What Makes the Bonding Stick? A Natural Experiment Testing the Legal Bonding Hypothesis

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Abstract

We use a US Supreme Court case, Morrison v. National Australia Bank (2010), as a natural experiment to test the legal bonding hypothesis. By decreasing the potential liability of US-listed foreign firms, particularly due to class action lawsuits, Morrison arguably eroded their legal bonding to compliance with disclosure duties. Nevertheless, we find evidence of an increase or insignificant change in share values. Tests of longer-run effects of the legal event indicate that foreign firms' disclosure quality and likelihood of facing enforcement actions remained stable, as did investors' revealed preferences for trading on US markets. These results go against the legal bonding hypothesis but are consistent with reputational bonding and with market-based accounts of US cross-listing. Our results may contribute to ongoing debate about civil enforcement of securities laws through class actions.

Keywords: bonding, class actions, cross-listing, corporate governance, civil liability, reputation

JEL Classifications: G15, G18, G38

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What Makes the Bonding Stick? A Natural Experiment Testing the Legal Bonding Hypothesis[*]

1. Introduction

Foreign firms that list on US stock exchanges experience a range of positive outcomes, but the mechanisms that produce these outcomes are still debated.[1] Specifically, it is unclear whether US exchanges attract firms because a US cross-listing signals a credible commitment to full disclosure by making a firm subject to US legal enforcement, an argument known as the *legal bonding hypothesis* (Stulz, 1999; Coffee, 1999). Alternatively, a US cross-listing may facilitate informal reputation building, an argument known as the *reputational bonding hypothesis* (Siegel, 2005). Both mechanisms are theoretically plausible, and they could operate in tandem, making it empirically difficult to disentangle their effects.[2]

In *Morrison v. National Australia Bank Ltd.*, argued and decided in 2010, the US Supreme Court overturned forty years of precedent to dramatically shrink the legal rights of investors in non-US firms that cross-list on US markets ("foreign private issuers" or FPIs). Investors in such firms who trade their securities on non-US markets lost their right to file or join a US class action in case of fraud. By restricting such legal enforcement to securities traded in the United States, *Morrison* limited firms' potential liability in class action lawsuits, which is

[*] We thank Chris Allen and Eyal Arazi for research assistance. We thank the Securities and Exchange Commission staff and the attorneys from the different sides who gave generously of their time in answering our questions. For helpful comments we thank an anonymous referee, Kee-Hong Bae, Bobby Bartlett, Bernard Black, Steve Choi, Jim Cox, Robert Jackson, Alon Klement, Tim Lohse, Roni Michaely, Graham Partington, and Ed Rock, as well as participants in seminars and conferences at University of New South Wales School of Business Research Workshop; University of Sydney School of Business Research Workshop; Corporate Governance in Emerging Markets, Infinity Conference on International Finance, Summer Finance at IDC, American Law and Economics, European Law and Economics, Boston University, City University of Hong Kong, Chinese University of Hong Kong, Hong Kong University, and Hong Kong University of Science and Technology. Any errors are our responsibility.

[1] For a comprehensive survey, see Karolyi (2006). Karolyi (2012) provides a review of cross-listing and bonding. See, in particular, Lel and Miller (2008) (top-management turnover), King and Segal (2009) (investor recognition), Hail and Leuz (2009) (cost of capital), Ball, Hail, and Vasvari (2013) (price of debt), Frésard and Salva (2010) (value of excess cash).

[2] See also Bushee and Leuz (2005), on mandatory disclosure by over-the-counter bulletin-board firms, and Doidge, Karolyi, and Stulz (2010) as well as Fernandes, Lel, and Miller (2010) on opting out of stringent disclosure.

routinely invoked as a central component of legal bonding. Such a massive and unanticipated change in the law of securities fraud is nearly unprecedented.[3]

We exploit this legal event to test the legal bonding hypothesis. The exclusion of non-US trades makes for smaller classes of eligible plaintiffs, lower expected damage awards and settlements, and lower attorney fees, all factors that fuel class actions (Coates, 2015). If exposure to US class actions facilitates value-creating legal bonding, the shrinkage of this type of civil liability should elicit negative market reactions. These reactions should be more intense for firms with greater non-US trading volume and firms with weaker home-country investor protection. Such firms presumably lose more of the advantages of using the US civil liability regime to credibly commit to compliance.

We first examine the stock returns of a comprehensive sample of US-listed firms in both US and home markets around the date of oral argument, when the Court's new stance emerged. Contrary to the prediction of the legal bonding hypothesis, we find significantly positive or insignificant abnormal returns. The abnormal returns are particularly positive for firms with above-median non-US capitalization—that is, the very firms that the Court most extensively *excluded* from the US anti-fraud liability regime. Moreover, the abnormal returns exhibit significant negative relations with certain measures of home-country institutional quality (and no significant relations with others). In other words, the weaker the institutions in firms' home markets, the more *positively* markets reacted to the dilution of US anti-fraud liability. Several tests of longer-term effects on reporting practices show no change in foreign firms' tendency to engage in earnings management or to switch auditors, which could indicate tensions over corporate candor. We also fail to observe more frequent earnings surprises, which would suggest

[3] Legislative reforms are preceded by lengthy public comment and hearings, and court decisions of such magnitude are exceedingly rare. See Cox and Thomas (2009) for a survey.

less transparent and informative disclosure. We do observe a slight *decrease* in the incidence of class actions against *Morrison*-affected foreign firms and no change in the frequency of SEC enforcement actions. Finally, we find no changes in the bid-ask spread, as a measure of adverse-selection risk, contrary to the prediction that *Morrison* should lead to higher spreads that reflect greater opacity.

Analyses at the investor level of price and return differentials and of trading volumes across markets during 2010 suggest that investors did not change their trading patterns by shifting trades to US markets to secure the advantages of the US enforcement umbrella.

This evidence blends two separate findings and their implications. First, the non-negative market reaction calls into question whether the US civil liability regime in fact facilitates legal bonding, and thus whether legal bonding is a primary motivation for cross-listing. The benefits of exposure to the type of US enforcement affected by *Morrison* appear, on average, to be no greater than the costs. Second, the evidence may shed light more broadly on the use of US-style class actions as a private enforcement mechanism against securities fraud. Our natural experiment focuses exclusively on cross-listed firms, but it can also be viewed as a clean laboratory in which to assess the efficacy of private enforcement in general. A US-style securities-fraud class action regime could be viewed as a regulatory burden for firms. Viewed through this lens, the *Morrison* Court decision relieved cross-listed firms from burdensome legal exposure; the positive link between market reactions and dilution of this regime is consistent with this idea. These results warrant further inquiry into the merits of the prevailing US civil liability regime for US firms and the importance of public enforcement.

The paper proceeds as follows. Part 2 explains the theory and reviews the related literature. Part 3 explicates the institutional setting, the proceedings in *Morrison*, and its

aftermath. Part 4 summarizes the hypotheses. Part 5 describes the data. Part 6 presents the results. Part 7 concludes.

2. The literature

2.1. Bonding and civil liability

The cross-listing-as-bonding argument draws on the signaling literature in holding that non-US firms can supplant or supplement deficient home-country institutions by cross-listing their securities on the better-regulated markets of the United States (Karolyi, 2012; Diamond, 1991; Klein and Leffler, 1981; Karpoff, 2012). US law specifies both disclosure duties and provides the legal mechanisms to enforce them. Ensuring firms' compliance with these duties is the key challenge here: the legal bonding hypothesis hinges on formal enforcement for credible commitment to full disclosure; by contrast, market-based mechanisms support voluntary disclosure (Beyer et al., 2010; Hollander, Pronk, and Roellfsen, 2010) as well as building a reputation for commitment to transparency and compliance. Siegel (2005) theorizes and shows empirically that cross-listed firms can use disclosure requirements and the extra scrutiny of market analysts to build up reputational assets even where formal law enforcement is weak. Bank debt contracting operates similarly, directly or indirectly (Graham, Li, and Qiu, 2008; Lin et al., 2013; Armstrong, Guay, and Weber, 2010). An alternative view holds that insiders who make cross-listing decisions are actually looking for weaker rules and weaker enforcement to exploit (Licht, 2003; see also Foley et al., 2014). These hypotheses are not mutually exclusive. As Stulz (2009, p. 349) notes, "Some firms will choose stronger securities laws than those of the country in which they are located and some firms will do the opposite."

Securities regulation regimes usually rely on deterrence to curb fraud. Coffee (2002, p. 1788) argued that the market appreciates civil liability as "a powerful engine of private

enforcement (e.g., the contingent fee-motivated plaintiffs bar) [that] stands ready to enforce US legal rules." Both public and private enforcement mechanisms appear to matter. Jackson and Roe (2009) show that the size of regulatory staffs and budgets affects financial-market outcomes. La Porta, Lopez-de-Silanes, and Shleifer (2006) specify that the rules that actually "work" in securities laws are those that govern disclosure and civil litigation (but not class actions or contingent fees). But scholars have questioned the merits of secondary-market civil liability as a mechanism to enforce disclosure duties. The "circularity argument" holds that, because the judgment or settlement award is paid either by the company or by its insurer, investors end up "compensating" investors in the same firm (Cox, 1997; Coffee, 2006). Insiders who commit fraud rarely have to pay anything directly, because D&O liability insurance effectively eliminates their liability risk; meanwhile, attorneys pocket approximately half the direct costs paid by the firm (Siegel, 2005; Klausner, Hegland, and Goforth, 2013; Caskey, 2014). That insurers derive income from public firms by providing insurance products in addition to D&O insurance might explain why, at certain companies, multiple generations of managers have repeatedly violated the securities laws (Baker and Griffith, 2011). Habib et al. (2014) thus conclude that "the relation between litigation risk and financial reporting quality proxies is often an open research question."[4]

Policy-makers, too, differ on the preferred approach to legal enforcement of disclosure duties. A 1995 reform of the civil liability regime yielded mixed results, and the general desirability of class-action-based antifraud liability remains debatable (Cox and Thomas, 2009). Several other countries have adopted some type of class action, and a few have implemented American-style class actions (Hensler, 2011). Governments that responded to *Morrison*

[4] Habib et al. (2014) note that financial reporting quality is a multidimensional concept, leading studies to consider the effect of alternative proxies besides accounting restatements.

endorsed different approaches to implementing civil liability for securities fraud, however. The British government voiced fundamental disagreement "as to the desirability and appropriateness of even having a private right of action against an issuer for securities fraud," citing the circularity problem and high costs (SEC, 2012, p. 24).

2.2. Related empirical literature

Whether the US legal regime promotes or discourages bonding is ambiguous (Doidge, Karolyi, Lins, Miller, and Stulz, 2009; Doidge, Karolyi, and Stulz, 2010). Much of the evidence is consistent with both legal bonding and reputational bonding (see, e.g., Frésard and Salva, 2010; Ammer, Holland, Smith, and Warnock, 2008; King and Segal, 2009). Research on voluntary disclosure provides evidence consistent with reputational bonding.[5] Siegel (2005) thus has advanced a theory and pursued a natural experiment to identify the importance of reputational bonding over legal bonding. Doidge et al. (2009, p. 428) argue, in contrast, that "direct U.S. securities laws and enforcement are more important constraints in the extraction of private benefits than is the scrutiny of financial analysts" (that is, reputation) (Fernandes, Lel, and Miller, 2010; Doidge, Karolyi, and Stulz, 2010).[6] Some work also expresses disenchantment with cross-listing as a value-increasing transaction (e.g., Gozzi, Levine, and Scmukler, 2008; Sarkissian and Schill, 2008; Iliev, Miller, and Roth, 2014). Leuz and Wysocki (2016, p. 577) observe that "the sources of the cross-listing effects are still unclear. . . . It is plausible that the documented effects stem from a combination of legal and market forces." Karolyi (2012, p. 524)

[5] See Shi, Magnan, and Kim (2012); Hope, Kang, and Kim (2013); Shi, Kim, and Magnan (2012). These studies are noteworthy because they consider earnings guidance. Unlike other voluntary disclosures that could nonetheless be in breach of U.S. anti-fraud laws, this type of forward-looking disclosure is essentially exempt from anti-fraud liability under safe-harbor provisions of the Private Securities Litigation Reform Act of 1995 and therefore cannot serve for legal bonding. Beyond the scope of this study, Naughton et al. (2014) present puzzling evidence that despite the lower litigation risk following *Morrison*, U.S.-listed FPIs decreased their voluntary disclosure, which calls for further research.

[6] In a study that appeared after a preliminary version of this paper came out, Gagnon and Karolyi (2011) fail to find in their sample a significant change in firms' market value on the oral argument event in *Morrison*, in contrast to what the legal bonding hypothesis implies.

thus observes: "A proper verdict about the bonding hypothesis, especially of its purer 'legal' form, has not yet been fully rendered. I think a more complete understanding of the enforcement mechanisms around the world, their financial needs as inputs and the full scope of legal outcomes is still needed."

Substantial empirical work shows that enforcement imposes adverse consequences on firms and their leaders only if the corporate misconduct involved significant stakeholders, such as financial misrepresentation to investors or consumer fraud (e.g., Karpoff, Lee, and Martin, 2008; Armour, Mayer, and Polo, 2015; Lel and Miller, 2014; see Karpoff, 2012 for a survey). Public enforcement by the SEC or its British counterpart appears to be pivotal in triggering these effects. Partialling out plausible legal costs following the discovery of financial reporting violations still leaves a substantial decrease in share value: this decrease represents a reputational loss (Karpoff, 2012).

Several studies emphasize enforcement mechanisms as necessary to overcome insiders' inclination to hide or delay bad news (Skinner, 1994), though little of the empirical literature distinguishes between public and private enforcement (but see La Porta et al., 2006). Daske, Hail, Leuz, and Verdi (2008) argue that capital-market benefits to more transparent firms flow only to firms from countries where the rule of law prevails. Christensen, Hail, and Leuz (2016) also find that the beneficial effects of market abuse and transparency regulation depend on implementation and public enforcement (see also Bushman and Piotroski, 2006; DeFond, Hung, and Trezevant, 2007; McTier and Wald, 2011). In a study of earnings management around the world, Leuz, Nanda, and Wysocki (2003) have found a significant negative relation between investor protection through the legal system and earnings management—that is, the practice of obfuscating financial disclosure to mislead certain stakeholders about firm performance (Healy

and Wahlen, 1999). In contrast, introducing US-like class actions in Canada met with negative market reactions (Willis, 2012). US firms that have experienced securities litigation have reduced their disclosure to investors (Rogers and Buskirk, 2009).

This study also draws on research about the relations between market trading and corporate governance (see Healy and Palepu, 2001, for a survey). We use the bid-ask spread as a measure of adverse selection risk due to disclosure quality. Prior research has shown that the spread is narrower for firms required to adhere to more stringent disclosure requirements and to better corporate governance in general (Leuz and Verrecchia, 2000; Chung, Elder, and Kim 2010), and for firms based in countries with better institutions (e.g., Chung, 2006; Eleswarapu and Venkataraman, 2006).

3. Legal liability before and after Morrison

The centerpiece of the US antifraud regime in the secondary market is SEC Rule 10b-5, implementing Section 10(b) of the Securities and Exchange Act of 1934 (SEA), which prohibits securities fraud (See Buxbaum, 2007; Painter, 2011, for more detail). The SEA does not explicitly provide for civil liability, and is silent about its extraterritorial reach. The US Supreme Court nonetheless held that Section 10(b) implies civil liability for securities fraud. Later the Court adopted the fraud-on-the-market doctrine, which allows numerous investors' suits to be grouped into a single class action. Few other countries recognize this doctrine, and class actions are much less expansive elsewhere, significantly limiting exposure to civil liability outside the United States. Since the 1960s US district courts have used tests, known as *conduct and effects tests*, to decide whether to apply US securities law when foreign elements are involved. The former test recognizes extraterritorial jurisdiction even if only a portion of the illegal conduct (e.g., fraud) occurred within US territory; the latter recognizes jurisdiction when the illegal

conduct occurred entirely abroad but exerted significant adverse effects within the United States. Both tests are fact-intensive and thus inevitably somewhat vague, but they have become well established in all federal circuits (see Buxbaum, 2007, for a review).

The SEC has always insisted that it can assert its jurisdiction extraterritorially, but in practice it has adopted a more reserved stance. Though an effective regulator in principle (Lohse, Pascalau, and Thomann, 2014; Brown, Tian, and Tucker, 2014), the SEC has promulgated more lenient regulations for foreign firms and granted them exemptions from certain corporate governance requirements (Licht, 2003; Li 2014; Shnitser, 2010). The SEC has also taken a relatively light approach to punishment of foreign firms and their insiders (Siegel, 2005). Whether this enforcement approach has persisted in more recent years is open to debate (compare Shnitser, 2010; Silvers, 2016).

Against this backdrop, *Morrison* involved an Australian bank whose common shares were trading in Australia and several other countries and American Depositary Receipts (ADRs) trading in the United States. The fraud took place within a wholly-owned Florida subsidiary but was communicated to the market by the bank in Australia. The media began to discuss the impending hearing on March 26, 2010.[7] Oral argument before the Supreme Court on March 29 lasted from 11:07 a.m. until 12:06 p.m. As the session proceeded, it became apparent that the justices were keen on curbing foreign access to US courts: a surprising convergence of views emerged among the justices, characterized by hostility toward the conduct and effects tests and

[7] In the widely-followed Conglomerate blog, Buxbaum (2010) emphasized that "the question the case presents is a more general one: how to define the scope of application of U.S. securities law in cases with foreign elements" (see also (Denniston (2010a)). Describing the case as "one of the most keenly-awaited of the year", the Times of London said: "The Supreme Court will on Monday hear a case that threatens to scare foreign companies from investing in America by hugely expanding overseas investors' rights to bring multi-million dollar securities actions in the US." A blogger on the *Wall Street Journal* Blogs wrote: "We can't remember a case about jurisdiction that's generated such feverish interest as the one to be argued Monday at the U.S. Supreme Court" (Jones, 2010).

support for an entirely new approach.[8] Media coverage pointed out that, rather than exploring ways to clarify forty years of precedent, the justices opted to replace the conduct and effects tests with a flat prohibition on foreign investors' use of the US legal system with regard to foreign firms.[9]

The Court's written opinion implemented the approach that had dominated the oral argument. The majority opinion adopted a new "transaction test" whereby US law applies only to transactions in the United States involving securities listed on a US stock exchange. Given the discussion during oral argument, the decision was not a surprise.[10] The decision was publicized on June 24, 2010; within less than 24 hours, on June 25, a congressional conference committee approved the final version of the Dodd-Frank Wall Street Reform and Consumer Protection Act of 2010 (DFA), which provided that US courts have jurisdiction over public enforcement of the Securities Acts by the SEC and the Department of Justice. In a joint letter to the SEC, twenty-six pension funds summarized *Morrison*'s effect on civil liability as follows:

> Stated plainly, *Morrison* and its progeny have stripped US investors of nearly all of the private rights and protections against fraud by foreign issuers previously

[8] Justice Breyer and Justice Kennedy entertained a theory of a purely territorial, exchange-based test (Morrison Transcript (2010), pp. 5-9), while Justice Ginsburg from the more liberal wing noted that the case "has 'Australia' written all over it" (p. 5). Justice Scalia explicitly stated: "We don't want the determination of whether there has been a misrepresentation on the Australian exchange and whether Australian purchasers relied upon that misrepresentation to be determined by an American court" (p. 16). Chief Justice Roberts complained that "there are a lot of moving parts in that [conduct] test. You know, significant conduct, material, you require it to have a direct causal relationship. Doesn't the complication of that kind of defeat the whole purpose?" (p. 41). Associated Press reported soon after the session, "none of the justices appeared to accept the investors' argument." (Sherman, 2010).

[9] Shortly after the oral argument concluded, at 12:28 pm, the leading SCOTUSblog posted an analysis titled "Curb on securities suits?" that said: "U.S. Supreme Court on Monday explored ways to sharply limit or perhaps even forbid private securities fraud lawsuits in U.S. courts that might intrude on foreign governments' powers to police their own stock markets. Little sentiment was expressed on the bench in favor of allowing foreign investors to come to America…" (Denniston, 2010b). At 1:37 pm, the Associated Press published its report saying: "The Supreme Court indicated Monday it could prohibit foreign investors from using U.S. securities law and American courts to sue a foreign bank for fraud." (Sherman, 2010). This report appeared later on that day in major channels such as Fox News and the Wall Street Journal.

[10] The Court's decision was unanimous (8-0, with Sotomayor recused). Justices Stevens and Ginsburg filed a concurring opinion, such that the Court's endorsing of the territorial "transaction test" was by a wide 6-2 margin. We are grateful to an anonymous referee for suggesting this point.

> afforded by federal securities laws. It cannot be overstated that, under *Morrison*, companies listed on a foreign exchange can commit financial fraud within United States borders but investors have virtually no private recourse in United States courts (Smith et al., 2012).

Strictly speaking, the case pertained only to private enforcement—which, as we have seen, some commentators characterize as the primary legal bonding mechanism. Much of the oral argument, however, revolved around general principles of law that are equally applicable to public enforcement. Legal commentators pointed out that the Court's approach could extend to public enforcement as well as civil liability (Painter, 2011; Coates, 2015). Indeed, the SEC was already concerned about its public enforcement powers before publication of the *Morrison* opinion, and later officially acknowledged that *Morrison* had curbed its public enforcement authority. Subsequent court rulings held that *Morrison* had in fact affected the SEC's extraterritorial public enforcement prior to the DFA. Moreover, these and other legal developments have extended *Morrison* beyond the context of securities regulation.[11] It is thus unclear whether stock-price reactions to the oral-argument event reflected market participants' assessments only of the imminent dilution of foreign issuers' civil liability, or also of the exclusion of non-US conduct from civil liability and public enforcement. The latter interpretation was more speculative at the time of the event (though later developments could vindicate it). Our empirical analyses thus focus conservatively on class-action-based private enforcement.

4. Hypotheses

The legal bonding hypothesis implies that markets will react negatively to blunting the threat of private enforcement, because doing so erodes the credibility of firms' disclosures. After

[11] We discuss these developments in more detail in an online Appendix.

Morrison, foreign firms and their insiders face reduced civil deterrence due to fewer potential investor-plaintiffs, smaller classes, and smaller damages awards or settlements. Lawyers have a weaker incentive to file class-action lawsuits because their fees are linked to the size of the class and thus to the likely award. Foreign firms are therefore less likely to be sued. This reduction in legal exposure—and thus in legal bonding—should correlate with non-US transactions as a fraction of all transactions. These expected effects suggest several testable hypotheses.

We expect the legal event to have exerted a negative effect on foreign issuers' value proportionate to the non-US fraction of their equity base. In particular, the weaker a firm's home-country institutional environment, the greater will be the harm to investors due to the weakening of US private enforcement mechanisms. We further expect this weaker deterrence effect to be followed by several corporate-governance-related changes. First, weaker legal bonding will degrade financial-reporting quality. Among other things, this effect may lead to more aggressive earnings management, erosion of market professionals' ability to assess earnings, and more frequent changes of auditors. Second, changes in disclosure-related civil litigation and public enforcement may ensue. As noted above, the decrease in incentives to bring class actions could lower the number of class actions against foreign issuers. The expected effect on public enforcement is more ambiguous. Officials at the SEC or the Department of Justice may strive to compensate for the likely decrease in private enforcement. Alternatively, allocation of limited public resources may warrant focusing on US issuers or on particularly salient foreign issuers. Finally, the diluted civil liability regime could increase the potential for disclosure defaults. We therefore expect the bid-ask spread to widen, particularly for firms with weak institutions at home, due to an increase in the adverse-selection component of the bid-ask spread that reflects this information asymmetry.

The legal bonding hypothesis pertains to the firm level of analysis; it addresses whether exposure to private enforcement affects firm value. The *Morrison* case also presents an opportunity to investigate investors' reaction at the individual level of analysis, and thus to look at some of the mechanisms of legal bonding. By denying non-US traders the right to sue in US class actions, *Morrison* exerted a uniform effect on affected firms but a differential effect on their investors, depending on the locations of their trades. Trading in the United States provides investors the option to share in class-action damages award should a transaction be tainted by fraud. In fact, such a contingent claim to a fraud-damages payout would represent a transfer from holders of securities not traded in the United States. By hypothesis, investors may respond to this discriminatory effect by paying a premium on US-traded securities, relative to foreign-traded securities (of equivalent equity capital amount), that would reflect this option value. Investors may also shift at least some of their trades to US exchanges to secure this option.

5. Data

5.1. Dependent variable

Our sample consists of foreign companies with cross listings on US stock exchanges; we also include foreign companies trading on Over-the-Counter (OTC) markets. We collected information on our sample firms from numerous sources. The primary sources were the SEC and the websites of the various exchanges, Compustat North America, the Center for Research in Security Prices (CRSP) Monthly Stock File, the Committee on Uniform Securities (CUSIP) Master File, and the depository services directories of Bank of New York Mellon (BNY Mellon), JP Morgan Chase, and Citigroup. We identify the set of cross-listed firms with SEC compliance at the end of 2009 along with their country of incorporation from the SEC website. Information on which exchanges the firms list on and whether they have a listing in a foreign market was also

verified using Capital IQ's screening tools. In addition to those principal sources, the other sources consulted are detailed in an appendix.

A total of 676 foreign issuers were listed in the US on December 31, 2009. We hand-match the list of cross-listed firms with CRSP, Compustat, Worldscope, and Capital IQ to obtain various identifiers for our sample. We require that firms have listings in both the US and home markets because the *Morrison* decision refers to transactions effected outside the US. We also require that sample firms have non-missing returns on at least one of the event days to maintain consistency in the cross-sectional regressions. We drop securities that traded fewer than 500 shares on average during the event to ensure that each issue in the sample was actively trading.[12] We further require that sample firms have at least 30 valid returns over the estimation period between January 1, 2008 and December 31, 2009 with at least 20 valid returns after November 15, 2009. We examine the US and home market returns separately, so firms do not need to meet these requirements simultaneously in both markets in order to be included in the analysis. A firm whose US listing meets the above requirements, but whose home listing does not, would be included in the US analysis and excluded from the analysis of returns on the foreign exchange. These requirements result in a sample of 397 cross-listed firms with home market return data and 575 cross-listed firms with US market return data. The difference between the US and home samples is due to data availability. US daily returns data come from CRSP and home market

[12] Using different cutoffs does not meaningfully change the results (see the Appendix).

daily returns data come from Capital IQ, which is also the source of bond returns data.[13] Our sample includes firms that are no longer traded but were covered by these data vendors.[14]

While in theory fraud in OTC securities is prohibited as in any other security, in practice there is little enforcement regarding them. We thus exclude foreign issuers not on the SEC list that trade exclusively over the counter, consistent with the sample the SEC (2012) identified as relevant in *Morrison* (but unlike Gagnon and Karolyi, 2011). For the sake of completeness, however, we repeat the analyses in a sample that includes those OTC firms and obtain similar results. The SEC's definition of a foreign private issuer excludes firms that are technically foreign but essentially American, i.e., firms incorporated outside the United States, in which the majority of voting rights are held by American shareholders and one of the following criteria is also met: the majority of the top management is American; the majority of assets is located in the US; the business of the firm is managed primarily from in the U.S (SEC Rule 405 and Rule 3b-4). We obtain the roster of firms that are foreign according to the banks' websites but are regarded as domestic firms by the SEC. Any cross-listed firm that is not on the SEC roster but is on the roster of the banks' websites is classified into this category of "foreign domestic". We further verify the foreign domestic status of these firms through the EDGAR database and Thomson Analytics. Finally, while many foreign firms enter US securities markets using ADRs

[13] Additionally, we matched FPIs to their home market issues in Datastream. The data from Capital IQ include more non-missing, non-zero returns as well as more information on daily volume. The Capital IQ data also include dividend adjusted prices to five decimal points, while Datastream's return index variable is available to the nearest thousandth. The means of the return distributions are identical to four decimal places, and the standard deviations differ in the thousandth place. Capital IQ data were used for all home market results presented in the tables.

[14] We constructed the first version of our sample in 2011, and early versions of this paper relied on those data. We put substantial effort into updating the sample with new data from Capital IQ in 2013 and 2014 to take advantage of improvements to the Excel plug-in and access to bulk data through WRDS. Since starting this project, S&P Capital IQ has released more than ten new versions of its product; updating our data allowed us to improve our sample, but did not alter our conclusions. First, we clarified the match between U.S. and foreign securities for ten firms. This change is especially relevant for the analysis of price differences between markets in Table 11. Second, we reduced the number of accounting variables with missing data. This expanded the sample for the cross-sectional regressions explaining abnormal returns in Table 6. Third, we extensively reviewed our home market bid-ask spreads to investigate outliers and compared these data with Datastream (see below for comparison of spread data) to further ensure accuracy.

or other depositary facilities issued by depositary banks, a subset of 269 foreign firms in our sample use direct listing, namely, they list the same shares or stocks that are listed in their home market. Such direct listing is common among Canadian and Israeli issuers and a small number of firms from other countries. We identify the direct listings from the above-mentioned sources.

The benchmark choice is a major methodological issue in event studies of cross-listed firms (Karolyi, 2012). Our primary market benchmark is the S&P 500 index, as it has the advantage of not including any foreign firms. For robustness tests we construct foreign-issuer-free (FPI-free) versions of the Morgan Stanley Capital International (MSCI) Europe, MSCI World, and Financial Times Stock Exchange (FTSE) World indexes. For each of these benchmarks we take the list of its constituent securities from March 25, 2010 and remove sample firms, which are affected by the Supreme Court decision. We obtained constituent lists for each benchmark directly from MSCI and FTSE.

5.2. Explanatory and control variables

The location of the market on which investors trade is the key factor in the *Morrison* Court's approach. We use the proportion of value traded and non-US market capitalization as proxies for this factor. Since each proxy may have its own advantages and disadvantages, we employ both for robustness as well as for capturing their different aspects.[15] *Non-US Value Traded* is the mean proportion of monthly total value-traded in the first six months of 2009 that occurred on non-US stock exchanges. We calculated this variable using data from Capital IQ. *Non-US Market Capitalization* is one minus the ratio of the market value of equity in the form of cross-listed securities in the US divided by company market value. We combine multiple

[15] An earlier version of this paper exclusively relied on non-U.S. market capitalization. The non-U.S. value traded variable has the advantage of referring more closely to transactions although it is sensitive to trading velocity. Data for this variable are available for a larger group of firms and the calculations are easier to replicate with commonly used financial databases. This variable is also less susceptible to measurement error. Note that we use value-traded rather than volume because volume does not account for ADR bundling ratios. Our main results obtain with both variables; the correlation between the two measures is approximately 0.66.

sources to calculate this variable. We begin by obtaining market capitalization data from CRSP, Compustat, and Capital IQ. We then incorporate data on the number and value of shares in the United States from company annual reports and information provided to us directly by NASDAQ, NYSE, and Tel Aviv Stock Exchange (TASE) (for Israeli issuers). Every firm with multiple share classes was individually reviewed using SEC filings, and we calculated non-US market capitalization for the firm and each security class.[16] Remaining ambiguities were reconciled through individual phone calls to investor relations.

We obtain data on countries' institutional factors from prior literature and international organizations. From the World Bank's Governance Indicators we obtain the Rule of Law index, which captures countries' legality and general protection of property rights. We also use the Polity IV index of *constraints on the executive* as an alternative measure of property rights protection. We use the indexes of legal rules on civil liability (*private litigation*) and on *disclosure* in securities regulation laws drawn from La Porta et al. (2006), namely, the securities regulation rules that these authors identify as ones that "work" against insiders. To identify the countries that have adopted US-style *class action* we draw on Hensler (2011), who uses information from the Global Class Actions Exchange at Stanford University. A measure of shareholder protection known as the *anti-director-rights index* (ADRI) comes from Djankov, La Porta, Lopez-de-Silanes, and Shleifer (2008). Refining and improving on a prior index of the latter three authors, the ADRI focuses on countries' company laws. Spamann (2010) discusses alternative codings for the legal provisions included in the ADRI so we also obtain his versions of this prominent index. In addition, we consider Djankov et al.'s (2008) *anti-self-dealing index* (ASDI) of formal shareholder protection that emphasizes legal process. From Jackson and Roe

[16] We use non-U.S. market capitalization calculated at the firm level in all tables. Using a security-level measure does not change our conclusions; the correlation between the firm- and security-level variables is 0.97.

(2009) we obtain data on *public enforcement* of securities laws as measured by the weighted sizes of the budget and of the staff of the regulatory agency.

We control for several firm-level characteristics using data from Compustat. *Tobin's q* is (market value of equity + total assets - common equity) / total assets. *Fixed Asset Intensity* is property, plant, and equipment divided by total assets. *Log (total assets)* is the natural logarithm of total assets and controls for firm size. *Return on Equity* is earnings before interest, taxes, depreciation, and amortization divided by total common equity. *Capital expenditure* is capital expenditure divided by total assets. *Sales growth* is one-year sales growth and controls for growth opportunities. *Leverage* is short-term debt divided by total assets. We obtain accounting data from Capital IQ and Worldscope. With data from the Stanford Securities Class Action Clearinghouse we identify all the firms in our sample that have been thus sued since 1996 and the number of such lawsuits. Data on SEC enforcement actions is from Lexis Securities Mosaic (LSM). We focus on SEC documents in the LSM database that describe a case outcome (e.g. judge's decision, settlements, penalties from commission hearings, and trading suspensions). For the analysis of price/return differentials we rely on price data provided by Capital IQ for both the US security and the home country security. We then limit the sample to days in which both securities have available prices. To avoid spurious effects from wrongly coded data (especially for ADR bundling ratios) we winsorize price data at the 0.5^{th} and 99.5^{th} percentiles of the distribution in price differences. For the analysis of trading patterns we identify all of a company's active equities in Capital IQ and categorize them by exchange into foreign- versus US-exchange groups. We then download a daily time series of value traded for all days from Capital IQ, and sum the value traded by group and month. The analysis of bid-ask spreads also relies on daily data from Capital IQ, which we winsorize at the 99.5^{th} percentile to remove a

number of outliers that are clearly data errors. We compared the bid and ask prices from Capital IQ with numbers from Datastream and get a similar distribution of spreads. Closer inspection of the highest spreads shows they are anomalies due to sudden changes in the bid or ask prices reported by the data providers. The choice of financial database does not change the substance of our results; we use Capital IQ because it has more observations for our sample and is also the source of our international pricing and returns data. After winsorizing at the 99.5th percentile, the maximum spread in our sample is 21.1% of the closing price. The mean spread is 1.2% and the median is 0.3%. We do not winsorize the lower tail because zero is a natural lower bound on the bid-ask spread.

5.3. Summary statistics

Panel A of Table 1 reports the number of sample US-listed firms by country. These firms are from 48 countries, based on the SEC's designation of incorporation countries (which usually determines their applicable corporate law), and are diverse in several respects. The countries with the most companies are Canada (166 FPIs), Cayman Islands (63), Israel (42), and the United Kingdom (32). Data from Capital IQ suggests, however, that only about 14 firms are headquartered in tax havens. Sample firms are geographically diverse and are also somewhat diverse in their legal origin. English legal origin has the greatest number of sample firms, with 321 coming from 15 countries.[17] Panel B reports a respective country distribution of firms for home market analysis. Panel C reports the summary statistics for the variables used in our cross-sectional regressions. We report the number of observations with non-missing value for a specific variable. We also report the mean, median, standard deviation, and the 5th and 95th percentiles of these variables across all sample firms.

[17] Specifically: Antigua and Barbuda, Australia, Bermuda, Canada, Cayman Islands, Hong Kong, India, Ireland, Israel, Liberia, New Zealand, Papua New Guinea, Singapore, South Africa, United Kingdom.

6. Results

The empirical analysis consists of two parts. At the firm level of analysis, we first test markets' reaction to the oral argument event using several methodologies. Next, we examine whether abnormal returns relate to institutional factors. We then consider firm-level effects within a broad time window of several months. At the investor level of analysis, we examine whether market participants adjusted their trading behavior to the legal change that would provide an advantage to trading on US exchanges. We thus consider differential price and return changes and changes in relative trading volumes.

6.1. Abnormal returns around the focal event

We begin with a matched sample approach to examine whether abnormal returns of US-listed firms in fact differ from the returns of as-similar-as-possible foreign firms not listed in the United States and thus unaffected by the legal event. Table 2 presents results for several sequential matching procedures as well as different nearest-neighbor matches based on propensity score matching. For each sample firm, we identify the foreign, non-cross-listed, publicly-traded peer firm that is closest in terms of market capitalization, book-to-market ratio, or total assets. We draw peer firms domiciled in the same country and operating within the same industry as the sample company. To accommodate differences in markets' trading hours, we report results for the full sample and for a sample consisting only of issuers located in North or South America. For Western-Hemisphere issuers we show results for both US- and home-market returns. The results are consistent throughout and support the inference that markets tended to react positively to the imminent legal change. The size of these abnormal returns is substantial, ranging between 0.6 and 1.1 percent. Similar analyses of a sample that also includes

OTC and "foreign domestic" firms yield similar results, and even somewhat higher excess returns (available in an appendix).

Next we present traditional Brown-Warner analyses of abnormal returns. Table 3 reports abnormal US returns using market model, market adjusted, and mean adjusted returns. In light of Fama and French (2000), the panel also reports results for which the independence assumption is dropped. We use versions of four different benchmarks (S&P 500, MSCI Europe, MSCI World, and FTSE World). The results are consistent across different combinations of tests, benchmarks, samples, and markets: US-listed foreign firms experienced insignificant or even positive abnormal returns on the oral argument event, in contrast to the theoretical prediction. The abnormal returns vary in size but most of the significantly positive returns are about 0.5 percent or higher. Table 3 thus suggests that market participants did not consider the legal developments negative.[18]

Table 4 tests markets' assessments of the legal event using a portfolio analysis approach. We consider US returns of both value-weighted and equal-weighted portfolios, using the four indexes as benchmarks. We use the sample firms to create a daily portfolio. We regress the portfolio returns on intercept, relevant benchmark returns, and an indicator that is 1 for March 29 and zero otherwise. We report the coefficient estimates of the indicator and t-statistics. We also examine subsamples without a 25 percent return restriction, excluding tax haven companies, and focusing on firms from emerging markets. This method presents difficulties for using robust standard errors, however, due to the narrow event relative to the estimation period (Long and

[18] Using particular benchmarks such as S&P 500, MSCI Europe, and several other sections of MSCI, we separately investigated sub-samples of ADRs and direct listings on different individual days, focusing in particular on the oral argument date since information on this event is unlikely to leak. The results are consistent in that we observe either significantly positive or insignificant abnormal returns especially on that date. Consistent results obtain in a sample that also includes OTC and "foreign domestic" firms. To address the cross-correlation of returns in the most rigorous way we also implemented Greenwood's (2005) methodology (not shown). The results were consistent in that in no case do we observe a negative market reaction as expected from the legal bonding hypothesis.

Ervin, 2000). We therefore tabulate portfolio approach results with both non-robust and robust standard errors. With non-robust standard errors, the abnormal returns are insignificant, although we do not find a negative market reaction. Results for a three-day event window are qualitatively similar (not shown).

In summary thus far, whether one prefers to focus on the significant or the insignificant market reactions, a most conservative interpretation of the results suggests that US-listed foreign firms did not decline in value when the news of the Supreme Court's new approach emerged. Such a market reaction cannot be taken lightly if one believes that civil liability for securities fraud is responsible for any beneficial bonding effect that the literature has suggested. Even a "non-result" interpretation of these findings cannot be reconciled with the "purer 'legal' form" of the bonding hypothesis (Karolyi, 2012).

We also considered the release of the written decision event on June 24, 2010. Tests at early stages of this study suggested a mixed market reaction, depending on the benchmark and method of calculating abnormal returns. An in-depth analysis of the event itself suggests, unfortunately, that it does not lend itself to a reliable event study. In terms of news value for market participants, the written decision provided relatively little. During oral argument, justices from both the conservative and more liberal wings signaled an intention to limit the reach of the US anti-fraud regime abroad; the majority's language in the decision did not stray from the course charted during oral argument. The complexities of this event arose, however, from the circumstances surrounding it: primarily its close proximity in time to congressional passage of the DFA, debated in Congress at the same time as publication of the written decision and concluded within less than 24 hours. Furthermore, the provisions added to the DFA with regard to the extraterritorial reach of public enforcement were anything but clear (Painter, 2011). By

contrast, no major news events or currency movements surrounded the oral argument, and we confirmed that currency movements did not drive our results. The same cannot be said of the publication event. In addition to passage of the DFA, banking sector and world economic news was published around that date, tainting the time window. We repeated the analyses with samples that also include foreign domestics and OTC compliant firms as well as samples that exclude tax havens and obtained similar results.

We also examined the Supreme Court announcement that it would hear the case (writ of certiorari), on November 30, 2009 and found mixed but mostly insignificant abnormal returns. The certiorari date too may not provide for a clean event study, because two important economic reports on manufacturing and housing were released on December 1, 2009. Finally, we tested for abnormal returns during a combined nine-day event consisting of the three three-day windows surrounding the writ of certiorari, oral argument, and the decision. In nearly all of the specifications, we observe positive and mostly strong abnormal returns. Because of our reservations about the certiorari and decision windows, we focus here on the oral argument event. (The results are available in an appendix.)

Finally, we investigated how the bond market responded to the legal event. Ball et al. (2013) argue that by cross-listing equity in the United States foreign firms may lower their cost of debt thanks to lower information costs attributable to the US disclosure regime and class actions. This view is in line with the bonding hypothesis although not necessarily thanks to legal bonding. The weakening of legal bonding to full disclosure due to *Morrison* thus should be met with a negative response from bond market participants. These are largely sophisticated institutional investors that generally respond quickly to negative news, including class actions (Billings, Klein, and Zur, 2012). Moreover, the wealth transfer hypothesis with regard to

stockholders and bondholders, which derives from the agency cost of debt, also implies that after the legal event, insiders may have more opportunities to extract value from bondholders (Cremers, Nair, and Wei, 2007; Francis et al., 2010; Klein and Zur, 2011).

As with the equity analysis, we assembled a bond sample, starting with all bonds issued by the exchange-traded foreign issuers having data on maturity date, offering amount, and offering price, from which we eliminated puttable and zero-coupon bonds. For firms with more than one bond, which is the case in most of our sample firms, we aggregated the bond returns for each firm. This is in line with Bessembinder et al. (2009), who note that aggregation has the advantage of not excessively weighting firms with many bonds and avoids the bias inherent in picking a single bond for each firm. We weight by the bond's offering amount because many bonds in our sample that trade outside the United States and do not have time series data on market value.[19]

Table 5 presents the results. We fail to observe meaningful market reactions, whether we implement a mean-adjusted approach or a matched portfolio approach for testing abnormal bond returns (again following Bessembinder et al., 2009). Except for one weakly positive and one weakly negative response, all of the bond return estimates are insignificant. Beyond supporting the above findings with regard to the questionable value of legal bonding, these tests indicate that any gain to shareholders due to the legal event did not come at the expense of debtholders.

6.2. Institutions and the value of enforcement

We now turn to examining how different factors may have affected markets' reactions to the legal developments. As Kothari and Warner (2007, p. 19) point out, such cross-sectional

[19] Bessembinder et al. (2009) study U.S. corporate debt using TRACE. In contrast, our sample includes bonds that are not dollar-denominated and trade outside the United States. Data on these international corporate bonds is sparser. The correlation between the offering amount and the amount outstanding on the oral argument date for bonds with non-missing data is 0.93. We also try an equal-weighted approach and find no meaningful difference in the results.

tests "are relevant even when the mean stock price effect of an event is zero." Table 6 reports the results of cross-sectional regressions where firm-level and country-level variables are used to explain variation in the abnormal returns of individual issuers during the oral argument event. The table presents piecewise linear specifications that distinguish between firms with above- and below-median proportions of their value traded or their non-US market capitalization as a proxy for the fraction of non-US transactions, which in turn proxies for US civil liability exposure. We consider several variables that capture different facets of the institutional environment in firms' home countries. These variables range from broad aspects of legality and protection of property rights, to corporate governance and investor protection, to specific aspects of securities regulation. We control for GDP per capita to avoid spurious effects from the level of national wealth and economic development. To avoid collinearity problems we enter the institutional variables one at a time.

The finding that stands out is that home market abnormal returns tended to be *higher* at firms with above-median non-US value traded or above-median non-US capitalization. These are the firms that would be more decisively excluded from US private enforcement. Most of the firm-level factors do not show significant relations to abnormal returns. Among institutional factors, measures of general property rights protection (rule of law), constraints on executive decision-making, US-like class actions, and home-country public enforcement exhibit either a negative or an insignificant sign. These results are contrary to the hypothesis that US private enforcement functions as a valuable bonding mechanism.

The finding that abnormal returns are more strongly positive for firms that appear to be most affected by *Morrison* goes beyond supporting the null hypothesis. One way to interpret this evidence may be called the "regulatory burden hypothesis". In this view, market participants

consider securities fraud class actions as a regime whose overall contribution to firm value is negative and proportionate to the firm's exposure to liability due to class size. The smaller the class of potential plaintiffs the better, goes this argument.[20] Another conjecture may derive from the reputational bonding theory. In this view, US-like class-action-based civil liability could crowd out or mask signals about better governance that insiders may want to convey through reputation building. Dilution of legal liability thus could make more room for such alternative signals. This reasoning does not necessarily imply that securities class actions have negative value for firms that are exposed to them; only that in comparison, legal bonding based on US-like class actions may be inferior to other bonding mechanisms.

In separate regressions using the basic specification in Table 6, we entered a dummy variable that takes a value of 1 for firms that have been sued in securities class actions or, alternatively, an index counting the number of lawsuits against the company, which may better account for recidivistic firms. Though this is not conclusive evidence of wrongdoing (Karpoff et al., 2014), procedural rules since 1995 significantly limit frivolous suits. Such a checkered history could thus indicate firm-level governance issues (Gande and Miller, 2012). These variables exhibit insignificant signs in both tests, however (not shown). Thus, even for firms whose investors have sought redress through the US civil liability system markets did not respond differently to the dilution of this legal protection. This evidence, too, is hard to reconcile with the legal bonding hypothesis.

6.3. Longer-term effects on disclosure, litigation and enforcement, and the spread

The findings presented thus far originate in the legal event's time window. Aside from the inherent limitations of the event study approach, which we mitigate by using several

[20] This conjecture warrants elaboration, of course. The impact of a class action on firms is not purely proportionate to class size. There are also fixed elements, including distraction of management attention, litigation costs, etc.

methodologies and numerous specifications, it is important to examine the governance-related effects of the legal event over a longer time span. In this section, we test whether *Morrison* was followed by changes in disclosure behavior by affected firms, by changes in private and public enforcement, and by market participants' perception of the weakened private enforcement as reflected in the bid-ask spread.

Table 7 examines several facets of disclosure quality. Panel A examines the effect of *Morrison* on earnings management as measured using accruals. We implement two different methods for calculating accruals, by cash flow and by balance sheet, and two different methodologies for assessing changes: a modified Jones (1991) model and a difference-in-differences model using domestic issuers as a control group. The results show that foreign private issuers had neither significantly higher nor lower discretionary accruals after *Morrison* and that this result is robust to various sample restrictions and different approaches to calculating accruals.[21] Panel B examines whether *Morrison* led to more frequent earnings surprises, either positive (exceeding market expectations) or negative (failing to meet expectations). To measure unexpected earnings we implement Foster et al.'s (1984) approach and alternatively use analyst expectations. Except for one specification (Model 4), that suggests some change among firms that were *less* affected by *Morrison*, the results show no change in the likelihood of earnings surprises. This is consistent with the idea that the change in civil liability regime did not affect the informational environment of foreign firms in terms of market professionals' ability to assess their financial performance.

We next examine whether foreign private issuers became more likely to switch auditors after *Morrison*. A higher propensity to change auditors could indicate more aggressive earnings

[21] Our conclusions are not affected by the choice of accrual formula, although we find that estimates using the statement of cash flows are more precise, which is consistent with Hribar and Collins (2002).

management or greater use of questionable accounting practices that auditors are better able to observe. We test this hypothesis using a difference-in-differences approach that compares foreign firms to domestic firms pre- and post-*Morrison*. The results in Panel C show no effect on foreign firms' tendency to change auditors. Together, the results in Table 7 support the view that pre-*Morrison* US private enforcement provided no marginal deterrence from misreporting or earnings manipulation. With the previous findings of positive market reactions to the legal event, the evidence suggests that the US Supreme Court bestowed net gains on foreign firms rather than merely changing the allocation of value among investors.

We also investigate if the loosening of legal bonding due to reduced civil liability has led to more frequent private or public enforcement actions, which could indicate an increase in corporate misreporting by foreign issuers or a substitution effect between private and public enforcement. Studies conducted in the years following *Morrison* documented stability in the number of class action filings and lower average settlements—a finding that was attributed to reduced class size due to the decision (Buckberg and Gulker, 2011; Patton, 2012; Coates, 2015).[22] Table 8 thus tests the effect of the legal event on the likelihood of private class actions and public SEC actions. Among the latter we further distinguish administrative law judge (ALJ) decisions, civil cases, and administrative proceedings. We control for financial variables as in Table 6 and for firm- and time-invariant effects. The results show that foreign private issuers became somewhat less likely to be sued in class actions after the *Morrison* argument, and that this effect is driven by firms that were more extensively shielded from civil liability. These findings are consistent with non-US firms becoming less attractive targets for class action lawyers after *Morrison* because they present smaller classes, lower likely awards, and lower

[22] Cheng, Srnivasan, and Yu (2014), too, assume that *Morrison* had a large impact on foreign firms' exposure to litigation risk, and therefore limit their study of litigation risk to the pre-*Morrison* period.

attorney fees. At less than one percentage point, the effect size is rather small, however. These results are in line with the findings reported in Table 7. That is, in the absence of more frequent post-event red flags such as switching of auditors or failure to meet analysts' forecasts, which often trigger class action lawsuits, there is less reason to expect an increase in such lawsuits. In tandem, no significant change is observed with regard to public enforcement measures post-*Morrison*. The apparent stability in foreign issuers' compliance despite the weakening of the civil liability regime that governs them therefore cannot be attributed to tighter public oversight à la Jackson and Roe (2009).

Finally, we consider changes in the bid-ask spread in the home market. The spread serves as a measure of adverse selection risk due to inferior expected disclosure; a wider spread would indicate that the likelihood of weaker private enforcement may have blunted the incentives for full disclosure. Figure 1 shows the home market spread for 30 business days before and after the oral argument event. Spreads do not increase in the period surrounding the event, a conclusion that is further supported by regression analyses using a longer time series. Table 9 examines home market bid-ask spread data for the eight-month period between January 1, 2010 and August 31, 2010. The main variable of interest is a post-event dummy taking a value of 1 for the four months after the oral argument and 0 otherwise. We control for non-US market capitalization and for the home-country institutional factors as discussed above. We also control for Canada and Australia as home markets to address idiosyncrasies in these markets.[23] To account for event-related effects of institutions on the spread we enter interaction terms with the post-event dummy. This dummy exhibits insignificant signs, which cannot support an

[23] Both Canada and Australia are markets with high-quality institutions yet bid-ask spreads there turn out to be exceptionally high. This caused the *general* coefficients (not the post-event interactions) for disclosure, class actions, and public enforcement to show a positive sign, contrary to what the literature implies and documents. These general institutional factors are beside the present analysis. Our results for the post-event interaction terms are not affected by these dummies.

inference that weaker deterrence by US private enforcement has led to greater adverse selection risk, as the legal bonding hypothesis implies. Spreads are significantly narrower the greater the firm's non-US capitalization, as expected, since liquidity attracts liquidity. Surprisingly, spreads are generally greater for firms whose countries have better private securities litigation rules (and rule of law), irrespective of the event, as if such rules were counterproductive in mitigating agency problems. Beyond this, however, spreads exhibit virtually no post-event institutionally-related sensitivity. Similar results obtain for US spreads (not shown). This evidence suggests that dilution of civil deterrence did not cause investors to hedge more vigorously against non-disclosure.

6.4. Investor level of analysis

This section examines market reactions to the legal event from investors' vantage-point. These analyses complement the preceding analyses of the (private) legal bonding hypothesis at the firm level; they examine the extent to which investors may *personally* appreciate a US location of trade, which *Morrison* made crucial for having a right to sue and collect damages in a US class action.

We first examine if investors have voted with their feet by shifting trading volume to the US market post-*Morrison* (Abdallah, Abdallah, and Saad, 2011). In Table 10 we regress the share of total trading volume that occurs on US markets, using specifications similar to those in Tables 8. The post-event dummy does not suggest that investors somehow changed their preferred trading venues in response to the event, in line with Bartlett's (2015) findings about institutional investors. The same pattern characterizes the interaction terms of this dummy with institutional factors, except for the model with Anti Director Rights Index. This finding suggests some post-event migration of trading *away from* US markets. Such migration was actually

stronger from countries with better shareholder protection, contrary to the thrust of the legal bonding hypothesis.

Next, we ask whether the legal event made investors more willing to pay a premium for US-traded securities. Note first that US-traded equities of foreign issuers command a premium of about 0.5 percent on average over similar equities traded on the home market. Figure 2 shows this premium over the 30 business days before and after the oral argument, adjusted for ADR bundling ratios, currency differences, splits, dividends, and the like. Table 11 presents regressions, in which the dependent variable is the difference between US and home market prices as a percentage of the home market price between January 1, 2010 and August 31, 2010.[24] We employ specifications similar to those used in the regressions in Table 6, which include a post-event dummy, non-US capitalization, institutions, and interaction terms. The results for the post-event dummy are mixed: one positive, some insignificant, and a few negative signs.I If anything, this evidence leans against imputing a positive value to attaining a US cause of action.[25] Among the interaction terms for institutional factors, we observe a positive sign for home country disclosure rules and a negative sign for home country public enforcement, which are hard to reconcile. Other interaction terms are insignificant. One may cautiously interpret these results as consistent with the circularity argument against securities class actions in

[24] This dependent variable measures the premium of the U.S. shares. If the decision in *Morrison* bestowed valuable rights exclusively on the U.S.-traded securities, then there should be a permanent increase in the price premia of the U.S. shares post-*Morrison.* Alternative measures, such as the difference between U.S. and home market returns during the event, do not capture the value of the class action option on the U.S. shares because differences in returns are transitory, but the rights uniquely granted to purchasers of shares in the United States were permanent. Nevertheless, we examine the difference in returns during the event in panel C of table 7 and fail to find evidence that the difference was unusual compared to other one- and three-day windows.

[25] We also considered Gagnon and Karolyi's (2011) finding of post-event changes in return differentials between firms' U.S.-listed securities and their respective home-market-listed shares. Using a similar regression of the return on a portfolio of return differentials of ADRs and direct listers on the return on a benchmark index and a dummy variable as an indicator for the event days, with non-robust standard errors, we failed to observe a significant deviation. In addition, we used bootstrapping to estimate the empirical distribution of the difference in returns between markets and compares the event period mean return difference to that distribution. The mean difference in returns during the event, using two time-windows, is not unusual. The results are available in the appendix.

general.[26] That is, a settlement award limited to US-trading investors may entail a certain relative transfer from non-US-trading investors, but in the long run all shareholders end up bearing the costs of exposure to class actions. The upshot could be that the value of having a class action protection may not be worth paying for.

7. Conclusion

This study examines the effects of the 2010 US Supreme Court decision in *Morrison v. National Australia Bank* that decreased US-listed foreign firms' potential liability from charges of financial misconduct, particularly through class action lawsuits. Averaging over all US-listed foreign firms, we find evidence of either an increase or an insignificant change in share value. The change in share value is positively related to the degree to which the firm was likely to be affected by the court decision. A series of tests of longer-run effects on foreign firms' disclosure quality fails to reveal deterioration in their reporting practices. In line with this finding, foreign firms have not become more likely to be sued in class actions or be subject to major public enforcement actions. We also fail to observe an increase in the bid-ask spread, or that investors worked to mitigate the effect of the legal change by trading more in US markets. These results are inconsistent with the legal bonding hypothesis, which holds that foreign firms benefit from listing in the US because they become subject to US enforcement of securities laws.

The results thus raise the question suggested in the title of this paper. Accepting that foreign firms use a US cross-listing for bonding—that is, to signal a credible commitment to high standards—the nature of the commitment mechanism remains unresolved. Investors may consider the disclosure duties specified by US laws to be valuable. But investors' concerns about compliance with these duties do not appear to depend on private legal enforcement in US

[26] We are grateful to an anonymous referee for suggesting this point.

courts. Foreign firms' commitment to compliance may thus hinge on informal mechanisms, most plausibly reputation-based mechanisms (e.g., Siegel, 2005; Carlin, Dorobantu, and Viswanathan, 2010; Hope, Kang, and Kim, 2013). Legal enforcement could support the maintenance of such a reputation, probably via public enforcement (Karpoff, 2012) or through indirect incentives from other markets (e.g., Lel and Miller, 2014) rather than civil-liability-based deterrence. The latter conjectures call for empirical testing, however, as this paper does not provide support for reputational bonding as much as it provides evidence that is inconsistent with legal bonding.

That there is no significant change in foreign firms' disclosure quality post-*Morrison* indicates that compliance does significantly hinge on expected legal penalties, but this leaves open what other market-based forces could be responsible for inducing compliance. The non-negative market reaction to *Morrison* suggests that whatever they are, such mechanisms are more efficient in equilibrium than legal penalties. Stock price reactions to the legal event appear more positive for firms with weaker informal home-market institutions, especially the rule of law and constraints on the executive in addition to home-country enforcement (see Table 6). In theory, one would expect the opposite, namely, that firms from weaker institutional environments would lose more from lesser US enforcement. In interpreting these somewhat puzzling results there is modest support for the idea that firms from emerging economies with relatively weaker non-legal institutions benefited more, all else equal, from the *Morrison* event. This particular evidence from the institutional controls is not uniformly statistically significant, however. Also, this effect of certain institutional controls could be explained by both of the following: (a) the firms from emerging economies with relatively weaker governance institutions in this sample having chosen to differentiated themselves from their home-country peers and having thus

voluntarily opted into better corporate governance practice (as shown in Hugill and Siegel, 2017) and (b) the market judged that the US private liability regime is particularly costly and ineffective for these firms..

A cynic might note that the only parties interested in the US private antifraud regime are insurers and lawyers. It is perhaps ironic but unsurprising therefore that securities class action lawsuits, many of which are supported by litigation funders, have become more prevalent in Australia (Morabito, 2016). US law firms are pioneering in implementing securities class actions in Europe and Asia, too, where "the American entrepreneurial spirit has overcome all obstacles" (Coffee, 2016). We do not subscribe to the idea that enforcement in general is unimportant. Subject to further in-depth analysis, the market reactions we report do not entail that civil liability should be abolished, let alone that public enforcement should. Private enforcement of securities laws may be beneficial if it is designed to exert effective deterrence—that is, if the actual perpetrators of fraud are not allowed to avoid virtually all liability.

Broadly speaking, the present results lend support to criticism of US secondary-market civil liability as it is currently structured. Which component of the US regime is particularly problematic—whether it is the fraud-on-the-market doctrine, or class action rules, or another legal institution dealing with insiders' liability—warrants further research. A legal system that provides weak private enforcement but entails considerable costs may simply not be worthwhile. A better-designed liability system that actually delivers targeted deterrence and compensation to aggrieved investors may be required for legal bonding to be effective. Our findings provide further impetus to seek such mechanisms—perhaps by reforming insurance arrangements to provide that insider transgressors personally face stiffer consequences, and by promoting a far more vigilant public enforcement system willing to impose civil penalties in addition to criminal

sanctions (compare, respectively, Baker and Griffith, 2011; Christensen, Hail, and Leuz, 2016). Meanwhile, policy makers in emerging economies who believe that local firms could piggyback on US legal institutions for bonding may need to invest in strengthening public enforcement at home.

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Tables and Figures

Table 1. Summary Statistics

This table reports summary statistics of our sample. Panel A reports the distribution of cross-listed FPIs included in the Table 3 sample by country as defined by the SEC. Panel B reports the distribution by country for firms included in the analyses of home market returns. Panel C reports the distributions of country- and firm-level variables of cross-listed foreign private issuers (FPIs). *N* is the number of cross-listed FPIs in Panels A, B, and C. *N* varies for different variables in Panel C due to data availability. We require that the sample securities trade at least 500 shares per day on average during the event. In Panel C, *Non-US Value Traded* is the proportion of total value-traded in the first six months of 2009 that occurred on non-US stock exchanges. *Capital Expenditure* is capital expenditure divided by total assets. *Leverage* is liabilities divided by total assets. *Fixed Assets Ratio* is property, plant, and equipment divided by total assets. *Sales Growth* is the change in annual revenues. *Return on Equity* is earnings before interest, taxes, depreciation, and amortization divided by average common equity. Tobin's *Q* is (market value of equity + total assets - common equity) / total assets. *Log (Total Assets)* is the logarithm of total assets. *Rule of Law* from the World Bank is a measure of confidence that people abide by rules and the quality of enforcement. *Constraint Exec* is the XCONST variable from the Polity IV Project and measures constraints imposed on executive decision-making by institutions. This variable is identical to the EXCONST variable from the same source. *Disclosure* is an index of securities law disclosure rules and *Private Litigation* is an index of securities litigation rules, both from La Porta et al. (2006). *Anti-Director Rights* is an index of shareholder protection laws, and *Anti-Self-Dealing Rights* is an index of self-dealing regulation, both from Djankov et al. (2008). *Class Actions* are US-style class actions from Hensler (2011). *Public Enforcement* is the weighted size of staff of the securities regulation agency from Jackson and Roe (2009).

Panel A. Country Distribution for US Market Analysis

Country	N	Country	N	Country	N
Antigua and Barbuda	1	Greece	3	Norway	1
Argentina	13	Hong Kong	4	Panama	2
Australia	9	Hungary	1	Papua New Guinea	1
Belgium	2	India	13	Peru	1
Bermuda	17	Indonesia	2	Philippines	1
Brazil	14	Ireland	8	Portugal	1
British Virgin Islands	13	Israel	42	Russia	4
Canada	166	Italy	4	Singapore	1
Cayman Islands	63	Japan	21	South Africa	6
Chile	11	Liberia	1	South Korea	9
China	12	Luxembourg	5	Spain	5
Colombia	1	Marshall Islands	15	Sweden	1
Denmark	2	Mexico	20	Switzerland	6
Finland	1	Netherlands	15	Taiwan	6
France	9	Netherlands Antilles	1	Turkey	1
Germany	7	New Zealand	1	United Kingdom	32
				Total	575

Panel B. Country Distribution for Home Market Analysis

Country	N	Country	N	Country	N
Argentina	11	Germany	7	New Zealand	1
Australia	8	Greece	3	Norway	1
Belgium	2	Hong Kong	4	Papua New Guinea	1
Bermuda	5	Hungary	1	Peru	1
Brazil	15	India	12	Philippines	1
British Virgin Islands	3	Indonesia	2	Portugal	1
Canada	149	Ireland	5	South Africa	6
Cayman Islands	12	Israel	2	South Korea	8
Chile	12	Italy	3	Spain	4
China	11	Japan	20	Sweden	1
Colombia	1	Luxembourg	4	Switzerland	5
Denmark	2	Marshall Islands	1	Taiwan	6
Finland	1	Mexico	16	Turkey	1
France	8	Netherlands	11	United Kingdom	29
				Total	397

Panel C. Variable Distributions

Variable	N	Mean	Median	Standard Deviation	5th Percentile	95th Percentile
Firm-Level Variables						
Non-US Market Cap	473	0.62	0.72	0.34	0.00	0.99
Non-US Value Traded	544	0.44	0.41	0.34	0.00	0.96
Sales Growth	540	-0.04	-0.15	1.31	-0.57	0.42
Capital Expenditure	573	0.06	0.04	0.06	0.00	0.19
Return on Equity	566	0.00	0.06	0.77	-0.55	0.33
Fixed Assets Ratio	575	0.34	0.25	0.30	0.00	0.86
Log(Total Assets)	575	7.71	7.54	2.72	3.61	12.41
Tobin's Q	575	1.99	1.33	5.62	0.73	4.19
Leverage	575	0.47	0.47	0.29	0.05	0.95

Table 2. Abnormal Returns of Cross-Listed FPIs Using Matched Samples

This table reports the percentage abnormal returns of cross-listed FPIs for the March 29 event using a foreign, non-cross-listed peer company for each sample firm. For the sequential matching approach, we identify the publicly-traded peer firm for each sample FPI that is closest in terms of market capitalization, book-to-market ratio, or total assets. We draw peer firms domiciled in the same country and operating within the same industry as the sample FPI. The nearest-neighbor propensity score approach relies on a probit model that calculates the probability of a firm being in the sample FPI group based on market capitalization, book-to-market ratio, and total assets. The peer firm then becomes the foreign company without a cross-listing that is closest to the sample FPI in terms of propensity score. In all columns we calculate market model returns for the sample FPIs using the peer firm as a benchmark, and *t*-statistics assuming independence as described by Brown and Warner (1985). Columns (1) and (2) present results for all sample FPIs, while columns (3) and (4) limit the analysis to firms located in North and South America. The coefficients represent the mean abnormal return of sample FPIs over the peer firm. Returns are positive and economically significant, contrary to the legal bonding hypothesis. We require that the sample securities trade at least 500 shares per day on average during the event. The data are from January 2008 through August 2010.

	All FPIs				Western Hemisphere FPIs			
	US Returns		Home Returns		US Returns		Home Returns	
	(1)		(2)		(3)		(4)	
	Ret.	t-stat	Ret.	t-stat	Ret.	t-stat	Ret.	t-stat
Sequential Matching								
Matching by: Country, 2-digit GICS Industry, and								
Market Capitalization	0.70 ***	4.02	0.66 ***	3.00	0.62 **	2.09	0.84 ***	2.61
Book-to-Market	0.79 ***	4.49	0.80 ***	3.77	0.83 ***	2.94	1.05 ***	3.48
Assets	0.68 ***	3.73	0.60 **	2.53	0.55 *	1.83	0.77 **	2.32
Matching by: Country, 3-Digit SIC Industry, and								
Market Capitalization	0.68 ***	3.47	0.68 ***	2.88	0.66 **	2.10	0.89 ***	2.70
Book-to-Market	0.79 ***	3.91	0.81 ***	3.34	0.83 ***	2.62	1.05 ***	3.19
Assets	0.63 ***	3.23	0.59 **	2.50	0.62 **	1.96	0.78 **	2.42
Nearest Neighbor Matching (2-digit GICS)								
Cap, B/M, Country	0.75 ***	3.06	0.93 ***	2.93	0.68 **	2.06	1.04 ***	3.04
Cap, Assets, Country	0.73 ***	2.97	0.93 ***	2.88	0.70 **	2.09	1.02 ***	2.81
Cap, Assets, B/M, Country	0.67 ***	2.68	0.89 ***	2.68	0.62 *	1.88	0.97 ***	2.71
Assets, B/M, Country	0.71 ***	3.00	0.92 ***	2.99	0.69 **	2.07	1.02 ***	3.91

Table 3. The Abnormal Returns of Cross-Listed FPIs

This table reports the percentage abnormal returns of cross-listed FPIs and Brown and Warner (1985) t-statistics for the March 29 oral argument event. Sample FPIs are all from the NYSE, AMEX, or Nasdaq, have listings in both the US and home countries, and are on the SEC's FPI compliance list. We present the results using a variety of benchmarks and measures of abnormal performance. For the market model returns we use the period from January 2008 to December 2009 as the estimation period. For the MSCI and FTSE benchmarks, we take the list of all constituent securities for each benchmark from March 25, 2010 and remove sample firms, which are affected by the Supreme Court decision. The market adjusted returns and mean adjusted returns are calculated as in Brown and Warner (1985), and we use January 2008 to December 2009 as the baseline period for mean adjusted returns. We require firms to trade at least 500 shares on the event day and report results assuming both dependence and independence in cross-sectional returns. The table presents results for both US and home market abnormal returns. Abnormal returns are either positive or insignificant, contrary to the legal bonding hypothesis. # of Positives is the number of FPIs with positive abnormal returns out of the total number of sample FPIs with available data. ***, **, and * indicate that estimates are significant at the 1%, 5%, and 10% levels, respectively, according to the Brown and Warner t-statistics, which are reported under the heading of BW t-stat. Following Morck, Yeung, and Wu (2000), we trim the stock returns for cross-listed FPIs from CRSP by excluding any daily return that exceeds 25% in absolute value. The data are from January 2008 through August 2010.

Exchange Listed FPIs, March 29 Window

	US Returns				Home Market Returns			
	Returns	Brown-Warner t-stat.		# of	Returns	Brown-Warner t-stat.		# of
		Independence	Dependence	Positives		Independence	Dependence	Positives
Market Model Returns								
S&P 500	0.45	3.25 ***	0.43	349/575	0.78	3.71 ***	0.45	259/397
MSCI Europe	0.44	2.89 ***	0.28	348/575	0.47	2.06 **	0.51	229/397
MSCI World	0.26	1.93 *	0.33	316/575	0.52	2.41 **	0.41	236/397
FTSE World	0.21	1.54	0.26	311/575	0.45	2.01 **	0.39	225/397
Market Adjusted Returns								
S&P 500	0.42	3.13 ***	0.40	346/575	0.56	2.55 **	0.29	241/397
MSCI Europe	0.17	1.10	0.10	305/575	0.31	1.13	0.31	218/397
MSCI World	0.31	2.26 **	0.36	327/575	0.45	2.02 **	0.36	229/397
FTSE World	0.27	1.94 *	0.31	318/575	0.41	1.77 *	0.36	226/397
Mean Adjusted Returns	1.01	5.75 ***	0.44	425/575	1.15	5.25 ***	0.54	301/397

Table 4. Portfolio Abnormal Returns of Cross-Listed FPIs

This table reports the percentage abnormal US returns of equal- and value-weighted portfolios of cross-listed FPIs for the March 29 event. We report the coefficient on an event dummy variable and the associated *t*-statistic. The narrow event presents difficulties for using robust standard errors (see Long and Ervin (2000)). We therefore present t-statistics that are not corrected for heteroskedasticity in addition to results using robust standard errors for comparison. Note that the results using robust errors are *more* significant than the results using non-robust errors due to a negative relationship between the event dummy variable and the residuals. The coefficients in the Returns column represent abnormal returns of sample FPIs during the event period. We fail to find a negative reaction associated with the legal event, which is inconsistent with the legal bonding hypothesis. We require that the sample securities trade at least 500 shares on the event day. The data are from January 2008 through March 2010.

	Equal-Weighted Portfolios				Value-Weighted Portfolios			
	Returns	Non-Robust	Robust		Returns	Non-Robust	Robust	
S&P 500 Benchmark								
ADRs – Baseline	0.49	0.53	11.82	***	0.38	0.44	9.83	***
Including Returns > 25%	0.47	0.50	10.66	***	0.32	0.36	7.73	***
Emerging Markets	0.77	0.60	13.54	***	0.61	0.60	13.21	***
Excluding Tax Havens	0.46	0.55	12.21	***	0.29	0.35	7.87	***
ADRs + Direct Listers – Baseline	0.63	0.64	14.38	***	0.45	0.44	9.64	***
Including Returns > 25%	0.62	0.61	13.61	***	0.36	0.34	7.21	***
Emerging Markets	0.77	0.64	14.47	***	0.46	0.47	9.96	***
Excluding Tax Havens	0.62	0.63	14.37	***	0.39	0.39	8.63	***
MSCI Europe								
ADRs – Baseline	0.52	0.30	6.31	***	0.40	0.25	5.20	***
Including Returns > 25%	0.50	0.28	5.76	***	0.34	0.20	4.13	***
Emerging Markets	0.82	0.39	8.32	***	0.63	0.37	7.83	***
Excluding Tax Havens	0.49	0.30	6.28	***	0.32	0.19	4.03	***
ADRs + Direct Listers – Baseline	0.65	0.37	8.47	***	0.43	0.28	6.27	***
Including Returns > 25%	0.64	0.36	8.00	***	0.33	0.21	4.61	***
Emerging Markets	0.82	0.40	8.64	***	0.47	0.29	6.19	***
Excluding Tax Havens	0.64	0.37	8.47	***	0.37	0.25	5.52	***
MSCI World								
ADRs – Baseline	0.29	0.36	7.56	***	0.19	0.27	5.33	***
Including Returns > 25%	0.27	0.33	6.97	***	0.11	0.17	3.54	***
Emerging Markets	0.56	0.46	9.78	***	0.41	0.48	9.61	***
Excluding Tax Havens	0.27	0.37	7.86	***	0.09	0.14	2.70	***
ADRs + Direct Listers – Baseline	0.43	0.51	10.97	***	0.25	0.33	6.43	***
Including Returns > 25%	0.41	0.49	10.80	***	0.15	0.19	4.04	***
Emerging Markets	0.56	0.50	10.65	***	0.27	0.34	6.49	***
Excluding Tax Havens	0.42	0.51	11.06	***	0.20	0.26	5.17	***
FTSE World								
ADRs – Baseline	0.24	0.27	5.75	***	0.13	0.18	3.68	***
Including Returns > 25%	0.22	0.24	5.02	***	0.06	0.08	1.69	*
Emerging Markets	0.50	0.40	8.49	***	0.36	0.41	8.28	***
Excluding Tax Havens	0.23	0.28	5.85	***	0.04	0.06	1.13	
ADRs + Direct Listers – Baseline	0.38	0.42	9.09	***	0.20	0.26	5.03	***
Including Returns > 25%	0.36	0.39	8.73	***	0.10	0.12	2.47	**
Emerging Markets	0.50	0.43	9.26	***	0.22	0.27	5.15	***
Excluding Tax Havens	0.37	0.41	9.12	***	0.15	0.19	3.78	***

Table 5. Abnormal Bond Returns

This table shows mean abnormal returns on the bonds of foreign private issuers on the date of the oral argument in *Morrison* (March 29, 2010). Each observation is a firm; for firms with multiple bonds, we calculate either an equal-weighted return or a return weighted by the offering amount of the bond. The USA columns refer to bonds traded in the United States (either on or off an exchange), while the Foreign columns refers to bonds traded outside the United States. The mean adjusted and matching portfolio returns are calculated as in Bessembinder et al. (2009). Panel A shows results using Bloomberg/Barclays indices as benchmarks. For the mean adjusted analysis, we match each sample bond with the index of US treasury securities of similar time to maturity or with the Bloomberg/Barclays Global Treasury index. For the matching portfolio analyses, the US Aggregate and Global Aggregate benchmarks use the indices of the same name from Bloomberg/Barclays. The Global Intermediate/Long benchmark matches sample bonds with less than 10 years to maturity to the Global Intermediate index and bonds with more than 10 years to maturity with the Global Long index. The Global Matched Maturity benchmark assigns each sample bond to either the 1-3, 3-5, 5-7, 7-10, or 10+ year Global Aggregate index based on time to maturity. Panel B matches sample bonds based on SEC country. The mean adjusted analysis matches each bond to a Datastream "All Lives" (i.e. all maturities) Government index. The matching portfolio analysis matches each bond to a country-specific Bloomberg/Barclays index (either a country specific "Global Aggregate" index or a country-specific "Emerging Markets Aggregate" index). ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A. US and Global Indices

	Equal-Weighted		Value-Weighted	
	USA	Foreign	USA	Foreign
Mean Adjusted				
US Treasury	0.00	0.01	-0.01	-0.01
	(0.05)	(0.03)	(0.05)	(0.02)
Global Treasury	-0.02	-0.01	-0.03	-0.03
	(0.05)	(0.03)	(0.05)	(0.02)
Matching Portfolio				
US Aggregate	0.01	0.04*	0.01	0.02
	(0.05)	(0.03)	(0.05)	(0.02)
Global Aggregate	-0.02	0.01	-0.02	-0.01
	(0.05)	(0.03)	(0.05)	(0.02)
Global Intermediate/Long	-0.02	0.02	-0.02	-0.01
	(0.05)	(0.03)	(0.05)	(0.02)
Global Matched Maturity	-0.03	0.00	-0.03	-0.02
	(0.05)	(0.03)	(0.05)	(0.02)
Observations (Firms)	62	129	62	129

Panel B. Matched Country Indices

	Equal-Weighted		Value-Weighted	
	USA	Foreign	USA	Foreign
Mean Adjusted				
DS All Lives Gov. Matched Country	-0.09	-0.03	-0.09	-0.06*
	(0.06)	(0.03)	(0.06)	(0.03)
Observations (Firms)	53	97	53	97
Matching Portfolio				
Bloomberg Matched Country	-0.04	0.01	-0.04	-0.02
	(0.06)	(0.03)	(0.06)	(0.03)
Observations (Firms)	61	128	61	128

Table 6. Cross-Sectional Regression Analysis of Abnormal Returns

This table reports the results of cross-sectional regressions where country- and firm-level variables of cross-listed foreign private issuers (FPIs) are used to explain cross-sectional variation in the abnormal returns of individual FPIs during the three-day, March 26-30 oral argument event. All the coefficient estimates are in percentage terms. Panel A presents results for US abnormal returns using the FPI-free MSCI World index, and Panel B repeats the analysis for home market returns. Panels C and D repeat the analyses using an alternative measure of exposure to US civil liability. *Non-US Value Traded* (used in Panels A and B) is the proportion of total value-traded in the first six months of 2009 that occurred on non-US stock exchanges. The regression is piecewise. *Below Median* equals *Non-US Value Traded* for firms below the median (0.41), and equals the median for firms above the median. *Above Median* equals 0 for firms below the median, and the difference between *Non-US Value Traded* and the median value for firms above the median. *Non-US Market Capitalization* (used in Panels C and D) is one minus the ratio of the market value of equity in the United States divided by the non-US company market value at the end of December 2009. This variable is also used in a piecewise fashion, splitting the sample on the median (0.75) and 0.60. *Rule of Law* from the World Bank is a measure of confidence that people abide by rules and the quality of enforcement. *Constraint Exec* is the XCONST variable from the Polity IV Project and measures constraints imposed on executive decision-making by institutions. This variable is identical to the EXCONST variable from the same source. *Disclosure* is an index of securities law disclosure rules and *Private Litigation* is an index of securities litigation rules, both from La Porta et al. (2006). *Class Actions* are US-style class actions from Hensler (2011). *Anti-Director Rights* is an index of shareholder protection laws from Djankov et al. (2008). *Public Enforcement* is the weighted size of staff of the securities regulation agency from Jackson and Roe (2009). *Tobin's q* is (market value of equity + total assets - common equity) / total assets. *Fixed Asset Ratio* is property, plant, and equipment divided by total assets. *Log (Total Assets)* is the logarithm of total assets. *Return on Equity* is earnings before interest, taxes, depreciation, and amortization divided by average common equity.. *Capital Expend* is capital expenditure divided by total assets. *Sales Growth* is one-year sales growth and controls for growth opportunities. *Leverage* is liabilities divided by total assets. *Log (GDP per capita)* is the logarithm of the GDP per capita of the home countries of individual FPIs. Standard errors are enclosed in parentheses and presented below the coefficients. ***, **, and * indicate that z-statistics are significant at the 1%, 5%, and 10% levels, respectively, according to bootstrapped standard errors using 5,000 replications. *N* is the number of observations. The control variables are from 2009.

Panel A. US Market Abnormal Returns, non-US Value Traded Proxies for Legal Exposure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Non-US Value Traded								
Above Median	2.55**	2.40*	2.65**	2.21	2.14	2.27*	2.99**	0.40
	(1.29)	(1.29)	(1.35)	(1.48)	(1.49)	(1.32)	(1.30)	(1.61)
Below Median	-3.35*	-2.44	-2.05	-1.13	-1.09	-0.27	-2.74	-0.44
	(1.85)	(1.93)	(2.63)	(2.75)	(2.85)	(2.97)	(2.46)	(3.22)
Fixed Asset Ratio	0 .64	0.78	1.43	1.22	1.23	1.12	1.58	2.28**
	(0.89)	(0.89)	(1.01)	(1.10)	(1.09)	(1.11)	(1.06)	(1.15)
Log(Assets)	-0.04	-0.03	-0.06	-0.05	-0.05	-0.09	-0.03	-0.06
	(0.15)	(0.14)	(0.15)	(0.15)	(0.16)	(0.17)	(0.15)	(0.15)
Return on Equity	-0.56	-0.51	-0.94	-0.97	-0.98	-1.21	-0.96	-1.77
	(0.81)	(0.84)	(1.01)	(1.09)	(1.05)	(1.13)	(1.01)	(1.64)
Capital Expenditure	6.19	4.54	3.34	4.77	4.86	6.13	3.75	5.78
	(4.13)	(3.92)	(4.54)	(4.35)	(4.25)	(4.61)	(4.53)	(5.05)
Sales Growth	0 .08	0.09	0.10	0.10	0.10	0.10	0.11	0 .13
	(0.24)	(0.20)	(0.17)	(0.23)	(0.23)	(0.23)	(0.22)	(0.34)
Tobin's Q	-0.13	-0.12	-0.16	-0.16	-0.16	-0.18	-0.16	-0.24
	(0.12)	(0.12)	(0.19)	(0.17)	(0.18)	(0.21)	(0.18)	(0.23)
Leverage	-0.20	-0.30	-0.29	-0.47	-0.46	-0.25	-0.44	0.45
	(1.38)	(1.35)	(1.49)	(1.54)	(1.56)	(1.86)	(1.44)	(1.46)
Log(GDP/Capita)	0.00	0.24	0 .01	0.10	0.13	-0.12	-0.17	0.44
	(0.18)	(0.31)	(0.23)	(0.25)	(0.27)	(0.32)	(0.19)	(0.35)
Rule of Law		-0.47						
		(0.39)						
Constraints Exec.			-0.59**					
			(0.26)					
Disclosure				-0.02				
				(0.11)				
Private Litigation					-0.30			
					(0.82)			
Class Actions						-0.29		
						(0.57)		
Anti-Director							-0.01	
							(0.17)	
Public Enforcement								-0.02
								(0.02)
Constant	1.15	-1.04	4.57**	-0.39	-0.62	1.85	2.33	-4.09
	(2.09)	(3.16)	(2.28)	(2.60)	(2.76)	(3.03)	(2.28)	(3.59)
N	507	498	404	390	390	333	411	269
p-value	0.48	0.63	0.04	0.40	0.34	0.09	0.27	0.07
R-squared	0.03	0.03	0.06	0.05	0.05	0.07	0.05	0.10
Adj. R-squared	0.01	0.01	0.04	0.02	0.02	0.04	0.03	0.06

Panel B. Home Market Abnormal Returns, non-US Value Traded Proxies for Legal Exposure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Non-US Value Traded								
Above Median	5.73***	5.56***	4.72***	4.63***	4.61***	3.54***	5.03***	1.91
	(1.46)	(1.36)	(1.30)	(1.45)	(1.41)	(1.17)	(1.31)	(1.39)
Below Median	-9.86***	-8.48***	-6.48***	-6.62***	-6.58**	-5.77**	-6.96***	-4.05
	(2.85)	(2.84)	(2.42)	(2.38)	(2.39)	(2.43)	(2.33)	(2.49)
Fixed Asset Ratio	2.52**	2.71***	1.94**	1.94**	1.95**	2.17**	2.17**	2.38**
	(1.05)	(1.03)	(0.89)	(0.92)	(0.93)	(0.87)	(0.89)	(1.01)
Log(Assets)	0.08	0.07	0.09	0.14	0.13	0.06	0.12	0.13
	(0.10)	(0.11)	(0.10)	(0.10)	(0.10)	(0.11)	(0.10)	(0.13)
Return on Equity	-0.23	-0.30	-0.17	-0.11	-0.12	-0.63	-0.17	-1.22
	(0.45)	(0.49)	(0.46)	(0.49)	(0.46)	(0.65)	(0.43)	(0.86)
Capital Expenditure	1.40	-0.95	2.26	2.30	2.46	2.31	2.06	2.07
	(4.77)	(4.77)	(3.74)	(3.76)	(3.83)	(4.02)	(3.87)	(4.09)
Sales Growth	0 .20	0.19	0.19	0 .20	0.20	0.18	0.21	0.17
	(0.16)	(0.19)	(0.15)	(0.18)	(0.19)	(0.15)	(0.17)	(0.28)
Tobin's Q	-0.14	-0.02	0.02	-0.03	-0.03	0.03	0.00	0.13
	(0.21)	(0.19)	(0.19	(0.19)	(0.18)	(0.21)	(0.20)	(0.22)
Leverage	-1.65*	-1.68*	-1.53	-1.87*	-1.86*	-0.95	-1.68*	-0.73
	(0.94)	(0.97)	(0.98)	(1.02)	(1.02)	(1.07)	(1.01)	(1.35)
Log(GDP/Capita)	-0.09	0.41	0 .08	-0.06	-0.04	0.11	-0.18	0.82**
	(0.21)	(0.38)	(0.24)	(0.26)	(0.26)	(0.31)	(0.20)	(0.33)
Rule of Law		-0.80**						
		(0.39)						
Constraints Exec.			-0.58**					
			(0.29)					
Disclosure				0.10				
				(0.10)				
Private Litigation					0.35			
					(0.62)			
Class Actions						-1.12**		
						(0.45)		
Anti-Director							-0.09	
							(0.18)	
Public Enforcement								-0.05***
								(0.02)
Constant	2.37	-2.11	3.24	-0.22	0.07	-0.29	2.06	-8.03**
	(2.55)	(3.68)	(2.37)	(2.66)	(2.73)	(3.04)	(2.32)	(3.34)
N	358	356	330	322	322	271	337	211
p-value	0.00	0.00	0.00	0.02	0.02	0.00	0.01	0.00
R-squared	0.10	0.11	0.10	0.08	0.07	0.12	0.09	0.14
Adj. R-squared	0.07	0.08	0.07	0.04	0.04	0.08	0.06	0.09

Panel C. US Market Abnormal Returns, non-US Market Capitalization Proxies for Legal Exposure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Non-US Market Cap								
(0.75, 1]	7.50**	8.90***	8.27***	8.05**	8.03**	7.97**	8.95***	3.74
	(3.31)	(3.19)	(3.05)	(3.38)	(3.48)	(3.66)	(3.04)	(4.73)
(0.60, 0.75]	4.47	5.65	7.11	6.02	6.11	1.91	8.00	-1.14
	(6.99)	(7.29)	(6.61)	(6.72)	(6.71)	(7.21)	(6.77)	(9.11)
(0, 0.60]	2.18	2.34	1.78	1.71	1.70	0.07	1.91	-0.73
	(1.63)	(1.57)	(1.82)	(1.84)	(2.01)	(2.12)	(1.85)	(2.33)
0	1.00	1.06	0 .01	-0.51	-0.50	-0.58	-0.33	-1.78
	(1.07)	(1.09)	(1.15)	(1.16)	(1.10)	(1.18)	(1.14)	(1.36)
Fixed Asset Intensity	0.17	0.67	1.51	1.26	1.24	1.12	1.52	2.32**
	(1.04)	(1.01)	(1.02)	(1.05)	(1.02)	(1.13)	(0.97)	(1.08)
Log (Total Assets)	-0.10	-0.07	-0.02	-0.03	-0.03	0.00	-0.02	-0.14
	(0.16)	(0.16)	(0.16)	(0.16)	(0.16)	(0.19)	(0.15)	(0.15)
Return on Equity	-0.49	-0.52	-1.07	-1.09	-1.09	-1.45	-1.07	-2.00
	(0.97)	(0.96)	(1.17)	(1.11)	(1.19)	(1.44)	(1.17)	(1.80)
Capital Expenditure	7.13	3.93	-1.21	1.05	1.00	1.86	-0.11	1.11
	(5.23)	(4.83)	(4.74)	(4.50)	(4.47)	(4.91)	(4.36)	(5.17)
Sales Growth	0.24	0.16	0.12	0.14	0.14	0.14	0.15	0.10
	(0.42)	(0.33)	(0.35)	(0.40)	(0.41)	(0.42)	(0.38)	(0.41)
Tobin's q	-0.11	-0.04	0.21	0.16	0.16	0.27	0.18	0.02
	(0.17)	(0.16)	(0.22)	(0.24)	(0.24)	(0.28)	(0.24)	(0.30)
Leverage	0.21	0.14	0.35	0.21	0.22	0.61	0.33	1.64
	(1.64)	(1.68)	(1.74)	(1.79)	(1.78)	(2.32)	(1.81)	(1.66)
Log (GDP per Capita)	0.13	0.55*	0.03	0.08	0.08	-0.29	-0.14	0.40
	(0.21)	(0.30)	(0.25)	(0.25)	(0.27)	(0.32)	(0.21)	(0.33)
Rule of Law		-0.83***						
		(0.32)						
Exec. Constraints			-0.65					
			(0.26)					
Disclosure				-0.05				
				(0.10)				
Private Litigation					-0.20			
					(0.74)			
Class Actions						0.28		
						(0.56)		
Anti-Director Rights							-0.15	
							(0.19)	
Public Enforcement								-0.01
								(0.02)
Intercept	-1.10	-4.98*	2.67	-1.72	-1.92	1.11	0.39	-4.08
	(2.16)	(2.82)	(2.14)	(2.27)	(2.43)	(2.97)	(2.24)	(3.15)
N	442	439	354	339	339	280	360	222
p-value	0.14	0.05	0.00	0.07	0.10	0.07	0.02	0.15
R-Squared	0.03	0.04	0.08	0.06	0.06	0.11	0.07	0.14
Adjusted R-Squared	0.0003	0.01	0.05	0.02	0.02	0.07	0.04	0.09

Panel D. Home Market Abnormal Returns, non-US Market Capitalization Proxies for Legal Exposure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Non-US Market Cap								
(0.75, 1]	8.93**	10.55***	11.07***	10.43***	10.44***	10.34***	11.74***	8.78**
	(3.61)	(3.49)	(3.35)	(3.54)	(3.61)	(3.18)	(3.29)	(4.14)
(0.60, 0.75]	4.72	7.46	8.06	7.59	7.28	3.47	9.75	9.17
	(7.72)	(7.58)	(6.91)	(7.02)	(6.84)	(6.14)	(6.76)	(8.00)
(0, 0.60]	1.55	2.10	1.89	1.57	1.59	1.03	2.26	1.99
	(1.62)	(1.56)	(1.48)	(1.51)	(1.54)	(1.60)	(1.56)	(1.73)
0	1.36	1.38	0.59	0.55	0.50	0.26	0.76	-0.41
	(1.64)	(1.48)	(1.19)	(1.26)	(1.25)	(1.15)	(1.25)	(1.41)
Fixed Asset Intensity	1.27	1.85*	2.01**	2.00**	2.00	2.56***	2.20**	2.92***
	(1.07)	(0.97)	(0.89)	(0.94)	(0.94)	(0.95)	(0.93)	(1.05)
Log (Total Assets)	0.05	0 .07	0.13	0.18	0.17	0.11	0.15	0.02
	(0.13)	(0.13)	(0.13)	(0.13)	(0.13)	(0.13)	(0.12)	(0.16)
Return on Equity	-0.02	-0.11	0.03	0.09	0.08	-0.45	0.06	-1.06
	(0.74)	(0.62)	(0.71)	(0.69)	(0.66)	(0.68)	(0.64)	(1.28)
Capital Expenditure	6.14	2.20	2.18	1.52	1.70	1.22	1.55	-0.22
	(5.38)	(4.70)	(4.46)	(4.37)	(4.46)	(4.64)	(4.44)	(4.96)
Sales Growth	0.38	0.27	0.42	0.44	0.44	0.38	0.46	0.22
	(0.35)	(0.32)	(0.44)	(0.48)	(0.46)	(0.39)	(0.46)	(0.42)
Tobin's q	-0.19	0.05	0.03	-0.03	-0.02	0.08	0.02	0.20
	(0.29)	(0.23)	(0.23)	(0.26)	(0.24)	(0.24)	(0.24)	(0.25)
Leverage	-1.83	-1.71	-1.84*	-2.21**	-2.20*	-1.38	-1.86	-0.37
	(1.22)	(1.14)	(1.09)	(1.12)	(1.16)	(1.13)	(1.14)	(1.42)
Log (GDP per Capita)	0.15	0.86**	0.13	-0.03	-0.02	0.18	-0.12	0.85**
	(0 .23)	(0.38)	(0.24)	(0.26)	(0.25)	(0.30)	(0.21)	(0.33)
Rule of Law		-1.18***						
		(0.39)						
Executive Constraints			-0.52*					
			(0.29)					
Disclosure				0.10				
				(0.10)				
Private Litigation					0.44			
					(0.61)			
Class Actions						-0.77*		
						(0.42)		
Anti-Director Rights							-0.16	
							(0.20)	
Public Enforcement								-0.05***
								(0.02)
Intercept	-2.61	-9.08***	-0.54	-3.27	-2.92	-4.18	-1.30	-9.72***
	(2.38)	(3.31)	(2.39)	(2.44)	(2.53)	(3.14)	(2.37)	(3.34)
N	318	317	294	286	286	235	300	180
p-value	0.02	0.00	0.00	0.01	0.03	0.00	0.00	0.00
R-Squared	0.06	0.09	0.11	0.09	0.09	0.17	0.11	0.19
Adjusted R-Squared	0.02	0.05	0.07	0.05	0.04	0.12	0.07	0.12

Table 7. Disclosure Quality Post-*Morrison*

This table examines the effect of the legal event on several facets of firm-level disclosure quality. In all panels, *Post-Morrison* equals 1 for foreign private issuers in quarters after the *Morrison* oral argument. *High NUSMC* equals 1 for firms with non-US market capitalization above the median at year-end 2009. Models with this variable use the *NUSMC* measure for 2009 described in the text. *FPI* is a dummy for foreign private issuer status; this variable is not collinear with the firm fixed effects because firms can change status. Model 1 shows a baseline specification including all firms. Model 2 excludes the financial sector (two-digit SIC codes between 60 and 69) because our sample period includes the financial crisis. Model 3 excludes OTC firms. Model 4 also excludes OTC firms, and includes an interaction with above median non-US market capitalization to test whether firms with less exposure to the United States behave differently.

Panel A examines the effect of Morrison on earnings management. Panel A1 shows results using the modified-Jones model using pre-Morrison quarters as the estimation period. We require that firms have at least 4 quarters of financial data during this period. Panel A2 presents results from a difference-in-differences model using non-foreign private issuers as a control group. All models in Panel A2 include firm and quarter fixed effects. Coefficients represent the change in discretionary accruals scaled by total assets in percentage points. Standard errors (in parentheses) are clustered by firm. The coefficients on *Post-Morrison* suggest that foreign private issuers did not manage earnings more aggressively as a result of the Supreme Court decision.

Panel B examines the effect of *Morrison* on earnings surprises. The dependent variable in all models is a standardized measure of unexpected earnings. A value of zero means earnings did not deviate from expectations; positive values indicate positive surprises while negative values indicate a failure to meet expectations. Panel B1 shows results using the approach of Foster et al. (1984) – specifically equation 8 and model 2 from that paper – to measure unexpected earnings. Like Foster et al. (1984), we require that firms have 10 consecutive earnings observations. Panel B2 calculates unexpected earnings from analyst expectations. All models include firm and quarter fixed effects. The coefficients on *Post-Morrison* generally suggest that foreign private issuers were not more likely to announce earnings surprises as a result of the Supreme Court decision.

Panel C examines the effect of *Morrison* on the tendency to change auditors. The dependent variable in all models is an indicator for whether a firm switched auditors in a given month. We identify auditor changes using form 8-K filings with the Securities and Exchange Commission. All models include firm and month fixed effects. The coefficients on *Post-Morrison* show that foreign private issuers were not more likely to switch auditors as a result of the Supreme Court decision.

Panel A. Earnings Management Post-*Morrison*

A1. Modified-Jones Model				
	(1)	(2)	(3)	(4)
	Baseline	No Finance	No OTC	High NUSMC
Cash Flow Method				
Post-*Morrison*	-0.94	-0.99	-0.07	-0.65
	(2.00)	(2.05)	(1.53)	(2.52)
Post-*Morrison* × High NUSMC				1.44
				(2.60)
High NUSMC				0.02
				(0.02)
Constant	-0.05**	-0.05**	-0.01	-0.02
	(0.02)	(0.02)	(0.01)	(0.02)
Observations	6,581	6,420	5,408	5,408
Balance Sheet Method				
Post-*Morrison*	1.03	-0.72	4.95	4.99
	(6.36)	(6.54)	(6.57)	(10.84)
Post-*Morrison* × High NUSMC				-0.11
				(11.65)
High NUSMC				0.07
				(0.14)
Constant	-0.05	-0.07	-0.06	-0.09
	(0.07)	(0.08)	(0.08)	(0.14)
Observations	7,800	7,215	6,459	6,459
A2. Difference in Differences				
Cash Flow Method				
Post-*Morrison* FPI	-1.85	-1.82	-0.53*	-0.71
	(2.10)	(2.12)	(0.31)	(0.45)
Post-*Morrison* FPI × High NUSMC				0.43
				(0.41)
FPI	0.78	0.70	-0.44	-0.35
	(2.70)	(2.75)	(0.42)	(0.46)
Observations	87,817	80,860	64,718	64,718
Firms	7,316	6,702	4,966	4,966
Balance Sheet Method				
Post-*Morrison* × FPI	-0.74	-0.91	0.02	-0.27
	(2.24)	(2.50)	(0.32)	(0.51)
Post-*Morrison* FPI × High NUSMC				0.65
				(0.56)
FPI	-4.22	-4.67	-1.11	-0.97
	(4.70)	(5.13)	(0.68)	(0.70)
Observations	108,002	91,469	80,304	80,304
Firms	8,722	7,067	5,956	5,956

Panel B. Earnings Surprises Post-*Morrison*

	(1) Baseline	(2) No Finance	(3) No OTC	(4) High NUSMC
B1. Earnings Forecast				
Post-*Morrison*	0.38	0.50	0.44	-0.32
	(0.65)	(0.74)	(0.71)	(0.29)
Post-*Morrison* × High NUSMC				1.87
				(1.66)
High NUSMC	-0.06	-0.19	-0.06	0.27
	(0.29)	(0.32)	(0.57)	(0.43)
Observations	78,399	62,834	73,895	73,895
Firms	4,009	3,188	3,665	3,665
B2. Analyst Expectations				
Post-*Morrison*	-1.19	-1.13	-1.27	-2.13**
	(0.76)	(0.85)	(0.82)	(1.00)
Post-*Morrison* × High NUSMC				2.39
				(1.67)
High NUSMC	0.12	0.10	0.11	0.54
	(0.30)	(0.32)	(0.63)	(0.67)
Analyst Following	-0.04	-0.05*	-0.04	-0.04
	(0.03)	(0.03)	(0.03)	(0.03)
Observations	101,439	81,401	95,251	95,251
Firms	5,151	4,094	4,736	4,736

Panel C. Auditor Changes Post-*Morrison*

	(1) Baseline	(2) No Finance	(3) No OTC	(4) High NUSMC
Post-*Morrison*	0.00126	0.00299	-0.00004	0.00049
	(0.00191)	(0.00204)	(0.00227)	(0.00250)
Post-*Morrison* × High NUSMC				-0.00403
				(0.00477)
High NUSMC	-0.00106	-0.00248	-0.00065	-0.00082
	(0.00248)	(0.00257)	(0.00379)	(0.00382)
Observations	670,440	498,660	443,844	443,844
Firms	9,103	6,862	6,121	6,121

Table 8. Class Actions and SEC Enforcement Post-*Morrison*

This table examines the effect of *Morrison* on the likelihood of class actions and SEC enforcement. Coefficients represent the percentage point change in the probability of a private or SEC action against the firm (i.e. the coefficients and standard errors from the linear probability model are multiplied by 100 for presentation purposes). *Post-Morrison FPI* equals 1 for foreign private issuers in quarters after the *Morrison* oral argument. *High NUSMC* equals 1 for firms with non-US market capitalization above the median at year-end 2009. Models with this variable use the *NUSMC* measure for 2009 described in the text. *FPI* is a dummy for foreign private issuer status; this variable is not collinear with the firm fixed effects because firms can change status. All models include firm and quarter fixed effects in addition to financial controls for the previous four quarters (not shown to conserve space). We include the same financial controls as in Table 5. Model 1 shows a baseline specification including all firms. Model 2 excludes the financial sector (two-digit SIC codes between 60 and 69) because our sample period includes the financial crisis. Model 3 excludes OTC firms. Model 4 also excludes OTC firms, and includes an interaction with above median non-US market capitalization to test whether firms with less exposure to the United States drive the results. The table shows results for both private and SEC actions, with SEC actions further categorized by type. The coefficients on *Post-Morrison FPI* suggest that foreign private issuers were less likely to be sued in private class actions after the *Morrison* case and that there was no change in the probability of SEC enforcement. The results are robust to dropping financial and over-the-counter firms. Estimates in column 4 indicate that firms with more market capitalization owned outside the United States drive the results for private class actions. Standard errors in parentheses are clustered by firm.

	(1) Baseline	(2) No Finance	(3) No OTC	(4) High NUSMC
Private Class Actions				
Post-*Morrison* FPI	-0.430***	-0.442***	-0.519***	-0.078
	(0.158)	(0.158)	(0.180)	(0.223)
Post-*Morrison* FPI × High NUSMC				-0.981***
				(0.338)
Any SEC Action				
Post-*Morrison* FPI	0.000	-0.010	0.027	0.009
	(0.025)	(0.026)	(0.026)	(0.017)
Post-*Morrison* FPI × High NUSMC				0.040
				(0.042)
SEC ALJ Decisions				
Post-*Morrison* FPI	0.016	0.014	0.019	-0.001
	(0.018)	(0.020)	(0.022)	(0.011)
Post-*Morrison* FPI × High NUSMC				0.044
				(0.041)
SEC Civil Cases				
Post-*Morrison* FPI	0.004	-0.003	0.011	0.012*
	(0.007)	(0.006)	(0.007)	(0.007)
Post-*Morrison* FPI × High NUSMC				-0.001
				(0.002)
SEC Administrative Proceedings				
Post-*Morrison* FPI	-0.001	0.001	0.020	0.002
	(0.022)	(0.024)	(0.023)	(0.013)
Post-*Morrison* FPI × High NUSMC				0.038
				(0.041)
Financial Controls	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	Yes	Yes
N	113,480	91,161	92,204	92,204
Firms	6,793	5,536	5,042	5,042

Table 9. Bid-Ask Spreads of Cross-Listed FPIs

This table reports the results of OLS regressions where country-level variables of cross-listed foreign private issuers (FPIs) are used in conjunction with a post-event dummy to explain the bid-ask spreads of the home market issues. The dependent variable is the difference between the ask and bid prices of the home market issues as a percentage of the closing price. Larger coefficients imply larger spreads. *Post-Event Dummy* is a dummy variable that is 1 for dates from March 31, 2010 to August 31, 2010 and 0 for dates from January 1, 2010 to March 25, 2010. The dependent variable was winsorized at the 99.5th percentile and the analysis was restricted to days in which an issue traded at least 500 shares. The left tail of the dependent variable was not winsorized because zero is a lower bound on the spread. *Non-US Value Traded* is defined as in Table 5. *Rule of Law* from the World Bank is a measure of confidence that people abide by rules and the quality of enforcement. *Constraint Exec* is the XCONST variable from the Polity IV Project and measures constraints imposed on executive decision-making by institutions. This variable is identical to the EXCONST variable from the same source. *Disclosure* is an index of securities law disclosure rules and *Private Litigation* is an index of securities litigation rules, both from La Porta et al. (2006). *Class Action* is US-style class actions from Hensler (2011). *Anti-Director Rights* is an index of shareholder protection laws from Djankov et al. (2008). *Public Enforcement* is the weighted size of staff of the securities regulation agency from Jackson and Roe (2009). The coefficients are mainly insignificant, and illustrate that the spreads of the home market issues were unaffected by the oral arguments in *Morrison*, contrary to the legal bonding hypothesis. Standard errors are clustered by firm and presented in parentheses below the coefficients. ***, **, and * indicate that *t*-statistics are significant at the 1%, 5%, and 10% levels, respectively. *N* is the number of observations. The data are from January 2010 through August 2010.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-Event	0.00	0.03	0.07	-0.08	-0.04	-0.02	0.00	-0.01
	(0.02)	(0.04)	(0.13)	(0.07)	(0.07)	(0.01)	(0.05)	(0.02)
Non-US Market Cap.								
[0.75, 1.0]	-3.85***	-3.24***	-3.52***	-3.55***	-3.37***	-1.61	-3.45***	-2.26
	(1.26)	(1.22)	(1.27)	(1.38)	(1.31)	(1.40)	(1.24)	(1.56)
[0.6, 0.75)	-3.78*	-3.63	-4.15*	-4.27*	-4.13*	-1.46	-4.05*	-3.54
	(2.26)	(2.29)	(2.26)	(2.32)	(2.17)	(2.43)	(2.16)	(2.57)
(0.0, 0.6)	-0.40	-0.33	-0.52	-0.56	-0.51	-0.18	-0.52	-0.40
	(0.54)	(0.53)	(0.53)	(0.57)	(0.53)	(0.55)	(0.53)	(0.56)
0	3.18	3.26*	4.42*	4.41*	4.43*	4.64*	4.41	5.44*
	(1.87)	(1.95)	(2.63)	(2.54)	(2.57)	(2.60)	(2.69)	(2.83)
Home Market Canada	0.59**	0.84***	0.65***	0.60**	1.05***	0.71***	0.63***	0.64**
	(0.26)	(0.27)	(0.25)	(0.24)	(0.27)	(0.23)	(0.23)	(0.29)
Home Market Australia	2.09**	2.22**	2.15**	2.42**	2.57**	2.38**	2.43**	1.19***
	(0.99)	(0.96)	(0.96)	(1.04)	(1.10)	(1.10)	(1.11)	(0.19)
Rule of Law		-0.20**						
		(0.09)						
Post-Event*Rule of Law		-0.03						
		(0.03)						
Constraint Exec			-0.07					
			(0.05)					
Post-Event*Constraint Exec			-0.01					
			(0.02)					
Disclosure				0.00				
				(0.03)				
Post-Event*Disclosure				0.01				
				(0.01)				
Private Litigation					-0.89***			
					(0.32)			
Post-Event*Private Litigation					0.07			
					(0.09)			
Class Action						0.15		
						(0.17)		
Post-Event*Class Action						0.03		
						(0.05)		
Anti-Director Rights							-0.02	
							(0.05)	
Post-Event*Anti-Director Rights							0.00	
							(0.01)	
Public Enforcement								0.00
								(0.01)
Post-Event*Public Enforcement								0.00
								(0.00)
Constant	1.08***	1.16***	1.45***	1.01***	1.44***	0.58**	1.06***	0.78***
	(0.22)	(0.23)	(0.48)	(0.30)	(0.30)	(0.24)	(0.35)	(0.24)
N	55494	55331	51948	50481	50481	45176	52918	34002
Clusters	437	434	367	354	354	311	372	238
p-value	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
R-squared	0.15	0.15	0.18	0.18	0.18	0.21	0.18	0.23
Adj. R-squared	0.15	0.15	0.18	0.18	0.18	0.21	0.18	0.23

Table 10. US Location of Trading in Stocks of Cross-Listed FPIs

This table reports the results of OLS regressions where country-level variables of cross-listed foreign private issuers (FPIs) are used in conjunction with a post-event dummy to explain the proportion of total monthly trading that occurs on US exchanges. The dependent variable is the percentage of value traded accounted for by US exchanges. Coefficient estimates represent the change in the share of total value traded that occurs in the United States associated with the independent variable. *Post-Event Dummy* is a dummy variable that is 1 for April to December of 2010 and 0 for the first three months of 2010. *Non-US Value Traded* is defined as in Table 5. *Rule of Law* from the World Bank is a measure of confidence that people abide by rules and the quality of enforcement. *Constraint Exec* is the XCONST variable from the Polity IV Project and measures constraints imposed on executive decision-making by institutions. This variable is identical to the EXCONST variable from the same source. *Disclosure* is an index of securities law disclosure rules and *Private Litigation* is an index of securities litigation rules, both from La Porta et al. (2006). *Class Actions* are US-style class actions from Hensler (2011). *Anti-Director Rights* is an index of shareholder protection laws from Djankov et al. (2008). *Public Enforcement* is the weighted size of staff of the securities regulation agency from Jackson and Roe (2009). The legal bonding hypothesis predicts migration of trading volume to US markets. The results do not suggest that trading migrated to the United States post-*Morrison*, which is inconsistent with this hypothesis. Standard errors are clustered by firm and presented in parentheses below the coefficients. ***, **, and * indicate that *t*-statistics are significant at the 1%, 5%, and 10% levels, respectively. *N* is the number of observations. The data are from January 2010 through December 2010.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-Event	0.23	-0.39	-1.11	1.65	0.72	0.63	-2.76*	0.90
	(0.40)	(0.71)	(2.42)	(1.73)	(1.21)	(0.63)	(1.57)	(0.90)
Non-US Market Cap.								
[0.75, 1.0]	-244.52***	-244.46***	-233.04***	-218.47***	-220.76***	-228.57***	-220.35***	-206.02***
	(16.18)	(16.36)	(17.13)	(18.63)	(18.91)	(22.62)	(17.01)	(23.90)
[0.6, 0.75)	-123.62***	-138.81***	-138.04***	-130.48***	-127.84***	-134.56***	-137.23***	-139.77***
	(45.66)	(42.33)	(39.62)	(41.11)	(39.77)	(43.22)	(39.32)	(50.28)
(0.0, 0.6)	8.88	8.84	-3.68	-1.28	-0.74	-6.86	-3.02	-4.09
	(8.63)	(8.75)	(8.78)	(8.53)	(8.72)	(9.00)	(8.59)	(10.01)
0	15.76***	14.87***	12.67*	12.49*	13.14	11.28	11.17	14.92*
	(4.91)	(4.76)	(7.32)	(7.51)	(8.01)	(8.19)	(7.80)	(7.81)
Rule of Law		-6.37***						
		(1.28)						
Post-Event*Rule of Law		0.60						
		(0.48)						
Constraint Exec			-3.44***					
			(1.27)					
Post-Event*Constraint Exec			0.22					
			(0.36)					
Disclosure				-0.56				
				(0.69)				
Post-Event*Disclosure				-0.15				
				(0.22)				
Private Litigation					-6.49			
					(4.79)			
Post-Event*Private Litigation					-0.27			
					(1.57)			
Class Action						-0.42		
						(3.34)		
Post-Event*Class Action						-0.36		
						(0.85)		
Anti-Director Rights							0.78	
							(1.21)	
Post-Event*Anti-Director Rights							0.84**	
							(0.39)	
Public Enforcement								0.11
								(0.11)
Post-Event*Public Enforcement								-0.02
								(0.03)
Constant	70.43***	77.09***	89.55***	68.83***	69.19***	65.90***	62.80***	61.68***
	(2.74)	(3.38)	(9.66)	(6.59)	(5.10)	(4.42)	(6.03)	(4.79)
N	5,347	5,312	4,509	4,337	4,337	3,688	4,584	3,014
Clusters	478	475	388	372	372	316	394	259
p-value	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
R-squared	0.51	0.54	0.49	0.45	0.45	0.49	0.46	0.44
Adj. R-squared	0.51	0.54	0.49	0.45	0.45	0.49	0.46	0.43

Table 11. Price Differences between US and Home Markets

This table reports the results of OLS regressions where country-level variables of cross-listed foreign private issuers (FPIs) are used in conjunction with a post-event dummy to explain the price premium of US listings relative to the home market listing. The dependent variable is the difference in the US and home market prices as a percentage of the home market price. *Post-Event Dummy* is a dummy variable that is 1 for dates from March 31, 2010 to August 31, 2010 and 0 for dates from January 1, 2010 to March 25, 2010. *Non-US Value Traded* is defined as in Table 5. *Rule of Law* from the World Bank is a measure of confidence that people abide by rules and the quality of enforcement. *Constraint Exec* is the XCONST variable from the Polity IV Project and measures constraints imposed on executive decision-making by institutions. This variable is identical to the EXCONST variable from the same source. *Disclosure* is an index of securities law disclosure rules and *Private Litigation* is an index of securities litigation rules, both from La Porta et al. (2006). *Class Action* is US-style class actions from Hensler (2011). *Anti-Director Rights* is an index of shareholder protection laws from Djankov et al. (2008). *Public Enforcement* is the weighted size of staff of the securities regulation agency from Jackson and Roe (2009). The data were winsorized at the 0.5^{th} and 99.5^{th} percentile and the analysis was restricted to days in which an issue traded at least 500 shares; standard errors are clustered by firm and appear in parentheses below the coefficients. The legal bonding hypothesis predicts that shares purchased on a US exchange would become more valuable owing to the legal rights uniquely assigned to those shares post-event. The insignificant, weakly significant, and sometimes negative sign of the coefficients on the post-event dummy are inconsistent with this hypothesis. ***, **, and * indicate that *t*-statistics are significant at the 1%, 5%, and 10% levels, respectively. *N* is the number of observations. Except for Panel C, the data are from January 2010 through August 2010.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-Event	0.06	0.17	-0.21	-0.35**	0.05	-0.16***	-0.40*	0.39*
	(0.07)	(0.13)	(0.46)	(0.18)	(0.14)	(0.04)	(0.23)	(0.21)
Non-US Market Cap.								
[0.75, 1.0]	1.61	1.57	2.09	2.21	1.88	-1.57	2.13	0.25
	(2.41)	(2.33)	(2.57)	(2.70)	(2.59)	(2.22)	(2.80)	(2.86)
[0.6, 0.75)	-3.45	-3.42	-3.23	-3.98	-4.33	-5.07	-4.94	-3.59
	(4.03)	(3.87)	(4.01)	(4.07)	(3.92)	(4.22)	(4.04)	(4.22)
(0.0, 0.6)	0.13	0.19	0.24	-0.80	-0.70	-1.07	0.11	-0.46
	(1.56)	(1.56)	(1.68)	(1.52)	(1.45)	(0.80)	(1.64)	(0.88)
0	-0.83	-0.78	-1.24	-1.37	-1.32	-1.17	-1.15	-0.96
	(0.90)	(0.90)	(1.31)	(1.27)	(1.26)	(1.22)	(1.23)	(1.42)
Rule of Law		-0.13						
		(0.26)						
Post-Event*Rule of Law		-0.10						
		(0.09)						
Constraint Exec			0.20					
			(0.26)					
Post-Event*Constraint Exec			0.04					
			(0.07)					
Disclosure				0.10				
				(0.11)				
Post-Event*Disclosure				0.06**				
				(0.03)				
Private Litigation					0.24			
					(0.34)			
Post-Event*Private Litigation					0.04			
					(0.15)			
Class Action						-0.66		
						(0.46)		
Post-Event*Class Action						0.15		
						(0.10)		
Anti-Director Rights							0.35	
							(0.25)	
Post-Event*Anti-Director Rights							0.12*	
							(0.07)	
Public Enforcement								-0.02
								(0.01)
Post-Event*Public Enforcement								-0.01*
								(0.01)
Constant	0.40	0.54	-0.97	-0.34	0.30	1.00	-0.93	0.75
	(0.39)	(0.48)	(1.75)	(0.89)	(0.45)	(0.67)	(1.11)	(0.59)
N	58,186	58,022	54,573	53,013	53,013	45,500	55,562	36,515
Clusters	457	454	383	368	368	312	389	252
p-value	0.47	0.57	0.65	0.53	0.65	0.00	0.54	0.71
R-squared	0.00	0.01	0.01	0.01	0.01	0.01	0.01	0.02
Adj. R-squared	0.00	0.00	0.01	0.01	0.01	0.01	0.01	0.02

Figure 1. Home Market Spread as Percentage of Home Market Price

Figure 1 shows the mean home market spread expressed as a percentage of the home market share price for the thirty business days surrounding March 29, 2010 (*x=0*). Spreads do not increase in the days surrounding the oral argument in *Morrison*, which does not support an inference that a weaker deterrence from US private enforcement led to greater adverse selection risk as the legal bonding hypothesis implies.

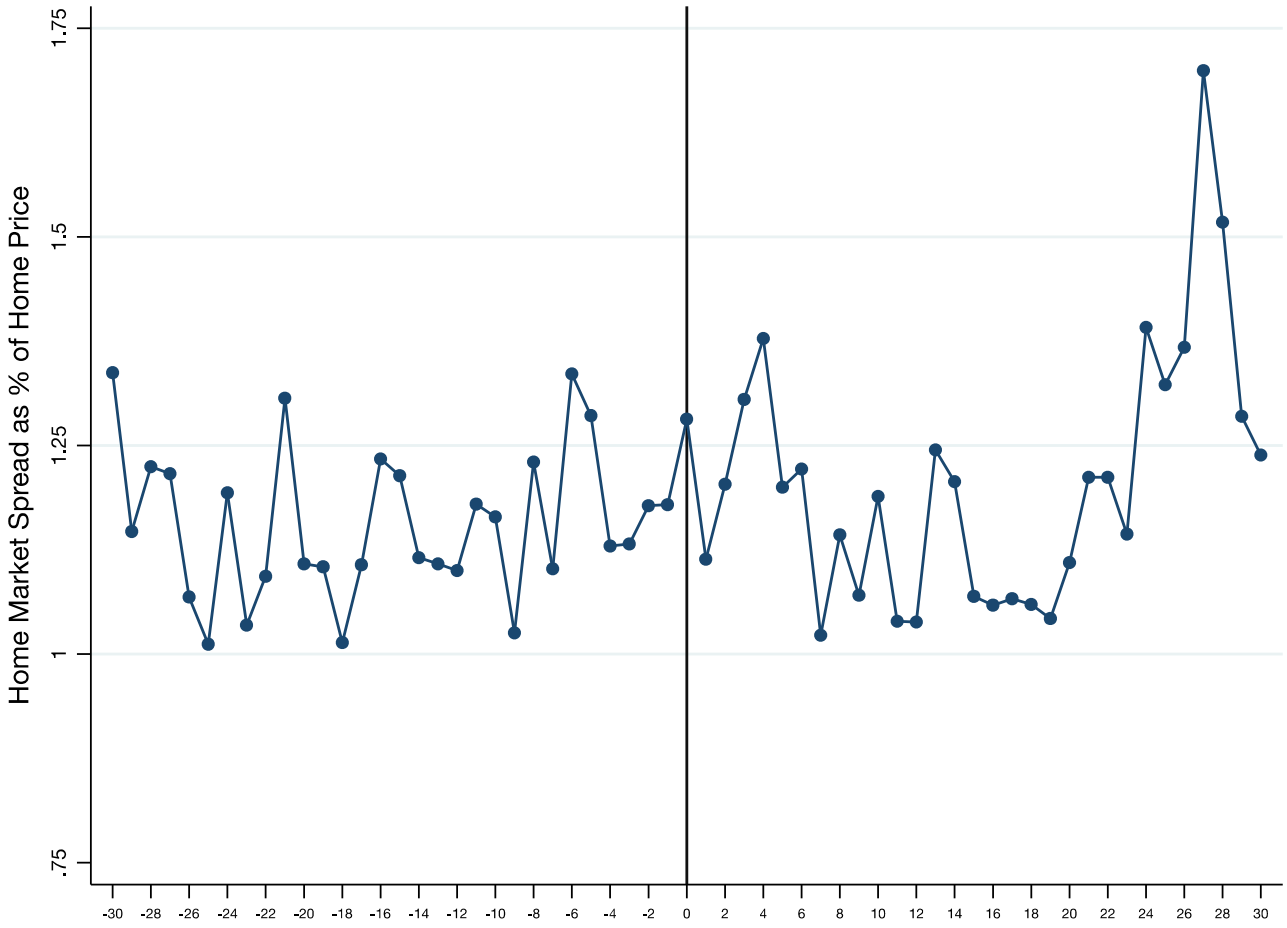
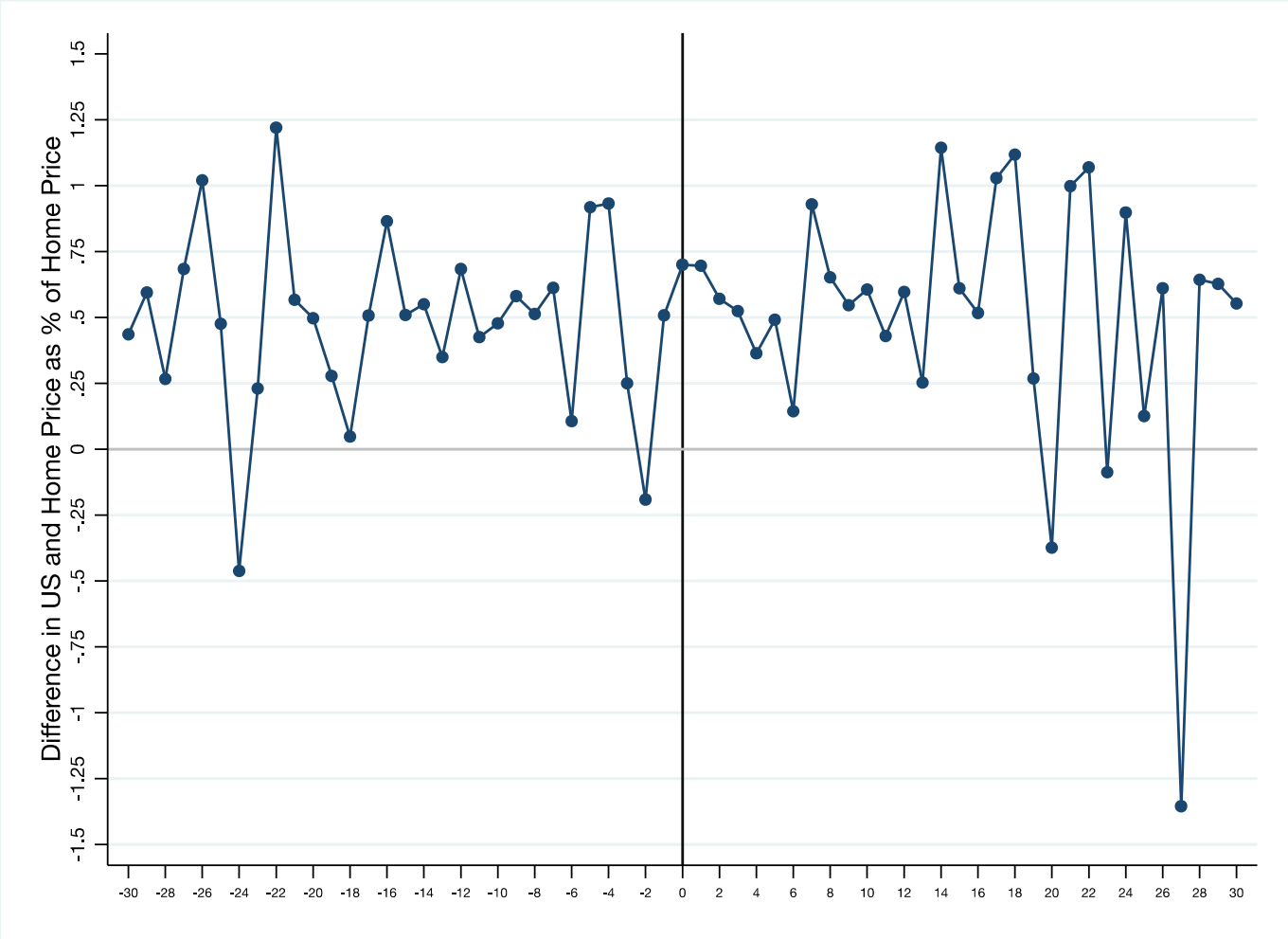


Figure 2. US Market Prices Relative to Home Market Prices

Figure 2 shows the mean difference in the US market and home market share prices expressed as a percentage of the home market share price for the thirty business days surrounding March 29, 2010 (*x=0*). There is no clear pattern in price premiums in the days surrounding the oral argument in *Morrison.* The institutional substitutes (legal bonding) hypothesis predicts that the U.S shares should become more valuable (the graph should move upwards) post-*Morrison*, when only transactions in those shares are afforded the protection of section 10(b). The lack of a revaluation suggests that the market did not see such protection as a source of value.



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