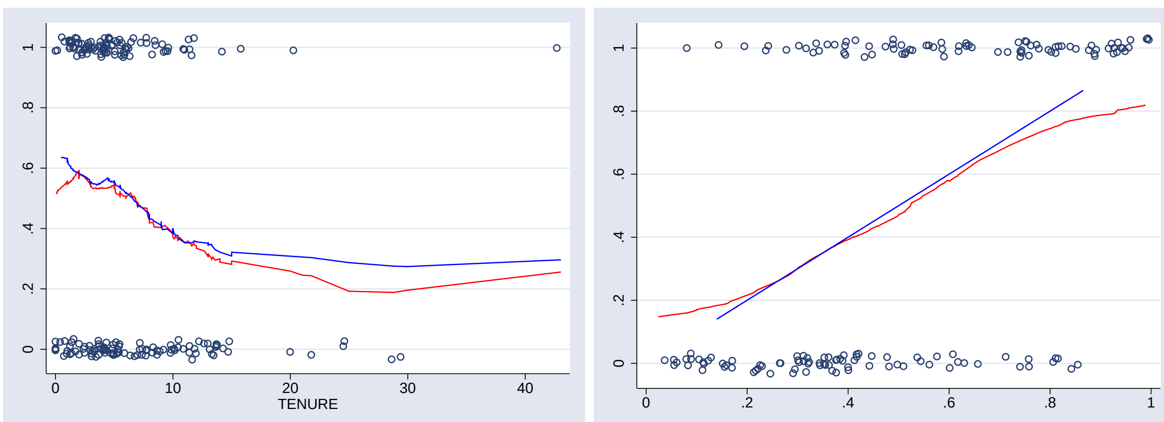
An alternative method for assessing the fit of a binary response model is due to Cook & Weisberg (1991). They introduce their *marginal model plots* (MMPs) as a non-parametric device to generate evidence against the hypothesis $H_0 : F = M_\theta$. The rationale underlying the procedure (details are given in Appendix A2) does not depend on the nature of the regressors. Estimates of two mean functions – one model-free and the other depending on the hypothesized model M – are contrasted. Severe differences between the functions are interpreted as evidence against H_0. elected plots exhibited in Figure 1 are representative for the pattern found in all cases.

[4] This test has known drawbacks. The procedure is insensitive to variation within the groups which causes the test to perform poorly in simulation experiments (le Cessie & van Houwelingen (1991)). We could overcome this apparent drawback by using a kernel based method as proposed by Conover (1980) or le Cessie & van Houwelingen (1991). Due to our sample size this is no viable option.



(a) $h(z) =$ CEO age

(b) $h(z) =$ total stock returns



(c) $h(z) =$ CEO tenure

(d) $h(z) = \hat{p}$

Figure 1: MMP for the mean with E_F = red and $E_{\hat{M}}$ = blue

Note that the observations on our dependent variable, the incidents of voluntary successions ($y = 0$) and forced events ($y = 1$) have been jittered to allow for a visualization of the density aspect. The MMP's in subfigures (a) and (b) involve explanatory variables in the model. In subfigure (c), $h(z)$ was chosen to be a variable not included in the model. While the last subfigure contains an MMP with fitted values. Apart from areas characterized by sparse data, we do not detect grave and/or systematic deviations between the estimated mean functions. Numerous experiments with random directions did not reveal cases which could have been used as convincing evidence against the null hypothesis. To summarize: under each of the two very different statistical concepts aimed at detecting a lack-of-fit, we fail to find significant evidence against our model M, i.e. we do not find conclusive evidence against the logistic model.

Assessing predictive power

To further evaluate the model we addressed it's predictive power - as a classifier of succession events based on a set of explanatory variables. To provide such a characterization, let $\hat{p}_i = F(\hat{\theta}' z_i)$ denote the model based estimate for the probability of the event *the i'th succession case is a dismissal* and define the classification rule

$$R_i(c) = \begin{cases} \hat{p}_i \geq c & \Rightarrow \text{case } i \text{ is a dismissal case} \\ \hat{p}_i < c & \Rightarrow \text{case } i \text{ is a voluntary case} \end{cases}$$

Implementing the classification rule $R(c = 0.5)$ we classify each case for which $\hat{p}_i$ exceeds 0.5 as a forced succession. The outcome of this can then be contrasted with the observed nature of the case. Each of the 165 probability estimates is processed in this way. The results are summarized in Table 11.

Table 11: Summary statistics for model based classification

classified by $R(0.5)$	true forced	true voluntary	$\sum$
forced	61	19	80
voluntary	22	63	85
$\sum$	83	82	165

In our sample, we observed 83 dismissal cases, while 82 of the 165 cases were events in which the succession was voluntary. Using the $R(0.5)$ rule for classification on the basis of the probability estimates, a total of 80 cases are classified as forced succession, while in 85 instances we predict that the case is a voluntary succession. On the basis of the information provided by the 2×2 contingency table the probability of the event *a case is correctly classified on the basis of the model* is readily estimated as 0.75. Several conditional probabilities related to success and failures of the model-based classifier are of interest.

The probability that a case is classified as forced turnover on the basis of our model, given that it was observed in reality, equals 0.73. For a succession case which has been observed to be of the voluntary type the chance of classifying it as voluntary using the model based classifier $R(0.5)$ amounts to 77%. Suppose it is known that the model based classifier indicates a dismissal then the probability for the event *a forced dismissal was observed* equals 0.76. The odds of observing voluntary succession given our model suggests a voluntary case is slightly lower, 0.74.

Given that *voluntary* is the true nature of a succession, we will classify such a case as *forced* using our model in 23.17% of all cases. Another type of error is committed if a succession is indeed forced and the classifier assigns the value "voluntary". The respective conditional error probability amounts to 0.27. Given the model suggests a forced succession, then in 23.75% of all cases our model will incorrectly suggest that the opposite holds. Finally, given our classifier suggests a voluntary succession, the odds for the event *the true nature of the succession is a dismissal* amounts to 25.88%.

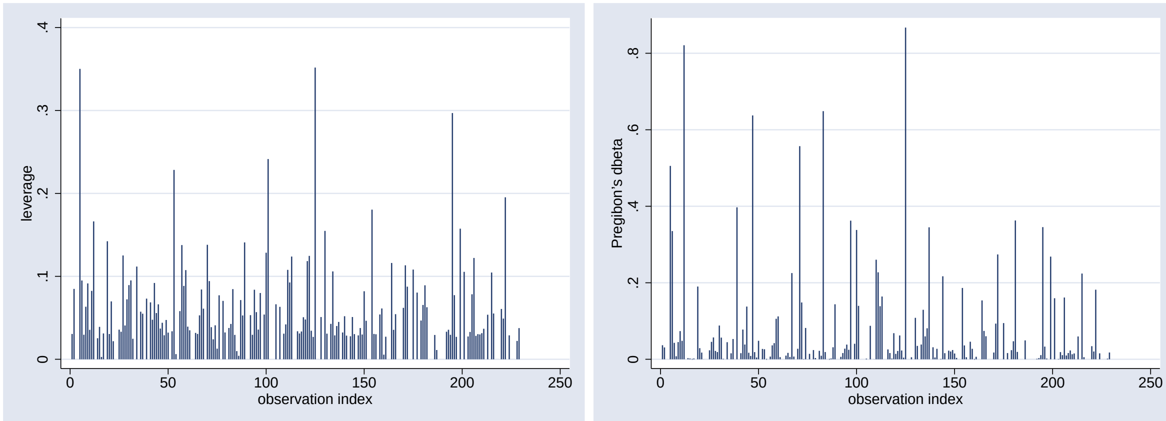
The in-sample predictive performance as it unfolds once we would start to vary the critical level c in our classifier between 0 and 1, was calculated and the respective ROC curve was constructed. [5] It suffices to say that the area under the ROC curve is approximately equal to 0.83. We conclude that our logistic model, apart from fitting the data well, performs surprisingly well as a classifier (in-sample).

Assessing stability of parameter estimates

Our choice of the maximum likelihood (ML) approach to estimation is motivated by its known statistical optimality properties. On the other hand, ML estimators are known to be sensitive to data points which are extreme in the response space and/or in the space of explanatory variables $\mathcal{Z}$. Since our data was not generated in the course of a controlled experiment, we have to guard against the possibility that the realization of our estimator $\hat{\theta}$ – therefore also our interpretation given below – depends on a single or several extreme observations. In addition, we screen our data for observations exerting strong influence on the goodness of fit measures which were discussed above.

Among the available methods suitable for detecting influential observations in binary response

[5] Details will be provided by the authors upon request.



(e) Leverage h_{ii}

(f) $\Delta_i(\beta)$ measure

Figure 2: Influence measures for $N = 165$ observations

models we relied on the *leverage measure* and the $\Delta_i(\beta)$ statistic both devised by Pregibon (1981). Based on earlier work in linear regression Andrews & Pregibon (1978), Pregibon introduced the h_{ii} statistic (leverage) in the context of binary response models. The statistic reflects whether an observation is poorly fit by the model and/or whether the observation constitutes an extreme point in the explanatory variables space (exerting influence on $\hat{\theta}$). Observations associated with a leverage value close to 1 deserve special attention. The hight of i'th the spike in Figure 2 (a) indicates the value of the leverage statistic for observation i, for $i = 1, 2, \ldots, N$. The highest leverage value equals .352, observed for case ♯125. According to the leverage criterion, none of the 165 succession cases appears to be problematic. Pregibon's $\Delta_i(\beta)$ measure reflects the effect on the (confidence contours of the) estimator due to deleting the i'th succession case from the sample. Inspecting the height of the spikes in subfigure Figure 2 (b) suggests that the $\Delta_{12}(\beta)$ and $\Delta_{125}(\beta)$ deserve our special attention.

To get an idea about the actual effect a deletion of observations will have on the elements of estimator, observations ♯12 and ♯125 were actually deleted from the data set one at a time and then simultaneously. In each case, the model was estimate. Table 12 allows the assessment of the effects. For each deletion we give the estimates of the statistically significant coefficients together with their p-values. We included the respective estimates based on the complete data set in columns two and three. In addition the likelihood ratio statistic as well as the value of the likelihood are given in each case.

Table 12: Summary of estimates after deletion of observations

statistic	full $N = 165$		drop $\sharp 12$		drop $\sharp 125$		drop $\sharp 12, 125$	
country-group dummy 1	-.965	0.037	-.992	0.032	-.970	0.037	-1.001	0.031
country-group dummy 2	-2.111	0.013	-2.817	0.011	-2.196	0.010	-2.921	0.009
country-group dummy 4	-1.409	0.060	-1.442	0.055	-1.383	0.065	-1.415	0.060
US board membership $\times$ return	-11.943	0.021	-12.498	0.019	-11.524	0.031	-12.106	0.028
LR $\chi^2(10)$		61.36		63.71		61.95		64.43
Likelihood		-83.68		-81.821		-82.691		-80.76

Deletions did neither affect signs of the estimates nor their statistical significance. We did not observe qualitative changes. Once we delete case $\sharp 12$ alone or in combination with observation $\sharp 125$, we observe that the deletions increased the absolute values of estimates and never increased the p-values. This statement also holds for the coefficients of country-group dummy 1 and country-group dummy 2 if we eliminate observation $\sharp 125$ alone. A slight decrease occurs in the absolute value of the coefficient of country-group dummy 4 and its p-values increase slightly. The interaction effect becomes stronger but the respective p-value increases slightly. Note also the tendency of the likelihood ratio statistic to increase as a consequences of the deletions. The same holds for the likelihood function evaluated at $\hat{\theta}$.

The observations $\sharp 12$ (dismissal event) and $\sharp 125$ (voluntary succession) are cases sharing one common characteristic: their scores on the performance variable *total share return* lie in the first quintile: industry adjusted extremely poor performance preceded the succession event. So far, there is no reason to believe that the two succession cases under scrutiny are in any sense invalid. Should it, in retrospect turn out that those data points would have to be deleted, then we can be sure that apart from small (even desirable) quantitative changes in strength of effects and their significance, our interpretation of the estimation results given in the sequel will be robust to the perturbation of the data set.

Errors in variables

The maximum likelihood procedure will provide minimum variance estimator if (1) the binary response model is appropriate, (2) the explanatory variables have been measured without error, and (3) no measurement error occurs in the binary response variable. In our case (3) implies that no errors occur when succession cases are associated with one of the categories either *forced* or *voluntary* turnover. Note that we have established (1) in Section 6.1. What can then be said about the potential for measurement error in the explanatory variables (risk factors)? The measurements on CEO age are hardly subject to error. Given the prominence of the firms, the CEO's are often public figures whose CV's are publicly available. The realizations on our performance measure total stock returns are based on publicly available data taken from reports which are subject to legal requirements. Although we do not know the exact procedure according to which the data was produced, it is hard to imagine that error sources apart from rounding errors etc. affect the measure. Although small disturbances are likely, gross measurement error is unlikely. Our country grouping variables should be free of error, since the classification rule known to us is operating on error free primary data. With large diversified companies the association with a single industry may be problematic in some cases, most of the classifications can hardly be questioned. The process of generating the variable *US exchange listing* and *US board membership* data generation had been double checked by the authors on an individual basis. When the information existed there was no reason to believe that it was faulty. As a consequence the probability of measurement errors is low for the bulk of our dismissal risk factors.

As mentioned in Section 4 our dichotomy implying the realizations on our dependent variable (dismissal) is based on eleven categories each reflecting a more or less precise reason for the observed succession. Neither the details of the research process nor the classification rules applied by the agency generating the data on our variable *reason category* are transparent. It is unclear whether one single analyst classified cases or whether a group of researchers worked on each case independently before a classification decision was taken. We also cannot exclude the possibility that the sources used by the researchers presented succession cases in different ways. The potential for erroneous misclassification is higher in the case of the dependent variable than among the factors thought to influence the dismissal risk.

The theoretical work by Michalek & Tripathi (1980) who consider a mix of categorical and continuous explanatory variables which resembles the situation in our data set, implies that in the presence of moderate errors-in-variables the statistical properties of $\hat{\theta}$ will be asymptotically stable. Focusing on errors-in covariates Carroll, Spiegelman, Lan, Bailey & Abbott (1984) point out that correction for measurement error is advantageous only, if the variance of the measurement error (and the sample size) is such that the bias in the standard ML estimator $\hat{\theta}$ is large relative to the increase in the variance which is typically produced by techniques aiming at bias correction. Their analysis also suggests that such conditions materialize in the presence of very large data sets. On the other hand, Cheng & Hsueh (1999) discuss promising methods of bias reduction when measurement error is only a problem of the dependent variable. Their techniques require a validation subsample. In addition, the procedures depend on information about the misclassification probabilities.

Given our earlier assessment of the likelihood of measurement error in the explanatory variables, we only have to be concerned with misclassification in the dependent variable. On the other hand, the techniques known for bias correction depend on information concerning the misclassification probabilities. We do not have this information. If we would apply the methods suggested by Cheng & Hsueh (1999), we would have to rely on strong, largely unfounded, assumptions. On the other hand, the asymptotic nature of the Michalek & Tripathi (1980) result has to be taken into consideration. In the interest of producing statistically valid results we need to explore whether our estimator is stable given our relatively small sample size and the possibility for misclassification of succession cases. We can do this by measuring our binary response's range of influence in our model of dismissal risk.

The procedure run can be described as follows: Let $\hat{\theta}(y)$ denote the ML estimate obtained on the basis of the original data set. Next, suppose we change the classification of i'th succession case. If the dependent variable has the value 1 (0), then the value 0 (1) is assigned to it. After making this marginal change to the data set we produce a new ML estimate $\hat{\theta}(y_{(*i)})$. To quantify the effect of the change in classification, we compute $d_i = d(\hat{\theta}(y), \hat{\theta}(y_{(*i)}))$, where d denotes some measure of distance. The effect of a single misclassification of the i'th succession case on the estimate is simulated in this way given that that all other responses are fixed. Since we are not only interested in the parameter estimates themselves but typically in functions of the parameter vector like standard deviations or t-values or confidence limits, we generalize the statistic to $d_i^f = d(\, f(\hat{\theta}(y)),\, f(\hat{\theta}(y_{(*i)}\,))$ where $f : \mathbb{R}^n \to \mathbb{R}^m$, with $n \geq m$.

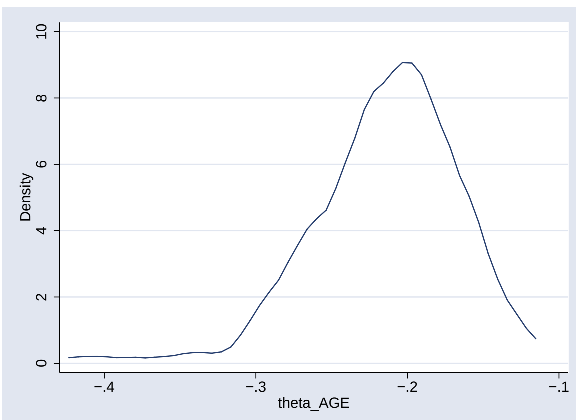
Our implementation of this procedure focused on (i) changes in the parameter estimates and on changes in (ii) the significance level. Under both aspects, it is not possible to identify cases which would change our results drastically. Both, our parameter estimates as well as our interpretation and conclusion given below will in all likelihood not be affected in a substantial way by a misclassification of singular succession cases.

Internal validation

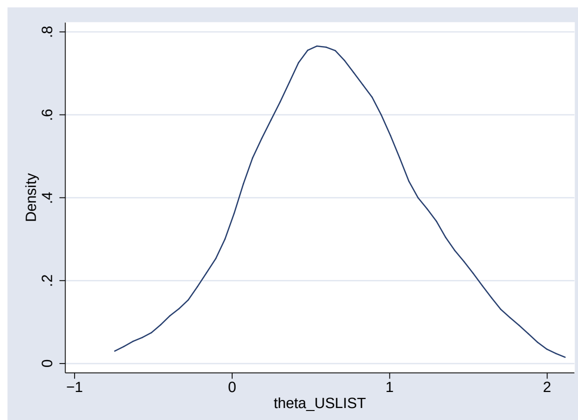
A complete discussion of validity issues covers the aspects of internal as well as external validity. To study external validity of our model, we need a sample from a population which differs along various dimensions from the one considered when estimating our logistic model. For the purpose of internal validation, we study the performance of the model in a sample of succession cases drawn from a population similar to the one which was used to estimate the original logistic model. Since we do not intend to use the model for prediction (classification), we will focus on the parameter estimates of interest, i.e. the main effects and the coefficient of the significant interaction term *US board membership* $\times$ *return*.

Several approaches to internal validation are considered. Since *bootstrapping* as an internal validation technique is recommended in the literature, we choose this technique in our validation effort. By means of a large scale simulation study Steyerberg, Harrell, J.J.M., Borsboom, Eijkemans, Vergouwe & Habbema (2001) have demonstrated that, in the context of the logistic model, the bootstrap validation strategy provided stable estimates with a small bias and clearly outperformed other approaches. The bootstrap mimics the process of drawing a sample from an underlying population by sampling with replacement from the sample available. The samples drawn are of the same size as the original data set. The rationale underlying this strategy, as well as its feasibility has been outlined, for instance, in Efron & Gong (1983) and Gleason (1988).

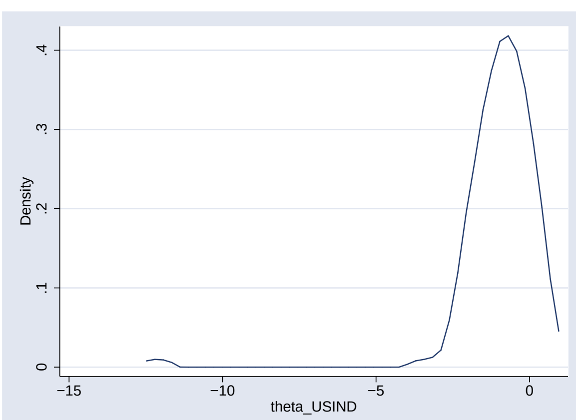
We drew 100 samples with replacement, each of size $N = 165$, from our original sample. After each draw the logistic model was estimated and the parameter estimates, apart from other statistics, were stored. In a first step, we produced non-parametric density estimators for each parameter estimate. A subset of those densities are displayed in Figure 3. In all cases the densities are fairly symmetric. The thin left tails are due to strange outcomes of very few replications. The mode of the bootstrapped densities lie close to the maximum likelihood estimators determined in Section 5.4. The evidence on the normal distribution of the estimates is mixed. While the distribution of estimates for the coefficients of some variables (CEO age, total stock returns, country-group dummy 1, US exchange listing) seems to be close to a normal distribution, this is not so for the coefficients of other variables.



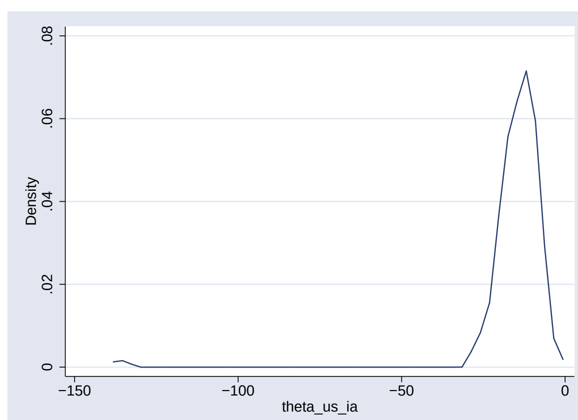
(g) $\hat{f}(\hat{\theta}_{CEOage})$



(h) $\hat{f}(\hat{\theta}_{US\, exchange\, listing})$



(i) $\hat{f}(\hat{\theta}_{US\, board\, membership})$



(j) $\hat{f}(\hat{\theta}_{US\, board\, membership-return\, interaction})$

Figure 3: Non-parametric density estimators (100 replications)

Since it is difficult to relate the information concerning the spread to the point estimators (c.f. Section 5.4), we compare the ML based interval estimators to the bootstrap interval estimators. The estimators are listed in Table 13 along with some supplementary information. In each case, we give the length of the interval in the column headed Δ. With the exception of the first country-group dummy, the bootstrap intervals are wider than their ML counterparts. The length of the intersection between the two confidence intervals expressed as a fraction of the ML confidence interval is given in the *coverage* column. A coverage of 1 indicates that the ML based interval is a true subset of the bootstrap interval. An inspection of Table 13 shows that there are four such cases. While a value of 0.8094 implies that approximately 81% of the ML interval contains points which are elements of both the ML interval and the bootstrap interval, as is the case for the coefficient of the variable CEO age. In instances where the coverage is smaller than 1, the bootstrap intervals tend to be shifted over to the left relative to the ML intervals. This fact is due to a few *special* simulation outcomes.

Table 13: 95% confidence intervals for ML and the bootstrap

	logit			bootstrap				Qual.
Variables	l_{low}	l_{up}	Δ_L	l_{low}	l_{up}	Δ_B	coverage	diff.
CEO age	-.2691	-.1157	0.0934	-.3206	-.1335	0.1871	0.8094	no
total stock return	-4.1224	2.4192	6.5416	-6.9583	1.1747	8.1330	0.8097	no
country-group dummy 1	-1.8716	-.0580	1.8136	-1.8813	-.1278	1.7535	0.9606	no
country-group dummy 2	-3.7756	-.4463	3.3293	-7.5012	-.8542	6.6470	0.8748	no
country-group dummy 3	-.7961	2.0553	2.8514	-1.1322	2.9038	4.0360	1.0000	no
country-group dummy 4	-2.8754	.0581	2.9335	-3.4995	-.1807	3.3188	0.9185	yes
US exchange listing	-.2976	1.5247	1.8223	-.3916	1.6868	2.0784	1.0000	no
US board membership	-1.9206	.4491	2.3694	-2.5411	.4618	3.0029	1.0000	no
US listing × return	-3.4299	4.4164	7.8463	-3.3633	8.3146	11.6779	1.0000	no
US board membership × return	-22.0950	-1.7910	20.304	-26.4969	-5.7453	20.7516	0.8052	no

The items in column nine of Table 13 indicate whether the decisions in tests of significance of partial coefficients based on the ML confidence interval and the bootstrap interval would differ, i.e. whether a qualitative difference would prevail. Such a qualitative difference is found in only one case. The coefficient of the fourth country dummy becomes clearly significant in the light of the bootstrap evidence.

Using the bootstrap strategy in our validation effort allows us to conclude that our ML estimation results are likely to understate the uncertainty associated with the ML estimator $\hat{\theta}$. Estimates based on samples from populations similar to our population will exhibit somewhat more variability than suggested by the asymptotic properties of the ML estimator. On the other hand our bootstrapping results also show that it is extremely unlikely that we ever would have to change our model, since first of all, neither the risk factor *US exchange listing* nor the factor *US board membership* have *main effects*, and secondly that a significant moderating effect is attributed to the variable *US independent board membership*. It clearly moderates the relationship between the firm performance and dismissal probability.

Finally, our validation exercise seems to be valuable when viewed from a different perspective. Our initial data set is constituted by all succession events occurring over a given period in the universe of the 250 largest European companies. If one works with such a data set, it would hardly be adequate to state that we are working with a sample, not to mention a random sample. Our use of term random sample throughout the statistics section can be justified by the fact that 20% of our observations were excluded due to missing observations. We spend considerable effort in Section 4.3 to show that the nature of the process which caused the missing observations is unsystematic. So it is, in a sense, a random mechanism which determined the 165 observations eventually ending up in our data set. Or to put it the other way around: each of the 165 succession cases had the same chance to end up in our random sample.

Discussion and conclusion

This paper addresses the globalization of corporate governance practices and focuses on CEO dismissal risk. The issue of CEO dismissal (and CEO incentives) is of great concern to public policy-makers, to investors and to regulators. This is reflected in a number of corporate governance reports from the OECD, FIBV, and central banks on the one hand, and in the corporate governance guidelines from sources such as the Cadbury Commission, the Sarbanes-Oxley Act of 2002, on the other. The focus here is on the influence of American financial markets on dismissal performance sensitivity in European firms. We argue that globalization of boards and foreign exchange listings are important financial facilitators for transfer of ideas and practices across various corporate governance regimes.

The applied research design provides a natural experiment on the issue of CEO dismissal and financial globalization. Whereas most corporate governance research is of a cross-sectional nature, this paper allows for a test that clearly reduces endogeneity problems. Taking a critical position with respect to our own modeling approach, we carried out extensive model diagnostics and implemented model validation routines. The procedures produced no evidence against the statistical model chosen.

We have found our results robust to the classification of the dismissal implications of mergers and acquisitions which are in general difficult to categorize (cf. footnote 3 on page 24). Moreover, we have found no impact on the dismissal relation of the Sarbanes-Oxley Act implemented during the period of investigation. Performance alone has been found to have no effect on the dismissal risk which is in line with a range of compensation studies that find a small and sometimes insignificant underlying pay-performance relationship (Tosi, Werner, Katz & Gomez-Mejia (2000)). Finally, our country dummies indicate that the UK and Irish firms are significantly less likely to force CEO dismissal than firms from the Rhine-region countries. Considering that the UK has a corporate governance system and shareholder protection environments comparable to the US (La Porta, de Silanes & Shleifer (1999)) this result triggers further research.

In line with our theory based prediction, the tests in this study show that globalization in the market for corporate control increases dismissal performance sensitivity. This indicates institutional contagion driven by American board membership on European corporate boards. We argue that the result is particularly interesting, since non-American firms are becoming increasingly concerned about the costs of American stock regulations in general and of the implementation of the Sarbanes-Oxley Act of 2002 in particular (Bartram, Stadtmann & Wissmann (2006), Economist (2006). Contrary to our predictions, the empirical results show no significant increase in dismissal performance sensitivity from American cross-listing. This effect might be due to the fact that foreign stock listing only indirectly affects boards' decision-making. The insignificance of the cross-listing variable may also reflect the inconclusiveness of previous research on European firms reflecting the relatively ”weak” commitment in undertaking a cross-border listing as compared to the more demanding corporate action of undertaking a cross-listing and an outright equity issue on the foreign market in one go (Modén & Oxelheim (1997)).

Appendix

A1: Variable Definitions

Variable	Definition
CEO age	The CEO's age (years) at the time of the dismissal
CEO tenure	The CEO's tenure (years) as CEO with the company
Market capitalization	Stock market value in millions of Euros at the time of the dismissal
Total stock returns	The total stock return to investors (including dividends) relative to firm's industry one year leading up to the dismissal event
US exchange listing	The company has a US-based stock exchange listing at the time of the dismissal: value of 1 if such listing exists - 0 otherwise.
US board membership	The company has at least one independent (non-employee) board member with a US citizenship: value of 1 if such board member(s) exist(s) - 0 otherwise.
US listing × return	Interaction term between Total stock returns and US exchange listing
US board membership × return	Interaction term between Total stock returns and US exchange listing
Country groups	All European countries were divided into five groups: (1) Anglo-Saxon := Ireland, United Kingdom (2) Benelux := Belgium, Luxembourg, Netherlands (3) Mediterranean := Italy, Portugal, Spain (4) Nordic := Denmark, Finland, Norway, Sweden (5) Rhine := France, Germany, Switzerland Rhine, where (5) serves as baseline in the dummy coding scheme.
Industry category	2-digit industry classification

A2: Marginal model plots: Concept and procedure

The conditional expectation under the true F is written as $E_F(y \mid h(z))$ where $h(z)$ is a *measurable* function taking $\mathbb{R}^p$ into $\mathbb{R}^1$ while $E_{\hat{M}}(y \mid h(z))$ denotes the conditional expectation based on $M_{\hat{\theta}}$. The estimates of the two mean functions are generated using non-parametric smoothing. A smooth of $E_F(y \mid h(z))$ versus the values of $h(z)$ giving $\hat{E}_F(y \mid h(z))$ and the smooth of $E_{\hat{M}}(y \mid h(z))$ versus $h(z)$ provides the estimated mean function based on the model $\hat{E}_{\hat{M}}(y \mid h(z))$. Using the same smoothing technique allows for a point-wise comparison of the estimated means functions. To generate an MMP plot one superimposes the estimates of the two mean functions on a plot of the binary dependent variable y against levels of $h(z)$. If the model M is a reasonable approximation to the true distribution F then for *any measurable function* of the explanatory variables z the estimates of the two mean function will be approximately equal

$$E_F(y \mid h(z)) \approx \hat{E}_{\hat{M}}(y \mid h(z)).$$

Approximate equality provides support for M. On the other hand, if one finds stark differences for some choice of $h(z)$ then this is interpreted as evidence against the specification M. The theoretical basis for the MMP plots consists in the following result due to Cook (1998)

Resultat 1 *Let* $h(z) : \mathbb{R}^p \to \mathbb{R}^1$ *be a measurable function of* z*. Then*

$$E_F(y \mid z) = E_{\hat{M}}(y \mid z), \forall\, z \in \mathcal{Z} \subset \mathbb{R}^p \;\Leftrightarrow\; E_F(y \mid h(z)) = E_{\hat{M}}(y \mid h(z)), \forall\, h$$

An in depth technical discussion of the conditions under which a graphical comparison of two non-parametric curves is feasible and meaningful is found in Bowman & Young (1996). An excellent discussion of MMP plots from a Bayesian perspective has been provided by Pardoe & Cook (2002). The latter reference also includes interesting innovations with respect to the MMP plot.

Some of the possible choices for $h(z)$ include individual explanatory variables which are in the model, fitted values, predictors outside the model and so-called random directions (which are linear combinations of predictors for which the weights are chosen at random). We prepared MMP plots for a variety of functions of the explanatory variables. The selected plots exhibited in Figure 1 are representative for the pattern found in all cases.

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