Spillover Effects of the SEC's Regulatory Oversight on Private Debt Contracting: Evidence from Cross-listed Foreign Firms

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ABSTRACT

We examine the effect of the SEC's regulatory oversight on private debt contracting outcomes, using the signing of the Multilateral Memorandum of Understanding (MMoU) as a natural experiment. The MMoU enables the SEC to take stricter punitive actions against wealth expropriation by cross-listed firms' insiders and enforce better compliance with applicable rules and regulations. We find that enhanced SEC oversight in the post-MMoU regime lowers loan spreads by 36 basis points, thus saving an average cross-listed firm approximately $9 million in direct loan costs over the life of a bank loan. Cross-sectional analyses show that the effect is more pronounced for borrowers from countries with weaker institutions, borrowers with greater accounting discretion, and for loans arranged by top lenders, and it is less pronounced when the SEC faces greater budgetary constraints. Enhanced SEC oversight also leads to an increase in loan maturity and a decrease in financial covenants. Our evidence suggests that while the SEC's primary mandate is to protect public equity and bond investors, its supervision yields substantial borrowing cost savings and more lenient non-price loan terms in the private debt markets as well.

Keywords: SEC oversight; Multilateral Memorandum of Understanding; bank monitoring; bank loans; debt contracting; cost of debt

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1. Introduction

Does the regulatory oversight of borrowers play a role in private debt contracting? While facilitating capital formation is a fundamental goal of policymakers, there is little direct evidence on whether regulatory oversight helps alleviate financing frictions in the private debt market. Given the ubiquity of regulations and the prominence of bank lending as a source of capital (e.g., Beck, Levine, and Loayza 2000; Shleifer 2010), this question remains important. In this study, we relate the regulatory oversight of the U.S. Securities and Exchange Commission (SEC) to borrowers' cost of capital in the private debt market.

We hypothesize that the SEC's oversight of borrowers reduces the cost of bank loans through two related mechanisms. First, enhanced SEC oversight of borrowers is likely to reduce the risk of corporate malfeasance and wealth expropriation by corporate insiders, thus mitigating banks' moral hazard concerns. As a result, banks may require less price protection and lower risk premium from borrowers when entering into loan contracts (e.g., Benmelech, Garmaise, and Moskowitz 2005; Bae and Goyal 2009). Second, theoretical studies on the role of banks as delegated monitors emphasize borrowers' incentives to avoid duplication of costly monitoring (Fama 1990; Diamond 1996), suggesting that the demand for bank monitoring may decline when the SEC's cross-monitoring of borrowers increases (e.g., Vashishtha 2014). Banks also have incentives to deter competition for lending relationships by passing along screening and monitoring cost savings to borrowers in the form of a lower cost of debt (Jensen and Meckling 1976; Diamond 1991; Blackwell and Winters 1997). Thus, in a competitive loan market, if the SEC's regulatory oversight reduces the demand for bank monitoring and/or saves banks' screening and monitoring costs, enhancement of the SEC's oversight can reduce the cost of bank loans.

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However, a bank's timely access to private information, sophistication in monitoring skills, and financial stake can make it a better monitor than the SEC, rendering the SEC's oversight redundant. Specifically, banks' access to private information facilitates active monitoring (Rajan 1992; Carrizosa and Ryan 2017), and concentrated ownership of bank loans reinforces monitoring (Qian and Strahan 2007; Gustafson, Ivanov, and Meisenzahl 2021). In contrast, the SEC does not have continuous access to borrowers' private information, nor does it have financial stakes in individual borrowers to motivate active monitoring. Further, the SEC's selective focus on egregious violators of applicable laws and standards may result in an under-provision of oversight for most firms (Shnitser 2010; Gunny and Hermis 2020). These arguments imply that increased SEC oversight may not impact the cost of bank loans.

Because the SEC's primary mandate is to protect public equity and debt investors,[1] the link between the SEC's oversight and private debt contracting may not be obvious. This may explain the paucity of direct evidence linking the SEC's oversight to bank loan contracting. Perhaps the most closely related evidence is by Ball, Hail, and Vasvari (2018), who find that enhanced oversight and transparency improvements following an equity cross-listing in the U.S. do not impact the cost of bank debt. They interpret this evidence as consistent with the argument that private lenders "already engage in active monitoring and rely on private access to borrower information (p. 390)" that obviates the need to rely on regulatory oversight. Other studies relate specific features of public enforcement (e.g., SEC comment letters) to private debt contracting, with results subject to contradictory interpretations.[2]

[1] The SEC states that "The mission of the SEC is to protect investors; maintain fair, orderly, and efficient markets; and facilitate capital formation. (https://www.sec.gov/about.shtml)". Prior studies focusing on equity and public debt markets show that enhanced SEC oversight mitigates IPO hyping (Li and Liu 2017), reduces bid-ask spreads (Johnstone and Petacchi 2017), and lowers offering yields in the public bond market (Ball et al. 2018).

[2] For example, Cunningham, Schmardebeck, and Wang (2017) find that banks charge higher interest rates when borrowers receive comment letters from the SEC. Relatedly, Li (2018) shows that the debt contracting value of

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More importantly, confounding events make it challenging to draw a conclusion on the relevance of the SEC's oversight to private lenders. For example, while equity cross-listing in the U.S. enhances regulatory oversight for foreign firms, it also coincides with improvements in growth opportunities that exacerbate debt-equity agency conflicts (Hail and Leuz 2009; Myers 1977). Similarly, while receiving a comment letter indicates that the SEC is monitoring a firm (e.g., Cunningham et al. 2017), it also signals underlying accounting issues with the firm that may independently affect the cost of bank debt (Costello and Wittenberg-Moerman 2011).

In this study, we provide first direct and causal evidence on how the SEC's oversight of borrowers impacts private debt contracting outcomes, using the staggered implementation of the Multilateral Memorandum of Understanding (MMoU). The promulgation of the MMoU affords the SEC broad powers to access bank, brokerage, and beneficial ownership records; ask for audit work papers and documents; seek witness testimony; and freeze and repatriate foreign assets of cross-listed foreign firms (Lang et al. 2020; Silvers 2020, 2021b). Anecdotal evidence suggests that the MMoU allows the SEC to enforce regulatory actions against asset tunneling, advancing prohibited loans to management, extraordinary payments to the departing CEO, and improper accounting practices (see Appendix A). Silvers (2020) finds that the probability that a U.S. cross-listed foreign firm will face SEC enforcement action increases by about three times after the signing of an MMoU with that firm's home country. Overall, the MMoU enables the SEC to take stricter punitive actions against wealth expropriation by cross-listed firms' insiders and enforce better compliance with applicable disclosure rules and securities regulations.

MMoU adoptions offer an ideal setting for our analysis. The impetus for MMoU adoptions was driven by regulators' attempts to curb terrorist financing and money laundering following the

accounting performance metrics declined following the passage of the Sarbanes-Oxley Act (SOX), which coincided with a significant expansion of regulatory oversight.

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9/11 terrorist attack. As the MMoU setting is plausibly exogenous to the borrower- and/or lender-specific factors, it alleviates omitted variable or reverse causality concerns. Moreover, exploiting the staggered implementation of the MMoU allows us to use both cross-sectional and time-series variations in the SEC's oversight to disentangle the effect of increased SEC oversight from macroeconomic and other confounding events that could otherwise bias the estimation.[3]

To test our prediction, we compare the cost of bank loans between the treatment and control groups before and after the implementation of the MMoU, using a generalized difference-in-differences (DiD) research design (e.g., Bertrand and Mullainathan 2003). The treatment group comprises foreign firms cross-listed in the U.S., and the control group comprises foreign firms domiciled in the same country as the treatment firms but not cross-listed in the U.S. We use country fixed effects to compare the treatment and control groups in a within-country setting. We require that our treatment firms are already cross-listed in the U.S. before the implementation of the MMoU and that sample firms do not change their cross-listing status following the implementation of the MMoU, thereby mitigating the effects of confounding events (e.g., changes in growth opportunities) that often coincide with U.S. cross-listing. We absorb industry-level time-invariant heterogeneities by including industry fixed effects and control for secular time trends by including year fixed effects. We also include loan-level and firm-level time-varying control variables, following prior research (e.g., Ferreira and Matos 2012; Chy and Kyung 2022).

Using a sample of 2,729 loan facilities issued to 891 unique firms domiciled in 31 countries during the sample period 1995–2018, we find that following the implementation of the MMoU, U.S. cross-listed firms experience a statistically significant reduction in the cost of bank loans relative to the control group of firms. The treatment effect is economically meaningful; loans to

[3] In contrast, the SEC's regulatory oversight of U.S. domestic firms applies to all U.S. domestic firms, making it challenging to draw credible inferences about how the SEC's oversight affects private debt contracting.

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U.S. cross-listed firms cost approximately 22.5% (36 basis points) lower following enhanced SEC oversight in the post-MMoU regime. This magnitude of the treatment effect is comparable to the economic magnitudes of effects documented in prior studies (e.g., Graham, Li, and Qiu 2008).[4] Using the approach of Francis et al. (2017) to quantify borrowing cost savings, we estimate that cross-listed firms save approximately $9 million, on average, in direct loan costs over the life of a bank loan in our sample. Thus, our key result implies that the SEC's oversight yields economically significant loan cost savings to borrowers in the private debt market.

A concern with our research design is that differences between treatment (ADR firms) and control (non-ADR firms from the same country of domicile) groups can confound our inferences. We use several approaches to mitigate this concern. First, we show that the effect on the cost of bank loans is not present in the pre-MMoU period and manifests only after the implementation of the MMoU, suggesting that pre-existing differential trends between cross-listed and non-cross-listed foreign firms do not drive our results. Second, we show that our results hold when we estimate the treatment effect using the treatment group only (that is, without any separate control group), assuaging concerns that our inferences are biased by unaccounted heterogeneity in treatment and control groups.[5] Third, we do not observe any significant effect on the cost of debt post-MMoU adoption for non-cross-listed firms domiciled in the MMoU-adopting countries. Hence, changes in the control group or confounding events contemporaneous with MMoU adoption in the home countries do not drive our results.

To enhance confidence in our inference, we show that the treatment effect exhibits predictable cross-sectional variations. First, we find that the treatment effect is more pronounced

[4] For example, Francis et al. (2017) show that bank loan spreads increase by 22% following auditor changes. Graham et al. (2008) find that financial restatements increase bank loan spreads by 65-72 basis points. Benmelech, Garmaise, and Moskowitz (2005) report that a decrease in asset liquidation value leads to a 58 basis point increase in loan spreads.
[5] We can estimate the treatment effect without a control group because the MMoU shocks are staggered.

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for borrowers domiciled in countries with weaker institutions, which suggests that banks rely more on SEC oversight when they have fewer protections against corporate malfeasance in the home country of borrowers. Second, the reduction in the cost of bank debt following MMoU implementation is less pronounced when the SEC faces more budgetary constraints that limit the effectiveness of its oversight. Third, the effect of MMoU on cross-listed firms' cost of bank loans is more pronounced for borrowers with greater accounting discretion, implying that the SEC's oversight enhances the credibility of borrowers' accounting discretion to private lenders. Last, the reduction in the cost of bank loans in the post-MMoU regime is more pronounced for loans arranged by top lead lenders, which is consistent with the idea that top lenders, who monitor more because of reputational concerns, are more likely to pass along the monitoring cost savings to borrowers when the SEC oversight increases.

We also test the robustness of our main findings by conducting a series of additional tests including using alternative sample selection choices, adding more restrictive fixed effects (e.g., firm fixed effects, country-year fixed effects), clustering standard errors by alternative variables, and testing for potential treatment effect biases in the staggered DiD approach outlined by Baker, Larcker, and Wang (2022). We continue to find that enhanced SEC oversight in the post-MMoU regime lowers loan spreads. We also rule out alternative explanations. Specifically, we show that changes in lenders' incentives following MMoU adoptions or changes in litigation risk for U.S. cross-listed foreign firms following the *Morrison* ruling do not drive our results.[6]

Finally, we examine changes in nonprice terms of bank loan contracts. Loan maturity for cross-listed firms increases relative to the non-cross-listed firms in the post-MMoU adoption period. Treatment firms also receive loans with fewer financial covenants. Collectively, our results

[6] This ruling eliminated on June 24, 2010, the right of shareholders who purchased shares from non-U.S. exchanges to pursue lawsuits in U.S. courts (Naughton et al. 2019).

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show that increased SEC oversight in the post-MMoU regime reduces loan spreads, lengthens loan maturity, and lowers financial covenant intensity in private lending agreements.

Our study makes several contributions to the literature. First, we provide first direct evidence that regulatory oversight by the SEC reduces the cost of bank loans. In particular, we exploit the staggered implementation of MMoU to capture the causal effect of enhanced SEC oversight on the U.S. cross-listed borrowers. We show that the SEC's oversight in the post-MMoU regime saves borrowers a substantial amount in direct loan costs, as well as leads to longer loan maturity and lower loan covenant intensity. Our results suggest that while the SEC's focus is on the public equity and debt markets, its oversight has spillover benefits in the form of borrowing cost savings and more lenient non-price loan terms in the private debt markets as well.

Second, our evidence speaks to the ongoing policy discussion regarding the effectiveness of the SEC's oversight. While the SEC has often been characterized as ineffective in public media and policy discussions (Eisinger 2002; Shnister 2010), our results show that even sophisticated investors like banks value the SEC's oversight. The findings also provide new insights into the conditions under which borrowers benefit from the SEC's oversight. For example, our results provide unique evidence that the effect of heightened SEC oversight is less (more) pronounced for borrowers when the SEC faces more (less) budgetary constraints. Relatedly, our evidence that the SEC's oversight yields important spillover benefits in the private debt markets is likely to be important to the SEC in evaluating the benefits and costs of its regulatory scrutiny (SEC 2021).

Third, we add to the literature evaluating the importance of public enforcement of securities laws. A common theme in this line of research is that it is the enforceability, not the existence, of laws and regulations that matters (Bhattacharya and Daouk 2002, 2009; Bae and Goyal 2009; Christensen, Hail, and Leuz 2013). We complement these studies by linking the SEC's enhanced

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enforcement to a reduction in bank debt costs. Our use of MMoU also directly responds to Leuz and Wysocki's (2016, p. 598) call for "more research on regulatory effects using experimental settings in which identification is given a priority. Such work could exploit natural experiments and, in particular, staggered implementations." Overall, the results in this paper enhance our understanding of how public enforcement affects stakeholders' incentives in the capital market.

The rest of the paper proceeds as follows. Section 2 reviews the institutional background of the MMoU, summarizes prior literature on SEC oversight and bank monitoring, and develops the hypothesis. In Section 3, we describe the sample and outline the research design. Section 4 reports the empirical results and Section 5 presents additional analyses. Section 6 concludes.

2. Institutional Background, Prior Research, and Hypothesis Development

2.1. Background of the MMoU

The International Organization of Securities Commission (IOSCO) developed the MMoU framework in May 2002 to enhance cross-border cooperation among securities regulators (Silvers 2020). The MMoU provides a standardized protocol for information sharing and cross-border enforcement actions among regulators in MMoU member countries (Lang et al. 2020). Silvers (2020) notes that "MMoU is a conduit designed to increase information flows (e.g., transfers of brokerage and beneficial ownership records, depositions, and testimony) and extend enforcement capabilities (e.g., restraining orders that freeze assets, reduce defendant flight risks, force the identification of accounts, and prohibit destruction of critical documents)."

Besides the U.S. SEC, which was among the early signatories of the MMoU, a large number of countries have adopted the memorandum since 2002.[7] The impetus to promulgate the MMoU came following the terrorist events of September 11, 2001, which spurred regulators

[7] See https://www.iosco.org/about/?subSection=mmou&subSection1=signatories for a comprehensive list of MMoU-adopting countries.

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around the world to prevent cross-border terrorist financing and money laundering. However, the MMoU enhances the SEC's overall oversight of U.S. cross-listed firms, not just malfeasance specifically relating to terrorist financing or money laundering. As Appendix A shows, examples of corporate malfeasance that have drawn the SEC's enforcement actions include transfers of assets to related entities, making loans to senior executives, extraordinary payments to former top management executives, fraudulent financial reporting, and improper accounting practices.[8]

Following prior studies (Lang et al. 2020, Silvers 2020), we conceptualize the MMoU setting as multiple staggered shocks to the SEC's oversight of cross-listed firms because cross-listed firms become subject to heightened SEC oversight once their home countries adopt the MMoU. Prior to the promulgation of the MMoU, the SEC faced considerable institutional barriers in enforcing regulatory actions against U.S. cross-listed firms (Silvers 2016; Lang et al. 2020). As such, SEC oversight of the cross-listed firms had been minimal (Lang et al. 2020; Silvers 2020). In contrast, the promulgation of the MMoU affords the SEC broad powers to access bank, brokerage, and beneficial ownership records; ask for audit work papers and documents; seek witness testimony; and freeze and repatriate foreign assets of cross-listed foreign firms (Lang et al. 2020; Silvers 2020, 2021b).[9] Silvers (2020) shows that the SEC increased its enforcement actions against cross-listed firms by 279% following the promulgation of the MMoU. In short, the MMoU affords the SEC broad powers to better identify misconduct and adopt enforcement actions against cross-listed foreign firms.

[8] We collect anecdotal evidence from the SEC litigation and press releases.

[9] Silvers (2020) also notes that "Unlike in bilateral arrangements, MMoU membership is all but required for participation in the global financial system: the IMF's Financial Sector Assessment Program and the Financial Stability Board each weigh MMoU membership when they consider a country's financial health, and IOSCO penalizes countries that are not part of the MMoU by revoking their IOSCO voting rights and membership (IOSCO, 2005). In most nations, a political motivation to stop money laundering and terrorist financing creates an important push for MMoU participation. One final incentive is that, by joining the MMoU, regulators can use the global support for IOSCO standards to justify needed changes to their laws."

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2.2. Prior Research

2.2.1 Prior Research on SEC Oversight

The mission of the SEC is to protect investors and facilitate capital formation. To this end, the SEC adopts several strategies, which include mandating compliance with certain disclosure practices, rewarding firm cooperation with leniency, and enforcing disciplinary actions (Kedia and Rajgopal 2011; Files 2012; Files, Martin, and Rasmussen 2019). In terms of the SEC's oversight of cross-listed foreign firms, prior research suggests that stricter SEC reporting requirements and enforcement yield higher valuation and cost of capital benefits for firms cross-listed in the U.S. (Reese and Weisbach 2002; Doidge 2004). Importantly, several recent studies show that the SEC's oversight of cross-listed firms increased following the implementation of the MMoU, leading to better reporting practices and voluntary disclosures, positive market reactions, and improvements in portfolio allocation (Silvers 2016, 2020, 2021b; Lang et al. 2020; Tsang et al. 2022).

However, a large stream of literature also questions the effectiveness of the SEC's regulatory oversight. For example, the SEC is more likely to focus on egregious violators of applicable laws and standards, suggesting that the SEC's oversight affects a small fraction of firms (Kedia and Rajgopal 2011; Gunny and Hermis 2020). Citing Blackburne (2014), Ege et al. (2020, p. 34) note that "budgetary constraints can affect the overall quality of SEC oversight." Studies also argue that regulators such as the SEC could also be susceptible to political and social connections (Stigler 1971; Correia 2014; Heese 2019; Mehta and Zhao 2020; Cao, Guo, and Yang 2021), corporate lobbying (Yu and Yu 2011), and scrutiny shopping (Calluzzo, Wang, and Wu 2021). Finally, prior evidence shows that cross-listed firms face lax enforcement from the SEC (Siegel 2005; Shnister 2010; Srinivasan, Wahid, and Yu 2015; Licht et al. 2018; Boone, Schumann-Foster, and White 2021).

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2.2.2 Prior Research on Bank Monitoring

Banks perform unique monitoring and governance functions (Diamond 1984, 1991; Ramakrishnan and Thakor 1984; Fama 1985, 1990; Allen 1990).[10] They have access to nonpublic proprietary information and possess economies of scale in screening information-problematic firms, giving banks a competitive advantage as delegated monitors (Ramakrishnan and Thakor 1984). As such, banks can improve information flows with the borrower through screening (Allen 1990) and active monitoring of borrowers (Diamond 1984).

Empirical studies suggest that banks develop lending relationships with borrowers allowing them to gather soft information about the firm and its management over time through private interactions, thus reducing information asymmetries and agency problems (Rajan 1992; Khan et al. 2019). Banks often require borrowers to provide nonpublic historical and forward-looking accounting information (Carrizosa and Ryan 2017; Demerjian, Donovan, and Jennings 2020). They also engage in monitoring of borrowers by using financial covenants as trip wires (Dichev and Skinner 2002), and through site visits or third-party appraisals (Gustafson, Ivanov, and Meisenzahl 2021), peer or supplier networks (Gong and Luo 2018; DeFranco, Edwards, and Liao 2021), as well as frequent renegotiations (Roberts 2015; Nikolaev 2018). Because bank loan ownership is typically concentrated, banks devote considerable resources to monitor each borrower continuously (Qian and Strahan 2007; Gustafson, Ivanov, and Meisenzahl 2021).

Other research finds that banks engage in monitoring borrowers' financial reporting quality (Bushman and Wittenberg-Moerman 2012; Frankel et al. 2020) as well as investment, financing, and operating decisions (Chava and Roberts 2008; Nini, Smith, and Sufi 2009, 2012; Beatty, Liao, and Weber 2010; Chava et al. 2019). Banks also enforce changes in borrowers' internal governance

[10] See Gorton and Winton (2003) and Saunders (2008) for comprehensive reviews of the role of banks as delegated monitors.

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structure, such as board independence, CEO compensation, retention, and succession (Marshall, McCann, and McColgan 2014; Balsam, Gu, and Mao 2018; Ferreira, Ferreira, and Mariano 2018; Akins, De Angelis, and Gaulin 2020). Recent evidence suggests that while banks possess superior monitoring capacity, they still rely on cross-monitoring by other stakeholders (Roberts and Edward 2010; Francis et al. 2012; Lin et al. 2013; Chakravarty and Rutherford 2017; Jiang and Zhou 2017; Chy, DeFranco, and Su 2021; Chy and Kyung 2022). In other words, lenders have incentives to avoid duplication of costly monitoring, which is consistent with the theories of cross-monitoring (Fama 1990; Diamond 1996).

2.3. Hypothesis Development

The enforcement capabilities under the MMoU enable the SEC to take stricter punitive actions against wealth expropriation by cross-listed firms' corporate insiders, thus lowering banks' moral hazard concerns and monitoring costs (Jensen and Meckling 1976; Fama 1990). Further, the SEC can enforce better compliance with applicable reporting and disclosure standards under the MMoU (Silvers 2021b), which reduces banks' screening costs (Bushman and Smith 2001; Armstrong, Guay, and Weber 2010; Costello and Wittenberg-Moerman 2011). The SEC's enforcement capabilities under the MMoU framework could also constitute a deterrent against corporate malfeasance by insiders. This follows because the SEC's enforcement actions impose significant monetary and reputational costs on firms and corporate insiders, such as the CEO and directors (Palmrose, Richardson, and Scholz 2004; Srinivasan 2005; Karpoff, Lee, and Martin 2008a; 2008b). Indeed, studies show that firms respond to the threat of SEC enforcement actions by improving compliance with applicable laws and standards and providing better voluntary disclosures (Lang, Lins, and Miller 2003; Nguyen and Nguyen 2017; Silvers 2021b; Tsang et al. 2022). In short, the SEC's enforcement capabilities under the MMoU mitigate the risk of wealth

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expropriation, enable the SEC to ensure better compliance with reporting standards, and act as a deterrent against corporate malfeasance by cross-listed firms. Hence, banks may require less price protection and lower risk premium when entering into loan contracts with cross-listed foreign firms (Benmelech, Garmaise, and Moskowitz 2005; Bae and Goyal 2009). Further, theory suggests that in a competitive loan market, banks have incentives to pass along the screening and monitoring cost savings to borrowers in the form of a lower cost of debt (Jensen and Meckling 1976; Diamond 1991; Blackwell and Winters 1997). Therefore, increased SEC oversight in the post-MMoU regime is likely to reduce the cost of bank loans for cross-listed firms.

The above discussion leads to our hypothesis, stated in the alternate form, as follows:

H1: MMoU adoption leads to a decrease in bank loan spreads for U.S. cross-listed foreign firms, ceteris paribus.

3. Research Design

3.1. Sample Selection

Our initial sample consists of loan facilities from the Loan Pricing Corporation's DealScan database with facility start dates from January 1, 1995, to December 31, 2018. Our sample begins in 1995 because the DealScan database becomes well-populated starting from 1995 (Carey and Hrycray 1999). We end the sample period in 2018 because the SEC signed into the Enhanced Multilateral Memorandum of Understanding (EMMoU) in 2019.[11] For a borrower to be in our sample, we require its home country to have signed into the MMoU before Dec 31, 2018. We remove loans to private firms, loans in which the borrower is in the financial industry (SIC codes 6000–6999), and loans in which the borrower is in the public sector (SIC codes 9000–9999). Following Ball et al. (2018), we also exclude loans with deal amounts below $10 million, as well

[11] The EMMoU further raises standards for information sharing among securities regulators.

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as loans with missing information on all-in drawn spread over LIBOR, maturity, and facility amount. Using the DealScan-Workscope link from Beyhaghi et al. (2021), we link the loan facility sample obtained from DealScan to Worldscope, the database used to obtain data for the other (borrowing) firm-level variables required for the analysis.[12]

Because testing our hypothesis requires a distinction between American Depository Receipt (ADR) borrowers from other borrowers, we identify ADR firms by extracting data from Compustat.[13] We focus on Level II and Level III ADRs because Level I ADRs are not subject to the SEC's oversight. We require each MMoU-adopting country to have at least one ADR firm and one non-ADR firm to be included in the sample. We use the up-to-date DealScan-Compustat link from Chava and Roberts (2008) to merge the ADR identification with our loan facility sample. The implementation of MMoU could potentially impact a firm's choice to cross-list in the U.S., which may confound the inferences. As such, we exclude firms that change ADR status following MMoU adoptions. We also remove Canadian firms from the sample because the SEC largely relies on Canadian regulatory oversight for U.S. cross-listed firms domiciled in Canada under the arrangement of the Multijurisdictional Disclosure System (MJDS).[14] We require a loan to be issued within ten years before or after a firm's home country adopts the MMoU.[15] Our final sample consists of 2,729 loan facilities issued to 891 unique borrowing firms located in 31 countries around the world. See Appendix B for the distribution of loan-level observations by country, along

[12] The link is available for download from WRDS (https://wrds-www.wharton.upenn.edu/pages/support/manuals-and-overviews/thomson-reuters/wrds-reuters-dealscan/overview-dealscan-worldscope-link/).

[13] Following prior studies, to identify foreign firms with an ADR that is active during the loan issuance year, we look for firm-year observations in Compustat that have "ADR" or "ADS" in the company name (CONM) or that have a non-missing and positive ADR ratio (ADRR). For further details, please refer to the Guide to ADRs and Research from WRDS (https://wrds-www.wharton.upenn.edu/pages/support/research-wrds/research-guides/guide-adrs-and-research/).

[14] The key inference is unaffected if we include Canadian firms in the sample. For more details on the MJDS, see https://www.sec.gov/corpfin/cf-manual/topic-16.

[15] In robustness tests (see Section 4.3.4), we show that our inferences are not sensitive to lengthening or shortening the event window centering the MMoU adoptions.

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with the country-level MMoU adoption dates.

3.2. Empirical Model

Our research design exploits the staggered adoptions of the MMoU by different countries. Specifically, we compare the costs of bank loans between the ADR foreign firms (treatment firms) and non-ADR foreign firms (control firms), before and after the adoption of the MMoU, in a generalized difference-in-differences (DiD) approach (e.g., Bertrand and Mullainathan 2003). As an illustrative example, consider two firms, A and B, both domiciled in the Netherlands. Firm A (treatment firm) is cross-listed in the U.S., whereas firm B (control firm) is not. When the Netherlands adopts the MMoU in 2007, the SEC's regulatory oversight of firm A in the post-2007 regime increases relative to the pre-2007 regime. Firm B, as it is not cross-listed, does not undergo such a change. We compute the change in treatment firm A's cost of bank loans before and after 2007 as well as the change in control firm B's cost of bank loans before and after 2007, and then compare the changes in cost for both firms. For all countries in the sample, we adopt a similar approach and compute the average treatment effect in a regression setting, which allows us to flexibly account for time-varying covariates and fixed effects.

We estimate the following specification using ordinary least squares (OLS) regression:

$$\begin{aligned} LogSPREAD_{ijt} &= \beta ADR_j \times MMoU_{jt} + \alpha ADR_j + \gamma MMoU_{jt} + LOANCONTROL_{ijt}\delta \\ &+ FIRMCONTROL_{j,t-1}\theta + COUNTRY_j + IND_j + LOANPURPOSE_i + YEAR_t \\ &+ \epsilon_{ijt}, \end{aligned} \tag{1}$$

where *i* indexes loan facility, *j* indexes borrower firm, and *t* indexes the year of loan inception. *LogSPREAD* measures the facility-level cost of a bank loan; it is computed as the natural logarithm of *SPREAD*, where *SPREAD* is the all-in-drawn spread over LIBOR in basis points. *ADR* is an

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indicator variable that equals one for loans issued to ADR foreign firms (treatment firms) and zero for loans issued to non-ADR foreign firms (control firms). *MMoU* is an indicator variable that equals one if a loan is issued after a firm's home country adopts the MMoU, zero otherwise. Our key independent variable is $ADR \times MMOU$, which equals one for a loan facility issued to a treatment firm (ADR foreign firms) after the adoption of the MMoU by the firm's home country and zero for a loan facility issued to a treatment firm before the adoption of MMoU, or issued to a control firm. In terms of H1, the key coefficient of interest is the DiD estimator β, which measures the change in bank loan spreads for U.S. cross-listed foreign firms following MMoU adoption relative to non-cross-listed foreign firms. Under H1, we expect β to be negative.

$LOANCONTROL_{ijt}$ is a vector of loan facility-level control variables, and $FIRMCONTROL_{j,t-1}$ is a vector of time-varying firm-level control variables. δ and θ are the column vectors of coefficients associated with the loan- and firm-level control variables. Following prior research (e.g., Ferreira and Matos 2012; Chy and Kyung 2022), the loan-level variables we control for include the logarithm of facility amount (*LogAMOUNT*), the logarithm of loan maturity in number of months (*LogMATURITY*), the logarithm of one plus the total number of covenants (*LogCOVENANT*), an indicator variable for secured loans (*SECURED*), and an indicator variable for the existence of performance pricing provisions (*PPP*). We also control for several firm-level variables measured as of the fiscal year-end immediately preceding the loan issuance. These firm-level variables include the log of sales (*SIZE*), total debt scaled by total book assets (*TOTALDEBT*), market-to-book value of equity (*MTOB*), R&D expenditures scaled by total book assets (*R&D*), profitability (*PROFITABILITY)*, shareholder payout in dividends and repurchases, scaled by operating income (*PAYOUT*), and stock return volatility (*RETVOL*). To mitigate the influence of extreme values, all continuous variables are winsorized at the 1% and

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99% levels. All variables are defined in Appendix C.

We note that the use of control firms from the same country as the treatment firms ensures that all time-varying country-level heterogeneities are differenced out. Further, because we use country fixed effects ($COUNTRY_j$), based on the borrower firm j's country of domicile, to compare treatment and control firms in a within-country setting, all time-invariant country-level heterogeneities are controlled for in our regressions. To further strengthen the inferences, we include industry fixed effects (IND_j), based on the borrower firm j's Fama-French 49-industry code, to absorb all time-invariant heterogeneities at the industry level. We include loan purpose fixed effects ($LOANPURPOSE_i$) because the primary purpose (e.g., general corporate, M&A deals) of the loan may affect the cost of debt (e.g., Murfin and Petersen 2016). The inclusion of year fixed effects ($YEAR_t$), based on loan i's inception year, accounts for secular time trends that may impact all firms in the sample. $\epsilon_{ijt} \sim N(0, \sum_g)$ is the error term that allows for within-group correlation. We cluster standard errors at the firm level in our main analyses and conduct robustness tests with alternative clustering choices (see Section 4.3.4). In line with prior studies (e.g., Qian and Strahan 2007; Ferreira and Matos 2012), we perform our analyses at the loan facility level.

4. Empirical Results

4.1. Descriptive Statistics

Table 1 presents the sample mean, median, and standard deviation for variables used in regression analyses. The mean and median all-in-drawn spreads are 159 and 125 basis points over LIBOR, respectively. The mean and median facility amounts are about $577 and $202 million, respectively, and the mean and median maturity is about 52 and 60 months, respectively. These loan characteristics fall within the range of descriptive statistics reported in prior studies (Ferreira

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and Matos 2012; Chy and Kyung 2022). Twenty-five percent of our sample loans are secured, and 9% have performance pricing provisions. The mean of *ADR* is 0.209, implying that approximately 21% of our loan facility sample is issued to the treatment firms. The mean of *MMoU* is 0.525; thus, approximately 53% of the loan facilities are issued in the post-MMoU regime. Other descriptive statistics for the firm-level variables are also consistent with those reported in prior studies (e.g., Ferreira and Matos 2012; Chy and Kyung 2022).

[Insert Table 1]

4.2. Correlations

Table 2 reports the correlations for variables used in our loan spread tests. Pearson (Spearman) correlations are presented below (above) the diagonal. Loan spreads are related to several features of the loan facility in the expected directions (e.g., Bradley and Roberts 2015). For firm-specific variables, the loan spread measure is positively correlated with leverage ratio and stock return volatility, and negatively correlated with firm size, market-to-book ratio, R&D ratio, and interest coverage ratio, indicating the importance of controlling for these variables in multivariate analysis.

[Insert Table 2]

4.3. Empirical Analysis

4.3.1 Main Results - Effect of MMoU on the Cost of Bank Debt

Table 3 presents the main regression results for alternative specifications of Equation (1) for the test of H1. In Column (1), we begin with a baseline specification in which we regress the cost of bank loans (*LogSPREAD*) on the treatment firm indicator *ADR*, the post-MMoU period indicator *MMoU*, the key variable of interest *ADR*×*MMoU*, as well as country, industry, loan purpose, and year fixed effects. To assess the baseline result, we do not include any time-varying

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control variables in Column (1). The estimated coefficient on our variable of interest, *ADR×MMoU*, equals -0.384 and is statistically significant (*t*-statistic = -3.54; $p < 0.01$), indicating that after MMoU adoption, loan spreads for cross-listed firms relative to non-cross-listed firms decrease significantly.

[Insert Table 3]

In Column (2), we include loan-facility characteristics that could influence the cost of bank loans as additional control variables. The coefficient on *LogAMOUNT* (*LogMATURITY*) is negative (positive) and statistically significant, indicating that larger loans carry lower interest spreads, while longer-maturity loans have higher interest spreads. The coefficients on *SECURED* and *LogCOVENANT* are positive and statistically significant, suggesting that loans requiring collateral and with more covenants are associated with higher interest costs. The estimated coefficient on *ADR×MMOU*, the primary variable of interest, is negative and statistically significant (*t*-statistic = -3.30; $p < 0.01$), indicating that loan spreads for cross-listed firms decrease relative to non-cross-listed firms after MMoU adoption.

In Column (3), we add firm characteristics to the regression estimation in Column (2). Spreads increase with firm leverage (*TOTALDEBT*) and stock return volatility (*RETVOL*), suggesting that firms with greater indebtedness and uncertainty pay higher costs of bank debt. The cost of bank debt is lower for bigger firms (*SIZE*), more valuable firms (*MTOB*), and firms that make a greater payout to shareholders (*PAYOUT*). The results in Column (3) show that after controlling for loan-facility and firm characteristics, the negative effect of the MMoU on the loan spread continues to be statistically significant. The estimated coefficient on *ADR×MMoU* is -0.255 (*t*-statistic = -2.89; $p < 0.01$). Thus, the inclusion of time-varying loan-level and firm-level control variables does not materially affect the key inference that the MMoU reduces the cost of bank

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loans for cross-listed firms relative to the non-cross-listed foreign firms. In sum, the results in Table 3 are consistent with our hypothesis that greater SEC oversight of cross-listed firms in the post-MMoU period causes banks to charge lower loan spreads relative to the pre-MMoU periods.

In terms of economic magnitude, Column (3) suggests that treatment firms experience a reduction of approximately 22.5% (= 1 − exp(-0.255)) in the cost of bank loans relative to control firms following the adoption of the MMoU. Benchmarked against the mean loan spread in the sample, the treatment effect in Column (3) suggests that treatment firms' loans cost approximately 36 basis points (= 158.7 × 22.5%) lower on average than those of control firms in the post-MMoU regime. In terms of the cost savings in dollar amounts, enhanced SEC oversight in the post-MMoU regime saves a typical borrower in our sample approximately $9 million in interest costs over the life of a bank loan.[16] The economic magnitude of the treatment effect is comparable to the magnitudes reported in prior studies. For example, Francis et al. (2017) show that auditor changes lead to an increase of 22% in bank loan spreads. Graham et al. (2008) find that financial restatements increase bank loan spreads by 65-72 basis points. Bae and Goyal (2009) estimate that the strengthening of property rights led to a decline of 67 basis points in bank loan spreads.

4.3.2 Parallel Trend and Dynamic Treatment Effect

The DiD framework we adopt in this study relies on the assumption that the differential effect on the cost of bank loans for U.S. cross-listed firms vis-à-vis the non-cross-listed firms is not present in the pre-MMoU periods. We follow prior studies (e.g., Bertrand and Mullainathan 2003; Chy and Hope 2021) to provide supporting evidence that the assumption is likely to hold in our setting. Specifically, we construct two pre-treatment effect indicator variables capturing the treatment effect two years (event-time = -2) and one year (event-time = -1) before country *j*'s

[16] For an average loan of 577.139 million in our sample, with an average maturity of 51.883 months, a decrease of 36 bps translates into approximately $9 million (=577.139×51.883/12×0.0036) in interest cost savings.

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MMoU adoption year. We also include post-treatment effect indicator variables capturing the treatment effect in the year of the MMoU adoption (event-time = 0), one year after the MMoU adoption (event-time = +1), as well as two and greater years after the MMoU adoption date (event-time $\geq$ +2). We regress the cost of bank debt measure (*LogSPREAD*) on these time-trend indicator variables, their interactions with the treatment firm indicator *ADR*, as well as all control variables and fixed effects. If the parallel trend assumption holds, we expect the coefficient estimates on the pre-treatment effect indicator variables to be indistinguishable from zero.

Figure 1 plots the coefficient estimates and associated confidence intervals for the pre-treatment and post-treatment time-trend indicator variables, which jointly capture the dynamic treatment effects of MMoU adoption on the cost of bank debt. As Figure 1 shows, the treatment effects in the pre-MMoU regime (event-times = -2 and -1) are close to zero and are statistically insignificant ($p > 0.10$). The treatment effect is also insignificant in the year of the MMoU adoption (event-time = 0). The treatment effect becomes statistically significant one year after the MMoU adoption (event-time = +1). After two-plus years following the MMoU adoption (event-times $\geq$ +2), the treatment effect is still negative and significant but the effect gradually settles down. The evidence in Figure 1 shows that the effect of MMoU implementation on bank loan spreads does not manifest before treatment assignment in the pre-MMoU period and only becomes gradually significant in the post-MMoU period. As such, the parallel trend assumption underlying the DiD framework is likely to hold in our setting.

[Insert Figure 1]

4.3.3 Separate Effects for Treatment and Control Samples

So far, our tests use a control sample of non-U.S. cross-listed foreign firms within the same country as the U.S. cross-listed foreign firms. Although cross-listed and non-cross-listed firms are

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from the same country, they may differ not just in cross-listing status but also in a variety of other ways that can influence our inferences. In Section 4.3.2 above, we mitigate this concern by examining the parallel trends in Figure 1, which shows that the treatment and control group of firms do not exhibit differential trends in the pre-MMoU period.

To further mitigate the concern that the choice of the non-U.S. cross-listed firms as a control group could confound our inferences, we estimate the treatment effects separately for the treatment and control samples in Table 4. In Column (1) of Table 4, we restrict the sample to U.S. cross-listed foreign firms (treatment group) and estimate the treatment effect based only on the treatment firms without using a separate control sample.[17] We can estimate the effect using the treatment-firms-only sample because the implementation of the MMoU occurred in staggered phases across different countries. While we have fewer observations and hence less power to detect the treatment effect, we continue to observe that the inference holds; the coefficient on *MMoU* (which captures the treatment effect of MMoU on the treatment firms absent any control group) is negative and statistically significant (t-statistic = -2.17, $p < 0.05$). Because the treatment-firms-only sample estimates the treatment effect without a control group, the heterogeneous traits between treatment and control firms cannot explain our results.

[Insert Table 4]

We next address the concern that concurrent changes may have occurred in the MMoU-adopting countries leading up to and during the adoption of the MMoU, which could bias our estimation. For example, the effect of the MMoU in lowering the cost of bank debt could simply be due to an improvement in governance mechanisms in the MMoU-adopting countries. This

[17] The total number of treatment (control) observations in Table 3 is 571 (2,158). Table 4, Column 1(2) has 565 (2,155) observations for the treatment (control) sample because singleton observations in each fixed effect category are dropped in fixed effects regressions.

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concern is mitigated, however, because the effects of concurrent changes, if any, will be differenced out as both the treatment and control firms in our sample are domiciled in the same countries. To further address this concern, we estimate the treatment effect for the control group. If concurrent changes in the MMoU-adopting countries drive our results, then we would observe the effect of the MMoU on the cost of bank loans for non-cross-listed firms in MMoU-adopting countries as well. Column (2) of Table 4 shows that the coefficient on *MMoU* (which captures the effect of MMoU on the control group of firms absent any treatment group) is materially indistinguishable from zero, both in terms of statistical significance and economic magnitude. Taken together, the results from the parallel trend test (Figure 1) and the inferences based on Table 4 suggest that the treatment effect we document occurs only for U.S. cross-listed firms after the adoption of the MMoU. The effect neither manifests before the MMoU adoption nor does it manifest for the control group of firms that are not subject to the SEC's oversight. These results indicate that the effect of MMoU on the cost of debt of the U.S. cross-listed foreign firms is due to the SEC's increased oversight of these firms.

4.3.4. Other Robustness Tests

In this section, we conduct several additional robustness tests. We include control variables in all robustness tests but do not present the coefficient estimates, for brevity.

Alternative Sample Choices

Our main results are based on a sample event window of up to ten years before to up to ten years after the MMoU adoption by each country (see Section 3.1 for more details on sample selection). To examine the sensitivity of the key inference to the choice of sample window, we estimate the treatment effect of the MMoU adoption on the cost of bank loans using two additional sample window choices in Columns (1) and (2) of Table 5 *Panel A*. In Column (1), we check if

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the key the result is sensitive to the shortening of the event-window by restricting the sample window to event-years -5 and +5 centered around the MMoU adoption (that is, five years before to five years after the MMoU adoption by each country). In Column (2), we examine the robustness of the main result to a longer event window and include all loans issued to the treatment and control firms issued between 1995-2018 (that is, we do not restrict the sample window to any particular event-periods). Columns (1)-(2) show that our key inference continues to hold.

Because foreign firms cross-listed in the U.S. could also be listed in stock exchanges of countries other than their home countries, the institutional changes in those countries may affect our inference. To mitigate this concern, Table 5 *Panel A*, Column (3) reports regression results after excluding loans issued to firms with stocks traded in exchanges of countries other than the U.S. and the borrowing firm's home country. Our inference holds.

In Column (4) of Table 5 *Panel A*, regression results are based on a sample after excluding loans issued to firms from the United Kingdom, a country with the largest number of observations in the sample (see Appendix B), which could disproportionately affect the treatment effect estimate. However, as Column (4) shows, our inference does not change when we remove U.K. firms from the sample. Overall, the evidence from these analyses highlights that our results are robust to alternative sample selection procedures.

Sensitivity to Fixed Effects and Clustering Choices

Our main results in Table 3 include fixed effects at the country, industry, loan purpose, and year levels. We do not employ borrower fixed effects because prior studies argue that loan-level analyses such as ours contain low within-firm variations, leading potentially to downwardly biased treatment effect estimates (e.g., Campello and Gao 2016; Chy and Kyung 2022). However, we recognize that firm-specific time-invariant heterogeneities could confound the treatment effect

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estimate. To mitigate this concern, we conduct a robustness test and estimate the treatment effect using borrower firm fixed effects regression in Column (1) of Table 5 *Panel B.* We continue to find that the SEC's increased oversight in the post-MMoU period leads to a reduction in the cost of debt for U.S. cross-listed foreign firms (t-statistic = -2.34; $p < 0.05$). Hence time-invariant heterogeneities at the firm level cannot explain our results.

We note that our main specification uses control firms from the same country as the treatment firms in a within-country setting; because treatment and control firms from the same country undergo the same time-varying country-level shocks, our main specification essentially differences out these time-varying country-level changes. To further mitigate the concern that country-level time-varying changes may impact our inference, we replace country and year fixed effects with more restrictive country-year (i.e., country×year) fixed effects in Column (2). This ensures that country-level time-varying changes are absorbed through separate country-year indicator variables for each country-year pair. Despite lower variations in this specification, our inference holds. In Column (3), we include both firm fixed effects and country-year fixed effects; the inference continues to hold.[18]

We cluster the standard errors at the firm level in our main specification. This ensures that the standard errors (statistical significance) are not understated (overstated) due to within-firm autocorrelation in the error term. In Columns (4)-(5), we check the robustness of our key inference to alternative choices of clustering. In Column (4), we run the main specification with country-year clustering instead of firm clustering. In Column (5), we include both firm fixed effects and country-year fixed effects as well as use country-year clustering. As Columns (4) and (5) show, clustering choices do not drive the statistical significance of our key result.

[18] Singleton observations are dropped in each fixed effect category, which results in a different number of observations in Columns (1)-(5) of Table 5 Panel B.

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Addressing Alternative Explanations

We consider two alternative explanations of our main result. In Column (1) of Table 5 *Panel C*, we examine whether our findings may be driven by changes in lender incentives due to enhanced SEC regulatory oversight. For example, lenders' incentives may change such that they are more stringent in screening borrowers in the post-MMoU period, which could explain the negative effect of the MMoU on the cost of bank loans. To address this concern, we control for lender × year fixed effects in Column (1) of *Panel C*, which ensures that our treatment effect removes all time-varying changes in lenders' incentives. Our results are not materially affected. In Column (2), we address the concern that our finding may be driven by changes in private litigation for cross-listed foreign firms. Specifically, private litigation risk for cross-listed foreign firms fundamentally changed in 2010 following *Morrison v. Australia National Bank* because shareholders who purchased shares of these firms from their domestic markets could still bring lawsuits against their companies in the U.S. courts prior to the Morrison ruling, but not so afterwards (Naughton et al. 2019). Thus, in Column (2), we restrict our sample to loans issued prior to 2010. The negative effect of the MMoU on treatment firms' loan spread continues to be statistically significant.

Treatment Effect Biases in Generalized DiD Specifications

Several recent studies suggest that treatment effect estimates could be biased in generalized DiD specifications that exploit staggered shocks (Callaway and Sant'Anna 2021; Goodman-Bacon 2021; Sun and Abraham 2021; Athey and Imbens 2022; Baker et al. 2022). The bias arises because earlier-treated firms are often used as control firms for later-treated firms in staggered DiD specifications. However, earlier-treated firms already experience treatment effects before they are used as controls for later-treated firms, leading to a "bad comparisons" problem in estimating the

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treatment effect (Baker et al. 2022).

We note that two features in our research design mitigate concerns about treatment effect biases due to the "bad comparisons" problem. First, Baker et al. (2022) show that treatment effect biases in generalized DiD specifications are more severe when there are no "never-treated" control firms (that is, when all firms in the sample are eventually treated). The bias becomes less pronounced as the sample contains more "never-treated" control firms. Because our sample contains control firms that remain untreated throughout the sample period, the likelihood of treatment effect biases is already mitigated.

Second, as Baker et al. (2022) note, the problem of "bad comparisons" arises because prior studies that rely on generalized DiD specifications often use earlier-treated firms as a control sample for later-treated firms. In contrast, we use each country's non-cross-listed firms as the control sample for the cross-listed treated firms in the same country by holding the borrowing firm's country fixed. Thus, our research design does not use the already-treated firms as a control sample for the later-treated firms.

Baker et al. (2022) suggest that when the long-run treatment effect persists in a generalized DiD, the likelihood of biases is greater. As we discussed above, the effect of MMoU on the costs of bank debt continues to be negative and statistically significant when we limit the sample window from five years before the MMoU adoption to five years after MMoU adoption, suggesting that the persistence of the long-run treatment effect does not confound our inferences. Next, we conduct stacked regression analyses in *Panel D* of Table 5, Columns (1)–(3), following the recommendation of Baker et al. (2022). In this framework, each country, with its treated cross-listed firms and a control group of non-cross-listed firms, constitutes a stack; thus, country fixed effects represent stack fixed effects. Following Baker et al.'s (2022) approach, we include event-

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year fixed effects instead of year fixed effects and estimate the treatment effect without any, and with, control variables in Columns (1) and (2), respectively. We find that the inference continues to hold in both columns. In Column (3), we restrict the sample window to event-years between -5 and +5 centered around the MMoU adoption by each country. The inference remains unaffected. Overall, the specific research design features of our setting, as well as the additional empirical analyses in *Panel D* of Table 5, suggest that the key inference is unlikely to be affected by biases arising from staggered DiD design.

4.4. Results of Cross-Sectional Analyses

To corroborate our hypothesis that the adoption of MMoU lowers bank loan spreads, we conduct several cross-sectional tests that exploit variations in institutional characteristics of the issuing firm's country of domicile, the SEC's resource constraints, borrowers' accounting discretion, and lender reputation. For the conditional analyses in this section, we estimate variations of the following model:

$$\begin{aligned} LogSPREAD_{ijt} = {} & \beta_1 ADR_j \times MMOU \times COND_VAR_{ijt} + \beta_2 ADR_j + \beta_3 MMOU_{jt} + \beta_4 COND_VAR_{ijt} \\ & + \beta_5 ADR_j \times MMOU_{jt} + \beta_6 ADR_j \times COND_VAR_{ijt} + \beta_7 MMOU \times COND_VAR_{ijt} \\ & + LOANCONTROL_{ijt}\,\delta + FIRMCONTROL_{j,t-1}\theta + COUNTRY_j + IND_j \\ & + LOANPURPOSE_i + YEAR_t \\ & + \epsilon_{ijt}, \end{aligned} \tag{2}$$

where $COND_VAR_{ijt}$ is a conditioning variable that moderates the effect of the MMoU on bank loan spreads. We explain the proxies below for the $COND_VAR_{ijt}$. For brevity, we do not present the coefficient estimates on the control variables when tabulating the regression results.

4.4.1. Institutional Strength

Our first conditioning variable is the strength of legal institutions in the home country of foreign firms. If banks perceive home-country institutional protection to be weak when entering

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loan agreements, then the incremental usefulness of the SEC's regulatory oversight to banks could be greater (e.g., Reese and Weisbach 2002). As a result, the treatment effect of the MMoU on the cost of bank loans should be more pronounced for U.S. cross-listed firms with weak home country institutions.[19]

For this conditional analysis, we use two proxies for institutional strength as partitioning variables. The first proxy is based on a country-level measure of the rule of law, "a summary indicator of the extent of compliance with laws and regulations" (Srinivasan et al. 2015, p. 1203). The rule-of-law index, which ranges from -2.5 to +2.5, captures "perceptions of the extent to which agents have confidence in and abide by the rules of society, and in particular the quality of contract enforcement, property rights, the police, and the courts, as well as the likelihood of crime and violence" (Kaufmann, Kraay, and Masttruzzi 2011). We code *RLINDEX LOW* as one if the rule of law index for the borrower firm country is below zero in the loan issuance year, zero otherwise. The second proxy is based on membership in the European Union, which requires its member countries to maintain strong institutions to facilitate law enforcement and investor protection (Daske et al. 2008; Christensen, Hail, and Leuz 2013). We code *Non-EU* as one if a borrower firm is not from a member country of the European Union at the facility issuance date, zero otherwise. We replace $COND_VAR_{ijt}$ with each of the two proxies in Eq. (2) and expect a negative coefficient on the three-way interaction term.

Regression results for this conditional analysis based on institutional strength are presented in Columns (1)–(2) of Table 6. We find that the effect of the MMoU on bank loan spreads is significantly more negative for borrowers in countries with a weaker rule of law and for borrowers

[19] Alternatively, if banks perceive the SEC's oversight and the legal institutions in the home country of foreign firms as complementary in ensuring creditor protections (e.g., Ball et al. 2018), the treatment effect of the MMoU on the cost of bank loans may be more pronounced for U.S. cross-listed firms with strong home country institutions.

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from non-EU countries. Thus, borrowers in countries with weak institutions exhibit a higher drop in bank loan spread after the signing of the MMoU, which is consistent with the notion that the SEC's oversight is more useful to banks when borrowers' home country institutions are weak.

[Insert Table 6]

4.4.2. SEC Resource Constraints

Our next conditioning variable is based on the SEC's resource constraints. Jackson and Roe (2009, p. 210) note that "higher budgets and greater staffing allow regulators to examine allegations of wrongdoing, to write its rules carefully, to conduct market surveillance and review filings, and to act more often to remedy, prevent, and punish wrongdoing." An implication is that budgetary resources available to regulators can affect the efficacy of public enforcement (see, e.g., Blackburne 2014). In other words, "the effectiveness of SEC's enforcement depends upon the amount of resources allocated to it" (Ege et al. 2020, p. 35). To the extent that fewer resources are allocated to the SEC for enforcement actions, banks may place less value on the SEC's regulatory oversight. Thus, we expect a less pronounced negative effect of MMoU implementation on the cost of loans when the SEC faces more resource constraints. To test this prediction, we construct an indicator for high budgetary constraints (*SEC BUDGET CONSTRAINT*), which equals 1 if a loan is issued during periods when the SEC's actual spending relative to budgeted spending was higher than the sample median, and zero otherwise. We replace COND_VAR_{ijt} with *SEC BUDGET CONSTRAINT* in Eq. (2). The regression result for this conditional analysis is presented in Column (3) of Table 6. The positive coefficient on the three-way interaction term suggests that the negative effect of the MMoU on bank loan spreads is offset when the SEC faces more resource constraints.

4.4.3. **Accounting Discretion**

Our next conditioning variable is based on the quality of borrowers' financial statements,

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which are an important source of information for lenders. In preparing financial statements, managers have considerable discretion to either convey the underlying fundamentals of the firm or to manage accounting numbers for opportunistic reasons. Thus, the credibility of managerial discretion underlying accounting estimates is important to lenders in private debt contracting (see, e.g., Bharath, Sunder, and Sunder 2008; Graham, et al. 2008; Kim, Song, and Zhang 2011; Chy et al. 2021). We use accruals to measure managers' discretion in accounting numbers. We construct an indicator variable (*ACCRUALS HIGH)*, which equals one if the borrower firm's total accruals scaled by total assets at the beginning of the loan issuance year is above the median of sample distribution within the loan issuance year and the borrower firm industry, zero otherwise. We replace $COND_VAR_{ijt}$ with *ACCRUALS HIGH* in Eq. (2). The regression result for this conditional analysis is presented in Column (4) of Table 6. The negative coefficient on the three-way interaction term suggests the negative effect of the MMoU on bank loan spreads is more pronounced when accounting discretion, as measured by accruals, is greater. This result is consistent with the notion that the SEC's regulatory oversight improves the credibility of managers' accounting discretion to private lenders.

4.4.4. Top Lender Reputation

Prior studies argue that top lenders are more likely to provide monitoring services due to reputational concerns (Pichler and Wilhelm 2001; Ball, Bushman, and Vasvari 2008; McCahery and Schwienbacher 2010; Gopalan, Nanda, and Yerramilli 2011). Investors also value the monitoring services provided by top lenders (Ross 2010), and firms agree to pay a higher cost of debt when entering loan contracts with top lenders (McCahery and Schwienbacher 2010). Therefore, in the pre-MMoU period with less SEC monitoring, top lenders are likely to charge higher spreads. This implies that the reduction in the cost of debt is likely to be more pronounced

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for loans arranged by top lenders, as they incur greater monitoring costs in the pre-MMoU period that can be saved and passed along to borrowers in the post-MMoU regime. Alternatively, the SEC's oversight may be less useful to top lenders because of their efficiency in monitoring borrowers, implying that the treatment effect may be less pronounced for loans arranged by top lenders.

For this conditional analysis, we construct an indicator variable *TOP10 LEAD ARRANGER*, which equals one if the majority of lead arrangers of a loan facility are ranked top 10 in terms of the total amount of loans arranged, zero otherwise. We replace $COND_VAR_{ijt}$ with *TOP10 LEAD ARRANGER* in Eq. (2). Column (5) of Table 6 shows that the reduction in the cost of bank loans in the post-MMoU regime is more pronounced for loans arranged by top lenders, which is consistent with the notion that top lenders passed along more savings in monitoring costs to borrowers in the post-MMoU regime. In an untabulated analysis, we alternatively define lead arranger based on the number of loans arranged and find consistent evidence.

The cross-sectional analyses show that the negative effect of MMoU adoption on the cost of bank debt varies along dimensions of the strength of legal institutions in the home country of borrower firms, the SEC's resource constraints, borrowers' accounting discretion, and lender incentives to monitor, as proxied by lender reputation. As such, banks value the SEC's oversight of borrowers in scenarios when such oversight is likely to be incrementally useful.

5. Additional Analyses

5.1. Other Contracting Terms

So far, we have focused on the effect of MMoU adoption on bank loan spreads. Prior literature documents that variations in enforceability or creditor protections affect the nonprice terms of bank loan contracts as well (Qian and Strahan 2007; Bae and Goyal 2009; Miller and

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Reisel 2012). To the extent that the MMoU changes the enforceability and regulatory power of the SEC over cross-listed foreign firms, it may also affect the nonprice contract terms between banks and U.S. cross-listed foreign firms that borrow from them. To test this prediction, we examine changes in loan maturity and loan covenants in Table 7.

[Insert Table 7]

Short-term loans allow lenders to review their lending decisions more frequently, an option likely to be more valuable when lenders have greater uncertainty about regulatory oversight (Bae and Goyal 2009). Longer maturity also exacerbates agency conflicts (Myers 1977; Barnea, Haugen, and Senbet 1980). Thus, when the SEC's regulatory oversight increases, the option value of frequent recontracting through short-term debt declines. Hence, based on prior theoretical insights (e.g., Diamond 2004) and empirical evidence (Bae and Goyal 2009), we expect an increase in loan maturity following the implementation of the MMoU. Column (1) of Table 7 reports the regression results using the log of loan maturity (*LogMATURITY*) as the dependent variable. The coefficient on *ADR*×*MMoU* is positive and statistically significant ($p < 0.01$), indicating that after MMoU adoption, cross-listed firms receive loans with longer maturity than non-cross-listed firms.

Next, we examine whether the SEC's regulatory oversight influences the use of financial covenants in private lending agreements. The need for financial covenants arises as a trip-wire mechanism to ensure greater monitoring of borrowers (Dichev and Skinner 2002) and to control agency costs (Roberts and Sufi 2009). Because banks' monitoring costs decline following the implementation of the MMoU, the demand for financial covenants may also decline.[20] Column (2) of Table 7 reports the regression results using the financial covenant intensity of a loan, measured

[20] We note that increased SEC oversight under the MMoU may also increase the use of financial covenants because the SEC oversight in the post-MMoU regime may facilitate better enforcement of covenant compliance (Hong, Hung, and Zhang 2016).

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by the log of one plus the total number of financial covenants included in the loan agreement, as the dependent variable.[21] We find a significant decrease in financial covenant intensity following the MMoU adoption, indicating that banks require less covenant protection when the SEC's oversight increases.[22]

5.2. Evidence from Public Bond Issues

Public bond investors also experience a reduction in the risk of wealth expropriation and moral hazard concerns when the SEC's regulatory oversight increases. Hence, we predict that the adoption of the MMoU also reduces the risk premium bondholders require in public debt markets (e.g., Ball et al. 2018). We examine the effect of MMoU adoption on public debt contracting for new bond issues, using data from Mergent's Fixed Income Securities Database (FISD). Consistent with the findings by Ball et al. (2018), we find that new bond issues are significantly less costly for treatment firms following the adoption of the MMoU (untabulated). This evidence that enhanced SEC supervision on cross-listed foreign borrowers affects the cost of public debt in the same direction as the cost of private debt corroborates our main argument.

6. Conclusion

In this paper, we investigate whether an increase in the SEC's regulatory oversight of U.S. cross-listed foreign firms affects the cost of bank loans issued to these firms. To answer this research question, we exploit the implementation of the MMoU, a cooperative arrangement among international securities regulators that gave the SEC broad enforcement powers (Silvers 2020, 2021a, 2021b). We hypothesize that the increased regulatory oversight by the SEC following the

[21] The sample size for covenant tests is smaller because the unit of observation is a loan package. Loan covenants are organized at the package level, with all facilities falling under the same set of covenants. If multiple facilities exist within the same loan package, we keep one facility with the greatest amount and longest maturity.
[22] In an untabulated analysis, we do not find significant changes in the use of non-financial covenants after countries adopt the MMoU.

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implementation of the MMoU leads to lower screening and monitoring costs for banks as well as lower moral hazard concerns. As a result, we predict U.S. cross-listed firms to exhibit a decrease in bank loan spreads after the implementation of the MMoU.

We find that following the implementation of the MMoU, U.S. cross-listed firms experience a significant reduction in the cost of bank loans relative to non-U.S.-listed foreign firms. When we control for an extensive array of loan-specific and borrower-specific characteristics, along with various fixed effects, our estimates imply a reduction in loan spread of approximately 36 basis points for treatment firms compared to the control firms. In cross-sectional analyses, we find that the decrease in loan spread is significantly larger for borrowers from countries with weaker institutions, for borrowers with greater accounting discretion, and for loans arranged by top lenders. We also find that after the MMoU, the decrease in loan spread is less when the SEC faces more budgetary constraints. In terms of changes in nonprice contract terms, cross-listed firms receive loans with greater maturity and lower covenant intensity relative to the non-cross-listed firms in the post-MMoU adoption period. Overall, our findings are consistent with the SEC's oversight being valued by banks, suggesting that the benefits of SEC enforcement go beyond the public equity and debt markets.

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Appendix A: Examples of SEC Enforcement Actions against Foreign Entities

An example of enforcement action against the transfer of assets to related entities
SEC enforcement against Fresenius Medical Care AG & Co. KGaA, a German company, for misconduct, i.e., improper payments through a variety of schemes, including using sham consulting contracts, falsifying documents, and funneling bribes through a system of third-party intermediaries.
https://www.sec.gov/news/press-release/2019-48

An example of enforcement action for making favorable loans to senior executives
SEC cease-and-desist order against a former CEO and CFO in regards to prohibited loans they received from their company, Stelmar Shipping Ltd., a Greek company. The former executives granted themselves interest-free loans from Stelmar Shipping Ltd. These loans constituted a violation of Section 13(k) of the Exchange Act. The cease-and-desist order also mentions a settlement offer between the SEC and the former executives.
https://www.sec.gov/litigation/admin/34-52865.pdf

An example of a temporary order to escrow payments while an SEC investigation occurs
SEC temporary order compelling Vivendi Universal, S.A., a French company, to place in escrow any extraordinary payments the company made or may make to their CEO (approximately $23 million) relating to a termination agreement. The escrow order is in relation to an SEC investigation into possible federal securities law violations by Vivendi and its directors or employees.
https://www.sec.gov/litigation/litreleases/lr18352.htm

An example of enforcement action for "misreporting" improper payments
SEC enforcement action against Hitachi, Ltd., a Japanese company, for inaccurately recording improper payments to the ruling political party of South Africa. Hitachi made payments to a politically connected front company in exchange for government contracts and recorded the payments as consulting fees or other legitimate payments. Hitachi agreed to pay the SEC $19 million to settle the charges. The SEC recognized the assistance from the African Development Bank and regulators in South Africa.
https://www.sec.gov/news/pressrelease/2015-212.html

An example of enforcement action for numerous improper accounting practices
SEC enforcement action charging two former executives from Panasonic Corp., a Japanese company. The executives' improper practices include misreporting consulting payments to government officials, circumventing internal accounting controls, giving false statements to the company's auditor, and recording revenue on backdated contracts. The SEC recognized the assistance from regulators in Switzerland, Canada, the U.A.E., Japan, Singapore, Malaysia, Australia, and Pakistan.
https://www.sec.gov/news/press-release/2018-290

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Appendix B: MMoU Adoption Dates and Number of Loan Observations by Country

	COUNTRY	MMoU Adoption Date	Number of Sample Loans
1.	ARGENTINA	6/12/2014	5
2.	AUSTRALIA	10/8/2002	45
3.	BELGIUM	4/3/2005	20
4.	BRAZIL	10/21/2009	87
5.	CHILE	11/22/2018	10
6.	CHINA	5/29/2007	110
7.	COLOMBIA	3/26/2012	13
8.	DENMARK	8/17/2006	7
9.	FINLAND	11/22/2007	14
10.	FRANCE	2/19/2003	91
11.	GERMANY	11/5/2003	108
12.	GREECE	10/18/2002	43
13.	INDIA	4/22/2003	248
14.	IRELAND	12/24/2012	64
15.	ISRAEL	7/2/2006	18
16.	ITALY	9/15/2003	18
17.	JAPAN	2/19/2008	104
18.	KOREA (SOUTH)	6/9/2010	110
19.	LUXEMBOURG	5/8/2007	22
20.	MEXICO	3/14/2003	55
21.	NETHERLANDS	11/22/2007	86
22.	NORWAY	12/11/2006	41
23.	RUSSIAN FEDERATION	2/16/2015	60
24.	SINGAPORE	11/17/2005	55
25.	SOUTH AFRICA	3/18/2003	48
26.	SPAIN	3/24/2003	54
27.	SWEDEN	5/17/2011	16
28.	SWITZERLAND	2/15/2010	68
29.	TAIWAN	3/15/2011	205
30.	TURKEY	11/14/2002	24
31.	UNITED KINGDOM	3/10/2003	880
	Total		2,729

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Appendix C: Variable Definitions

Variables	Definitions
Dependent Variable	
LogSPREAD	Log of all-in-drawn spread over LIBOR in bps at loan contract inception
Variables of Interest	
ADR	One if a loan facility is issued to an ADR firm, zero otherwise
MMoU	One if a loan facility is issued after the adoption of the MMoU by the borrower's country of origin, zero otherwise
Control Variables	
LogAMOUNT	Log of facility amount in U.S. dollars
LogCOVENANT	Log of one plus the total number of covenants
LogMATURITY	Log of maturity in number of months
MTOB	Market value of equity divided by book value of equity (Worldscope item 8001/item 3501)
PAYOUT	Common dividends plus stock repurchases divided by operating income ((Worldscope item 5376 + item 3499)/item 1250)
PPP	One if a loan includes performance pricing provisions, zero otherwise
PROFITABILITY	Net income before extraordinary items divided by sales (Worldscope item 1551/item1001)
R&D	R&D expenditures divided by total assets (Worldscope item 1201/item 2999)
RETVOL	Standard deviation of monthly stock returns over the one-year period prior to the facility start date. We require at least six monthly stock returns for the standard deviation computation.
SECURED	One if a loan is secured, zero otherwise
SIZE	Log of sales in $ millions (Worldscope item 1001)
TOTALDEBT	Total debt divided by total assets (Worldscope item 3255/item 2999)
Partitioning Variables	
ACCRUALS HIGH	One if the borrower firm's total accruals scaled by total assets ((Worldscope item18191 - item 4860)/item 2999) at the beginning of the loan issuance year is above the median of sample distribution within the loan issuance year and the borrower firm industry, zero otherwise
Non-EU	One if a borrower firm is not from a member country of the European Union at the facility issuance date, zero otherwise
RLINDEX LOW	One if the rule of law index for the borrower firm country is below zero in the loan issuance year, zero otherwise (https://info.worldbank.org/governance/wgi/)
SEC BUDGET CONSTRAINT	One if a loan is issued during periods when the SEC's actual spending relative to budgeted spending was higher than the sample median, zero otherwise
TOP10 LEAD ARRANGER	One if more than half of the lead arrangers of a loan are among the top 10 lead arrangers in terms of the total amount of arranged loans during the calendar year of the facility start date, zero otherwise.

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Figure 1: Time Trend in the Cost of Bank Debt Prior to and After MMoU Adoption

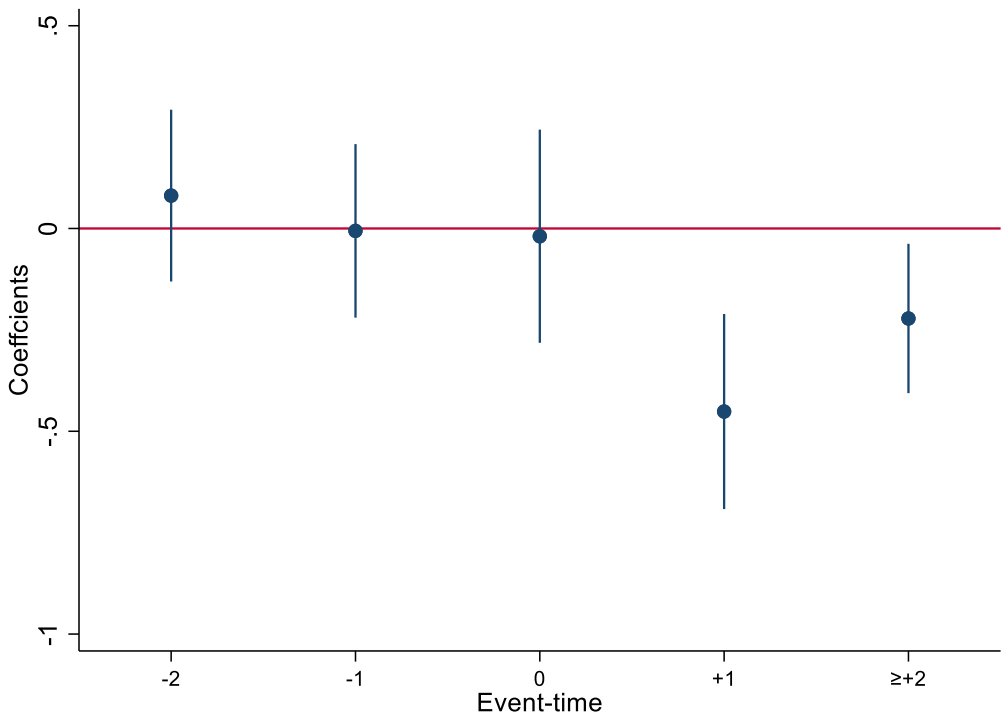

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Table 1: Sample Descriptive Statistics

This table reports descriptive statistics. The empirical analysis is conducted at the loan-facility level. See Appendix C for variable definitions.

Variables	N	MEAN	STDEV	MEDIAN
SPREAD	2,729	158.724	133.242	125.000
ADR	2,729	0.209	0.407	0.000
MMoU	2,729	0.525	0.499	1.000
FACILITY AMOUNT ($Mil)	2,729	577.139	1021.731	202.054
MATURITY	2,729	51.883	26.517	60.000
COVENANT	2,729	0.477	1.249	0.000
SECURED	2,729	0.250	0.433	0.000
PPP	2,729	0.091	0.287	0.000
SIZE	2,729	22.689	2.990	22.381
TOTALDEBT	2,729	0.314	0.158	0.308
MTOB	2,729	2.408	4.790	1.717
R&D	2,729	0.011	0.023	0.000
PROFITABILITY	2,729	0.060	0.122	0.056
PAYOUT	2,729	0.299	0.563	0.187
RETVOL	2,729	0.113	0.061	0.100

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Table 2: Correlations

This table presents pairwise correlations for variables used in our loan spread tests. Pearson (Spearman) correlations are presented below (above) the diagonal. Bold text indicates statistical significance at the 5% level. All continuous variables are winsorized at 1% and 99% levels. Variables are as defined in Appendix C.

	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o
a.LogSPREAD	1.00	**-0.17**	**0.31**	**-0.26**	**0.13**	**0.07**	**0.39**	**0.08**	**-0.25**	**0.14**	**-0.19**	**-0.18**	**-0.11**	**-0.24**	**0.30**
b.ADR	**-0.17**	1.00	**-0.10**	**0.28**	**-0.09**	**0.09**	**-0.10**	**0.12**	**0.17**	**-0.05**	**0.11**	**0.14**	**0.11**	**0.08**	**-0.04**
c.MMoU	**0.31**	**-0.10**	1.00	**0.13**	0.04	**0.12**	**0.13**	**0.10**	**-0.06**	**0.07**	-0.01	-0.00	0.00	-0.01	**-0.12**
d.LogAMOUNT	**-0.23**	**0.27**	**0.12**	1.00	**-0.10**	**-0.05**	**-0.17**	**0.07**	**0.14**	-0.03	**0.18**	**0.10**	**0.12**	**0.21**	**-0.26**
e.LogMATURITY	**0.19**	**-0.10**	**0.08**	**-0.11**	1.00	0.00	**0.26**	0.02	**-0.18**	-0.00	**0.04**	**-0.07**	**0.10**	0.02	**-0.05**
f.LogCOVENANT	**0.10**	**0.11**	**0.11**	**-0.07**	0.03	1.00	**0.14**	**0.71**	**-0.05**	-0.04	**0.05**	**0.16**	-0.00	-0.01	0.00
g.SECURED	**0.38**	**-0.10**	**0.13**	**-0.16**	**0.23**	**0.17**	1.00	**0.10**	**-0.17**	**0.08**	**-0.07**	-0.03	**-0.09**	**-0.12**	**0.11**
h.PPP	**0.08**	**0.12**	**0.10**	**0.06**	0.03	**0.71**	**0.10**	1.00	**-0.10**	-0.02	**0.10**	**0.10**	-0.01	-0.02	**-0.04**
i.SIZE	**-0.23**	**0.13**	**-0.10**	**0.09**	**-0.21**	**-0.09**	**-0.18**	**-0.11**	1.00	**0.09**	**-0.08**	**0.17**	-0.04	**0.04**	**-0.10**
j.TOTALDEBT	**0.16**	-0.04	**0.09**	-0.03	0.01	-0.02	**0.09**	-0.01	**0.09**	1.00	**-0.14**	**-0.14**	**-0.17**	**-0.16**	**0.07**
k.MTOB	**-0.07**	**0.04**	0.00	**0.07**	0.01	**0.07**	-0.00	**0.07**	**-0.06**	**-0.05**	1.00	**0.13**	**0.29**	**0.09**	**-0.18**
l.R&D	**-0.10**	**0.12**	0.00	**0.06**	**-0.08**	**0.14**	0.03	**0.13**	-0.02	**-0.16**	**0.12**	1.00	-0.03	**0.14**	**-0.06**
m. PROFITABILITY	**-0.12**	**0.08**	-0.01	**0.09**	**0.06**	0.00	**-0.09**	-0.01	0.00	**-0.17**	**0.11**	**-0.06**	1.00	**0.16**	**-0.19**
n.PAYOUT	**-0.12**	0.02	0.04	**0.09**	0.02	**0.06**	**-0.08**	**0.05**	**0.04**	**-0.09**	**0.07**	**0.07**	**0.08**	1.00	**-0.26**
o.RETVOL	**0.32**	-0.03	**-0.09**	**-0.19**	-0.03	0.01	**0.14**	**-0.04**	**-0.09**	**0.12**	**-0.09**	-0.00	**-0.23**	**-0.12**	1.00

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Table 3: Main Results – MMoU and Cost of Bank Debt

This table presents the regression results of the impact of MMoU on loan spread. *LogSPREAD* is the log of all-in-drawn spread over LIBOR in bps at loan contract inception. *ADR* equals one if a loan facility is issued to an ADR firm, zero otherwise. *MMoU* equals one if a loan facility is issued after the adoption of the MMoU by the borrower's country of origin, zero otherwise. The analysis is conducted at the loan-facility level. The sample period is 1995-2018. The event window is [-10, +10] years centered around the MMoU adoption for each country. Test statistics (two-sided), in parentheses, are based on robust standard errors clustered at the firm level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively. See Appendix C for other variable definitions.

	(1)	(2)	(3)
VARIABLES		LogSPREAD	
ADR×MMoU	*-0.384****	*-0.326****	*-0.255****
	(-3.54)	*(-3.30)*	*(-2.89)*
ADR	-0.120	-0.083	0.001
	(-1.43)	(-1.25)	(0.02)
MMoU	0.005	0.020	0.004
	(0.06)	(0.27)	(0.06)
LogAMOUNT		-0.110***	-0.037***
		(-8.24)	(-2.95)
LogMATURITY		0.084**	0.075**
		(2.35)	(2.41)
LogCOVENANT		0.136**	0.093*
		(2.17)	(1.84)
SECURED		0.380***	0.242***
		(7.85)	(5.82)
PPP		-0.032	-0.013
		(-0.35)	(-0.17)
SIZE			-0.132***
			(-9.40)
TOTALDEBT			0.601***
			(4.87)
MTOB			-0.005*
			(-1.67)
R&D			-0.531
			(-0.59)
PROFITABILITY			-0.173
			(-1.18)
PAYOUT			-0.084***
			(-3.27)
RETVOL			2.234***
			(6.36)
Country, Ind, Loan Purpose & Year FE	Yes	Yes	Yes
Observations	2,729	2,729	2,729
Adjusted R^2	0.520	0.589	0.665

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Table 4: Separate Effects for Treatment and Control Firms

This table presents the regression results of the impact of MMoU on loan spread separately for treatment and control firms. *LogSPREAD* is the log of all-in-drawn spread over LIBOR in bps at loan contract inception. *MMoU* equals one if a loan facility is issued after the adoption of the MMoU by the borrower's country of origin, zero otherwise. Column (1) uses the ADR foreign firms and Column (2) uses the non-ADR foreign firms from the MMoU-adopting countries. The analysis is conducted at the loan-facility level. The sample period is 1995-2018. The event window is [-10, +10] years centered around the MMoU adoption for each country. Test statistics (two-sided), in parentheses, are based on robust standard errors clustered at the firm level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively. See Appendix C for other variable definitions.

	Treatment Group (1)	Control Group (2)
VARIABLES	LogSPREAD	
MMoU	*-0.327***	*0.007*
	(-2.17)	*(0.09)*
LogAMOUNT	-0.051	-0.032**
	(-1.49)	(-2.44)
LogMATURITY	0.014	0.073**
	(0.34)	(2.05)
LogCOVENANT	0.071	0.074
	(0.72)	(1.50)
SECURED	0.241*	0.203***
	(1.84)	(4.51)
PPP	0.040	0.013
	(0.35)	(0.16)
SIZE	-0.139***	-0.129***
	(-3.22)	(-9.09)
TOTALDEBT	1.053***	0.506***
	(3.35)	(3.82)
MTOB	-0.011	-0.006*
	(-1.11)	(-1.90)
R&D	-0.086	-0.220
	(-0.04)	(-0.24)
PROFITABILITY	0.111	-0.301*
	(0.31)	(-1.86)
PAYOUT	0.048	-0.094***
	(0.82)	(-3.35)
RETVOL	4.643***	1.707***
	(5.65)	(4.59)
Country, Ind, Loan Purpose & Year FE	Yes	Yes
Observations	565	2,155
Adjusted R^2	0.744	0.659

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Table 5: Robustness Tests

In this table, we demonstrate the robustness of our main findings to different regression specifications and alternative explanations. *Panel A* reports results using alternative samples, *Panel B* tabulates results with alternative fixed effects and/or clustering choices, *Panel C* rules out alternative explanations, and *Panel D* employs stacked regression analyses. *LogSPREAD* is the log of all-in-drawn spread over LIBOR in bps at loan contract inception. *ADR* equals one if a loan facility is issued to an ADR firm, zero otherwise. *MMoU* equals one if a loan facility is issued after the adoption of the MMoU by the borrower's country of origin, zero otherwise. The analysis is conducted at the loan-facility level. Test statistics (two-sided), in parentheses, are based on robust standard errors clustered at the firm level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively. See Appendix C for variable definitions.

Panel A: Sensitivity to Alternative Sample Choices

	Shortening Event Window to [-5, +5]	Without Event Window Restriction	Excluding Firms Cross-listed in Two or More Countries	Excluding U.K. Firms
	(1)	(2)	(3)	(4)
VARIABLES	LogSPREAD			
ADR×MMoU	*-0.260****	*-0.195***	*-0.276***	*-0.278***
	(-2.81)	*(-2.20)*	*(-2.56)*	*(-2.37)*
ADR	0.033	-0.035	0.057	0.026
	(0.44)	(-0.60)	(0.85)	(0.35)
MMoU	-0.043	0.098*	0.017	0.001
	(-0.42)	(1.65)	(0.24)	(0.02)
Control Variables	Yes	Yes	Yes	Yes
Country, Ind, Loan Purpose & Year FE	Yes	Yes	Yes	Yes
Observations	1,592	3,423	2,545	1,849
Adjusted R^2	0.634	0.666	0.657	0.658

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Panel B: Sensitivity to Fixed Effects and Clustering Choices

	Firm Fixed Effects	Country × Year Fixed Effects	Firm and Country × Year Fixed Effects	Country × Year Clustering	Firm and Country × Year Fixed Effects, Country × Year Clustering
	(1)	(2)	(3)	(4)	(5)
VARIABLES			LogSPREAD		
ADR×MMoU	*-0.274***	*-0.252****	*-0.295***	*-0.255****	*-0.295****
	(-2.34)	*(-3.16)*	*(-2.43)*	*(-3.45)*	*(-2.65)*
ADR	0.024	-0.000	0.085	0.001	0.085
	(0.14)	(-0.01)	(0.48)	(0.03)	(0.61)
MMoU	0.006	-0.280	-0.319	0.004	-0.319
	(0.07)	(-1.11)	(-1.21)	(0.05)	(-1.43)
Control Variables	Yes	Yes	Yes	Yes	Yes
Country FE	No	No	No	Yes	No
Industry FE	No	Yes	No	Yes	No
Firm FE	Yes	No	Yes	No	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	No	No	Yes	No
Country × Year FE	No	Yes	Yes	No	Yes
Observations	2,387	2,619	2,282	2,729	2,282
Adjusted R^2	0.816	0.732	0.861	0.665	0.861

Panel C: Addressing Alternative Explanations

	Changes in Lender Incentives	*Morrison* Ruling (Changes in Litigation Risk)
	(1)	(2)
VARIABLES	LogSPREAD	
ADR×MMoU	*-0.310****	*-0.367****
	(-3.24)	*(-3.62)*
ADR	0.028	0.052
	(0.41)	(0.81)
MMoU	0.016	0.101
	(0.18)	(1.14)
Control Variables	Yes	Yes
Country, Ind, & Loan Purpose FE	Yes	Yes
Year FE	No	Yes
Lender × Year FE	Yes	No
Observations	2,128	1,891
Adjusted R^2	0.703	0.641

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Panel D: Treatment Effect Biases in Generalized Difference-in-differences Regression

	Stacked Regression Analyses		
	(1)	(2)	(3)
VARIABLES	LogSPREAD		
ADR×MMoU	*-0.371****	*-0.249****	*-0.262****
	(-3.36)	*(-2.87)*	*(-2.70)*
ADR	-0.079	0.020	0.031
	(-0.92)	(0.32)	(0.39)
MMoU	0.152	0.141	0.144
	(1.39)	(1.38)	(1.41)
Control Variables	No	Yes	Yes
Country, Ind, & Loan Purpose FE	Yes	Yes	Yes
Year FE	No	No	No
Event-Year FE	Yes	Yes	Yes
Observations	2,729	2,729	1,592
Adjusted R^2	0.476	0.639	0.614

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Table 6: MMoU and Cost of Bank Debt: Cross-sectional Variations in the Treatment Effect

This table reports the effect of the MMoU on loan spread. *LogSPREAD* is the log of all-in-drawn spread over LIBOR in bps at loan contract inception. *ADR* equals one if a loan facility is issued to an ADR firm, zero otherwise. *MMoU* equals one if a loan facility is issued after the adoption of the MMoU by the borrower's country of origin, zero otherwise. In Column (1), *RLINDEX LOW* equals one if the Rule of Law Index for a borrowing firm's country of origin is below zero in the loan issuance year, zero otherwise. In Column (2), *Non-EU* equals one if a borrower is not from one of the European Union countries at the facility issuance date, zero otherwise. In Column (3), *SEC BUDGET CONSTRAINT* equals one if a loan is issued during periods when the SEC's actual spending relative to budgeted spending was higher than the sample median, zero otherwise. In Column (4), *ACCRUALS HIGH* equals one if the firm's total accruals scaled by total assets at the beginning of the loan issuance year is above the median of sample distribution within the loan issuance year and the borrowing firm industry, zero otherwise. In Column (5), *TOP10 LEAD ARRANGER* equals one if more than half of the lead arrangers of a loan are among the top 10 lead arrangers in terms of the total amount of arranged loans during the calendar year of the facility start date, zero otherwise. The analysis is conducted at the loan-facility level. The sample period is 1995-2018. The event window is [-10, +10] years centered around the MMoU adoption for each country. Test statistics (two-sided), in parentheses, are based on robust standard errors clustered at the firm level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively. See Appendix C for variable definitions.

	(1)	(2)	(3)	(4)	(5)
VARIABLES			LogSPREAD		
ADR×MMoU×RLINDEX LOW	*-0.577****				
	(-3.16)				
ADR×MMoU×Non-EU		*-0.375***			
		(-2.30)			
ADR×MMoU×SEC BUDGET CONSTRAINT			*0.387***		
			(2.23)		
ADR×MMoU×ACCRUALS HIGH				*-0.323***	
				(-2.01)	
ADR×MMoU×TOP10 LEAD ARRANGER					*-0.461***
					(-2.14)
ADR×MMoU	-0.123	-0.079	-0.479***	-0.159*	-0.204**
	(-1.28)	(-0.74)	(-4.36)	(-1.71)	(-2.13)
ADR	-0.044	-0.088	0.042	-0.071	-0.008
	(-0.65)	(-1.17)	(0.59)	(-1.10)	(-0.13)
MMoU	0.038	0.071	0.526***	-0.054	-0.048
	(0.51)	(0.80)	(9.57)	(-0.70)	(-0.69)
RLINDEX LOW	0.240*				
	(1.73)				
SEC BUDGET CONSTRAINT			-0.081		
			(-1.55)		
ACCRUALS HIGH				-0.083*	
				(-1.76)	
TOP10 LEAD ARRANGER					-0.083
					(-1.02)
ADR×RLINDEX LOW	0.153				
	(1.08)				
ADR×Non-EU		0.208*			

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		(1.83)			
ADR×SEC BUDGET CONSTRAINT			-0.117		
			(-1.02)		
ADR×ACCRUALS HIGH				0.203**	
				(2.05)	
ADR×TOP10 LEAD ARRANGER					0.139
					(0.91)
MMoU×RLINDEX LOW	-0.104				
	(-0.86)				
MMoU×Non-EU		-0.112			
		(-1.33)			
MMoU×SEC BUDGET CONSTRAINT			0.215***		
			(2.80)		
MMoU×ACCRUALS HIGH				0.150**	
				(2.46)	
MMoU×TOP10 LEAD ARRANGER					0.300***
					(2.76)
Control Variables	Yes	Yes	Yes	Yes	Yes
Country, Ind, Loan Purpose, & Year FE	Yes	Yes	No	Yes	Yes
Observations	2,729	2,729	2,729	2,663	2,652
Adjusted R^2	0.669	0.668	0.573	0.668	0.659

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Table 7: Effects of the MMoU on Other Contracting Terms

This table reports the effect of the MMoU on loan maturity and covenant intensity. *LogMATURITY (LogFINCOV)* is the natural log of the loan maturity in number of months (one plus the total number of financial covenants). *ADR* equals one if a loan facility is issued to an ADR firm, zero otherwise. *MMoU* equals one if a loan facility is issued after the adoption of the MMoU by the borrower's country of origin, zero otherwise. The analysis is conducted at the facility (package) level in Column 1 (2). The sample period is 1995-2018. The event window is [-10, +10] years centered around the MMoU adoption for each country. Test statistics (two-sided) based on firm-level clustered standard errors are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively. See Appendix C for variable definitions.

	(1)	(2)
VARIABLES	LogMATURITY	LogFINCOV
ADR×MMoU	*0.278****	*-0.066***
	(3.34)	*(-2.11)*
ADR	-0.102	0.094***
	(-1.58)	(3.70)
MMoU	-0.029	0.023
	(-0.47)	(0.79)
LogSPREAD	0.091**	0.003
	(2.27)	(0.26)
LogMATURITY		0.003
		(0.20)
LogAMOUNT	0.005	-0.013*
	(0.33)	(-1.89)
LogCOVENANT	-0.003	
	(-0.06)	
SECURED	0.095**	-0.006
	(2.10)	(-0.22)
PPP	0.065	1.018***
	(0.80)	(37.76)
SIZE	-0.041***	-0.006
	(-2.99)	(-0.96)
TOTALDEBT	-0.069	0.026
	(-0.64)	(0.56)
MTOB	0.001	0.001
	(0.43)	(0.72)
R&D	-0.575	0.204
	(-0.74)	(0.44)
PROFITABILITY	0.226	0.014
	(1.64)	(0.23)
PAYOUT	0.027	0.016
	(1.26)	(1.33)
RETVOL	-1.086***	0.197
	(-4.00)	(1.20)
Country, Ind, Loan Purpose, & Year FE	Yes	Yes
Observations	2,729	1,869
Adjusted R^2	0.278	0.623

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