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The Effects of Financial Reporting on Bank Loan Contracting in Global Markets: Evidence from Mandatory IFRS Adoption

Tai-Yuan Chen, Chen-Lung Chin, Shiheng Wang, and Wei-Ren Yao

ABSTRACT: This study examines the effects of the mandatory adoption of International Financial Reporting Standards (IFRS) on the contract terms of bank loans in a global setting. Using a difference-in-differences design based on 26,474 bank loans in 31 countries during the 2000–2011 period, we find that borrowers who mandatorily adopt IFRS experience an increase in interest rates, a reduction in the use of accounting-based financial covenants, an increase in the likelihood that a loan is collateralized, a reduction in loan maturity, and an increase in the fraction of a loan retained by lead arrangers. These findings are robust to the removal of the 2008 financial crisis from our analysis, as well as to the matching of IFRS and non-IFRS borrowers on various country- and firm-level characteristics. Furthermore, we find that these changes are more pronounced for borrowers with greater financial reporting changes, as well as those with poorer accounting quality after IFRS adoption.

Keywords: information asymmetry; accounting quality; mandatory IFRS adoption; bank loan contracts.

JEL Classifications: G15; G21; F34; M41.

I. INTRODUCTION

Prior research provides substantial evidence for the influence of financial reporting in the design of loan contracts for U.S. firms. This research shows that, despite banks' superior information processing abilities and access to private information, financial reporting plays

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a critical role in determining the terms of bank loan contracts by reducing the information asymmetry between lenders and borrowers. Specifically, lenders look to a borrower's financial statements for information that they then use in considering more favorable loan contract terms such as lower interest rates, longer loan maturity, and fewer collateral requirements (Bharath, J. Sunder, and S. Sunder 2008; Graham, Li, and Qiu 2008; Costello and Wittenberg-Moerman 2011; Kim, Song, and Zhang 2011a). Furthermore, better financial reporting quality mitigates the moral hazard between lead arrangers and participant banks in syndicated loans, leading to more dispersed loan ownership for borrowers with better financial reporting quality (Sufi 2007).

Despite the effects of financial reporting documented for U.S. borrowers, the relationship between financial reporting and bank loan terms is not as well understood for borrowers outside of the U.S. One challenge in assessing the impact of financial reporting in an international setting is that financial reporting properties and financing decisions could be simultaneously determined by a country's legal origin or other institutional features, such as creditor rights protections or property rights enforcement (Djankov, McLiesh, and Shleifer 2007; Hong, Hung, and Zhang 2015), making it difficult to tease out the primary effects of financial reporting properties. To address this gap, our study examines the impact of borrowers' financial reporting properties on subsequent loan terms in a global setting. To do so, we use the 2005 mandated adoption of International Financial Reporting Standards (IFRS) to identify the impact of changes in financial reporting properties on bank loans for a sample of borrowers.

The setting of mandatory IFRS adoption provides several advantages for our study. First, since this reform is exogenous to individual firms, it mitigates potential endogeneity or self-selection concerns. Second, this setting allows us to use a difference-in-differences (DID) research design by comparing the changes in bank loan contracts for borrowers in countries adopting IFRS to those changes for borrowers in countries not adopting IFRS. This comparison allows us to mitigate potentially confounding factors, such as other economic conditions, that could affect bank loan contracts but are unrelated to mandatory IFRS adoption. Finally, the broad adoption of IFRS provides us with a sample of thousands of companies worldwide, thus allowing for a cross-sectional analysis.

At the conceptual level, we expect that mandatory IFRS adoption may impact the level of information asymmetry between borrowers and lenders in two possible ways. On the one hand, IFRS may improve borrowers' accounting quality and thus reduce the information asymmetry between lenders and borrowers. For example, IFRS eliminates certain accounting alternatives under local accounting standards; this may constrain borrowers' opportunistic earnings management and, by extension, decrease lenders' monitoring costs.¹ In addition, since IFRS emphasizes substance over form, it may improve the informativeness of accounting information with respect to the underlying economics and thus decrease the ability of management to use complicated transaction structures to circumvent accounting standards (Barth, Landsman, and Lang 2008; Langmead and Soroosh 2009). By limiting the ability to circumvent standards, IFRS adoption makes it easier and less costly for lenders to understand and monitor borrowers' operating activities. Furthermore, IFRS places greater emphasis on fair value accounting, which encourages more timely recognition of economic gains and losses. This, in turn, decreases the decision transfer time after a borrower suffers deterioration in credit quality. Under this perspective, the use of IFRS decreases the information risk and monitoring costs for lenders and thus leads to more favorable contract terms for borrowers.

¹ For example, IFRS prohibits the last-in-first-out accounting method for inventory. In addition, error corrections and voluntary changes in accounting policies must be accounted for only retrospectively, instead of through a choice of current income or retrospective correction.

On the other hand, IFRS may increase the information asymmetry between lenders and borrowers by introducing complexity due to switching to a new financial reporting system and by increasing complexity due to the subjectivity inherent in its accounting estimates. Specifically, since IFRS changes the way that current earnings and net assets map into future cash flow capacity, lenders must commit additional learning and monitoring costs to understand how the credit quality and the future cash flow capacity of borrowers are captured by financial statements prepared under IFRS. In addition, the principles-based, fair-value orientation of IFRS allows for more complex and subjective estimates, which may provide managers with greater flexibility and discretion for opportunistic earnings management (Ahmed, Neel, and Wang 2013). For example, Kim, Liu, and Zheng (2012) find that the greater judgment latitude and audit complexity created by IFRS adoption lead to increased auditing effort and subsequent fees. Similarly, the reporting complexity of IFRS requires lenders to exert more intense security over borrowers' accounting estimations and judgments, leading to higher information and monitoring costs. Furthermore, fair value is likely to incorporate transitory shocks to future expected cash flows into earnings, making current earnings a less powerful predictor of future cash flows from which the debt repayment will be serviced (Ball, Li, and Shivakumar 2013). This increased earnings volatility exacerbates lenders' estimation risks and makes borrowers more likely to break their financial covenants, leading to higher renegotiation costs. Therefore, under this perspective, IFRS may increase information asymmetry, monitoring requirements, and renegotiation costs. If this were the case, then we would expect borrowers who have adopted IFRS to receive more restrictive contract terms.

Given these two perspectives, we posit that mandatory IFRS adoption could have two opposing effects on the information asymmetry between borrowers and lenders, with the actual resultant impact becoming an empirical issue. To study this issue, we capture post-IFRS adoption changes in information asymmetry between borrowers and lenders using changes in the following loan contract features: interest rates, the intensity of accounting-based financial covenants, the requirement of collateral, loan maturity, and the portion of a loan held by its lead arrangers. Note that our study does not purport to provide a direct test of the effect of IFRS on information asymmetry. Instead, we draw on bank loan features to understand the implications of IFRS adoption on the information asymmetry between borrowers and lenders. We compare the changes of these loan features after IFRS adoption between two samples: (1) a treatment sample of 3,829 bank loans issued to 504 borrowers from 17 countries mandating IFRS adoption in 2005, and (2) a benchmark sample of 22,645 bank loans issued to 2,623 borrowers from 14 countries that have not mandated IFRS adoption as of 2005.

After controlling for loan- and firm-specific determinants of contract terms, as well as industry fixed effects, we find that interest rates increase by about 10 basis points after borrowers mandatorily adopt IFRS, representing a 7 percent increase compared to interest rates prior to IFRS adoption. By contrast, interest rates to borrowers from benchmark countries decrease by 24 basis points during the same period, representing a 12 percent reduction compared to the pre-IFRS period. Furthermore, we find that, relative to the benchmark sample, IFRS borrowers experience a significant reduction in both their use of accounting-based financial covenants and loan maturity; however, they experience a significant increase in collateral provisions, as well as the portion of their loans retained by lead arrangers. These findings are robust to the matching of treatment and benchmark samples on various country- and firm-level characteristics.

This study also explores the channels through which mandatory IFRS adoption impacts loan terms. Our cross-sectional analyses regarding this question yield several interesting findings. First, based on a firm-specific index for the differences between local accounting standards and IFRS, we find that borrowers whose pre-IFRS financial reporting deviates more from IFRS experience a greater *reduction* in accounting-based financial covenants, but a greater *increase* in collateral provisions and portions of their loans held by lead arrangers. These findings are consistent with the

prediction that greater accounting changes require banks to commit more effort and costs to understanding how a borrower's credit quality and future cash flow capacity are captured under IFRS. Second, we find that mandatory IFRS adoption has an uneven impact on borrowers' accounting quality, with some borrowers improving their accounting quality but others seeing a reduction in their accounting quality. More importantly, we find that borrowers who suffer deterioration in accounting quality after mandatory IFRS adoption experience a greater increase in interest rates, collateral provisions, and portions of their loans held by lead arrangers, as well as a greater decrease in the use of accounting-based financial covenants. These findings are consistent with the prediction that banks adjust loan contract terms in response to the higher information and monitoring costs associated with IFRS adoption.

Finally, we test the robustness of our results through various sensitivity tests, including limiting the sample to nonfinancial crisis periods, controlling for concurrent enforcement changes, expanding the sample to a larger population, controlling for country-level factors such as debt market characteristics and economic development, adding additional control variables in the regression, and jointly estimating the determinants of multiple loan contract terms using the seemingly unrelated regression method. In general, our inferences under these alternative specifications remain qualitatively similar to those of our main analyses.

Our study contributes to the existing literature in several important ways. First, our research design allows us to study the *causal* effects of financial reporting properties on loan contracts across different countries. While there is a large body of research examining the institutional determinants of bank loan contract terms across countries (Djankov et al. 2007; Hong et al. 2015; Esty and Megginson 2003; Qian and Strahan 2007; Bae and Goyal 2009; Lin, Ma, Malatesta, and Xuan 2012), these studies suffer from potential endogeneity concerns regarding country-level infrastructure and loan terms. To mitigate these concerns, our study uses an *exogenous* financial reporting reform together with a DID research design that controls for time-invariant country factors.

In addition, our study complements previous research that documents a significant cross-sectional association between financial reporting quality and debt contract terms in the U.S. (e.g., Bharath et al. 2008; Graham et al. 2008; Costello and Wittenberg-Moerman 2011; Chan, K. Chen, and T. Chen 2013). While studies in this stream of research focus on a single country, their settings also suffer from endogeneity concerns. For example, loan contract terms for the borrowers in their respective samples may be impacted by general firm characteristics such as size or performance, which are associated with factors such as accounting restatements (Graham et al. 2008), the disclosure of internal control weaknesses (Costello and Wittenberg-Moerman 2011), or the adoption of clawback provisions (Chan et al. 2013). In another study, Kim, Tsui, and Yi (2011b) examine loan contract terms for a set of voluntary IFRS adopters and find that banks charge lower interest rates, set less restrictive covenants, and offer longer maturity for voluntary IFRS adopters. However, their voluntary adopters are nonrandom and are thus subject to a potential self-selection bias. Our research expands on this previous body of work by considering the exogenous change in accounting standards created by IFRS adoption, thereby providing robust evidence for the causal impact of financial reporting on loan terms. Furthermore, some previous studies focus on a country-level aggregate flow of bank loans (Beneish, Miller, and Yohn 2012), which may reflect changes in investment flow patterns due to financial globalization (Doidge, Karolyi, and Stulz 2010, 2013). By contrast, we conduct a subsample analysis of the channels through which IFRS adoption affects loan contract terms, thus providing greater confidence in our results.

Our study also adds to a growing literature that examines the impact of mandatory IFRS adoption on debt markets (Beneish et al. 2012; Florou, Kosi, and Pope 2013; Florou and Kosi 2013; Kraft and Landsman 2014). A concurrent study, Ball et al. (2013), also finds a decrease in the use of financial covenants after IFRS adoption. However, our study differs from theirs in two ways. First,

similar to other concurrent studies examining the effects of IFRS adoption on debt markets, they focus on one single dimension of loan contract terms, financial covenants. In contrast, we examine the relationship between IFRS adoption and multiple price and nonprice bank loan terms, and loan ownership structure. Thus, we are able to document the impact of IFRS adoption on the trade-off between alternative contract terms. In doing so, we paint a more comprehensive picture of IFRS impacts on the various mechanisms that banks utilize to compensate for any information asymmetries arising from IFRS adoption. A second way in which we differ from [Ball et al. \(2013\)](#) is in our focus on the channels through which IFRS impacts contract terms. In their study, [Ball et al. \(2013\)](#) propose several unfavorable aspects of IFRS that can negatively impact debt contracting, including greater managerial flexibility in choosing and applying accounting rules, greater uncertainty about future new rules, and greater emphasis on fair value accounting. However, their empirical design does not allow them to pinpoint any of these in determining which channel drives their findings. In contrast, we examine firm-specific proxies for our two possible channels—accounting complexity and accruals quality—to provide supporting evidence for our hypotheses.

The rest of the paper proceeds as follows. Section II reviews the literature on IFRS adoption, discusses the association between information asymmetry and loan contracts, and puts forward our empirical predictions. Section III outlines our empirical design. Section IV presents our sample selection and main results. Sections V and VI report the results of our additional analyses and robustness tests. Section VII concludes the paper.

II. LITERATURE REVIEW AND EMPIRICAL PREDICTIONS

The Relationship between Information Asymmetry and Bank Loan Contract Terms

This section provides an overview of the research related to the level of information asymmetry between lenders and borrowers and the specific loan terms demanded by lenders. Numerous studies have shown that lenders facing information asymmetry factor their perceptions of borrower quality into their estimations of post-contract monitoring and renegotiating costs. [Easley, Hvidkjaer, and O'Hara \(2002\)](#) and [Easley and O'Hara \(2004\)](#) posit that limited information is a source of nondiversifiable risk that should be priced in securities. [Lambert, Leuz, and Verrecchia \(2007\)](#) argue that limited information reduces the ability of both firms and capital suppliers to coordinate capital investment decisions. They thus suggest that rational capital suppliers should demand higher risk premiums when faced with poor quality information. Consistent with these propositions, [Bharath et al. \(2008\)](#) find that banks charge lower interest rates for firms with higher accruals quality. [Graham et al. \(2008\)](#), [Costello and Wittenberg-Moerman \(2011\)](#), and [Kim et al. \(2011a\)](#) find that borrowers with financial misstatements or weaker internal controls pay higher debt costs. Overall, this research highlights the relation between information asymmetry and subsequent decision making in determining the price term in bank loan contracts.

One area of lender decision making we explore in our study is the use of financial covenants. In an earlier study, [Leftwich \(1983\)](#) argues that lenders create covenants based on accounting measures of financial performance or liquidation value to be used as an early warning sign of increased post-contract credit risk. If borrowers violate financial covenants, then lenders have the option to require early repayment, increase interest rates, or restrict dividend payments ([H. DeAngelo, L. DeAngelo, and Skinner 1994](#); [Dichev and Skinner 2002](#)). Since such risk is typically determined by analyzing a firm's financial statements, we expect that lenders are more likely to include accounting-based financial covenants in their contract terms when a borrower's accounting numbers more reliably indicate either its operating performance ([Li 2010a](#)) or its liquidation value ([Demerjian 2011](#)). Indeed, [Hong et al. \(2015\)](#) find that bank loan covenants are more prevalent in countries with higher financial reporting quality.

Our predictions are also guided by current research on the role of information asymmetry in the use of collateral in contract terms. [Jimenez, Salas, and Saurina \(2006\)](#) find that borrowers use collateral to obtain lower interest rates when their information quality is low. Likewise, [Rajan and Winston \(1995\)](#) suggest that the use of collateral can reduce uncertainty associated with assessing borrower financial health when information is limited or costly to obtain.

Several studies have also examined the relationship between information asymmetry and loan maturity. Specifically, these studies find that greater information asymmetry is associated with shorter loan maturity ([Barclay and Smith 1995](#); [Ortiz-Molina and Penas 2008](#)), as shorter maturity allows lenders to obtain more frequent information about a borrower's ability to pay and to renegotiate contract terms if necessary.

Finally, the impact of information asymmetry also manifests itself in the ownership structure of a syndicated loan. Prior research shows that, besides the typical agency problems that exist between the borrower and the lender, a borrower's information opacity can generate an additional element of moral hazard between the lead arranger and other participant banks in a syndicated loan. The lead arranger is responsible for conducting due diligence about the borrower prior to the loan, presenting the results in a confidential memo to participant banks, collecting and processing borrower information throughout the loan lifecycle, and monitoring the borrower during the duration of the loan. Since a borrower with low financial reporting quality and high information opacity requires more intense due diligence and monitoring efforts, the lead arranger has less incentive to monitor a borrower when ownership of the loan is spread to different lenders. Participant banks, in turn, take this moral hazard problem into account and demand that a greater fraction of the loan be held by the lead arranger when greater information asymmetry exists. Consistent with this scenario, [Ball, Bushman, and Vasvari \(2008\)](#) document that lead arrangers hold a smaller portion of syndicated loans when a borrower's accounting numbers are more informative with regard to the borrower's credit quality. Therefore, it is important to examine the impact of a financial reporting change on loan ownership structure, as ownership structure serves as an alternative monitoring mechanism under conditions of information asymmetry.

The Effect of Mandatory IFRS Adoption on Bank Loan Contract Terms

The impact of mandatory IFRS adoption on equity markets has been examined in numerous studies that have explored its role in increasing liquidity, lowering the cost of equity, and improving earnings forecast accuracy ([Daske, Hail, Leuz, and Verdi 2008](#); [Li 2010b](#); [Tan, Wang, and Welker 2011](#)). Furthermore, other studies have found that borrowers voluntarily adopting IFRS are offered more favorable bank loan contract terms compared to other borrowers ([Kim et al. 2011b](#)). Nonetheless, despite this body of research, the overall impact of IFRS adoption on debt markets remains unclear for several reasons. First, [Ball et al. \(2013\)](#) argue that the use of IFRS may sacrifice the debt contracting usefulness of a borrower's accounting information in favor of the equity holders' interests.² Second, prior literature suggests that IFRS adoption benefits the equity market largely through increased disclosure and improved comparability ([Daske et al. 2008](#); [DeFond, Hu, Hung, and Li 2011](#); [Tan et al. 2011](#)). However, due to banks' access to private information, the additional disclosures under IFRS, such as cash flow statement, segment disclosures, and statements of changes in shareholders' equity, were likely available to banks before IFRS adoption, suggesting limited lender benefit gained from IFRS adoption. Finally, voluntary IFRS adoption may reflect a

² For example, IFRS 9 allows for fair valuing a firm's liabilities, which aids shareholders who require consistent and symmetric measurement methods for all balance sheet accounts. However, it ignores the fact that debt contracts inherently imply matching assets with the face value of debt, as measured by amortized historical costs. Thus, fair valuing liability can cause a downward bias in leverage, leading to an underestimation of borrower credit risk.

broader management strategy to commit to better financial reporting quality (Leuz and Verrecchia 2000). Therefore, it is not IFRS *per se*, but managerial incentives that lead to better financial reporting quality. In contrast, under the mandatory adoption setting, since firms are uniformly required to implement IFRS, we may disregard managerial incentives as a reason for adoption.

Ex ante, the impact of mandatory IFRS adoption on lenders' information costs is not obvious. As mentioned, since IFRS eliminates certain alternative accounting choices, it can reduce both managerial discretion and reporting uncertainty. Moreover, the focus of IFRS on substance over form constrains management from circumventing accounting standards through complicated transaction structuring. This provides two potential benefits for lenders: an improvement in financial statement informativeness with respect to underlying economics (Barth et al. 2008; Langmead and Soroosh 2009), and a reduction in monitoring difficulty. In addition, the use of extensive fair value accounting allows for more timely information about economic gains and losses. Consistent with these arguments, Chen, Tang, Jiang, and Lin (2010) find that mandatory IFRS adoption decreases earnings management toward a target, absolute level of discretionary accruals and standard deviation of unexplained accruals. In another study, Barth, Landsman, Lang, and Williams (2012) find that the earnings smoothing, accruals quality, and timeliness of loss recognition become more comparable to those of U.S. firms after a country adopts IFRS.

However, other studies find that mandatory IFRS adoption has a negative impact from a lender perspective. As mentioned, lenders need to incur additional learning and monitoring costs to determine how borrower credit quality and cash flow capacity are captured under IFRS, relative to prior domestic accounting standards. Furthermore, IFRS inherently requires more complex and subjective accounting estimates and judgments. Thus, without credible enforcement and detailed implementation guidelines, IFRS may provide managers with greater discretion for opportunistic earnings management (Schipper 2003; Maines et al. 2003; Langmead and Soroosh 2009). Consistent with this perspective, Ahmed et al. (2013) find that firms using IFRS show a significant increase in earnings smoothing and the aggressiveness of accounting accruals, as well as less timely loss recognition. Kraft and Landsman (2014) find that the predictability of an accounting-based model for CDS spread decreases after mandatory IFRS adoption. In addition, Kim et al. (2012) find that the increase in accounting complexity after IFRS adoption leads to higher auditing costs.

Other arguments for a negative impact of IFRS adoption on lenders relate to the focus of IFRS on fair value accounting. Specifically, fair value accounting can introduce transitory shocks of expected cash flows into a borrower's earnings, weakening both earnings persistence and the ability of current earnings to predict future cash flows. Furthermore, in the absence of a liquid market for the relevant assets, fair values of assets may be subjectively determined. Finally, the ability of firms to fair value certain liabilities can cause a downward bias in the measurement of leverage. Overall, these features of fair value accounting can reduce the verifiability and reliability of accounting numbers, causing financial covenants to be less effective under IFRS (Ball et al. 2013).

Given the mixed results in the literature regarding the potential effect of mandatory IFRS adoption on the information costs to lenders, we argue that its effect on bank loan contract terms is an open question. If the use of IFRS improves the relevance of accounting information in determining credit quality, then we would expect banks to charge lower interest rates and rely more heavily on accounting-based financial covenants. We would also expect banks to rely less on collateral as a signal of credit quality and to offer longer loan maturities. Finally, if IFRS reduces the need for intensive monitoring from lead arrangers, thus mitigating the moral hazard between the lead arranger and participant banks, then we would expect a more dispersed loan ownership for borrowers adopting IFRS.

Alternatively, if IFRS adoption reduces the reliability of reported accounting numbers, then we would expect banks to charge higher interest rates and to refrain from using accounting measures in developing financial covenants. In addition, we would expect banks to rely more heavily on

collateral to assess borrower credit quality and to shorten loan maturity to allow for more frequent reevaluation of the borrower. Finally, we would expect a higher moral hazard between the lead arranger and participating banks, and thus a greater fraction of a loan held by its lead arranger.

III. RESEARCH DESIGN

In this section, we outline our research design and models. Our first model allows us to examine the temporal changes in loan contract terms within IFRS adopting countries (treatment sample). In our second model, we use a difference-in-differences design to compare changes in loan contract terms in IFRS adopting countries to changes in loan contract terms in non-IFRS adopting countries (benchmark sample). Specifically, we estimate the following two models:

$$\begin{aligned} \text{Contractual terms} = & \beta_0 + \beta_1 \text{POST} + \sum \beta_j \text{Loan Characteristics} + \sum \beta_k \text{Firm characteristics} \\ & + \beta_l \text{Market Benchmark} + \text{Country Dummies} + \text{Industry Dummies}, \end{aligned} \quad (1)$$

$$\begin{aligned} \text{Contractual terms} = & \beta_0 + \beta_1 \text{POST} + \beta_2 \text{IFRS} + \beta_3 \text{POST} * \text{IFRS} + \sum \beta_j \text{Loan Characteristics} \\ & + \sum \beta_k \text{Firm characteristics} + \beta_l \text{Market Benchmark} + \text{Industry Dummies}, \end{aligned} \quad (2)$$

where *POST* is a dummy variable that takes the value of 1 for loans issued to a treatment firm once the firm starts to use IFRS mandatorily, and 0 otherwise. For our benchmark sample, *POST* takes the value of 1 for any loans issued after December 31, 2005, and 0 otherwise. *IFRS* is an indicator variable that takes the value of 1 if a borrower is part of the treatment sample, and 0 if the borrower is part of the benchmark sample. The coefficient on *POST* * *IFRS* captures the change in the dependent variable from the pre- to post-IFRS period for the treatment sample compared to the change over the same period for the benchmark sample.³

Following prior literature (Bharath et al. 2008; Graham et al. 2008; Costello and Wittenberg-Moerman 2011; Kim et al. 2011a; Sufi 2007), we examine five loan contract terms that are found to be affected by financial statement information quality: (1) *Interest rate*, defined as the all-in-drawn spread (plus any upfront fee and annual fee) in basis points in excess of the LIBOR rate; (2) *Financial covenants*, the number of accounting-based financial covenants included in a loan (#FC); (3) *Secured*, an indicator variable that takes the value of 1 if a loan requires the borrower to provide collateral, and 0 otherwise; (4) *Maturity*, the natural log of the number of months from the loan issue date to the maturity date; and (5) *%leadarranger*, the fraction of a loan retained by the lead arranger(s).

In addition, following the literature on bank loan contracting, we incorporate various loan- and borrower-specific control variables in our models (Graham et al. 2008; Costello and Wittenberg-Moerman 2011; Kim et al. 2011a). In particular, we include the following loan-specific control variables: (1) *Log(LoanSize)*, the natural logarithm of the loan amount in millions of U.S. dollars; (2) *Institutional Loan*, an indicator variable taking the value of 1 if the loan type is term loan B, C, or D (institutional term loans), and 0 otherwise; (3) *Credit Rating*, the numerical equivalent of Standard & Poor's (S&P) or Moody's senior debt rating, for which a higher value indicates *poorer* credit quality; (4) *Term Loan* dummy; (5) *Revolver* loan dummy; (6) *#Lenders*, the number of lenders participating in the loan; (7) *Past relation*, the number of loans that lead arrangers have led within the five years preceding the current loan, scaled by the total number of loans issued to the borrower during that five-

³ To avoid multicollinearity, we do not include country dummies in Model (2), because the country-level fixed effects are subsumed by the dummy variable *IFRS*. As a robustness check, we drop the dummy variable *IFRS*, but add the control for country-level fixed effects. Our results in Panel B of Table 3 remain similar.

year period; and (8) *Loan purpose*, a dummy variable to indicate the purpose for a loan. We also include the following borrower-specific control variables: (1) *Log(Assets)*, the natural logarithm of the book value of total assets in millions of U.S. dollars; (2) *Leverage*, the ratio of the borrower's total liabilities to total assets; (3) *ROA*, the borrower's operating income scaled by the average total assets at the beginning and end of the fiscal year; (4) *LOSS*, a dummy variable indicating firm-years with an accounting loss; (5) *Tangibility*, the ratio of the net value of property, plants, and equipment to total assets; (6) *Current Ratio*, the ratio of current assets to current liabilities; (7) *MTB*, the ratio of market value of equity to book value of equity in the year prior to when a loan is issued. Finally, to control for non-IFRS-adoption factors that may affect loan terms, we follow the procedure outlined in [Daske et al. \(2008\)](#). That is, we include in our regressions a contemporaneous variable, *Market Benchmark*, which is the yearly mean value of dependent variables from observations in countries that do not mandate IFRS adoption. All variables are defined in Appendix A. Finally, we control for industry fixed effects and adjust the standard errors of our coefficient estimates for country clusters. Our industry classification follows [Campbell \(1996\)](#).

IV. EMPIRICAL RESULTS

Sample Selection and Descriptive Data

To obtain our sample, we use information from the Reuters Loan Pricing Corporation (LPC) database on bank loans issued during the 2000–2011 period. We obtain these data from DealScan, which compiles information on each transaction or deal. This dataset indicates whether each deal has either one facility or a package of several facilities with different price and nonprice terms. For example, a deal may consist of a line of credit and a term loan with different interest rates. In our sample, we consider each facility as a separate observation.⁴ In selecting our sample, we start with the set of loans issued in all DealScan countries that are also covered by Worldscope (for non-U.S. borrowers) or Compustat (for U.S. borrowers). This allows us to obtain the necessary accounting information for our regression analyses. The initial sample covers 157,256 loans issued to borrowers from 46 countries from 2000 to 2011.

To obtain our treatment sample, we obtain historical information on the use of accounting standards based on the item “Accounting standards followed” (item 07536) in the Worldscope database. We define mandatory IFRS adopters as firms that report under local accounting standard before their countries mandate IFRS adoption and that start to use IFRS as of the fiscal year end of December 31, 2005, or thereafter. The *Post-IFRS* period starts from the first fiscal year that a firm mandatorily adopts IFRS. To obtain our benchmark sample, we identify borrowers that report under local financial reporting standards in non-IFRS adoption countries across the sample period.

For non-U.S. borrowers, we merge our loan dataset with the Worldscope dataset, matching by both country and firm name. For U.S. borrowers, we draw on [Chava and Roberts \(2008\)](#), who provide a link table between the DealScan and Compustat databases. In this way, we are able to identify 65,174 loans issued to borrowers covered by either Worldscope or Compustat. For the merged dataset, we exclude 16,249 loans issued to financial institutions, 6,951 loans for which we cannot obtain the accounting variables required for our regression analysis, and 1,809 loans issued to voluntary IFRS adopters.⁵ We further require that each borrower have at least one loan during the pre-IFRS period and at least one loan during the post-IFRS period. This requirement mitigates concerns that our results may

⁴ [Graham et al. \(2008\)](#), [Costello and Wittenberg-Moerman \(2011\)](#), and [Kim et al. \(2011a\)](#) examine loan contract terms based on individual facilities.

⁵ We exclude voluntary IFRS adopters from both our treatment and benchmark countries, as these borrowers may be subject to a potential self-selection bias.

be driven by changes in the sample composition from the pre- to post-IFRS period. It also controls for potential changes in borrowers' financing choices between alternative debt markets over the sample period (for instance, the financing choices of issuing public bonds versus issuing bank loans). This procedure removes an additional 13,584 loans from our sample. Finally, we require that each country have at least three borrowers that meet our data requirements. This requirement removes an additional 107 loans. Our final sample consists of 26,474 loans across 31 countries, comprising 3,829 loans to 504 borrowers from 17 treatment countries and 22,645 loans to 2,623 borrowers from 14 benchmark countries. Appendix B summarizes our sample selection procedures. Table 1 presents the sample distribution by country and time period. Note that the U.K. and the U.S. have the largest population of unique firms and the total number of loans in our treatment and benchmark samples, respectively, while Greece and Chile have the smallest number of observations.

To test our predictions regarding the effects of mandatory IFRS adoption on loan terms, we conduct a univariate analysis of the five loan features described earlier around the mandatory IFRS adoption, using a difference-in-differences design. We first compute the difference in loan terms between treatment and benchmark samples in the pre- and post-IFRS periods, respectively. We then compare the relative change in these terms over time. Panel A of Table 2 reports the mean values of the five loan terms across the treatment and benchmark samples for the pre- and post-IFRS periods. These statistics show that, during the pre-IFRS period, loans to borrowers in the treatment sample contain lower interest rates, fewer accounting-based financial covenants, fewer collateral requirements, longer maturity, and greater proportions of lead arranger holdings than do loans to borrowers in the benchmark sample.

Next, we note that our treatment sample experiences a substantially greater increase in interest rates than our benchmark sample during the post-IFRS period. We further find that both the treatment and benchmark samples use fewer accounting-based financial covenants in the post-IFRS period, but that our treatment sample experiences a greater *percentage* decrease in the use of covenants. On average, the number of financial covenants issued to the treatment sample decreases by 60 percent after IFRS adoption, compared to 30 percent for the benchmark sample over the same period.

The statistics in Table 2 further show that collateral provisions more than double for the treatment sample during the post-IFRS period, but decrease slightly for the benchmark sample. In terms of loan maturity, both samples experience an increase in loan maturity during the post-IFRS period, with a larger increase for the benchmark sample. Finally, we find that lead arrangers hold a greater proportion of loans during the post-IFRS period for both samples, with a larger increase in the treatment sample.

Overall, the univariate results show that borrowers who undergo mandatory IFRS adoption experience a greater *increase* in interest rates, collateral provisions, and portions of their loan retained by lead arrangers, but a greater *percentage* decrease in the intensity of their financial covenants and a smaller increase in their loan maturity than those not undergoing mandatory IFRS adoption. However, since our univariate analysis does not control for firm characteristics that may influence loan contract terms, we next conduct a series of multivariate analyses to better assess the impact of mandatory IFRS adoption on loan contract terms.

Panel B of Table 2 presents the differences in firm characteristics and other loan features across our treatment and benchmark samples. It shows that loans issued to borrowers in the treatment sample are larger, involve a greater number of lenders (*#Lenders*), and are more likely to be led by prior lead arrangers. The mean values further show that loans to borrowers in the treatment sample are less likely to be revolving loans (*Revolver*), to be held by institutional lenders (*Institution Loan*), or to be issued to rated borrowers (*Rated*). In addition, we find that our treatment borrowers are larger (*Log(Assets)*) and have higher growth prospects (*MTB*) than borrowers in the benchmark sample, but that they have lower current ratios and tangibility. The mean values further show that borrowers in the treatment sample are less likely to report a loss. These differences indicate the importance of controlling for loan and firm characteristics in our multivariate analyses.

TABLE 1
Sample Distribution by Country

Country	Number of Firms	Number of Loans		
		Pre-IFRS	Post-IFRS	Total
Australia	52	265	293	558
Belgium	7	21	27	48
Denmark	4	12	19	31
Finland	18	55	49	104
France	70	305	257	562
Germany	35	161	175	336
Greece	4	10	5	15
Hong Kong	44	113	132	245
Ireland	7	35	21	56
Italy	20	82	75	157
The Netherlands	22	84	69	153
Norway	11	30	27	57
Philippines	7	77	17	94
Spain	28	81	153	234
Sweden	25	63	62	125
Switzerland	10	50	34	84
U.K.	140	500	470	970
Treatment Sample	504	1,944	1,885	3,829
Brazil	9	63	36	99
Canada	128	433	624	1,057
Chile	6	19	13	32
China	8	20	44	64
India	25	89	111	200
Indonesia	11	26	35	61
Japan	537	1,736	2,770	4,506
Malaysia	11	32	19	51
Mexico	12	51	39	90
Russia	9	44	52	96
South Korea	35	289	221	510
Taiwan	141	566	606	1,172
Thailand	13	66	38	104
U.S.	1,678	8,424	6,179	14,603
Benchmark Sample	2,623	11,858	10,787	22,645
Full Sample	3,127	13,802	12,672	26,474

The full sample comprises 26,474 loans to 3,127 firms from 31 countries between 2000 and 2011. We split the sample into the following two groups (1) treatment sample: 3,829 loans to 504 firms from 17 countries that mandate IFRS adoption for the fiscal year ending December 31, 2005 and afterward; and (2) benchmark sample: 22,645 loans to 2,623 firms from 14 countries that do not mandate IFRS adoption by the fiscal year ending December 31, 2005. We identify IFRS adopters and the related adoption dates based on the item "Accounting standards followed" in the Worldscope database (field: 07536). For the treatment sample, *Pre-IFRS* refers to loans issued before a firm mandates IFRS adoption, and *Post-IFRS* otherwise. For the benchmark sample, *Pre-IFRS* refers to loans issued before December 31, 2005 and *Post-IFRS* otherwise.

TABLE 2
Descriptive Statistics

Panel A: Difference-in-Differences Analysis of Loan Terms around the IFRS Mandate

		Pre-IFRS	Post-IFRS	Difference	% Difference
		(a)	(b)	(b) – (a)	[(b) – (a)]/(a)
Interest rate					
Treatment Sample	(i)	97.147	168.841	71.694***	74%
Benchmark Sample	(ii)	179.636	204.305	24.669***	14%
Difference	(i) – (ii)	–82.489***	–35.464***	47.025***	60%
Number of financial covenants					
Treatment Sample	(i)	0.154	0.061	–0.093***	–60%
Benchmark Sample	(ii)	1.150	0.803	–0.347***	–30%
Difference	(i) – (ii)	–0.996***	–0.742***	0.254***	–30%
Likelihood of being secured by collateral					
Treatment Sample	(i)	0.083	0.188	0.105***	127%
Benchmark Sample	(ii)	0.351	0.330	–0.021**	–6%
Difference	(i) – (ii)	–0.268***	–0.142***	0.126***	133%
Maturity					
Treatment Sample	(i)	51.264	53.507	2.243**	4%
Benchmark Sample	(ii)	41.448	47.720	6.272***	15%
Difference	(i) – (ii)	9.816***	5.787***	–4.029***	–11%
Percentage of a loan held by all lead arrangers					
Treatment Sample	(i)	0.577	0.788	0.211***	37%
Benchmark Sample	(ii)	0.517	0.573	0.056***	11%
Difference	(i) – (ii)	0.060***	0.215***	0.155***	26%

Panel B: Descriptive Statistics for Loan and Firm Characteristics

	Treatment Sample		Benchmark Sample		Difference between	
	(n = 3,829)		(n = 22,645)		Treatment and Benchmark	
	Mean	Median	Mean	Median	Mean	Median
	(1)	(2)	(3)	(4)	(1) – (3)	(2) – (4)
Loan Characteristics						
<i>Loan Size</i>	1,194	386	335	100	859***	286***
<i>Revolver</i>	0.452	0.000	0.499	0.000	–0.047***	0.000***
<i>Term Loan</i>	0.326	0.000	0.317	0.000	0.009	0.000
<i>Institution Loan</i>	0.034	0.000	0.069	0.000	–0.035***	0.000***
<i>#Lenders</i>	11.197	8.000	7.861	6.000	3.336***	2.000***
<i>Past relation</i>	0.247	0.125	0.230	0.080	0.017***	0.045***
<i>Rated</i>	0.342	0.000	0.426	0.000	–0.084***	0.000***
Firm Characteristics						
<i>Log(Assets)</i>	8.332	8.307	7.516	7.461	0.816***	0.846***
<i>MTB</i>	2.518	1.884	2.192	1.661	0.326***	0.223***
<i>Leverage</i>	0.644	0.649	0.634	0.627	0.010	0.022***
<i>ROA</i>	0.077	0.070	0.077	0.070	0.000	–0.000
<i>LOSS</i>	0.054	0.000	0.105	0.000	–0.051***	0.000***

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TABLE 2 (continued)

	Treatment Sample (n = 3,829)		Benchmark Sample (n = 22,645)		Difference between Treatment and Benchmark Samples	
	Mean (1)	Median (2)	Mean (3)	Median (4)	Mean (1) – (3)	Median (2) – (4)
<i>Current Ratio</i>	1.246	1.107	1.519	1.320	–0.273***	–0.213***
<i>Tangibility</i>	0.587	0.556	0.662	0.632	–0.075***	–0.076***

***, **, * Indicate that the difference between pre-IFRS and post-IFRS periods (or between treatment and benchmark samples) is significant at the 1 percent, 5 percent, and 10 percent levels (two-tailed), respectively.

We winsorize all continuous variables at the 1st and 99th percentiles.

This table reports loan and firm characteristics for 3,829 loans to 504 firms from 17 treatment countries and 22,645 loans to 2,623 firms from 14 benchmark countries during the 2000–2011 period. Panel A reports the difference-in-differences analysis of interest rates, the number of accounting-based financial covenants in a loan, the likelihood of a loan secured with collateral, loan maturity, and the percentage of a loan held by all lead arrangers between treatment and benchmark samples and between pre-IFRS and post-IFRS periods. The table reports mean values of these variables for each sample group and each sample period. For the treatment sample, *Pre-IFRS* refers to loans issued before a firm mandates IFRS adoption, and *Post-IFRS* otherwise. For the benchmark sample, *Pre-IFRS* refers to loans issued before December 31, 2005 and *Post-IFRS* otherwise. Panel B reports the mean and median values of loan and firm characteristics for treatment and benchmark samples, respectively.

All variables are defined in Appendix A.

Multivariate Analyses of the Impact of Mandatory IFRS Adoption on Loan Contract Terms

Table 3 presents the results for our multivariate analyses of the effects of mandatory IFRS adoption on loan contract terms. Panel A reports the coefficient estimates for Model (1), which examines the effects of mandatory IFRS adoption on loan contract terms within treatment countries. Column (1) shows that the coefficient on *POST* is significantly positive at the 10 percent level, suggesting that the treatment sample experiences an increase in interest rates after mandatory IFRS adoption.⁶ At the bottom of the table, we report the *Pre-IFRS predicted value*, as well as the *Marginal effects*. The *Pre-IFRS predicted value* represents the predicted value of each dependent variable when *POST* = 0 and all control variables are held at the sample mean values of the treatment sample. The *Marginal effects* of the dummy variable *POST* represent the change in the value of each dependent variable from the pre- to post-IFRS period (i.e., *POST* increases from 0 to 1) when all control variables are held at the sample mean values of the treatment sample. The data reported at the bottom of Column (1) show that the treatment sample experiences a 17 percent increase in interest rates (18.836/111.491) from the pre- to the post-IFRS period after controlling for loan and firm characteristics.

Column (2) reports the Poisson regression estimates for the effect of mandatory IFRS adoption on the number of accounting-based financial covenants in a loan.^{7,8} When interpreting these results, it is useful to remember that the Poisson model is a nonlinear model, where the coefficient estimates

⁶ The sample size for the tests of interest rates and the portion of a loan held by its lead arrangers is smaller than that reported in Table 1 because of missing information for some contracts in the DealScan database. This reduced sample size is consistent with previous studies (see Qian and Strahan 2007; Kim et al. 2011a; Sufi 2007; Lin et al. 2012).

⁷ When we estimate the regression in Column (2) using a negative binomial model, all inferences remain the same.

⁸ Due to missing information regarding the interest rates of many contracts in the DealScan database (see footnote 6), we do not control for interest rates in the regressions using *#FC*, *Secured*, *Maturity*, and *%leadarranger* as dependent variables. As a robustness check, we control for interest rates in these regressions for a reduced sample. The results are reported in the “Control for the Relation between Interest Rates and Other Loan Terms” section and Table 6, Panel H.

TABLE 3
The Effects of Mandatory IFRS Adoption on Loan Terms

Panel A: Analysis of Treatment Sample Only

Variables	<i>Spread</i> (1)	<i>#FC</i> (2)	<i>Secured</i> (3)	<i>Maturity</i> (4)	<i>%leadarranger</i> (5)
<i>POST</i>	18.836* (1.73)	-0.705** (-2.54)	0.582*** (3.82)	-0.111** (-2.29)	0.344*** (5.30)
<i>Log(LoanSize)</i>	-9.437*** (-3.70)	-0.046 (-0.61)	-0.010 (-0.25)	-0.038** (-2.30)	-0.018 (-1.63)
<i>Maturity</i>	25.930** (2.50)	-0.173 (-1.13)	0.319*** (4.21)		0.064*** (2.76)
<i>Credit Rating</i>	1.074*** (2.89)	0.039 (1.41)	0.017* (1.85)	-0.001 (-0.02)	0.006* (1.77)
<i>(Credit Rating)²</i>				-0.000 (-0.10)	
<i>Term Loan</i>	29.622* (1.84)	0.814*** (4.58)	0.002 (0.02)	0.544*** (7.40)	-0.169*** (-5.16)
<i>Revolver</i>	-61.567*** (-3.56)	0.327* (1.74)	-0.004 (-0.04)	0.566*** (4.30)	-0.199*** (-5.44)
<i>#FC</i>	12.630* (1.86)		0.362*** (3.68)	-0.024 (-1.01)	-0.006 (-0.25)
<i>Secured</i>	86.908*** (7.07)	1.140*** (3.93)		0.281*** (4.52)	0.016 (0.34)
<i>Institution Loan</i>	41.701*** (3.29)	-0.784*** (-3.76)	0.959*** (7.53)	0.248*** (4.22)	0.084 (1.47)
<i>#Lenders</i>	-0.031 (-0.12)	0.036*** (4.95)	-0.021*** (-3.85)	-0.005** (-2.16)	
<i>Past relation</i>	-34.441*** (-7.71)	0.649 (1.47)	-0.593*** (-5.74)	-0.112*** (-2.95)	-0.094*** (-2.83)
<i>Log(Assets)</i>	-11.276*** (-3.29)	-0.006 (-0.07)	-0.032 (-0.51)	0.004 (0.43)	0.009 (0.79)
<i>Leverage</i>	84.619*** (4.91)	1.151*** (2.92)	0.625** (2.39)	-0.096 (-1.19)	-0.178* (-1.95)
<i>MTB</i>	-1.942*** (-3.34)	0.042** (2.18)	-0.005 (-0.24)	-0.001 (-0.21)	-0.000 (-0.07)
<i>ROA</i>	-119.343** (-2.31)	-2.517* (-1.92)	-1.314* (-1.78)	0.468* (1.94)	0.043 (0.21)
<i>LOSS</i>	51.594*** (5.69)	1.084*** (5.10)	0.464** (2.03)	-0.157*** (-5.89)	0.113* (1.73)
<i>Tangibility</i>	-12.445 (-1.26)	-0.196 (-0.45)	0.263 (1.31)	0.057 (0.66)	0.014 (0.35)
<i>Current Ratio</i>	-11.266** (-2.13)	0.189 (1.53)	0.040 (0.51)	-0.019 (-1.00)	0.009 (0.62)
<i>Market Benchmark</i>	1.251*** (11.04)	0.660 (0.83)	1.736 (1.13)	0.774*** (7.34)	-3.256*** (-5.58)
<i>Constant</i>	-214.577*** (-4.51)	-4.303* (-1.90)	-4.265*** (-5.63)	0.027 (0.06)	2.533*** (5.74)
<i>Pre-IFRS predicted value</i>	111.491	0.111	0.042	44.612	0.525

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TABLE 3 (continued)

Variables	Spread (1)	#FC (2)	Secured (3)	Maturity (4)	%leadarranger (5)
Marginal effects of POST	18.836*	−0.056**	0.084***	−1.118**	0.344***
Percentage change	17%	−50%	200%	−3%	65%
Loan purpose fixed effects	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes
Number of loans	2,206	3,829	3,814	3,829	1,272
R ² or Log likelihood	48%	−1,108	−1,075	25%	−207

Panel B: Analysis of the Full Sample

Variables	Spread (1)	#FC (2)	Secured (3)	Maturity (4)	%leadarranger (5)
POST	−24.247*** (−5.46)	−0.114 (−0.93)	0.101 (1.14)	−0.015 (−0.37)	0.073*** (3.80)
IFRS	−41.937*** (−4.71)	−1.769*** (−7.01)	−0.796*** (−5.90)	0.220*** (2.95)	0.115*** (2.91)
POST * IFRS	33.988*** (3.59)	−0.750*** (−2.77)	0.579*** (3.66)	−0.122* (−1.75)	0.158*** (3.36)
Log(LoanSize)	−4.331*** (−4.55)	0.216** (2.41)	0.139*** (2.68)	0.037 (1.57)	−0.014* (−1.94)
Maturity	7.144 (1.43)	0.015 (0.34)	0.219*** (4.20)		−0.019 (−0.79)
Credit Rating	1.407*** (7.60)	−0.001 (−0.70)	0.025*** (11.38)	0.067*** (9.44)	0.003*** (3.09)
(Credit Rating) ²				−0.002*** (−11.54)	
Term Loan	35.883** (2.56)	0.339*** (5.58)	0.100 (0.59)	0.820*** (4.56)	−0.062 (−1.08)
Revolver	−2.123 (−0.26)	0.335*** (3.74)	0.107 (0.72)	0.524** (2.29)	−0.127*** (−2.76)
#FC	1.181 (1.47)		0.482*** (11.36)	0.018 (0.96)	−0.024 (−1.43)
Secured	56.293*** (13.57)	0.962*** (5.80)		0.125** (2.53)	0.082*** (2.79)
Institution Loan	55.774*** (6.60)	−0.010 (−0.18)	0.556*** (4.35)	0.082 (0.53)	0.184*** (11.07)
#Lenders	−1.733*** (−4.36)	0.014*** (6.52)	−0.009** (−2.01)	0.003 (1.23)	
Past relation	−32.389*** (−6.21)	0.134** (2.44)	−0.568*** (−8.47)	−0.234*** (−2.93)	−0.122*** (−5.26)
Log(Assets)	−15.392*** (−5.61)	−0.252*** (−4.38)	−0.206*** (−7.52)	0.012*** (3.14)	−0.031* (−1.92)
Leverage	117.236*** (11.69)	0.034 (0.49)	0.669*** (2.77)	−0.150* (−1.78)	−0.008 (−0.16)
MTB	−1.876*** (−6.13)	0.003 (0.80)	−0.018*** (−3.86)	−0.002 (−1.26)	0.001 (0.38)

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TABLE 3 (continued)

Variables	<i>Spread</i> (1)	<i>#FC</i> (2)	<i>Secured</i> (3)	<i>Maturity</i> (4)	<i>%leadarranger</i> (5)
<i>ROA</i>	−189.272*** (−2.90)	−0.670*** (−5.51)	−2.362*** (−6.14)	0.707*** (6.82)	−0.124*** (−3.89)
<i>LOSS</i>	48.679*** (8.02)	0.066 (1.41)	0.442*** (10.82)	−0.094*** (−6.93)	0.114*** (4.05)
<i>Tangibility</i>	−13.996* (−1.99)	−0.118** (−2.03)	−0.070 (−0.58)	−0.026 (−1.22)	−0.040*** (−3.77)
<i>Current Ratio</i>	4.249 (0.99)	0.015* (1.79)	0.064*** (4.67)	−0.002 (−0.17)	0.029*** (6.79)
<i>Market Benchmark</i>	1.074*** (8.00)	0.568*** (3.15)	−0.200 (−0.44)	0.720*** (4.59)	0.205 (0.55)
Constant	−76.256*** (−2.89)	−0.471 (−1.57)	−2.335*** (−10.06)	−0.435 (−0.60)	0.558 (1.50)
Treatment Sample					
<i>Pre-IFRS predicted value</i>	141.333	0.146	0.076	42.606	0.631
<i>Marginal effects of POST</i>	9.741***	−0.074***	0.099***	−1.147*	0.231***
<i>Percentage change</i>	7%	−51%	130%	−3%	37%
Benchmark Sample					
<i>Pre-IFRS predicted value</i>	201.301	0.663	0.251	35.199	0.509
<i>Marginal effects of POST</i>	−24.247***	−0.172	0.033	−1.015	0.073***
<i>Percentage change</i>	−12%	−27%	13%	−3%	14%
<i>Difference in Marginal Effects</i>	33.988***	0.098***	0.066***	−0.132*	0.158***
Loan purpose fixed effects	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes
Number of loans	17,337	26,474	26,474	26,474	10,082
R ² or Log likelihood	44%	−26,988	−9,976	32%	−2,158

***, **, * Indicate significance at the 1 percent, 5 percent, and 10 percent levels, respectively (two-tailed).

This table reports the effects of mandatory IFRS adoption on loan contract terms. The sample consists of 3,829 loans to 504 firms from 17 IFRS adopting countries (treatment sample) and 22,645 loans to 2,623 firms from 14 non-IFRS adopting countries (benchmark sample) during the 2000–2011 period. Panel A presents regression estimates for the treatment sample. Panel B presents regression estimates for the full sample. The dependent variable in Column (1) is *Spread*, the all-in-drawn spread (plus the upfront fee and annual fee, if any) in basis points in excess of the LIBOR rate. The dependent variable in Column (2) is *#FC*, the number of accounting-based financial covenants in a loan contract. The dependent variable in Column (3) is *Secured*, an indicator variable that takes the value of 1 if a loan is backed by collateral, and 0 otherwise. The dependent variable in Column (4) is *Maturity*, the natural logarithms of the number of months from the date the loan is issued up until its maturity. The dependent variable in Column (5) is *%leadarranger*, the percentage of a loan held by all lead arrangers. For the treatment sample, *POST* takes the value of 1 for loans issued after a borrower adopts IFRS mandatorily, and 0 otherwise. For the benchmark sample, *POST* takes the value of 1 for loans issued after December 31, 2005, and 0 otherwise. *IFRS* takes the value of 1 if the borrower is from an IFRS-adopting country, and 0 otherwise. Columns (1) and (4) are estimated using OLS regressions. Column (2) is estimated using a Poisson model. Column (3) is estimated using a Probit model. Column (5) is estimated using a Tobit model. In Panel A, all regressions control for loan purpose fixed effects, country fixed effects, and industry fixed effects. In Panel B, all regressions control for loan purpose fixed effects and industry fixed effects. Standard errors of coefficient estimates in both panels are adjusted for country clusters.

reflect the average *percentage* change in the number of financial covenants that accompanies a one-unit change in the independent variable. Column (2) shows that the coefficient on *POST* is significantly negative, suggesting that the introduction of IFRS is associated with a reduction in the use of accounting-based financial covenants. More specifically, *Marginal effects* indicate that, after controlling for firm- and loan-specific determinants, the number of accounting-based financial

covenants in a loan decreases by 50 percent (0.056/0.111) from the pre- to post-IFRS period for the treatment sample.

Column (3) reports our results regarding the effect of mandatory IFRS adoption on the likelihood of collateral being required of a borrower. Here, we see that the coefficient on *POST* is significantly positive, suggesting that banks are more likely to require borrowers who have adopted IFRS to provide collateral. Specifically, *Marginal effects* suggest that, after controlling for loan- and firm-specific determinants, the proportion (i.e., the likelihood) of loans with a collateral provision increases from 4.6 percent in the pre-IFRS period to 12.6 percent ($0.042 + 0.084$) in the post-IFRS period for the treatment sample.

Column (4) presents the results regarding the impact of mandatory IFRS adoption on loan maturity. These results show a significant negative coefficient on *POST*, indicating that mandatory IFRS adopters receive a shorter loan maturity term, with the loan maturity period decreasing by one month after IFRS adoption.

Finally, Column (5) presents the results for the impact of mandatory IFRS adoption on the portion of a loan held by the lead arrangers. The significant positive coefficient on *POST* suggests that lead arrangers take a larger proportion of a loan after mandatory IFRS adoption. Specifically, after controlling for loan- and firm-specific determinants, we find that the proportion of a loan retained by its lead arrangers increases from 53 percent in the pre-IFRS period to 87 percent ($= 0.525 + 0.344$) in the post-IFRS period.

We now turn to our findings for Model (2), which includes both the treatment and benchmark samples and implements a difference-in-differences design. The results in Panel B of Table 3 show that the coefficient on *POST*, after controlling for loan and firm characteristics, is significantly negative in Column (1), significantly positive in Column (5), and insignificantly different from 0 in Columns (2)–(4). These results suggest that our benchmark sample firms have lower interest rates and greater loan portions held by lead arrangers during the post-IFRS period, but do not experience any change in the use of accounting-based financial covenants, the requirement of collateral provisions, or the maturity for their loans. The coefficient on *IFRS* represents the difference in loan contract terms between treatment sample and benchmark sample during the pre-IFRS period. Consistent with the descriptive data reported in Panel A of Table 2, the coefficient on *IFRS* indicates that loans issued to the treatment sample prior to IFRS adoption charge lower interest rates, use fewer financial covenants, and are less likely to require collateral provision than those issued to the benchmark sample; however, the treatment sample loans exhibit longer maturity and greater lead arranger ownership.

Our findings for Model (2) further show that the coefficient on *POST * IFRS* is significantly positive in Columns (1), (3), and (5) and significantly negative in Columns (2) and (4). These results indicate that, relative to the benchmark sample, our treatment sample experiences a greater increase in loan interest rates,⁹ the likelihood of collateral requirements, and the portion of their loans held by lead arrangers in the post-IFRS period. However, they experience a greater decrease in the number of financial covenants,¹⁰ as well as a shorter loan maturity.

⁹ Florou and Kosi (2013) examine the impact of mandatory IFRS adoption on bank loan interest rates from 2000 to 2009 and find a *positive* but *insignificant* effect of IFRS adoption on interest rates. We conjecture that the difference between their finding and ours may be attributable to the difference in our respective samples. We require a borrower to have a bank loan during both the pre- and post-IFRS periods. This restriction allows us to ensure that any observed changes in loan contract terms are due to IFRS adoption, instead of changes in sample composition. By contrast, Florou and Kosi (2013) include in their sample some firms that issue loans before IFRS adoption, but not after, as well as some firms that start issuing loans only after IFRS adoption. In addition, their smaller sample size may impact their finding of an insignificant effect.

¹⁰ As we use a Poisson model, the coefficient estimates reflect the average *percentage* change in the number of financial covenants that accompanies a one-unit change in the independent variable. Therefore, the results in Column (3) suggest that the treatment sample experiences a greater percentage decrease in the number of financial covenants, compared to the benchmark sample. This is consistent with the univariate statistics reported in Panel A of Table 2.

We report the *Pre-IFRS predicted value*, as well as the *marginal effects* for our treatment and benchmark samples, respectively, at the bottom of Table 3. To estimate the *Pre-IFRS predicted value* and *Marginal effects* for the treatment sample, we first set $IFRS = 1$. The *Pre-IFRS predicted value* of each dependent variable for the treatment sample is then calculated as the predicted value when $POST = 0$ and all control variables are held at the sample mean values of the treatment sample. The *Marginal effects* of $POST$ for the treatment sample are calculated as the change in the value of each dependent variable when $POST$ increases from 0 to 1 and when all control variables are held at the sample mean values of the treatment sample. Similarly, to estimate the *Pre-IFRS predicted value* and *Marginal effects* for the benchmark sample, we first set $IFRS = 0$. The *Pre-IFRS predicted value* and *Marginal effects* of the benchmark sample are then calculated in the same way as for the treatment sample. The marginal effects of the interaction term $POST * IFRS$ are equivalent to the difference in the *Marginal effects* of $POST$ between the treatment and benchmark samples. We determine the significance level for the marginal effects of the interaction term $POST * IFRS$ following Ai and Norton (2003) and Karaca-Mandic, Norton, and Dowd (2011).

Our results for this set of analyses show that, after controlling for loan and firm characteristics, our treatment firms experience a 7 percent increase in their interest rates (9.741/141.333), a 51 percent decrease in their number of financial covenants ($-0.074/0.146$), a 130 percent increase in the likelihood of collateral requirements (0.099/0.076), a 3 percent decrease in loan maturity ($-1.147/42.606$), and a 37 percent increase in the percentage of a loan held by the lead arrangers (0.231/0.631) from the pre- to post-IFRS period. Furthermore, the *Difference in Marginal Effects* at the bottom of the table show that these changes are all more pronounced for our treatment borrowers compared to those in the benchmark sample.¹¹ These results are consistent with our descriptive data in Panel A of Table 2.

In general, we find that our control variable coefficient estimates are consistent with findings in the extant literature that high risk borrowers receive less favorable contract terms. For example, we find that borrowers with poor credit ratings (*Credit Rating*), smaller size (*Log(Assets)*), higher leverage (*Leverage*), and lower profitability (*ROA*) are charged higher interest rates and are more likely to secure a loan with collateral. We also find that loans to borrowers with a smaller size (*Log(Assets)*), lower profitability (*ROA*), and fewer tangible assets (*Tangibility*) use a greater number of accounting-based financial covenants. Furthermore, we find that borrowers with a smaller size (*Log(Assets)*), higher leverage (*Leverage*), and lower profitability (*ROA*) are offered shorter loan maturity, while those with poor credit ratings (*Credit Rating*), lower profitability (*ROA*), and fewer tangible assets (*Tangibility*) have a greater portion of their loans held by lead arrangers.

Regarding loan characteristics, we find that lenders in our sample charge lower interest rates for larger loans (*Log(LoanSize)*), loans with a greater number of participants (*#Lenders*), and loans to a prior borrower (*Past relation*). By contrast, they charge higher interest rates for term loans (*Term Loan*), loans with a collateral provision (*Secured*), or loans held by institutional lenders (*Institution Loan*).¹²

¹¹ It is useful to remember that the Poisson model is a nonlinear model, where the coefficient estimates reflect the average *percentage* change in the number of financial covenants that accompanies a one-unit change in the independent variable. Therefore, when comparing the marginal effects of $POST$ between the treatment sample and benchmark sample reported in Panel B of Table 3, it is more informative to compare the “%change” (i.e., *Marginal effects* divided by *Pre-IFRS predicted value*) between the two groups.

¹² Kim et al. (2012) document that audit fees increase after mandatory IFRS adoption. To examine the impact of audit fees on bank loan contract terms, we add firm-year audit fees to Model (2), where audit fees are measured by the logarithm value of audit fees in thousands of U.S. dollars. Our results continue to hold after controlling for audit fees. This analysis suggests that the increase in audit effort does not fully eliminate the additional information costs and monitoring efforts that banks need to commit after mandatory IFRS adoption.

Robustness Checks of the Main Results

To address the heterogeneity in our treatment and benchmark firms across countries, we perform several robustness checks. For brevity, we report the coefficients for only our main variables of interest, *POST*, *IFRS*, and *POST * IFRS*. First, as noted, the U.S. comprises a large portion of our benchmark sample. Since U.S. GAAP differs from the GAAP of other countries, the dominance of U.S. firms in our sample may impact our benchmark findings, as well as our treatment and benchmark comparisons. To test for this possibility, we exclude U.S. loans from our benchmark sample and repeat our estimation of Model (2). The results in Panel A of Table 4 show that the coefficient on *POST * IFRS* remains significant for most contract terms, with the exception of loan maturity. Therefore, we conclude that our main findings are not sensitive to the exclusion of U.S. borrowers from our sample.

In a second robustness test, we examine whether our results reflect conditions due to the financial crisis of 2008–2009. Since the crisis originated in the U.S. and then spread to continental Europe, we rerun our regressions after benchmarking either all IFRS borrowers or only European IFRS borrowers against U.S. borrowers. This test aims to mitigate the concern that our results are driven by the financial crisis instead of IFRS adoption. The results in Panels B and C of Table 4 show that the coefficients on *POST * IFRS* are again similar to those of our main analysis, with the exception of loan maturity.

Next, we pair up treatment and benchmark samples in terms of legal origin. [La Porta, Lopez-de-Silanes, Shleifer, and Vishny \(1997\)](#) argue that legal origin is important as it determines various institutional infrastructures, such as property rights enforcement and credit rights protections, which impact private credit market development as well as loan contracting ([Djankov et al. 2007](#); [Hong et al. 2015](#); [Esty and Megginson 2003](#); [Qian and Strahan 2007](#); [Bae and Goyal 2009](#)). Panel D of Table 4 reports the estimation of Model (2) for common- and code-law countries, respectively. These results show that our findings continue to hold for both common- and code-law countries, with the exception of collateral in common-law countries and loan maturity in code-law countries.

Finally, we match our treatment and benchmark samples based on a more specific set of country or firm characteristics. At the country level, we consider four factors including credit rights protection, legal enforcement, the mean ratio of private credit to GDP, and the mean value of GDP per capita. The credit rights protection index is obtained from [Djankov et al. \(2007\)](#) and the legal enforcement measure is based on the rule-of-law index developed by [Kaufmann, Kraay, and Mastruzzi \(2007\)](#). In this set of analyses, we first rank all sample countries along each factor, calculate each country's mean ranking of the four factors, and then rank the 31 sample countries from 1 to 31 based on the mean ranking. Next, for each treatment country, we identify a benchmark country with the rank one notch above or below the treatment country. These procedures yield four country pairs including Germany versus Japan, the Philippines versus Indonesia, Sweden versus Canada, and Switzerland versus the U.S. At the firm level, we use the propensity score method to match treatment and benchmark firms on pre-IFRS firm size, leverage, profitability, market-to-book ratio, tangibility, and current ratio. We are able to match 500 treatment firms with 500 benchmark firms, for a total of 9,178 loans during the sample period. The country-level results in Panel E show the coefficient on *POST * IFRS* continues to be significant for most of the loan contract terms with the exception of loan maturity, while the firm-level results in Panel F show that the coefficient on *POST * IFRS* remains significant for all loan contract terms.

In sum, both the univariate analysis presented in Table 2 and the multivariate regressions presented in Tables 3 and 4 suggest that mandatory IFRS adoption borrowers experience higher interest rates, fewer accounting-based financial covenants, greater collateral requirements, shorter loan maturity, and greater portions of their loans held by the lead arrangers compared to borrowers that do not adopt IFRS. These findings are consistent with the proposition that mandatory IFRS adoption increases information asymmetry between banks and borrowers, causing banks to set relatively less favorable loan terms.

TABLE 4
The Effects of Mandatory IFRS Adoption on Loan Terms
Robustness Checks of Main Results

Panel A: Excluding the U.S. from the Benchmark Sample

Variables	<i>Spread</i> (1)	<i>#FC</i> (2)	<i>Secured</i> (3)	<i>Maturity</i> (4)	<i>%leadarranger</i> (5)
<i>POST</i>	-23.278** (-2.13)	1.113 (1.27)	-0.014 (-0.10)	-0.003 (-0.03)	0.095* (1.70)
<i>IFRS</i>	-12.354 (-0.76)	0.108 (0.14)	-0.608*** (-2.88)	0.367** (2.57)	0.000 (0.00)
<i>POST * IFRS</i>	41.882** (2.50)	-1.959** (-2.19)	0.737*** (3.63)	-0.077 (-0.56)	0.140** (2.38)
Number of Loans	4,122	11,871	11,856	11,871	4,515

Panel B: Using the U.S. as the Only Benchmark Country

Variables	<i>Spread</i> (1)	<i>#FC</i> (2)	<i>Secured</i> (3)	<i>Maturity</i> (4)	<i>%leadarranger</i> (5)
<i>POST</i>	-14.942*** (-11.89)	-0.199*** (-10.22)	0.207*** (7.42)	-0.024 (-1.31)	0.121*** (11.65)
<i>IFRS</i>	-54.651*** (-6.09)	-1.954*** (-8.42)	-0.846*** (-9.92)	0.218*** (3.66)	0.105*** (2.83)
<i>POST * IFRS</i>	26.834*** (3.27)	-0.711*** (-2.73)	0.463*** (3.54)	-0.143 (-1.55)	0.156*** (3.54)
Number of Loans	15,421	18,427	18,412	18,427	6,839

Panel C: Benchmarking EU IFRS Adopting Countries against the U.S.

Variables	<i>Spread</i> (1)	<i>#FC</i> (2)	<i>Secured</i> (3)	<i>Maturity</i> (4)	<i>%leadarranger</i> (5)
<i>POST</i>	-13.433*** (-27.92)	-0.202*** (-12.73)	0.225*** (14.66)	-0.036*** (-3.17)	0.135*** (10.14)
<i>IFRS</i>	-51.907*** (-5.62)	-1.992*** (-7.09)	-0.818*** (-8.55)	0.134*** (3.22)	0.121*** (3.19)
<i>POST * IFRS</i>	37.808*** (5.33)	-0.905*** (-2.77)	0.608*** (6.21)	0.018 (0.43)	0.241*** (6.07)
Number of Loans	15,009	17,389	17,389	17,389	6,295

Panel D: Common-Law and Code-Law Countries, Respectively

Variables	<i>Spread</i> (1)	<i>#FC</i> (2)	<i>Secured</i> (3)	<i>Maturity</i> (4)	<i>%leadarranger</i> (5)
Common-Law Countries					
<i>POST</i>	-13.305*** (-6.95)	-0.212*** (-16.24)	0.238*** (8.25)	-0.033 (-1.69)	0.113*** (10.07)
<i>IFRS</i>	-52.114*** (-3.68)	-1.568*** (-5.73)	-0.831*** (-6.93)	0.242** (2.88)	0.087 (1.24)

(continued on next page)

TABLE 4 (continued)

Variables	Spread (1)	#FC (2)	Secured (3)	Maturity (4)	%leadarranger (5)
<i>POST</i> * <i>IFRS</i>	17.195** (1.98)	-0.432** (-2.14)	0.274 (1.02)	-0.282** (-2.00)	0.088* (1.73)
Number of Loans	14,657	17,834	17,834	17,834	6,510
Code-Law Countries					
<i>POST</i>	-28.231** (-2.36)	2.279*** (4.89)	-0.128* (-1.91)	0.007 (0.08)	0.087 (1.16)
<i>IFRS</i>	12.103 (0.73)	0.926 (1.01)	-0.494 (-1.56)	0.497** (2.59)	-0.071* (-1.95)
<i>POST</i> * <i>IFRS</i>	42.957*** (3.87)	-3.863*** (-3.40)	0.967*** (6.27)	0.023 (0.16)	0.214*** (3.46)
Number of Loans	2,680	8,640	8,625	8,640	3,572

Panel E: Country-Level Match between Treatment and Benchmark Samples

Variables	Spread (1)	#FC (2)	Secured (3)	Maturity (4)	%leadarranger (5)
<i>POST</i>	-14.826*** (-8.63)	-0.209*** (-17.45)	0.176*** (4.07)	-0.004 (-0.06)	0.053 (1.49)
<i>IFRS</i>	-40.322*** (-3.70)	-1.814*** (-3.25)	-0.855*** (-3.14)	0.404** (2.44)	0.131*** (3.93)
<i>POST</i> * <i>IFRS</i>	40.855* (1.86)	-2.473*** (-4.81)	0.810** (2.14)	-0.134 (-0.66)	0.314*** (3.83)
Number of Loans	14,103	20,861	20,861	20,861	7,603

Panel F: Firm-Level Propensity Score Match between Treatment and Benchmark Samples

Variables	Spread (1)	#FC (2)	Secured (3)	Maturity (4)	%leadarranger (5)
<i>POST</i>	-33.796*** (-10.32)	-0.125 (-0.65)	0.007 (0.06)	-0.026 (-0.69)	0.088 (1.61)
<i>IFRS</i>	-44.536*** (-3.28)	-1.674*** (-5.55)	-0.684*** (-4.13)	0.296*** (3.73)	0.081* (1.86)
<i>POST</i> * <i>IFRS</i>	46.997*** (5.53)	-0.785*** (-2.80)	0.608*** (3.69)	-0.116* (-1.84)	0.185*** (3.57)
Number of Loans	5,544	9,178	9,125	9,178	3,292
Control Variables in All the Panels					
Loan purpose fixed effects	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes

*, **, *** Indicate significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

The standard errors of coefficient estimates are adjusted for country clusters.

This table reports the effects of mandatory IFRS adoption on loan terms under alternative sample specifications. Panel A reports the regression estimates after excluding U.S. firms from the benchmark sample. Panel B reports the regression estimates using the U.S. as the only benchmark country. Panel C reports the regression estimates for the sample that includes European IFRS adopting countries and the U.S. Panel D presents the regression estimates for common-law and code-law countries, respectively. Panel E presents the effects of mandatory IFRS adoption on loan terms for a sample that matches treatment and benchmark samples based on country-level characteristics. Panel F presents the effects of mandatory IFRS adoption on loan terms for a sample that matches treatment and benchmark samples based on firm-level characteristics. All regressions control for loan purpose fixed effects and industry fixed effects.

V. CROSS-SECTIONAL ANALYSES CONDITIONAL ON FIRM-SPECIFIC CHANGES IN INFORMATION ASYMMETRY POST-IFRS ADOPTION

To gain further insight into our results, we next explore the channels through which mandatory IFRS adoption affects bank loan contract terms. We first examine the effect of the differences between IFRS and local accounting standards. We expect that the greater the deviation of local accounting standards from IFRS, the more effort and cost banks must commit to understanding how a borrower's credit quality and future cash flow capacity are captured by its financial statements when it adopts IFRS. This prediction is consistent with the finding of [Kim et al. \(2012\)](#) that greater differences between local accounting standards and IFRS lead to greater audit complexity and higher audit fees.

To test this prediction, we measure the deviation of local accounting standards from IFRS by the extent to which a firm reconciles its key financial statement items from local accounting standards to IFRS. In the first year of IFRS adoption, firms are required to reconcile their prior year's financial statements from local standards to IFRS. Thus, we can use this reconciliation as an outcome-based measure of the extent of the firm-specific financial reporting change when switching from local accounting standards to IFRS.

Using the reconciliation data from Worldscope, we identify the following balance sheet and income statement accounts that are reconciled most frequently: (1) short-term investment, (2) PPE, (3) intangible assets, (4) leases, (5) deferred taxes, (6) provisions, (7) post-retirement benefits, (8) revenue, (9) cost of goods sold, and (10) discontinued operation. These items capture a borrower's financial position and performance from several perspectives. First, short-term investments, PPE, and intangible assets reflect both the liquidity and tangibility of assets, while leases, deferred taxes, provisions, and post-retirement benefits affect long-term liability and leverage. Furthermore, balance sheet items in aggregate reflect the net realizable value of assets for a firm. Finally, revenue, cost of goods sold, and transitory earnings, i.e., discontinued operation, indicate a firm's financial performance.

To test whether the extent of the accounting change impacts a borrower's subsequent loan terms, we develop a dummy variable for each item to indicate whether the treatment firm reconciles the item when switching from local standards to IFRS. We then develop an aggregate reconciliation score by summing ten dummy variables. A higher score indicates a greater number of reconciliations and thus a greater difference between local standards and IFRS. After determining an aggregate reconciliation score for each firm, we partition our treatment sample into two groups based on the median value of the firm aggregate reconciliation score, using *HIGH_ΔGAAP* to represent firms with a greater accounting change and *LOW_ΔGAAP* to represent firms with a smaller accounting change. Comparing these groups to our benchmark, we expect changes in loan contract terms to be more pronounced among firms in our *HIGH_ΔGAAP* than *LOW_ΔGAAP* treatment sample.

We next examine whether our findings are due to a change in accounting quality after IFRS adoption. To study this possibility, we use the variance in abnormal total accruals as a proxy for accounting quality ([Dechow and Dichev 2002](#)). Specifically, we expect that a large variance is an indication of poor mapping of earnings into cash flows, making it more difficult for lenders to estimate future operating cash flows. Indeed, [Bharath et al. \(2008\)](#) find that banks charge higher interest rates and set more stringent nonprice terms for borrowers with poorer accruals quality. Following [Kim et al. \(2012\)](#), we measure abnormal total accruals using the residuals estimated from the [Dechow and Dichev \(2002\)](#) model augmented by the fundamental variables:

$$TAC_{it} = \alpha_0 + \alpha_1 CFO_{i,t-1} + \alpha_2 CFO_{i,t} + \alpha_3 CFO_{i,t+1} + \alpha_4 \Delta Sales_{i,t} + \alpha_5 PPE_{i,t} + \varepsilon_{i,t}, \quad (3)$$

where TAC_{it} represents the total accruals of firm i at year t , measured by net income before extraordinary items minus operating cash flows. $CFO_{i,t-1}$, $CFO_{i,t}$, and $CFO_{i,t+1}$ are the operating cash flows of firm i at year $t-1$, t , and $t+1$, respectively. $\Delta Sales_{i,t}$ is firm i 's change in sales from

year $t-1$ to t . Finally, $PPE_{i,t}$ is the gross amount of PPE for firm i at the end of year t . All variables are scaled by lagged total assets.

To estimate the abnormal total accruals for our treatment firms, we first obtain Worldscope data for all firms with our required accounting variables during the 2000–2011 period (whether the firm is covered in our loan sample or not). We then estimate Equation (3) for every country-industry-year with at least ten firms, using the industry classification outlined in Campbell (1996). Abnormal total accruals are represented by the residuals of Equation (3). After obtaining the abnormal total accruals, we calculate the change in abnormal total accruals variance for every treatment firm from the pre- to post-IFRS period. An increase in the variance of abnormal total accruals indicates poorer accounting quality after IFRS adoption.^{13,14}

We next partition our treatment sample into two groups based on the median value of changes in the abnormal total accruals variance. $HIGH_ΔAQ$ is a dummy variable indicating a larger change in abnormal accruals variance and $LOW_ΔAQ$ is a dummy variable indicating a smaller change in abnormal accruals variance.

The descriptive statistics in Panel A of Table 5 show that, on average, our treatment firms in the $LOW_ΔGAAP$ group reconcile 3,469 accounting items when switching from local standards to IFRS, while our treatment firms in the $HIGH_ΔGAAP$ group reconcile 7,990 items. The difference in the number of reconciled items between the two groups is significant at the 1 percent level. With respect to changes in accruals quality, treatment firms in the $LOW_ΔAQ$ group experience a reduction in the variance of abnormal accruals by -0.026 , indicating an improvement in accruals quality after IFRS adoption. By contrast, treatment firms in the $HIGH_ΔAQ$ group experience an increase in the variance of abnormal accruals by 0.031 , indicating a decrease in accruals quality after IFRS adoption. The changes in accruals quality between the two groups are significantly different at the 1 percent level. This finding suggests that mandatory IFRS adoption can increase or decrease accounting quality for a firm, relative to its accounting quality under local accounting standards.

Panel A also presents the statistics related to changes in firm characteristics after IFRS adoption for our subsamples. Here, we see that treatment firms in all four subsamples experience an increase in firm size ($Log(Assets)$) and profitability (ROA) but a decrease in their proportion of tangible assets ($Tangibility$) from the pre- to post-IFRS period. The only notable difference between subsamples is that the $LOW_ΔAQ$ group experiences a decrease in MTB while the $HIGH_ΔAQ$ group experiences an increase in MTB after IFRS adoption. Overall, we conclude that any observed differences in changes to loan contract terms between the subgroups are likely due to the differential impact of IFRS adoption on accounting standards or accounting quality, rather than to differential changes in firm characteristics.

From the results in Panel B of Table 5, we see that the coefficients on $POST * LOW_ΔGAAP$ are significantly positive in Columns (1) and (5), but insignificantly different from 0 in Columns (2)–(4). These findings indicate that firms with smaller accounting changes experience higher interest rates and greater loan portions held by lead arrangers after IFRS adoption. By contrast, the coefficients on $POST * HIGH_ΔGAAP$ are significant in Columns (1), (2), (3), and (5), but not in Column (4).¹⁵ These findings indicate that firms with greater accounting changes experience higher

¹³ The results are qualitatively similar if we use the changes in a firm's mean value of absolute abnormal total accruals from the pre- to post-IFRS period as a proxy for changes in accounting quality; again, abnormal total accruals are estimated from Equation (3).

¹⁴ To calculate the changes in the variance of abnormal total accruals from the pre- to post-IFRS period, a treatment firm needs at least six years of abnormal accruals data (three years in each period, respectively). This requirement eliminates 68 of our treatment firms that do not have enough data to calculate the change in the variance of abnormal total accruals.

¹⁵ As a robustness check, we drop the two dummy variables $HIGH_ΔGAAP$ and $LOW_ΔGAAP$ in Panel B of Table 5 and $HIGH_ΔAQ$ and $LOW_ΔAQ$ in Panel C of Table 5 and add the control for the country fixed effects. Our results remain similar.

TABLE 5

The Effects of Mandatory IFRS Adoption on Loan Terms, Conditional on Firm-Specific Changes in Accounting Standards and Accounting Quality

Panel A: Descriptive Statistics for Changes in Accounting Standards and Changes in Accounting Quality

	Changes in Accounting Standards						Changes in Accounting Quality					
	LOW_ΔGAAP (n = 213)			HIGH_ΔGAAP (n = 291)			LOW_ΔAQ (n = 218)			HIGH_ΔAQ (n = 218)		
	Pre-IFRS	Post-IFRS	Post-Pre	Pre-IFRS	Post-IFRS	Post-Pre	Pre-IFRS	Post-IFRS	Post-Pre	Pre-IFRS	Post-IFRS	Post-Pre
ΔGAAP			3.469			7.990						
ΔAQ												
Log(Assets)	7.440	8.107	0.667***	8.459	8.844	0.385***	0.060	0.034	-0.026***	0.038	0.071	0.031***
MTB	2.538	2.530	-0.009	2.421	2.391	-0.030	8.205	8.593	0.388***	8.175	8.602	0.428***
Leverage	0.600	0.597	-0.003	0.671	0.664	-0.007	2.766	2.217	-0.549**	2.133	2.502	0.369**
ROA	0.074	0.082	0.007**	0.068	0.076	0.007**	0.637	0.627	-0.010	0.658	0.649	-0.010
LOSS	0.030	0.022	-0.008	0.067	0.056	-0.011	0.071	0.078	0.007*	0.070	0.079	0.009**
Current Ratio	1.373	1.392	0.018	1.184	1.174	-0.010	0.071	0.030	-0.041***	0.055	0.036	-0.019
Tangibility	0.555	0.505	-0.050*	0.648	0.574	-0.073***	1.188	1.199	0.011	1.286	1.311	0.025
							0.633	0.544	-0.090***	0.603	0.529	-0.074***

Panel B: Analysis Conditional on Firm-Level Changes in Accounting Standards

Variables	Spread (1)	#FC (2)	Secured (3)	Maturity (4)	%leadarranger (5)
POST	-24.194*** (-5.38)	-0.114 (-0.93)	0.103 (1.15)	-0.015 (-0.37)	0.074*** (3.81)
LOW_ΔGAAP	-45.047*** (-3.33)	-1.468*** (-3.96)	-0.868*** (-4.91)	0.176** (2.17)	0.031 (0.74)
HIGH_ΔGAAP	-40.808*** (-5.22)	-1.957*** (-8.25)	-0.760*** (-5.96)	0.239*** (2.82)	0.159*** (3.90)
POST * LOW_ΔGAAP	21.773** (2.45)	-0.580 (-1.58)	0.316 (1.17)	-0.116 (-1.27)	0.123** (2.24)

(continued on next page)

TABLE 5 (continued)

Variables	Spread (1)	#FC (2)	Secured (3)	Maturity (4)	%leadarranger (5)
<i>POST * HIGH_ΔGAAP</i>	35.334*** (3.51)	-0.906** (-2.07)	0.694*** (5.09)	-0.119 (-1.17)	0.233*** (5.25)
<i>A = B (F-stat)</i>	1.17	4.27**	3.83**	0.01	9.91***
Loan purpose fixed effects	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes
Number of loans	17,337	26,474	26,474	26,474	10,082
R ² or Log likelihood	44%	-26,970	-9,958	32%	-2,142

Panel C: Analysis Conditional on Firm-Level Changes in Accounting Quality

Variables	Spread (1)	#FC (2)	Secured (3)	Maturity (4)	%leadarranger (5)
<i>POST</i>	-24.413*** (-5.52)	-0.115 (-0.95)	0.105 (1.17)	-0.014 (-0.32)	0.069*** (3.65)
<i>LOW_ΔAQ</i>	-39.262*** (-4.05)	-1.528*** (-5.87)	-0.652*** (-4.14)	0.218** (2.38)	0.168*** (3.54)
<i>HIGH_ΔAQ</i>	-43.371*** (-4.50)	-1.790*** (-5.69)	-0.816*** (-5.35)	0.235*** (2.80)	0.102*** (2.65)
<i>POST * LOW_ΔAQ</i>	13.235 (1.03)	-0.412 (-1.13)	0.198 (1.00)	-0.106 (-0.95)	0.094** (2.02)
<i>POST * HIGH_ΔAQ</i>	45.269*** (4.02)	-1.148*** (-3.78)	0.665*** (3.26)	-0.164 (-1.36)	0.186*** (3.48)
<i>A = B (F-stat)</i>	6.02**	3.72*	4.85**	0.35	5.35**
Loan purpose fixed effects	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes
Number of loans	17,022	25,922	25,922	25,922	9,915
R ² or Log likelihood	45%	-26,857	-9,766	32%	-2,061

***, **, * Indicate significance at the 1 percent, 5 percent, and 10 percent levels, respectively (two-tailed).
All regressions control for loan purpose fixed effects and industry fixed effects.

(continued on next page)

TABLE 5 (continued)

Standard errors of the coefficient estimates are adjusted for country clusters.

This table reports the effects of changes to accounting standards and changes in accounting quality on loan contract terms after mandatory IFRS adoption. The sample consists of 3,829 loans to 504 firms from 17 IFRS adopting countries (treatment sample), and 22,645 loans to 2,623 firms from 14 non-IFRS adopting countries (benchmark sample) during the 2000–2011 period. Panel A presents descriptive statistics for changes in accounting standards, changes in accounting quality, and changes in firm characteristics. *LOW_ΔGAAP* takes the value of 1 if a treatment firm's change in accounting standards upon IFRS adoption is below the median change of all treatment firms, and 0 otherwise. *HIGH_ΔGAAP* takes the value of 1 if a treatment firm's change in accounting standards upon IFRS adoption is above the median change of all treatment firms, and 0 otherwise. *LOW_ΔAQ* takes the value of 1 if a treatment firm's change in the variance of abnormal total accruals from the pre-IFRS to post-IFRS period is below the median change of all treatment firms, and 0 otherwise. *HIGH_ΔAQ* takes the value of 1 if a treatment firm's change in the variance of abnormal total accruals from the pre-IFRS to post-IFRS period is above the median change of all treatment firms, and 0 otherwise. Panel B reports the effects of changes in accounting standards on loan contract terms after mandatory IFRS adoption. Panel C reports the effects of changes in accounting quality on loan contract terms after mandatory IFRS adoption. For the treatment sample, *POST* takes the value of 1 for loans issued after a borrower adopts IFRS mandatorily, and 0 otherwise. For the benchmark sample, *POST* takes the value of 1 for loans issued after December 31, 2005, and 0 otherwise.

interest rates, greater collateral requirements, greater loan portions held by lead arrangers, and fewer financial covenants after IFRS adoption. The F-statistics reported at the bottom of Panel B of Table 5 reinforce the finding that the latter three results are more pronounced in treatment firms with a greater change in accounting standards compared to those with a smaller change post-IFRS adoption. Together, these results support the prediction that borrowers experiencing greater accounting changes after IFRS adoption require higher information and monitoring costs for lenders to understand the accounting transition. They further support the idea that banks factor these costs into their loan contract terms.¹⁶

Finally, the results in Panel C of Table 5 show that the coefficients on $POST * LOW_ΔAQ$ are significantly positive in Column (5), but insignificantly different from 0 in Columns (1)–(4), indicating that the only significant change for firms undergoing a small change in abnormal accruals variance is an increase in the portion of their loans held by lead arrangers. By contrast, the coefficients on $POST * HIGH_ΔAQ$ are all significant with the exception of the loan maturity coefficient, indicating that borrowers with greater changes in abnormal accruals variance experience higher interest rates, greater collateral requirements, a greater portion of their loans held by lead arrangers, and fewer accounting-based financial covenants. The F-statistics reported at the bottom of Panel C of Table 5 reinforce these findings. Thus, we conclude that this set of analyses provides additional evidence that changes in loan contract terms after IFRS adoption are largely driven by borrowers experiencing lower accruals quality.^{17,18} On a broader level, these analyses indicate that changes in both accounting complexity and accounting quality arising from mandatory IFRS adoption play an important role in the subsequent design of loan contract terms.¹⁹

¹⁶ We also perform analyses on each reconciliation component. For each component, we develop two dummy variables, *Reconcile* and *No_Reconcile*, to indicate firms that reconcile the component and firms that do not reconcile the component, respectively. Again, we benchmark these two groups of IFRS firms against non-IFRS firms. We find that the coefficient on $POST * Reconcile$ is more positive than that on $POST * No_Reconcile$ for nine out of ten components, seven out of ten components, and all ten components in the tests of interest rates, likelihood of required collateral, and the portion of a loan held by lead arrangers, respectively. We further find that the coefficient on $POST * Reconcile$ is more negative than that on $POST * No_Reconcile$ for nine out of ten components and eight out of ten components in the tests of financial covenants and loan maturity, respectively. Therefore, the respective magnitudes of the coefficient estimates for individual reconciliation components support our overall hypothesis. Furthermore, the weaker results for the individual components analysis are consistent with the notion that the aggregate reconciliation score captures the collective effect of financial reporting changes.

¹⁷ We note that borrowers in the $LOW_ΔAQ$ group, i.e., borrowers with a reduction in the variance of abnormal accruals, also experience an increase in the portion of their loans held by lead arrangers after IFRS adoption. It is possible that, *ex ante*, lead arrangers still experience certain costs related to understanding the transition from local standards to IFRS for these firms. In another analysis (unreported), we add the two dummy variables, $HIGH_ΔGAAP$ and $LOW_ΔGAAP$, and rerun the regression for interest rates. We find that the coefficient on $POST * LOW_ΔAQ$ becomes insignificant while the coefficient on $POST * HIGH_ΔAQ$ remains significantly positive. We also find that the coefficient for $POST * HIGH_ΔAQ$ is significantly greater than that for $POST * LOW_ΔAQ$.

¹⁸ To rule out the possibility that documented changes in loan contract terms are driven by changes in accounting quality independent of mandatory IFRS adoption, we add a firm-year accrual quality measure as a control variable in the regressions reported in Panel C of Table 5, where accrual quality is measured by abnormal total accruals variance in the past five years before a loan is issued. The results are qualitatively similar.

¹⁹ We also examine the impact of IFRS adoption on loan contract terms via the channel of fair value accounting. Based on the self-constructed fair value accounting index from Ball et al. (2013), which measures the fair value differences between domestic accounting standards and IFRS, we partition the treatment sample into two groups and compare each group to the benchmark sample. We find a greater reduction in the use of financial covenants and a greater increase in the portion of loans held by lead arrangers for loans issued to treatment borrowers who experience a greater increase in the use of fair value accounting after IFRS adoption, relative to treatment borrowers who experience a smaller change in the use of fair value accounting in the same period. However, we find no difference between these treatment subgroups with regard to observed post-IFRS increases in interest rates, collateral usage, or loan maturity.

VI. SENSITIVITY ANALYSES

In this section, we present the results of a number of supplementary analyses that we conduct to provide greater confidence in our findings. In particular, we conduct analyses that (1) rule out the confounding effects of the financial crisis by dropping loans issued in 2008 and 2009, as well as by limiting the sample period to either 2000–2007 or 2003–2007; (2) rule out the confounding effects of concurrent enforcement changes by conducting a separate analysis for IFRS adopting countries with and without concurrent enforcement changes; (3) expand the sample by relaxing our data requirements; (4) control for country-level debt market and economic developments; (5) control for interest rates in the regressions using *#FC*, *Secured*, *Maturity*, or *%leadarranger* as dependent variables and control for *%leadarranger* in the regressions using *Interest rate*, *#FC*, *Secured*, or *Maturity* as dependent variables; and (6) jointly estimate the determinants of each loan term using a seemingly unrelated regression. To conserve space, we report the coefficient estimates for only the main variables of interest, *POST*, *IFRS*, and *POST * IFRS*.

Potential Effects of the 2008–2009 Financial Crisis

Panels A–C of Table 6 report the results of our respective tests for three alternative sample periods: the period omitting 2008 and 2009, the period from 2000 to 2007 and the period from 2003 to 2007. Specifically, we find that the coefficients on *POST * IFRS* remain qualitatively similar to those in our main analysis in Panel B of Table 3 when 2008 and 2009 are excluded from the sample. We further find that the coefficients on *POST * IFRS* remain comparable to those in our main analysis for most loan contract terms except the percentage of a loan held by lead arrangers for the 2000 (2003) to 2007 sample periods. Therefore, we conclude that our primary findings are not a consequence of the impact of the financial crisis.

Concurrent Enforcement Changes

In their study, [Christensen, Hail, and Leuz \(2013\)](#) suggest that the proactive review of financial statements by five EU countries, concurrent with the adoption of IFRS, actually drives their finding of an improved liquidity effect associated with the 2005 IFRS adoption.²⁰ To address the possibility that our results reflect only those countries without concurrent enforcement changes, we repeat our analysis separately for the five countries with concurrent enforcement changes and the remaining adopting countries. The subsample results in Panels D and E of Table 6 are qualitatively similar to those reported in Panel B of Table 3, with the exception of loan maturity for the five EU countries and the number of financial covenants for the remaining IFRS adopting countries. Therefore, we conclude that our inferences hold across country subsamples and that concurrent enforcement changes cannot explain our results.

Expanded Sample

To ensure that changes in loan terms are driven by changes in accounting standards rather than by our sample composition, our main analyses require a borrower to have at least one loan during the pre-IFRS period and at least one loan during the post-IFRS period. Here, we test for potential survivor bias by relaxing this requirement and including all loans with available variables during the sample period in our regression analysis. The revised sample comprises 6,574 loans to 1,471 borrowers from 17 treatment countries and 33,384 loans to 6,713 borrowers from 14 benchmark

²⁰ The five IFRS adopting countries with concurrent enforcement changes are Finland, Germany, The Netherlands, Norway, and the U.K.

TABLE 6
Sensitivity Tests

Panel A: Drop Financial Crisis Period (Years 2008 and 2009)

Variables	<i>Spread</i> (1)	<i>#FC</i> (2)	<i>Secured</i> (3)	<i>Maturity</i> (4)	<i>%leadarranger</i> (5)
<i>POST</i>	−27.489*** (−5.54)	−0.162* (−1.92)	0.088 (1.52)	0.046 (1.33)	0.081*** (4.16)
<i>IFRS</i>	−44.644*** (−4.57)	−1.812*** (−7.65)	−0.747*** (−5.18)	0.210*** (3.32)	0.102*** (2.85)
<i>POST * IFRS</i>	35.679*** (3.76)	−0.619** (−2.04)	0.540*** (3.60)	−0.113* (−1.93)	0.173*** (4.01)
Number of loans	14,990	22,198	22,183	22,198	8,317

Panel B: The Effects of Mandatory IFRS Adoption during 2000–2007

Variables	<i>Spread</i> (1)	<i>#FC</i> (2)	<i>Secured</i> (3)	<i>Maturity</i> (4)	<i>%leadarranger</i> (5)
<i>POST</i>	−30.294*** (−7.56)	−0.152*** (−3.93)	0.157** (2.17)	0.060*** (3.63)	0.090*** (3.94)
<i>IFRS</i>	−38.622*** (−3.03)	−1.499*** (−5.08)	−0.682*** (−4.62)	0.221*** (4.40)	0.109*** (2.86)
<i>POST * IFRS</i>	25.583*** (2.81)	−1.366*** (−5.05)	0.571*** (3.08)	−0.109* (−1.93)	0.070 (1.64)
Number of loans	11,238	16,408	16,394	16,408	6,273

Panel C: The Effects of Mandatory IFRS Adoption during 2003–2007

Variables	<i>Spread</i> (1)	<i>#FC</i> (2)	<i>Secured</i> (3)	<i>Maturity</i> (4)	<i>%leadarranger</i> (5)
<i>POST</i>	−15.190*** (−5.62)	0.006 (0.18)	−0.013 (−0.24)	0.044 (1.57)	0.082 (1.09)
<i>IFRS</i>	−37.589*** (−2.94)	−1.473*** (−5.30)	−0.880*** (−5.87)	0.128 (1.58)	0.207*** (4.65)
<i>POST * IFRS</i>	21.786** (2.28)	−1.254*** (−4.01)	0.721*** (4.06)	−0.093* (−1.69)	−0.036 (−0.93)
Number of loans	7,678	11,510	11,475	11,510	4,313

Panel D: Five IFRS Countries with Enforcement Change versus Non-IFRS Countries

Variables	<i>Spread</i> (1)	<i>#FC</i> (2)	<i>Secured</i> (3)	<i>Maturity</i> (4)	<i>%leadarranger</i> (5)
<i>POST</i>	−24.404*** (−5.55)	−0.124 (−1.07)	0.114 (1.27)	−0.018 (−0.40)	0.060*** (2.76)
<i>IFRS</i>	−34.711*** (−5.71)	−1.710*** (−6.10)	−0.970*** (−10.26)	0.140* (1.80)	0.147*** (2.97)
<i>POST * IFRS</i>	42.801*** (6.68)	−0.663*** (−4.98)	0.731*** (6.60)	0.044 (0.58)	0.198*** (3.43)

(continued on next page)

TABLE 6 (continued)

Variables	<i>Spread</i> (1)	<i>#FC</i> (2)	<i>Secured</i> (3)	<i>Maturity</i> (4)	<i>%leadarranger</i> (5)
Number of loans	16,127	24,260	24,260	24,260	9,180

Panel E: IFRS Countries without Enforcement Change versus Non-IFRS Countries

Variables	<i>Spread</i> (1)	<i>#FC</i> (2)	<i>Secured</i> (3)	<i>Maturity</i> (4)	<i>%leadarranger</i> (5)
<i>POST</i>	-24.078*** (-5.63)	-0.121 (-1.03)	0.102 (1.16)	-0.013 (-0.30)	0.065*** (3.37)
<i>IFRS</i>	-51.674*** (-4.73)	-1.841*** (-5.37)	-0.697*** (-4.52)	0.247*** (3.38)	0.099** (2.29)
<i>POST * IFRS</i>	29.773** (2.40)	-0.832 (-1.56)	0.493** (2.48)	-0.234* (-1.93)	0.143*** (2.62)
Number of loans	16,341	24,849	24,834	24,849	9,712

Panel F: Expanded Sample during 2000–2011

Variables	<i>Spread</i> (1)	<i>#FC</i> (2)	<i>Secured</i> (3)	<i>Maturity</i> (4)	<i>%leadarranger</i> (5)
<i>POST</i>	-33.819*** (-4.47)	-0.106 (-0.72)	-0.007 (-0.09)	-0.012 (-0.36)	0.015 (1.09)
<i>IFRS</i>	-48.121*** (-8.19)	-1.747*** (-7.29)	-0.931*** (-7.32)	0.256*** (4.75)	0.047 (1.15)
<i>POST * IFRS</i>	36.108*** (2.80)	-0.745*** (-3.15)	0.711*** (3.97)	-0.140 (-1.56)	0.161*** (3.73)
Number of loans	25,290	39,958	39,958	39,958	16,169

Panel G: Control for Country Institutions

Variables	<i>Spread</i> (1)	<i>#FC</i> (2)	<i>Secured</i> (3)	<i>Maturity</i> (4)	<i>%leadarranger</i> (5)
<i>POST</i>	-12.703*** (-2.86)	-0.166*** (-3.05)	0.191*** (5.98)	-0.000 (-0.01)	0.118*** (3.69)
<i>IFRS</i>	-62.366*** (-3.67)	-1.304*** (-3.16)	-0.717*** (-4.64)	0.192 (1.32)	-0.028 (-0.52)
<i>POST * IFRS</i>	29.708*** (3.38)	-0.901*** (-3.46)	0.477*** (3.34)	-0.127 (-1.36)	0.184*** (3.78)
Number of loans	15,266	25,113	25,098	25,113	8,764

Panel H: Control for Interest Rates and Percentage of a Loan Held by Its Lead Arrangers

Variables	<i>Spread</i> (1)	<i>#FC</i> (2)	<i>Secured</i> (3)	<i>Maturity</i> (4)	<i>%leadarranger</i> (5)
<i>POST</i>	-23.416*** (-6.11)	0.072 (0.35)	-0.036 (-0.39)	-0.012 (-0.70)	0.115*** (3.76)
<i>IFRS</i>	-42.994*** (-6.28)	-1.339*** (-5.42)	-0.790*** (-4.30)	0.165*** (4.52)	0.148*** (4.92)

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TABLE 6 (continued)

Variables	Spread (1)	#FC (2)	Secured (3)	Maturity (4)	%leadarranger (5)
<i>POST</i> * <i>IFRS</i>	23.341** (2.05)	-0.461** (-2.44)	0.334*** (3.33)	-0.158* (-1.93)	0.133*** (2.72)
Number of loans	6,783	6,783	6,783	6,783	6,783

Panel I: Seemingly Unrelated Regression Estimates

Variables	Spread (1)	#FC (2)	Secured (3)	Maturity (4)	%leadarranger (5)
<i>POST</i>	-18.861*** (-5.51)	0.103* (1.87)	0.044*** (3.60)	-0.034** (-2.05)	0.135*** (11.27)
<i>IFRS</i>	-29.675*** (-5.04)	-0.947*** (-16.60)	-0.076*** (-3.51)	0.113*** (3.84)	0.126*** (7.90)
<i>POST</i> * <i>IFRS</i>	28.185*** (3.29)	-0.104** (-2.22)	0.046** (2.48)	-0.177*** (-4.13)	0.145*** (6.24)
Number of loans	6,783	6,783	6,783	6,783	6,783

Control Variables in All the Panels

Loan purpose fixed effects	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes

*, **, *** Indicate significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

The standard errors of coefficient estimates are adjusted for country clusters.

This table reports the effects of mandatory IFRS adoption on loan terms under alternative sample and regression specifications. Panel A reports the effects of mandatory IFRS adoption on loan terms after dropping years 2008 and 2009. Panel B reports the effects of mandatory IFRS adoption on loan terms during the 2000–2007 period. Panel C presents the effects of mandatory IFRS adoption on loan terms during the 2003–2007 period. Panel D reports the effects of mandatory IFRS adoption on loan contracts in five EU countries with concurrent enforcement changes. Panel E reports the effects of mandatory IFRS adoption on loan contracts in IFRS countries without concurrent enforcement changes. Panel F presents the effects of mandatory IFRS adoption on loan terms for an expanded sample, which includes loans to borrowers in treatment and benchmark samples during the sample period (2000–2011), without the requirement that a sample firm must have loans during both pre- and post-IFRS periods. Panel G reports the regression estimates by controlling for country-level debt market characteristics and economic development. Panel H reports the regression estimates after controlling for loan spread or %leadarranger. Panel I reports seemingly unrelated regression (SUR) estimates. All regressions control for loan purpose fixed effects and industry fixed effects.

countries. The results in Panel F of Table 6 indicate comparable *POST* * *IFRS* coefficients. Thus, we conclude that our primary inferences hold when our data restrictions are relaxed.

Control for Country-Level Debt Market and Economic Development Factors

In our main analysis, we use *IFRS* to capture the average difference between treatment and benchmark samples. As a robustness check, we further control for the following country-level institutional characteristics that have been shown to affect loan features (Bae and Goyal 2009): (1) *Log(GDP)*, the natural logarithm of GDP per capita for each country-year during the sample period; (2) *Debt/GDP*, the ratio of private credit to GDP for each country-year during the sample period; (3) *Sovereign Rating*, the natural logarithm of the sovereign credit rating published by S&P, with a higher value indicating better credit quality; (4) *Credit Rights*, a country's laws protecting creditors, using the creditor rights protection index outlined in Djankov et al. (2007); and (5) *Property Rights*, the mean value of three indices from La Porta, Lopez-de-Silanes, Shleifer, and Vishny (1998): (a) government corruption; (b) the risk of expropriation of private property by the government; and (c)

the risk of government repudiation of contracts. Higher values for our *Property Rights* variable indicate lower government corruption. The results, reported in Panel G of Table 6, show that our main findings remain qualitatively similar after controlling for these factors, with the exception of loan maturity.

Control for the Relation between Interest Rates and Other Loan Terms

The results in prior literature suggest a trade-off exists between loan terms. For example, when accounting quality is low, banks are likely to move away from accounting-based financial covenants and toward price protection and collateral provisions (Costello and Wittenberg-Moerman 2011). To control for this trade-off, we add *Interest rate* as a control variable in the regressions that use *#FC*, *Secured*, *Maturity*, or *%leadarranger* as dependent variables. In addition, we replace *#Lenders* with *%leadarranger* in the regression that use *Interest rate*, *#FC*, *Secured*, or *Maturity* as dependent variables. The results in Panel H of Table 6 show that the coefficients on *POST * IFRS* remain similar to those in our main analysis. Thus, we conclude that our primary inferences hold when we control for contract term trade-offs.

Joint Determination of Loan Terms

Following the robustness protocols observed in prior literature (Costello and Wittenberg-Moerman 2011; Kim et al. 2011), we address the possible joint determination of price and nonprice terms by estimating a system of equations using the seemingly unrelated regression (SUR) procedure. As our database is missing information for the *%leadarranger* variable for a number of the loan contracts in our main sample, this procedure reduces our sample size to 6,783 loans. The results in Panel I of Table 6 show that the coefficients on *POST * IFRS* remain similar for our new sample. Therefore, we conclude that our primary inferences remain unchanged when we use a seemingly unrelated regression procedure.

VII. CONCLUSION

Despite extensive research on the determinants of loan features across countries, limited attention has been paid to the impact of corporate financial reporting quality on loan contract terms. To address this issue, we examine the effect of mandatory IFRS adoption on subsequent loan terms. We use the 2005 mandatory adoption of IFRS as an exogenous event in order to study the impact of financial reporting on bank loans in a global setting.

Using a difference-in-differences design, we find that borrowers who mandatorily adopt IFRS experience an increase in interest rates, an increase in the likelihood that a loan will require collateral, and an increase in the fraction of a loan retained by its lead arrangers compared to those not adopting IFRS. Furthermore, we find that IFRS adopters experience a reduction in the number of accounting-based financial covenants, as well as their loan maturity. Among IFRS adopters, we further note that changes in loan contract terms are more pronounced for borrowers who experience greater accounting changes, as well as those whose accounting quality decreases after IFRS adoption. Overall, our study suggests that financial reporting plays a critical role in the determination of loan contract terms in an international setting.

While our study provides insight into the relation between financial reporting properties and loan terms, it should be noted that our conclusions are based on the results for a sample of borrowers with consistent loan activities both before and after mandatory IFRS adoption. However, our analysis is silent about those borrowers receiving loans before but not after IFRS adoption, as well as those borrowers who initiate loans only after adopting IFRS. Furthermore, our study explores several channels by which IFRS might influence information asymmetry, but we do not

attempt to disentangle whether one channel dominates the other. Instead, we acknowledge that the observed changes in bank loan contract terms after IFRS adoption could represent the joint effect of these channels. Future studies could conduct more in-depth examinations of these channels, as well as other mechanisms through which financial reporting might influence bank loan contracts and, on a broader level, debt markets.

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APPENDIX A

Variable Descriptions

Variable	Definition
Dependent Variables	
<i>Interest rate</i>	The all-in-drawn spread reported by DealScan. It is equal to the amount the borrower pays in basis points over LIBOR for each dollar drawn down, plus the annual fee per dollar paid to the lender.
<i>#FC</i>	The number of financial covenants imposed by a loan agreement.
<i>Secured</i>	An indicator variable that takes the value of 1 if a loan is backed by collateral, and 0 otherwise.
<i>Maturity</i>	The natural logarithm of the number of months between the facility's issue date and the date when the loan matures.
<i>%leadarranger</i>	The percentage of a loan held by its lead arrangers.
Independent Variables	
<i>POST</i>	An indicator variable denoting the post-IFRS adoption period. For firms from mandatory IFRS adopting countries, <i>POST</i> takes the value of 1 for the period after a firm mandatorily adopts IFRS, and 0 otherwise. For firms from non-IFRS countries, <i>POST</i> takes the value of 1 for any loans issued since December 31, 2005, and 0 otherwise.
<i>IFRS</i>	An indicator variable that takes the value of 1 if a firm comes from mandatory IFRS adopting countries, and 0 otherwise.
<i>Log(LoanSize)</i>	The natural logarithm of the facility amount in millions of U.S. dollars.
<i>Institution Loan</i>	An indicator variable that takes the value of 1 if a loan's type is term loan B, C, or D (institutional term loans), and 0 otherwise.
<i>Term Loan</i>	An indicator variable that takes the value of 1 if a loan's type is term loan, and 0 otherwise.
<i>Revolver</i>	An indicator variable that takes the value of 1 if a loan type is line of credit, and 0 otherwise.
<i>#Lenders</i>	The number of banks joining a facility.
<i>Past relation</i>	The number of loans that lead arrangers have led in the five years prior to the current loan, scaled by the total number of loans a firm received in those five years.
<i>Credit Rating</i>	The numerical equivalent of S&P's or Moody's senior debt rating. It takes the value of 1 for the highest senior debt rating, through 21 for the lowest senior debt rating. For firms not rated by S&P, we assign Moody's senior debt rating. We use a conventional conversion scheme to match ratings from the two rating agencies. The variable is set to 22 for firms without an available S&P or Moody's debt rating.
<i>Loan Purpose</i>	A dummy variable to indicate the purpose for a loan.
<i>Log(Assets)</i>	The natural logarithm of a borrower's total assets in millions of U.S. dollars in the year prior to when a loan is issued.
<i>Leverage</i>	The ratio of the sum of short- and long-term debt to total assets in the year prior to when a loan is issued.
<i>MTB</i>	The ratio of market value of equity to book value of equity in the year prior to when a loan is issued.
<i>ROA</i>	The ratio of operating income to total assets in the year prior to when a loan is issued.
<i>LOSS</i>	A dummy variable to indicate a firm-year that reports an accounting loss.
<i>Tangibility</i>	The ratio of net PPE to total assets in the year prior to when a loan is issued.

(continued on next page)

APPENDIX A (continued)

Variable	Definition
<i>Current Ratio</i>	The ratio of current assets to current liabilities in the year prior to when a loan is issued.
<i>Market Benchmark</i>	The yearly mean value of the dependent variables (i.e., <i>Interest rate</i> , <i>#FC</i> , <i>Secured</i> , <i>Maturity</i> , and <i>%leadarranger</i>) from the observations in countries that do not mandate IFRS adoption.
<i>LOW_ΔGAAP</i>	An indicator variable that takes the value of 1 if an IFRS adopting firm's change in accounting standards from local standards to IFRS is below the median change of the IFRS sample firms, and 0 otherwise.
<i>HIGH_ΔGAAP</i>	An indicator variable that takes the value of 1 if an IFRS adopting firm's change in accounting standards from local standards to IFRS is above the median change of the IFRS sample firms, and 0 otherwise.
<i>LOW_ΔAQ</i>	An indicator variable that takes the value of 1 if an IFRS adopting firm's change in the variance of abnormal total accruals from the pre-IFRS to post-IFRS period is below the median change of the IFRS sample firms, and 0 otherwise.
<i>HIGH_ΔAQ</i>	An indicator variable that takes the value of 1 if an IFRS adopting firm's change in the variance of abnormal total accruals from the pre-IFRS to post-IFRS period is above the median change of the IFRS sample firms, and 0 otherwise.

APPENDIX B

Sample Selection Procedures

Number of Loans Issued During the 2000–2011 Period in DealScan	157, 256 ^a
Less:	
Loans with missing firm ID in Worldscope (non-U.S. firms) or Compustat (U.S. firms)	(92,082)
	65,174
Loans issued to financial firms ($6000 \leq \text{SIC} \leq 6999$)	(16,249)
	48,925
Loans with missing control variables in regression analysis	(6,951)
	41,974
Loans issued to voluntary IFRS adopters from both IFRS and non-IFRS countries	(1,809)
	40,165
Loans to borrowers in either the pre-IFRS, or post-IFRS period, but not both	(13,584)
	26,581
Countries with fewer than three firms	(107)
Final Sample	26,474

^a This sample starts with 46 countries in DealScan that are also covered by Worldscope (non-U.S. firms) or Compustat (U.S. firms), including Argentina, Australia, Austria, Belgium, Brazil, Canada, Chile, China, Czech Republic, Denmark, Egypt, Estonia, Finland, France, Germany, Greece, Hong Kong, Hungary, India, Indonesia, Ireland, Israel, Italy, Japan, Luxembourg, Malaysia, Mexico, The Netherlands, Norway, Pakistan, Peru, Philippines, Poland, Portugal, Russia, Slovenia, South Africa, South Korea, Spain, Sweden, Switzerland, Taiwan, Thailand, the U.K., Venezuela, and the U.S. The final sample is reduced to 31 countries, due to the filtering procedures stated above.

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