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Bank Lending Standards and Borrower Accounting Conservatism

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Abstract. Bank lending standards vary over time. Periods in which firms find it relatively easy to borrow are followed by periods in which banks scrutinize borrowers more and tighten lending. We predict that changes in lending standards affect the accounting conservatism of bank-dependent firms. Using (i) a natural experiment that leads to certain banks tightening lending standards for plausibly exogenous reasons and (ii) time series variation in economy-wide bank lending standards, we find that borrowers increase their asymmetric timely loss recognition in response to the tightening of lending standards. Further, riskier borrowers, borrowers less likely to violate loan covenants, and borrowers whose banks tighten lending standards to a greater extent display larger increases in conservatism following the tightening of lending standards. These results suggest that borrowers internalize the costs and benefits of increasing conservatism. Finally, borrowers do not seem to decrease conservatism immediately after the lending standards are loosened. Overall, our results illuminate a commonly observed banking phenomenon that can influence firms' incentives to recognize losses, suggesting that developments in the banking sector can shape the information produced by firms in the real sector.

History: Accepted by Suraj Srinivasan, accounting.

Keywords: bank lending • lending standards • accounting conservatism • financial crises • bank monitoring

1. Introduction

Regulators, bankers, and the financial press often speak of cycles in bank lending. Periods in which firms find it relatively easy to borrow are followed by periods in which lending standards are tightened with more stringent loan terms.[1] A tightening of lending standards is accompanied by increased scrutiny of borrowers by banks whereas less scrutiny is observed in loosening periods. For example, the media often notes that during loosening periods, "[b]anks are lending quicker and requiring less information and documentation" (Gamble 1994, p. 45). In a survey of business lending practices in the United States from 1995 to 1997, when lending standards were relatively loose, the Federal Reserve notes that "formal projections of a borrower's future performance were present in only 20 to 30 percent of the loan approval documents reviewed, and that formal analysis of alternative or 'downside' scenarios was even less common."[2] In this study, we examine whether changes in lending standards over time affect borrowers' incentives to report conservatively.

Theories suggest that lending standards are partly shaped by the state of the economy. Economic upswings decrease the expected benefit of borrower-specific inspection because borrowers, on average, are more likely to meet their debt obligations than they are during other times (Ruckes 2004). As a result, banks have incentives to loosen their lending standards to save on costly scrutiny of borrowers during economic upswings. Lending standards can also change following shocks to banks, which affect banks' risk tolerance and influence their incentives to scrutinize borrowers (e.g., Van den Heuvel 2002, Murfin 2012). Such banking shocks can arise from various sources, such as bank capital losses, changes in bank regulatory or competitive environments, or changes in monetary policies, and the corresponding change in bank lending standards can occur *independently* of the state of the economy. For example, Chava and Purnanandam (2011) show that U.S. banks that suffer large losses from their foreign businesses tighten their domestic lending even when the U.S. economy is rather stable.

Exploiting the tightening of lending standards arising from banking shocks, we examine the resulting changes in borrowers' incentives to recognize losses. When banks suffer capital losses and become less risk tolerant, they inspect borrowers more closely (e.g., Chava and Purnanandam 2011, Murfin 2012). For example, they intensify the collection and analysis of information from both public and proprietary sources, which allows them to cross-check the financial information and projections provided by borrowers for loan acquisition or debt compliance purposes. To

analyze alternative or "downside" scenarios, banks especially pay close attention to borrowers' reporting of losses. Meanwhile, borrowers can expect increases in the likelihood and intensity of bank inspections during tightening periods. When banks find signs of loss understatements or gain overstatements, this damages the borrower's trustworthiness, potentially resulting in more scrutiny and less credit access (Chen 2016). To avoid these costs, borrowers have incentives to recognize losses in a timelier manner than gains during tightening periods. Therefore, we expect to observe an increase in borrowers' asymmetric timely loss recognition (i.e., conditional conservatism) in tightening periods, an effect we refer to as the deterrent effect of bank inspection.

The deterrent argument applies to firms dependent on banks for credit, including those that must demonstrate debt compliance on current loans and those considering new debt or expecting to renew or renegotiate their existing debt contracts. However, increasing conservatism is not costless to borrowers. Some arguments predict that borrowers maintain or even decrease their conservatism when lending standards are tightened. For example, firms will weigh the benefits of increasing conservatism against the costs of violating debt covenants. Firms have incentives to decrease conservatism to avoid triggering covenant violations when they expect lenders to be intransigent in debt renegotiations, which predicts a reduction in borrower conservatism during tightening periods when banks are particularly intransigent. Consistent with the effect of bank intransigence on borrowers' reporting incentives, Martin and Roychowdhury (2015) find that borrowers decrease their conservatism after banks hedge their credit exposures via credit default swaps (CDSs), which makes banks more intransigent in renegotiations upon negative credit events experienced by the borrower.

Other studies highlight the costs associated with decreasing conservatism, such as reputational losses in debt markets (e.g., Ahmed et al. 2002). Therefore, firms have incentives to maintain their conservatism rather than to increase it if such increases are unlikely to persist. Finally, instead of increasing conservatism to appease existing lenders, borrowers can try to obtain capital from other sources, including new banks with relatively loose lending standards.[3] However, switching lenders is costly for borrowers because of new lenders' concerns of adverse selection (e.g., Rajan 1992) and the loss of benefits associated with relationship loans (e.g., Bharath et al. 2011). Overall, whether and how changes in lending standards affect borrowers' conservatism are empirical issues.

Exploiting a natural experiment based on capital shocks exclusive to certain U.S. banks that did not affect U.S. bank-dependent firms, our first test assesses whether *borrowers* of affected banks adjust their reporting conservatism in response. This experiment is based on the emerging-market financial crises of the late 1990s, that is, the Asian crisis of 1997, the Russian crisis of 1998, and the Latin American crisis of 1999. Although these events occurred outside of the United States, U.S. banks with credit exposure to the crisis countries suffered significant capital losses, which, in turn, affected their capacity to extend loans. These shocks resulted in a rather exogenous tightening of loans to the U.S. borrowers of exposed banks (Chava and Purnanandam 2011, Lo 2014). During these crises, the media noted that "[c]ompanies in the U.S.—not just in far-off Asia or Latin America—are finding it tougher to get the credit they need to expand their businesses... Nothing has changed about the firm, and suddenly the capital is available only at a premium or not at all" (Little 1998, p. A1). Furthermore, although U.S. banks that had credit exposure to the crisis countries were affected by the crisis events (Kho et al. 2000), other U.S. banks were not, and we use their borrowers as a control group in a difference-in-differences test to identify the effects of a tightening of lending standards on borrowers' conservatism.[4]

We ensure that our sample borrowers have minimal direct exposure to the crisis events. The tests focus on the change in borrowers' conservatism from two years before the start of the crises in 1997Q3 to two years afterward and compare these changes across borrowers of exposed banks and other borrowers after controlling for concurrent changes in borrower characteristics. Compared with other borrowers who display no significant change in their conservatism, we find that borrowers of exposed banks show a significant increase in accounting conservatism.

To reinforce our inferences, we conduct cross-sectional tests *within* borrowers of exposed banks. We find that the increase in conservatism is greater among borrowers who were subject to a greater tightening of lending standards during the crisis period because either (i) their banks had greater exposure to the crisis countries or (ii) the borrowers exhibited greater credit risk and attracted more scrutiny when their banks became less risk tolerant. Further, our evidence suggests that borrowers internalize the costs of potential covenant violations in their decision to increase conservatism following the tightening of lending standards; they increase conservatism to a lesser extent if they have a higher likelihood of violating covenants. Finally, we examine borrowers of exposed banks that obtain additional loans during the crisis period separately from other treatment borrowers. We find that both groups of borrowers increase conservatism during the crisis period, suggesting that our findings are applicable to borrowers that must demonstrate debt compliance on their existing loans as well as borrowers seeking additional bank credit.

Our next set of tests assesses whether economy-wide changes in bank lending standards in the United States

affect bank-dependent firms' conservatism. This analysis is based on an extended period from 1990Q2 to 2014Q1 and allows us to confirm whether inferences from the natural experiment discussed previously apply to other periods. Furthermore, cyclical changes of lending standards in the time series allow us to evaluate the effects of tightening versus loosening of lending standards on borrowers' conservatism.

We use the Federal Reserve Board's Senior Loan Officer Opinion Survey on Bank Lending Practices (SLOOS) to identify economy-wide changes in bank lending standards. After controlling for concurrent changes in macroeconomic conditions, we find that bank-dependent firms increase their conservatism in response to an economy-wide tightening of lending standards. However, economy-wide loosening does not immediately lead to a parallel decrease. This latter finding is in line with prior research highlighting the costs associated with decreasing conservatism (Basu 1997, Ahmed et al. 2002, Chung and Wynn 2008, Nikolaev 2010).

Overall, our results from both the natural experiment and economy-wide analyses suggest that borrowers increase their conservatism in response to a tightening of lending standards, presumably because of increased bank scrutiny. To further reinforce our inferences, we conduct tests at the loan level. For example, we find that loans negotiated in tightening periods include significantly more contractual terms that necessitate scrutiny (i.e., covenants) than in other periods. We view this as reflective of banks' inclination to scrutinize more during tightening periods. Anticipating such a tendency of banks, borrowers have incentives to report more conservatively.

Our study contributes to the literature in several ways. First, it complements cross-country studies showing that the significance of banks in the economy is positively associated with firms' conservatism (e.g., Ali and Hwang 2000, Ball et al. 2008). By studying *intertemporal* changes in lending standards of banks in the United States and the corresponding change in their scrutiny efforts, we illuminate an important *mechanism* through which banks affect firms' conservatism. More generally, our results are in line with the view that developments in the banking sector can shape the information produced by firms in the real sector.

Banks have a competitive advantage in scrutinizing borrowers (Diamond 1984, Fama 1985), which may create positive externalities for the borrowers' other stakeholders. For example, Gul and Tsui (1997) argue that bank scrutiny lowers audit fees because it can decrease auditors' concerns about a borrower's misreporting risk. Although studies have discussed the links between bank scrutiny and financial reporting outcomes, little direct evidence of such links has emerged. By showing that more stringent lending standards and bank scrutiny induce borrowers' conservatism, we offer concrete examples of the potential externalities.

Finally, prior studies show that modified loan terms are related to borrowers' conservatism (e.g., Beatty et al. 2008). We extend this literature by highlighting that the influence of banks on borrowers' reporting incentives is more pervasive than previously documented. Intertemporal changes in bank lending standards and scrutiny routinely occur in practice. Our findings suggest that such changes can incentivize borrowers to change their reporting conservatism, and this can happen even if the borrower is not seeking new loans, which is accompanied with changes in loan terms.

Our study is closely related to a few recent studies. Tan (2013) finds that firms increase conservatism following covenant violations,[5] especially in situations such as when lenders put chief restructuring officers in place. His results highlight the transfer of control rights from shareholders to lenders as a mechanism through which banks increase firms' conservatism. Studying lender representation on a borrower's board as an alternate bank monitoring mechanism, Erkens et al. (2014) show that such monitoring leads to a reduction, rather than an increase, in the borrower's conservatism.[6] We complement these results by highlighting that banks' influence can be much more pervasive; indeed, the influence can be observed (i) well *before* the control rights transfer and in the absence of lender representation on borrowers' boards and (ii) through the scrutiny associated with lending standards.

Using a sample of 529 U.S. firms, Martin and Roychowdhury (2015) highlight that hedging by banks via CDSs can decrease their monitoring of borrowers while simultaneously making them more intransigent in renegotiations. This provides borrowers with the incentive and opportunity to decrease their conservatism after a CDS trade initiation to avoid triggering covenant violations. Our context is different. Although banks' intransigence in renegotiations and monitoring of borrowers are important features in our context, *both* of these features are expected to increase during periods of tightening lending standards and decrease during loosening periods. As such, borrowers' reporting incentives and constraints conflict with each other, leaving their reporting choices an empirical issue that we address in this study.[7]

In a contemporaneous paper, Li and Lobo (2015) document a reduction in firms' accounting conservatism during credit boom years relative to nonboom years. Although we are also interested in how credit cycles and lending standards influence borrowers' reporting incentives, our approach is different; we draw our main inferences using a natural experiment that results in tightening of certain banks' lending standards while leaving their U.S. borrowers relatively unaffected. This allows us to interpret a more causal effect of banks' lending standards on their borrowers' reporting choices as economic theories suggest that

lending standards and macroeconomic conditions that influence firm fundamentals are tightly linked with each other (see Section 2.1), making it difficult to identify the effects of the former.[8]

Finally, Lo (2014) shows that shocks exclusive to banks lead their borrowers to increase *voluntary* disclosures, presumably to facilitate the replacement of bank financing with funds from the public capital markets (particularly bonds). We differ from Lo by (i) focusing on bank-dependent borrowers with limited access to bond markets and (ii) exploring a different mechanism through which bank-specific shocks affect these borrowers' reporting. Our results highlight that borrowers increase the conservatism of their *mandatory* disclosures in response to greater bank scrutiny following bank-specific shocks.

2. Related Literature and Hypothesis Development

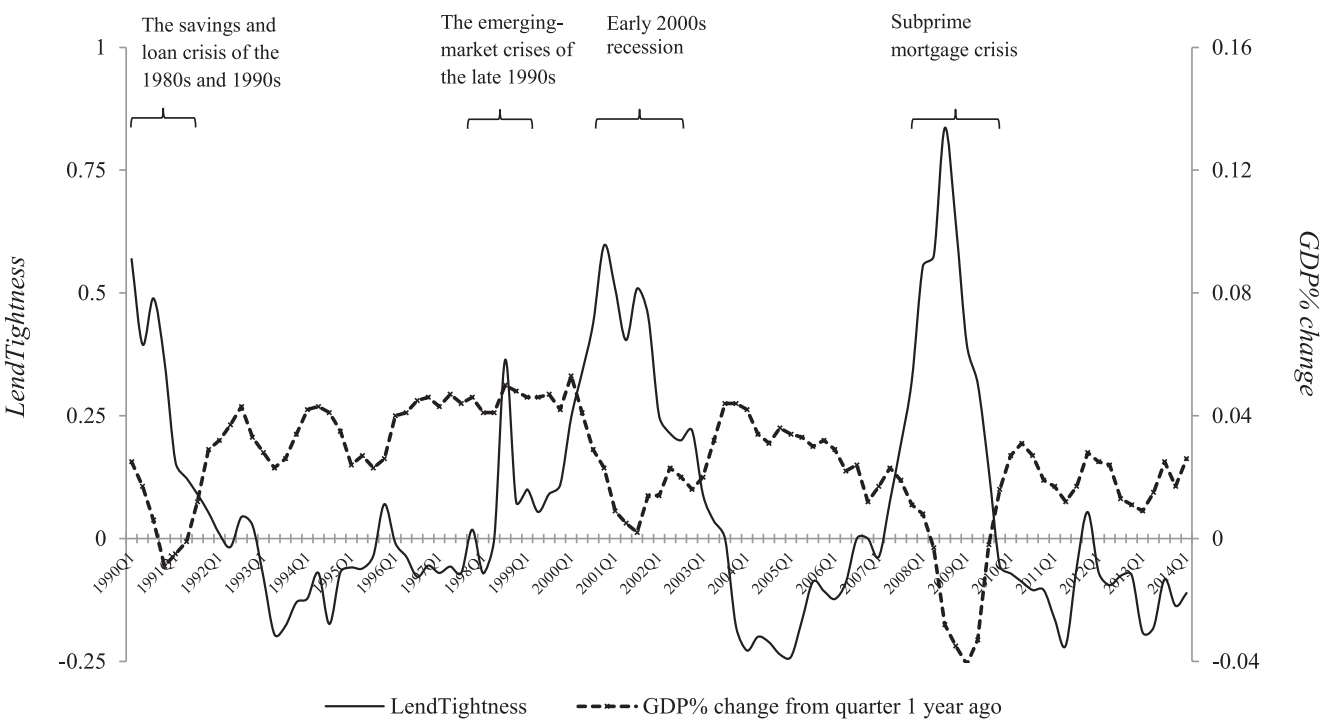
2.1. Related Literature

Research suggests a significant change in accounting conservatism over time both in the United States (Basu 1997, Givoly and Hayn 2000) and internationally (Ball et al. 2000). Ball et al. (2008) conjecture that such a change may be attributable to time-series changes in the economic importance of corporate debt markets but do not offer direct evidence. Separately, other research notes that banks vary their lending standards and scrutiny of borrowers over time (Asea and Blomberg 1998). Our research complements these studies by linking changes in bank lending standards to changes in borrower conservatism.

Figure 1 shows the changes in lending standards in the United States over our sample period from 1990Q1 to 2014Q1 (solid line). We identify economy-wide changes in the lending standards (*LendTightness*) using the SLOOS. As detailed in Section 5.1, increases in *LendTightness* suggest that more banks impose stricter lending standards and reflect greater tightening overall. In general, tightening comes in cycles with tightening in one period strongly predicting tightening in the foreseeable future. The first- and fourth-order autocorrelations of *LengTightness* in Figure 1 are about 0.9 and 0.5, respectively.

Figure 1 also shows that *LendTightness* is negatively correlated with the quarterly percentage change in the U.S. gross domestic product (GDP; dotted line). This is consistent with theories that suggest tightening of lending standards is associated with economic declines. In contrast, when economic prospects are bright, banks loosen lending standards to minimize costly scrutiny of borrowers. Ruckes (2004) suggests that during economic booms, the default risk of the average borrower improves, which, in turn, decreases the

Figure 1. Change in Bank Lending Standards (*LendTightness*) and GDP Percentage Change in the United States



Notes. The data are from the Federal Reserve Bank of St. Louis. *LendTightness* is our measure of changes in bank lending standards in the United States. It is taken from the data series on the "Net Percentage of Domestic Banks Tightening Standards for Commercial and Industrial Loans to Large and Middle-Market Firms" (https://fred.stlouisfed.org/series/DRTSCILM), in which large and middle-market firms refer to those with annual sales of $50 million or more. GDP information is taken from the data series on "Real Gross Domestic Product, Percent Change from Quarter One Year Ago, Seasonally Adjusted" (https://fred.stlouisfed.org/series/A191RO1Q156NBEA#).

expected benefits of borrower-specific inspection. Thus, banks inspect borrowers less rigorously, which makes it easier for firms to borrow.

Lending standards can also change following shocks to banks, and such changes can occur independent of the state of the economy. For example, although Figure 1 shows that the change in U.S. GDP in the late 1990s appears rather stable, there is a spike in *LendTightness* around that period as certain U.S. banks suffered losses in the emerging-market financial crises (see Section 3.2 for more details). In general, banks suffering significant losses are concerned about undercapitalization, which decreases their risk tolerance.[9] As a result, they intensify their risk identification and evaluation, reassess the riskiness of their current business, constrain their risk-taking practices, and tighten their lending standards even when there is no apparent change in borrowers' risk (e.g., Van den Heuvel 2002, Chava and Purnanandam 2011, Murfin 2012). In addition, banking shocks can arise from other sources, such as changes in bank regulatory or competitive environments or changes in monetary policies. These various factors together with the general business cycles jointly influence the observed changes in bank lending standards.

Regardless of the cause of the change in lending standards, we expect banks to increase their demand for information during tightening periods to facilitate the screening and monitoring of borrowers. The media often notes that loan officers verify borrower information extensively during tightening periods (Braithwaite 2013). Conversely, when lending standards are relaxed, banks decrease their information demands. Consistent with these observations, Lisowsky et al. (2017) report that, during the housing boom of the early 2000s, when lending standards were relaxed, banks decreased their collection of audited financial statements from private borrowers in the construction industry. Building on this insight, we ask whether borrowers adjust their accounting conservatism in response to changes in lending standards.

2.2. Hypothesis Development

We expect banks to inspect borrowers more closely during tightening periods. This occurs, for example, after banks suffer capital losses and become less risk tolerant. To minimize unknown risk exposures and to better discriminate among borrowers, banks are likely to collect and analyze more information, which can help confirm the bank's understanding of the borrowers' inherent risk and verify their debt compliance. Roberts (2015, p. 69) highlights that, even in relationship lending, "the incentives for borrowers to engage in ex post moral hazard are persistent." Thus, banks acquire information about current borrowers, especially when they are relatively less risk tolerant and desire to limit potential loss exposures. Consistent with these arguments, Murfin (2012) shows that banks reevaluate their risk assessment ability after experiencing loan defaults. They tighten contracting terms even with borrowers in industries and geographical areas unrelated to the defaulting borrowers. Thus, tightening of lending standards has implications even for borrowers that are not experiencing deterioration in their credit risk, and we predict that they would change their financial reporting in response.

Banks can make on-site visits to the borrower; examine the borrower's internal financial and operating records; or clarify the borrower's reported information with its employees, directors, and auditors.[10] They can collect information from all these sources, and they can use this information more frequently in scrutinizing borrowers and for a greater proportion of their borrowers. Such close inspections help banks to verify their borrowers' financial condition and minimize their exposure to borrowers' underreporting of losses.[11]

We further argue that borrowers perceive an increased likelihood and intensity of bank inspection during tightening periods. As the condition of lending standards is well publicized by regulators and the media, borrowers are alert to any tightening of standards and the accompanying bank scrutiny.[12] They can expect more frequent and thorough bank inspection. If banks find indications of loss understatements or gain overstatements, it can hurt the borrower's trustworthiness, potentially leading to more scrutiny and less credit access (Chen 2016). To avoid these costs, borrowers have incentives to recognize losses in a timelier manner than gains, especially when they perceive an increased threat of bank inspection during tightening periods. We therefore predict an increase in borrowers' asymmetric timely loss recognition in such periods. We refer to this effect as the deterrent effect of bank inspection, and it applies to borrowers that must demonstrate debt compliance on current loans and those considering new debt or expecting to renew or renegotiate current loans.

Prior studies reinforce our arguments. For example, studies of loan renegotiations have found that declines in banks' financial health and a tightening of lending standards (i) are among the most important predictors of renegotiations and (ii) accelerate the timing of renegotiations (Roberts and Sufi 2009b, Denis and Wang 2014, Roberts 2015).[13] As renegotiations prompt bank inspections, borrowers are likely to perceive an increased threat of bank inspection during tightening periods, which can affect their incentives for timely recognition of losses. Accordingly, our hypothesis is stated in alternate form as follows.

Hypothesis 1 (H1). *Borrowers' asymmetric timely loss recognition (i.e., conditional conservatism) increases when banks tighten their lending standards.*

Nevertheless, alternative arguments predict that borrowers may not increase their conservatism during tightening periods. First, conservatism can lead to early

violation of debt covenants (e.g., Zhang 2008, Nikolaev 2010), which is associated with declines in the borrower's leverage and capital investment and an increase in CEO turnover (e.g., Chava and Roberts 2008, Roberts and Sufi 2009a, Nini et al. 2012). Given these costly consequences, borrowers may not increase their conservatism during tightening periods. Further, Martin and Roychowdhury (2015) argue that borrowers prefer reducing conservatism to avoid violating covenants when they expect lenders to be intransigent in loan renegotiation. This argument predicts that borrowers may even decrease their conservatism during tightening periods as banks are particularly intransigent in such times.[14]

Second, borrowers may be reluctant to increase conservatism if the increase is unlikely to persist. Prior studies highlight different costs associated with decreasing conservatism, including loss of borrower's reputation in debt markets (Ahmed et al. 2002, Nikolaev 2010), increase in litigation risks (Basu 1997, Qiang 2007, Chung and Wynn 2008, Khan and Watts 2009), and higher auditor pressure and audit fees (Basu 1997, Holthausen and Watts 2001, DeFond et al. 2015). These costs can incentivize borrowers to maintain their conservatism during tightening periods if they do not expect any increases in it to persist.

Third, borrowers may not have incentives to increase conservatism if tightening of lending standards is temporary and short-lived. However, this is unlikely to be the case because, as reported previously, tightening of lending standards is cyclical with tightening in one period predicting tightening in the foreseeable future.

Finally, rather than increasing conservatism, borrowers can seek funds from other banks that are not tightening their lending standards. However, concerns of adverse selection can hinder such attempts. The new uninformed bank will be hesitant to grant credit to the borrower as it is unable to determine whether (i) the borrower needs an alternate source of funds because of the poor health of its prior banks or (ii) the prior bank is not granting additional credit because it has private negative information about the borrower (Sharpe 1990, Rajan 1992, Schenone 2010). Consistent with information asymmetries limiting borrowers' ability to switch lenders, Bharath et al. (2011) show that even when borrowers manage to obtain loans from a new bank, these loans are associated with higher loan spreads, a smaller amount, and more collateral requirements.[15] Indeed, when approaching new uninformed banks, borrowers can reasonably expect these banks to inspect them intensively, which deters borrowers from underreporting losses and may even lead to an increase in conservatism.

3. Research Design

3.1. Overall Test Strategy

Testing the relationship between bank policies and borrower reporting requires careful consideration. One concern is correlated omitted variables—factors affecting lending standards (e.g., borrower prospects) can simultaneously affect borrowers' reporting incentives. Reverse causality can also be a concern—borrowers' reporting may directly influence banks' perception about credit risk and lead them to alter their lending decisions (Bharath et al. 2008). To mitigate these concerns, our first test studies shocks to lending standards and assesses whether borrowers adjust their conservatism in response. As these lending shocks originate from the emerging-market financial crises, which were neither a direct result of nor had obvious links with the financial reporting of U.S. borrowers, this test mitigates concerns that the changes in lending standards (i.e., our treatment) are endogenous to changes in borrower reporting.

Two additional features of this test help to strengthen its inferences. First, we can use borrowers whose banks are not subject to the lending shocks as control borrowers to mitigate the effect of concurrent macroeconomic changes. Second, as there is considerable variation in the treatment effect across the treatment borrowers, we can conduct cross-sectional tests *only* among these borrowers. This mitigates concerns that the treatment and control borrowers are different and, hence, our difference-in-differences analyses are confounded. We discuss these tests in greater detail in Sections 3 and 4.

Our first set of tests is based on a specific banking shock in the late 1990s. To determine whether similar inferences apply to other time periods, Section 5 presents another set of tests based on an extended period from 1990Q2 to 2014Q1, which examines whether economy-wide changes in bank lending standards affect borrower conservatism after controlling for concurrent macroeconomic changes. To provide additional insights, we also exploit the tightening versus loosening of lending standards in the extended time series to assess how these opposite changes affect borrower's reporting.

3.2. Emerging-Market Financial Crises and U.S. Loan Markets

Our first test is a natural experiment based on shocks exclusive to certain U.S. banks arising from their exposure to the emerging-market financial crises—the Asian crisis of 1997, the Russian crisis of 1998, and the Latin American crisis of 1999. Prior studies, including Chava and Purnanandam (2011) and Lo (2014), discuss in detail how these foreign events affected the exposed banks' domestic lending to U.S. borrowers. We provide the background in the following.

In the late 1990s, severe speculative currency attacks led to forced currency devaluation and debt default in the crisis countries, including Thailand, Indonesia, Malaysia, the Philippines, South Korea, Russia, and Brazil (Eichengreen 2003). During these events, several money center banks in the United States experienced

large losses based on their exposure to the crisis countries in the forms of sovereign debt and private business loans. Appendix A lists the sample banks that were exposed to the emerging-market financial crises.

As noted by Kraus (1998, p. 1), "The exposure is concentrated in a handful of large U.S. banks whose capital could quickly erode... That, in turn, could tighten domestic lending." Chava and Purnanandam (2011) and the September 1998 Federal Reserve SLOOS indicated that U.S. banks exposed to the crises decreased domestic lending volume and raised loan spreads to a greater extent than other U.S. banks following the crises. Lo (2014) discusses various types of evidence from the Federal Reserve and his own analyses, suggesting that these foreign events led to a significant tightening of domestic lending standards by exposed banks. For example, according to the Federal Reserve, more than a quarter of the surveyed large U.S. banks cited lower risk tolerance and tightening of their lending standards during the crises.[16] There were fewer loan renewals and new loan approvals. When the banks did grant loans, they imposed stricter price and nonprice loan terms. The loan spread on commercial and industrial (C&I) loans spiked significantly during the crises. By the end of 1998, the average loan spread on C&I loans was at a level last seen during the U.S. savings and loan crisis (Lo 2014).

As the crises mainly occurred outside of the United States, they hurt the ability of the exposed U.S. banks to lend at home, and these events created a rather exogenous loan tightening for the U.S. borrowers of exposed banks. We test whether these borrowers (i.e., our treatment borrowers) increased their accounting conservatism during the crisis period. Other U.S. banks were little affected by the events, which allows us to use their borrowers as controls in difference-in-differences analysis to identify the treatment effect.

3.3. Measuring Accounting Conservatism

Our main tests use Ball and Shivakumar's (2005) methodology to capture the asymmetry in the timely recognition of losses versus gains. We estimate a regression model based on the following equation:

$$ACC_{it} = \beta_i + \beta_1 DCFO_{it} + \beta_2 CFO_{it} + \beta_3 DCFO_{it} \times CFO_{it} + \varepsilon_{it} \quad (1)$$

where *ACC* is accruals scaled by average total assets, *CFO* is cash flow from operations scaled by average total assets, and *DCFO* is an indicator variable equal to one if *CFO* is negative and zero otherwise. This model uses *CFO* to capture gains and losses that can be recognized via accruals. If the recognition of losses is timelier, then the coefficient on the interaction term $DCFO \times CFO$ (β_3) is expected to be positive, and it captures accounting conservatism.

3.4. Difference-in-Differences Test of Change in Conservatism

To implement our difference-in-differences research design, we modify Equation (1) to a regression as follows:

$$\begin{aligned} ACC_{it} = {} & \beta_i + \beta_1 DCFO_{it} + \beta_2 CFO_{it} + \beta_3 DCFO_{it} \times CFO_{it} \\ & + \beta_4 DCFO_{it} \times ExpoMBK_i \\ & + \beta_5 CFO_{it} \times ExpoMBK_i \\ & + \beta_6 DCFO_{it} \times CFO_{it} \times ExpoMBK_i + \beta_7 Crisis_t \\ & + \beta_8 DCFO_{it} \times Crisis_t + \beta_9 CFO_{it} \times Crisis_t \\ & + \beta_{10} DCFO_{it} \times CFO_{it} \times Crisis_t \\ & + \beta_{11} Crisis_t \times ExpoMBK_i \\ & + \beta_{12} DCFO_{it} \times Crisis_t \times ExpoMBK_i \\ & + \beta_{13} CFO_{it} \times Crisis_t \times ExpoMBK_i \\ & + \boldsymbol{\beta_{14} DCFO_{it} \times CFO_{it} \times Crisis_t \times ExpoMBK_i} \\ & + \sum_{k=1}^{K} \gamma_k FirmControls + \varepsilon_{it}, \end{aligned} \quad (2)$$

where *Crisis* is an indicator variable equal to one for the crisis period (1997Q3 to 1999Q2). The precrisis period (1995Q3 to 1997Q2) is the benchmarking period. *ExpoMBK* is an indicator variable equal to one if borrower *i*'s main bank was exposed to the crisis events. As *ExpoMBK* is time-invariant for each borrower, its main effect is subsumed by the firm-fixed effect. Thus, *ExpoMBK* is not included separately in the regression. The other variables are defined as before. (Bold formatting in Equations (2)–(9) indicates the variable of interest.)

We control for firm-fixed effects (denoted by β_i in Equation (2)) so the identification of the effect of interest comes from the variation in conservatism for the *same* firm before and after the crises. This allows us to use each borrower as its own control to account for conservatism determinants that tend to be time-invariant, such as industry membership and governance and ownership structures (Watts 2003, Ahmed and Duellman 2007, Erkens et al. 2014).

Our test also controls for time-varying factors. In terms of systematic macroeconomic changes, exploiting shocks that stem from emerging markets implicitly holds the U.S. economy relatively constant over time. To explicitly account for subtle macroeconomic changes, we use borrowers of *unexposed* banks as controls. If all borrowers are affected by similar economy-wide factors, then changes in conservatism in the crisis period for borrowers of unexposed banks (captured by the coefficient β_{10} on the interaction term $DCFO \times CFO \times Crisis$) reflect the effect of concurrent macroeconomic changes on borrowers' reporting. As a result, the *differential changes* in the conservatism of borrowers of exposed banks (captured by the coefficient β_{14} on the interaction term $DCFO \times CFO \times Crisis \times ExpoMBK$) are attributed to the tightening of lending standards. We predict that β_{14} is positive and significant.

Regarding changes in firm-specific factors, we ensure that the borrowers in our sample were not directly affected by the crisis events. If one type of borrower was directly exposed to the crisis events and the other type was not, then the differential changes in conservatism that the coefficient β_{14} captures will be confounded by the borrowers' different exposures. We follow Lo (2014) to identify borrower exposures to the crisis countries. For each borrower, we assess the associations between its stock returns and the changes in currency values of the crisis countries before the crisis events.[17] If any of the associations are statistically significant, regardless of magnitude, we assume that the borrower might have been exposed. The rationale is that if the borrower was exposed, then the financial developments and changes in currency values of the crisis countries should affect its stock price. We find that only 5% (4%) of the treatment (control) borrowers were potentially exposed to the crisis countries. Our tests exclude these borrowers. As reported in Table 2, data from the COMPUSTAT segment file confirm that the remaining borrowers had minimal foreign businesses, reinforcing that our sample firms were not directly affected by the crisis events. Our results are robust to removing all firms with foreign businesses.

To further alleviate concerns that the observed changes in conservatism are driven by differential impact of the crisis on the fundamentals of borrowers of exposed banks and other borrowers, we include time-varying firm characteristics as controls (Chava and Purnanandam 2011). Following prior research, our main tests include firm size, the market-to-book ratio, and leverage (Martin and Roychowdhury 2015). We also include the ratio of cash to total assets to control for the availability of internal funds. We add these firm attributes and their interactions with *DCFO*, *CFO*, and *ExpoMBK* as firm-specific controls.[18]

3.5. Classifying Time Periods and Finding a Borrower's Main Bank and the Exposure of the Bank

Following Lo (2014), we estimate the regressions using a two-year precrisis period from 1995Q3 to 1997Q2 and a parallel two-year crisis period from 1997Q3 to 1999Q2.[19] To address concerns arising from data issues related to management forecasts, Lo also incorporates a one-year postcrisis period from 1999Q3 to 2000Q2 in his tests. To simplify our analyses, we do not include the postcrisis period, in part because our estimation is based on an asymmetric, piecewise linear model with firm-fixed effects, so the relatively short time horizon of the postcrisis period poses challenges for obtaining precise estimates for that period.[20]

To identify a borrower's main bank, we follow Lo's approach and refer to the firm's borrowing history before the events. First, for every borrower, we identify all loans obtained during the six-year period from 1991Q3 to 1997Q2. Second, we identify the lead lender for each of the loans. Third, we calculate the following ratio for each borrower-lead bank pair:

$$LoanReli_{ik} = \frac{\textit{Total amount lent by bank } k \textit{ to firm } i \textit{ during the 6-year period}}{\textit{Total loan amount borrowed by firm } i \textit{ during the 6-year period}}$$

We classify the bank upon which the borrower relied the most as the main bank.[21]

In estimating *LoanReli*, the lead bank receives full credit for the total amount of a loan even though a syndicate could have funded some of the loan. This approach is justified because we focus on how bank scrutiny affects borrower conservatism, and the lead bank represents the syndicate for scrutinizing the borrower (Dennis and Mullineaux 2000). As Sufi (2007, p. 632) notes, "Lead arrangers establish and maintain a relationship with the borrower, and take on the primary information collection and monitoring responsibilities." *LoanReli* allows us to identify the main bank that monitors most of the borrower's loans and assess how changes in bank monitoring over time influences borrower's reporting incentives.[22]

We also note that we use precrisis period loan information to identify borrowers' main bank. The advantage of this approach is that the choice of banks is predetermined. Unless the borrowers could foresee the banking shocks and planned to make reporting changes around the crisis when they first chose their bank, which is unlikely,[23] our tests are relatively free from endogeneity arising from the joint determination of the choices of banks and financial reporting. However, because borrowers might borrow from another bank during the crisis period, one might argue that our approach of identifying a borrower's main bank contains measurement error.[24] To mitigate this concern, in a robustness check, we exclude all borrowers whose main bank changed during the crisis and confirm that our inferences remain unchanged.

Finally, to determine whether a bank was exposed to the crisis events, we follow Lo's (2014) approach and use the classification applied by Kho et al. (2000). The exposed banks identified by Kho et al. experienced significant declines in bank stock price during the crises while other banks did not. Chava and Purnanandam (2011) show that the correlation between the classification of exposed banks according to Kho et al. and other measures of exposure, such as charge-offs on loans to foreign borrowers, is more than 80%.

4. Sample, Descriptive Statistics, and Results for Difference-in-Differences Tests

4.1. Data Sources and Sample

We collect our sample from the intersection of the CRSP/COMPUSTAT and DealScan databases.[25] Table 1

Table 1. Sample Construction for the Test Exploiting the Emerging-Market Financial Crises as Shocks to Lending Standards

Number of firms	Description	Sample size
Nonbanking firms covered by the CRSP/COMPUSTAT database and with borrowing information from DealScan to identify main banks		4,481
Minus:		
Firms with more than one main bank	(163)	
Borrowers with one main bank		4,318
Minus:		
Firms not incorporated in the United States or firms with a foreign main bank	(566)	
Firms with access to the bond market	(963)	
Firms potentially directly exposed to the crisis countries	(108)	
Firms without required data for main analyses	(418)	
Borrowers used in the tests		2,263
Classifying borrowers using information from Kho et al. (2000) about bank exposure		
Borrowers of exposed banks		898
Other borrowers		1,365

summarizes the sample selection process. We begin by identifying 4,481 nonbanking firms included in the CRSP/COMPUSTAT database that have borrowing information in DealScan to identify their main bank. For each borrower's loans obtained in the six-year period before the crisis events, we use the "Lead Arranger Credit" indicator to identify the lead banks (Sufi 2007). We then ascertain the extent to which the borrower relies on each of its lead bank for loans (*LoanReli*) and identify the bank(s) the firm depends on most. Most firms' borrowing is concentrated with one main bank (4,318 out of 4,481, or 96%). Our analyses focus on these firms because changes in lending standards at the main bank have the most profound consequences for such firms. Furthermore, we remove firms whose main bank is not based in the United States. Because of our focus on bank-dependent firms, we exclude firms with access to bond markets. We also exclude firms that might have been directly exposed to the crisis countries (see Section 3.4). The final sample with the necessary data contains 2,263 borrowers; 898 firms borrowed from exposed banks, and we classify them as the treatment borrowers.

4.2. Descriptive Statistics

We report descriptive borrower-level information in Table 2. In the precrisis period, the borrowers had minimal business exposure to countries outside of the United States. Foreign sales scaled by total sales for the average treatment borrower and other borrower are 0.09 and 0.07, respectively. As these figures aggregate sales from all foreign countries, the borrowers likely had even more limited exposure to the crisis countries than indicated. In terms of borrowing in the precrisis period, the borrowers of exposed banks participated in slightly more loan deals and borrowed greater total amounts. However, they depended on their main bank for borrowing as much as the other borrowers. The average *LoanReli* ratio is approximately 90%.[26] As a percentage of a firm's total assets, the mean total loan amount borrowed from the main bank during the six-year measurement period is 48% for borrowers of exposed banks and 41% for other borrowers.

Table 3 provides descriptive statistics at the firm-quarter level for the two borrower types in the precrisis and crisis periods. If the two borrower types had responded differently to macroeconomic changes, they would have displayed different changes in firm characteristics over time, which would raise concerns about the suitability of using borrowers of unexposed banks as a control group. However, inconsistent with such concerns, the changes in firm characteristics are largely similar between the two sets of borrowers. With the exception of cash holdings (which we control for in our tests), the difference-in-differences of firm characteristics between the two borrower types are insignificant.[27]

4.3. Main Results

Table 4 reports the results of estimating Equation (2) using OLS regressions that include firm-fixed effects. For the sake of brevity, we only report the estimated coefficients on the main variables of interest. We calculate the t-statistics using robust standard errors adjusted for clustering across firms in a quarter.[28]

We begin by estimating the regression using the sample of borrowers of exposed banks only. Column (1) shows that the coefficient on the interaction term $DCFO \times CFO \times Crisis$ is positive and significant (coefficient = 0.276, t-statistic = 3.79), suggesting that these borrowers increased their conservatism significantly between the precrisis and crisis periods. This result provides initial evidence in support of H1. Column (2) shows that the results in column (1) are robust after other borrowers are used as a control group to control for concurrent macroeconomic changes. The coefficient on the interaction term $DCFO \times CFO \times Crisis$ is

Table 2. Pre-Crisis Period Borrower-Level Information

	Borrowers of exposed banks (N = 898)		Other borrowers (N = 1,365)	
	Mean	Median	Mean	Median
Information on foreign business				
Foreign sales scaled by total sales[a]	0.091	0.000	0.067	0.000
Borrowing information[b]				
Total number of loan deals	2.337	2.000	1.958	1.000
Total loan amount ($ millions)	264.169	80.00	106.279	25.00
Reliance on main bank for borrowing (*LoanReli*)	0.898	1.000	0.908	1.000
Total amount borrowed from main bank/total assets	0.479	0.402	0.412	0.323

[a]This is measured using the latest annual financial statements before the crises. Firms typically report their business in a larger geographical region, such as "Europe" or "Asia" rather than providing country-level information. Foreign sales aggregate all sales outside the United States, including those in developed countries, such as Australia, Britain, Canada, France, and Germany, and those in other important economies, such as China, India, Mexico, and the Middle East.

[b]Borrowing information in the precrisis period refers to information in the six-year measurement period that ends in 1997Q2 (i.e., prior to the Asian crisis of 1997). $LoanReli_{ik}$ ranges between zero and one; a higher value suggests the reliance of borrower i on its main bank k for loans is greater. It is computed as follows:

$$LoanReli_{ik} = \frac{\textit{Total amount lent by bank k to firm i in the 6-year period}}{\textit{Total amount borrowed by firm i in the 6-year period}}$$

As discussed in Section 3.5, *LoanReli* can also be interpreted as the proportion of total loans scrutinized by the main bank.

insignificant (coefficient = 0.099, t-statistic = 1.22), suggesting no significant increase in the conservatism of other borrowers during the crisis period.[29] However, the borrowers of exposed banks show a differential increase. The coefficient on the interaction term $DCFO \times CFO \times Crisis \times ExpoMBK$ is positive and significant (coefficient = 0.178, t-statistic = 1.85).[30]

In sum, the results support H1, suggesting that following an external shock to the capital of exposed banks, borrowers responded by increasing their accounting conservatism in the midst of credit tightening and increased bank scrutiny. The increase in asymmetric timely loss recognition for borrowers of exposed banks is economically significant. Based on the results in Column (1), such an increase is about 39% of the borrowers' conservatism level in the precrisis period (=0.276/0.706).

4.4. Cross-Sectional Variation Within Borrowers of Exposed Banks

If borrowers increase their conservatism in response to a tightening of lending standards, then the increase should be greater when the lending standards are tightened to a greater extent and are accompanied by more

Table 3. Descriptive Statistics

	Borrowers of exposed banks						Other borrowers						Diff-in-diff in means
	(A) Precrisis period			(B) Crisis period			(A1) Precrisis period			(B1) Crisis period			
	N	Mean	Median	N	Mean	Median	N	Mean	Median	N	Mean	Median	(B–A) – (B1–A1)
Accruals (*ACC*)	4,629	−0.010	−0.010	4,465	−0.017	−0.012	7,031	−0.013	−0.011	6,771	−0.020	−0.013	0.000
Operating cash flow (*CFO*)	4,629	0.028	0.028	4,465	0.029	0.029	7,031	0.016	0.024	6,771	0.016	0.023	0.001
Negative CFO dummy (*DCFO*)	4,629	0.308	0.000	4,465	0.305	0.000	7,031	0.364	0.000	6,771	0.354	0.000	0.007
Log of firm size (*Size*)	4,629	5.041	5.050	4,465	5.312	5.359	7,031	4.470	4.431	6,771	4.704	4.687	0.037
Market-to-book (*MTB*)	4,629	3.035	1.963	4,465	3.120	1.986	7,031	3.152	2.109	6,771	3.142	2.012	0.095
Leverage	4,629	0.516	0.236	4,465	0.613	0.289	7,031	0.433	0.203	6,771	0.552	0.237	−0.022
Cash	4,629	0.092	0.034	4,465	0.086	0.032	7,031	0.107	0.040	6,771	0.111	0.040	−0.010**

Notes. This table presents descriptive statistics of borrowers of exposed banks and other borrowers across the precrisis and crisis periods. The table also reports the difference-in-differences in the means of the variables for the two sets of borrowers.

**Significance at the 5% level (two-tailed).

Table 4. Differential Change in Conservatism for Borrowers of Exposed Banks in the Crisis Period

	(1) Borrowers of exposed banks only		(2) Including other borrowers as a control group	
Dependent variable = accruals (ACC_{it})	Coefficient	t-statistics	Coefficient	t-statistics
$DCFO_{it}$	0.00	−0.050	0.045	5.150
CFO_{it}	−1.113	−20.710	−0.931	−15.640
$DCFO_{it} \times CFO_{it}$	0.706	5.460	0.824	4.210
$DCFO_{it} \times ExpoMBK_i$			−0.046	−3.110
$CFO_{it} \times ExpoMBK_i$			−0.182	−2.560
$DCFO_{it} \times CFO_{it} \times ExpoMBK_i$			−0.118	−0.440
$Crisis_t$	−0.003	−1.220	−0.011	−3.710
$DCFO_{it} \times Crisis_t$	0.001	0.280	−0.007	−1.630
$CFO_{it} \times Crisis_t$	−0.090	−2.740	−0.022	−0.790
$\boldsymbol{DCFO_{it} \times CFO_{it} \times Crisis_t}$	**0.276**	**3.790**	0.099	1.220
$Crisis_t \times ExpoMBK_i$			0.008	2.490
$DCFO_{it} \times Crisis_t \times ExpoMBK_i$			0.008	1.330
$CFO_{it} \times Crisis_t \times ExpoMBK_i$			−0.068	−1.870
$\boldsymbol{DCFO_{it} \times CFO_{it} \times Crisis_t \times ExpoMBK_i}$			**0.178**	**1.850**
Additional controls and interactions	Yes		Yes	
Firm-fixed effects	Yes		Yes	
Clustering	Quarter		Quarter	
R^2	0.67		0.64	
N	9,094		22,896	

Notes. This table presents results from the modified Ball and Shivakumar (2005) regression, which examines whether U.S. borrowers change their conservatism in response to a tightening of lending standards because of their banks' exposures to the emerging-market financial crises. Regression (1) tests for the change for borrowers of exposed banks only. Regression (2) compares the change for borrowers of exposed banks to that for other borrowers. Appendix B provides variable definitions. The model includes firm-fixed effects, and standard errors are clustered by quarter. In regression (2), the main effect for the indicator variable identifying borrowers of exposed banks (*ExpoMBK*) is not included because it is subsumed by firm-fixed effects. The sample includes firm-quarters for the period 1995Q3–1999Q2. Bold formatting indicates the variable of interest.

intense scrutiny of borrowers. In this section, we exploit cross-sectional variation in the treatment effect *within* borrowers of exposed banks. This test only uses treatment borrowers, mitigating concerns that the treatment and control borrowers in Table 4, Column (2), are inherently different and, hence, the difference-in-differences results may be confounded. We modify our test in Equation (2) to a regression as follows:

$$\begin{aligned} ACC_{it} = {} & \beta_i + \beta_1 DCFO_{it} + \beta_2 CFO_{it} + \beta_3 DCFO_{it} \times CFO_{it} \\ & + \beta_4 DCFO_{it} \times CondVar_i + \beta_5 CFO_{it} \times CondVar_i \\ & + \beta_6 DCFO_{it} \times CFO_{it} \times CondVar_i + \beta_7 Crisis_t \\ & + \beta_8 DCFO_{it} \times Crisis_t + \beta_9 CFO_{it} \times Crisis_t \\ & + \beta_{10} DCFO_{it} \times CFO_{it} \times Crisis_t \\ & + \beta_{11} Crisis_t \times CondVar_i \\ & + \beta_{12} DCFO_{it} \times Crisis_t \times CondVar_i \\ & + \beta_{13} CFO_{it} \times Crisis_t \times CondVar_i \\ & + \boldsymbol{\beta_{14} DCFO_{it} \times CFO_{it} \times Crisis_t \times CondVar_i} \\ & + \textstyle\sum_{k=1}^{K} \gamma_k FirmControls + \varepsilon_{it}. \end{aligned} \tag{3}$$

The difference in the treatment effect within borrowers of exposed banks is captured by two alternative conditioning variables (*CondVar*). We discuss these variables in more detail in the following. All other variables are defined as before.

In this test, treatment borrowers subject to less stringent bank scrutiny during the crisis period are used as a benchmark, and we assess whether other treatment borrowers increase their conservatism more. The first conditioning variable (*MoreExpoMBK*) exploits the variation in banks' exposure to the crisis events. Banks with *more* exposure are expected to tighten their lending standards to a greater extent, so we examine whether their borrowers increase their conservatism more. Following Lo (2014), we use the exposure information included in the 1997 Country Exposure Reports of the Federal Financial Institutions Examination Council and other studies (e.g., Houpt 1999) to identify more exposed banks, which include Bank of America, Chase Manhattan, Citicorp, First Chicago NBD, and J.P. Morgan. If the borrowers' main bank is one of these banks, then *MoreExpoMBK* is set to one and zero otherwise. Column (1) of Table 5 shows that borrowers of more exposed banks increase their conservatism to a greater extent; the coefficient on the interaction term $DCFO \times CFO \times Crisis \times MoreExpoMBK$ is positive and significant (coefficient = 0.293, t-statistic = 1.72).

Table 5. Cross-Sectional Variation Within Borrowers of Exposed Banks

	Variation in bank exposure		Variation in borrower credit risk	
	(1) *CondVar* = *MoreExpoMBK*		(2) *CondVar* = *CreditRisk_High*	
Dependent variable = accruals (ACC_{it})	Coefficient	*t*-statistics	Coefficient	*t*-statistics
$DCFO_{it}$	−0.006	−0.680	−0.009	−0.790
CFO_{it}	−1.150	−19.600	−1.099	−14.010
$DCFO_{it} \times CFO_{it}$	0.754	5.920	0.286	1.350
$DCFO_{it} \times CondVar_i$	0.018	2.630	−0.001	−0.130
$CFO_{it} \times CondVar_i$	0.022	1.010	0.110	1.780
$DCFO_{it} \times CFO_{it} \times CondVar_i$	0.110	1.230	−0.274	−3.950
$Crisis_t$	−0.011	−3.140	−0.002	−0.610
$DCFO_{it} \times Crisis_t$	0.006	1.010	0.000	0.040
$CFO_{it} \times Crisis_t$	0.003	0.070	−0.045	−0.800
$DCFO_{it} \times CFO_{it} \times Crisis_t$	0.106	1.180	0.125	1.110
$Crisis_t \times CondVar_i$	0.012	3.980	0.005	0.860
$DCFO_{it} \times Crisis_t \times CondVar_i$	−0.007	−0.790	−0.015	−1.770
$CFO_{it} \times Crisis_t \times CondVar_i$	−0.152	−5.100	−0.136	−2.100
$\boldsymbol{DCFO_{it} \times CFO_{it} \times Crisis_t \times CondVar_i}$	**0.293**	**1.720**	**0.381**	**3.020**
Additional controls and interactions	Yes		Yes	
Firm-fixed effects	Yes		Yes	
Clustering	Quarter		Quarter	
R^2	0.67		0.70	
N	9,094		4,574	

Notes. This table presents results from the modified Ball and Shivakumar (2005) regression, which examines cross-sectional variation in the increase in conservatism within borrowers of exposed banks in response to a tightening of lending standards because of their banks' exposures to the emerging-market financial crises. Appendix B provides variable definitions. The model includes firm-fixed effects, and standard errors are clustered by quarter. The conditioning variable (*CondVar*) takes one of the following two forms: (i) *MoreExpoMBK* is an indicator for borrowers of the more exposed banks. These banks include Bank of America, Chase Manhattan, Citicorp, First Chicago NBD, and J.P. Morgan. (ii) *CreditRisk_High* is an indicator variable that equals one if the credit risk score (as developed by Bharath et al. 2008) of the borrower is in the top quartile and zero otherwise. Each conditioning variable is measured based on precrisis period information. Its main effect is not included in the regression because it is subsumed by firm-fixed effects. The sample includes firm-quarters for the period 1995Q3–1999Q2. Bold formatting indicates the variable of interest.

The second conditioning variable (*CreditRisk_High*) exploits the variation in borrower credit risk measured right before the crises. After suffering capital losses, exposed banks are less risk tolerant and scrutinize riskier borrowers to a greater extent (Leary 2009, Murfin 2012). Thus, we expect riskier borrowers to increase their conservatism more in the crisis period. We use *CreditRisk_High*, an indicator variable that equals one if the credit risk score of a borrower is in the top quartile and zero otherwise, to capture borrower credit risk. Following Bharath et al. (2008), the credit risk score of a borrower is the principal-component score of various credit risk proxies such as (i) the Altman *Z*-score, (ii) the Ohlson *O*-score, and (iii) the expected default frequency measure based on Merton (1974). We find that riskier borrowers display a greater increase in conservatism during the crisis period. In Column (2) of Table 5, the coefficient on the interaction term *DCFO* × *CFO* × *Crisis* × *CreditRisk_High* is positive and significant (coefficient = 0.381, *t*-statistic = 3.02).

In sum, the increase in conservatism for borrowers of exposed banks during the crisis period documented in Table 4 is most pronounced among the borrowers most affected by the tightening of lending standards and increases in bank scrutiny.

4.5. The Cost–Benefit Trade-Off of Increasing Conservatism

Increasing conservatism during the crisis period is not costless for borrowers. Conservative accounting choices are associated with earlier covenant violations (e.g., Zhang 2008, Nikolaev 2010), which are followed by a decline in the borrower's leverage, capital expenditures, and acquisitions and an increase in CEO turnover (e.g., Chava and Roberts 2008, Roberts and Sufi 2009a, Nini et al. 2012). Accordingly, we expect that borrowers will internalize the costs and benefits of increasing conservatism, and borrowers with a higher likelihood of violating covenants will display lower increases in conservatism during the crisis period. To

investigate this conjecture, we modify Equation (3) in the following way:

$$\begin{aligned} ACC_{it} = {} & \beta_i + \beta_1 DCFO_{it} + \beta_2 CFO_{it} + \beta_3 DCFO_{it} \times CFO_{it} \\ & + \beta_4 DCFO_{it} \times CovStrictness_i \\ & + \beta_5 CFO_{it} \times CovStrictness_i \\ & + \beta_6 DCFO_{it} \times CFO_{it} \times CovStrictness_i \\ & + \beta_7 DCFO_{it} \times CreditRisk_High_i \\ & + \beta_8 CFO_{it} \times CreditRisk_High_i \\ & + \beta_9 DCFO_{it} \times CFO_{it} \times CreditRisk_High_i \\ & + \beta_{10} Crisis_t + \beta_{11} DCFO_{it} \times Crisis_t \\ & + \beta_{12} CFO_{it} \times Crisis_t \\ & + \beta_{13} DCFO_{it} \times CFO_{it} \times Crisis_t \\ & + \beta_{14} Crisis_t \times CovStrictness_i \\ & + \beta_{15} DCFO_{it} \times Crisis_t \times CovStrictness_i \\ & + \beta_{16} CFO_{it} \times Crisis_t \times CovStrictness_i \\ & + \boldsymbol{\beta_{14} DCFO_{it} \times CFO_{it} \times Crisis_t} \\ & \times \boldsymbol{CovStrictness_i} + \beta_{15} Crisis_t \\ & \times CreditRisk_High_i + \beta_{16} DCFO_{it} \times Crisis_t \\ & \times CreditRisk_High_i + \beta_{17} CFO_{it} \times Crisis_t \\ & \times CreditRisk_High_i + \beta_{18} DCFO_{it} \times CFO_{it} \\ & \times Crisis_t \times CreditRisk_High_i \\ & + \sum_{k=1}^{K} \gamma_k FirmControls + \varepsilon_{it}, \end{aligned} \tag{4}$$

where *CovStrictness* is a measure of financial covenant strictness developed by Demerjian and Owens (2016). *CovStrictness* uses the information about covenant slack, volatility of underlying parameters, and their covariance across financial covenants in a debt contract to determine covenant strictness. We use *CovStrictness* as an estimate of the likelihood that a borrower will breach debt covenants and measure it at the beginning of the crisis period. A higher value of *CovStrictness* indicates that a borrower is more likely to violate covenants. Because uncertainty about borrowers' future prospects and creditworthiness can influence the inclusion as well as intensity of covenants (e.g., Rajan and Winton 1995, Demerjian 2017), we control for borrower credit risk using *CreditRisk_High* and its interaction terms. In addition, Equation (4) includes all other firm-specific time-varying controls and their interactions as Equation (3).

Consistent with our previous results, Table 6 shows that the treatment borrowers with higher credit risk have a greater increase in conservatism during the crisis period (coefficient on $DCFO \times CFO \times Crisis \times CreditRisk_High$ = 0.286, t-statistic = 2.13). Importantly, after controlling for credit risk, the treatment borrowers that are more likely to violate covenants display relatively lower increases in conservatism. The coefficient on the interaction term $DCFO \times CFO \times Crisis \times CovStrictness$ is negative and significant (coefficient = −0.397, t-statistic = −2.01), consistent with the argument that borrowers weigh their incentives to increase conservatism during the crisis period against potential costs associated with violating covenants.

5. Evidence from Economy-Wide Changes in Bank Lending Standards

In this section, we supplement our emerging-crisis results by investigating the effect of economy-wide changes in bank lending standards on borrower conservatism from 1990Q2 to 2014Q1.

5.1. Change in Borrower Conservatism in Response to Changes in Bank Lending Standards

We identify economy-wide changes in lending standards using the Federal Reserve's SLOOS. Every quarter, the SLOOS surveys up to 80 U.S. banks that are typically the largest in each of the 12 Federal Reserve districts. The survey asks about the changes in the overall standards and specific terms of each bank's lending. Appendix C includes examples of the relevant questions from the survey and the response options available to the bank's loan officers.

The SLOOS "provides valuable insights on credit market and banking developments" and "is critical to the Federal Reserve's monitoring of bank lending practices."[31] Many studies have used the SLOOS to identify changes in economy-wide lending standards over time (e.g., Lown et al. 2000, Lown and Morgan 2006, Leary 2009, Balasubramanyan et al. 2014). Following the approach used by regulators and in prior studies, we use the fraction of banks tightening their standards for commercial and industrial loans minus the fraction of banks loosening standards to measure economy-wide changes in lending standards. *LendTightness* captures this net fraction and ranges from −1 to +1. A higher value of *LendTightness* indicates a larger number of banks tightening their lending standards and, hence, greater tightening in the overall economy. Empirically, *LendTightness* is highly correlated with the tightening of specific loan terms reported in the SLOOS, for example, amount and covenants (see Appendix C). This survey evidence is consistent with the loan-level evidence in Section 6.

We modify Equation (1) and estimate the following regression:

$$\begin{aligned} ACC_{it} = {} & \beta_i + \beta_1 DCFO_{it} + \beta_2 CFO_{it} + \beta_3 DCFO_{it} \times CFO_{it} \\ & + \beta_4 LendTightness_{t-1} \\ & + \beta_5 DCFO_{it} \times LendTightness_{t-1} \\ & + \beta_6 CFO_{it} \times LendTightness_{t-1} \\ & + \boldsymbol{\beta_7 DCFO_{it} \times CFO_{it} \times LendTightness_{t-1}} \\ & + \beta_8 \Delta GDP_t + \beta_9 DCFO_{it} \times \Delta GDP_t \\ & + \beta_{10} CFO_{it} \times \Delta GDP_t \\ & + \beta_{11} DCFO_{it} \times CFO_{it} \times \Delta GDP_t \\ & + \sum_{k=1}^{K} \gamma_k FirmControls + \varepsilon_{it}, \end{aligned} \tag{5}$$

where *LendTightness* is defined as before to capture lending standards in place at the beginning of the quarter, *LendTightness* is measured as of t–1. ΔGDP

Table 6. Impact of Covenant Strictness on the Change in Conservatism of Borrowers of Exposed Banks

	Variation in borrower covenant strictness	
Dependent variable = accruals (ACC_{it})	Coefficient	t-statistics
$DCFO_{it}$	−0.023	−1.330
CFO_{it}	−1.165	−6.840
$DCFO_{it} \times CFO_{it}$	0.555	1.970
$DCFO_{it} \times CovStrictness_i$	0.013	1.340
$CFO_{it} \times CovStrictness_i$	−0.027	−0.350
$DCFO_{it} \times CFO_{it} \times CovStrictness_i$	−0.001	0.000
$DCFO_{it} \times CreditRisk_High_i$	0.004	0.410
$CFO_{it} \times CreditRisk_High_i$	0.065	0.950
$DCFO_{it} \times CFO_{it} \times CreditRisk_High_i$	−0.092	−0.500
$Crisis_t$	−0.001	−0.270
$DCFO_{it} \times Crisis_t$	0.002	0.210
$CFO_{it} \times Crisis_t$	−0.085	−2.270
$DCFO_{it} \times CFO_{it} \times Crisis_t$	0.203	1.240
$Crisis_t \times CovStrictness_i$	−0.003	−0.280
$DCFO_{it} \times Crisis_t \times CovStrictness_i$	−0.018	−1.200
$CFO_{it} \times Crisis_t \times CovStrictness_i$	0.179	1.710
$\boldsymbol{DCFO_{it} \times CFO_{it} \times Crisis_t \times CovStrictness_i}$	**−0.397**	**−2.010**
$Crisis_t \times CreditRisk_High_i$	0.007	0.980
$DCFO_{it} \times Crisis_t \times CreditRisk_High_i$	−0.012	−1.060
$CFO_{it} \times Crisis_t \times CreditRisk_High_i$	−0.167	−2.180
$DCFO_{it} \times CFO_{it} \times Crisis_t \times CreditRisk_High_i$	0.286	2.130
Additional controls and interactions	Yes	
Firm-fixed effects	Yes	
Clustering	Quarter	
R^2	0.74	
N	2,398	

Notes. This table presents results from the modified Ball and Shivakumar (2005) regression, which examines cross-sectional variation in the increase in conservatism within borrowers of exposed banks in response to a tightening of lending standards because of their banks' exposures to the emerging-market financial crises. We condition the increase in conservatism on the borrowers' covenant strictness. Covenant strictness (*CovStrictness*) information is obtained from Demerjian and Owens (2016). Appendix B provides variable definitions. The model includes firm-fixed effects, and standard errors are clustered by quarter. *CovStrictness* is measured based on precrisis period information. Its main effect is not included in the regression because it is subsumed by firm-fixed effects. The sample includes firm-quarters for the period 1995Q3–1999Q2. Bold formatting indicates the variable of interest.

captures the quarterly change in U.S. gross domestic product. As we control for firm-fixed effects, Equation (5) focuses on how a firm's conservatism varies intertemporally with *LendTightness* after controlling for concurrent macroeconomic changes. As we expect firms to report more conservatively after economy-wide lending standards are tightened, we predict that the coefficient on the interaction term $DCFO \times CFO \times LendTightness$ (β_7) is positive.

Our sample comprises nonfinancial bank-dependent U.S. firms. The sample period starts in 1990Q2, when required information from the SLOOS became available.[32] Table 7 presents descriptive information. The mean of *LendTightness* is 0.068, suggesting that lending standards, on average, were tightened during our sample period. Table 8 reports the results of estimating Equation (5). For the sake of brevity, we report only the coefficients on the main variables of interest. Consistent with H1, the coefficient on the interaction term $DCFO \times CFO \times LendTightness$ is positive and significant (coefficient = 0.295, t-statistic = 4.59). The results are economically relevant. A one-standard-deviation increase in *LendTightness* is associated with a 9.8% (= (0.295 × 0.239)/0.721) increase in conservatism.[33]

5.2. Tightening vs. Loosening of Lending Standards and Borrower Conservatism

Next, we investigate the effects of the tightening versus loosening of lending standards on borrowers' conservatism. We argue that bank-dependent firms have incentives to increase their conservatism during tightening periods as banks intensify their scrutiny of borrowers. However, it is unclear whether the incentives to change conservatism are symmetric in the sense that borrowers decrease their conservatism during loosening periods. As discussed previously, borrowers may not easily decrease their conservatism because of concerns such as a loss of reputation in debt markets and pressure from auditors.

Table 7. Change in Bank-Dependent Firms' Conservatism in Response to Changes in Economy-wide Bank Lending Standards: Descriptive Statistics

	N	Mean	Standard deviation	P25	Median	P75
Variables measured at the firm-quarter level						
Accruals (*ACC*)	212,891	−0.025	0.105	−0.059	−0.017	0.018
Cash flow from operations (*CFO*)	212,891	0.003	0.142	−0.030	0.022	0.071
Negative CFO dummy (*DCFO*)	212,891	0.369	0.483	0.000	0.000	1.000
Log of firm size (*Size*)	212,891	4.712	1.702	3.465	4.651	5.919
Market-to-book (*MTB*)	212,891	3.597	6.995	1.173	1.958	3.516
Leverage	212,891	0.519	1.228	0.034	0.160	0.492
Cash	212,891	0.174	0.218	0.020	0.079	0.245
Variables measured at the calendar-quarter level						
LendTightness	96	0.068	0.239	−0.088	−0.005	0.196
ΔGDP	96	0.025	0.019	0.017	0.027	0.041

Notes. This table presents descriptive statistics for the analyses exploiting economy-wide changes in bank lending standards. The sample includes firm-quarters for the period 1990Q2–2014Q1. Appendix B provides variable definitions.

We investigate the effects of the tightening versus loosening of economy-wide lending standards by allowing *LendTightness* to have a different coefficient when it reflects either condition. We decompose *LendTightness* into two variables based on its sign. If *LendTightness* is greater than zero (e.g., 1990Q3, see Figure 1), indicating that tightening takes place in the quarter, we set *LendTightness_Pos* as equal to that positive value for the quarter and zero otherwise. When *LendTightness* takes a negative value (e.g., 2004Q2), indicating that economy-wide loosening takes place in the quarter, we set *LendTightness_Neg* as equal to that negative value for the quarter and zero otherwise. Using these two variables, we modify Equation (5) as follows:

$$\begin{aligned} ACC_{it} = {} & \beta_i + \beta_1 DCFO_{it} + \beta_2 CFO_{it} + \beta_3 DCFO_{it} \times CFO_{it} \\ & + \beta_4 LendTightness_Pos_{t-1} \\ & + \beta_5 DCFO_{it} \times LendTightness_Pos_{t-1} \\ & + \beta_6 CFO_{it} \times LendTightness_Pos_{t-1} \\ & + \boldsymbol{\beta_7 DCFO_{it} \times CFO_{it} \times LendTightness_Pos_{t-1}} \\ & + \beta_8 LendTightness_Neg_{t-1} \\ & + \beta_9 DCFO_{it} \times LendTightness_Neg_{t-1} \\ & + \beta_{10} CFO_{it} \times LendTightness_Neg_{t-1} \\ & + \boldsymbol{\beta_{11} DCFO_{it} \times CFO_{it} \times LendTightness_Neg_{t-1}} \\ & + \beta_{12} \Delta GDP_t + \beta_{13} DCFO_{it} \times \Delta GDP_t \\ & + \beta_{14} CFO_{it} \times \Delta GDP_t \\ & + \beta_{15} DCFO_{it} \times CFO_{it} \times \Delta GDP_t \\ & + \textstyle\sum_{k=1}^{K} \gamma_k FirmControls + \varepsilon_{it}, \end{aligned} \tag{6}$$

where all the variables are defined as before. The coefficient on *DCFO* × *CFO* captures conservatism when *LendTightness* is equal to zero, that is, there is no change in economy-wide lending standards. We expect the interaction term *DCFO* × *CFO* × *LendTightness_Pos* (β_7) to have a positive coefficient, consistent with our previous results that borrowers increase conservatism after lending standards are tightened. If the effect of loosening is symmetrical such that borrowers decrease their conservatism after lending standards are loosened, we expect a positive coefficient on the interaction term *DCFO* × *CFO* × *LendTightness_Neg* (β_{11}).

Table 9 reports the results of estimating Equation (6). Similar to the inferences drawn from the previous tables, we find that conservatism increases with the tightening of lending standards. The coefficient on the interaction term *DCFO* × *CFO* × *LendTightness_Pos* is

Table 8. Change in Bank-Dependent Firms' Conservatism in Response to Changes in Economy-wide Bank Lending Standards: Multivariate Evidence

Dependent variable = accruals (ACC_{it})	Coefficient	*t*-statistics
$DCFO_{it}$	0.014	4.700
CFO_{it}	−0.970	−39.630
$DCFO_{it} \times CFO_{it}$	0.721	18.790
$LendTightness_{t-1}$	0.007	2.180
$DCFO_{it} \times LendTightness_{t-1}$	−0.013	−2.000
$CFO_{it} \times LendTightness_{t-1}$	−0.056	−1.770
$\boldsymbol{DCFO_{it} \times CFO_{it} \times LendTightness_{t-1}}$	**0.295**	**4.590**
ΔGDP_t	0.176	3.560
$DCFO_{it} \times \Delta GDP_t$	−0.112	−0.240
$CFO_{it} \times \Delta GDP_t$	0.065	1.050
$DCFO_{it} \times CFO_{it} \times \Delta GDP_t$	1.799	2.610
Additional controls and interactions	Yes	
Firm-fixed effects	Yes	
Clustering	Quarter	
R^2	0.44	
N	212,891	

Notes. This table presents results from the modified Ball and Shivakumar (2005) regression which examines whether bank-dependent firms change their conservatism in response to changes in economy-wide bank lending standards in the United States after controlling for concurrent macroeconomic changes. Appendix B provides variable definitions. The model includes firm-fixed effects, and standard errors are clustered by quarter. The sample includes firm-quarters for the period 1990Q2–2014Q1. Bold formatting indicates the variable of interest.

Table 9. Change in Bank-Dependent Firms' Conservatism in Response to the Tightening vs. Loosening of Bank Lending Standards

Dependent variable = accruals (ACC_{it})	Coefficient	t-statistics
$DCFO_{it}$	0.016	3.930
CFO_{it}	−0.975	−35.020
$DCFO_{it} \times CFO_{it}$	0.701	15.470
$LendTightness_Pos_{t-1}$	0.007	1.290
$DCFO_{it} \times LendTightness_Pos_{t-1}$	−0.017	−1.510
$CFO_{it} \times LendTightness_Pos_{t-1}$	−0.043	−0.870
$\boldsymbol{DCFO_{it} \times CFO_{it} \times LendTightness_Pos_{t-1}}$	**0.336**	**3.390**
$LendTightness_Neg_{t-1}$	0.008	1.010
$DCFO_{it} \times LendTightness_Neg_{t-1}$	−0.004	−0.290
$CFO_{it} \times LendTightness_Neg_{t-1}$	−0.096	−1.060
$\boldsymbol{DCFO_{it} \times CFO_{it} \times LendTightness_Neg_{t-1}}$	**0.163**	**1.050**
ΔGDP_t	0.175	3.350
$DCFO_{it} \times \Delta GDP_t$	−0.045	−0.090
$CFO_{it} \times \Delta GDP_t$	0.043	0.570
$DCFO_{it} \times CFO_{it} \times \Delta GDP_t$	2.008	2.500
Additional controls and interactions	Yes	
Firm-fixed effects	Yes	
Clustering	Quarter	
R^2	0.44	
N	212,891	

Notes. This table presents results from the modified Ball and Shivakumar (2005) regression, which examines whether bank-dependent firms change their conservatism in response to the tightening versus loosening of economy-wide bank lending standards in the United States. Appendix B provides variable definitions. The model includes firm-fixed effects, and standard errors are clustered by quarter. The sample includes firm-quarters for the period 1990Q2–2014Q1. Bold formatting indicates the variable of interest.

positive and significant (coefficient = 0.336, t-statistic = 3.39). However, there is no evidence that borrowers immediately decrease their conservatism following the loosening of lending standards. The coefficient on the interaction term $DCFO \times CFO \times LendTightness_Neg$ is positive but insignificant (coefficient = 0.163, t-statistic = 1.05).

In sum, the evidence in this section is consistent with that from the emerging-market financial crises tests. Using economy-wide changes in lending standards and after controlling for concurrent macroeconomic changes, we document that bank-dependent firms increase conservatism in response to the tightening of lending standards. However, loosening is not associated with a parallel decrease.

6. Robustness Tests and Additional Analyses at the Loan level

6.1. Comparing Borrowers Obtaining Additional Credit to Other Borrowers

In our emerging-market crises tests, we do not distinguish between existing borrowers of banks and those borrowers seeking additional credit during the crisis period. We expect that the deterrent effect of bank inspection applies to both types of borrowers. Existing borrowers must demonstrate debt compliance, so they should anticipate bank scrutiny, especially during tightening of lending standards. For borrowers seeking additional credit, they should expect that during tightening periods, banks scrutinize borrowers more closely when considering whether to extend additional loans. To verify that our arguments apply to both types of borrowers, we modify Equation (3) in the following way:

$$\begin{aligned} ACC_{it} = {} & \beta_i + \beta_1 DCFO_{it} + \beta_2 CFO_{it} + \beta_3 DCFO_{it} \times CFO_{it} \\ & + \beta_4 DCFO_{it} \times AddLoans_i \\ & + \beta_5 CFO_{it} \times AddLoans_i \\ & + \beta_6 DCFO_{it} \times CFO_{it} \times AddLoans_i + \beta_7 Crisis_t \\ & + \beta_8 DCFO_{it} \times Crisis_t + \beta_9 CFO_{it} \times Crisis_t \\ & + \beta_{10} DCFO_{it} \times CFO_{it} \times Crisis_t \\ & + \beta_{11} Crisis_t \times AddLoans_i \\ & + \beta_{12} DCFO_{it} \times Crisis_t \times AddLoans_i \\ & + \beta_{13} CFO_{it} \times Crisis_t \times AddLoans_i \\ & + \boldsymbol{\beta_{14} DCFO_{it} \times CFO_{it} \times Crisis_t \times AddLoans_i} \\ & + \textstyle\sum_{k=1}^{K} \gamma_k FirmControls + \varepsilon_{it}. \end{aligned} \tag{7}$$

In Equation (7), we identify borrowers obtaining additional credit during the crisis period using an indicator variable *AddLoans*, which equals one for borrowers that took additional loans during the crisis period and zero otherwise. For this analysis, we restrict our sample to borrowers of exposed banks. Existing borrowers not seeking additional credit are the reference group. Table 10 shows that the coefficient on the interaction term $DCFO \times CFO \times Crisis$ is positive and significant (coefficient = 0.231, t-statistic = 1.80), suggesting an increase in conservatism during the crisis period for existing borrowers. For borrowers seeking additional credit, the coefficient on $DCFO \times CFO \times Crisis \times AddLoans$ is positive but insignificant (coefficient = 0.103, t-statistic = 0.80), consistent with the idea that these borrowers also increase their conservatism during the crisis period but their increase is no different from that of existing borrowers.

In untabulated tests, we further examine the group of borrowers seeking additional credit. We compare the change in conservatism for borrowers that changed their main bank during the crisis to other borrowers within this group. We find that although both sets of borrowers display increases in conservatism during the crisis period, the increase for borrowers that switched their main banks is significantly greater, consistent with these borrowers anticipating intense scrutiny from new, uninformed banks.

6.2. Alternative Conservatism Measures, Additional Controls, and Alternative Samples

Untabulated tests confirm that our results from the emerging-market crises tests are robust to the following changes in research choices. First, as an alternate to

Table 10. Differential Change in Conservatism of Borrowers Borrowing Additional Amounts in the Crisis Period

	Variation in borrowing during crisis period	
Dependent variable = accruals (ACC_{it})	Coefficient	t-statistics
$DCFO_{it}$	0.000	0.020
CFO_{it}	−1.139	−21.880
$DCFO_{it} \times CFO_{it}$	0.811	5.820
$DCFO_{it} \times AddLoans_i$	−0.008	−1.460
$CFO_{it} \times AddLoans_i$	0.032	0.930
$DCFO_{it} \times CFO_{it} \times AddLoans_i$	−0.349	−5.720
$Crisis_t$	−0.007	−1.690
$DCFO_{it} \times Crisis_t$	0.007	1.480
$CFO_{it} \times Crisis_t$	−0.018	−0.350
$DCFO_{it} \times CFO_{it} \times Crisis_t$	0.231	1.800
$Crisis_t \times AddLoans_i$	0.007	1.540
$DCFO_{it} \times Crisis_t \times AddLoans_i$	−0.008	−0.920
$CFO_{it} \times Crisis_t \times AddLoans_i$	−0.119	−2.080
$\boldsymbol{DCFO_{it} \times CFO_{it} \times Crisis_t \times AddLoans_i}$	**0.103**	**0.800**
Additional controls and interactions	Yes	
Firm-fixed effects	Yes	
Clustering	Quarter	
R^2	0.67	
N	9,094	

Notes. This table presents results from the modified Ball and Shivakumar (2005) regression, which examines whether U.S. borrowers change their conservatism in response to a tightening of lending standards because of their banks' exposures to the emerging-market financial crises. The regression studies the change for borrowers of exposed banks only and compares the change for borrowers that borrowed additional amounts during the crisis period to that for other borrowers. *AddLoans* is an indicator variable that equals one for borrowers obtaining new loans during the crisis period and zero otherwise. The model includes firm-fixed effects, and standard errors are clustered by quarter. The main effect for the indicator variable identifying borrowers that borrowed additional amounts (*AddLoans*) is not included because it is subsumed by firm-fixed effects. The sample includes firm-quarters for the period 1995Q3–1999Q2. Bold formatting indicates the variable of interest.

Equation (2), we apply the methodology used by Basu (1997) and estimate the following regression:

$$\begin{aligned} X_{it} = {} & \beta_i + \beta_1 NEG_{it} + \beta_2 RET_{it} + \beta_3 NEG_{it} \times RET_{it} \\ & + \beta_4 NEG_{it} \times ExpoMBK_i + \beta_5 RET_{it} \times ExpoMBK_i \\ & + \beta_6 NEG_{it} \times RET_{it} \times ExpoMBK_i + \beta_7 Crisis_t \\ & + \beta_8 NEG_{it} \times Crisis_t + \beta_9 RET_{it} \times Crisis_t \\ & + \beta_{10} Neg_{it} \times RET_{it} \times Crisis_t \\ & + \beta_{11} Crisis_t \times ExpoMBK_i \\ & + \beta_{12} NEG_{it} \times Crisis_t \times ExpoMBK_i \\ & + \beta_{13} RET_{it} \times Crisis_t \times ExpoMBK_i \\ & + \boldsymbol{\beta_{14} Neg_{it} \times RET_{it} \times Crisis_t \times ExpoMBK_i} \\ & + \textstyle\sum_{k=1}^{K} \gamma_k FirmControls + \varepsilon_{it}, \end{aligned} \tag{8}$$

where X is the net income for firm i in quarter t scaled by lagged market value of equity, RET is the quarterly cumulative stock returns, and NEG is an indicator variable equal to one if RET is negative and zero otherwise. Other variables are as defined before. This regression is analogous to the Ball and Shivakumar regression in our main test. The main distinction is that it uses RET to capture economic gains and losses that are recognized in earnings, and the coefficient of interest is the one on the interaction term $NEG \times RET \times Crisis \times ExpoMBK$ (β_{14}).

Second, we use Khan and Watts's (2009) *CScore* as an alternative conservatism measure. We estimate a firm-quarter specific *CScore* and then estimate the following firm-fixed effects model as an alternate to Equation (2):

$$\begin{aligned} CScore_{it} = {} & \varphi_i + \varphi_1 Crisis_t + \boldsymbol{\varphi_2 Crisis_t \times ExpoMBK_i} \\ & + \textstyle\sum_{l=1}^{L} \omega_l FirmControls + \varepsilon_{it}. \end{aligned} \tag{9}$$

Firm-specific time-varying controls include stock return volatility, investment cycle, firm age, bid–ask spread, and litigation risk (Khan and Watts 2009, Ettredge et al. 2012). The coefficient of interest is the one on the interaction term $Crisis \times ExpoMBK$ (φ_2), which captures the differential change in conservatism for the treatment borrowers in the crisis period.

Third, we control for potentially confounding sticky costs. Banker et al. (2016) show that an asymmetric response of costs to sales increases versus decreases can be a correlated omitted variable in models examining the asymmetric timeliness of recognizing losses versus gains. Following their suggestion, we adjust for a piecewise linear effect of sales changes in both our Basu tests and our Ball and Shivakumar tests.

Fourth, we control for nondiscretionary accounting conservatism. Accounting standards mandating asset impairments often require assets to be written down when their fair values fall below their book values. The lagged market-to-book ratio (MTB_{t-1}) proxies for the slack that firms have developed as of the beginning of the period to absorb bad news without having to write down their assets. Following Lawrence et al. (2013) and other studies (e.g., Badia et al. 2017), we replace MTB_t and its interactions with MTB_{t-1} and its corresponding interaction terms to control for nondiscretionary conservatism.[34]

Fifth, we exclude borrowers that issued equity. Our sample already excludes borrowers with access to bond markets. However, the sample borrowers can still issue equity to ease credit constraints during tightening periods, which can affect their reporting incentives (Lo 2014). To ensure that our results are attributed to borrowers increasing conservatism in response to greater bank scrutiny rather than to their desire to access public financing, we repeat our tests after excluding borrowers with equity issuance.[35]

Sixth, we examine two alternative samples. First, we construct a constant sample, requiring that borrowers

are present throughout the precrisis and the crisis period. This additional data requirement reduces our sample to 9,440 firm-quarter observations with 3,824 observations corresponding to borrowers of exposed banks. Second, we study a matched sample. For each borrower of exposed banks, we find a control borrower from the other borrowers. We require that (i) the two borrowers are in the same industry and (ii) the control borrower is within 5% of the treatment borrower in terms of *Size*. If we find multiple matches, we pick the firm with the closest *MTB* to the treatment borrower as the control. Industry classification is based on Fama and French 48 industries classification, and we measure *Size* and *MTB* using their average values over the precrisis period. The test includes 349 matched pairs with 7,262 firm-quarter observations. In both cases, despite the smaller sample sizes, our inferences remain robust.

6.3. Nonbank Dependent Firms and Subperiods

In this section, we conduct two untabulated additional analyses. First, our main analyses are restricted to bank-dependent borrowers who are not rated by a credit rating agency. We expect these borrowers who have limited funding alternatives to bank credit to be most responsive to changes in lending standards. As an additional analysis, we examine the response of rated firms to the change in bank lending standards. Relative to bank-dependent borrowers, rated firms can access alternate credit via public debt markets more easily and at a lower cost. Hence, they might have lower incentives to change their reporting conservatism in response to change in lending standards. Consistent with this expectation, we find no evidence suggesting that rated borrowers display an increase in conservatism during the emerging-market crises period. In our tests utilizing economy-wide changes in lending standards, however, we find some limited evidence of rated firms also increasing conservatism when lending standards are tightened. The coefficient of interest is only marginally significant.

Finally, we investigate the pervasiveness of the change in lending standards on borrower reporting incentives by estimating our economy-wide tests in subperiods. We partition our sample period into two subperiods: (i) 1990Q2 to 2002Q4 and (ii) 2003Q1 to 2014Q1. The partition splits our sample period into roughly two equal subperiods and allows for significant variation in lending standards in each subperiod: the first subperiod covers the savings and loan crisis in the early 1990s, the emerging-market financial crises, and the dotcom bubble and recession in early 2000s whereas the second subperiod includes the subprime banking crisis (see Figure 1). Like in our main tests, in both subperiods, we find that bank-dependent borrowers display increases in conservatism following the tightening of lending standards.

6.4. Loan-Level Evidence

We argue that borrowers increase conservatism in response to the tightening of lending standards because of the deterrent effect of bank inspection. To strengthen our inferences, we perform an additional test investigating whether banks generally increase their tendency to inspect during tightening periods. Although we cannot directly measure how the intensity of bank inspection varies over time, we can use the contractual features of loans negotiated in different periods to infer banks' changing tendency to inspect. If banks tend to inspect borrowers more intensively during tightening periods, we expect that this tendency is reflected in their greater use of contractual terms that necessitate inspection.

Rajan and Winton (1995, p. 1114) highlight that the use of covenants requires banks to inspect. For example, they note that covenants would not be an effective contractual device "unless [the bank] acquires enough information to show that the covenants have been violated." Assuming banks' tendency to inspect correlates with their use of covenants, we assess whether the use of covenants is greater during tightening periods after controlling for borrower fundamentals and loan characteristics. In untabulated tests, we find that banks include more covenants in loan contracts negotiated in tightening periods. This loan-level result confirms the findings from media and regulatory reports of lending practices: "For banks that tightened commercial standards, reasons noted were changing risk appetite along with increased use of covenants" (OCC 2014, p. 4). We interpret the greater use of covenants as reflecting banks' increased tendency to inspect borrowers in tightening periods. Anticipating such an increase in bank scrutiny, borrowers in a tightening cycle have incentives to report more conservatively.

Finally, we examine whether banks reward conservative borrowers in the form of favorable loan amounts and loan costs to a greater extent during tightening periods. In this test, we measure conservatism using the C-score. Consistent with prior studies that examine the contracting benefits of conservatism (Zhang 2008, Callen et al. 2016), untabulated results show that more conservative firms are able to borrow larger amounts and at a lower initial cost during tightening periods. Hence, anticipating increased bank inspection, borrowers in a tightening cycle have incentives to adjust their reporting.

In designing loan contracts, banks have an array of contractual terms at their disposal, and they are determined simultaneously. Our tests include controls

for other loan terms (e.g., when examining the number of covenants in a loan contract, we control for loan amount, loan spread, loan maturity, the use of performance pricing and collateral requirement, etc.). To further assess the robustness of our results, we apply seemingly unrelated regressions to account for simultaneous determination of loan terms. Although our inferences remain unchanged in these additional tests, we caution readers that we cannot completely remove concerns arising from the simultaneous determination of loan terms.

7. Conclusion

Bank lending is characterized by cycles of loosening lending standards, followed by tightening cycles with more stringent loan terms and greater bank scrutiny of borrowers. Using (i) the emerging-market financial crises as shocks affecting the lending standards of certain U.S. banks and (ii) time-series variation in economy-wide lending standards in the United States, we document that U.S. borrowers increase their asymmetric timely loss recognition in response to a tightening of lending standards. We also find that borrowers whose banks tighten their lending standards more and more risky borrowers display relatively greater increases in conservatism. Such increases in conservatism are consistent with borrowers' expectations of increased bank scrutiny during tightening periods. Further, we find that borrowers that are more likely to violate covenants increase conservatism to a lesser extent following the tightening of lending standards. This suggests that borrowers internalize the potential costs of increasing conservatism when making reporting choices. We also find that following periods of loosening of lending standards, borrowers do not show a parallel decrease in their conservatism, indicating that other factors (e.g., reputation concerns) can deter firms from easily decreasing conservatism.

This study highlights that banks, by changing their lending standards and scrutiny efforts, can significantly affect firms' reporting. At a broader level, by illuminating an important mechanism through which banks affect firms' reporting, we provide evidence that the banking sector is an institutional feature that affects financial reporting behavior in an economy (Ball et al. 2000). The bank effects we highlight also extend the large amount of literature that examines debt contracting and conservatism (e.g., Ahmed et al. 2002, Ball and Shivakumar 2005, Beatty et al. 2008, Wittenberg-Moerman 2008, Zhang 2008, Armstrong et al. 2010). Our study is not merely an indirect test of previously tested predictions that show that modified loan terms are positively associated with borrowers' accounting conservatism (e.g., Beatty et al. 2008). Rather, we document that bank scrutiny can have an impact on borrowers' reporting incentives even in the *absence* of obtaining new loans, which comes with changes in loan terms. Therefore, we highlight that the influence of banks on borrowers' reporting incentives is more pervasive than previously documented as intertemporal changes in bank lending standards and scrutiny that are widely observed in practice can incentivize borrowers to change their reporting conservatism.

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Appendix A. List of Exposed Banks

	Exposures to	
Bank name	Asian crisis	Latin crisis
Bank Boston	YES	YES
Bank of America	YES	YES
Bank of New York	YES	YES
Bankers Trust New York	YES	YES
Chase Manhattan	YES	YES
Citicorp	YES	YES
Comerica	NO	YES
Cullen Frost Bank	NO	YES
First American Tennessee	YES	NO
First Tennessee National	YES	NO
Fleet Financial Group	YES	YES
J.P. Morgan	YES	YES
Republic New York	YES	YES
Union Bank of California	YES	YES
Wachovia	NO	YES

Notes. This table shows the sample banks that were exposed to the emerging market financial crises. We identify exposed banks based on Kho et al. (2000) who determine if a bank was affected by a crisis based on whether the bank had exposures to the corresponding country/countries according to its annual report.[36] We only include banks based in the United States in our analysis. YES (NO) indicates that the bank was (was not) exposed to a particular crisis. The Asian crisis includes exposure to crises in Korea and other East Asian countries in 1997 and the 1998 Russia crisis. The Latin crisis includes exposure to crises in Brazil and other Latin American countries in 1998.

Appendix B. Variable Definitions

Firm-quarter level tests in Tables 3–6 and Table 10		
ACC	=	Accruals scaled by average total assets. Accruals are defined as earnings reported in the cash flow statement minus cash flow from operations, which is also from the cash flow statement.
CFO	=	Cash flow from operations from the cash flow statement, scaled by average total assets
DCFO	=	1 if CFO is negative and 0 otherwise
Crisis	=	1 for the financial crisis period 1997Q3–1999Q2 and 0 otherwise
ExpoMBK	=	1 if the firm's main bank was exposed to the emerging-market financial crises and 0 otherwise. The procedures for identifying a firm's main bank and its exposure to the crisis events follow Lo (2014).
MoreExpoMBK	=	1 if the borrower's main bank is classified as more exposed and 0 otherwise. The more exposed banks include BankAmerica, Chase Manhattan, Citicorp, First Chicago NBD, and J.P. Morgan.
CreditRisk_High	=	1 if the borrower's credit risk score is in the sample top quartile and 0 otherwise. Following Bharath et al. (2008, pp. 10–11), the credit risk score of a borrower is the principal-component score of various credit risk proxies, such as the Altman *Z*-score, the Ohlson *O*-score, and the expected default frequency measure based on Merton (1974).
CovStrictness	=	The average probability of covenant violation for loans taken in the six-year period from 1991Q3 to 1997Q2. Covenant violation probability is obtained from Demerjian and Owens (2016).
AddLoans	=	1 if the borrower borrowed additional amounts during the crisis period and 0 otherwise
Size	=	Natural log of market value of equity
MTB	=	Market value of equity scaled by book value of equity
Leverage	=	Sum of debt in current liabilities and long-term debt scaled by market value of equity
Cash	=	Cash scaled by total assets. Both items are measured at the beginning of the fiscal period.
Additional variables in Tables 7–9		
LendTightness	=	The net percentage of domestic banks tightening standards for commercial and industrial loans to large and midsized borrowers over the previous quarter. *LendTightness* ranges from −1 to 1 and is calculated using information from the Federal Reserve Board's Senior Loan Officer Opinion Survey on Bank Lending Practices.
LendTightness_Pos	=	The value of *LendTightness* if it is greater than zero for the quarter and 0 otherwise
LendTightness_Neg	=	The value of *LendTightness* if it is less than zero for the quarter and 0 otherwise
ΔGDP	=	Percentage change in the current quarter's GDP relative to the same quarter one year ago
Additional variables mentioned in Section 6.2		
X	=	Net income scaled by beginning-of-quarter market value of equity
RET	=	Quarterly cumulative stock returns
NEG	=	1 if *RET* is negative and 0 otherwise
CScore	=	Firm-quarter specific *CScore* estimated quarterly based on the cross-sectional model seen here: $X_{it} = \beta_0 + \beta_1 NEG_{it} + RET_{it}(\mu_1 + \mu_2 Size_{it} + \mu_3 MTB_{it} + \mu_4 Lev_{it}) + (NEG_{it} \times RET_{it})(\lambda_1 + \lambda_2 Size_{it} + \lambda_3 MTB_{it} + \lambda_4 Lev_{it}) + (\delta_1 Size_{it} + \delta_2 MTB_{it} + \delta_3 Lev_{it} + \delta_4 NEG_{it} \times Size_{it} + \delta_5 NEG_{it} \times MTB_{it} + \delta_6 NEG_{it} \times Lev_{it}) + \varepsilon_{it}$ where *i* and *t* are firm and quarter subscripts, respectively. *CScore* is computed based on the estimated coefficients (i.e., $\widehat{\lambda_1} + \widehat{\lambda_2} Size_{it} + \widehat{\lambda_3} MTB_{it} + \widehat{\lambda_4} Lev_{it}$).
Return Volatility	=	Standard deviation of daily stock returns in the quarter
Investment Cycle	=	Depreciation expense scaled by total assets at the beginning of the quarter
Age	=	Firm age at the end of the quarter, measured as the number of years the firm has been listed by CRSP
Spread	=	Average bid–ask spread in the quarter scaled by the midpoint of the bid–ask spread in the quarter
Lit Risk	=	1 if the firm's litigation risk increases from the previous quarter and 0 otherwise. Following Ettredge et al. (2012), litigation risk is calculated using the coefficients in Table 3 from Shu (2000). Specifically, it is the inverse log of [−10.049 + 0.276(Size) + 1.153(Inventory) + 2.075(Receivables) + 1.251(ROA) − 0.088(Current ratio) + 1.501(Leverage) + 0.301(Sales growth) − 0.371(Stock return) − 2.309(Stock volatility) + 0.235(Beta) + 1.464(Stock turnover) + 1.060(Delist dummy) + 0.928(Technology dummy) + 0.463(Qualified opinion dummy)].

Appendix C. The Federal Reserve's Senior Loan Officer Opinion Survey of Bank Lending Practices

For our tests of economy-wide changes in lending standards, the data comes from the Federal Reserve's Senior Loan Officer Opinion Survey of Bank Lending Practices (SLOOS). The Federal Reserve has queried banks quarterly using the SLOOS about changes in their lending standards for the major categories of loans to households and business beginning with April 1990. Up to 80 domestic banks, usually the largest in each of the 12 Federal Reserve districts, participating in the survey are asked to report whether they have changed their lending standards during the survey quarter in the following seven categories of core loans: (i) commercial and industrial (C&I), (ii) commercial real estate, (iii) residential mortgages to purchase homes, (iv) home equity lines of credit, (v) credit cards, (vi) auto, and (vii) consumer loans other than credit cards or auto loans.

In our analysis, we focus on changes in the overall lending standards with respect to C&I loans. The specific question about this change is of the following form:

> *"Over the past three months, how have your bank's credit standards for approving applications for C&I loans or credit lines—other than those to be used to finance mergers and actions—to large and middle-market firms changed?"*

Large and middle-market firms refer to those with annual sales of $50 million or more. Banks answer this question using one of the following possible responses: (i) eased considerably, (ii) eased somewhat, (iii) remained basically unchanged, (iv) tightened somewhat, or (v) tightened considerably.

For example, the SLOOS for 2009Q2 indicates that 35.2% of respondent banks tightened the standards, 3.7% loosened, and the rest remained unchanged.[37] The net percentage of banks tightening in this case is 31.5% (= 0.352 − 0.037). We use *LendTightness* to capture this information, which ranges from −1 to +1. A higher value of *LendTightness* indicates a higher number of banks tightening lending standards and, hence, greater tightening in the overall economy.

Along with changes in the overall lending standards, the SLOOS also asks the banks about specific changes in the terms of the loans approved over the past three months. The terms of the loans surveyed include (i) maximum size, (ii) maximum maturity, (iii) spread, (iv) premium charged on riskier loans, (v) loan covenants, (vi) collaterization requirements, and (vii) use of interest rate floors. Banks answer these additional questions using the same possible responses described previously. In practice, a tightening of these terms is highly correlated with *LendTightness*. During the period over which SLOOS data about changes in loan terms is available (i.e., from 1996Q4 onward), the correlations between *LendTightness* and a tightening of each of the specific loan terms are generally over 0.90.

Endnotes

[1] In practice, lending standards refer to the terms and conditions under which banks extend or renew credit, such as requirements related to the amount of credit-related information to collect from borrowers and the terms to impose on loans extended (e.g., amount, pricing, maturity, covenants, and collateral requirements) (OCC 2014).

[2] http://www.federalreserve.gov/boarddocs/srletters/1998/sr9818.htm.

[3] Our empirical analyses focus on nonrated firms, so they are unlikely to have access to public debt markets. Also, our inferences are robust to the exclusion of borrowers that issue equity. Section 6.2 provides the details.

[4] Unlike the emerging-market crises, the subprime U.S. banking crisis was an event that directly impacted nearly all U.S. banks and U.S. borrowers. Hence, we do not use the subprime U.S. banking crisis as our main test setting.

[5] Tan (2013) shows that, on average, 6% of firm quarters in a year are associated with covenant violations.

[6] Only 11% of the sample from Erkens et al. (2014) has a commercial banker on the board.

[7] For instance, during tightening periods, although borrowers have incentives to reduce conservatism to avoid covenant violations and dealing with intransigent lenders, their opportunity to do so may be limited because of increased bank scrutiny. We examine borrowers' reporting choices when they face these conflicting incentives and constraints.

[8] We note that in their firm-year panel regressions (e.g., Table 2), Li and Lobo (2015) do not have explicit controls for contemporaneous macroeconomic changes except that they include year and industry fixed effects. These fixed effects are unlikely to provide control for time-varying macroeconomic conditions.

[9] Regulators may pressure banks with losses to limit risk taking, which also leads to banks' decreased risk tolerance.

[10] For instance, Jones Lang LaSalle Inc. allows its lenders "to visit and inspect any of the Property of the Parent or any of its Subsidiaries, to examine all of their books of account, records, reports and other papers, to make copies and extracts therefrom, and to discuss their respective affairs, finances and accounts with their respective officers, employees, and independent public accountants." Similar lender inspection rights are widely observed in loan contracts in the United States (Wight et al. 2009).

[11] For example, on-site visits allow banks to review inventory and other assets for damage, obsolescence, and other losses. Examining internal records enables banks to assess the reasonableness of receivables and provisions, etc.

[12] Examples of media coverage of lending standards include http://www.ft.com/intl/cms/s/0/d492cf34-973e-11e5-95c7-d47aa298f769.html#axzz3w1PLum4l and ww2.cfo.com/credit/2015/11/fed-finds-little-change-lending-conditions/.

[13] These studies also show that a large majority (75%) of loans are renegotiated before maturity with only a small minority (18%) of these renegotiations being triggered by default or covenant violations (Roberts and Sufi 2009b).

[14] Section 4.5 discusses additional tests that assess whether borrowers who are more likely to violate their debt covenants increase conservatism to a lesser extent.

[15] Consistent with the difficulties in switching lenders, we find that in our sample for the emerging-market crises tests only 79 borrowers of exposed banks (<10%) started borrowing from unexposed banks during the crisis period.

[16] See the report published in September 1998 (http://www.federalreserve.gov/boarddocs/snloansurvey).

[17] We use the two-year precrisis period ending on June 30, 1997, as the assessment period. For each borrower, we regress its daily stock returns on the crisis countries' daily percentage changes in foreign exchange rate. The coefficients on the changes in exchange rates capture how sensitive the borrower was to these countries' conditions.

[18] For example, to control for firm size (*Size*), we include the variable *Size* and the following interactions: (i) *DCFO* × *Size*, (ii) *CFO* × *Size*, (iii) *CFO* × *DCFO* × *Size*, (iv) *ExpoMBK* × *Size*, (v) *DCFO* × *ExpoMBK* × *Size*, (vi) *CFO* × *ExpoMBK* × *Size*, *and* (vii) *CFO* × *DCFO* × *ExpoMBK* × *Size*.

[19] Given that the Asian, Russian, and Latin American crises occurred in close proximity and many banks were exposed to multiple crises (see Appendix A), we examine the effect of all these crises together rather than individually. However, the extent to which the exposed banks were impacted by the emerging-market crises varies. Accordingly, in Section 4.4 we examine whether borrowers of banks that were relatively more exposed to the crises increased conservatism to a greater extent.

[20] Figure 1 also shows a tightening of lending standards in the United States before the recession in the early 2000s. Consistent with this observation, we find in unreported tests that both control and treatment borrowers increase their conservatism in the postcrisis period relative to the precrisis period. However, we find no evidence of a differential increase in conservatism for treatment borrowers.

[21] When aggregating prior loans from a particular bank, we take bank mergers and acquisitions into account. For example, Bank of America acquired Continental Bank in 1994. If a firm had a loan from Continental Bank in 1993 and another loan from Bank of America in 1997, then we treat the two loans as loans from the same bank.

[22] Another reason for not using the fraction of the loan funded by the lead bank is that this information is often unavailable in the DealScan database.

[23] As noted by Desai (2003, p. 87, italic added), "[t]he Asian financial crisis that originated in Thailand in the summer of 1997 caught

everyone off guard by its *unpredictability* and scale. . . . .It came as a *shock* because the affected economies had chalked up impressive private sector-led economic performance marked by high growth and low inflation rates, public sector balanced budgets and economy-wide savings."

[24] Even if borrowers borrow from another bank during the crisis period, they are likely to have outstanding loans that continue to be monitored by their previous main bank.

[25] Because of data limitations, our sample is restricted to borrowers participating in the syndicated loan market. We caution readers to be careful in generalizing our findings to firms that do not borrow in these markets.

[26] Because the main lead bank syndicates and monitors, on average, 90% of the loans for the borrowers in our sample, we focus on whether the main lead bank is exposed to the crisis or not to identify treatment borrowers. In an untabulated robustness test, we find that our inferences are unchanged if we restrict our analysis to borrowers with all loans syndicated and monitored by only one lead bank (i.e., their *LoanReli* equals one).

[27] Following prior research, our test controls for leverage. Also, we control for cash holdings. However, these firm characteristics can be affected by bank lending standards (e.g., credit-constrained firms likely increase their reliance on cash to fund their operations). Hence, controlling for these firm characteristics can partly control for our effect of interest. Consistent with this argument, untabulated analyses show that our results become stronger if we do not control for leverage and cash.

[28] The firm-fixed effects regressions control for dependence in residuals because of time-invariant firm characteristics (i.e., clustering across time within a firm). Nonetheless, our inferences remain similar if we cluster by firm.

[29] The two borrower types display similar levels of conservatism in the precrisis period. The coefficient on the interaction term $DCFO \times CFO \times ExpoMBK$ is insignificant (coefficient = −0.118, t-statistic = −0.44).

[30] In an untabulated test in Section 6.2, we repeat the regression in Column (2) using a sample matched on industry, size, and growth. Our inferences remain robust.

[31] See http://www.federalreserve.gov/BoardDocs/snloansurvey/about.htm. Moreover, Bassett and Rezende (2015) show that quarterly *changes* in lending standards reported in the SLOOS and annual *levels* of lending standards reported in supplementary surveys conducted by the Fed contain common information useful for gauging bank lending practices.

[32] Although the survey has been conducted since 1966, questions have been asked consistently only since 1990.

[33] In an untabulated test, we assess whether the increase in conservatism triggered by the tightening of lending standards varies with ΔGDP. We find that such an increase in conservatism does not vary with macroeconomic conditions.

[34] Our inferences remain the same regardless of whether we control interactively for *Lev*, *Size*, and *Cash* in these additional tests. Also, our inferences remain unchanged if we drop firms with MTB_{t-1} below one from the sample.

[35] Following Chang et al. (2009), we use cash flow statement data to identify firms issuing equity. Firms whose net equity issuances as a percentage of the total assets exceed 5% are excluded from the sample.

[36] When Kho et al. (2000) collected the data, First Chicago NBD had already been acquired by another bank, so exposure information for the bank was not available. Following Lo (2014), we treat First Chicago NBD as an exposed bank. Also following Lo's approach, we treat NationsBank, another bank not covered by Kho et al. because of mergers and acquisitions, as nonexposed. Unlike First Chicago NBD and other exposed banks, NationsBank was not among the "money center banks" in the 1997 Country Exposure Reports of the Federal Financial Institutions Examination Council. Further, its foreign activities were significantly lower than those of the exposed banks. In 1998, only 4% of NationsBank's total assets were foreign assets, and that figure for exposed banks is often greater than 30% (Houpt 1999).

[37] See http://www.federalreserve.gov/BoardDocs/snloansurvey/200908/table1.pdf.

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