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Publication date
2010
Document Version
Final published version

Duisenberg school of finance - Tinbergen Institute Discussion Paper

TI 10-129 / DSF 5

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This version: November 2010

This paper was previously titled "The Effect of Corporate Governance Regulation on Transparency: Evidence from the Sarbanes-Oxley Act of 2002". We are grateful to Régis Breton, Miguel Ferreira, Denis Gromb, Peter Iliev, Dalida Kadyrzhanova, Andrew Karolyi, Karl Lins, and seminar participants at the SUERF-UPF conference "Disclosure and Market Discipline: What Role for Transparency?" (Barcelona, December 2010), the European Finance Association Meetings 2010 in Frankfurt, ESCP Europe, and Aarhus School of Business for very helpful suggestions. We would like to thank Klára Čelechovská, Wietse van Drooge, and Diederik Ligtenberg for excellent research assistance. Comments are very welcome. All errors are our own.

Did the Sarbanes-Oxley Act of 2002 Make Firms Less Opaque?

Evidence from Analyst Earnings Forecasts

Abstract

We study whether the Sarbanes-Oxley Act (SOX) of 2002 made firms less opaque. For identification, we use a difference-in-differences estimation approach and compare EU firms that are cross-listed in the US—and therefore subject to SOX—with comparable EU firms that are not cross-listed. We derive proxies for corporate opaqueness from analyst earnings forecasts. Our findings suggest that, relative to the control group, cross-listed firms became significantly less opaque after the implementation of SOX. We provide evidence that this effect was particularly pronounced for firms operating in informationally sensitive industries. We complement our analysis with a textual analysis of corporate annual reports in order to shed light on how SOX may have affected firms' reporting behavior.

Keywords: Sarbanes-Oxley Act, Analyst Forecasts, Corporate Governance, Disclosure Regulation

JEL-classification: G1, G3

1. Introduction

The Sarbanes Oxley Act of 2002 (SOX) is considered one of the most important corporate disclosure and governance reforms in US history. As stated in the preamble of the Act, a primary objective of SOX is "to protect investors by improving the accuracy and reliability of corporate disclosures". Our aim in this paper is to shed light on the question whether SOX has achieved this objective. More specifically, we ask whether firms that are subject to SOX became less "opaque" following SOX, and if so whether this effect was more pronounced for some types of firms than for others.

We derive proxies for firm-level opaqueness from analyst earnings forecasts.[1] Specifically, we construct two variables that pertain to the ability of financial analysts to accurately predict earnings: *forecast error* and *forecast dispersion*. Forecast error is the relative distance between average earnings per share (EPS) forecasts and actual EPS, while forecast dispersion is the absolute value of the standard deviation of EPS forecasts divided by the mean. Forecast error measures how far the analyst consensus is from actual earnings, whereas forecast dispersion measures the degree of "disagreement" among analysts. We argue that either measure provides a natural proxy for the degree to which investors and other market participants perceive firms to be opaque.

The central challenge of our analysis has to do with the question how to control for contemporaneous influences that may affect opaqueness but cannot be attributed to SOX. We address this challenge by exploiting the fact that SOX not only applies to US domiciled listed firms but also to foreign firms that are cross-listed in the US. This allows us to devise a clean test where the change in opaqueness of SOX-affected cross-listed firms is compared against the change in opaqueness of their SOX-unaffected peers. To implement this approach, we adopt a difference-in-differences regression setting and focus on firms that are domiciled in the European Union (EU-15). Our main question is whether cross-listed EU-15 firms became less opaque after SOX, *relative* to comparable EU-15 firms that are not cross-listed.[2]

[1] Using analyst earnings forecasts or bond ratings to derive proxies for firm-level opaqueness or transparency is fairly standard in the literature. See, among others, Lang and Lundholm (1996), Morgan (2002), Lang, Lins, and Miller (2003), Livingston, Naranjo, and Zhou (2007), Tong (2007), and Bannier, Behr, and Guettler (2010).

[2] The advantage of focusing on EU-15 firms is two-fold. First, as opposed to firms from, e.g., Asia or South America, the universe of EU-15 firms constitutes a sizable sample of treatment and control firms that are exposed to fairly similar economic conditions (except for SOX). Second, while some EU-15 countries had their own disclosure and governance reforms before or after SOX, these reforms were not only substantially different from SOX but also occurred at different points in time (an exception stems from the 2005 adoption of IFRS in the EU; we will come back to this below). This differs, for example, from Canada where the legislator passed a SOX-like reform in 2003 ("Bill 198"). Thus, to the extent that SOX and its Canadian equivalent are substitutes, a DID setting based on firms from Canada may underestimate the transparency-enhancing effect of SOX.

In implementing our research design, we face two additional identification issues. The first issue has to do with the fact that over our sample period (2001-2007) a significant number of cross-listed firms delisted from US exchanges. If these firms were inherently more opaque than firms that did not delist,[3] we might spuriously detect an opaqueness-decreasing effect of SOX merely because over time relatively opaque firms dropped out of the sample of treatment firms. To address this "survivorship bias" problem, we limit the treatment sample in our main regressions to firms that were continuously cross-listed over the entire sample period. The second issue stems from the possibility that the treatment status could, in principle, be endogenous: firms may endogenously choose to delist in an attempt to avoid SOX-compliance.[4] To mitigate this concern, we provide as a robustness check an instrumental variables estimation approach where we instrument the treatment status with cross-listing in the year 2000. In constructing this instrument for the treatment status of a firm, we exploit the fact that SOX was passed and enacted in 2002 in response to a string of accounting and governance scandals in 2001 and early 2002. SOX-avoidance could therefore not have been a reason for firms to delist in the year 2000, as firms could not possibly have been aware of SOX at this point in time. The cross-listing status in 2000 is a viable instrument for the treatment status as it fulfills the relevancy and exclusion conditions. The relevancy condition is fulfilled as cross-listing in 2000 is correlated with cross-listing over the period 2001-2007 (a partial *F*-test of the instrument is highly significant). The instrument is likely to also satisfy the second requirement, i.e., it should not directly affect analyst forecasts in the years 2001-2007, except through its effect on the instrumented variable.[5]

Our main finding is that while both treatment and control firms experienced a decrease in opaqueness following the passage and implementation of SOX, this decrease was significantly larger for cross-listed firms. In other words, relative to the sample of control firms, SOX-affected firms became less opaque. This finding is robust to controlling for a wide set of variables that may affect analyst earnings forecasts, to using firm as well as country-year fixed effects, and to accounting for delistings, endogeneity of the treatment status, and changes in corporate risk taking.[6] Our results are further robust to removing the

[3] We do provide some evidence suggesting that this is indeed the case.

[4] The question whether SOX actually induced firms to delist remains controversial. See, among others, Engel, Hayes, and Wang (2007), Leuz (2007), Leuz, Triantis, and Wang (2008), Piotroski and Srinivasan (2008), Doidge, Karolyi, and Stulz (2009, 2010), and Zingales (2007).

[5] Iliev (2009) uses a similar approach to instrument for SOX Section 404 treatment status for a sample of US firms.

[6] We control for changes for risk taking to mitigate the concern that our results could be driven by a reduction in corporate risk taking, rather than an increase in transparency *per se*. Bargeron, Lehn, and Zutter (2009) provide evidence suggesting that US firms reduced their risk taking following SOX. Litvak (2008) provides similar

time series dimension and aggregating the data into a pre- and post-SOX period in order to address possible downward biases in the standard errors due to serial correlation in the error terms (Bertrand, Duflo, and Mullainathan 2004). We also provide evidence suggesting that the opaqueness-decreasing effect of SOX was more pronounced for firms operating in relatively opaque industries, such as the technology sector and financial services.

A potential concern to our findings could be that contemporaneous disclosure-related regulatory changes in the EU, such as the adoption of IFRS in 2005, may be driving our results. Indeed, our finding that both cross-listed and non-cross-listed firms became less opaque over time may suggest that contemporaneous local reforms had a positive effect on transparency. This would be of major concern for our difference-in-differences analysis if firms that are cross-listed on US exchanges responded more *positively* to these local regulatory changes than firms that are not cross-listed (as in this case the effect of SOX on transparency would be overestimated).[7] We believe this is unlikely to be the case. As non-cross-listed firms are not subject to US listing requirements and SEC oversight, it seems plausible that these firms are inherently more opaque than cross-listed firms (e.g., Lang, Lins, and Miller 2003, Doidge, Karolyi, and Stulz 2004). We would thus expect that disclosure-related reforms in the EU had a *stronger* effect on non-cross-listed firms than on cross-listed firms. Consistent with this view, Daske, Hail, Leuz, and Verdi (2008) find that cross-listed firms experienced lower, if any, market liquidity benefits following the adoption of IFRS compared to firms that are not cross-listed. To the extent that liquidity is positively related to our transparency measures, their findings suggest that the adoption of IFRS in the EU had a stronger transparency-enhancing effect on non-cross-listed firms than on cross-listed firms. If anything, therefore, we would therefore expect our findings to *underestimate* rather than overestimate the effect of SOX on opaqueness.[8]

We complement our analysis with evidence for a potential channel through which SOX could have affected opaqueness. To this end, we undertake a comprehensive textual analysis of corporate annual reports, and study how firms' disclosure and reporting behavior changed after the passage and implementation of SOX.[9] We subsequently compare the

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evidence for SOX-affected cross-listed firms. Using a structural estimation setting, Kang, Liu and Qi (2010) find that, relative to UK firms, US firms applied higher discount rates after 2002.

[7] We employ country-year fixed effects to account for regulatory changes at the country level, e.g. domestic corporate governance codes, which affect treatment and control firms similarly.

[8] Likewise, if SOX also affected the control firms because of governance externalities, this would bias our results against finding transparency effects that can be attributed to SOX.

[9] Textual analysis is increasingly used in finance to analyze the tone and informational content of corporate documents (see, e.g., Antweiler and Frank 2004, Loughran and McDonnald 2009, and Li 2008).

changes between the treatment firms and a set of country, industry, and size matched control firms. For a set of qualitative and quantitative measures, we find that, relative to the control sample, the annual reports of cross-listed firms became more comprehensive, provided more forward looking information, and provided more information on items that seem particularly relevant for analysts conducting accurate forecasts.[10]

Our research contributes to an ongoing debate on the effects of SOX. A number of recent papers have used event study methodology to construct *ex ante* measures of the economic consequences of SOX (e.g., Chhaochharia and Grinstein 2007, Litvak 2007, Zhang 2007, Hochberg, Sapienza, and Vissing-Jorgenson 2009). For example, Litvak (2007) finds that, relative to comparable non-cross-listed firms, cross-listed firms experienced declines in their stock prices following SOX-related legislative and regulatory announcements. This suggests that investors expected SOX to have a negative valuation effect.[11] Our paper uses a similar identification strategy in that it compares cross-listed firms with firms that are not cross-listed, but it isolates one (potentially beneficial) aspect of SOX, namely, the effect on opaqueness, and it uses an *ex post* measure of how firms were actually affected by the law.

Begley, Cheng, and Gao (2007) show that SOX temporarily increased the quality of information of US firms, measured also using analyst forecasts. Contrary to our paper, their study does not compare cross-listed and non-cross-listed firms using a difference-in-differences approach, which makes it more difficult to establish causality. Finally, Cohen, Dey, and Lys (2008) show that earnings management decreased after SOX, and Iliev (2009), using a regression discontinuity design, documents that SOX Section 404 led to more conservative reported earnings. Their evidence complements our work by pointing to another potential channel—earnings management—through which SOX could have affected corporate behavior.

Our paper further contributes to the literature on cross-listings. Previous research has documented that non-US firms that cross-list on US exchanges have higher valuations (Doidge, Karolyi, and Stulz 2004), lower costs of capital (Errunza and Miller 2000, Hail and Leuz 2008), positive abnormal returns when announcing a cross-listing (Foerster and Karolyi 1999, Miller 1999), higher stock price informativeness (Fernandes and Ferreira 2008), and stronger return reactions to earnings announcements (Bailey, Karolyi, and Salva 2006). Lang,

[10] For example, annual reports contain more discussion on future risks and opportunities, more explicit information about expected future earnings, and more information on past unusual or nonrecurring events and their past effects on the company.

[11] However, as emphasized by Leuz (2007), it may not be clear whether the negative price reactions are due to SOX per se or inconsistencies with local regulation making SOX more costly for foreign firms.

Lins, and Miller (2003) document that cross-listed firms have lower forecast errors and are followed by more financial analysts. Some of these benefits have been attributed to gains from moving from a poor corporate governance environment to an environment with increased governance enforcement and corporate transparency ("bonding hypothesis"). Our paper provides some direct evidence on a channel through which changes in US corporate governance regulation affected cross-listed firms, while controlling for other contemporaneous effects and reforms.

More generally, our paper contributes to a large literature on the economic consequences of changes in the regulation of transparency and corporate disclosure (for a survey, see Leuz and Wysocki 2008). For example, our research complements Tong (2007) who analyzes the effect of the International Monetary Fund's Special Data Dissemination Standard (SDDS) initiative on analyst forecast accuracy and dispersion in thirty developing countries for the period 1990-2004. Our paper is also related to Bushee and Leuz (2005) who examine the consequences of a regulatory change mandating OTC bulletin board firms to comply with reporting rules under the Securities Exchange Act. This change resulted in a substantial increase in information disclosure of firms that previously did not file with the SEC and, eventually, led to an increase in their liquidity. Studying the 1964 Securities Acts Amendments, another important disclosure reform that extended disclosure requirements to OTC firms, Greenstone, Vissing-Jorgenson, and Oyer (2006) provide evidence indicating that investors valued these disclosure requirements.

The rest of the paper is organized as follow. Section 2 provides a brief description of the institutional set-up, Section 3 contains a description of the data and the variables, and Section 4 reports the empirical findings. Section 5 provides evidence from a textual analysis of annual reports, and Section 6 concludes.

2. Institutional Background

The Sarbanes-Oxley Act was signed into law on July 30, 2002. As stated in the preamble of the Act, its aim is "to protect investors by improving the accuracy and reliability of corporate disclosures". The Act applies to both US and foreign companies registered and reporting with the SEC. Such foreign firms typically are either directly listed on a US stock exchange or have Level 2 or 3 ADR programs.

SOX may reduce the opaqueness of firms through a variety of disclosure requirements and corporate governance mandates (e.g., Coates 2007, Krozner 2003, Holmstrom and Kaplan 2005). Title IV, for example, mandates additional financial disclosures on items such as off balance sheet transactions (Section 401), pro forma figures (Section 401), insider trading (Section 403), and material changes in the financial condition or operations of a company (Section 409). Section 404(a) requires management to assess and certify the effectiveness of the internal control structure and procedures for financial reporting, and to report their findings in a special management's report. Section 404(b) requires an auditor to attest to management's assessment of the effectiveness of internal control over financial reporting. Title III may also affect opaqueness by making requirements for the composition and working of the audit committee (Section 301) and by requiring the CEO and CFO to certify that, based on their knowledge, the annual report contains all material information and represents the financial condition and results fairly (Section 302). Section 906 contains a similar certification requirement, and imposes criminal penalties for knowingly or willingly filing false certifications. Finally, the provisions in Title II on independent auditors and audit partner rotation and the provisions in Title VIII on whistleblower protection may have led to more scrutiny over firms' financial reporting.

While many of the provisions and mandates of SOX were effective immediately or over the course of 2003, companies were given more time to put in place internal control systems to be able to comply with Section 404—arguably one of the most important provisions from a transparency perspective. Initially, the SEC required foreign firms to begin to comply with Section 404 for the fiscal year ending on or after April 15, 2005 (SEC Release 33-8328, June 5, 2003). Over the coming months and years, the SEC repeatedly extended this deadline. Ultimately, foreign firms with public floats between USD 75m and 700m ("accelerated filers") had to comply with Sections 404(a) and (b) by July 15, 2006 and July 15, 2007, respectively. Large accelerated foreign filers (public float above USD 700m) had to comply with Sections 404(a) and (b) by July 15, 2006.[12] The timing of events suggests that it may be difficult to pin down an exact cut-off date where SOX started to affect corporate disclosure behavior and analyst earnings forecasts. To account for this, we will consider two alternative cut-off dates in our empirical analysis below. Specifically, we will consider in a first step that the years before 2005 constitute the "before SOX" period and the years 2005

[12] Non-accelerated foreign filers (public float below USD 75m) had to comply with Sections 404(a) and (b) by December 15, 2007 and December 15, 2009, respectively. Our sample of treatment firms does not include non-accelerated foreign filers.

and beyond constitute the "after SOX" period. We will subsequently show that our findings are robust to considering beginning of 2006 as an alternative cut-off date to account for the extension of Section 404 compliance deadlines.

3. Data and Summary Statistics

We gather analyst earnings forecast and actual EPS data from the IBES database. For reasons discussed in the Introduction, we focus on firms from the EU-15 countries. The sample period is 2001 to 2007. We focus on full-year EPS forecasts with a one-year forecasting horizon. This means that for each given firm we collect forecasts made in a given fiscal year for full-year earnings of *that* year. We restrict attention to EPS forecasts made within one quarter after the report date of previous full-year earnings. In case an analyst provides more than one EPS forecast within this period, we use the last forecast issued by the analyst within this period. We exclude firms for which we cannot compute our opaqueness measures in at least one year. This leaves us with a sample of 2,489 firms. The country distribution of the sample is reported in Table 1.

From the analyst forecast and actual EPS data, we construct two measures of corporate opaqueness: *Forecast Error* and *Forecast Dispersion.* Both measures pertain to the ability of financial analysts to accurately predict earnings. The first measure, *Forecast Error*, is the absolute value of the difference between the average earnings per share (EPS) forecast and actual EPS, scaled by the absolute value of actual EPS, i.e.,

$$Forecast\ Error = \left| \frac{Mean\_Estimate - Actual}{Actual} \right|$$

The second measure, *Forecast Dispersion*, is the absolute value of the standard deviation of EPS forecasts divided by the mean forecast (i.e., the coefficient of variation),

$$Forecast\ Dispersion = \left| \frac{SD\_Estimate}{Mean\_Estimate} \right|$$

Forecast Error measures how far the analyst consensus is from actual earnings, whereas *Forecast Dispersion* measures the degree of "disagreement" among analysts. We argue that either measure provides a natural proxy for firm-level opaqueness. To be able to construct our measures, we require observations with at least two EPS estimates and non-zero actual and mean estimate EPS. We therefore disregard observations with only one EPS estimate or where actual EPS or mean estimate EPS are zero. To remove outliers, we winsorize our

opaqueness measures at 5%. Our results are similar if we do not winsorize (reported in Table 8).

We identify cross-listed firms from the annual SEC lists of foreign companies registered and reporting with the SEC.[13] These lists contain all foreign companies registered and reporting with the SEC at year end. We do not consider firms that are traded on OTC markets. Furthermore, we exclude firms with market capitalizations below USD 75m, as these firms had to comply with Sections 404(a) and (b) only by end of 2007 and 2009, respectively, and firms for which we were unable to find data for at least one firm-year in our analyst database. This leaves us with 189 firms that were cross-listed on December 31, 2000. Out of these firms, 76 firms were continuously cross-listed from December 31, 2000 to December 31, 2007, while the rest delisted between 2001 and 2007.

As discussed in the Introduction, if firms that delisted during the sample period were inherently more opaque than firms that did not delist,[14] we might spuriously detect an opaqueness-reducing effect of SOX merely because over time relatively opaque firms dropped out of the sample of cross-listed firms. To avoid this "survivorship bias" problem, we limit the treatment sample in our main specifications to firms that were continuously cross-listed over the entire sample period. Figure 1 provides an overview of the firms that were cross-listed at the end of the year 2000 and of how many firms delisted in the years till 2007.

Table 1 shows that the country-distribution of cross-listed and non-cross-listed firms is roughly similar. We document in the robustness section that our results are robust to excluding UK firms, which constitute the biggest country group, and firms from The Netherlands, which are somewhat overrepresented in the treatment group.

We complement our analyst data with information on firm characteristics from Datastream Worldscope. Table 2 provides summary statistics for the cross-listed and non-cross-listed firms, respectively. All variables are defined in Appendix A-1. As expected, and consistent with Lang, Lins, and Miller (2003), cross-listed firms are larger and followed by more analysts. While forecast dispersion does not differ significantly between the two groups, cross-listed firms have significantly lower forecast error.

[13] See http://www.sec.gov/divisions/corpfin/internatl/companies.shtml.
[14] For supportive evidence, see the results in Appendix A-2.

4. Empirical Results

4.1 Between Group Differences: Univariate Results

As explained above, we assume in a first step that the years 2001 to 2004 constitute the "before SOX" period, and the years 2005 to 2007 constitute the "after SOX" period. Table 3 provides some first statistics of our two opaqueness measures, *Forecast Error* and *Forecast Dispersion*, for the periods before and after SOX. The table reports average values for the two measures, separately for firms that were cross-listed in the US (treatment group) and those that were not (control group). While both groups of firms experienced a decrease in both *Forecast Error* and *Forecast Dispersion* in the years after SOX, the table shows that the decrease in both measures was significantly larger for firms that were cross-listed and hence subject to SOX. This provides some univariate evidence suggesting that, relative to the control group of firms that were not subject to SOX, cross-listed firms became less opaque following the implementation of SOX.

4.2 Multivariate Results

Table 4 extends the univariate analysis from Table 3 to a difference-in-differences regression setting to control for a wide set of factors that may affect analyst forecasts. Our basic empirical specification is as follows:

$$Opaqueness_{it} = Post\ SOX_t * Cross\text{-}Listed_i + Post\ SOX_t + Cross\text{-}Listed_i + \mathbf{X}_{it} + \boldsymbol{y}_i + \varepsilon_{it} \qquad (1)$$

where *t* denotes year, *i* denotes firm, *Post SOX* is a dummy taking the value one if and only if *t*=2005 or later, and *Cross-Listed* is a dummy taking the value one if and only if a firm is in the treatment sample. The coefficient of interest is the coefficient of the interaction dummy, *Post SOX * Cross-listed*. The dependent variable in our regressions, proxying for opaqueness, is either *Forecast Error* or *Forecast Dispersion*. A decrease in the dependent variable thus corresponds to a decrease in opaqueness. We use the natural logarithm of *Forecast Error* and *Forecast Dispersion*, as both variables are highly positively skewed. The regressions use firm fixed effects to account for unobserved heterogeneity at the firm level. Standard errors are heteroskedasticity robust and clustered at the firm level to account for intra-firm correlation in the panel. For robustness, we also report regressions with year fixed effects and country-

year fixed effects (to account for, e.g., changes in local governance regimes). Our results are robust if we use industry fixed effects (not reported).

As control variables we include proxies for firm size and leverage. We further include the absolute value of the first difference in EPS scaled by previous year's EPS ("*Surprise*") to control for the fact that a large change in earnings is likely to increase forecast error and dispersion. We also include a dummy ("*Loss*") that is one whenever a firm had negative earnings in the previous year, and a dummy ("*Quarter Report*") that is one whenever a firm reports on a quarterly basis. Lastly, as governance and disclosure regulation may affect analyst following, which in turn may affect our opaqueness measures, we also control for the number of analysts over time (*"Analyst Number").*

The estimates in Panel A (*Forecast Error*) and Panel B (*Forecast Dispersion*) confirm the univariate results: relative to the control firms, cross-listed firms experienced a significantly stronger decrease in both *Forecast Error* and *Forecast Dispersion* following the passage and implementation of SOX. The results are robust to using firm fixed effects, country-year fixed effects, and even both firm and country-year fixed effects. While cross-listed firms experienced a stronger decrease in opaqueness according to both measures, the results are particularly pronounced for the forecast error measure. In terms of economic magnitudes, based on the estimates in column (4), cross-listed firms experienced a 32% larger reduction in the (log of the) forecast error than non-cross-listed firms. Relative to the panel standard deviation of the (log of the) forecast error, this corresponds to a substantial (0.32/1.45=) 22%.[15] Similarly, the reduction in forecast dispersion was 17% larger for the treatment firms, which equals (0.17/1.06=) 16% of the variable's standard deviation.

Figure 2 depicts the evolution of *Forecast Error* (Panel A) and *Forecast Dispersion* (Panel B) over the sample period. The estimates for the changes in the earnings forecast measures are obtained from regression estimates i.e., after controlling for a wide set of variables, and are indexed at 100 in the year 2001. The graphs show that both measures decreased substantially faster for cross-listed firms in each of the years after SOX came in effect, i.e., in 2005, 2006, and 2007. The graphs also show that *prior* to SOX the treatment and control firms' outcome variables followed a roughly similar trend. This is important since a key identifying assumption underlying our estimation approach is that the outcome variables of the treatment firms would have followed a similar trend as the outcome variables of the control firms if the treatment firms had *not* been subject to the treatment (e.g., Angrist

[15] Note that Table 2 reports descriptive statistics of the forecast error and forecast dispersion before taking the logarithm.

and Pischke 2009). While it is difficult to directly test the validity of this assumption, a common plausibility check is to verify whether the treatment and control firms' outcome variables followed a similar trend *prior* to the treatment. Figure 2 suggests that this is indeed the case.[16]

One might expect that the documented effects of SOX are stronger for firms that are inherently opaque due to the nature of their business activities, and for firms that are located in countries with relatively weak domestic disclosure standards. To investigate these issues, we separate the sample firms based on (i) the industries they operate in, and (ii) the legal origin of their home countries. We assume that some industries are generally more opaque and exposed to information asymmetries vis-à-vis investors (e.g., because firms operating in these industries rely more on intangible assets). We consider the technology sector and financial services to be highly informationally sensitive industries, and the consumer goods, utilities, transport, energy, and health sectors to be less informationally sensitive. La Porta et al. (1998) and La Porta et al. (2006) have shown that common law countries typically have stronger disclosure standards than civil law countries. We thus use legal origin as a proxy for the strength of disclosure standards.

The results in Table 5 suggest that the effect of SOX was particularly pronounced for firms operating in informationally sensitive industries. The effects of SOX are both economically and statistically different between firms from the two different industry samples. We also provide some, albeit weak, evidence that SOX had a stronger impact on forecast dispersion in civil law countries than in common law countries: the coefficient of interest for firms from civil law countries is significant and exceeds (in terms of absolute sign) the corresponding coefficient for firms from common law countries. The difference between the two coefficients is moreover marginally significant (the *p*-value of a Wald-test comparing the two coefficients equals 11.11%). The results on forecast error are less conclusive as the difference between the two coefficients of interest is highly insignificant.

4.3 Robustness Checks

Through Sections 404 and 906, SOX significantly increased the personal liability risk of corporate executives. This may have suppressed executives' willingness to take corporate risks and thereby led to a decrease in corporate risk-taking (e.g., Bargeron, Lehn, and Zutter

[16] See the Introduction for a discussion of the related concern that our results could be driven by contemporaneous local changes in regulation, such as the adoption of IFRS in 2005.

2009, Litvak 2008). As risk taking may affect analysts' forecasts (corporate earnings may become more predictable) and therefore our opaqueness measures, it is possible that our results are partially explained by a reduction in risk-taking rather than a decrease in opaqueness *per se*. To address this concern, we construct various proxies for risk-taking and use these variables as additional controls in an attempt to account for changes in corporate risk-taking. We use investment (capital expenditure over total assets), stock price beta, and stock price volatility as risk proxies (cf., Bargeron, Lehn, and Zutter 2009, Litvak 2008). We include these variables in our regressions both individually and interacted with the post SOX dummy to allow the coefficient of the risk-taking variable to be different before and after the introduction of SOX. The obtained regression results, reported in Table 6, show that our results are robust to controlling for changes in corporate risk-taking.

In the next robustness check, we use an instrumental variables estimation approach to account for the potential endogeneity of the treatment status: firms may, in principle, delist in an attempt to evade SOX-compliance. This may bias our results. To construct an instrument for the treatment status of a firm, we exploit the fact that SOX was passed and enacted in 2002. SOX-avoidance could therefore not have been a reason for firms to delist in the year 2000, as firms were not aware of SOX at this point in time.[17] Following this approach, we create a dummy variable that takes the value one if and only if a company was cross-listed in the US in 2000 and use it as an instrument for the treatment status. More specifically, given that two of our variables (*Cross-Listed* and *Post SOX * Cross-Listed*) are endogenous, we run two first stage regressions:[18]

$$\textit{Cross-Listed}_i = \textit{Cross-Listed in 2000}_i + \textit{Post SOX}_t * \textit{Cross-Listed in 2000}_i + \textit{Post SOX}_t + \mathbf{X}_{it} + \varepsilon_{it} \quad (2)$$

$$\textit{Post SOX} * \textit{Cross-Listed}_i = \textit{Cross-Listed in 2000}_i + \textit{Post SOX}_t * \textit{Cross-Listed in 2000}_i + \textit{Post SOX}_t + \mathbf{X}_{it} + \mu_{it} \quad (3)$$

where *Cross-Listed in 2000* is a dummy variable taking the value one if and only if a firm was cross-listed in the US at the end of year 2000, and *Cross-Listed* is a dummy variable taking the value one if and only if a firm was cross-listed in the US from end of 2000 to end

[17] Similarly, Iliev (2009), looking at US firms, uses firm size in 2002 to instrument for firm size in 2004, which in turn determines whether a firm has to comply with Section 404 of SOX. These firms had to comply for the first time with Section 404 in 2004 and were unaware of the size trigger in 2002.

[18] Tsoutsoura (2010) uses a similar methodology to study the effect of succession taxes on firm succession and investment decisions.

of 2007.[19] The two instruments are *Cross-Listed in 2000* and *Cross-Listed in 2000 * Post SOX*. To analyze the effect of SOX on opaqueness, we estimate our main regression specification using IV 2SLS. Instruments should fulfill the relevancy and the exclusion conditions. Our two instruments fulfill the first condition as they are highly significantly correlated with the endogenous variables (partial *F*-tests of the instruments are highly significant). The instruments are likely to also satisfy the second requirement, i.e., they should not directly affect the outcome variable, earnings forecasts in the years 2001-2007, except through their effect on the instrumented variables.

The IV estimates are reported in Table 7. The standard errors of the IV regressions are robust and clustered at the firm level. The estimates show that our key coefficient remains negative and significant, even after accounting for the potential endogeneity of the cross-listing status. The results are very similar if we include firm fixed effects.

Table 8 provides further robustness checks. The table presents coefficients of the interaction dummy (*Post SOX * Cross-Listed*) for different regression specifications. In column 1 the dependent variable is *Forecast Error*, while in column 2 it is *Forecast Dispersion*. All regressions include *Analyst Number*, *Loss*, *Log(Surprise)*, *Log(Firm size)*, and *Leverage* as controls.

In the first robustness check, the treatment group consists of firms that were cross-listed at the end of year 2000, regardless of whether they delisted at a later point in time. Similar to the IV approach, this robustness check mitigates concerns that our results are biased due to the possibility that firms for which SOX would have a negative effect on transparency decided to delist to evade SOX-compliance. This would leave only those firms in the treatment sample for which SOX had a positive effect on transparency.[20] Leaving delisting firms in the treatment group should create estimates that are biased *against* finding a significant effect of SOX on transparency. In the second robustness check, the treatment group consists of firms that were cross-listed in the US from end of 2000 to end of 2006 (rather than end of 2000 to end of 2007). In the third robustness check, the *Post SOX* dummy takes the value one for the years 2006 and 2007 and zero otherwise. We perform this robustness check to analyze how robust our results are to different compliance dates. This may be important in view of the fact that the SEC repeatedly extended SOX 404 compliance

[19] Notice that we use a linear specification for the first stage models. As emphasized by Angrist and Krueger (2001), using probit or logit models in the first stage to instrument for dummy endogenous regressors would produce inconsistent second-stage estimators. By contrast, linear first stage models produce consistent second-stage estimators.

[20] Even if this were the case, our baseline findings would still suggest that SOX increased transparency for firms that were cross-listed in the US and did not delist over the sample period.

dates for foreign issuers. In the fourth robustness check, the dependent variables are not winsorized. In the fifth robustness check, we restrict the control sample to firms with a market capitalization above USD 75m. We do this to mitigate concerns that our results may be driven by systematic size differences between the control and treatment firms. In the sixth robustness check, we exclude firms from the UK. Firms from the UK are the biggest group in the sample, making up about 30% of all observations. This analysis allows to assess to what extent our results are purely driven by UK firms. In the seventh robustness check, we exclude firms from the Netherlands, as these firms are somewhat overrepresented in the treatment group (cross-listed firms) compared to the control group (non-cross-listed firms). The estimates reported in Table 8, rows 1 to 7, show that our results are generally robust to these various alternative specifications.

As emphasized by Bertrand, Duflo, and Mullainathan (2004), difference-in-differences regressions may produce downward biased standard errors due to the potential serial correlation in the error terms. We address this concern in our last robustness check. Following a procedure proposed by Bertrand, Duflo, and Mullainathan (2004), we proceed by ignoring the time series dimension and averaging the data before and after SOX. We subsequently run our regressions using the averaged data. The results reported in Table 8, row 8, show that the coefficient of interest remains significant for the forecast error measure. Our results are weaker if we use forecast dispersion as the dependent variable.[21]

5. Textual Analysis of Annual Reports

Our results thus far suggest that, relative to a control sample of SOX-unaffected firms, cross-listed firms became less opaque following SOX. More specifically, we found that, relative to the control firms, cross-listed firms experienced a significantly stronger decrease in both analyst forecast dispersion and analyst forecast error. To understand a possible channel behind these findings, we conduct a comprehensive textual analysis of the annual reports of the firms in our sample. Textual analysis is increasingly used in finance and accounting to measure the tone and informational content of corporate documents (e.g., Loughran and McDonnald 2009, Li 2008, Antweiler and Frank 2004).

[21] However, as discussed by Bertrand, Duflo, and Mullainathan (2004), this may be due to the low statistical power of this procedure. Power issues are likely to be particularly severe in our case where the sample size is relatively small (the treatment group consists only of 76 firms).

We expect that if SOX made firms less opaque, we should find some evidence in firms' annual reports pointing to such decreases in opaqueness. More specifically, we expect to find that the annual reports of SOX-affected firms became more comprehensive and provided more information about items that analysts deem particularly relevant for conducting accurate forecasts. We look at annual reports as they constitute an important, publicly disclosed source of information for analysts and the investor community at large, and contain information about the past, current and future earnings of a firm.

To perform our analysis, we collect the annual reports for the years 2002 and 2007 for all cross-listed firms. We then compare, according to several qualitative and quantitative measures, how disclosure in annual reports changed from 2002 to 2007. To control for other contemporaneous influences, we again compare the changes in the disclosure of cross-listed firms (treatment group) with those of a matched set of non-cross-listed firms (control group). The firms in the control group were selected based on a country, industry, and size match from the full set of non-cross-listed EU-15 firms. We were able to find such matches and the required annual reports for 50 of the 76 cross-listed firms.

We develop eight measures for the annual report analysis based on a set of interviews that we ran with financial analysts to understand what they deem crucial for conducting accurate forecasts. Our first three measures are of a quantitative nature and measure the number of pages, the number of words, and the number of sentences with forward looking information in the annual reports. To measure the latter, we perform a textual analysis and define a set of 30 words that are likely to be associated with forward looking information (e.g., "anticipate", "expect", or "forecast"). We then count in how many sentences these words occur in the annual reports. Our next five measures are more of a qualitative nature and measure whether firms explicitly provide information on future risks or opportunities, provide an explicit statement of the expected future growth in earnings, and discuss unusual or nonrecurring events and their past effects on the company. Finally, we measure whether firms provide a comparison of the realization of opportunities, risks, and plans versus the expectations they had about these issues. For all these measures, we manually read and analyze all annual reports and create dummy variables taking the value 1 if we can find information on the above issues.

The corresponding results are reported in Table 9, separately for cross-listed and non-cross-listed firms. They show that for both types of firms annual reports became more comprehensive, provided more forward looking information, and discussed more items that are relevant for financial analysts when making financial forecasts. Most importantly, seven

of the eight measures suggest that these changes have been more pronounced for cross-listed firms. These findings provide some indication for a possible channel through SOX could have reduced the opaqueness of firms.

6. Conclusions

The Sarbanes-Oxley Act of 2002 provides a natural experiment to study the effect of corporate governance and disclosure reform on corporate opaqueness. The reason is that SOX does not only apply to US-domiciled firms but also to cross-listed foreign firms. One can thus devise a clean test where changes in opaqueness of cross-listed firms that are subject to SOX are compared against changes in opaqueness of comparable firms that are not cross-listed and hence not subject to SOX.

Following this approach, we find that while both treatment and control firms experienced a reduction in opaqueness following SOX, this decrease was significantly larger for cross-listed firms. We construct proxies for firm-level opaqueness from analyst earnings forecasts. Our findings are robust to controlling for a wide set of variables that may affect analyst earnings forecasts, and to accounting for the potential endogeneity of the treatment status and changes in corporate risk taking. We find that the opaqueness-reducing effect of SOX was particularly pronounced for firms operating in informationally sensitive industries.

We also provide evidence for a channel through which SOX may have affected opaqueness by studying how disclosure and reporting in annual reports changed after SOX. For a set of qualitative and quantitative measures, we find that annual reports of cross-listed firms became more comprehensive, provided more forward looking information, and provided more information on number of items that analysts deem crucial for conducting accurate forecasts.

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Table 1: Country Distribution of Sample

This table presents the country distribution of the sample and reports the number of firm-year observations for non-cross-listed and cross-listed firms. A firm is considered cross-listed if it was continuously cross-listed in the United States from end of 2000 to end of 2007. The total number of firms in the sample is 2,489. All firms are from EU-15 countries.

Country	*Non-Cross-Listed*		*Cross-Listed*		*All Firms*	
	Firm-years	*Percent*	*Firm-years*	*Percent*	*Firm-years*	*Percent*
Austria	162	2%	0	0%	162	2%
Belgium	350	3%	0	0%	350	3%
Denmark	336	3%	7	1%	343	3%
Finland	570	6%	14	3%	584	5%
France	1,161	11%	63	13%	1,224	11%
Germany	1,236	12%	53	11%	1,289	12%
Greece	308	3%	11	2%	319	3%
Ireland	194	2%	34	7%	228	2%
Italy	618	6%	21	4%	639	6%
Luxembourg	34	0%	9	2%	43	0%
Netherlands	562	5%	97	19%	659	6%
Portugal	98	1%	6	1%	104	1%
Spain	488	5%	28	6%	516	5%
Sweden	754	7%	0	0%	754	7%
United Kingdom	3,421	33%	158	32%	3,579	33%
	10,292	100%	501	100%	10,793	100%

Table 2: Descriptive Statistics for Cross-Listed and Non-Cross-Listed Firms

This table provides summary statistics for the cross-listed and non-cross-listed firms in the sample. A firm is defined as cross-listed if it was continuously cross-listed in the United States from end of 2000 to end of 2007. For definitions of all variables see Appendix A-1. All cross-listed and non-cross-listed firms are publicly traded firms from the EU-15 countries. The sample period is from 2001 to 2007.

	Non-Cross-Listed Firms				*Cross-Listed Firms*				*Difference (p-values)*	
Variable	*Obs.*	*Mean*	*Median*	*STD*	*Obs.*	*Mean*	*Median*	*STD*	*Means*	*Medians*
Forecast Error	10292	0.542	0.212	0.772	501	0.478	0.161	0.754	0.0700	0.0007
Forecast Dispersion	8884	0.201	0.106	0.244	480	0.209	0.104	0.253	0.5008	0.2881
Analyst Number	10292	7.243	5.000	6.931	501	20.269	19.000	11.097	0.0000	0.0000
Surprise	9860	1.675	0.326	21.784	499	0.676	0.240	1.272	0.3058	0.0000
Loss	9860	0.140	0.000	0.347	499	0.136	0.000	0.343	0.8365	0.8364
Firm Size (million EUR)	10175	2902	507	8278	501	31615	14743	40254	0.0000	0.0000
Leverage	10219	5.870	2.539	13.580	500	9.623	2.847	16.402	0.0000	0.0000
Quarter Report	10292	0.966	1.000	0.180	501	1.000	1.000	0.000	0.0000	0.0000
Investment	9981	0.055	0.037	0.310	496	0.041	0.033	0.039	0.3166	0.0007
Price Volatility	7890	32.962	30.970	18.465	475	35.720	32.390	15.801	0.0015	0.0008
Beta	10019	1.034	0.860	0.960	495	1.373	1.240	2.017	0.0000	0.0000

Table 3: Changes in Earnings Forecasts and SOX: Between Group Differences

This table reports between group differences for *Forecast Error* (Panel A) and *Forecast Dispersion* (Panel B). Both variables are used as proxies for firm-level opaqueness. *Forecast Error* is the absolute value of the difference between the average EPS forecast for a given firm-year and actual EPS, divided by actual EPS. *Forecast Dispersion* is the absolute value of the coefficient of variation of EPS forecasts for a given firm-year. Column I contains the average values of *Forecast Error* (Panel A) and *Forecast Dispersion* (Panel B) pre SOX (i.e., 2001-2004). Column II contains the average values of *Forecast Error* (Panel A) and *Forecast Dispersion* (Panel B) for the years Post SOX (i.e. 2005-2007). Column III contains the change in average *Forecast Error* and *Forecast Dispersion* (Post SOX – Pre SOX). In all cases, the table compares non-cross-listed and cross-listed firms from EU-15 countries and reports the between group differences (Non-Cross-Listed – Cross-Listed). A firm is defined as cross-listed if it was continuously cross-listed in the United States from end of 2000 to end of 2007. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

Panel A: Forecast Error

	Pre SOX (Before)	*Post SOX (After)*	*Post SOX – Pre SOX (After-Before)*
	(I)	*(II)*	*(II-I)*
Non-Cross-Listed	0.636***	0.367***	-0.269***
Cross-Listed	0.628***	0.266***	-0.362***
Between Group Difference (Cross-Listed – Non-Cross-Listed)	-0.008	-0.101**	Diff-in-diff -0.093**

Panel B: Forecast Dispersion

	Pre SOX (Before)	*Post SOX (After)*	*Post SOX – Pre SOX (After-Before)*
	(I)	*(II)*	*(II-I)*
Non-Cross-Listed	0.231***	0.147***	-0.084***
Cross-Listed	0.262***	0.131***	-0.131***
Between Group Difference (Cross-Listed – Non-Cross-Listed)	0.031*	-0.016	Diff-in-diff -0.047**

Table 4: Analyst Forecast Error and Dispersion: Difference-in-Differences Estimates

This table looks at the determinants of the logarithm of *Forecast Error* and *Forecast Dispersion*. Both variables are used as proxies for firm-level opaqueness. *Forecast Error* is the absolute value of the difference between the average EPS forecast for a given firm-year and actual EPS, divided by actual EPS. *Forecast Dispersion* is the absolute value of the coefficient of variation of EPS forecasts for a given firm-year. Both variables are winsorized at 5%. *Post SOX* is a dummy variable that takes the value 1 for the years 2005 to 2007, i.e., for the post SOX period. *Cross-Listed* is a dummy variable that takes the value 1 if a firm was continuously cross-listed in the US from end of 2000 to end of 2007. For definitions of all variables see Appendix A-1. The regressions use annual data from 2001 to 2007. In regressions (4) and (5), year dummies for 2001 and 2007 are omitted to avoid multicollinearity. Robust t-statistics are reported in parentheses. All standard errors are clustered at the firm level. Constants were included in the regressions but are not reported. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

Panel A

	Dependent variable: log(*Forecast Error*)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post SOX * Cross-Listed			-0.3182**	-0.3177**	-0.3087**	-0.2514**	-0.3401**	-0.2838**	-0.3370**
			(-2.37)	(-2.39)	(-2.30)	(-2.25)	(-2.44)	(-2.54)	(-2.39)
Post SOX		-0.4121***	-0.3922***	-0.6092***	-0.6210***	0.0585	0.3190	-0.0743	-0.3093
		(-11.82)	(-10.88)	(-10.12)	(-9.88)	(0.16)	(0.90)	(-0.10)	(-0.31)
Cross-Listed						0.2580***		0.3374***	
						(2.66)		(3.37)	
Analyst Number	-0.0198	-0.0003	-0.0012	-0.0252	-0.0293	-0.1118***	-0.0141	-0.0258	-0.0162
	(-0.67)	(-0.01)	(-0.04)	(-0.86)	(-0.96)	(-8.38)	(-0.46)	(-1.34)	(-0.50)
Loss	0.1010	0.0071	0.0028	0.0168	0.0264	0.5431***	0.0276	0.4895***	0.0319
	(1.50)	(0.11)	(0.04)	(0.26)	(0.39)	(13.24)	(0.42)	(11.52)	(0.47)
Log(Surprise)	0.2376***	0.2199***	0.2195***	0.2239***	0.2230***	0.3695***	0.2213***	0.3645***	0.2206***
	(20.49)	(19.19)	(19.20)	(19.43)	(19.21)	(39.76)	(19.16)	(39.39)	(18.97)
Log(Firm Size)					0.0287			-0.0784***	0.0174
					(0.82)			(-5.82)	(0.48)
Leverage					0.0008			0.0021**	0.0009
					(0.40)			(2.01)	(0.46)
Quarter Report								-0.0350	
								(-0.51)	
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	No	Yes	No	Yes
Country-Year Fixed Effects	No	No	No	No	No	Yes	Yes	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	No	No	No	No
Obs.	10359	10359	10359	10359	10274	10359	10359	10274	10274
adj. R-sq	0.073	0.099	0.100	0.110	0.109	0.281	0.114	0.284	0.113

Panel B	Dependent variable: log(*Forecast Dispersion*)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post SOX * Cross-Listed			-0.1663	-0.1749*	-0.2268**	-0.1381	-0.1856*	-0.1551*	-0.2514**
			(-1.62)	(-1.70)	(-2.18)	(-1.63)	(-1.74)	(-1.83)	(-2.34)
Post SOX		-0.3122***	-0.3008***	-0.2632***	-0.0230	-0.9392**	0.0941	-0.7841*	-0.2281
		(-11.79)	(-10.94)	(-6.42)	(-0.55)	(-2.57)	(0.31)	(-1.67)	(-0.40)
Cross-Listed						0.1098		0.1361*	
						(1.57)		(1.89)	
Analyst Number	0.1028***	0.1276***	0.1276***	0.0893***	0.1157***	0.0670***	0.0961***	0.0979***	0.1320***
	(4.28)	(5.55)	(5.55)	(3.77)	(4.74)	(5.46)	(3.84)	(5.57)	(5.11)
Loss	0.8071***	0.7431***	0.7398***	0.6976***	0.6454***	1.0710***	0.6953***	1.0572***	0.6340***
	(14.51)	(13.81)	(13.75)	(12.89)	(11.54)	(29.65)	(12.63)	(28.54)	(11.13)
Log(Surprise)	0.1007***	0.0868***	0.0864***	0.0832***	0.0838***	0.1842***	0.0801***	0.1826***	0.0801***
	(10.96)	(9.64)	(9.63)	(9.30)	(9.36)	(24.72)	(8.98)	(24.44)	(9.01)
Log(Firm Size)					-0.1519***			-0.0243**	-0.1805***
					(-5.04)			(-2.11)	(-5.87)
Leverage					-0.0006			-0.0006	-0.0009
					(-0.33)			(-0.51)	(-0.48)
Quarter Report								-0.0582	
								(-0.87)	
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	No	Yes	No	Yes
Country-Year Fixed Effects	No	No	No	No	No	Yes	Yes	Yes	Yes
Year Fixed Effects	No	No	No	Yes	Yes	No	No	No	No
Obs.	9096	9096	9096	9096	9021	9096	9096	9021	9021
adj. R-sq	0.098	0.134	0.135	0.153	0.159	0.321	0.165	0.321	0.173

Table 5: Analyst Forecast Error and Dispersion: Estimates for Different Types of Firms

This table looks at the determinants of logarithms of *Forecast Error* (column 1-4) and *Forecast Dispersion* (column 5-8) for different subsets of firms. Both variables are used as proxies for firm-level opaqueness. In columns 1-2 and 5-6, firms are separated based on the degree of information asymmetry of the industries in which they are operating. We assume that technology firms and financials are characterized by a high degree of information asymmetry, and all other industries by a low degree. In columns 3-4 and 7-8, firms are separated based on whether they come from a common law or civil law country (La Porta et al. 1998). The table also reports the *p*-value of a Wald-test testing whether the coefficients of *Post SOX*Cross-Listed* differ between the samples of firms from industries with high and low information asymmetries, and from common law and civil law countries, respectively. *Post SOX* is a dummy variable that takes the value 1 for the years 2005 to 2007. *Cross-Listed* is a dummy variable that takes the value 1 if a firm was continuously cross-listed in the US from end of 2000 to end of 2007. For definitions of all variables see Appendix A-1. The regressions use annual data from 2001 to 2007. Robust t-statistics are reported in parentheses. All standard errors are clustered at the firm level. Constants were included in the regressions but are not reported. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

	Dependent variable: log(*Forecast Error*)				Dependent variable: log(*Forecast Dispersion*)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Firms from Industries with Low Info. Asymm.	Firms from Industries with High Info. Asymm.	Firms from Common Law Countries	Firms from Civil Law Countries	Firms from Industries with Low Info. Asymm.	Firms from Industries with High Info. Asymm.	Firms from Common Law Countries	Firms from Civil Law Countries
Post SOX * Cross-Listed	-0.3296*	-0.7545**	-0.4855**	-0.3650	-0.0760	-0.7276***	-0.1503	-0.3900*
	(-1.79)	(-2.08)	(-2.52)	(-1.21)	(-0.58)	(-2.68)	(-1.00)	(-1.78)
Post SOX	0.8388	-1.9044**	0.5543	-0.0766	0.3079	-2.0729	-0.1100	-0.0090
	(1.47)	(-2.07)	(1.30)	(-0.23)	(0.42)	(-1.12)	(-0.33)	(-0.04)
Analyst Number	0.0266	-0.0594	-0.0121	0.0260	0.2237***	0.1234**	0.1774***	0.2092***
	(0.55)	(-0.88)	(-0.26)	(0.37)	(5.30)	(2.50)	(4.54)	(3.54)
Loss	-0.0519	0.0296	-0.0717	0.0427	0.8167***	0.9751***	0.8615***	0.8645***
	(-0.55)	(0.18)	(-0.76)	(0.25)	(9.30)	(6.02)	(10.59)	(4.25)
Log(Surprise)	0.2315***	0.2466***	0.2079***	0.2963***	0.0893***	0.0811***	0.0818***	0.1021***
	(13.74)	(9.84)	(12.56)	(11.88)	(6.58)	(4.37)	(6.22)	(5.12)
Log(Firm Size)	0.0071	0.0502	0.0392	0.0076	-0.2341***	-0.1714***	-0.1865***	-0.2489***
	(0.14)	(0.59)	(0.68)	(0.12)	(-4.97)	(-2.59)	(-4.15)	(-3.61)
Leverage	0.0001	0.0038	0.0041	0.0003	-0.0010	-0.0043	-0.0006	-0.0037
	(0.03)	(1.05)	(1.08)	(0.09)	(-0.30)	(-1.30)	(-0.18)	(-1.13)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	No	No	No	No	No	No	No	No
Country-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	6633	3641	6682	3592	5827	3194	5948	3073
adj. R-sq	0.094	0.103	0.083	0.120	0.169	0.170	0.168	0.157
p-value of Wald-test comparing the coefficient of *Post SOX * Cross-Listed*	0.0215		0.5321		0.0000		0.1111	

Table 6: Robustness: Controlling for Risk-Taking

Panel A looks at the determinants of the logarithm of *Forecast Error* and Panel B at the determinants of the logarithm of *Forecast Dispersion*. Both variables are used as proxies for firm-level opaqueness. The regressions control for different proxies for risk-taking: investment (capital expenditure/total assets), beta, and price volatility. *Post SOX* is a dummy variable that takes the value 1 for the years 2005 to 2007. *Cross-Listed* is a dummy variable that takes the value 1 if a firm was continuously cross-listed in the US from end of 2000 to end of 2007. For definitions of all variables see Appendix A-1. The regressions use annual data from 2001 to 2007. Robust t-statistics are reported in parentheses. All standard errors are clustered at the firm level. Constants were included in the regressions but are not reported. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

Panel A:

	Dependent variable: log(*Forecast Error*)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post SOX * Cross-Listed	-0.3199**	-0.3245**	-0.3382**	-0.3059**	-0.3127**	-0.3278**	-0.2788*	-0.2179
	(-2.26)	(-2.30)	(-2.38)	(-2.13)	(-2.22)	(-2.31)	(-1.91)	(-1.51)
Post SOX	0.5284	0.4860	0.5357	0.5008	0.4931	0.5170	0.7511*	0.5310
	(1.28)	(1.25)	(1.29)	(1.27)	(1.17)	(1.31)	(1.76)	(1.28)
Analyst Number	-0.0191	-0.0123	-0.0199	-0.0206	-0.0187	-0.0132	-0.0271	-0.0217
	(-0.58)	(-0.33)	(-0.60)	(-0.54)	(-0.57)	(-0.36)	(-0.83)	(-0.57)
Loss	0.0283	0.0974	0.0625	0.1231	0.0239	0.0940	0.0203	0.0903
	(0.41)	(1.18)	(0.89)	(1.45)	(0.34)	(1.13)	(0.29)	(1.06)
Log(Surprise)	0.2190***	0.2249***	0.2236***	0.2277***	0.2184***	0.2249***	0.2215***	0.2254***
	(18.38)	(16.47)	(18.79)	(16.16)	(18.31)	(16.46)	(18.63)	(16.04)
Log(Firm Size)	0.0235	0.0414	0.0224	0.0425	0.0162	0.0400	-0.0022	0.0275
	(0.65)	(0.86)	(0.61)	(0.86)	(0.44)	(0.82)	(-0.06)	(0.56)
Leverage	0.0010	0.0007	0.0005	0.0005	0.0011	0.0007	0.0006	0.0005
	(0.50)	(0.29)	(0.26)	(0.22)	(0.51)	(0.32)	(0.31)	(0.22)
Investment	0.0124			0.2653	0.0035			-0.1050
	(1.24)			(0.69)	(0.30)			(-0.22)
Price Volatility		0.0053		0.0077		0.0051		0.0036
		(1.08)		(1.52)		(1.04)		(0.70)
Beta			-0.0359	-0.1697**			-0.0133	-0.0315
			(-0.92)	(-2.40)			(-0.36)	(-0.41)
Post SOX * Investment					1.2183**			1.0134
					(2.22)			(1.55)
Post SOX * Price Volatility						-0.0016		0.0094**
						(-0.44)		(2.11)
Post SOX * Beta							-0.2731***	-0.3266***
							(-5.15)	(-3.99)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	No	No	No	No	No	No	No	No
Obs.	9986	8169	10034	7821	9986	8169	10034	7821
adj. R-sq	0.111	0.122	0.113	0.121	0.112	0.122	0.117	0.125

Panel B:	Dependent variable: log(*Forecast Dispersion*)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post SOX * Cross-Listed	-0.2427**	-0.2246**	-0.2432**	-0.2079*	-0.2358**	-0.2373**	-0.2190**	-0.1691
	(-2.23)	(-2.08)	(-2.25)	(-1.90)	(-2.19)	(-2.16)	(-1.97)	(-1.51)
Post SOX	0.0041	0.0049	0.0001	0.0004	-0.0235	0.1162	0.1042	0.0947
	(0.01)	(0.02)	(0.00)	(0.00)	(-0.08)	(0.40)	(0.37)	(0.33)
Analyst Number	0.1349***	0.1350***	0.1337***	0.1395***	0.1353***	0.1325***	0.1310***	0.1378***
	(5.12)	(4.89)	(5.08)	(4.90)	(5.14)	(4.80)	(4.99)	(4.86)
Loss	0.6432***	0.6365***	0.6200***	0.6464***	0.6385***	0.6233***	0.5963***	0.6097***
	(11.03)	(9.66)	(10.51)	(9.53)	(10.97)	(9.43)	(10.16)	(9.04)
Log(Surprise)	0.0789***	0.0758***	0.0837***	0.0784***	0.0783***	0.0755***	0.0824***	0.0763***
	(8.69)	(7.99)	(9.15)	(8.00)	(8.64)	(7.96)	(9.04)	(7.87)
Log(Firm Size)	-0.1747***	-0.1779***	-0.1811***	-0.1735***	-0.1784***	-0.1841***	-0.1943***	-0.1886***
	(-5.62)	(-5.03)	(-5.76)	(-4.77)	(-5.71)	(-5.18)	(-6.04)	(-5.14)
Leverage	-0.0009	-0.0006	-0.0010	-0.0008	-0.0009	-0.0004	-0.0010	-0.0007
	(-0.47)	(-0.26)	(-0.54)	(-0.37)	(-0.48)	(-0.17)	(-0.49)	(-0.31)
Investment	0.0080			-0.1015	-0.3290			-0.5215
	(0.03)			(-0.33)	(-0.92)			(-1.27)
Price Volatility		0.0137***		0.0138***		0.0132***		0.0108***
		(3.77)		(3.69)		(3.66)		(2.82)
Beta			0.0085	0.0144			0.0254	0.1042*
			(0.34)	(0.28)			(0.84)	(1.79)
Post SOX * Investment					1.0088**			0.9376*
					(2.07)			(1.94)
Post SOX * Price Volatility						-0.0056*		0.0011
						(-1.85)		(0.26)
Post SOX * Beta							-0.1323***	-0.1892***
							(-3.04)	(-2.82)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	No	No	No	No	No	No	No	No
Obs.	8777	7247	8807	6940	8777	7247	8807	6940
adj. R-sq	0.173	0.193	0.174	0.196	0.174	0.194	0.177	0.201

Table 7: Instrumental Variable 2SLS Estimates

This table presents coefficients of the interaction dummy (*Post SOX * Cross-Listed*) using an IV 2SLS estimation approach. Panel A reports estimates of the first stage and Panel B reports estimates from IV 2SLS regressions. The endogenous variables are *Cross-Listed* and *Post SOX * Cross-Listed*. We instrument the endogenous variables with *Cross-Listed in 2000* and *Post SOX * Cross-Listed in 2000*. *Cross-Listed* is a dummy variable taking the value one if and only if a firm was cross-listed in the US from end of 2000 to end of 2007. *Cross-Listed in 2000* is a dummy variable taking the value one if and only if a firm was cross-listed in the US at the end of 2000. All regressions include *Analyst Number*, *Loss*, *Log(Surprise)*, *Log(Firm Size)*, and *Leverage* as controls. The regressions in columns 3 and 4 further contain the proxies for risk-taking (*Investment*, *Price Volatility*, *Beta*). Results are similar if we include firm fixed effects. For definitions of all variables see Appendix A-1. Robust t-statistics are reported in parentheses. All standard errors are clustered at the firm level. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

Panel A: First Stage Regressions

	Two Endogenous Variables: Cross-Listed, Post SOX * Cross-Listed			
Dependent variable:	*Cross-Listed*	*Post SOX * Cross-Listed*	*Cross-Listed*	*Post SOX * Cross-Listed*
	(1)	(2)	(3)	(4)
Post SOX * Cross-Listed in 2000	0.0724***	0.5169***	0.0448**	0.5357***
	(4.21)	(12.50)	(2.29)	(12.80)
Cross-Listed in 2000	0.3787***	-0.0110***	0.4112***	-0.0152***
	(10.08)	(-4.30)	(9.84)	(-4.56)
Post SOX	-0.0094***	-0.0034***	-0.0094***	-0.0027***
	(-3.93)	(-4.58)	(-3.48)	(-2.85)
Controls	Yes	Yes	Yes	Yes
Risk-Taking Controls	No	No	Yes	Yes
F-Statistic	24.57	25.99	20.23	22.7
Obs.	10278	10278	7824	7824
adj. R-sq	0.374	0.510	0.408	0.525

Panel B: IV 2SLS Regressions

Dependent variable:	log(*Forecast Error*)	log(*Forecast Dispersion*)	log(*Forecast Error*)	log(*Forecast Dispersion*)
	IV 2SLS	IV 2SLS	IV 2SLS	IV 2SLS
	(1)	(2)	(3)	(4)
Post SOX * Cross-Listed	-0.4115**	-0.2637**	-0.4316**	-0.2692**
	(-2.25)	(-2.05)	(-2.36)	(-2.06)
Cross-Listed	0.6980***	0.4655***	0.5529***	0.3917***
	(3.71)	(3.31)	(2.96)	(2.70)
Post SOX	-0.3109***	-0.5465***	-0.3083***	-0.5774***
	(-4.11)	(-8.52)	(-3.73)	(-8.32)
Controls	Yes	Yes	Yes	Yes
Risk-Taking Controls	No	No	Yes	Yes
Country-Year Fixed Effects	Yes	Yes	Yes	Yes
Obs.	10274	9021	7821	6940
adj. R-sq	0.282	0.318	0.292	0.342

Table 8: Further Robustness Checks

This table presents coefficients of the interaction dummy (*Post SOX * Cross-Listed*) for various regression specifications. In the first robustness check, the treatment group consists of firms that were cross-listed in the US at the end of 2000 (regardless of whether they delisted at a later point in time). In the second robustness check, the treatment group consists of firms that were cross-listed in the US from end of 2000 to end of 2006. In the third robustness check, the *Post SOX* dummy takes the value 1 for the years 2006 and 2007 and 0 otherwise. In the fourth robustness check, the opaqueness measures are not winsorized at 5%. In the fifth robustness check, we restrict our sample to firms with a market capitalization above USD 75 million. In the sixth robustness check, we exclude firms from the UK. In the seventh robustness check, we exclude firms from The Netherlands. In the eighth robustness check, we remove the time series dimension by aggregating the data into a pre- and post-SOX period (Bertrand et al. 2004). All regressions include *Analyst Number*, *Loss*, *Log(Surprise)*, *Log(Firm Size)*, and *Leverage* as controls. The regressions include firm fixed effects as well as country-year fixed effects (except for the eighth robustness check where we use country fixed effects). For definitions of all variables see Appendix A-1. Robust t-statistics are reported in parentheses. All standard errors are clustered at the firm level. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

Reported coefficients for: Post SOX * Cross-Listed	Dependent variable:		
	Log(Forecast Error)	Log(Forecast Dispersion)	Number Treatment Firms
	(1)	(2)	
1. Treatment Status = Cross-listed end of 2000	-0.2265**	-0.1697**	N=189
	(-2.10)	(-2.14)	
2. Treatment Status = Cross-listed end of 2000 to end of 2006	-0.2812**	-0.2206**	N=117
	(-2.38)	(-2.50)	
3. Post SOX period = 2006 to 2007	-0.3739**	-0.2557**	N=76
	(-2.54)	(-2.40)	
4. Opaqueness measures not winsorized	-0.4414***	-0.2473*	N=76
	(-2.62)	(-1.95)	
6. Size>USD 75million	-0.3291**	-0.2419**	N=76
	(-2.32)	(-2.26)	
6. Without firms from UK	-0.3685**	-0.1541	N=52
	(-2.32)	(-1.32)	
7. Without firms from The Netherlands	-0.3574**	-0.2520**	N=62
	(-2.28)	(-2.17)	
8. Ignoring Time Series Dimension (Bertrand et al. 2004)	-0.2111*	-0.0834	N=76
	(-1.90)	(-0.98)	

Table 9: Textual Analysis of Annual Reports

This table provides statistics from a textual analysis of firms' annual reports. We analyze how the informational content of annual reports changed over time, according to three quantitative and five qualitative measures. We use 2002 reports for the "before SOX" period and 2007 reports for the "after SOX" period, and compare cross-listed firms (treatment group) with a matched sample of non-cross-listed firms (control group). The control firms were selected based on a country, industry, and size match from the full set of non-cross-listed EU-15 firms. We were able to find such matches and the required annual reports for 50 of the 76 cross-listed firms that were continuously cross-listed over the sample period.

	Cross-Listed			*Non-Cross-Listed*			
Measures	*2002*	*2007*	*Change (2007-2002)*	*2002*	*2007*	*Change (2007-2002)*	*Diff-in-Diffs*
Number of pages of the annual report	118	177	58	102	148	47	11
Number of words in the annual report	55236	92130	36894	37771	64533	26762	10132
Number of sentences with forward looking information	48	85	36	26	51	25	11
Discussion of future risks (dummy)	90%	98%	8%	51%	88%	37%	-29%
Discussion of future opportunities (dummy)	68%	94%	26%	71%	82%	11%	15%
Statement on expected future earnings growth (dummy)	74%	90%	16%	71%	84%	13%	3%
Information about unusual or nonrecurring events and their past effect on the company (dummy)	52%	64%	12%	86%	84%	-2%	14%
Comparison actual vs. expected opportunities, risks, and plans (dummy)	28%	48%	20%	12%	28%	16%	4%

Figure 1: Cross-Listed Firms and Delistings

This figure presents an overview of cross-listings and delistings in our sample of EU-15 firms. Cross-Listed in 2000 (column 1) reports the number of sample firms that were cross-listed in the US at the end of the year 2000. Columns 2, 3, and 4 report the number of sample firms that delisted in 2001-2005, 2006, and 2007, respectively. Column 5 reports the number of sample firms that were continuously cross-listed in the US from end of 2000 to end of 2007.

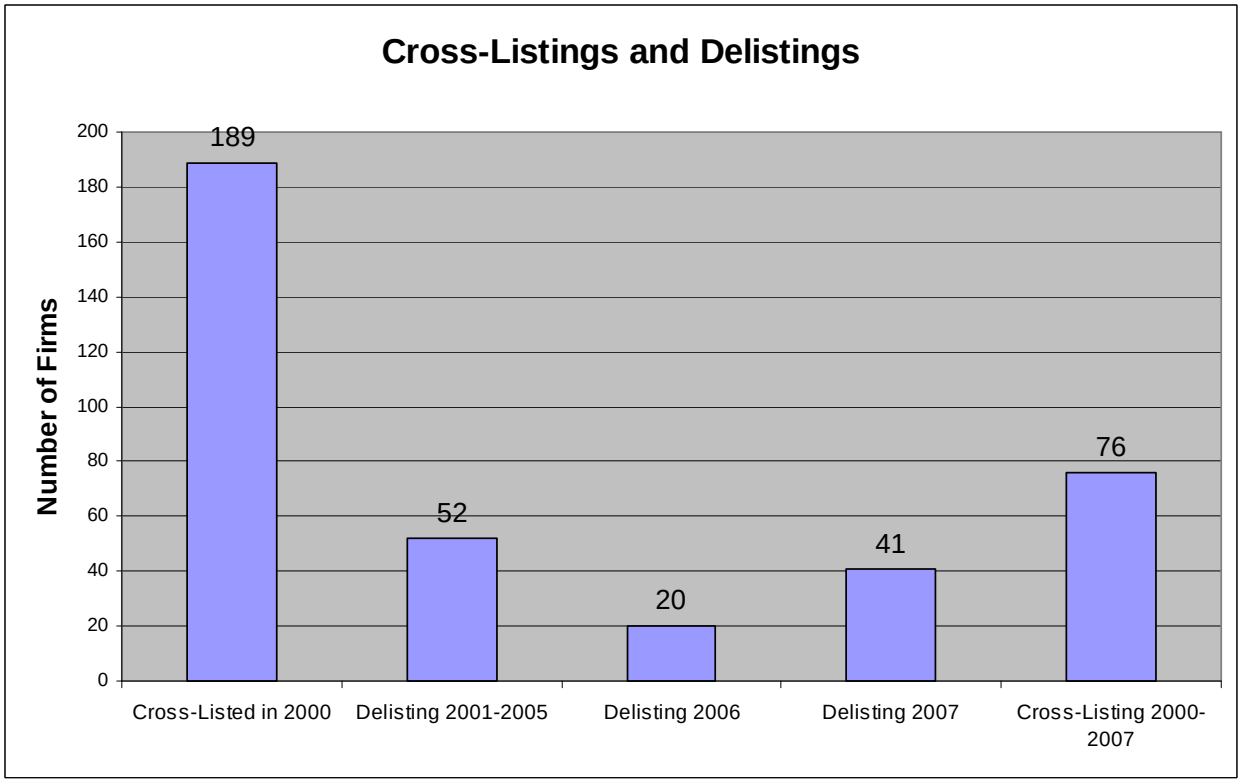
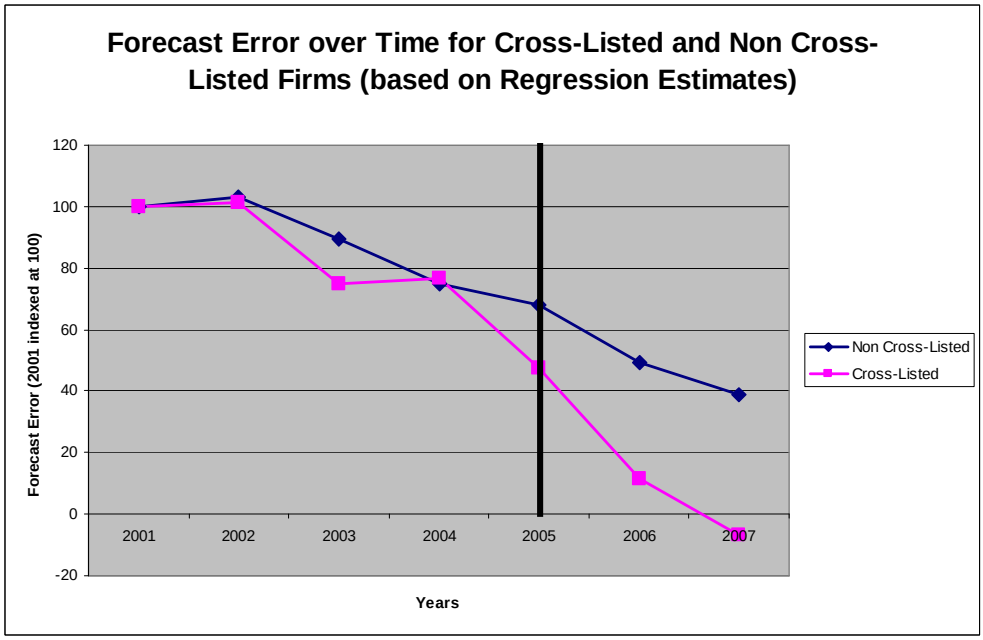


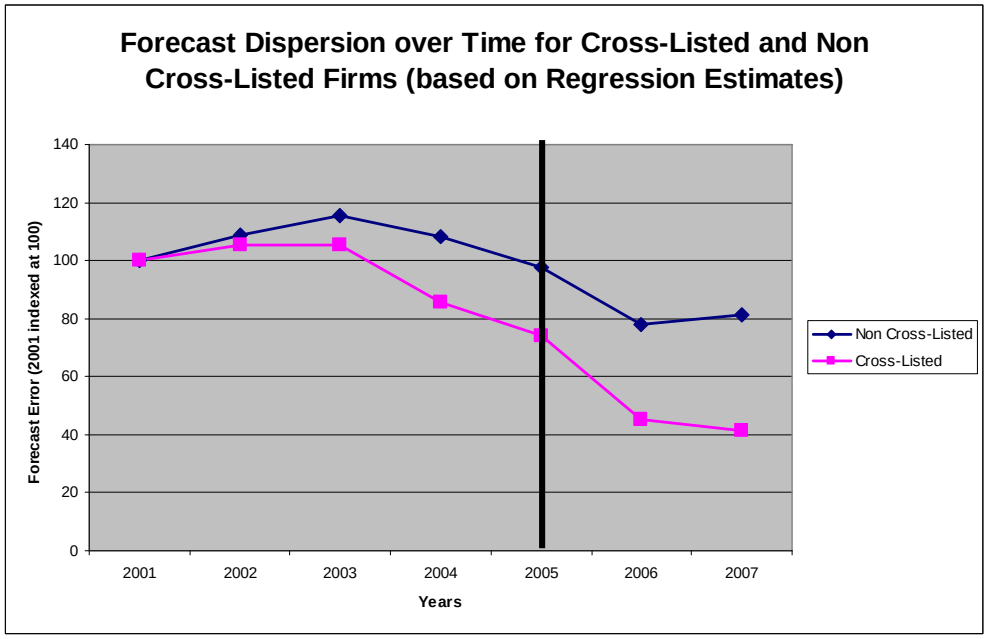
Figure 2: Evolution of Forecast Error and Forecast Dispersion

The figures depict the evolution of *Forecast Error* (Panel A) and *Forecast Dispersion* (Panel B) over the period 2001-2007. Both variables are used as proxies for firm-level opaqueness. *Forecast Error* is the absolute value of the difference between the average EPS forecast for a given firm-year and actual EPS, divided by actual EPS. *Forecast Dispersion* is the absolute value of the coefficient of variation of EPS forecasts for a given firm-year. The figures report *Forecast Error* and *Forecast Dispersion* separately for cross-listed firms and for the control firms. A firm is defined to be cross-listed if it was continuously cross-listed over the sample period. The figures are obtained from regression estimates of the logarithm of *Forecast Error* and *Forecast Dispersion* on a set of year dummies, a cross-listing dummy variable, interactions terms of the year dummies with the cross-listing dummy, and a set of controls. The regressions include firm fixed-effects. *Forecast Error* and *Forecast Dispersion* are both indexed at 100 in 2001.

Panel A:



Panel B:



Appendix A-1: Definition of Variables

This table presents definitions of the variables used in the empirical analysis and reports their respective data sources.

Variable	Definition	Source
Forecast Error	Absolute value of the difference between the average EPS estimate for a given firm-year and the actually reported EPS, divided by the actually reported EPS.	IBES
Forecast Dispersion	Absolute value of the standard deviation of the EPS estimate for a given firm-year divided by the average EPS estimate	IBES
Post SOX	Dummy variable which takes the value 1 for the years 2005-2007	Self-constructed
Cross-Listed	Dummy variable which takes the value 1 if a firm was continuously cross-listed in the US from end of 2000 to end of 2007	SEC
Cross-Listed in 2000	Dummy variable which takes the value 1 if a firm was cross-listed in the US at the of 2000	
Surprise	Absolute value of the difference between the actually reported EBS in t minus the actually reported EPS in t-1, divided by the actually reported EPS in t-1	IBES
Loss	Dummy variable which takes the value 1 if earnings are negative	Datastream
Analyst Number	Number of analysts covering a company	IBES
Quarter Report	Dummy value which takes the value 1 if a company reports quarterly earnings	IBES
Firm Size	Market capitalization in EUR	Datastream
Leverage	Book value of total assets divided by book value of common equity	Datastream
Common Law	Dummy variable which takes the value 1 if a company is domiciled in a common law country	LLSV (1998)
Post SOX 2006	Dummy variable which takes the value 1 for the years 2006-2007	Self-constructed
Delisting 2006 (2007)	Dummy variable which takes the value 1 if a firm terminated its US cross-listing in the year 2006 (2007)	SEC
Beta	Equity beta of a firm, calculated based on month-end stock prices over a period of 2 years	Datastream
Price Volatility	Stock price volatility in %	Datastream
Investment	Capital expenditures divided by total assets	Datastream

Appendix A-2: Opaqueness and Delistings

This table provides logit regressions explaining delisting decisions in the years 2006 and 2007, respectively. *Forecast Error* is the absolute value of the difference between the average EPS forecast for a given firm-year and actual EPS, divided by actual EPS. *Forecast Dispersion* is the absolute value of the coefficient of variation of EPS forecasts for a given firm-year. Robust t-statistics are reported in parentheses. *, **, and *** indicate significance at 10%, 5%, and 1%, respectively.

	Dep. Variable: *Delisting 2006*				Dep. Variable: *Delisting 2007*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
log(Forecast Error)	0.3603**			0.2115	-0.0446			0.2450
	(1.97)			(0.67)	(-0.29)			(0.91)
log(Forecast Dispersion)		0.7743***		0.8356**		-0.2039		-0.5160
		(3.04)		(2.05)		(-0.93)		(-1.53)
Analyst Number			-0.0558	-0.0484			0.0725**	0.0618
			(-1.33)	(-1.21)			(2.02)	(1.51)
Log(Surprise)			-0.3296	-0.5654			-0.2222	-0.2198
			(-1.17)	(-1.42)			(-0.50)	(-0.48)
Loss			0.3610	-0.2686			-0.7776	-0.5249
			(0.34)	(-0.23)			(-0.65)	(-0.44)
Log(Firm Size)			0.0000	0.0000			-0.0000**	-0.0000**
			(0.98)	(1.08)			(-2.27)	(-2.15)
Leverage			-0.0301	-0.0228			-0.0626**	-0.0629**
			(-1.25)	(-1.39)			(-2.08)	(-2.01)
Constant	-1.7513***	-0.8985	-1.6362***	0.5675	-1.5885***	-1.9135***	-1.3933**	-1.9102*
	(-3.98)	(-1.52)	(-2.67)	(0.57)	(-3.62)	(-3.07)	(-2.36)	(-1.79)
Obs.	146	135	144	134	124	114	124	114
Pseudo R-sq.	0.024	0.081	0.043	0.133	0.001	0.005	0.130	0.135