ESSAYS ON LIQUIDITY SHOCKS AND FIRMS' FINANCIAL REPORTING QUALITY

by

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Abstract

Problems of endogeneity often cloud interpretation in studies on the relation between firm disclosure and external financing. This dissertation uses two different liquidity shocks as natural experiments to provide new evidence in this research area. The first essay makes use of tightened liquidity supply in the banking industry following monetary contractions for examining the impact of credible accounting information on corporate financing and investments. Theory suggests that asymmetric information will restrict banks' ability to raise additional financing to offset the liquidity losses caused by monetary contractions. The attendant liquidity shortfall will force banks to hold back their investments (i.e., lending). Using external audits to proxy for accounting credibility and a sample of 9,910 small banks for which audits are voluntary, this paper finds that audited banks enjoy greater access to outside financing than other banks. Correspondingly, the lending of audited banks is less affected by policy-induced liquidity issues. Further results indicate that audited banks disclose higher quality accounting information that gives them greater reporting credibility and reduced information problems. In summary, this paper offers new evidence that credible disclosure facilitates corporate financing and investments.

The second essay identifies negative shocks to the supply of bank loans as exogenous events that motivate firms' disclosure of management forecasts. Following loan supply distortions, borrowers have greater motives to finance through alternate capital sources. If forecasts

enhance firms' access to the public capital markets, borrowers have incentives to increase forecasts to facilitate capital raising to substitute for previously available loans. The test exploits the emerging-market financial crises in the late 1990s. These events were plausibly external to the U.S. loan markets, yet their effect was transmitted through U.S. banks' large loss exposures to the crisis areas which ultimately limited the lending of exposed banks. Accordingly, I predict and find evidence consistent with borrowers of exposed banks increasing forecasts following the crises to ease access to public financing. Further, these borrowers changed forecast characteristics in ways consistent with the use of forecasts to reduce investor uncertainty. Overall, these disclosure changes provide new evidence on how capital supply affects firms' incentives to issue forward-looking information.

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Dedication

To Grace

Chapter 1: Introduction

Whether and how a firm benefits from enhanced disclosure are central issues in accounting research. Answers to these questions provide important input to managers when they choose their disclosure policy. For regulators and researchers, empirical evidence of the benefits of increased disclosure is key to understanding managers' disclosure incentives, which in turn helps us to better understand different fundamental issues such as the forces that influence firms' information environment, and the need for disclosure regulations in capital markets.

This dissertation includes two essays relating to a direct mechanism through which disclosure can benefit a firm: the access to external financing. In their conceptual framework, both the Financial Accounting Standards Board (FASB) and the International Accounting Standards Board (IASB) state that the intended purpose of financial reporting "is to provide financial information about the reporting entity that is useful to existing and potential investors, lenders, and other creditors in making decisions about providing resources to the entity (FASB 2010, OB2)." It has often been suggested that credible disclosure can help firms reduce information asymmetry problems and attract the required financing for investments (e.g., Healy and Palepu 2001; Bushman and Smith 2001).

To test this notion, empirical accounting research often examines whether firm disclosure is associated with external financing and investments in ways that are consistent with the benefits

of disclosure (e.g., Botosan 1997; Sengupta 1998; Botosan and Plumlee 2002; Biddle and Hilary 2006; Biddle et al. 2009; and Chen et al. 2011). However, because firm operations and disclosure are typically endogenous, it is difficult to draw strong conclusions from their associations about the *causal* effect of disclosure (e.g.,Nikolaev and Van Lent 2005; Larcker and Rusticus 2010).[1] In fact, in their review of the disclosure literature, Healy and Palepu (2001, 430) conclude that "[p]otential endogeneity is the most important limitation" of the findings in studies on the capital market consequences of financial reporting.

To sidestep this problem, the first essay presented in Chapter 2 makes use of tightened liquidity supply in the banking industry following monetary contractions for examining the impact of credible disclosure on corporate financing and investments. Theory suggests that asymmetric information will restrict banks' ability to raise additional financing to offset the liquidity losses caused by monetary contractions. The attendant liquidity shortfall will force banks to hold back their investment activities (i.e., lending) (e.g., Kashyap and Stein 2000). Based on these findings, I conjecture that banks committed to credible reporting have relatively more favorable outside funding (and hence less negative lending) responses to monetary tightening. By focusing on a

[1] For example, firms choose their external financing and investment policies based on factors such as the business nature and investment opportunities. Similarly, firms decide their disclosure according to factors such as firm fundamentals, the investment opportunity set, and perceived disclosure costs (e.g., the costs of releasing proprietary information and the risk of litigation). As such, in the normal course of time, a firm's disclosure is likely a function of factors that also determine the firm's financing and investments.

homogenous sample of banks and their differential responses to exogenous liquidity shocks, the research design reduces the effect of confounding cross-sectional factors common to studies examining directly the endogenous relation between disclosure choices and firm operations. In turn, the evidence helps to establish the direct link from financial reporting credibility to firms' ability to raise external funding and to make investments.

In addition to offering the above identification benefits, banks allow researchers to assess whether findings in prior studies can be generalized to highly regulated industries. To facilitate bank supervision, regulators have long required banks to file Reports of Condition and Income (a.k.a. Call Reports). At the same time, regulators are obliged to review the adequacy of the bank's allowance for loan losses during periodic on-site examinations (Gunther and Moore 2003). Besides these regulatory efforts, many small (non-public) banks *voluntarily* engage external auditors to attest to the integrity of their financial reports. This raises the question of whether there are important capital market benefits from such discretionary actions. On the one hand, voluntarily submitting to an audit can signal small banks' commitment to credible reporting. On the other hand, the incremental benefit of audited financial statements would be limited if investors believe regulatory monitoring is effective and sufficient. Thus, it is not clear whether audited accounting information plays the same role in mitigating asymmetric information for banks as it does for industrial firms typically examined in prior studies. To shed light on this issue, I test

whether the benefits of having audited financial reports manifest themselves in small banks' responses to liquidity losses caused by monetary tightening.

Using a sample of 9,910 small non-public banks, I find that audited small banks disclose higher quality accounting information that gives them greater reporting credibility than unaudited banks. Consistent with the decreased information uncertainty for audited small banks, these banks enjoy greater access to outside financing than unaudited banks as they obtain additional funding to counteract sudden liquidity losses induced by monetary contractions. In turn, audited banks' lending ability is less restricted by liquidity issues. These results are notable because the added credibility of audited financial statements is incremental to the effect of the monitoring by bank regulators. Overall, Chapter 2 demonstrates one way banks can benefit from having greater financial disclosure credibility. At a broader level, these findings help establish the causal effect of enhanced accounting disclosure on firm financing and investment activities.

In Chapter 2 just discussed, the focus is on whether committing to enhanced disclosure mitigates constraints on external financing. In the second essay presented in Chapter 3, I switch this focus to examining whether in response to tightening in the supply of external financing, firms expand their disclosure to facilitate capital raising. By investigating such disclosure responses, Chapter 3 offers new evidence as to whether managers act as if increased disclosure enhances their ability to alleviate funding constraints. Assuming managers have rational expectations, their

reactions correspond to equilibrium outcomes. Thus, the results in Chapter 3 help us better understand the dynamic nature of the relations between firm disclosure and external financing.

More specifically, Chapter 3 identifies negative shocks to the supply of bank loans as exogenous events that motivate borrowers to increase the disclosure of voluntary management forecasts. These forecasts provide outside investors with direct input for firm valuation, and are expected to be effective in reducing information asymmetry (e.g., Coller and Yohn 1997). If forecasts can increase firms' access to alternate external funding sources, particularly the public capital markets, then constrained borrowers have incentives to adjust disclosure policies to facilitate capital raising to substitute for previously available loans.

To test this hypothesis, I exploit the emerging-market crises in the late 1990s. As explained further in Chapter 3, these events were plausibly external to the U.S. markets. Yet their effect was transmitted through some U.S. banks' large exposures to the crisis areas which ultimately limited the lending of these exposed banks back in the U.S. (Chava and Purnanandam 2011). In summary, these events created significant contractions in the loan supply to U.S. borrowers for plausibly exogenous reasons. Accordingly, I predict and find evidence consistent with borrowers of exposed banks increasing the disclosure of forecasts following the crises to ease capital raising. Further, I find significant changes in the forecast characteristics for these borrowers. For example, they tended to increase the forecast horizon in the crisis period. At the same time, they tend-

ed not to compromise forecast precision even though factors such as longer forecast horizon or increased market volatility in the crisis period would otherwise cause firms to issue less precise forecasts. These results are supportive of managers' use of forecasts to lower investor uncertainty, which is likely to be more effective if the forecasts are more timely and precise. Finally, I find that these disclosure changes are accompanied by an increase in public financing desire for the exposed bank borrowers who used public capital to replace tightened bank loans during the crisis period.

Overall, the results in Chapter 3 provide timely insights into the disclosure actions firms take to mitigate loan supply issues. At the same time, these results offer new evidence for the public capital market transaction motives for voluntary disclosure. This is important because endogeneity issues associated with firms' decision to make public securities offerings often cloud the interpretation of the evidence in existing studies (Beyer et al. 2010). Yet, as mentioned before, clear understanding of managers' disclosure incentives is crucial to understanding different fundamental issues such as the forces shaping firms' information environment, and the need for regulating corporate disclosure in capital markets.

To summarize, the results in Chapters 2 and 3 highlight the interactions between firm disclosure and external financing. The evidence offers new and relatively clear support for the notion that enhanced disclosure increases the ability of a firm to raise capital for investments,

which in turn gives rise to managers' disclosure incentives. These findings should be of interests to managers, regulators and researchers.

The remainder of the dissertation is structured as follows. Chapter 2 examines whether committing to credible disclosure mitigates constraints on external financing for banks. Chapter 3 investigates whether in response to tightening in the supply of bank loans, borrowers expand their disclosure to facilitate capital raising. Chapter 4 summarizes the findings and concludes.

Chapter 2: The external financing and investment benefits of credible accounting information: Evidence from reactions of small banks to monetary policy

2.1. Introduction

Investment policies and value creation can often be distorted by problems arising from information asymmetry between firms and outside investors (e.g., Stein 2003). Motivated by this observation, an emerging research area in accounting examines whether and through what channels financial disclosure affects firm investments (e.g., Biddle and Hilary 2006; McNichols and Stubben 2008; Beatty et al. 2010; and Francis and Martin 2010). In a recent study, Biddle et al. (2009) show that financial reporting quality is positively associated with the level of investment among liquidity constrained firms. They infer from this finding that financial disclosure allows constrained firms to attract external financing and make investments that would otherwise be forgone. However, because of endogeneity issues and the possibility of reverse causality, Biddle et al. (2009, 129) call for more refined research to "explore the casual link between financial reporting quality and investment efficiency… [and] study whether the negative relation between reporting quality and under-investment is due to firms' ability to raise debt and/or equity capital." This paper uses a novel research design to demonstrate the functioning of this financing channel. To this end, the evidence lends important support to a crucial mechanism through which reliable accounting disclosures facilitate firm investments.

The research design exploits the interaction between macroeconomic shocks and firm-level information problems in the banking industry. Specifically, I investigate whether banks committed to high quality, credible financial reporting are less affected by adverse liquidity shocks than other banks. The basic idea is as follows. After a contraction of monetary policy by the Federal Reserve (Fed), banks' ability to use insured deposits as a funding source will be directly compromised. Banks can try to restore their position by using alternate, uninsured financing such as large certificates of deposits (CDs). But access to uninsured funding will be restricted by investor uncertainty about the issuing bank's financial standing. Building on this observation, a series of economic studies document that liquidity losses due to monetary contractions can lead to suppression of bank investment (i.e., lending). This effect is more pronounced among small banks which are perhaps the most opaque banks (e.g., Kashyap and Stein 2000). Based on these findings, I conjecture that small banks committed to credible reporting have relatively more favorable outside funding (and hence less negative lending) responses to monetary tightening.

The focus on *exogenous* liquidity shocks helps to establish the direct link from financial reporting credibility to banks' ability to raise outside financing and to make investments. At the same time, because monetary policies change over time and are hard to predict, the financing

need caused by monetary tightening is unlikely to be fully anticipated by small banks.[2] Thus, this setup mitigates endogeneity concerns such as strategic accounting actions induced by well-planned funding events (i.e., reverse causality). Furthermore, studying *changes* in financing and investment following liquidity shocks helps alleviate problems due to correlated omitted variables. Any confounding factors must explain the differential responses between banks of varying reporting credibility. Finally, the focus on a homogenous sample of small banks which face the same liquidity shocks and have similar operations further mitigates spurious inferences.

In addition to offering the above identification benefits, small banks allow researchers to assess whether findings in prior studies can be generalized to highly regulated industries. Small banks (with less than $500 million in total assets) are an important group of financial companies,[3] specializing in making relationship-based loans to "informationally opaque" borrowers such as start-up firms and small businesses (Keeton 2003). Due to their specialization, small banks are subject to considerable information asymmetry, as the information they have about

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[2] The Fed routinely adjusts its policy to achieve various macroeconomic goals. According to Mishkin (2006, 327), "the Fed has incentives to hide its actions from the public and from politicians to avoid conflicts with them." Market observers generally agree that it is difficult "to draw any firm conclusions about the direction of monetary policy… unless the investor possesses inside information on the motivation of Federal Reserve actions" (Rose and Marquis 2006, 400). Thus, it is very unlikely that small banks can fully anticipate an imminent policy tightening when they make accounting-related choices that would affect their information environment.

[3] Based on information from the Reports of Condition and Income database, more than 80 percent of U.S. banks in 2008 could be classified as small banks, with their aggregate outstanding lending exceeding $650 billion.

their loans is often hard for outsiders to observe. To reduce this opacity and facilitate bank supervision, regulators have long required small banks to file Reports of Condition and Income (a.k.a. Call Reports).[4] At the same time, regulators are obliged to review the adequacy of the bank's allowance for loan losses during periodic on-site examinations (Gunther and Moore 2003). Besides these regulatory efforts, many small non-public banks *voluntarily* engage external auditors to attest to the integrity of their financial reports. This raises the question of whether there are important capital market benefits from such discretionary actions. On the one hand, voluntarily submitting to an audit can signal small banks' commitment to credible reporting. On the other hand, the incremental benefit of audited financial statements would be limited if investors believe regulatory monitoring is effective and sufficient. Thus, it is not clear whether audited accounting information plays the same role in mitigating asymmetric information for banks as it does for industrial firms that prior studies typically examine. To shed light on this issue, I test whether the benefits of having audited financial reports manifest themselves in small banks' responses to liquidity losses caused by monetary tightening.

[4] Every national bank, state member bank and insured nonmember bank is required by the Federal Financial Institutions Examination Council (FFIEC) to file a Call Report as of the close of business on the last day of each calendar quarter. Call Reports are widely used by regulators and the public in their monitoring activities. Unless otherwise instructed, banks are required to provide financial data that are prepared in accordance with GAAP.

The sample is collected from the Call Report database, consisting of 9,910 small non-public banks and a total number of 327,084 bank-quarters in the period 1988:Q1 – 2000:Q4. Consistent with credible accounting information reducing banks' financing frictions in the capital markets, results show that audited banks have greater access to uninsured liabilities (e.g., large certificates of deposits) than other banks as they obtain outside financing to counteract policy-induced liquidity outflows. Correspondingly, the lending ability of audited small banks is less restricted by liquidity issues. These findings are consistent with the functioning of the financing channel through which credible accounting information affects bank investments.

On the other hand, if audited banks have unique characteristics (apart from more credible reporting) that somehow cause their monetary policy responses to be different from those of unaudited banks, the results will be confounded by omitted correlated variables. I address this issue in four ways. First, factors that may be associated with both the audit status and bank policy responses are identified from prior studies and explicitly controlled for. These factors include bank size, organizational form, operating environments, bank risk taking, liquidity position, and composition of loans. To the extent that the list of controls is comprehensive, the differential policy responses can be attributable to credible reporting.

Second, I use both the Heckman (1979) two-stage approach and the predicted probability of an audit as an instrument to more formally address potential selection issues. The estimates

indicate that if anything selection bias works against finding the value of an audit. Such bias can arise if banks prone to liquidity losses endogenously choose to have audited financial statements as a precaution. Third, because of known limitations in applying the two-stage approaches in accounting research (e.g., Francis et al. 2010), I conduct corroborating tests that exploit time-series changes in bank audit status. For example, using each bank as its own control, I find that banks are better able to withstand liquidity losses in periods when they are audited than when they are not.

Finally, I explore the differences in the positive effect of an audit across banks of different sizes. Smaller banks which have limited internal accounting sophistication (Minnis 2010) or lower firm reputation (e.g., Blackwell et al. 1998) are more likely to benefit from auditor assurance than the larger banks. Moreover, regulatory oversight likely focuses more on larger banks, leaving auditors a particularly important monitoring role among the smaller banks. Thus, I expect and find that the observed differential monetary policy responses between audited and unaudited banks are more pronounced among the smaller bank subsample. This evidence further supports the main results showing that the added credibility of audited financial reports alleviates information asymmetry and improves investment efficiency.

Having demonstrated the working of the financing channel in investment among audited banks, I provide further evidence on an important maintained assumption about this channel: that

audited banks issue higher quality disclosure that gives them greater reporting credibility and reduced information asymmetry. Since studies on the differential reporting quality between audited and unaudited firms are very limited, and even fewer examine this issue using banks, this analysis is important for establishing the inference that the benefits for audited banks are attributable to more reliable disclosures.

To shed light on the above issue, I compare the timeliness of loan loss recognition between audited and unaudited banks. Loan loss accounting is studied because lenders and depositors tend to pay special attention to potential losses (Watts and Zimmerman 1986). Also, because loan loss accounting requires a considerable degree of judgment based on the private information managers obtained in the lending process, it is an area particularly subject to reporting bias (Healy and Whalen 1999) and one in which auditors can play an important monitoring role (e.g., U.S. General Accounting Office 1991). Consistent with audited banks providing investors with more reliable information, I find that loan loss provisions for these banks reflect relatively more timely information about current and future credit losses.

Overall, by providing new evidence that credible reporting reduces liquidity problems that could lead to firm under-investment, this paper substantiates an important way through which accounting information affects firm investments. Additionally, the results help us to better understand the value of reporting commitment in highly regulated industries. Such evidence is of par-

ticular interest because dual monitoring of regulators and auditors is in place internationally (e.g., Bank for International Settlements 2002), yet our understanding of the capital market benefits of audited financial statements in this context is very limited.

This paper also contributes to the recent studies on the value of financial statement verification. For example, Hope et al. (2011) use a World Bank survey of privately-held industrial firms from 68 countries and find that firms with financial statements reviewed by an external auditor are negatively associated with managers' perceived difficulty in accessing external financing. Kim et al. (2011) and Minnis (2011) examine the relation between audited accounting information and the cost of debt using Korean and U.S. privately-held industrial firms respectively. This paper is different from these studies in at least three aspects. First, the research contexts are different. It is not clear whether results from firms operating in different business and institutional environments will hold for U.S. banks which are under mandatory monitoring by regulators. Second, with respect to research scope, I study how credible reporting affects firms' ability to make investments through the availability to external financing. The articulation of this financing channel strengthens the inference about the casual effect of accounting disclosure on firm investments (e.g., Biddle et al 2009). Prior studies do not provide direct evidence on this important issue. Finally, my research focuses on firms from a homogenous industry within a single coun-

try and uses exogenous shocks to identify the value of audited financial statements. This research design is less subject to correlated omitted variables and endogeneity issues.

The remainder of the paper is organized as follows. Section 2.2 reviews prior research and develops the testable hypotheses. Section 2.3 presents the research design. Sample selection, descriptive statistics, and empirical evidence are all provided in Section 2.4. Section 2.5 discusses robustness tests. Section 2.6 reports the loan loss accounting test results. Section 2.7 concludes.

2.2. Background, prior research and hypothesis development

This paper builds on two lines of research: 1) studies examining the impact of financial reporting on firms' ability to finance investment projects, and 2) studies showing that monetary tightening reduces bank liquidity and lending. Below, I briefly review these studies and develop the hypotheses on the differential policy reactions between audited and unaudited small banks.

2.2.1. Accounting disclosure, external financing and firm investments

Recent studies on the link between accounting disclosure and firms' ability to finance investment projects predicate on the limited substitutability between internal and external capital in the presence of imperfect information. In the ideal world envisaged in Modigliani and Miller (1958), a firm's optimal investment level should be only driven by available opportunities. If the firm does not have enough internal funds, the (assumed) frictionless access to external capital will enable it to fully offset any internal funding deficit. However, in reality, problems due to information

asymmetry can make it more difficult for firms to use external capital than internal capital. If internal capital falls below the projected needs and external capital is unavailable (or prohibitively costly), the firm will be forced to give up otherwise positive net-present-value investment opportunities.[5] This logic suggests that accounting disclosure may play a mitigating role. If accounting information reduces adverse selection that leads investors to infer that a firm raising capital has inherently poor prospects, or if accounting information mitigates investors' concerns with moral hazard, the ability of the reporting firm to obtain the required amount of funding from outside sources will likely increase (e.g., Healy and Palepu 2001). In turn, the firm's investment will be less sensitive to (or constrained by) the supply of internally generated cash flows.

Building on the above intuition, some studies use the investment-cash flow sensitivity to measure firms' difficulty in funding investments (e.g., Biddle and Hilary 2006; Beatty et al. 2010),[6] and show that this measure is negatively correlated with accounting quality (e.g., accrual quality). This approach is subject to two main limitations. First, there are concerns about the relevance of the reported findings, as prior studies show that the investment-cash flow sensitivity is not a valid measure of financial constraints (e.g., Kaplan and Zingales 1997, 2000). Second,

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[5] The observation that informational frictions will limit firms' ability to fund investment projects using external capital has been demonstrated in numerous finance studies. Refer to Hubbard (1998) and Stein (2003) for literature reviews. Also see, for example, Houston et al. (1997) for a bank-specific study.

[6] The basic working assumption in these studies is that the sensitivity of firm investments to internally generated cash flow should be higher for firms that face a higher level of external financial constraints.

the findings are consistent with plausible alternative interpretations. In particular, weak investment-cash sensitivity can capture other benefits that quality accounting helps generate, such as reduced over-investment of free cash flows by empire-building managers (e.g., Biddle et al. 2009). Thus, it is important to have more refined studies to identify the external financing channel through which credible accounting information affects firm investments.

Existing studies have also used the relation between the levels of firm investment and disclosure quality to infer the working of the external financing channel (e.g., Biddle et al. 2009; Lu 2009). This approach raises concerns over correlated omitted variables and the possibility of reverse causality. For instance, new investment opportunities generate financing needs, which in turns can affect firms' disclosure incentives and reporting properties. Equally important, there is no direct evidence that the reported negative relation between accounting quality and under-investment is due to firms' ability to raise external financing (Biddle et al. 2009, 129).

To address the above empirical issues, this paper exploits adverse liquidity shocks induced by monetary contractions and test whether small banks committed to high quality, credible reporting are relatively less affected by these exogenous events. To simplify the discussion, I outline the intuition that underlies the research setting in the following section. Appendix A provides a simple example illustrating how monetary tightening reduces a bank's liquidity, and ultimately its lending.

2.2.2. Monetary policy, bank liquidity and bank lending

Monetary tightening weakens banks' liquidity position through reducing their reserves. U.S. banks fund their operations through selected deposit liabilities and are legally required to hold reserves in the form of vault cash or deposits at the Fed. The size of required reserves for each bank is determined by applying the legal reserve ratios to the bank's reservable liabilities. For example, assume that the legal reserve ratio is 10 percent, and that the bank holds $100 million reservable liabilities, the required reserves is then $10 million. An important implication of this fractional reserve system is that the ability of a bank to finance through reservable liabilities directly depends on the amount of reserves it holds. Based on the same legal reserve ratio in the above example, and suppose that a bank holds $10 million reserves, the bank will be restricted to maintain at most $100 million reservable liabilities to comply with the legal requirement. Any reserve reductions caused by monetary contractions will decrease the bank's ability to use reservable liabilities as a funding source.

Thus, following monetary contractions, banks generally have greater incentives to use other forms of financing that require lower reserves or even no reserves (e.g., large certificates of deposits (CDs)). However, unlike reservable liabilities, which are primarily federally insured deposits, nonreservable capital providers lack the same insurance protection. As such, investors' concern with adverse selection and moral hazard will limit banks' ability to obtain financing

from these alternate sources (e.g., Lucas and McDonald 1992). As a result of liquidity shortage, many banks will have to hold back new lending.[7] Consistent with this idea, Kashyap and Stein (1995, 2000) find that lending of small banks (who are more subject to information issues) is more negatively affected by monetary tightening than large bank lending. This negative effect is more pronounced if the bank lacks marketable assets to sell to generate liquidity.

Subsequent research has exploited a range of other cross-sectional differences between banks to confirm the varying impacts of monetary tightening on bank lending along these differences. For instance, Campello (2002) and Ashcraft (2006) focus on affiliations with a multibank holding company. Kishan and Opiela (2000, 2006) examine the role of bank capital. More recently, Holod and Peek (2007) show that small, privately-held banks are the most sensitive group. The authors conjecture that the relatively limited financial disclosures provided by these banks contribute to their main findings, but they do not test this conjecture.

2.2.3. Hypothesis development

Because of the limited supply of other readily available information, financial statements provide depositors and other funds suppliers with a particularly important source of information concerning a small bank's financial position. As such, it is often suggested that "reliable financial reports

[7] Stein (1998) develops an adverse selection model articulating the direct impact of a Fed-induced tightening on bank lending. See Kashyap and Stein (1994) for an expanded discussion of this view.

are necessary for [small banks] to raise capital" (Federal Register 1999, 57095). However, accounting reliability will be low if managers abuse their discretion in accounting policies and estimates. The following discussion focuses on both important regulatory and voluntary efforts that enhance the reliability of bank disclosure.

The regulatory monitoring over financial reporting occurs in the context of periodic on-site "safety-and-soundness" examinations. These examinations take place at each bank at least once every eighteen months. The aim is to evaluate the financial health of the bank and to provide early identification of both problems and corrective actions. As such, bank examiners focus on assessing whether the overall management quality is sufficient for the nature and scope of the bank's business (especially the high-risk areas relating to lending). Such assessments are based on, for example, appraisals of bank internal controls, reviews of loan records and other confidential documentations such as internal audit work papers, and interviews with bank personnel. After evaluating the bank's credit control and loan quality, examiners are required to verify the bank's financial disclosures and determine whether its allowance for loan losses is adequate (Federal Reserve Board of Governors 1999).[8] Consistent with the monitoring of examiners, Gun-

[8] Similar to external auditors, bank examiners may review the working papers that support the information disclosed in the bank's financial reports and verify whether the disclosures agree to the bank's general ledger.

ther and Moore (2003) show that regulatory reviews are positively associated with accounting restatements that correct loan loss underreporting.

To the extent that regulatory oversight is effective, it assures a minimal level of disclosure quality. If investors believe such efforts are effective and sufficient, then banks having voluntary mechanisms safeguarding reporting reliability are unlikely to have significant incremental benefit in terms of reducing information asymmetry. On the other hand, it is important to note that regulatory reviews are not without limitations. For example, examiners have difficulty measuring banks' loan loss exposures, and have agreed with overstatements of asset values made by banks that later failed (U.S. General Accounting Office 1991). Such failures can be partly attributable to regulators' resource constraints that reduce the effectiveness of their oversight (the Federal Deposit Insurance Corporation 1997, Chapter 12). Separately, examiners do not opine on the fair presentation of the bank's financial reports. In fact, they do not release the examination findings to the public. As such, investors may welcome additional monitoring and assurance by independent parties. Consistent with this demand, many small banks *voluntarily* engage external auditors to signal their commitment to credible reporting. In the discussion below, I provide further institutional details concerning bank auditing.

Under Section 36 of the Federal Deposit Insurance Act, as implemented by 12 CFR Part 363 of the Federal Deposit Insurance Corporation (FDIC)'s regulations, banks with $500 million

or more in total assets at the beginning of their fiscal year must have an annual audit conducted in accordance with generally accepted auditing standards (GAAS) by an independent public accountant.[9] This requirement, together with others specified in Section 36, are "intended to mitigate information asymmetries between banks and their stakeholders by improving the quality and oversight of financial reporting" (LaFond and You 2010, 76). However, due to high compliance costs, banks below the stated threshold are not subject to Section 36. Thus, apart from a full-scale financial statement audit, small banks can choose one of several low-cost alternatives. Before 2000, the ending period of when the audit status is used in this paper, the acceptable alternatives were as follows: "a well-planned directors' examination, an independent analysis of internal controls or other areas, a report on the balance sheet, or specified procedures" by a qualified independent third party (Federal Register 1996, 32440).[10]

Because an external audit involves significantly more extensive planning and procedures to verify the information provided in accounting reports than the other alternatives (Singh 2007),

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[9] This requirement applies with respect to fiscal years of insured banks beginning after December 31, 1992. Before this date, private insured banks were generally not required to have an annual audit.

[10] It is unlikely that these low-cost alternatives provide comparable monitoring to what an external audit offers. Take the directors' examinations as an example, which consist of agreed-upon procedural reviews of financial statements by bank directors themselves or other authorized parties. Partly because of state requirements, the majority of unaudited banks obtain directors' examinations. However, despite their prevalence, no widely accepted national standards exist for the specific procedures that must be performed (Federal Register 1996, 32441). Also, the responsible parties do not report on the fairness of the financial statements, nor do they attest to the effectiveness of the internal control over financial reporting (Federal Register 1999, 57098).

bank regulators routinely advocate for an audit as the preferred choice to enhance the reliability of financial reporting (Federal Register 1996, 32439). Similarly, in their report to Congress on banking failures, the General Accounting Office (1991, 8) argues that "without the discipline of an audit, troubled institutions are more able to cover up their financial difficulties." To more formally assess the effect of auditors on bank disclosure, I analyze in Section 2.6 whether audited small banks exhibit relatively more timely recognition of loan losses in earnings.

In its exposure draft for the conceptual framework for financial reporting, the Financial Accounting Standards Board (FASB, 2008, BC2.43) stresses that accounting "information will not be of much help in decision making if users do not consider it worthy of belief …… Whether users consider the information in an entity's financial report to be credible will depend heavily on their view of the trustworthiness of the entity's management and auditors, as well as on their view of the relevance of the information in the report and the degree to which it faithfully represents the underlying economic phenomena." In this paper's context, this argument suggests that investors view audited small banks as having more credible disclosures and fewer information problems than unaudited banks, even though they are all subject to regulatory scrutiny. Based on this argument, I predict that audited small banks will have greater access to nonreservable debt when they react to monetary contractions. Stated in alternative form, the hypothesis is as follows:

H1 The growth of uninsured nonreservable liabilities during periods of monetary policy tightening is higher for audited banks than for unaudited banks.

As discussed earlier, if a bank is not able to replace the amount of insured deposit outflow with nonreservable liabilities after a contraction of monetary policy, it will likely be forced to hold back new lending. Thus, I test the following hypothesis on the impact of accounting credibility on banks' investments:

H2 Suppression of lending during periods of monetary policy tightening is lower for audited banks than for unaudited banks.

The above hypotheses suggest one situation where banks may benefit from the added credibility of audited financial reports. A remaining question is, if the benefits are significant, why then would we not observe all small banks being audited to demonstrate their reporting commitment. Note that the audit choice depends on the differential costs and perceived benefits for each bank. For example, because audit fees contain a fixed component, it may not be cost effective for smaller banks to obtain an audit (Kohlbeck 2005).[11] Conditional on these bank characteristics, the audit choice also depends on managers' personal beliefs and experience. Some managers

[11] For example, auditors have to spend a significant amount of time in order to acquire the required familiarity with the specific business activities and operating environments of a new client. Consistent with the argument of cost effectiveness, Table 2.5 column (8) shows that only 47 percent of the subsample of small banks with total assets between $25 M and $50 M are audited, compared to 81 percent in the subsample of banks with total assets between $100 M and $300 M (column (10)).

might simply believe that the benefits from voluntary audits are limited, as banks are all subject to regulatory audits anyhow.[12] On the other hand, other managers who truly believe that auditors provide additional monitoring might be concerned with unwanted regulatory interventions. Especially, regulators use accounting information to identify weak banks and determine penalty actions such as restrictions on operations or even forced management turnover (e.g., Cheng et al. 2011). Having additional monitoring by auditors will limit managers' ability to hide their bank's problems from regulators. Adding to this concern is that obtaining an audit is difficult to reverse, suggesting that the additional monitoring will be in place for an extended period of time.[13] To sum, at a given point in time, the choice of having an external audit or not likely reflects external factors that affect managers' perception about the monitoring of auditors (e.g., managers' past experience working with CPAs). At the same time, certain bank characteristics are also expected to play a role. As discussed in the next section, such bank characteristics could give rise to identification challenges, an issue I explicitly address using different research strategies.

[12] When asked about their views on having mandatory external audits for all small banks, some bank managers expressed that "Banks our size were exempted by Congress from having full audits. My concern is the addition of red tape for no real gain." Other managers argued that "I don't know of any evidence that bank failures have been caused by the lack of banks having certified audits (Cocheo 1998, 7)."

[13] For example, firing auditors without justifiable reasons raises questions concerning the underlying motives. It can send negative signals to investors and regulators about the financial viability of the bank.

2.3. Empirical model specification

2.3.1. Identification issues

Despite operating in the same industry segment, audited and unaudited small banks could have different equilibrium financing and lending patterns. Regardless of these differences, audited banks are predicted to grow uninsured financing more rapidly and decrease lending more slowly following the same exogenous liquidity losses caused by monetary tightening. By focusing on such *differential changes*, the research design here reduces the effect of confounding cross-sectional factors common to studies examining directly the endogenous relation between financial reporting decisions and firm operations.

On the other hand, if audited banks have unique characteristics (apart from more credible reporting) that somehow cause their monetary policy responses to differ from those of unaudited banks, the results will be confounded by omitted correlated variables. For example, if banks inherently prone to liquidity losses tend to obtain an audit as a precaution, such tendency can lead to underestimation of the expected benefits of having an audit. In principle, net bias due to confounding bank characteristics could be either positive or negative. I thus use a number of strategies to deal with this issue. First, the main tests explicitly control for factors that may be associated with both bank audit status and monetary policy reactions, as identified in prior studies. To the extent that the list of control variables is comprehensive, differences in policy re-

sponses between audited and unaudited banks can be attributable to differential reporting credibility. Second, I use both the Heckman (1979) two-stage approach and the predicted probability of an audit as an instrument to more formally address potential selection issues. The estimates indicate that if anything selection bias tends to work against finding the value of an audit. Third, due to known limitations in applying the two-stage approaches in accounting research, I conduct corroborating tests that exploit time-series changes in bank audit status to ensure the results are robust to alternate specifications. Finally, I test whether the predicted differential changes between audited and unaudited banks are more pronounced among smaller banks who likely benefit from auditor assurance more than the larger banks.

2.3.2. Empirical model

I use the following pooled time-series cross-sectional model for the main tests:

$$\begin{aligned} \text{D\_Growth}_{it} = {} & \alpha + \textstyle\sum_{j=0}^{5} \beta_j \, \text{TightMP}_{t-j} + \gamma \, \text{Audited}_{it-4} + \sum_{j=0}^{5} \delta_j \, \text{TightMP}_{t-j} \times \text{Audited}_{it-4} \\ & + \zeta \, \text{Bank-level control}_{it-1} + \textstyle\sum_{j=0}^{5} \eta_j \, \text{TightMP}_{t-j} \times \text{Bank-level control}_{it-1} + \ldots\ldots \\ & + \textstyle\sum_{j=1}^{5} \theta_j \, \text{D\_Growth}_{it-j} \; + \sum_{j=0}^{5} \iota_j \, \text{GDP\_Growth}_{t-j} + \sum_{j=0}^{5} \kappa_j \, \text{CPI\_Growth}_{t-j} \\ & + \lambda \, \text{Basel}_t + \textstyle\sum_{j=1}^{50} \mu_j \, \text{State}_{ij} + \nu \, \text{Time trend}_t + \sum_{j=1}^{3} \xi_j \, \text{Quarter}_{jt} + \varepsilon_{it} \end{aligned} \qquad (1)$$

where the *i* and *t* subscripts denote the bank and quarter, respectively. In general, the quarterly growth of the dependent variable (D_Growth), which measures changes in bank uninsured liabilities or lending, is regressed against a set of monetary tightening indicators (*TightMP*), an audit indicator (*Audited*), its interaction with policy variables (*TightMP* × *Audited*), and a set of

controls. The focus is on the moderating effect of an audit on banks' financing and lending responses to monetary policy tightening. This effect is captured by the sum of the coefficients on the interaction term *TightMP* × *Audited* (i. e. , $\sum \delta$). Further explanation of the regression model is provided below, with details of variable measurement given in Appendix B (Panels A and B).

2.3.2.1. Dependent variables

To test for the financing benefits of credible reporting (H1), I study changes of uninsured liabilities that banks commonly use to adjust liquidity (a.k.a. managed liabilities). A particularly important component of managed liabilities are large certificates of deposits (CDs) (e.g., Kashyap and Stein 1994; Kishan and Opiela 2000; Holod and Peek 2007). Large CDs are those issued in denominations above the $100,000 limit for deposit insurance coverage applicable during the sample period. According to Morris and Walter (1998), most large CDs have original maturities within one year, though some have maturities as long as five years or even more. Many of them are negotiable: like bonds, these large CDs can be resold in a secondary market before they mature. Primarily for this reason, public investing communities such as business corporations, financial institutions, local authorities, and municipalities often buy large CDs as investments of their idle funds (e.g., Murdeshwar 1970; Mishkin 2006, Chapter 9). Importantly, these investors

routinely use bank accounting information to assess the quality of large CD issuers.[14] It follows that the perceived reliability of financial reporting can affect investors' decisions.

In addition to large CDs, the measure for managed liabilities also includes other forms of debt financing that can be affected by the supply of credible accounting information: brokered deposits,[15] subordinated notes, and other borrowed money.

Following existing research (e.g., Campello 2002), I use the quarterly growth of total loans to assess the investment impact of credible reporting (H2).

2.3.2.2. Independent variables of interest: monetary policy and the external audit status

To capture the effect of monetary actions on individual banks, I use a tightening indicator (*TightMP*) that equals one if contractionary policies take place in the quarter. To determine the quarterly policy stance, I draw on the narrative index developed by Boschen and Mills (1995) based on their study of the policy records of the Federal Open Market Committee (FOMC) and

[14] For example, the investment policy of Troy Capital Resource Corporation (CRC) states that "investments in time deposits and certificates of deposits are to be made with banks or trust companies. Their annual reports shall be reviewed by the CRC Treasurer as Chief Fiscal Officer to determine financial strength" (See Section 11, http://www.troyny.gov/economic_development/crc/CRCInvestmentPolicy.pdf, retrieved 2 Oct 2010). Similar examples can be found in the investment policies of Dutchess Tobacco Asset Securitization Corporation (see Section IIIB, http://www.co.dutchess.ny.us/CountyGov/Departments/DTASC/DTInvestPolicy.pdf , retrieved 2 Oct 2010), County Of Sullivan Industrial Development Agency (see Section B2, http://www.sullivanida.com/policies/InvestmentPolicy.pdf, retrieved 2 Oct 2010), and the Illinois State Treasurer (see Section 4.0, http://www.treasurer.il.gov/about-us/pdf/GRInvestmentPolicy20081224.pdf, retrieved 2 Oct 2010).

[15] To proxy for uninsured brokered deposits, total brokered retail deposits issued in denominations of less than $100,000 are excluded.

other historical documents.[16] I corroborate the results using a widely-used money market indicator of monetary policy (See Section 2.5.7).[17] Since the results based on either measure are similar, for ease of exposition, I focus on the results based on the Boschen-Mills index.

The FOMC policy records indicate that monetary policy is consistently set in terms of its intended effect on inflation and real economic activity. Contractionary policies are generally motivated by policymakers' desire to reduce inflation. In contrast, expansionary policies are intended to promote real economic growth. As such, depending on the importance that policymakers assigned to reducing inflation relative to promoting real growth in their policy direction, Boschen and Mills (1995) classify the policy stance each month into five categories from "strongly contractionary" (coded -2) to "strongly expansionary" (coded 2).[18] A "neutral" policy is coded a zero value.

The primary advantage of the Boschen-Mills index is that it uses the stated objectives of the policymakers to determine the policy stance. Prior research assessing the validity of the

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[16] The FOMC makes key decisions regarding the conduct of open market operations—purchases and sales of U.S. government and federal agency securities— through which monetary policy actions are implemented and the supply of bank reserves are affected (Mishkin 2006, Chapter 12).

[17] A limitation of using money market variables as alternate policy measures is that these variables can fluctuate for reasons other than monetary policy decisions.

[18] See Boschen and Mills (1995, 27 footnote 7 and Table I) for extracts from the FOMC historical policy records that indicate contractionary policies. These extracts highlight the Fed's intention that "money market conditions should be permitted to tighten still further", or the Fed's desire to have "further reduction in net reserve availability" in the banking system.

Boschen-Mills index concludes that it is a reliable measure of policy stance (e.g., Jefferson 1998). Also, Boschen and Mills (1995) show that contractionary policies as per their index are associated with significant reductions in the supply of bank reserves. Finally, many previous studies have applied the index in various contexts to capture monetary tightening (e.g., Thorbecke 1997; Campello 2002; Weise 2008).

Figure 2.1 charts the value of the Boschen-Mills index at each quarter end throughout the sample period (1988:Q1 – 2000:Q4). Panel A plots this index along with the share of bank assets funded by insured deposits. The share information is computed using aggregate balance sheet data provided by the Fed for all small domestically chartered commercial banks. As expected, this chart shows that monetary tightening reduces the supply of insured deposits. In line with banks financing more through uninsured liabilities when policy is tightened, Panel B indicates that there was a corresponding increase in banks' use of managed liabilities (large time deposits and borrowings). Hence, changes in banks' funding mix were well correlated with contractionary periods as per the Boschen-Mills index in the expected direction. I thus set the value of the policy indicator (*TightMP*) to one accordingly. Altogether, there were four separate contractionary cycles between 1988 and 2000. On average, each cycle lasted for about six quarters, with contractionary policies in each quarter of the cycle.

If banks' responses to contractionary actions are both swift and complete, then policy shocks in one quarter would have no impact on future quarters. However, studies have consistently found delays in banks' responses to monetary contractions (e.g., Bernanke and Blinder 1992). There are also frictions that prevent banks from shifting lending within a short period of time. For example, because bank loans are quasi-commitment contracts, banks tend to adjust lending gradually. If this delay is not considered, researchers will underestimate the overall effect of the contractionary actions. To avoid this problem, I follow prior research and include a number of lags of the policy indicator in the regression.[19] Specifically, I allow a given contractionary quarter to have prolonged effect for up to five subsequent quarters (See Panel A of Figure 2.2).[20] The cumulative effect of the contractionary quarter can then be gauged from the sum of the coefficients on *TightMP*, and a *t*-test whether this sum is statistically significant (i.e., H_0: $\sum \beta = 0$).[21] Section 2.4.3.2 provides further details about an alternative interpretation of this sum.

[19] I also perform tests that include only the contemporaneous contractionary actions. This assumes that the effect of current contractions will be fully realized in the same quarter. The details of the tests are provided in Section 2.4.3.

[20] It is theoretically unclear how many lags of contractionary policy one should include, which depends on the time it takes for the economy to fully respond to a policy action. Research suggests this process can take up to two years (Bernanke and Blinder 1992; FRBSF 1999). Thus, some studies include as many as eight lags in their analyses (e.g., Campello 2002; Ashcraft and Campello 2007), although most studies use four to six lags (e.g., Kashyap and Stein 2000). Alternatively including four or six policy lags does not affect the main results in this paper qualitatively.

[21] See Wooldridge (2003, 139) for testing hypotheses about a single linear combination of the parameters.

The main question of interest is whether there are significant cross-sectional differences in the way audited and unaudited banks respond to monetary contractions. Thus, to allow the responses of audited banks to vary, the policy variables (*TightMP*) are interacted with an audit indicator (*Audited*) for banks that received a full-scale financial statement audit in the previous year.[22] An external audit is predicted to facilitate raising external financing, thereby mitigating the negative policy effects on total loan growth. The sum of the coefficients on the interaction term (i. e. , $\sum \delta$) is thus expected to be positive in both tests of funding and lending responses.

2.3.2.3. Bank-level control variables

The first set of control variables includes bank-level characteristics ($Bank\text{-}level\ control$). To isolate the effect of credible reporting, I include eight variables to control for attributes that may correlate with both the bank's audit status as well as its policy reactions. Prior research shows that bank size dominates the audit status (Kohlbeck 2005). I therefore add the natural log of total assets (*Ln(TA)*) to account for the differences in financing prospects associated with bank size (Kashyap and Stein 2000).

Next, affiliations with a bank holding company are considered. Bank holding companies are a dominant industry feature, and are divided into either multibank or one-bank holding com-

[22] A potential follow-up test is to assess whether the identity of external auditors (e.g., big auditors versus small auditors), which affects investors' perception of auditor quality, has an impact on banks' responses to monetary policy. Unfortunately, information on auditor identity is not available for conducting this type of follow-up test.

panies. I exclude small banks owned by a multibank holding company. Compared with other small banks, these banks are relatively unaffected by monetary policy because of the supply of internal capital by other members within the same banking group (e.g., Campello 2002; Ashcraft 2006). Since banks are also likely to be audited through the consolidated audit of the holding company, excluding them limits the potential confounding affiliation advantages.

The situation is different for one-bank holding companies, which are often merely vehicles used by small banks to circumvent restrictions on branching and other operating aspects (Rousakis 1989). As is commonly understood (and verified in unreported tests), the assets of the bank subsidiary typically represent substantially all of the holding company's consolidated total assets. Further, Table 2.2 Panel B shows that the proportion of banks owned by a one-bank holding company is similar regardless of the audit status. Hence, to preserve sample size, I retain banks that are owned by a one-bank holding company. An indicator (*OBHC*) for these affiliated banks is included to account for any potential impacts due to differences in organizational forms.[23]

In addition, an indicator for banks that operate in a Metropolitan Statistical Area (*MSA*) is introduced. This is intended to adjust for the potentially greater financing and lending opportuni-

[23] As a robustness check, I repeat the tests separately for independent banks and the banks affiliated with a one-bank holding company. Unreported results show that the main results hold for both groups of banks.

ties (and perhaps, the availability of an audit) for banks that operate in urban areas (Campello 2002). Its interaction term with the policy variables is predicted to be positive.[24]

Audited and unaudited banks differ in terms of capitalization (Kohlbeck 2005). And because banks with a low capital to total assets ratio tend to face more financing frictions (Kishan and Opiela 2000), an indicator variable for these banks is included (*LowCap*).[25] Following Ashcraft (2006), I set *LowCap* to one if the bank's equity to assets ratio is below six percent. Using alternate cut-off points does not affect inferences. To further account for differences in risk profile between audited and unaudited banks, I include the ratio of non-performing loans to total loans (*NPL*), and an indicator for banks reporting net losses (*Loss*).[26] Like *LowCap*, their interaction terms with the policy proxies are expected to be negative.

Banks' liquidity position can affect their audit decision as well as their responses to monetary contractions. To control for bank liquidity, I include the ratio of liquid assets to total assets (*Liquid assets*) (Kashyap and Stein 2000). I also include the bank's ability to generate internal cash flows (*Internal CF*), as proxied by the ratio of (i) the sum of income before extraordinary

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[24] Unreported tests that split the sample based on whether banks operating in a Metropolitan Statistical Area or not show that the positive effect of audited financial statements hold for both groups of banks.

[25] Banks' capitalization can be mechanically related to their leverage ratio, which in turns can affect growth in liabilities in unexpected ways. Thus, for robustness checks, I exclude *LowCap* from the tests. All results are qualitatively similar with this change in the regression specification.

[26] Robustness tests show that the main results persist after adding an additional performance control (ROA). To keep the regression parsimonious, I do not include this control in the main tests.

items and provision for loan losses to (ii) beginning of period total loans (Houston et al. 1997). The signs for their policy interaction terms are predicted to be negative (positive) in the test of H1 (H2). Ceteris paribus, banks with greater internal liquidity are less likely to need external funds. Similarly, their loan portfolios are less likely affected by liquidity shocks.

2.3.2.4. Economy-wide and other factors

The list of control variables also includes economy-wide and other factors. First, I add five lags of the dependent variable (*D_Growth*) to account for bank-specific unobservables that affect the bank's current growth (e.g., distinct business strategies). Current and five lags of each of the growth rates of GDP (*GDP_Growth*), and the consumer price index (*CPI_Growth*), are used to control for overlapping macroeconomic changes and inflation, respectively. An indicator for the time period 1988 – 92 (*Basel*) is included to account for changes in bank capital regulations in late 1980s.[27] I also include a set of indicators for the state in which the bank operates to adjust for local economic conditions.[28] Finally, a linear time-trend, and three quarterly indicators are added to control for time and seasonal effects. To keep the regression model parsimonious, I only include the main effects of these variables.

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[27] Risk-based capital requirement were adopted in the U.S. in 1988 and phased-in through the end of 1992.

[28] Results are not sensitive to alternatively applying state employment, and state personal income growth rates to capture the effects of local economic conditions.

2.4. Sample, descriptive statistics, and empirical results

2.4.1. Data sources and sample

Consolidated individual bank-level data are obtained from the Federal Reserve Bank's Report of Condition and Income database (Call Reports). The sample period starts in 1988:Q1 when information on bank audit status became available. It ends in 2000:Q4 to avoid complications introduced by known changes in the regulatory environment. First, monetary actions became more transparent over time and especially after 2000,[29] making them less a shock to banks. This change works against the maintained assumption that banks cannot fully anticipate the forthcoming monetary tightening. At the same time, there were different changes in the bank auditing and reporting environments.[30] To reduce the impact of these changes, I measure bank audit status until December 31, 1999, with the funding and loan responses to monetary policy being assessed until 2000:Q4. As shown in Figure 2.1, the sample period covers twenty three contractionary quarters clustered in four separate policy cycles.

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[29] For instance, the Federal Open Market Committee announced in 2000 that it would publicly communicate the stance of monetary policy following each regularly scheduled meeting.

[30] For example, a new interagency policy statement concerning external auditing programs became effective on January 1, 2000 (Federal Register 1999). This new statement describes additional alternatives to a financial statement audit. Also, due to changes in reporting requirements, variable definitions in the Call Reports for key regression variables (e.g., total loans) were changed after 2000.

Table 2.1 reports the sample selection process and results. The initially available observations include 616,469 bank-quarters. To create a broadly homogenous sample, I exclude entities other than FDIC-insured commercial banks. Foreign banks, banks inactive in the loan markets, credit-card banks, and banks subject to special analysis by regulators are removed because of their different operations and regulatory supervision. Further, I exclude all publicly-traded banks and banks with total assets greater than $500 million because they have mandatory audit requirements. As discussed in Section 2.3, banks affiliated with a multi-bank holding company are also excluded. These exclusions reduce the sample by 263,611 bank-quarters

Next, mergers among banks confound balance sheet measures of changes in liabilities and lending. Thus, I exclude all bank-quarters in which a merger occurs. Observations in the first three years of a bank's operations are also excluded because banks during this period are subject to different regulatory supervision (Singh 2007). Banks with non-positive total assets, missing audit indicator, or missing required financial data are all removed. Finally, I exclude observations with quarterly total asset growth greater than 50 percent to minimize the influence of potential data errors and outliers. The final sample includes 327,084 bank-quarters.

2.4.2. Descriptive statistics

Table 2.2 Panel A provides summary statistics of commonly reported balance sheet items and other bank-level regression variables for the full sample. By construction, the sample banks are

small, with mean total assets of $70 million. On average, banks have about half of total assets in loans, and a quarter in liquid assets. On the liability side, total liabilities contribute more than 90 percent of total assets. The most important component of liabilities is deposit accounts of $100,000 or less (core deposits), which comprise 79 percent of liabilities. On average, about 15 percent of total liabilities are managed liabilities, which consist mainly of large certificates of deposits (CDs). Finally, despite the absence of a mandated audit requirement, 54 percent of the bank-quarters were audited.

Panel B presents the summary statistics separately for audited and unaudited banks. Audited banks are on average twice as large as unaudited banks, so controlling for bank size will be important. Further, audited banks generally hold a lower level of on-balance-sheet liquid assets and invest more in loans, including relatively illiquid loans such as commercial and industrial (C&I) loans and real estate loans. Audited banks have on average a higher frequency of loss observations, a lower level of return on equity, and a higher ratio of nonperforming loans. On the liability side, audited banks hold slightly less equity and rely relatively less on core deposits, which are compensated by a higher level of nonreservable funds such as large CDs. This statistics suggests that audited banks are more willing or able to take risks.

2.4.3. Testing the financing and investment benefits of credible accounting information

This section reports the main regression results. Table 2.3 presents the financing test results (H1), while Table 2.4 shows the corresponding loan test results (H2). In regressions (1) and (2) of both tables, the monetary contraction indicator variable (*TightMP*) includes only policy shocks in the contemporaneous quarter, thus assuming no impact of current contractionary actions on subsequent quarters. In regressions (3) and (4), five lagged policy terms are added to capture delays in banks' responses to monetary tightening. The coefficients associated with *TightMP* in these two columns are the sums of the six coefficients on the contemporaneous and the lagged monetary policy variables. The *t*-statistics in parentheses (for both individual coefficients and the sums of the coefficients on policy terms) are computed using robust standard errors clustered by bank. In columns (3) and (4), I also report p-values for the *F*-test that the coefficients on the five lagged interactions *TightMP* × *Audited* are jointly zero. For brevity, the tables omit the economy-wide and other factors discussed in Section 2.3.2.4.

2.4.3.1. Statistical significance

The dependent variable in Table 2.3 is the quarterly growth of managed liabilities (*ML_Growth*). Results reported in column (1) show that the coefficient on *TightMP* is significantly positive, consistent with small banks issuing more managed liabilities in response to contractionary ac-

tions. More importantly, in line with the first hypothesis that audited banks face relatively less funding frictions, the coefficient on *TightMP* × *Audited* is positive and significant (t = 7.07).

The results supporting H1 is robust to controlling for factors related to bank size, operating environments, financial health, and liquidity position. Consistent with prior studies, column (2) shows that access to managed liabilities when banks react to tight policies is strongly positively associated with bank size, and a metropolitan (*MSA*) location. However, it is generally negatively correlated with attributes related to bank risk (i.e., a low level of equity to assets ratio, and frequency of losses), and the availability of internal funds (holding of liquid assets and the ability to generate internal cash flows). In general, the results show no significant effects related to a one-bank holding company structure (*OBHC*).

Columns (3) and (4) report the results that include the contemporaneous and five lags of monetary policy. Both the magnitude and statistical significance of the coefficients on *TightMP* associated variables increase. These results are consistent with banks' delayed response to monetary policy.

Turning to the test of H2, Table 2.4 shows the results of estimating Equation (1) using the quarterly growth of total loans as the dependent variable (*Loan_Growth*). Column (3) shows that audited banks are better able to protect their lending from policy-induced liquidity losses. In particular, the sum of the coefficients on *TightMP* × *Audited* is positive (t = 11.61). As shown in

column (4), the results are robust to adding controls for different bank-level characteristics. In line with prior studies, the lending responses are positively associated with bank size, a metropolitan location, and internal liquidity. On the other hand, they are largely negatively correlated with bank risk.

2.4.3.2. Economic significance

To interpret the economic benefits for audited banks, I focus on the results from the full model as reported in column (4) of Table 2.3 and Table 2.4. First, I use the coefficient estimates and bank-level controls evaluated at their median values to predict a typical bank's responses to a given contractionary quarter, including the effects in quarters Q to Q+5 (see Panel A of Figure 2.2). Appendix C shows the computations. An unaudited bank responding to monetary contraction in quarter Q is expected to have a cumulative 9.84 percent increase in managed liabilities over the subsequent five quarters. Importantly, an external audit is associated with an additional 1.93 percent increase (the sum of the six coefficients on *TightMP* × *Audited*). This represents 19.61 percent (=1.93 / 9.84) of the predicted policy response. Turning to total loan growth, the model predicts a cumulative 2.49 percent decline in total loans for an unaudited bank five quarters after the given contractionary quarter. An external audit is expected to mitigate such a reduction by 20.88 percent (= 0.52 / 2.49). These estimates suggest that voluntary external audits dampen the effect of monetary contraction on the operations of small private banks by about one-fifth.

Alternatively, the results can be interpreted from the perspective of a given quarter that follows contractionary actions in past quarters. Specifically, consider the final quarter of a six-quarter contractionary cycle (see Panel B of Figure 2.2).[31] The sum of coefficients on *TightMP* × *Audited* then suggests that the growth of managed liabilities in quarter Q is 1.93 percentage points higher for an audited bank than for an otherwise similar unaudited bank. Correspondingly, the decline in total loans in the quarter is predicted to be 0.52 percentage point lower for the audited bank. These are economically relevant effects in this context considering the unconditional mean (median) quarterly growth of managed liabilities for the sample period is only 2.2 (0.7) percent and that for loans is 1.9 (1.8) percent (See Panel A of Table 2.2).

2.5. Further tests of H1 and H2

In this section, I discuss results from various sensitivity tests reported in Table 2.5 and Table 2.6. For ease of presentation, except otherwise stated, only the sum of the coefficients on *TightMP* × *Audited* (i.e., $\sum \delta$) is reported. Panel A of Table 2.5 presents the financing test results (H1), while Panel B shows the results on the test of investment in loans. The basis for all tests is the model shown in Equation (1). Column (1) of Table 2.5 shows the benchmark results from column (4) of Tables 2.3 and 2.4.

[31] As mentioned in Section 2.3.2, each contractionary cycle in the sample period lasted for about six quarters.

2.5.1. Differential impacts of monetary contractions on bank borrowers

Table 2.2 showed some differences in the composition of loans between audited and unaudited banks. These differences can confound inferences if monetary actions have systematically different effects on different types of borrowers (not banks themselves). In particular, if borrowers of unaudited banks are somehow relatively more sensitive to monetary actions, then following monetary contractions, unaudited banks' lending opportunities will be more restricted. In turn, unaudited banks may require less external funding to fulfill their loan demand than audited banks. According to this logic, the differential impacts of monetary contractions on borrowers, rather than the differences in banks' access to the external capital markets, are the driving force behind the main results.

The above scenario assumes that borrowers of unaudited banks are more affected by monetary contractions than those of audited banks. However, this assumption is questionable. In particular, audited banks generally hold more of their loan portfolios in commercial and industrial (C&I) loans. Since C&I loans often have short terms and carry variable interest rates, borrowers of C&I loans tend to be more sensitive to increases in short-term rates than other borrowers. Thus, to the extent that contractionary policies affect the loan demand for small banks, *audited* banks seem to be the group of banks that will be affected more.

Nonetheless, to address this concern, I repeat the main analyses, adding information about loan portfolios and the corresponding interaction terms. In particular, I include variables for 1) commercial and industrial loans; 2) real-estate loans; and 3) individual loans. I also include loan commitments to hold constant the impact of the bank's exposure to credit on demand (Morgan 1998). All these loan variables are scaled by total loans. The results in column (2) of Table 2.5 suggest that differences in loan mix are unlikely the driving force of the main results.

2.5.2. Differential reactions to other macroeconomic changes

Since monetary policy changes and other macroeconomic movements may overlap, it is possible that the documented results are driven by the differential reactions of banks to other economy-wide changes (rather than differences in funding access). To address this issue, I repeat the analyses, adding new interaction terms between each of the bank-level characteristics and changes in the general economy (captured by the current and the five lagged values of GDP growth). Column (3) shows that the main results continue to hold.

2.5.3 Confounding unobservables: the Heckman two-step approach

Banks use their discretion to choose whether to be audited. If unobserved bank characteristics that drive the audit choice also affect how banks react to monetary tightening, this can introduce bias to the coefficient estimate on *TightMP* × *Audited*. To address this issue, I apply the Heckman (1979) two-stage approach to more formally address possible selection bias.

Francis et al. (2010) emphasize that, to successfully address selection issues using the two-stage approach, researchers have to identify at least one independent variable that is strongly correlated with the endogenous choice in the first-stage selection model, but not significantly correlated with the dependent variable in the second-stage regression. In my test, I use an indicator variable for the presence of an audit five years ago (*PastAudit*) to meet this exclusion restriction requirement. This is motivated by the sticky nature of an audit. Since obtaining an audit is costly to reverse, small banks with an audit in the past are strongly associated with the same audit status in the future (Kohlbeck 2005). In contrast, it is unclear that a bank's past audit choice would be at all associated with the current quarterly growth of managed liabilities (or total loans), which happens more than five years later.

Additionally, I include in the audit choice model bank characteristics that are positively related to both bank complexity as well as the propensity of obtaining an audit. These consist of: whether the bank has other branches, the ratio of non-interest income to total assets, and the five-year standard deviation of return on assets (Kohlbeck 2005). Further, I include: (i) all the bank-level controls used in Equation (1), Section 2.3.2; (ii) the four loan related variables used in Section 2.5.1 (to capture differences in lending operations); and (iii) a set of indicators for the year and state in which the bank operates. The details of all these explanatory variables are further discussed in Appendix D.

The probit model of audit choice is estimated using annual data. The sample period starts in 1992, the first year I have information about a bank's audit status five years ago. It ends in 1999, corresponding to the last period when the audit status of a bank is measured in the main tests. After applying the same sample selection criteria reported in Table 2.1, there are 36,676 firm-years with required data. As reported in Appendix D, the model fits the data well. The pseudo-R^2 is approximately 50 percent, and the model is able to sort firms into the right group more than 90 percent of the time.[32] This degree of accuracy largely reflects the strong association between *PastAudit* and current audit status.

Estimates from the probit model are then used to compute the inverse Mills' ratio (*IMR*) for each sample bank. In the second-stage policy response regressions, *IMR* and its interaction terms with monetary policy serve as new control variables, in addition to those included in Equation (1). The reported results in column (4) of Table 2.5 show that the positive effect associated with an audit remains. Further, the sum of the coefficients of the *IMR* interaction term indicates that if anything the main results are negatively biased. Such bias can arise if banks inherently sensitive to liquidity losses endogenously choose to have audited financial statements as a precaution.

[32] In contrast, given the fraction of audited banks in the sample, a naïve model is expected to classify only roughly half of the sample correctly.

2.5.4. Confounding unobservables: the predicted probability as an instrument

In addition, I use the predicted probability of an audit from the previous probit model as an instrument for *Audited*. As shown in column (5), the main results hold under this alternative specification. To the extent that the predicted probability reflects only the part of the audit decision that is not related to banks' responses to monetary tightening, the differential responses between audited and unaudited banks can be attributable to credible reporting.

2.5.5. Confounding unobservables: within-bank variations in audit status

Because of the difficulty finding truly exogenous variables that satisfy the requirement of exclusion restrictions, recent studies have questioned the efficacy of using two-stage approaches over single stage methods that do not require researchers to find valid exclusion restrictions (Francis et al. 2010). Thus, I conduct tests that exploit time-series changes in bank audit status and assess whether the main results are robust to alternate model specifications. The first test studies small banks that changed audit status. If accounting credibility were the cause of differences in monetary policy reactions, liquidity constraints would be lower when banks are audited than when they are not. Supporting this conjecture, the results in column (6) are similar to the main findings. Since each bank is used as its own control in the tests, plausible correlated omitted variables have to explain differences in monetary policy responses within banks.

On the other hand, one might argue that the above test is limited because changes in audit status could simply reflect other changes in bank operations that simultaneously affect banks' responses to monetary tightening. For example, changes in management team might lead to changes in both the audit status as well as the bank's responses to monetary actions. To address this concern, I also compare policy responses only within banks that remained *constant* in audit status throughout the sample period. Since the audit status had been the same for years, it is much harder to imagine that the observed audit choice was a by-product of time-varying factors that also affected bank performance during current policy shocks. Also, if there were to be a set of omitted factors (e.g., managers' accounting expertise, and management style) consistently driving both the bank's audit decision over time as well as how it reacts to monetary actions, bank level fixed effects regressions would deal with the impact of such factors. Results shown in column (7) suggest that the main results persist even after controlling for these time-invariant bank specific factors.

2.5.6. Analyses based on subsamples of similarly sized banks

I repeat the tests separately for banks of different size categories for two important reasons. First, because bank size is the dominant variable that explains both banks' ability to issue managed liabilities (Morris and Walter 1998) and the likelihood of obtaining an audit (see Appendix D), an important issue related to the main tests is the confounding size advantage associated with

audited banks. It is therefore useful to verify whether the main results hold in subsamples of similarly sized banks. Second, this analysis allows for the contrast of results from the larger and smaller bank subsamples. Since smaller banks have limited internal accounting sophistication (Minnis 2010) or lower firm reputation (e.g., Blackwell et al. 1998), they likely benefit more from auditor assurance than do larger banks. Also, regulatory oversight likely focuses more on larger banks, leaving auditors a relatively more important monitoring role among the smaller banks. Thus, if differential reporting credibility underlies the main results, the observed differential changes between audited and unaudited banks should be more pronounced within the smaller bank subsample.

The test divides the full sample into five size categories that are typically applied by investors and bank regulators. Banks in the smallest group (with total assets below $25 million) are excluded to ensure that the sampled banks have sufficient ability to obtain managed liabilities. To increase banks' flexibility in audit decisions, the largest banks ($300 - 500 million) are also left out. Figure 2.3 shows the distribution of total assets for audited and unaudited banks in the full sample and in each of the three remaining size categories (A: $25 – 50 million; B: $50 – 100 million; and C: $100 – 300 million). In all three subsamples, the overlap in size between audited and unaudited banks improves considerably in comparison to that in the full sample. For example, average total assets are $37.2 million and $36.1 million for audited and unaudited banks in

size group A (in comparison to $90.5 million and $46.0 million in the full sample). Like in the main analyses, the test controls for the remaining size differences through the inclusion of *Ln(TA)*. The results are shown in columns (8), (9) and (10) of Table 2.5. As predicted, the positive impact of an audit is stronger among the smaller banks. To test this more formally, I pool banks from the three groups and further condition audited banks' policy responses on bank size. The coefficient on the sum of the three-way interaction terms *TightMP* × *Audited* × *Ln(TA)* is negative and significant at the five percent level in both the funding and loan regressions, consistent with the greater benefits for smaller audited banks (unreported).

2.5.7. Alternative proxies for monetary policy

To ensure the results are not sensitive to the choice of monetary policy measures, I repeat the tests using an alternate policy proxy: the spread between the federal funds rate (i.e., the overnight rate in the market for bank reserves) and the ten-year Treasury bond rate (*FF-Bond*). Different from the Boschen-Mills narrative index, which is developed based on a reading of the policy documents of the FOMC, *FF-Bond* captures policy stance through the effect of policy actions on money market variables.[33] Increases in spread correspond to a tighter policy. Thus, I set *TightMP*

[33] Bernanke and Blinder (1992) assess the reliability of *FF-Bond* versus other common interest-rate variables as an indicator of policy stance. The authors conclude that *FF-Bond* is relatively more reliable because (1) it tracks more closely the impact of policy actions on the supply of bank reserves, and (2) it is not sensitive to non-policy induced variations in the federal funds rate.

to one if the quarterly change of *FF-Bond* (*ΔFF-Bond*) is positive, and repeat the regressions in column (4) of Tables 2.3 and 2.4. As shown in Table 2.6, the results are largely similar for both tests of funding (column (1)) and lending (column (3)). Also, the main findings are robust to using *ΔFF-Bond* directly as the policy variable (columns (2) and (4)), as is typically applied in prior studies (e.g., Kishan and Opiela 2000).

2.6. More timely recognition of loan losses among audited banks?

The results so far show that audits have significant effects on banks' financing and investment. As important assumption in this analysis is that audited banks issue higher quality disclosure that gives them greater reporting credibility and reduced information asymmetry. However, studies on the differential reporting quality between audited and unaudited firms are very limited, and even fewer examine this issue using banks. Therefore, it is important to directly test the validity of this assumption to support the inference that the benefits for audited banks are attributable to more reliable disclosures.

Specifically, I test whether audited banks are associated with relatively more timely recognition of loan losses. Loan loss accounting is studied because lenders and depositors tend to pay special attention to potential losses (Watts and Zimmerman 1986). Also, because loan loss accounting requires a considerable degree of judgment based on the private information managers obtained in the lending process, it is an area particularly subject to reporting bias (Healy and

Whalen 1999) and the focus of regulatory oversight. Given such regulatory efforts, if I can still find positive evidence for auditor oversight in this reporting area, this can be taken as strong support for auditor reporting discipline in general.

To assess differences in the timeliness of loan loss recognition between audited and unaudited banks, I focus on the relations between loan loss provisions and changes in nonperforming loans. In particular, I apply the regression model below to test for differential timeliness of loan loss recognition between audited and unaudited banks:

$$\mathrm{LLP}_{it} = \alpha + \sum_{j=0}^{2} \beta_j \Delta\mathrm{NPL}_{it+j} + \gamma\ \mathrm{Audited}_{it} + \sum_{j=0}^{2} \delta_j \mathrm{Audited}_{it} \times \Delta\mathrm{NPL}_{it+j} + \zeta Control + \ldots\ldots + \sum_{j=1}^{50} \mathrm{o}_j\ \mathrm{State}_{ij} + \sum_{j=87}^{98} \pi_j\ \mathrm{Year}_{jt} + \varepsilon_{\mathrm{it}} \quad (2)$$

where the *i* and *t* subscripts denote the bank and year, respectively. LLP_t indicates the loan loss provision for year *t* divided by beginning of year total loans; ΔNPL_t indicates the ratio of nonperforming loans to total loans for year *t* minus the same ratio for year *t-1*; *Control* represents a set of control variables. *Audited*, *State*, and *Year* are indicators for audited banks, the state where the bank operates, and the reporting year, respectively. Details for variable measurement are given in Appendix B Panel D.

Loan loss provisions are accrued expenses that reflect estimates of future credit losses, subject to managers' forecasting ability and discretion. They thus indicate the timeliness with which banks recognize loan loss expectations in reported income. In contrast, nonperforming loans include all loans more than 90 days overdue on interest or principle. They are disclosed as sup-

plemental financial statement information. Because nonperforming loans are relatively free from managers' discretion, prior studies have used changes in nonperforming loans as the benchmark to gauge managers' incorporation of loan default information into current loan loss provisions (LLP_t) (e.g., Liu and Ryan, 1995; Nichols et al. 2009). If banks are timely in recognizing potential losses, there will be a positive relation between LLP_t and changes in nonperforming loans in current and future periods (i.e., ΔNPL_t, ΔNPL_{t+1}, and ΔNPL_{t+2}).

To compare the timeliness of loan loss provisions across audited and unaudited banks, each of the three nonperforming loan variables is interacted with the current period audit indicator ($Audited_t$). The main issue is whether the coefficients on these interaction terms (i.e., δ_0, δ_1, and δ_2) are positive, which indicates that current loan loss provisions for audited banks reflect more timely information about current and future credit losses relative to unaudited banks.

I include a number of control variables to account for differences in expected loan loss provisions across small banks. Prior studies find that LLP_t is positively related to the change in non-performing loans in the past year (ΔNPL_{t-1}), current and future net loan chargeoffs (NCO_t, NCO_{t+1}, and NCO_{t+2}), current loan growth ($Loan\ growth_t$), and equity ratio ($Equity_t$), but negatively related to past period loan loss allowance (LLA_{t-1}), and homogenous loans (HOM_{t-1}). Prior studies also find that LLP_t is related to firm size ($Ln(TA)_t$), but there is no conclusive evidence on the direction of the effect. To control for differences in economic condi-

tions across bank locations and time, I also include a set of state and year indicators. To save space, I relegate the details on the control variables to Panel D of Appendix B.

Like the audit prediction model, Equation (2) is estimated using annual data from 1987 to 1999.[34] After applying the same sample selection criteria as that reported in Table 2.1, 72,584 firm-years had the required data. Table 2.7 presents the summary statistics of the regression variables. Table 2.8 reports the regression results with t-statistics in parentheses computed using robust standard errors clustered by bank.

As reported in Table 2.8, Panel A, LLP_t for unaudited banks is significantly positively correlated with ΔNPL_t. However, there is no evidence that LLP_t for unaudited banks is significantly associated with ΔNPL_{t+1} or ΔNPL_{t+2}. These results indicate that unaudited banks' loan loss provisions are timely only relative to current but not future changes in nonperforming loans. In contrast, the results show that the coefficients on $Audited_t \times \Delta NPL_t$ and $Audited_t \times \Delta NPL_{t+1}$ are both positive and significant (t=3.5 and 2.9), indicating that audited banks are relatively more timely in recognizing current and future loan loss information. The signs and levels of significance for all the control variables are generally in line with prior studies.

[34] Prior research focuses on annual regressions because "the ability of quarterly changes in nonperforming loans to explain quarterly loan loss provisions in an OLS regression is very low" (Liu and Ryan 1995, 86).

To ensure that the associations between LLP and ΔNPL are not driven by differences in bank size or credit risk in loan portfolios between audited and unaudited banks, I interact each of the seven variables related to changes in nonperforming loans or net loan chargeoffs (i.e., ΔNPL_{t-1} to ΔNPL_{t+1}, and NCO_t to NCO_{t+2}) with each of $Ln(TA)_t$, $Loangrowth_t$, and $Equity_t$. As shown in column (1) of Panel B, the differential timeliness remains after adding these interaction terms. Further, focusing on banks that switched audit status during the sample period, I find more timely recognition of loan losses when banks are audited than when they are not. Similarly, the bank-fixed effects test studying only banks that had a constant audit status suggests that audited banks generally report more timely loan loss information. The same conclusion is also supported by the Heckman two-stage test,[35] and the instrumental variable regression.

2.7. Conclusion

Examining how accounting information affects firm investment process provides important insights into the effect of financial disclosure on real business activities and firm value creation. Recent research conjectures that financial reporting mitigates investment problems arising from information asymmetry and underfunding, but it does not provide direct evidence on this conjec-

[35] The inverse Mills ratio is added as a new control variable, along with those in Equation (2). To allow its coefficient to vary between audited and unaudited banks, an interactive variable (*Audited*IMR*) is also included.

ture (Biddle et al. 2009). To address this gap in the literature, I exploit exogenous bank liquidity shocks induced by monetary tightening, and test if small non-public banks with an optional audit are affected differently. In essence, I hypothesize and find that audited small banks disclose higher quality accounting information that lends them greater reporting credibility than unaudited banks. Consistent with the decreased information uncertainty for audited small banks, these banks face relatively lower financing frictions when they obtain new external funding to counteract sudden liquidity outflows. In turn, their lending is relatively less responsive to monetary tightening. These results are notable because the added credibility provided by audited financial statements is incremental to the effect of the monitoring by bank regulators. Overall, the findings demonstrate how banks can benefit from having audited financial reports. At a broader level, these findings reliably support the link between the financing channel and investment decisions. The articulation of this channel helps establish the casual effect of accounting disclosure on firm investments.

Chapter 3: Do distortions in bank loan supply affect borrowers' voluntary disclosure? Evidence from international propagation of banking shocks

3.1. Introduction

As reported in a large number of economic studies,[36] and also forcefully demonstrated in the recent subprime mortgage crisis, distortions of bank loan supply can restrict borrowers' ability to fund regular operations and even threaten their survival. Less understood are the measures borrowers apply to mitigate these negative consequences. Even less is known about borrowers' reactions through financial disclosures. However, if voluntary disclosures can increase firms' access to alternate external funding sources, particularly the public capital markets, then constrained borrowers have incentives to adjust disclosure policies to facilitate capital raising to substitute for previously available loans. In this paper, I examine whether and how borrowers change voluntary disclosure decisions in response to adverse shocks to loan supply. This investigation is important as it provides timely insights into the financial reporting actions firms take to relax costly capital constraints. Additionally, the results help connect the large disclosure literature in accounting with the sizable stream of economic research that examines banking shocks and their implications for bank borrowers.

[36] These studies include, for example, Gibson (1995), Kang and Stulz (2000), Peek and Rosengren (2000), Hubbard et al. (2002), Gan (2007), Almeida et al (2009), Duchin et al. (2010), and Chava and Purnanandam (2011).

Studying the disclosure impact of bank loan supply disruptions is also motivated by the continuing calls from different researchers for refined investigations to demonstrate the public capital market transaction motives for voluntary disclosure (e.g., Healy and Palepu 2001; and Beyer et al. 2010). Empirical studies that attempt to show such disclosure motives face a critical identification challenge of controlling for omitted factors correlated with firms' decision to raise external financing. As pointed out by both Healy and Palepu (2001, 427) and Beyer et al. (2010, 306) in their reviews of the disclosure literature, debt and equity offerings are likely to occur when firms have new investment opportunities. Because investment opportunities can independently induce voluntary disclosures in the absence of securities offerings, it is difficult to control for this confounding effect and attribute managers' disclosure strategies around capital market transactions to the issuance of public securities. This identification problem would be avoided if researchers could randomly assign public financing needs to firms. However, because such experiments are infeasible, a close substitute is to find settings where firms' desire for public funding has no clear relation with new investment projects. This practically excludes all public financing activities driven by firms' *demand* for additional capital, although prior studies have typically used this research setting despite its limitations to answer this important research question (e.g., Ruland et al. 1990, Frankel et al. 1995; Marquardt and Wiedman 1998; and Lang and Lundholm 2000). On the other hand, changes in public funding motives induced by shocks to

supply of private capital are potentially more fruitful. As such, I examine whether unexpected disruptions of loan supply affect borrowers' voluntary disclosure.

The identification strategy exploits the tightening of bank loans to U.S. borrowers as a result of a series of emerging-market financial crises in the late 1990s (e.g., the Asian crisis of 1997 and the Russian crisis of 1998). These events were plausibly external to the U.S. markets, yet their effect was transmitted through U.S. banks' large loss exposures to the crisis areas which ultimately limited exposed banks' lending back in the U.S. (Chava and Purnanandam 2011). Thus, these events created significant contractions in loan supply that were relatively unconnected with *U.S. borrowers'* business plans. In turn, this setting allows researchers to identify firms with increased motivations to access the public capital markets *for reasons separate from new investment opportunities*, and conduct direct tests of corresponding changes in these firms' disclosure policies. These financial crises provide a natural experiment because the events affected only a subset of U.S. banks that had substantial exposures to the crisis areas (e.g., Kho et al. 2000). Borrowers of banks with minimal exposures were therefore relatively unaffected, and they could be used to reliably control for concurrent economic changes that may confound inferences.

Turning to the form of voluntary disclosure to examine, I follow prior research and study management forecasts (e.g., Frankel et al. 1995). These forecasts provide outside investors with

direct inputs for firm valuation and are expected to be effective in reducing information asymmetry (e.g., Coller and Yohn 1997). As such, I predict that borrowers have greater incentives to issue management forecasts when loan supply decreases. The sample includes 1,111 U.S. borrowers. As predicted, in the two-year crisis period beginning in 1997:Q3, compared with the two years before, both the tendency to make forecasts and the number of forecasts issued increased significantly more for borrowers of exposed banks than for other borrowers. This differential disclosure change is accompanied by an increase in public financing desire for exposed bank borrowers, who substituted significantly more public capital for bank loans over the same period (Figure 3.2).

One possible concern with my tests is that unique borrower characteristics (rather than public financing motives) might somehow influence borrowers of exposed banks to be more forthcoming over time than other borrowers. I perform two additional tests to address this concern. First, I conduct further cross-sectional analyses *within* the sample of borrowers of exposed banks, and show that those subject to greater loan tightening had a greater disclosure increase in the crisis period. Specifically, the disclosure increase is more pronounced for borrowers more likely to be credit rationed, either because their banks had particularly large exposures to the crisis events, or due to their own credit risk characteristics (i.e., company has low availability of collaterals or engages in risky projects). These differential treatment effects within the borrowers of

exposed banks reinforce the main results and mitigate concerns about confounding firm characteristics.

Second, if there were truly some confounding factors causing a general disclosure increase over time for the borrowers of exposed banks, I should observe the differential disclosure increase to continue independent of credit conditions. However, results indicate a weakening forecast tendency for borrowers of exposed banks relative to other borrowers when the crisis events subsided. Moreover, in other periods when we do not expect differential tightening (i.e., the lack of treatment), I find no evidence for the predicted differential forecast increases.

Having demonstrated borrowers of exposed banks increased their forecasting activity in the crisis period, I then examine how their forecast characteristics changed to further understand the disclosure motives. If the forecast increase is driven by managers' desire to ease access to external financing, I expect to find changes in the forecast characteristics that would lower information asymmetry. The analyses focus on the three most important forecast characteristics: (1) forecast horizon, (2) precision, and (3) news content (Hirst et al. 2008). First, relative to other borrowers, those of exposed banks significantly increased their forecast horizon. At the same time, they were less likely to compromise forecast precision even though factors such as longer forecast horizon or increased market volatility in the crisis period would otherwise cause firms to issue less precise forecasts. These results are supportive of managers' use of forecasts to lower

investor uncertainty, which is likely to be more effective if the forecasts are more timely and precise. Finally, if managers use forecasts to mitigate information asymmetry, then those forecasts would not depend on whether the manager has positive or negative private information (e.g., Marquardt and Wiedman 1998). I thus expect borrowers of exposed banks to issue both good and bad news forecasts, even though firms seeking financing have incentives to disclose selectively to "hype" stock prices before potential security offerings (Lang and Lundholm 2000). Consistent with this expectation, I find no evidence that the tendency to issue good news (or withhold bad news) increased more for borrowers of exposed banks. This last result should not be surprising given the high litigation risk associated with selective disclosure.

Collectively, the forecast increases and the changes in forecast characteristics for borrowers of exposed banks provide new evidence on the motives for the disclosure of forward-looking information. This is important because endogeneity issues often cloud the interpretation of the evidence in existing studies. Yet, clear understanding of managers' disclosure incentives is key to understanding different fundamental issues such as the forces shaping firms' information environment, the need for regulating corporate disclosure in capital markets, and what types of disclosure should be mandated (e.g., Healy and Palepu 2000, 412).

In addition to complementing and strengthening prior research on managers' disclosure incentives in general, this paper contributes to the important literature on the relation between

disclosure and the cost of capital. Specifically, by investigating borrowers' reporting practices in periods of tight loan supply, this paper offers new evidence that managers act as if increased disclosure enhances their ability to raise capital in public financing markets in terms of increased amounts of funding, or reduced cost of capital, or both. If managers have rational expectations, their actions correspond to equilibrium outcomes.

Finally, this paper also contributes to the emerging research on the relation between macro-economic environment and firms' disclosure practices. Existing studies have examined how voluntary disclosures are shaped by macro information environment (Baginski et al. 2008), and investor sentiment (Bergman and Roychowdhury 2008). More recently, Kim et al. (2010) show that macroeconomic uncertainty reduces management forecast activities. This paper extends this literature by studying firms' disclosure responses to macroeconomic shocks to capital supply. To this end, the results provide timely insights into the financial reporting actions firms take to mitigate the negative impact of bank loan disruptions.

The remainder of the paper is organized as follows. Section 3.2 describes the crises, reviews relevant disclosure studies, and develops the testable hypothesis. Section 3.3 presents the empirical design. Section 3.4 provides sample selection procedures, descriptive statistics, and empirical evidence. Section 3.5 examines borrowers' forecast characteristics. The final section concludes.

3.2. Background, prior research and hypothesis development

This section provides the background information for the research setting. As discussed below, the crisis events significantly negatively affected the willingness or the ability of exposed U.S. banks to supply loans. In turn, these banks' clients, facing tightening borrowing conditions, were motivated to raise capital from alternate (public) financing sources in order to avoid costly capital constraints. Thus, I conjecture that, as the bank loan supply decreases, constrained borrowers have greater incentives to make voluntary disclosure to reduce information asymmetry to facilitate access to public capital markets.

3.2.1. The emerging-market financial crises

A dramatic series of financial crises struck different emerging-market countries in the last quarter of the 1990s. The first in this series of events occurred on July 2, 1997, when the Bank of Thailand announced its decision not to defend the Thai baht's peg to the U.S. dollar. The baht promptly lost its value. Massive capital outflows from the country ensued and stock market plunged severely (Eichengreen 2003). In the following months, similar financial catastrophes hit other Asian countries including Indonesia, Malaysia, the Philippines, and South Korea. The Asian markets remained turbulent into 1998, setting the stage for the later collapses of debt-ridden countries in other regions (Desai 2003). In August, 1998, Russia surprised the markets by devaluing the ruble and at the same time unilaterally suspending payments on most of its debts.

As the decline of the Russian economy fatally eroded investor confidence in emerging markets, the consequent turmoil swiftly spread to Latin America (e.g., Weidner 1998).[37] In face of continued capital outflows and downward pressure on the Brazilian real, the Brazilian authorities allowed the real to devalue on January 18, 1999. Further financial uproar followed.

As these financial crises evolved, some U.S. banks were subject to substantial losses through (1) exposure to sovereign debt owed by crisis countries, and (2) exposure to the private borrowers in these countries.[38] As highlighted by Kraus (1998, 1), "[t]he exposure is concentrated in a handful of large U.S. banks whose capital could quickly erode if sizable write-offs were required. That, in turn, could tighten domestic lending…." Consistent with the exposure concentration, Palmer (2000, 91) notes that, over the 1997 – 1999 period, a small number of large money center banks consistently accounted for about 80 percent of the claims of U.S. banks on emerging-market counterparties.[39] For these banks, emerging-market claims as a percentage of tier 1 capital exceeded 225 percent right before the onset of the Asian crisis. Because of the large loss potential, exposed U.S. banks lost significant market value in the midst of the emerging-

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[37] Other noteworthy events associated with the Russian crisis include the abrupt failure of the hedge fund Long-Term Capital Management (LTCM).

[38] There are other suggested reasons for U.S. bank losses, such as exposures to currency and derivative losses, and losses on brokerage credit to Long-Term Capital Management (LTCM) (Chava and Purnanandam 2011).

[39] The aggregate claims of U.S. banks on emerging-market counterparties were close to $200 billion right before the onset of the Asian crisis (Palmer 2000, 83).

market crises (Kho et al. 2000; Gatev et al. 2006; and Fissel et al. 2006). For example, stock prices for money center banks fell more than one-fourth in the aftermath of the Russian debacle, and did not return to their previous highs until 2002. Also, the default premiums on large bank subordinated debt increased rapidly in 1998 and remained higher than the pre-crisis level through the second half of 1999 (Fissel et al. 2006). Accounting-based performance measures also indicate the deterioration of bank health. Relative to the pre-crisis period, U.S. banks made significantly greater charge-offs on loans to foreign borrowers and suffered large related losses in 1997 – 1999 (FDIC Quarterly Banking Profile 1998a, 1998b; Bomfim and Nelson 1999; and Chava and Purnanandam 2011).

Given the large loss potential, exposed U.S. banks had to actively reduce risk-taking and find ways to bolster their capital over the entire crisis period (Palmer 2000). In turn, their willingness and ability to supply loans to domestic borrowers was directly compromised.[40] Chava and Purnanandam (2011) show that, relative to the two years before the 1998 Russian crisis, exposed U.S. banks decreased domestic lending volume and increased loan spreads substantially more than other U.S. banks after the crisis. These results are consistent with the findings in a

[40] A deterioration in bank-health can decrease the supply of bank loans for at least three related reasons: (1) a direct reduction in loanable funds at the affected banks; (2) poor financial prospects can limit affected banks' ability to obtain external capital to support lending activities; and (3) a lower risk-appetite (e.g., due to capital adequacy constraints) can lead banks to adjust their asset-mix in favor of relatively less risky investments.

special Senior Loan Officer Opinion Survey conducted by the Federal Reserve in September 1998.[41] The survey's aim was to assess the effect of the ongoing emerging-market financial crises on the U.S. bank loan market. Citing a reduced tolerance for risk, more than a quarter of large domestic bank respondents reported tightening of business lending standards. The tightening was mainly applied to large firms. Not only did loan approval become more difficult, in cases where firms were able to obtain a loan, loan terms (such as fees, loan spreads, collateralization requirements, and the use of loan covenants) became more onerous.

Subsequent editions of the Senior Loan Officer Opinion Survey show that some large U.S. banks continued to impose tougher lending standards for large business loans. Indicative of the strength of these actions, data from the Federal Reserve's quarterly Survey of Terms of Business Lending show that the average loan spread on new business loans started to widen from the onset of the 1997 Asian crisis. Following the Russian crisis, the average loan spread reached a level that had only last been seen in the savings and loan crisis of the 1980s and early 1990s (See Figure 3.1). Similarly, the share of loan originations secured by collateral in 1999 stayed close to levels near the top of the historical range (Basset and Zakeajsek 2000). Notice that these loan data were collected from a large group of banks including banks exposed and unexposed to the

[41] The survey is available at www.federalreserve.gov/boarddocs/snloansurvey/, retrieved August 15, 2010.

crisis areas.[42] However, it is primarily the exposed banks that had tightened lending standards (Chava and Purnanandam 2011). Hence, borrowers of exposed banks likely faced even more difficult loan access in the crisis period than what the lending surveys indicate.

To avoid the costly consequences of capital constraints, restricted borrowers need to find alternate capital sources, including the public capital markets. Consistent with this observation, in its 1999 annual review of developments in the U.S. banking industry, the Federal Reserve notes that bank loans as a share of non-mortgage credit market debt owed by nonfinancial businesses declined in the midst of the crisis events, as constrained borrowers fulfilled more of their financing needs by issuing public debt such as commercial paper and bonds (Basset and Zakeajsek 2000). Similarly, Chava and Purnanandam (2011) emphasize that the public debt market functioned at reasonably normal levels in the crisis period and provided constrained borrowers with important capital substitutes.[43]

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[42] The Survey of Terms of Business Lending is based on data from a representative sample of up to 348 domestically chartered commercial banks and up to 50 U.S. branches and agencies of foreign banks. The sample data are used to estimate the terms of loans extended during the survey period at all domestic banks and at all foreign-related institutions. The data are available at www.federalreserve.gov/releases/E2/, retrieved August 15, 2010.

[43] Consistent statistics can also be found in Eckbo et al. (2007, 252). Studying the annual frequency of security offerings in the U.S. public capital markets over the period 1980 – 2003, Eckbo et al. report a sudden surge in the number of public debt issuance from 1997 and afterwards. That number did not move back to the pre-crisis level until the early 2000s, consistent with firms changing their financing mix to adjust to restricted bank loans during the crises events.

More direct evidence on the substitution between public financing and bank loans comes from studying separately the changes in external financing by borrowers of exposed banks and other borrowers over time. Based on my sample firms, Figure 3.2 shows individually the new issuances of public financing and bank loans by the two borrower groups during the pre-crisis period (1995:Q3 to 1997:Q2) and the crisis period (1997:Q3 to 1999:Q2). Total bank loans obtained by borrowers of exposed banks decreased by about 31 percent from $285.8 billion to $198.2 billion in the crisis period. On the other hand, public financing (including public debt and equity) increased by 54 percent from $105.7 billion to $162.8 billion. Thus, public financing (especially public debt) served as an important substitute for bank loans for borrowers of exposed banks during the crisis period. In contrast, other borrowers had fairly stable financing pattern over time, which is consistent with their relatively undistorted financing motives.

The observation that constrained borrowers turn to the public capital markets as bank loan supply becomes limited is not new and has been reported in different prior studies (e.g., Kashyap et al. 1993; Leary 2009). What is unknown is whether and how firms adjust voluntary disclosures to mitigate potential information asymmetry in the public capital markets. Findings of such changes will shed light on the disclosure actions firms take to alleviate costly capital constraints. Further, the results will present new evidence for the public capital market transaction motives for voluntary disclosures. Below I briefly review the relevant studies.

3.2.2. Related disclosure studies and hypothesis development

Theoretical work suggests a negative association between disclosure and the cost of capital (e.g., Amihud and Mendelson 1986; Diamond and Verrecchia 1991).[44] Separately, a large finance literature posits that a substantial portion of both (1) the underpricing in the equity and public debt markets, and (2) floatation costs (e.g., underwriting fees; probability of withdrawals) are attributable to asymmetric information (Eckbo et al. 2007). To the extent that voluntary disclosure reduces such asymmetry, it lowers the cost of raising capital. Based on these findings, prior research predicts and finds that firms in anticipation of public financing activities have increased voluntary disclosures. For example, firms seeking external financing are more likely to voluntarily disclose earnings forecasts than other firms (e.g., Ruland et al. 1990; Frankel et al. 1995). While Lang and Lundholm (2000) do not find an increase in forecasts prior to equity issuances, they report increases in all other types of discretionary disclosures. There are also studies indicating that firms with increased analyst ratings of disclosures are associated with increased subsequent sale of public debt (e.g., Healy et al. 1999).

[44] For example, Amihud and Mendelson (1986) argue that the cost of equity capital is higher for securities with wider bid-ask spreads because investors demand a higher return to compensate for added transaction costs. Increased disclosure reduces the adverse selection component of the bid-ask spread, thereby reducing the cost of capital.

While several prior studies suggest a public financing motive for voluntary disclosures, serious correlated omitted factors limit the ability of researchers to draw a strong conclusion. Specifically, both Healy and Palepu (2001, 427) and Beyer et al. (2010, 306) caution that new investment opportunities that often underlie firms' decisions to make public offerings can confound inferences.[45] Supportive of their arguments, prior studies indicate that new investment opportunities can independently induce voluntary disclosures in the absence of public capital market transactions. For example, studying firms with no public financing activities, Miller (2002) reports that firms significantly increase voluntary disclosures when they expect growth opportunities and good earnings performance.

This identification problem is basically a result of (unobserved) new investment opportunities driving firms' *demand* for additional outside capital. To avoid this problem, I focus on changes in public funding motives induced by sudden interruptions in bank loan *supply*. Because the economic shocks exploited here were plausibly external to the U.S. loan markets, yet linked through U.S. banks' large loss exposures to the crisis areas, these events are exogenous declines in loan supply that are reasonably uncorrelated with *borrowers'* new business plans. The conse-

[45] For instance, Healy and Palepu (2001, 427) argue that "firms that have public capital market transactions are also likely to be facing changes in their investment opportunity sets. It is then difficult to assess whether the relation between high levels of disclosure and increases in disclosure for these firms is attributable to the public issue per se."

quent increase in constrained borrowers' desire to use public financing in turn enables sharper tests of voluntary disclosure responses with fewer confounding factors.

To support the argument that the crisis events were largely external to the U.S. markets, Figure 3.3 plots two primary economic indicators over the period 1995:Q3 – 2000:Q2. Panel A shows the quarterly percent change in real gross domestic product (GDP). Panel B depicts the quarterly change in the number of nonfarm payroll jobs. Relative to the pre-crisis period, the pattern of GDP growth in the crisis period was broadly similar. Likewise, there were no significant changes in the employment conditions. Thus, the U.S. economy-wide conditions were rather stable during the sample period.

Next, I provide evidence that the declines in loans and the consequent increase in the public financing motives in the crisis period for borrowers of exposed banks (see Figure 3.2) were largely unrelated to new business opportunities. As depicted in Panel A of Figure 3.4, growth opportunities (proxied by market-to-book ratio) were trending closely for both borrowers of exposed banks and other borrowers throughout the sample period. Similarly, quarterly buy-and-hold size-adjusted returns, a common measure for value-relevant news, were moving in tandem for the two groups. As such, differential changes in investment opportunities were unlikely the driving force behind the greater increase in public financing desire in the crisis period for borrowers of exposed banks. Thus, this setting provides a natural experiment for examining the

public capital market transaction motives for voluntary disclosure. A greater increase in disclosure for borrowers of exposed banks is consistent with such motives. The prediction is summarized in the following hypothesis (stated in alternative form):

> *H1: Borrowers of exposed U.S. banks increase their disclosure in periods of uncertain loan supply relative to the change in other borrowers' disclosure over the same period.*

As mentioned before, my tests focus on a high profile form of disclosure that provides outside investors with direct inputs for firm valuation: management forecasts. Thus, I predict a greater increase in the forecast likelihood for borrowers of exposed banks in the crisis period relative to other borrowers. This prediction does not differentiate the different types of forecasts (e.g., long-run versus short-run forecasts, or quantitative versus qualitative forecasts), but focuses on managers' use of forecasts in general to reduce information asymmetry, which is similar to the focus of closely related prior studies (e.g., Marquardt and Wiedman 1998). In additional tests detailed in Section 3.5, I further assess how borrowers' forecast characteristics changed to provide new insights into the impact of capital market considerations on the nature of the forecast issued.

Before turning to the next section for empirical model specification, two points are worth noting. First, in counteracting loan tightening, constrained borrowers might also have greater incentives to use non-public capital substitutes, such as internal funds, loans from a different bank, nonbank private debt, and private equity. The possibility of these alternate substitutes

should not confound my results. Specifically, because these other capital sources are all private in nature, changes in the need to use these financing sources do not predict increases in borrowers' incentives to disclose forward-looking information to the public.[46]

Second, heightened litigation concerns around financing events can deter firms from increasing disclosure. Consistent with these concerns, Frankel et al.'s (1995) investigation of disclosure around financing transactions and Lang and Lundholm's (2000) study of disclosure around seasoned equity offerings both indicate no increase in the number of earnings forecasts shortly before securities offerings.[47] While the legal risk of disclosing forward-looking information in the 1980s and early 1990s (the sample periods used in prior studies) was much higher than that in my sample period, partly due to the safe harbor provision of the Private Securities Litigation Reform Act of 1995 (Johnson et al. 2001), remaining legal concerns (and other disclosure costs such as proprietary costs) can dampen the predicted forecast changes in H1.

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[46] Given the different alternate capital sources, some readers might wonder why constrained borrowers did not rely exclusively on the private capital sources. A plausible reason is that these substitutive sources could not fully fulfill borrowers' funding needs. Since the investment decisions of private investors tend to be relationship-based, before a strong relationship deepens over time, limited capital supply from new investors is expected.

[47] On the other hand, Marquardt and Wiedman (1998) report that managerial participation in a secondary stock offering is positively associated with voluntary disclosure of earnings forecasts. While Marquardt and Wiedman (1998, 521) study any earnings forecasts up to nine months before the registration date of the equity offering, the majority of the forecasts they identified occur between one and three months before registration.

3.3. Empirical model specification

3.3.1. Potential identification issues and remedies

While the loan supply shock setting offers distinct identification advantages, a remaining issue requires further explanations. Specifically, I use firms' choice of main bank before the crisis events to identify borrowers more subject to the loan tightening. If there is reason to believe that firms jointly determine the choice of capital suppliers and their future disclosure decisions, then this endogeneity would confound my analyses. However, since the crisis events were largely unexpected,[48] it is very unlikely that firms would be able to fully anticipate the future shocks and plan subsequent management forecast changes when they first chose their main bank. As such, this source of endogeneity is unlikely an important issue.

Perhaps a bigger concern is that some omitted factors affecting the pre-crisis banking relationships are correlated with borrowers' future disclosure changes. In evaluating the validity of this concern, note that banking relationships generally take a long time to develop and tend to be sticky over time (e.g., Bharath et al. 2009). It is thus unclear that banking relationships initiated

[48] As noted by Desai (2003, 87), "[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."

and nurtured years prior to the crisis events would be strongly associated with events that predict *changes* in borrowers' forecast decisions following the crises.

Nonetheless, to ensure that the results are not driven by confounding factors, I employ a number of other research strategies. First, the main tests control for firm fixed effects. This nets out the impact of all time-invariant factors that could affect both firms' choice of lenders over time as well as their disclosure responses to tighter loan supply. Second, in further tests reported in Section 3.4.4, I exploit the cross-sectional differences in treatment effects *within* the group of borrowers of exposed banks to examine to what extent the degree of tightening influences forecast propensity. Since these tests use the subsample of exposed bank borrowers only, the analyses should not be affected by any selection issues arising from relationships with an exposed bank. Third, in Section 3.4.5, I examine the variation in treatment effect over time. If the hypothesized effect holds, then there should be weakening forecast tendency for borrowers of exposed banks when the crises subsided. Furthermore, in other time periods when we do not observe differential loan tightening, there should be no significant difference in forecast changes for exposed bank borrowers versus other borrowers. As such, if confounding factors exist, they must explain why their effects show up only among exposed bank borrowers in periods when loan supply was constrained. Finally, I also examine changes in exposed bank borrowers' forecast characteristics to see if these changes are consistent with firms using management forecasts to

reduce information asymmetry (see Section 3.5). Any confounding factors must also explain these related changes.

3.3.2. Empirical model

To test the effect of bank loan supply distortions on the probability of management forecast issuance, I use the following logit regression:

$$Pr(MF\_DUM_{it} = 1) = \alpha_i + \beta\ Crisis_t + \gamma\ Crisis_t \times ExpoMBK_i + \delta\ Controls_{it-1} + \varepsilon_{it} \quad (1)$$

where MF_DUM_{it} is an indicator variable that equals one if borrower *i* issues at least one management forecast during quarter *t*. α_i denotes firm-fixed effects. $Crisis_t$ is an indicator variable set to one for the financial crisis period (1997:Q3 – 1999:Q2). The pre-crisis period (1995:Q3 – 1997:Q2) is used as the benchmark. The indicator variable $ExpoMBK_i$ identifies whether borrower i's main bank was exposed to the events. Because $ExpoMBK_i$ is time-invariant for each firm, its main effect is subsumed by the firm fixed effect (see below). Thus, I do not include $ExpoMBK_i$ separately in the model. $Controls_{it-1}$ is a set of firm-specific time-varying control variables. As explained below, the effect of adverse loan supply shocks on management forecasts is captured by the coefficient of the interaction term (γ).

Since I control for firm-fixed effects, identification of main parameters comes from comparing forecast activities for the *same* firm before and after the crisis events. That is, I can use

each firm as its own benchmark to adjust for all time-invariant disclosure determinants (and those that change slowly over time), including industry membership, business complexity, corporate governance and ownership structures, disclosure costs such as litigation exposure, and analyst following etc.[49] Importantly, to the extent that there were to be a set of firm-specific omitted factors consistently driving the firm's choice of capital providers, and that such factors affect the firm's disclosure responses to the future loan supply shocks, controlling for firm-fixed effects would also control for these correlated omitted factors. Such confounds may include management style or managers' ability to negotiate new funding sources.

While the regression controls for all sources (i.e., observed or unobserved) of time-invariant factors, time-varying factors that affect disclosure could still confound inference. There are two types of such factors: (1) systematic, and (2) firm-specific time-varying variables. Regarding the systematic ones (e.g., changes in general economic uncertainty and investor sentiment), focusing on shocks originated in emerging markets already implicitly "holds" the business conditions of the U.S. economy relatively stable over time (See Figure 3.3).

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[49] Prior research emphasizes the benefits of using each firm as its own control. For example, Miller (2002, 178) highlights that "this methodology eliminates the need for assumptions regarding the determinants of the untreated level of disclosure." To see why this is useful, consider the following two firms in my sample: Systems & Computer Technology Corp., and Max & Erma's Restaurants. Due to different operating complexity, as well as business environments, the two firms likely differ significantly in their existing disclosure practices. As such, if the test does not use each firm as its own benchmark, it would need an extensive list of controls for across-firm variations in the baseline disclosures, along with assumptions regarding the functional form these controls take.

To explicitly control for more subtle changes in macroeconomic trends, I exploit the fact that some borrowers (whose main bank had a minimal exposure to the crisis areas) were relatively unaffected by the loan supply changes. Suppose firms are all subject to similar macroeconomic factors, the changes in forecast behaviors following the crisis events for unaffected borrowers (captured by coefficient β) reflect concurrent changes in macroeconomic environment. Accordingly, the *differential changes* in forecast tendency for borrowers of exposed banks (captured by coefficient γ) could be attributable to tighter loan supply. A significantly positive coefficient γ would be consistent with hypothesis H1.

Regarding firm-specific time-varying variables, Figure 3.4 has already shown that differential changes in investment opportunities are unlikely a reason for any differential disclosure changes for borrowers of exposed banks. Nonetheless, to explicitly control for unknown sources of confounding within-firm changes, I include different firm-specific time-varying disclosure determinants that have been commonly used in prior studies (*Controls*). These include: (1) growth opportunities, as captured by the market-to-book ratio (*MTB*), and the growth of total assets (*ATGROWTH*); (2) firm performance, as captured by return on assets (*ROA*), the occurrence of losses (*LOSSDUM*), buy-and-hold size-adjusted returns (*BHAR*), and the occurrence of business restructuring (*RESTRUCT*);[50] and (3) risk and uncertainty, as captured by stock beta

[50] Table 2 shows that *RESTRUCT* has very little variation across firms or over time. Hence, it drops out of the tests.

(*BETA*), operating cash flow volatility (*CFVOL*), and return volatility (*RETVOL*). I also include the natural logarithm of market value (*Ln(MV)*) to reduce concerns due to changes in firm size. The details of variable measurement are given in Appendix E. In the next sub-section, I provide further information on the main explanatory variables of interest in Equation (1).

3.3.3. Variable of interest: the financial crisis period ($Crisis_t$)

I use a dummy variable $Crisis_t$ to indicate the crisis quarters. The crisis period begins when the Asian events started in 1997:Q3. As noted by Desai (2003, 172), "[t]he economies of the Crisis Five in East Asia and of Russia and Brazil revived in 1999 and 2000 leaving behind them the worst consequences of capital outflows and plunging currencies." Thus, the crises covered about two years; I define the end of the crisis period as 1999:Q2, before the recovery of the areas began. For the benchmark period, I use the two years before the first crisis in Thailand from 1995:Q3 to 1997:Q2. The disclosure impact of the crisis events can then be gauged by the changes in forecast behaviors over the two periods. See Figure 3.5 for the timeline.

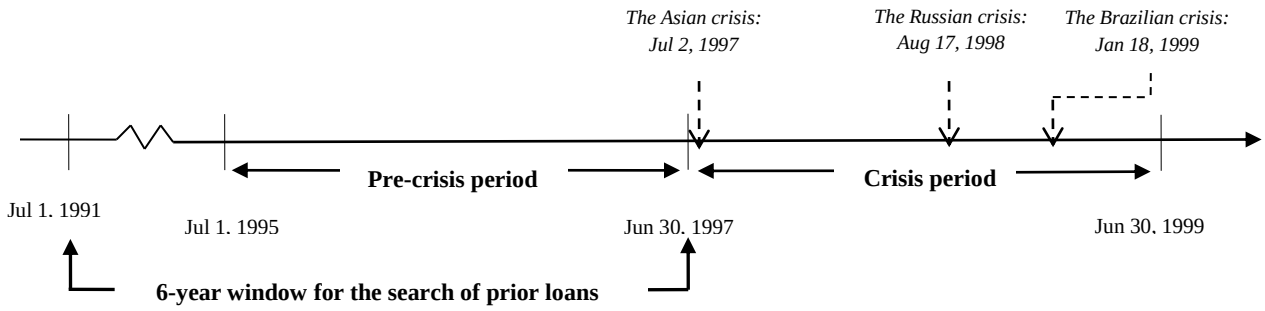

Figure 3.5 Timeline

3.3.4. Variable of interest: a firm's main bank, and its exposure to the crises ($ExpoMBK_i$)

I refer to a firm's main bank as the firm's largest loan supplier, and identify the main bank using loan information in the pre-crisis period. The identification process follows prior research on relationship lending (e.g., Bharath et al. 2009). First, for each firm, I search all past loan deals over a six-year window that ends in 1997:Q2 (see Figure 3.5). This window is chosen because a majority of the loans contracted before the crisis events (more than 85 percent) had maturities of less than or equal to six years. Hence, this window captures most loans made immediately before the events, and ensures that the banking relationships were active.

Next, for each of these prior loans, I record the name of the lead bank. Sole lender transactions by construction have an identified lead bank. For loans by a syndicate of lenders, I look for the "lead arranger". According to Sufi (2007, 629 - 630), "[t]he 'lead arranger' establishes a relationship with the firm, negotiates terms of the contract, and guarantees an amount for a price range. The lead arranger then turns to 'participant' lenders that fund part of the loan." Typically, the lead arranger holds a substantially greater share of the loan than any of the participants. Thus, the lead arranger is potentially the borrower's main bank.

Finally, for a given borrower *i*, if all of its prior loans were from the same lead bank,[51] this bank is the main bank. But if the loans were from different lead banks, the main bank is the one upon which borrower *i* relied most for loans. To measure the financing reliance of borrower *i* on a particular lead bank *k*, I compute the following ratio:

$$LoanReli_{ik} = \frac{Total\ loan\ amount\ firm\ i\ borrowed\ from\ bank\ k\ in\ the\ 6-year\ window}{Total\ loan\ amount\ firm\ i\ borrowed\ in\ the\ 6-year\ window}$$

When I aggregate the amount of prior loans from a bank, I also include loans provided by the bank's predecessors if the bank was the result of mergers and acquisitions.[52]

To illustrate how to identify the main bank using *LoanReli*, suppose there are two prior loans for borrower *i*: one of $700 million from lead bank *m*, and another of $100 million from lead bank *j*. $LoanReli_{im}$ is thus 0.875 (= 700/ (700+100)), and $LoanReli_{ij}$ is 0.125. Since loans of borrower *i* are more concentrated with bank *m*, bank *m* is treated as its main bank.

Note that *LoanReli* gives the lead bank full credit for the total amount of a particular prior loan even though the loan could be funded by a syndicate of lenders.[53] The reason for not using

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[51] Loans granted by banks within the same banking group are treated as loans from the same lead bank. For example, Wells Fargo is the lead bank for loans from either Wells Fargo Texas, or Wells Fargo Iowa.

[52] For example, Bank of America acquired Continental Bank in 1994. If a firm had borrowed from Continental Bank in 1992, and from Bank of America in 1996, the two loans are all treated as loans from Bank of America.

[53] In rare cases, there are multiple lead arrangers for the same syndicated loan. Suppose there are N lead arrangers, I retain all of them as the lead banks of that loan. Each bank then gets credit for (1/N)th of the loan amount.

the exact dollar amount lent by the lead bank is that this information is often unavailable. To the extent that this compromise excludes other banks that also have a large loan commitment to borrower *i*, it will likely bias against finding the predicted results. This measurement error occurs because the borrower can readily increase borrowing from another bank to accommodate unexpected changes in the conditions of loan supply, but this information is not recognized in the tests.

Once the main bank's identity is found, I use an indicator variable to indicate whether or not it was exposed to the crisis events. The exposure information is provided by Kho et al. (2000), who analyzed the annual reports of 78 large U.S. banks covered in the Datastream database and identified exposed banks on the basis of a search of disclosed material exposures to a given crisis event.[54] This list of exposed banks is consistent with those indicated in other sources. For example, their list of exposed banks includes those banks that reported in the "Money Center Banks" category on the 1997 Country Exposure Reports of the Federal Financial Institutions Examination Council.[55] As mentioned before, emerging-market claims as a percentage of tier 1

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[54] Kho et al. (2000) further show that the stock performance of their exposed banks was significantly negatively affected by the crisis events. On the other hand, other banks were largely unaffected.

[55] The report is available at: http://www.ffiec.gov/e16.htm, retrieved August 15, 2010. There are six banks comprising the "Money Center Banks" category, including BankAmerica, Bankers Trust, Chase Manhattan, Citicorp, First Chicago NBD, and J.P. Morgan. Kho et al. (2000) do not provide exposure information for First Chicago NBD, as the bank had already been acquired at the time when the authors collected their data. I update their classifications and include First Chicago NBD as one of the exposed banks.

capital for these banks exceeded 225 percent right before the onset of the Asian crisis. Further, Chava and Purnanandam (2011, 130) find that Kho et al.'s classification is highly correlated with alternate exposure measures (over 80%), such as quarterly charge-offs on loans and leases made to foreign borrowers. Thus, I use Kho et al.'s classification and set the exposed bank indicator (*ExpoMBK*) to one if the main bank had identified exposures to any of the crises of "Asia 1997", or "Russia 1998", or "Latin 1998". All other domestic banks are taken as unaffected because of their limited international activities (Houpt 1999; Palmer 2000).

3.4. Sample, Descriptive Statistics, and Empirical Results

3.4.1. Data sources and sample

The sample is collected from the intersection of First Call's Company Issued Guidelines (CIG) database, the DealScan database, and the CRSP/COMPUSTAT database. Panel A of Table 3.1 summarizes the sample selection process below. First, I obtain management forecasts from First Call. All types of available forecasts are retained (e.g., forecasts of earnings, or other performance measures such as cash flows), as they can all potentially reveal managers' private information. I then require all included firms to have at least one forecast before or during the two-year pre-crisis period (i.e., 1995:Q3 to 1997:Q2). This is to ensure that I only study firms that have already been covered by First Call in the earlier time period. Further, these firms have prior forecasting activities, indicating that they consider issuing forecasts as part of their disclosure

policies despite disclosure costs. I thus expect them to be sensitive to changes in forecast incentives, which is necessary for powerful tests of the corresponding forecast changes. There are 1,994 non-banking firms that meet this initial sample requirement.

To determine the choice of main bank prior to the crisis events, for every firm retained, I use DealScan to find all past loan deals over the six-year pre-crisis measurement window. DealScan reports important information on large commercial loans and has been commonly employed to study banking relationships (e.g., Hubbard et al. 2002; Bharath et al. 2007, 2009; and Schenone 2009).[56] Since there is no identifier connecting DealScan with the other two datasets, I need to match manually the datasets using borrower names and other available firm characteristics (i.e., sales, location, and industry).[57] This step excludes 623 firms that are not covered by DealScan. To ensure the sample firms are active borrowers, I further exclude 114 firms with no loans reported over the six-year measurement window. This leads to a sample of 1,257 firms with a total of 3,653 loan deals. Next, for each of these prior loan deals, I follow Sufi (2007) and identify the lead bank using the "Lead Arranger Credit" indicator provided by DealScan. I then determine the borrower's reliance ($LoanReli$) on each of its lead banks for loans and

[56] Researchers can obtain more comprehensive borrowing information from DealScan than from public sources solely. This is because, in addition to collecting loan data from SEC filings, DealScan receives loan information through lenders themselves, other private contacts, and a staff of reporters.

[57] To be conservative, I assign a match only when there is no ambiguity in the correspondence between the firm on COMPUSTAT and that on DealScan.

note the identity of bank(s) upon which the firm depends most. The vast majority of firms (= 1,224/1,257, or 97%) concentrate their borrowing from a single main bank. My analyses focus on these firms because they are more likely to be affected by the conditions of their main bank. Finally, I study only firms incorporated in the U.S. and whose main bank is a U.S. bank. The final sample consists of 1,111 distinct borrowers.

Panel B of Table 3.1 shows the list of main banks (as of 1997:Q2) by their loss exposures and the corresponding borrower frequency. There are 592 borrowers from 14 exposed banks.[58] As shown in Panel C, there are on average 2.9 loan deals per borrower in the six-year measurement window for the full sample. The mean total loan amount is $760 million. Although the exposed bank borrowers tend to be larger companies (see Table 3.2) with loan demand of greater amount (mean $1,203 million versus $254 million for other borrowers), they rely on their main bank for loans as much as the other borrowers, with the average $LoanReli$ ratio close to 90 percent (Panel D). Thus, contractions of loan supply by the main bank are expected to have a significant impact on the borrower's external financing.

[58] Partly due to their poor performance, two exposed banks were acquired by other banks towards the end of the sample period. These include the purchases of First Chicago NBD by Banc One, and BankAmerica by NationsBank, with both deals being finalized in 1998:Q4. These acquisitions could possibly affect the loan supply to the borrowers of the acquired banks in the subsequent periods. As a sensitivity check, I repeat all analyses excluding the affected borrowers after the merger completion date. The results are robust to this change in the sample.

Two additional notes on the sample are worth noting. First, since the sample is restricted to a specific set of firms covered concurrently by different databases, it is important to examine whether the sample exhibits any atypical firm characteristics. For this purpose, I compare the sample and all firms in the COMPUSTAT universe before the crisis events. Untabulated results show that the two samples have similar firm characteristics in fiscal year 1996, such as growth and performance (market-to-book, sales growth, asset growth, return on assets and 12-month cumulative stock returns), capital structure (leverage), and default risk (the Altman Z-score). However, the sample firms are generally bigger, likely because DealScan covers primarily firms seeking large commercial loans. While selecting such firms may limit the generalizability of the results, it is a necessary condition for improving the test power. As mentioned in Section 3.2.1, the loan tightening was mainly applied to borrowers requiring loans of large amount. For such borrowers, alternate private sources of capital are less likely to be able to fully fulfill their financing needs. Hence, I expect large firms to have greater incentives to shift into public capital sources in response to tighter loan supply, and to make the predicted disclosure changes.

Second, I use data from the COMPUSTAT segment file to estimate the extent of borrowers' activities in the crisis countries prior to the events. Instead of disclosing country-level information, firms typically report their operations in a larger geographical region such as "Europe" or "Asia". For both borrowers of exposed banks and other borrowers, foreign sales on average

were less than 10 percent of total sales in fiscal year 1996 (untabulated). Since this aggregate figure also includes business activities in developed foreign countries (e.g., Australia, Britain, Canada, France, and Germany) and other important non-crisis economies (e.g., China, India, Mexico, and the Middle East), borrowers' operations in the crisis countries before the crisis events were likely much more limited. The events therefore should not have a material direct impact on borrowers' performance. Further, two aspects of the empirical specification help address concerns due to changing performance over time. First, the difference-in-differences specification naturally removes the impact of foreign markets common to both borrower groups. Second, the tests explicitly control for different performance-related variables, such as market-to-book ratio, return on assets, a loss indicator, and buy-and-hold size-adjusted returns.[59]

3.4.2. Descriptive statistics

Table 3.2 Panel A shows the industry composition of the sample firms based on the Fama and French (1997) industry classification. The firms operate in diverse industries, spreading over all of the 47 defined business sectors. On the whole, both borrowers of exposed banks and other borrowers are similar in their industry distribution.

[59] As a robustness check, I repeat the analyses using a smaller sample of borrowers with additional restrictions on firms' overseas operations. Section 4.5 provides the test details.

For each firm for the period 1995:Q3 – 1999:Q2, I include all calendar quarters for which the firm has available basic COMPUSTAT items (e.g., total assets). This results in an unbalanced panel of 16,555 firm-quarters. Table 3.2 Panel B shows the summary statistics measured before the crisis events (as of 1997:Q1) by borrowers types. Borrowers of exposed banks are generally larger. This is consistent with larger firms tending to borrow from bigger banks who have the ability to satisfy loan requests of greater amount. Reflecting their larger firm size, borrowers of exposed banks have less volatile stock returns. On the other hand, the two borrower types have similar characteristics in terms of market-to-book, asset growth, return on assets, the incidences of losses and business restructuring, stock returns, beta, and operating cash flow volatility.

The pattern of firm changes from the pre-crisis to the crisis period is also very similar across the two borrower types. Table 3.2 Panel C shows that growth opportunities (as captured by market-to-book, and asset growth), performance (as captured by return on assets, the occurrence of losses, and stock returns) and beta are generally lower in the crisis period. On the other hand, return volatility is relatively higher, so is firm size. The other characteristics (i.e., cash flow volatility, and the occurrence of business restructuring) are rather stable over the two periods. The parallel firm changes support the use of borrowers of unexposed banks to control for concurrent changes in economy-wide factors.

Finally, Panel C shows that the proportion of firms issuing at least one forecast in a quarter increases significantly for borrowers of exposed bank from roughly 19 percent in the pre-crisis period to 25 percent in the crisis period. In contrast, that proportion for other borrowers is similar across the two periods, and stays at around 18.5 percent. Similarly, the mean number of forecasts issued increases significantly more for borrowers of exposed banks (from 0.24 to 0.39) than for other borrowers (from 0.23 to 0.26). These results are consistent with the predicted differential forecast increases for borrowers of exposed banks. In the analyses below, I control for the possibility that such forecast increases are attributable to firm changes over time.

3.4.3. Main results

Table 3.3 reports the main results. The test statistics in parentheses are computed using robust standard errors adjusted for firm-level clustering. Model (1) shows that the coefficient on *Crisis*× *ExpoMBK* is significantly positive (=0.399; z = 4.408). Thus, the results support H1 that borrowers of exposed banks are more likely to issue management forecasts in the crisis period. However, given that the coefficient on *Crisis* is not significantly different from zero (z = -1.096), there is no evidence of changes in forecast propensity for other borrowers in the crisis period.

As reported in model (2), the differential increase in forecast propensity in the crisis period for borrowers of exposed banks is not explained by within-firm changes in size (*Ln(MV)*), growth opportunities (*MTB, ATGROWTH*), performance (*ROA, LOSSDUM, BHAR*), and risk

and uncertainty (*BETA, CFVOL, RETVOL*). Consistent with prior studies, forecast likelihood is positively associated with firm size (e.g., Baginski et al. 2002). Also, like prior research, I find that forecast issuance is negatively associated with growth (e.g., Lennox and Park 2006), and with performance (e.g., Bergman and Roychowdhury 2008; and Chen et al. 2008).

Finally, to estimate the economic significance of the reported changes in forecast tendency following the loan supply shocks, I switch to a basic linear probability specification.[60] As shown in model (3), the differential increase in forecast probability in the crisis period for borrowers of exposed banks is about 5 percent (t = 3.99). Since the pre-crisis mean proportion of forecast firms is about 19 percent (See Panel C of Table 3.2), this is a fairly large effect.

3.4.4. Variations in the change in management forecast propensity

If loan tightening is at all related to management forecasts, the relationship should be stronger when the strength of the tightening, and hence the consequent increase in the public capital market motives, is greater. The differential treatment effects *within* the exposed bank borrowers can therefore be used as an additional source of evidence. To study these differential effects, I modify equation (1) to the following specification:

$$Pr(MF\_DUM_{it} = 1) = \alpha_i + \beta\ Crisis_t + \gamma\ Crisis_t \times Condition\_Var_i + \delta\ Controls_{it-1} + \varepsilon_{it} \quad (1b)$$

[60] I switch the test specification because the fixed effects logit model does not identify firm fixed effects in estimation. Thus, it cannot be used to predict marginal probabilities (Cameron and Trivedi 2009).

As discussed below, the strength of loan tightening is captured by three different conditioning variables ($Condition\_Var$).[61] Essentially, the test uses the exposed bank borrowers subject to a lower level of tightening as the control group and assesses whether the others that were more affected had a greater increase in forecast propensity. Since the test uses the subsample of exposed bank borrowers only, the analyses should not be affected by any biases caused by firm characteristics unique to borrowers of exposed banks. In other words, selection issues due to relationships with an exposed bank are further mitigated. For brevity, the coefficients on the control variables are not reported. In line with an increase in forecast tendency for borrowers of exposed banks, column (1) of Table 3.4 shows that the coefficient on *Crisis* is positively significant (z= 4.476).

First, a greater loan tightening is expected if the bank had a greater loss exposure to the crisis events. I thus examine whether borrowers of the more exposed banks had a higher forecast increase than those of other exposed banks. The conditioning variable for this test is an indicator variable that is equal to one if the firm's main bank is classified as more exposed ($ExpoMBK_{HighExpo}$). These banks include BankAmerica, Chase Manhattan, Citicorp, First Chica-

[61] Since $Condition\_Var$ is time-invariant for each firm, its main effect is subsumed by the firm fixed effect. Thus, I do not include $Condition\_Var$ separately in the model.

go NBD, and J.P. Morgan.[62] This classification is in line with the exposure information reported in the 1997 Country Exposure Reports of the Federal Financial Institutions Examination Council and in related studies (e.g., Houpt 1999; and Chava and Purnanandam 2011). Consistent with a greater forecast increase for the borrowers of these exposed banks, column (2) shows that the coefficient on $Crisis \times ExpoMBK_{HighExpo}$ is significantly positive (z = 2.127).

Instead of using bank attributes to identify variations in the strength of the loan supply shocks, the second test exploits an important credit risk-related borrower characteristic – the availability of collateral. Banks often require collateral to reduce credit risk, and as mentioned in Section 3.2.1, exposed banks raised their collateral requirements in the crisis period. This likely had a lower impact on the loan access for borrowers with more tangible assets. I thus expect that these borrowers had a lower increase in forecast tendency than other borrowers of exposed banks. The conditioning variable for this test is $Tangibility$, defined as the decile rank of the ratio of the borrowers' tangible assets (i.e., property, plant and equipment plus inventories) to total assets at the start of fiscal year 1997. In line with a lower increase in forecast propensity for borrowers with better credit risk, column (3) shows that the coefficient on $Crisis \times Tangiblilty$ is significantly negative (z = -2.503).

[62] Some studies suggest that Bankers Trust rank also among the top exposure banks (e.g., Chava and Purnanandam 2011). I obtain similar results if Bankers Trust is also included as one of the more exposed banks.

Alternatively, I identify firms particularly subject to the tightening based on borrowers' engagement in risky projects, as captured by the existence of research and development (R&D) activities. Managers in R&D firms often possess private knowledge about the prospect of their R&D projects (e.g., Myers 1977; Blazenko 1987; and Aboody and Lev 2000; Sufi 2007). The greater information asymmetry and the high perceived credit risk could lead to more restricted loan access for R&D firms in the crisis period. I thus study if these exposed bank borrowers had a relatively higher increase in forecast tendency. Issuing management forecasts as compared to other types of disclosure has the benefit of limiting the details about the R&D projects that competitors could exploit. At the same time, management forecasts have more straightforward firm valuation implications for outsiders. In this test, I set the conditioning variable equal to one if the borrower reported any R&D activities prior to the crisis events, and zero otherwise (*R&D_firm*).[63] Consistent with a greater increase in forecast tendency for these firms, column (4) shows that the coefficient on *Crisis* × *R&D_firm* is significantly positive (z=2.534). Importantly, column (5) shows that the results remain consistent when I include all three conditioning variables.

[63] Using the decile rank of the ratio of R&D expenses to sales at the end of fiscal year 1996 as the conditioning variable provides qualitatively similar results.

To ensure that the above differences in the change in forecast propensity is due to different strength of loan supply shocks, I contrast these results with those based on the subsample of the borrowers of other (unexposed) banks. Since these borrowers were less subject to increasing lending standards in the crisis period regardless of their availability of collateral (or R&D activities), I do not expect them to exhibit similar differential disclosure changes. As predicted, I find that the coefficient on $Crisis \times Tangiblility$ is not significantly different from zero (z = 0.603) in the test of available tangible assets (column (2a)). Similarly, I find no evidence of a greater forecast increase for R&D borrowers (column (3a)). Overall, the results in this section provide further support to the effect of bank loan supply reductions on borrowers' forecast activities.

3.4.5. Analyses based on other time periods

So far I have focused on the cross-sectional variation in forecast increase for borrowers of exposed bank in the crisis period. I now change the focus and study the variation in forecast tendency over time. This test mitigates concerns that the main results are spuriously driven by a general disclosure increase over time for exposed bank borrowers. Before turning to the results, readers should note that there was potential financial turmoil emerged in other countries at the end of 2000 (e.g., Argentina and Turkey; see Desai 2003). These events could affect bank health and introduce new loan supply uncertainty, thereby confounding the test here. Another complication is the adoption of Regulation Fair Disclosure by the SEC in late 2000, which led to signifi-

cant increases in the issuance of management forecasts by U.S. firms (Hirst et al. 2008).[64] If for unknown reasons the two borrower groups have differential responses to the new regulation, then the results will be confounded. To minimize the impacts of these complications, I focus on how borrowers' forecast tendency changed in the year following the crisis period. Correspondingly, the last year of the crisis period (1998:Q3 - 1999:Q2) is used as the benchmark.[65] The regression specification is shown below.

$$Pr(MF\_DUM_{it} = 1) = \alpha_i + \beta \ After_t + \gamma \ After_t \times ExpoMBK_i + \delta \ Controls_{it-1} + \varepsilon_{it} \qquad (1c)$$

Essentially, the test replaces the *Crisis* indicator in Equation (1) with an alternate indicator (*After*) for the four quarters from 1999:Q3 to 2000:Q2. Column (1) of Table 3.5 Panel A shows that the coefficient on *After* × *ExpoMBK* is significantly negative (z=-1.966), consistent with a weakening forecast tendency for exposed bank borrowers as the crises subsided.

I also repeat the same test using other time periods when we do not observe differential changes in loan supply. Specifically, I use three different two-year windows around Jun 30 of 1996, 2003 and 2004. See Figure 3.6 for the timeline.

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[64] Regulation FD became effective in October 2000, but discussions concerning draft versions of the regulation began in December 1999. Thus, in anticipation of the regulation, companies could have started altering their forecast behaviors as early as in the beginning of 2000.

[65] Using both years of the crisis period as the benchmark provides qualitatively similar results.

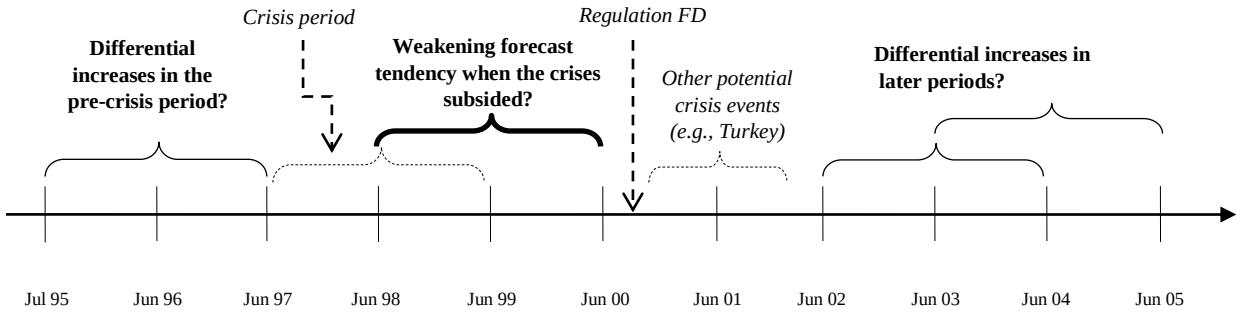

Figure 3.6 Timeline

If there were truly some unknown factors causing an increase in disclosure over time for borrowers of exposed banks, I should find similar increases even when there were no changes in loan supply. However, consistent with the lack of treatment effect in these alternate periods, in none of the three cases is the coefficient on *After* × *ExpoMBK* significantly positive (columns (2) to (4)).

3.4.6. Robustness checks

This sub-section reports the results for three sensitivity tests of the main results in Table 3.3.

Robustness check 1: logit regression with no firm fixed effects

For completeness, I repeat the main test using the more traditional logit regression with no firm fixed effects. As explained before, this test is more susceptible to correlated omitted variables. I therefore do not use this specification in the main analyses, although the findings are similar to those previously reported (see column (1) of Table 3.6).

Robustness check 2: additional restrictions on firms' foreign operations

I repeat the main test using a restricted sample of 865 firms. This sample excludes all firms with aggregate sales across specific regions (Asia, Europe, Pacific Basin, and South America) greater than 5 percent of total sales in the fiscal year prior to the crisis events. Thus, this sample includes only borrowers with immaterial exposure to the crisis events. However, this sample tends to exclude larger firms which require loans of large amount.[66] For smaller firms, alternate private capital sources are more likely to be able to fully fulfill their financing needs, so public financing is a less important substitute. Despite this bias against my hypothesis and a smaller sample, I find that the main results persist (column (2)), although with a 15 percent smaller coefficient and a lower level of statistical significance (coefficient on *Crisis× ExpoMBK* drops from 0.344 to 0.291, z statistics drops from 4.047 to 3.043).

Robustness check 3: tests based on management forecast frequency

Finally, rather than using the issuance of management forecasts as the dependent variable, I repeat the main test using the frequency of management forecasts during the quarter. This relaxes the assumption that a firm issuing multiple forecasts is the same as one that issues only a single forecast in the period. The test uses a Poisson specification. The results are similar and are in fact

[66] On average, the excluded firms are about three times larger (in terms of total assets) than the remaining ones.

more significant, supporting the hypothesis that borrowers of exposed banks increased their forecast frequency in the crisis period significantly more than other borrowers (column (3)).

3.5. Characteristics of issued forecasts

In addition to the decision of whether to issue a forecast, management also chooses various characteristics of these forecasts. If the objective of reducing information asymmetry is what explains the forecast increase for borrowers of exposed banks in the crisis period, then I expect to find changes in forecast characteristics that are consistent with that objective. Prior studies suggest that more voluntary information disclosure includes (1) a higher forecast frequency, (2) forecasts over longer horizons, and (3) more precise forecasts (e.g., Baginski et al 2002, 29). I thus separately examine the changes in the horizon and the precision of the forecasts issued over the two periods. I further study any changes in the forecast news content in order to depict a more comprehensive picture of how loan supply distortions affect firms' forecast decisions. In the tests, the analysis is at the individual forecast level. There are a total of 4,650 forecasts issued in the two periods. Table 3.7 provides the summary statistics. The details are discussed below.

3.5.1. Forecast horizon

Forecast horizon (*Horizon*) is measured as the number of calendar days between the forecast issuance date and the forecasting fiscal period-end date.[67] On one hand, because longer-horizon forecasts are more likely to turn out to be inaccurate, the entailed legal risk can deter firms from issuing such forecasts. On the other hand, firms making longer-horizon forecasts can reduce information asymmetry by providing investors with more timely information (Baginski et al. 2002). After loan supply shocks, if exposed bank borrowers have increased incentives to reduce investor uncertainty, then I expect them to issue longer-horizon forecasts because of the higher (perceived) benefits of such forecasts. In contrast, the incentives for issuing longer-horizon forecasts likely remained relatively similar over time for other borrowers. Consistent with this conjecture, Table 3.7 Panel B shows that the median *Horizon* increases from 10 days to 20 days in the crisis period for other borrowers, while the change for exposed bank borrowers is much larger (from 13 days to 59 days).

To more formally test the differential increase in forecast horizon, I adjust the model presented in Equation (1) by replacing the dependent variable with an alternate indicator variable (MF_DUM_{Long}) that equals one if the forecast is a long-run forecast. Following prior research

[67] *Horizon* is a negative number if the forecast is issued after the fiscal period end but before the earnings announcement date. These forecasts are typically referred to as earnings preannouncements (Hirst et al. 2008).

(e.g., Chen et al. 2008), long-run forecasts are defined as forecasts with a horizon longer than a fiscal quarter. The results reported in Model (1) of Table 3.8 shows that the change for borrowers of exposed banks is significantly positive (*Crisis* × *ExpoMBK* = 0.672, z = 3.319), consistent with these borrowers becoming more likely to issue long-run forecasts in the crisis period. In contrast, there is no evidence that other borrowers increased the propensity of issuing long-run forecasts (*Crisis* = -0.120, z = -0.628).. Model (2) shows that the results are not driven by within-firm changes from pre-crisis to crisis periods. Finally, I use an alternate dependent variable that is continuous rather than discrete, the log of *Horizon* (*Log(Horizon)*), to directly test for any changes in the length of forecast horizon.[68] As seen in the OLS regression (3), the inferences are similar.

3.5.2. Forecast precision

During the crisis period, managers faced opposing incentives to provide more precise forecasts. Prior research suggests that less precise forecasts, such as general impression forecasts (e.g., "above expectations"), lower the likelihood of ex post forecast inaccuracy, and hence the firm's legal exposure. In the crisis period, increased market volatility (as reflected in higher return volatility (Table 3.2)) can induce firms to issue less precise forecasts to avoid unexpected events that

[68] To enable a logarithmic transformation, $Horizon$ is recoded to a small positive value (0.5) if the forecast is an earnings preannouncement. Alternatively, excluding earnings preannouncements altogether from the sample yields similar results.

cause missing forecasts. On the other hand, precise forecasts are likely to be more effective for reducing information uncertainty, so firms concerned with information problems have incentives to maintain (or even increase) the precision of their forecasts in the crisis period. I thus conjecture that, relative to other firms, borrowers of exposed banks have less negative change in forecast precision in the crisis period. To test this conjecture, I follow prior research (e.g., Baginski and Hassell 1997) to define a forecast precision variable (*Precision*). Specifically, point, range, open-ended, and general impression forecasts are coded 3, 2, 1, 0, respectively. Table 3.7 Panel B shows that the mean *Precision* for borrowers of exposed banks drops by about 6 percent from 2.07 to 1.94 in the crisis period. In comparison, the mean *Precision* for other borrowers decreased by 13 percent from 2.04 to 1.77. Thus, borrowers of exposed banks seem to reduce forecast precision relatively little despite the significant increase in their forecast horizon (see Section 3.5.1).

To conduct a more formal test, I adjust the model in Equation (1) by replacing the dependent variable with *Precision*, and using an ordered logit regression. Because firm-fixed effects ordered logit model is not feasible,[69] the test includes no firm-fixed effects. Model (1) of Table

[69] According to Greene and Hensher (2009, 207-210), using conditional maximum likelihood is the recommended approach for estimating nonlinear fixed effects models. However, for ordered choice models, there is currently no sufficient statistic available to use to condition the fixed effects out of log likelihood. Alternatively, estimating the models unconditionally "by brute force by including the dummy variables in the model …would induce the biases of the incidental parameters problem." As such, I decide against the use of this latter approach.

3.9 shows that other borrowers decreased the propensity of issuing precise forecasts in the crisis period (coefficient = -0.404, z = -4.805). In contrast, the coefficient on the interaction term *Crisis* × *ExpoMBK* is significantly positive (coefficient = 0.260; z = 2.383), consistent with the greater incentives for borrowers of exposed banks to maintain forecast precision. The findings persist after including firm-level control variables (model (2)). As shown in Model (3), the results are also robust to changing the specification to a binary logit model with firm-fixed effects. In this model, the dependent variable is alternatively defined as an indicator variable (MF_DUM_{Quan}) that equals one if the forecast is a quantitative forecast (i.e., a point or range estimate).

3.5.3. Forecast news

If managers' use forecasts to reduce information asymmetry, then those forecasts would not depend on whether the manager has positive or negative private information. Both good and bad news disclosures can help "correct investors' perceptions about current or future performance, so that the stock is priced off company-provided information rather than misinformation", as suggested by chief financial officers (CFOs) interviewed in Graham et al.'s (2005, 54) survey on voluntary disclosure practices. Based on similar arguments, prior research contends that managers use both good and bad news forecasts to lower information asymmetry before securities offerings (e.g., Marquardt and Wiedman 1998). On the other hand, firms seeking external financing have incentives to selectively disclose good news (or withhold bad news) to "hype"

stock prices and increase the financing proceeds (Lang and Lundholm 2000), although such strategies have a high risk of civil litigation or criminal penalties. To shed light on the motives that dominated borrowers' disclosure decisions in the crisis period, I examine the changes in borrowers' tendency to release good news over time. Consistent with the focus on how voluntary disclosure affects stock valuation, I classify a forecast as conveying good news if the 3-day size-adjusted abnormal returns centered on forecast date (*MF_CAR*) is greater than one percent (e.g., Cheng and Lo 2006).[70] Because the stock market effects of coincidental forecasts cannot be separated, multiple forecasts by the same firm on the same day are treated as a single forecast in this analysis (e.g., Brockman et al. 2008).

Table 3.7 Panel B reveals that the proportion of good news forecasts increased in the crisis period for borrowers of exposed banks from 31.3 percent to 37.1 percent. The results also show a similar increase for other borrowers (from 30.7 percent to 35.7 percent). Thus, loan supply shocks do not seem to have a particularly strong influence on firms' tendency to release good news forecasts. To examine this further, I adjust the model in Equation (1) by replacing the dependent variable with an alternate indicator variable ($MF\_DUM_{GoodNews}$) that equals one if the forecast is a good news forecast. Model (1) of Table 3.10 shows that the coefficient for *Crisis* × *ExpoMBK* is insignificant (z = 1.484), thus providing no support for an incremental increase in

[70] Using the sign of the abnormal return to classify good news versus bad news yields qualitatively similar results.

the tendency to release good news for borrowers of exposed banks. Model (2) shows similar results after controlling for within-firm changes in firm-level controls. Finally, I use *MF_CAR* as the dependent variable to test for the changes in the magnitude of good news disclosed. Again, as reported in regression (3), there is no evidence that the forecasts issued in the crisis period by borrowers of exposed banks tended to contain more good news (or less unfavorable information) ($t = 0.704$).

Overall, the results in this section suggest that constrained borrowers tended to increase (or at least not compromised) the horizon and precision of their forecasts, presumably because more timely and precise forecasts are more effective for lowering information asymmetry. Further consistent with the preference for reduced information asymmetry (rather than selective disclosure behaviors), there is no evidence of changes in constrained borrowers' tendency to issue good news forecasts (or withhold bad news). A plausible explanation for this result is the high litigation risk associated with selective disclosure. Alternatively, public financing is a repeated game, so managers believe a reputation for providing timely and accurate disclosure is important for "easier access to capital in the future or a lower cost of capital," as noted by Graham et al. (2005, 54).

3.6. Conclusion

The emerging-market financial crises in the late 1990s were plausibly external to the U.S. loan markets, yet their effect was transmitted through U.S. banks' large loss exposures to the crisis areas which ultimately limited the lending of exposed banks back in the U.S. In this paper, I use this exogenous tightening of loan supply to U.S. borrowers as a natural experiment for studying the impact of loan supply distortions on borrowers' management forecasts. These forecasts can potentially enhance firms' access to alternate (public) capital markets. Accordingly, I predict that constrained borrowers had increased incentives to issue management forecasts in the crisis period in order to facilitate the substitution for previously available loans.

As predicted, relative to other U.S. borrowers, those of exposed banks issued significantly more forecasts in the crisis period. This differential disclosure increase is more pronounced if the borrower is more likely to be credit rationed, either because its banks had particularly large exposures to the crises, or due to its own credit risk characteristics (i.e., company has low availability of collaterals or engaged in risky projects). Further, the predicted forecast increases show up only in periods when loan supply was constrained, but not in other periods when the crises subsided, and when differential loan tightening was not expected. All these results provide help to establish the causal impact of tight loan supply on disclosure.

Finally, besides the forecast increases, borrowers of exposed banks showed differential changes in their forecast characteristics in the crisis period. First, the forecast horizon increased significantly more. At the same time, the forecast precision were less likely to be compromised even though factors such as longer forecast horizon or increased market volatility in the crisis period would otherwise result in less precise forecasts. These results support managers' use of management forecasts to reduce information asymmetry, which is likely to be more effective if the forecasts are more timely and precise. Further consistent with this information explanation, I find no evidence that the tendency to issue good news forecasts (or withhold bad news) increased more for borrowers of exposed banks, even though these borrowers had incentives to disclose selectively to "hype" stock prices before potential funding events.

Overall, this paper extends prior literature by highlighting firms' disclosure responses to negative loan supply shocks. To this end, this paper also offers new evidence on the public capital market transaction motives for voluntary disclosure. This is important because endogeneity issues associated with changes in firms' demand for financing often cloud the interpretation of the evidence in existing studies. Yet, clear understanding of managers' disclosure incentives is key to understanding different fundamental issues such as the forces shaping firms' information environment, the need for regulating corporate disclosure in capital markets, and what types of disclosure should be mandated.

Chapter 4: Conclusion

Whether increased disclosure affects firms' access to required financing for investments is an important issue for managers and regulators. A major difficulty in examining this issue is that firm disclosure and operations are typically endogenous. Thus, it is difficult to draw strong conclusions about the causal impact of disclosure. To circumvent this problem, Chapter 2 focuses on monetary contractions that give rise to exogenous tightening of liquidity supply for banks, and examines whether banks committed to credible reporting are less affected by adverse liquidity shocks than other banks. Using external audits to proxy for accounting credibility and a sample of 9,910 small non-public banks for which audits are voluntary, I find that audited banks have greater ability to attract new financing to offset policy-induced liquidity losses. Correspondingly, the lending of audited banks is less affected by liquidity issues. Overall, Chapter 2 offers new evidence that credible disclosure facilitates corporate financing and investment activities.

In evaluating these results, some caveats should be noted. First, the specific context may reduce the generality of the results. Second, despite studying homogenous firms from a single industry and using numerous ways to address possible biases due to non-random audit choices, I cannot fully rule out the possibility of correlated omitted variables which could lead to spurious inferences. Finally, from a macroeconomic perspective, Chapter 2 appears to suggest that higher quality bank-level accounting information counteracts economy-wide reduction in lending in

opposition to central bank policy. However, readers should note that this is a generally expected outcome in market economies in which firms and individuals make decisions that maximize their own interests. Regulations, fiscal policies, and monetary policies generally have smaller impact under dynamic conditions than under static conditions because market participants change their actions in response to, and in anticipations of, government actions.

Chapter 2 suggests that enhanced disclosure increases firms' ability to alleviate constraints on financing. If this is the case, constraints on financing can induce firms to expand disclosure in order to alleviate funding issues. To shed light on this dynamic relation between firm disclosure and external financing, I examine firms' disclosure responses to reductions in the supply of external financing in Chapter 3. Specifically, I identify the tightening of bank loan supply to the borrowers of banks that were exposed to the emerging-market crises in the late 1990s as exogenous events that motivate borrowers to expand the voluntary disclosure of management forecasts. If forecasts can increase borrowers' access to alternate external funding sources, particularly the public capital markets, then constrained borrowers have incentives to adjust disclosure policies to facilitate capital raising to substitute for previously available loans. Accordingly, I predict and find evidence consistent with borrowers of exposed U.S. banks increasing forecasts in the crisis period to ease capital raising. Further, these borrowers changed forecast characteristics in ways

consistent with the use of forecasts to reduce investor uncertainty. Overall, Chapter 3 highlights that capital supply conditions affect firms' disclosure of forward-looking information.

One caveat of Chapter 3 is that it focuses solely on firms' disclosure of management forecasts. Prior research suggests that managers' tendency to provide other forms of voluntary disclosure (e.g., performance related statements) around public securities offerings may be different from that of issuing forecasts (e.g., Lang and Lundholm 2000). Thus, future research can consider disclosure alternatives to forecasts in order to depict a more comprehensive view of firms' disclosure responses to distortions in capital supply.

Despite the aforementioned limitations of Chapters 2 and 3, the findings highlight the dynamic relations between firm disclosure and external financing. The evidence provides new support to the idea that enhanced disclosure increases the ability of a firm to raise capital for investments, which in turn gives rise to managers' disclosure incentives. These results should be of interests to both managers and regulators.

Table 2.1: Sample construction

The sample is collected from the Federal Reserve's *Report of Condition and Income* (*Call Reports*), including 9,910 distinct banks and a total number of 327,084 bank-quarter observations in the period 1988:Q1 – 2000:Q4. The table below describes the sample selection process.

		Sample Size
Total number of bank-quarters in the period 1988:Q1 – 2000:Q4		616,469
Less:		
Various bank types including 1) entities other than FDIC-insured commercial banks, [a] 2) foreign-owned banks, 3) banks inactive in the loan market, [b] 4) credit card banks, [c] 5) banks subject to special analysis by regulators, [c] 6) publicly-traded banks, [d] 7) banks with total assets exceeding $500 millions, and 8) banks affiliated with a multi-bank holding company [e]	263,611	
Bank-quarters in which a merger occurs, [f] bank observations in early years, [g] and observations with non-positive total assets	10,839	
Observations with missing audit indicator, or missing required financial data	12,494	
Outliers [h]	2,441	(289,385)
Final sample of bank observations		327,084

[a] Deposit insurance status (RSSD9424) and entity type (RSSD9331) are used to identify FDIC-insured banks.

[b] These include banks with a loans-to-assets ratio below 10 percent.

[c] Credit card banks and banks subject to special analysis are identified by their bank type (RSSD9425). In addition, credit card banks include all banks that have a value of credit card loans to total loans exceeding 50 percent.

[d] Following Holod and Peek (2007), publicly-traded banks include 1) all stand-alone banks whose equity is publicly traded, and 2) all other banks that are indirectly publicly traded through their parent bank holding company. The historical linkage between regulatory entity codes (RSSD9001) and CRSP permcos provided by the Federal Reserve Bank of New York is used to identify publicly-traded banks. The linkage is only available from 1990 onwards.

Hence, trading status is assumed to be the same in 1988 and 1989 as in 1990. This assumption seems reasonable, given the low rate of change in trading status during 1990-2000.

[e] Affiliation with a multi-bank holding company is identified if the bank is controlled by a direct (RSSD9379) or regulatory holder (RSSD9348), and that holder controls more than one banks.

[f] The merger file from the Federal Reserve Bank of Chicago is used to identify times when a bank merger occurs.

[g] Including observations that are within three years from their opening date (RSSD9950).

[h] Similar to previous studies (e.g., Campello, 2002), bank-quarters with total asset growth greater than 50 percent are removed.

Table 2.2: Descriptive statistics

This table reports common balance sheet and other selected information about the sample. The sample and data are collected from the Call Reports, including 9,910 distinct banks and a total number of 327,084 bank-quarters in the period from 1988:Q1 – 2000:Q4. Variable definitions are presented in Appendix B Panel B.

Panel A: Full sample

	Mean	Std. Dev.	Q1	Median	Q3
Total assets (*millions*)	70.12	68.81	24.73	44.72	81.96
Log of total assets	10.72	0.87	10.12	10.71	11.31
Liquid Assets (÷ *by total assets*)	0.279	0.183	0.125	0.268	0.411
Total loans (÷ *by total assets*)	0.533	0.139	0.438	0.549	0.639
C&I loans (÷ *by total loans*)	0.178	0.110	0.096	0.153	0.235
Real estate loans (÷ *by total loans*)	0.514	0.186	0.379	0.522	0.656
Other loans (÷ *by total loans*)	0.308	0.182	0.160	0.280	0.423
Non-performing loans (÷ *by total loans*)	0.017	0.019	0.003	0.010	0.023
Total Liabilities (÷ *by total assets*)	0.902	0.029	0.888	0.909	0.923
Core deposits (÷ *by total liabilities*)	0.787	0.112	0.718	0.803	0.873
Managed liabilities (÷ *by total liabilities*)	0.147	0.103	0.089	0.131	0.189
Large CDs (÷ *by total liabilities*)	0.108	0.068	0.055	0.093	0.146
Equity (÷ *by total assets*)	0.098	0.029	0.077	0.091	0.112
Other variables used in the main tests					
Quarterly growth of total loans (*Loan_Growth*)	0.019	0.050	-0.011	0.018	0.047
Quarterly growth of managed liabilities (*ML_Growth*)	0.022	0.182	-0.065	0.007	0.105
Audit indicator	0.543	0.498	0.000	1.000	1.000
Controlled by a one-bank holding company (*OBHC*)	0.623	0.485	0.000	1.000	1.000
Located in a Metropolitan Statistical Area (*MSA*)	0.382	0.486	0.000	0.000	1.000
Low equity ratio indicator (*LowCap*)	0.051	0.780	0.000	0.000	0.000
Loss indicator	0.114	0.317	0.000	0.000	0.000
Return on Equity	0.034	0.021	0.024	0.036	0.049
Internal cash flows (*Internal CF*)	0.011	0.011	0.008	0.014	0.018
Number of observations			327,084		

Descriptive statistics

Panel B: Sample by audit status

	Audited sample					Unaudited sample				
	Mean	Std. Dev.	Q1	Median	Q3	Mean	Std. Dev.	Q1	Median	Q3
Total assets (*millions*)	90.46	80.59	33.98	60.88	108.53	45.97**	39.64	18.87	32.24**	54.96
Log of total assets	11.01	0.85	10.43	11.02	11.60	10.38**	0.76	9.85	10.38**	10.91
Liquid Assets (÷ *by total assets*)	0.267	0.176	0.122	0.253	0.387	0.293**	0.190	0.130	0.288**	0.437
Total loans (÷ *by total assets*)	0.547	0.138	0.455	0.564	0.651	0.517**	0.139	0.419	0.529**	0.623
C&I loans (÷ *by total loans*)	0.189	0.117	0.101	0.164	0.254	0.164**	0.099	0.092	0.143**	0.213
Real estate loans (÷ *by total loans*)	0.552	0.181	0.427	0.565	0.691	0.469**	0.181	0.332	0.472**	0.603
Other loans (÷ *by total loans*)	0.259	0.168	0.121	0.225	0.351	0.367**	0.180	0.225	0.349**	0.494
Non-performing loans (÷ *by total loans*)	0.017	0.019	0.004	0.011	0.024	0.016**	0.018	0.003	0.010**	0.022
Total Liabilities (÷ *by total assets*)	0.906	0.027	0.892	0.911	0.925	0.899*	0.030	0.882	0.905*	0.920
Core deposits (÷ *by total liabilities*)	0.767	0.117	0.691	0.783	0.857	0.810**	0.101	0.750	0.825**	0.888
Managed liabilities (÷ *by total liabilities*)	0.156	0.105	0.097	0.142	0.201	0.135**	0.099	0.081	0.119**	0.173
Large CDs (÷ *by total liabilities*)	0.116	0.069	0.062	0.103	0.157	0.098**	0.064	0.049	0.083**	0.132
Equity (÷ *by total assets*)	0.094	0.027	0.075	0.089	0.108	0.101**	0.030	0.079	0.095**	0.117
Other variables used in the main tests										
Quarterly growth of total loans (*Loan_Growth*)	0.019	0.048	-0.009	0.018	0.047	0.018**	0.051	-0.013	0.017**	0.048
Quarterly growth of managed liabilities (*ML_Growth*)	0.021	0.174	-0.065	0.010	0.101	0.024**	0.191	-0.065	0.005**	0.112
Controlled by a one-bank holding company (*OBHC*)	0.603	0.489	0.000	1.000	1.000	0.647**	0.478	0.000	1.000**	1.000
Located in a Metropolitan Statistical Area (*MSA*)	0.419	0.460	0.000	0.000	1.000	0.338**	0.437	0.000	0.000**	1.000
Low equity ratio indicator (*LowCap*)	0.063	0.757	0.000	0.000	0.000	0.036**	0.813	0.000	0.000*	0.000
Loss indicator	0.130	0.336	0.000	0.000	0.000	0.094**	0.292	0.000	0.000**	0.000
Return on Equity	0.033	0.022	0.023	0.036	0.049	0.035*	0.020	0.024	0.036*	0.050
Internal cash flows (*Internal CF*)	0.011	0.011	0.007	0.014	0.017	0.012**	0.011	0.008	0.015**	0.018
Number of observations	177,593 (54% of full sample)					149,491 (46% of full sample)				

Asterisks (**, *) denote a significant difference in the variable of interest between the audited and unaudited sub-samples at the one per-cent, and five percent level (two-tailed), respectively.

Table 2.3: H1: Comparing the responses of managed liabilities to monetary policy across audited and unaudited banks

This table reports results from regressing the quarterly growth of managed liabilities (*ML_Growth*) on the contractionary policy indicators (*TightMP*), an audit indicator for the bank's audit status in the previous year (*Audited*), the policy-audit interaction terms (*TightMP*×*Audited*), and various control variables. Regressions (1) and (2) include only policy shocks in the contemporaneous quarter ($TightMP_t$), thus assuming no delays in banks' responses to monetary tightening. The specification for regression (2) is shown below:

$$\begin{aligned}\text{ML\_Growth}_{it} = {} & \alpha + \beta\ \text{TightMP}_t + \gamma\ \text{Audited}_{it-4} + \delta\ \text{TightMP}_t \times \text{Audited}_{it-4} \\ & + \zeta\ \text{Bank-level control}_{it-1} + \eta\ \text{TightMP}_t \times \text{Bank-level control}_{it-1} + \ldots\ldots \\ & + \textstyle\sum_{j=1}^{5} \theta_j\ \text{ML\_Growth}_{it-j} + \sum_{j=0}^{5} \iota_j\ \text{GDP\_Growth}_{t-j} + \sum_{j=0}^{5} \kappa_j\ \text{CPI\_Growth}_{t-j} \\ & + \lambda\ \text{Basel}_t + \textstyle\sum_{j=1}^{50} \mu_j\ \text{State}_{ij} + \nu\ \text{Time trend}_t + \sum_{j=1}^{3} \xi_j\ \text{Quarter}_{jt} + \varepsilon_{it}\end{aligned}$$

where *TightMP* is an indicator that equals one if contractionary policies take place in the quarter. Bank-level controls include the natural log of total assets (*Ln(TA)*), a set of indicators for banks owned by a one-bank holding company (*OBHC*), banks located in a Metropolitan Statistical Area (*MSA*), banks with an equity to assets ratio below six percent (*LowCap*), and loss-making banks (*Loss*), the ratio of non-performing loans to total loans (*NPL*), the ratio of liquid assets to total assets (*Liquid assets*), and the ability to generate internal cash flows (*Internal CF*). Each regression also includes five lags of the dependent variable, the current and five lags of each of the growth rates of GDP (*GDP_Growth*), and the consumer price index (*CPI_Growth*), a dummy variable for the time period 1988 – 92 (*Basel*), a set of state indicators, a time trend, and a set of three quarter indicators. Variable definitions are presented in Panel B of Appendix B. The sample consists of 327,084 bank-quarters in the period from 1988:Q1 – 2000:Q4.

In regressions (3) and (4), five lagged terms of *TightMP* are added to capture delays in banks' responses to monetary contractions (see Figure 2.2). The specification for regression (4) is shown below:

$$\begin{aligned}\text{ML\_Growth}_{it} = \alpha &+ \textstyle\sum_{j=0}^{5} \beta_j \ \text{TightMP}_{t-j} + \gamma \ \text{Audited}_{it-4} + \sum_{j=0}^{5} \delta_j \ \text{TightMP}_{t-j} \times \text{Audited}_{it-4} \\ &+ \zeta \ \text{Bank-level control}_{it-1} + \textstyle\sum_{j=0}^{5} \eta_j \ \text{TightMP}_{t-j} \times \text{Bank-level control}_{it-1} + \ldots\ldots \\ &+ \textstyle\sum_{j=1}^{5} \theta_j \ \text{ML\_Growth}_{it-j} \ + \sum_{j=0}^{5} \iota_j \ \text{GDP\_Growth}_{t-j} + \sum_{j=0}^{5} \kappa_j \ \text{CPI\_Growth}_{t-j} \\ &+ \lambda \ \text{Basel}_t + \textstyle\sum_{j=1}^{50} \mu_j \ \text{State}_{ij} + \nu \ \text{Time trend}_t + \sum_{j=1}^{3} \xi_j \ \text{Quarter}_{jt} + \varepsilon_{it}\end{aligned}$$

The coefficients on the *TightMP*-related variables in these two columns are the sums of the six coefficients on the contemporaneous and the lagged monetary policy variables. The t-statistics in parentheses (for both individual coefficients and the sums of the coefficients on policy terms) are based on robust standard errors clustered by bank. In columns (3) and (4), I also report p-values for the F-test that the coefficients on the five lagged interactions *TightMP* × *Audited* are jointly zero.

H1: Comparing the responses of managed liabilities to monetary policy across audited and unaudited banks

	Pred. Sign	No lagged policy variables (1)		No lagged policy variables (2)		Adding five lagged policy variables (3)		Adding five lagged policy variables (4)	
		Coef.	t-stat.	Coef.	t-stat.	Coef.	t-stat.	Coef.	t-stat.
Intercept		-0.0031	(-0.17)	0.0134	(0.72)	0.0603	(3.32)	0.0960	(4.98)
Monetary policy variables						*For policy variables: shown are sums of coefficients*			
TightMP	+	0.0239	(17.68)	0.0010	(0.09)	0.0913	(30.81)	0.0409	(2.40)
TightMP × Audited	**+**	**0.0103**	**(7.07)**	**0.0046**	**(2.87)**	**0.0290**	**(10.40)**	**0.0193**	**(6.46)**
TightMP × Ln(TA)	+			0.0032	(3.10)			0.0075	(4.59)
TightMP × OBHC	?			0.0006	(0.37)			-0.0042	(-1.60)
TightMP × MSA	+			0.0115	(7.15)			0.0204	(7.42)
TightMP × LowCap	-			-0.0086	(2.37)			-0.0296	(-5.14)
TightMP × Loss	-			-0.0072	(-1.98)			-0.0167	(-2.89)
TightMP × NPL	-			-0.0372	(-0.99)			0.1056	(1.72)
TightMP × Liquid assets	-			-0.0273	(-6.89)			-0.0603	(-10.02)
TightMP × Internal CF	-			-0.1181	(-2.06)			-0.2543	(-3.18)
Bank-level variables									
Audited		-0.0019	(-1.58)	0.0004	(0.40)	-0.0088	(-6.43)	-0.0035	(-2.52)
Ln(TA)		-0.0014	(-2.81)	-0.0027	(-4.25)	-0.0013	(-2.58)	-0.0040	(-4.97)
OBHC		-0.0022	(-2.73)	-0.0027	(-2.66)	-0.0023	(-2.87)	0.0002	(0.19)
MSA		-0.0015	(-1.80)	-0.0065	(-6.01)	-0.0013	(-1.58)	-0.0072	(-5.37)
LowCap		-0.0353	(-19.69)	-0.0257	(-15.84)	-0.0356	(-19.86)	-0.0221	(-7.94)
Loss		-0.0368	(-20.63)	-0.0338	(-15.09)	-0.0364	(-20.40)	-0.0310	(-11.17)
NPL		-0.5888	(-30.83)	-0.5659	(-23.55)	-0.5879	(-30.82)	-0.6558	(-22.05)
Liquid assets		-0.0420	(-14.01)	-0.0386	(-12.35)	-0.0421	(-14.06)	-0.0300	(-9.16)
Internal CF		0.1655	(3.80)	0.2314	(4.77)	0.1950	(4.48)	0.2959	(5.47)
Economy-wide and other factors		Yes		Yes		Yes		Yes	
Number of observations		327,084		327,084		327,084		327,084	
Adj. R^2		0.0633		0.0639		0.0661		0.0678	
F-stat. and p-value for the joint significance of the five lagged *TightMP × Audited*						14.39 (0.001)		7.02 (0.001)	

Table 2.4: H2: Comparing the responses of total loans to monetary policy across audited and unaudited banks

This table reports results from regressing the quarterly growth of total loans (*Loan_Growth*) on the contractionary policy indicators (*TightMP*), an audit indicator for the audit status in the previous year (*Audited*), the policy-audit interaction terms (*TightMP*×*Audited*), and various control variables. Regressions (1) and (2) include only policy shocks in the contemporaneous quarter ($TightMP_t$), thus assuming no delays in banks' responses to monetary tightening. The specification for regression (2) is shown below:

$$\begin{aligned}\text{Loan\_Growth}_{it} = {} & \alpha + \beta\ \text{TightMP}_t + \gamma\ \text{Audited}_{it-4} + \delta\ \text{TightMP}_t \times \text{Audited}_{it-4} \\ & + \zeta\ \text{Bank-level control}_{it-1} + \eta\ \text{TightMP}_t \times \text{Bank-level control}_{it-1} + \ldots\ldots \\ & + \textstyle\sum_{j=1}^{5} \theta_j\ \text{Loan\_Growth}_{it-j} + \sum_{j=0}^{5} \iota_j\ \text{GDP\_Growth}_{t-j} + \sum_{j=0}^{5} \kappa_j\ \text{CPI\_Growth}_{t-j} \\ & + \lambda\ \text{Basel}_t + \textstyle\sum_{j=1}^{50} \mu_j\ \text{State}_{ij} + \nu\ \text{Time trend}_t + \sum_{j=1}^{3} \xi_j\ \text{Quarter}_{jt} + \varepsilon_{it}\end{aligned}$$

See notes to Table 2.3 for details on the variables and the sample.

In regressions (3) and (4), five lagged terms of *TightMP* are added to capture delays in banks' responses to monetary contractions (See Figure 2.2). The specification for regression (4) is shown below:

$$\begin{aligned}\text{Loan\_Growth}_{it} = {} & \alpha + \textstyle\sum_{j=0}^{5} \beta_j\ \text{TightMP}_{t-j} + \gamma\ \text{Audited}_{it-4} + \sum_{j=0}^{5} \delta_j\ \text{TightMP}_{t-j} \times \text{Audited}_{it-4} \\ & + \zeta\ \text{Bank-level control}_{it-1} + \textstyle\sum_{j=0}^{5} \eta_j\ \text{TightMP}_{t-j} \times \text{Bank-level control}_{it-1} + \ldots\ldots \\ & + \textstyle\sum_{j=1}^{5} \theta_j\ \text{Loan\_Growth}_{it-j} + \sum_{j=0}^{5} \iota_j\ \text{GDP\_Growth}_{t-j} + \sum_{j=0}^{5} \kappa_j\ \text{CPI\_Growth}_{t-j} \\ & + \lambda\ \text{Basel}_t + \textstyle\sum_{j=1}^{50} \mu_j\ \text{State}_{ij} + \nu\ \text{Time trend}_t + \sum_{j=1}^{3} \xi_j\ \text{Quarter}_{jt} + \varepsilon_{it}\end{aligned}$$

The coefficients on the *TightMP*-related variables in these two columns are the sums of the six coefficients on the contemporaneous and the lagged monetary policy variables. The t-statistics in parentheses (for both individual coefficients and the sums of the coefficients on policy terms) are based on robust standard errors clustered by bank. In columns (3) and (4), I also report p-values for the F-test that the coefficients on the five lagged interactions *TightMP* × *Audited* are jointly zero.

H2: Comparing the responses of total loans to monetary policy across audited and unaudited banks

	Pred. Sign	No lagged policy variables (1)		No lagged policy variables (2)		Adding five lagged policy variables (3)		Adding five lagged policy variables (4)	
		Coef.	t-stat.	Coef.	t-stat.	Coef.	t-stat.	Coef.	t-stat.
Intercept		0.0326	(7.53)	0.0333	(7.47)	0.0247	(5.72)	0.0402	(8.77)
Monetary policy variables						*For policy variables: shown are sums of coefficients*			
TightMP	-	-0.0062	(-21.34)	-0.0123	(-5.11)	-0.0252	(-29.18)	-0.0552	(-13.49)
TightMP × Audited	**+**	**0.0015**	**(4.38)**	**0.0011**	**(2.92)**	**0.0077**	**(11.61)**	**0.0052**	**(7.61)**
TightMP × Ln(TA)	+			0.0004	(1.69)			0.0023	(5.93)
TightMP × OBHC	?			0.0006	(1.87)			0.0010	(1.60)
TightMP × MSA	+			0.0013	(3.41)			0.0056	(8.53)
TightMP × LowCap	-			-0.0025	(-2.86)			-0.0082	(-6.03)
TightMP × Loss	-			-0.0018	(2.07)			-0.0052	(-3.79)
TightMP × NPL	-			-0.0087	(-0.97)			0.0144	(0.99)
TightMP × Liquid assets	+			0.0067	(7.02)			0.0177	(12.36)
TightMP × Internal CF	+			-0.0163	(-1.19)			-0.0184	(-0.97)
Bank-level variables									
Audited		0.0006	(1.68)	0.0013	(5.20)	-0.0012	(-3.57)	-0.0003	(-0.96)
Ln(TA)		-0.0018	(-14.79)	-0.0019	(-12.21)	-0.0018	(-14.82)	-0.0025	(-13.17)
OBHC		-0.0006	(-3.03)	-0.0010	(-4.11)	-0.0006	(-2.99)	-0.0008	(-2.60)
MSA		0.0033	(16.10)	0.0023	(9.04)	0.0032	(15.83)	0.0011	(3.31)
LowCap		-0.0068	(-16.01)	-0.0055	(-10.28)	-0.0065	(-15.23)	-0.0044	(-6.63)
Loss		-0.0096	(-22.58)	-0.0082	(-15.38)	-0.0096	(-22.65)	-0.0063	(-9.56)
NPL		-0.2431	(-51.60)	-0.2359	(-40.35)	-0.2405	(-51.28)	-0.2459	(-34.29)
Liquid assets		0.0212	(29.14)	0.0193	(25.52)	0.0208	(28.71)	0.0169	(21.41)
Internal CF		-0.2071	(-19.96)	-0.2024	(-17.52)	-0.2049	(-19.83)	-0.2016	(-15.69)
Economy-wide and other factors		Yes		Yes		Yes		Yes	
Number of observations		327,084		327,084		327,084		327,084	
Adj. R^2		0.2132		0.2139		0.2202		0.2222	
F-stat. and p-value for the joint significance of the five lagged *TightMP × Audited*						38.53 (0.001)		17.82 (0.001)	

Table 2.5: Further tests of H1 and H2

This table presents results on further tests of the differential responses to contractionary monetary policy (*TightMP*) between audited and unaudited banks (see Section 2.5 for specific details). For ease of exposition, except for column (4), only the sum of the coefficients on the interaction terms between monetary policy and the audit indicator (*TightMP* × *Audited*) is reported. Panel A presents the results for the quarterly growth of managed liabilities (*ML_Growth*), while Panel B shows the loan regression results (*Loan_Growth*). See Tables 2.3 and 2.4 for model specifications. The t-statistics in parentheses are computed from robust standard errors clustered by bank. Regression (1) is the benchmark regression, as shown in column (4) of Tables 2.3 and 2.4. Regression (2) is the regression with additional loan composition control variables. Regression (3) includes additional interaction terms between each of the bank-level characteristics and the GDP growth variables. Regression (4) is the second stage regression based on the Heckman two-stage procedure. Appendix D provides the first stage probit regression results. Regression (5) uses the predicted probability of an audit from the probit model as an instrument for *Audited*.

	Benchmark results (1)		Additional loan-related controls (2)		Interacting bank attributes with GDP growth (3)		Heckman two-stage test* (4)		Using predicted audit probability* (5)	
	Coef.	t-stat.	Coef.	t-stat.	Coef.	t-stat.	Coef.	t-stat.	Coef.	t-stat.
Panel A. ML_Growth (sum of coef.)										
TightMP × Audited	**0.0193**	**(6.46)**	**0.0187**	**(6.17)**	**0.0220**	**(5.32)**	**0.0238**	**(3.21)**	**0.0233**	**(3.13)**
TightMP × IMR							-0.0100	(-1.75)		
Number of observations	327,084		327,084		327,084		146,704		146,704	
Proportion audited	0.54		0.54		0.54		0.52		0.52	
Adj. R^2	0.0678		0.0697		0.0693		0.0599		0.0599	
Panel B. Loan_Growth (sum of coef.)										
TightMP × Audited	**0.0052**	**(7.61)**	**0.0050**	**(6.95)**	**0.0062**	**(6.32)**	**0.0066**	**(4.18)**	**0.0064**	**(4.38)**
TightMP × IMR							-0.0022	(-1.78)		
Number of observations	327,084		327,084		327,084		146,704		146,704	
Proportion audited	0.54		0.54		0.54		0.52		0.52	
Adj. R^2	0.2222		0.2343		0.2239		0.2235		0.2234	

Further tests of H1 and H2

Regression (6) studies only the group of banks that changed audit status during the sample period. Regression (7) is a bank-fixed effects regression. It includes only the group of banks that had a constant audit status throughout the sample period. Finally, regressions (8), (9), and (10) include subsamples of banks of different sizes.

	Audit-switched banks (6)		Constant-status banks and bank-fixed effects (7)		Size category A: $25 M – 50 M (8)		Size category B: $50 M – 100 M (9)		Size category C: $100 M – 300 M (10)	
	Coef.	t-stat.	Coef.	t-stat.	Coef.	t-stat.	Coef.	t-stat.	Coef.	t-stat.
Panel A. ML_Growth (sum of coef.)										
TightMP × Audited	**0.0156**	**(4.57)**	**0.0281**	**(6.85)**	**0.0285**	**(5.36)**	**0.0163**	**(3.04)**	**0.0092**	**(1.44)**
Number of observations	120,117		206,967		96,150		91,975		60,336	
Proportion audited	0.46		0.59		0.47		0.61		0.81	
Adj. R^2	0.0657		0.1035		0.0746		0.0793		0.0823	
Panel B. Loan_Growth (sum of coef.)										
TightMP × Audited	**0.0035**	**(3.15)**	**0.0066**	**(7.93)**	**0.0066**	**(4.18)**	**0.0046**	**(2.95)**	**0.0036**	**(1.78)**
Number of observations	120,117		206,967		96,150		91,975		60,336	
Proportion audited	0.46		0.59		0.47		0.61		0.81	
Adj. R^2	0.2375		0.2712		0.2420		0.2263		0.1902	

* The required data for the first stage probit prediction model is only available for 36,676 bank years. 180,380 (=327,084 – (36,676*4)) bank-quarters are therefore excluded from the tests.

Table 2.6: Alternative proxies for monetary policy stance

This table reports results from regressing the quarterly growth of managed liabilities (columns (1) and (2)) or total loans (columns (3) and (4)) on the contractionary policy variables (*TightMP*), an audit indicator for the audit status in the previous year (*Audited*), the policy-audit interaction terms (*TightMP*×*Audited*), and various control variables. See regression (4) in Tables 2.3 and 2.4 for model specification. In columns (1) and (3), *TightMP* is set to one if the quarterly change of the spread between the federal funds rate and the ten-year Treasury bond rate (*ΔFF-Bond*) is positive. In columns (2) and (4), *TightMP* is alternatively set to the value of *ΔFF-Bond* (in decimal form). The t-statistics in parentheses are computed from robust standard errors clustered by bank. See Section 2.5.7 for more details.

		ML_Growth					*Loan_Growth*			
	Pred. Sign	TightMP = 1 if ΔFF-Bond > 0 (1)		TightMP = ΔFF-Bond (2)		Pred. Sign	TightMP = 1 if ΔFF-Bond > 0 (3)		TightMP = ΔFF-Bond (4)	
		Coef.	t-stat.	Coef.	t-stat.		Coef.	t-stat.	Coef.	t-stat.
Intercept		0.0366	(1.91)	-0.0288	(-1.60)		0.0573	(11.24)	0.0208	(4.79)
Monetary policy variables (sum of coef.)										
TightMP	+	0.0380	(1.72)	1.8987	(1.03)	-	-0.0785	(-14.93)	-4.7243	(-10.75)
TightMP × Audited	**+**	**0.0201**	**(5.44)**	**1.6606**	**(5.13)**	**+**	**0.0056**	**(6.82)**	**0.4346**	**(6.03)**
TightMP × Ln(TA)	+	0.0058	(2.75)	0.6818	(3.88)	+	0.0041	(8.05)	0.1461	(3.49)
TightMP × OBHC	?	-0.0069	(-2.02)	-0.2599	(-0.93)	?	0.0002	(0.26)	-0.0256	(-0.39)
TightMP × MSA	+	0.0319	(8.96)	1.8955	(6.37)	+	0.0091	(10.72)	0.6561	(9.27)
TightMP × LowCap	-	-0.0361	(-4.79)	-2.6220	(-4.44)	-	-0.0088	(-4.90)	-0.7365	(-5.23)
TightMP × Loss	-	-0.0221	(-2.96)	-1.5979	(-2.59)	-	-0.0086	(-4.87)	-0.4165	(-2.83)
TightMP × NPL	-	-0.0064	(-0.08)	-0.0454	(-0.70)	-	-0.0112	(-0.58)	0.0115	(0.75)
TightMP × Liquid assets	-	-0.0987	(-19.61)	-0.0494	(-5.47)	+	0.0213	(17.30)	0.0135	(6.29)
TightMP × Internal CF	-	-0.2987	(-4.10)	0.0158	(0.12)	+	0.0079	(0.46)	0.0856	(2.74)
Bank-level variables		Yes		Yes			Yes		Yes	
Economy-wide and other factors		Yes		Yes			Yes		Yes	
Number of observations		327,084		327,084			327,084		327,084	
Adj. R^2		0.0653		0.0660			0.2110		0.1962	
F-stat. and p-value for the joint significance of the five lagged *TightMP* × *Audited*		8.15 (0.001)		7.07 (0.001)			16.53 (0.001)		14.93 (0.001)	

Table 2.7: Descriptive statistics of the loan loss provision regression variables

This table reports the distribution of the loan loss provision regression variables for all banks (Panel A), and separately for audited banks and unaudited banks (Panel B). The loan loss provisions regression is estimated using annual data. The sample period spans from 1987 to 1999, corresponding to the period when the audit status of a bank is measured in the tests of H1 (Table 2.3) and H2 (Table 2.4). After applying the same sample selection criteria as that reported in Table 2.1, 72,584 firm-years had the required data. LLP_t indicates the ratio of loan loss provision for year *t* to beginning of year total loans; ΔNPL_t indicates the ratio of nonperforming loans to total loans at the end of year *t* minus the same ratio for year *t-1*; NCO_t indicates the ratio of net loan charge-offs for year *t* to beginning of year total loans; $Ln(TA)_t$ indicates the natural log of total assets at the end of year *t*; LLA_{t-1} indicates the ratio of loan loss allowance for year *t-1* to total loans at the end of year *t-1*; HOM_{t-1} indicates homogenous loans (captured by the proportion of small or infrequently renegotiated loans in total loans) at the end of year t-1; $Loan\ growth_t$ indicates the loan growth rate for year *t*; $Equity_t$ indicates the ratio of total equity capital to total assets at the end of year *t*. Appendix B Panel D provides the details on variable measurement.

Panel A: Distribution of the loan loss provision regression variables for the full sample (N=72,584)

	Mean	Std.	Q1	Median	Q3
LLP_t	0.005	0.008	0.001	0.003	0.006
ΔNPL_t	-0.001	0.014	-0.006	0.000	0.004
NCO_t	0.006	0.008	0.001	0.003	0.008
$Ln(TA)_t$	10.772	0.855	10.173	10.758	11.354
LLA_{t-1}	0.017	0.009	0.011	0.014	0.020
HOM_{t-1}	0.178	0.112	0.096	0.153	0.234
Loan $growth_t$	0.098	0.030	0.077	0.091	0.112
$Equity_t$	0.085	0.122	0.010	0.075	0.147
Audit $indicator_t$	0.532	0.499	0.000	1.000	1.000

Descriptive statistics of the loan loss provision regression variables

Panel B: Summary statistics separately for audited banks (N=38,599) and unaudited banks (N=33,985)

	Audited sample		Unaudited sample	
	Mean	Median	Mean	Median
LLP_t	0.006	0.003	0.005	0.002
ΔNPL_t	-0.001	0.001	-0.001	0.001
NCO_t	0.007	0.004	0.006	0.003
$Ln(TA)_t$	11.059	11.067	10.445	10.447
LLA_{t-1}	0.016	0.014	0.017	0.014
HOM_{t-1}	0.184	0.162	0.171	0.145
Loan $growth_t$	0.094	0.089	0.102	0.094
$Equity_t$	0.090	0.078	0.080	0.073

Table 2.8: Comparing the timeliness of current loan loss provisions relative to the current and future changes in nonperforming loans across audited and unaudited banks

This table reports results from regressing annual current period loan loss provisions (LLP_t) on changes in nonperforming loans in current and future periods (i.e., $\Delta NPL_{\ t}$ to $\Delta NPL_{\ t+2}$), their interaction terms with an audit indicator ($Audited_t$), and various control variables:

$$\begin{aligned}\mathrm{LLP}_{it} = {} & \alpha + \textstyle\sum_{j=0}^{2} \beta_j \Delta\mathrm{NPL}_{it+j} + \gamma\ \mathrm{Audited}_{it} + \sum_{j=0}^{2} \delta_j \mathrm{Audited}_{it} \times \Delta\mathrm{NPL}_{it+j} \\ & + \zeta\ \Delta\mathrm{NPL}_{it-1} + \textstyle\sum_{j=0}^{2} \eta_j \mathrm{NCO}_{it+j} + \theta\ \mathrm{Audited}_{it} \times \Delta\mathrm{NPL}_{it-1} + \sum_{j=0}^{2} \iota_j\ \mathrm{Audited}_{it} \times \mathrm{NCO}_{it+j} \\ & + \kappa\ \mathrm{Ln(TA)}_{it} + \lambda\ \mathrm{LLA}_{it-1} + \mu\ \mathrm{HOM}_{it-1} + \nu\ \mathrm{Loan\ growth}_{it} + \xi\ \mathrm{Equity}_{it} \\ & + \textstyle\sum_{j=1}^{50} \mathrm{o}_j\ \mathrm{State}_{ij} + \sum_{j=87}^{98} \pi_j\ \mathrm{Year}_{jt} + \varepsilon_{\mathrm{it}}\end{aligned}$$

Refer to Table 2.7 for sample selection process and variable definitions. Panel A shows the main regression results. Panel B reports the results for further tests. Regression (1) includes additional controls for the effect of bank size and credit risk on the *LLP*-ΔNPL associations. Specifically, the regression includes additional interaction terms between (i) each of the seven variables relating to changes in nonperforming loans or net loan charge-offs (i.e., ΔNPL_{t-1} to ΔNPL_{t+1}, and NCO_t to NCO_{t+2}) and (ii) each of *Ln(TA)*, *Loangrowth*, and *Equity*. Regression (2) studies only the group of banks that changed audit status during the sample period. Regression (3) is a bank-fixed effects regression. In this test, only the group of banks that had a constant audit status throughout the sample period are included. Regression (4) is the second stage regression based on the Heckman two-stage procedure. See Appendix D for the first stage probit regression results. Regression (5) uses the predicted probability of an audit from the probit model as an instrument for *Audited*. For brevity, Panel B reports only the coefficients associated with the interaction terms between the audit indicator and the changes in nonperforming loans in current and future periods. In both panels, the t-statistics in parentheses are computed from robust standard errors clustered by bank.

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Comparing the timeliness of current loan loss provisions relative to the current and future changes in nonperforming loans across audited and unaudited banks

Panel A: Main results

Variable	Predicted sign	Coefficient estimate	t-stat.
Intercept	?	0.006	(7.79)
ΔNPL_t	+	0.076	(12.56)
ΔNPL_{t+1}	+	0.005	(0.89)
ΔNPL_{t+2}	+	-0.009	(-1.65)
Audited_t	?	0.001	(1.45)
$\mathbf{Audited_t \times \Delta NPL_t}$	**+**	**0.030**	**(3.50)**
$\mathbf{Audited_t \times \Delta NPL_{t+1}}$	**+**	**0.023**	**(2.90)**
$\mathbf{Audited_t \times \Delta NPL_{t+2}}$	**+**	**0.004**	**(0.58)**
Other control variables:			
ΔNPL_{t-1}	+	0.051	(11.03)
NCO_t	+	0.710	(102.90)
NCO_{t+1}	+	0.124	(20.89)
NCO_{t+2}	+	0.039	(7.08)
$\text{Audited}_t \times \Delta\text{NPL}_{t-1}$	?	0.009	(1.37)
$\text{Audited}_t \times \text{NCO}_t$	?	-0.015	(-1.64)
$\text{Audited}_t \times \text{NCO}_{t+1}$	?	0.023	(2.71)
$\text{Audited}_t \times \text{NCO}_{t+2}$	?	-0.009	(-1.19)
Ln(TA)_t	?	0.001	(3.66)
LLA_{t-1}	-	-0.431	(-48.43)
HOM_{t-1}	-	-0.006	(-10.98)
Loan growth_t	+	0.006	(16.92)
Equity_t	+	0.001	(0.29)
State and year indicators		Yes	
Number of observations		72,584	
Proportion audited		0.53	
Adj. R^2		0.719	
The sum of ΔNPL and $Audited \times \Delta NPL$			
$\mathbf{\Delta NPL_t + Audited_t \times \Delta NPL_t}$		**0.105**	**(17.44)**
$\mathbf{\Delta NPL_{t+1} + Audited_t \times \Delta NPL_{t+1}}$		**0.028**	**(4.93)**
$\mathbf{\Delta NPL_{t+2} + Audited_t \times \Delta NPL_{t+2}}$		**-0.005**	**(-0.91)**

Comparing the timeliness of current loan loss provisions relative to the current and future changes in nonperforming loans across audited and unaudited banks

Panel B: Further tests

		Further conditioning the *LLP*-ΔNPL associations on *Ln(TA)*, *Loan growth*, and *Equity* (1)		Audit-switched banks (2)		Constant-status banks and bank-fixed effects (3)		Heckman two-stage test (4)		Using predicted audit probability as an instrument (5)	
Variable	Pred. sign	Coef.	t-stat.	Coef.	t-stat.	Coef.	t-stat.	Coef.	t-stat.	Coef.	t-stat.
Intercept	?	0.005	(6.26)	0.005	(4.83)	-	-	0.003	(2.70)	0.003	(2.550)
ΔNPL_t	+	0.092	(2.07)	0.075	(15.41)	0.033	(8.73)	0.067	(7.87)	0.059	(6.040)
ΔNPL_{t+1}	+	0.160	(3.74)	0.014	(2.87)	-0.017	(-4.19)	-0.003	(-0.37)	-0.013	(-1.340)
ΔNPL_{t+2}	+	0.077	(1.96)	-0.001	(-0.32)	-0.019	(-5.20)	-0.010	(-1.54)	-0.020	(-2.480)
$Audited_t$	?	0.001	(3.08)	0.001	(2.63)	-	-	0.000	(1.41)	-0.000	(-0.910)
$Audited_t \times \Delta NPL_t$	**+**	**0.023**	**(3.26)**	**0.016**	**(2.27)**	**0.034**	**(6.77)**	**0.043**	**(3.46)**	**0.058**	**(3.670)**
$Audited_t \times \Delta NPL_{t+1}$	**+**	**0.026**	**(3.68)**	**0.010**	**(1.80)**	**0.032**	**(6.16)**	**0.040**	**(3.33)**	**0.059**	**(3.700)**
$Audited_t \times \Delta NPL_{t+2}$	**+**	**0.003**	**(0.50)**	**0.004**	**(0.69)**	**0.010**	**(1.99)**	**0.027**	**(2.67)**	**0.046**	**(3.330)**
Other control variables		Yes		Yes		Yes		Yes		Yes	
State and year indicators		Yes		Yes		Yes		Yes		Yes	
Number of observations		72,584		25,891		46,693		34,677		34,677	
Proportion audited		0.53		0.46		0.57		0.51		0.51	
Adj. R^2		0.757		0.733		0.844		0.633		0.633	
The sum of ΔNPL and $Audited \times \Delta NPL$											
ΔNPL_t + $Audited_t \times \Delta NPL_t$		**0.115**	**(2.48)**	**0.092**	**(15.95)**	**0.067**	**(20.90)**	**0.110**	**(12.22)**	**0.117**	**(11.19)**
ΔNPL_{t+1} + $Audited_t \times \Delta NPL_{t+1}$		**0.186**	**(4.14)**	**0.023**	**(4.33)**	**0.015**	**(4.44)**	**0.037**	**(4.23)**	**0.046**	**(4.48)**
ΔNPL_{t+2} + $Audited_t \times \Delta NPL_{t+2}$		**0.080**	**(1.93)**	**0.003**	**(0.62)**	**-0.010**	**(-2.98)**	**0.017**	**(2.09)**	**0.026**	**(2.75)**

Table 3.1: Sample construction and information on pre-crisis bank borrowing

Panel A: Sample selection

The sample is collected from the intersection of (1) First Call's Company Issued Guidelines database, (2) the DealScan database, and (3) the CRSP/COMPUSTAT database. The final sample includes 1,111 distinct firms. The table below describes the firm selection process.

		Sample size
Non-banking firms that have at least one management forecast before or during the two-year pre-crisis period (1995:Q3 – 1997:Q2) according to First Call		1,994
Less:		
Firms not covered by DealScan	(623)	
Firms with no bank loans reported in DealScan during the six-year measurement window from 1991:Q3 to 1997: Q2	(114)	
Active borrowers		1,257
Less:		
Firms with multiple main banks identified	(33)	
Borrowers with a single main bank		1,224
Less:		
Firms incorporated outside the U.S. or firms with a foreign main bank	(113)	
Borrowers used in the analyses		**1,111**
Classification of borrowers		
Borrowers of exposed banks		592
Other borrowers		519

Sample construction and information on pre-crisis bank borrowing

Panel B: List of main banks as of 1997:Q2, and distribution of sample bank borrowers

Bank	Number of borrowers	% of borrowers
Exposed banks (per Kho et al. 2000)[71]		
BankAmerica	104	17.57%
BankBoston	42	7.09%
Bankers Trust	27	4.56%
Bank of New York	30	5.07%
Chase Manhattan	146	24.66%
Citicorp	49	8.28%
Comerica	20	3.38%
Cullen/Frost Bankers	1	0.17%
First Chicago NBD	66	11.15%
First Tennessee National	3	0.51%
Fleet Financial	29	4.90%
J.P. Morgan	57	9.63%
Republic New York	1	0.17%
Wachovia	17	2.87%
	592	100%
Other banks		
Banc One	20	3.85%
Firstar	11	2.12%
First Union	49	9.44%
KeyCorp	11	2.12%
Mellon Financial	11	2.12%
National City	10	1.93%
NationsBank	113	21.77%
PNC Financial	16	3.08%
SunTrust Bank	19	3.66%
Wells Fargo	44	8.48%
Other	215	41.43%
	519	100%

[71] Kho et al. (2000) do not provide exposure information for First Chicago NBD, as the bank had already been acquired by NationsBank at the time when the authors collected the data. I treat First Chicago NBD as an exposed bank. According to the 1997 Country Exposure Reports of the Federal Financial Institutions Examination Council, First Chicago NBD comprised the group of banks that had the largest emerging-market exposures (i.e., "Money Center Banks"). On the other hand, I treat NationsBank as not exposed. First, NationsBank was not among the "Money Center Banks" in the Country Exposure Reports. Second, NationsBank had significantly fewer foreign activities than the exposed banks. For example, in fiscal year 1998, NationsBank's foreign assets comprised only about 4 percent of total assets, while that figure for exposed banks typically exceeded 30 percent (Houpt 1999).

Sample construction and information on pre-crisis bank borrowing

Panel C: Pre-crisis bank borrowing for the full sample (N=1,111)

This panel shows bank borrowing information for the sample firms in the six-year measurement window that ends in 1997:Q2. $LoanReli_{ik}$ measures the reliance of borrower *i* on its main bank *k* for loans. It is computed as follows:

$$LoanReli_{ik} = \frac{Total\ loan\ amount\ firm\ i\ borrowed\ from\ bank\ k\ in\ the\ 6-year\ window}{Total\ loan\ amount\ firm\ i\ borrowed\ in\ the\ 6-year\ window}$$

The higher the measure is, the greater financing reliance of the borrower on its main bank is assumed. See Section 3.3.4 for further details.

	Mean	Std. Dev.	Q1	Median	Q3
Total number of loan deals	2.897	2.012	1.000	2.000	4.000
Total number of loan facilities	3.877	3.022	2.000	3.000	5.000
Total loan amount ($ millions)	759.69	2195.58	30.00	160.00	560.00
Reliance on main bank for loans (*LoanReli*)	0.893	0.165	0.813	1.000	1.000

Panel D: Borrowing information separately for borrowers of exposed banks and other borrowers

	Borrowers of exposed banks (N=592)		Other borrowers (N=519)	
	Mean	Median	Mean	Median
Total number of loan deals	3.309	3.000	2.428	2.000
Total number of loan facilities	4.459	4.000	3.212	2.000
Total loan amount ($ millions)	1202.98	350.00	254.05	55.00
Reliance on main bank for loans (*LoanReli*)	0.890	1.000	0.897	1.000

Table 3.2: Industry distribution and firm characteristics

Panel A: Industry distribution of sample firms, by borrower type

Industry (per Fama and French 1997)	Number of borrowers of exposed banks	% of borrowers	Number of other borrowers	% of borrowers
Apparel	13	2.20%	7	1.35%
Automobiles and Trucks	13	2.20%	6	1.16%
Business Services	45	7.60%	64	12.33%
Business Supplies	14	2.36%	4	0.77%
Chemicals	20	3.38%	6	1.16%
Computers	29	4.90%	34	6.55%
Construction	5	0.84%	10	1.93%
Construction Materials	13	2.20%	8	1.54%
Consumer Goods	21	3.55%	12	2.31%
Electrical Equipment	8	1.35%	7	1.35%
Electronic Equipment	31	5.24%	36	6.94%
Entertainment	10	1.69%	16	3.08%
Food Products	10	1.69%	12	2.31%
Healthcare	13	2.20%	24	4.62%
Insurance	22	3.72%	6	1.16%
Machinery	23	3.89%	24	4.62%
Measuring and Control Equipment	14	2.36%	6	1.16%
Medical Equipment	15	2.53%	21	4.05%
Petroleum and Natural Gas	15	2.53%	11	2.12%
Pharmaceutical Products	12	2.03%	8	1.54%
Printing and Publishing	14	2.36%	5	0.96%
Recreational Products	5	0.84%	10	1.93%
Restaurants, Hotels, Motels	12	2.03%	13	2.50%
Retail	58	9.80%	47	9.06%
Rubber and Plastic Products	11	1.86%	2	0.39%
Steel Works, Etc.	14	2.36%	14	2.70%
Telecommunications	12	2.03%	11	2.12%
Textiles	10	1.69%	5	0.96%
Trading	11	1.86%	6	1.16%
Transportation	19	3.21%	15	2.89%
Utilities	9	1.52%	4	0.77%
Wholesale	27	4.56%	33	6.36%
Other*	44	7.43%	32	6.17%
	592	100%	519	100%

* Other industries include the 15 industries that comprise less than one percent of the final sample of 1,111 firms: Agriculture, Aircraft, Alcoholic Beverages, Candy & Soda, Coal, Defense, Fabricated Products, Miscellaneous, Non-Metallic Mining, Personal Services, Precious Metals, Real Estate, Shipbuilding and Railroad Equipment, Shipping Containers, and Tobacco Products.

Panel B: Summary statistics as of 1997:Q1, by borrower type

	Borrowers of exposed banks (N=592)					Other borrowers (N=519)					p-values of the difference	
	Mean	Median	Q1	Q3	Std.	Mean	Median	Q1	Q3	Std.	Mean	Median
Market value of equity (1997 $ million)	2,042	628	194	2,499	2,971	552	182	66	522	1,236	0.001	0.001
Log(market value) *(Ln(MV))*	6.430	6.446	5.234	7.796	1.732	5.152	5.081	4.095	6.135	1.421	0.001	0.001
Market-to-book (*MTB*)	2.923	2.218	1.376	3.661	2.242	2.785	2.082	1.359	3.241	2.180	0.307	0.174
Asset growth *(ATGROWTH)*	0.027	0.016	-0.017	0.061	0.090	0.028	0.024	-0.028	0.078	0.097	0.839	0.419
Return on assets (*ROA*)	0.007	0.011	0.002	0.022	0.029	0.003	0.011	-0.004	0.024	0.036	0.060	0.684
Loss dummy (*LOSSDUM*)	0.218	0.000	0.000	0.000	0.413	0.237	0.000	0.000	0.000	0.426	0.460	0.460
Abnormal return (*BHAR*)	-0.020	-0.036	-0.128	0.080	0.180	-0.036	-0.038	-0.189	0.101	0.224	0.216	0.207
Restructuring indicator *(RESTRUCT)*	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	--	--
Beta *(BETA)*	0.881	0.815	0.522	1.202	0.492	0.871	0.788	0.457	1.278	0.552	0.775	0.429
Cash flow volatility *(CFVOL)*	0.064	0.057	0.044	0.076	0.029	0.064	0.057	0.040	0.079	0.033	0.960	0.381
Return volatility (*RETVOL*)	0.027	0.023	0.016	0.035	0.015	0.036	0.034	0.023	0.045	0.016	0.001	0.001

Notes to Panel B and Panel C: See Table 3.1 for the selection process of sample firms. Panel B reports summary firm characteristics as of 1997:Q1. Panel C provides descriptive statistics for 16,555 firm-quarters (in the period of 1995:Q3 – 1999:Q2) that have the required financial data. Summary statistics are calculated using available observations. T-tests (Wilcoxon rank tests) are used to test the difference between the comparison groups in means (medians). Market value of equity are measured in the beginning of the quarter and reported in millions of chained (1997) dollars. *Ln(MV)* is the natural logarithm of beginning of quarter market value of firm equity. *MTB* is the beginning of quarter market-to-book ratio. *ATGROWTH* is growth of total assets in the previous quarter. *ROA* is the lagged return on assets. *LOSSDUM* is an indicator set to one for firms that report losses in the last quarters. *BHAR* is the buy-and-hold size adjusted return measured over the prior 3 months. *RESTRUCT* is an indicator set to one if the firm recognized restructuring charges in the previous quarter. *BETA* is the lagged equity beta. *CFVOL* is cash flow volatility. *RETVOL* is the volatility of the gross stock return over the prior 3 months. *MF_DUM* is an indicator variable set to one for firms that issue at least one management forecast in the current quarter. *MF* is the number of management forecasts issued in the current quarter. See Appendix E for variable definitions.

Industry distribution and firm characteristics

Panel C: Summary statistics for sample firm-quarters by borrower type, and by time period

	Borrowers of exposed banks					Other borrowers					p-values of the difference in the changes
	Pre-crisis period		Crisis period		p-values of the change	Pre-crisis period		Crisis period		p-values of the change	
	N	Mean	N	Mean		N	Mean	N	Mean		
Market value of equity (1997 $ million)	4475	1940	4235	2413	0.001	3867	515	3667	647	0.001	0.001
Log(market value) *(Ln(MV))*	4475	6.488	4235	6.534	0.220	3867	5.259	3667	5.344	0.666	0.231
Market-to-book (*MTB*)	4442	3.015	4200	2.822	0.031	3861	2.987	3669	2.726	0.001	0.162
Asset growth *(ATGROWTH)*	4454	0.033	4181	0.022	0.001	3889	0.044	3591	0.023	0.001	0.001
Return on assets (*ROA*)	4562	0.010	4269	0.007	0.001	3997	0.007	3705	0.002	0.001	0.106
Loss indicator (*LOSSDUM*)	4562	0.184	4269	0.220	0.001	3997	0.213	3705	0.258	0.001	0.515
Abnormal return (*BHAR*)	4359	-0.013	4154	-0.021	0.044	3824	-0.019	3609	-0.023	0. 579	0.482
Restructuring indicator *(RESTRUCT)*	4533	0.000	4192	0.000	--	3986	0.000	3606	0.000	--	--
Beta *(BETA)*	4361	0.886	4173	0.767	0.001	3828	0.854	3649	0.729	0.001	0.757
Cash flow volatility *(CFVOL)*	4474	0.062	4214	0.063	0.545	3940	0.063	3688	0.061	0.108	0.118
Return volatility (*RETVOL*)	4357	0.027	4153	0.031	0.001	3821	0.035	3609	0.039	0.001	0.155
Forecast indicator *(MF_DUM)*	4566	0.192	4279	0.249	0.001	4003	0.188	3707	0.181	0.371	0.001
Forecast frequency *(MF)*	4566	0.240	4279	0.391	0.001	4003	0.226	3707	0.264	0.055	0.001

Table 3.3: H1: Differential change in forecast propensity for borrowers of exposed banks following the crisis events

This table reports the regression results of the following firm-fixed effects model:

$$Pr(MF\_DUM_{it} = 1) = \alpha_i + \beta\ Crisis_t + \gamma\ Crisis_t \times ExpoMBK_i + \delta\ Controls_{it-1} + \varepsilon_{it} \quad (1)$$

MF_DUM_{it} is an indicator variable that equals one if borrower *i* issues at least one management forecast during quarter *t*. α_i is firm-fixed effects. $Crisis_t$ is the indicator for the financial crisis period (1997:Q3 – 1999:Q2). The indicator variable $ExpoMBK_i$ identifies whether borrower *i*'s main bank was exposed to the crisis areas. Since $ExpoMBK_i$ is time-invariant for each firm, its main effect is subsumed by the firm fixed effect. Thus, the model does not include $ExpoMBK_i$ separately. The control variables ($Controls_{it-1}$) are measured at one lag, and include variables that capture with-in firm changes in size (*Ln(MV)*), growth opportunities (*MTB*, *ATGROWTH*), performance (*ROA*, *LOSSDUM*, *BHAR*), and risk and uncertainty (*BETA*, *CFVOL*, *RETVOL*). See notes to Table 3.2 Panel B or Appendix E for the details of these variables. The regressions are based on 16,555 available firm-quarters in the period of 1995:Q3 – 1999:Q2. The test statistics reported in parentheses are computed using robust standard errors adjusted for firm-level clustering.

		Logit Model (1)		Logit Model (2)		Linear Prob. Model (3)	
	Predict	Coef.	z-stat	Coef.	z-stat	Coef.	t-value
Crisis	?	-0.072	(-1.096)	-0.051	(-0.778)	-0.002	(-0.176)
Crisis × ExpoMBK	**+**	**0.399**	**(4.408)**	**0.344**	**(4.047)**	**0.053**	**(3.990)**
Firm Controls							
Ln(MV)				0.707	(11.050)	0.102	(11.125)
MTB				-0.048	(-2.753)	-0.006	(-2.348)
ATGROWTH				-0.195	(-0.830)	-0.027	(-0.745)
ROA				-2.918	(-3.165)	-0.417	(-2.883)
LOSSDUM				-0.015	(-0.245)	-0.004	(-0.403)
BHAR				-0.920	(-8.233)	-0.139	(-8.255)
BETA				0.018	(0.263)	0.004	(0.349)
CFVOL				-0.073	(-0.056)	-0.008	(-0.040)
RETVOL				-1.724	(-0.730)	-0.060	(-0.167)
Firm fixed effects		Yes		Yes		Yes	
Number of firm-quarters		16,555		15,328		15,328	
Wald χ^2 (p-value)		61.97	(0.001)	246.99	(0.001)	-	-
Adjusted R^2		-		-		0.133	

Table 3.4: Variations in the change in management forecast propensity within each borrower group

This table reports the regression results of the following firm-fixed effects model:

$$Pr(MF\_DUM_{it} = 1) = \alpha_i + \beta\ Crisis_t + \gamma\ Crisis_t \times Condition\_Var_i + \delta\ Controls_{it-1} + \varepsilon_{it} \quad (1b)$$

MF_DUM_{it} is an indicator variable that equals one if borrower *i* issues at least one management forecast during quarter *t*. α_i is firm-fixed effects. $Crisis_t$ is the indicator for the financial crisis period (1997:Q3 – 1999:Q2). The pre-crisis period is used as the benchmark. $Condition\_Var_i$ is a time-invariant conditioning variable for each firm, so its main effect is subsumed by the firm fixed effect. Accordingly, the model does not include $Condition\_Var_i$ separately. $Condition\_Var_i$ takes one of the following three forms: $ExpoMBK_{HighExpo_i}$ is the indicator for borrowers of the more exposed banks. These banks include BankAmerica, Chase Manhattan, Citicorp, First Chicago NBD, and J.P. Morgan; $Tangibility_i$ is the decile rank of the ratio of tangible assets to total assets at the start of fiscal year 1997. Tangible assets include property, plant and equipments plus inventories; and $R\&D\_firm_i$ is the indicator for exposed bank borrowers with research and development activities before the crisis events. All tests include control variables ($Controls_{it-1}$), measured at one lag. See Appendix E for the definition of these variables. For simplicity, the coefficients on control variables are not reported. The regressions for the borrowers of exposed banks (other borrowers) are based on 8,116 (7,212) available firm-quarters in the period of 1995:Q3 – 1999:Q2. The test statistics reported in parentheses are computed using robust standard errors adjusted for firm-level clustering.

		Borrowers of exposed banks						Other borrowers			
	Predict	(1)	(2)	(3)	(4)	(5)	Predict	(1a)	(2a)	(3a)	(4a)
Crisis	+	0.289 (4.476)	0.092 (0.819)	0.539 (4.514)	0.157 (1.906)	0.197 (1.165)	?	-0.052 (-0.748)	-0.120 (-0.904)	-0.096 (-1.111)	-0.239 (-1.431)
Crisis × ExpoMBK$_{HighExpo}$	**+**		**0.272** **(2.127)**			**0.255** **(1.989)**					
Crisis × Tangibility	**−**			**-0.049** **(-2.503)**		**-0.040** **(-2.005)**	**0**		**0.013** **(0.603)**		**0.024** **(1.000)**
Crisis × R&D_firm	**+**				**0.291** **(2.534)**	**0.247** **(2.121)**	**0**			**0.113** **(0.860)**	**0.167** **(1.177)**
Firm Controls		Yes	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Firm fixed effects		Yes	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Number of firm-quarters		8,116	8,116	8,116	8,116	8,116		7,212	7,212	7,212	7,212
Wald χ^2		136.69	140.94	142.27	142.63	150.35		120.50	120.77	121.14	121.93
(p-value)		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)		(0.001)	(0.001)	(0.001)	(0.001)

Table 3.5: Differential change in forecast tendency for borrowers of exposed banks in other time periods

This table reports the regression results of the following firm-fixed effects model:

$$Pr(MF\_DUM_{it} = 1) = \alpha_i + \beta\ After_t + \gamma\ After_t \times ExpoMBK_i + \delta\ Controls_{it-1} + \varepsilon_{it} \quad (1c)$$

MF_DUM_{it} is an indicator variable that equals one if borrower *i* issues at least one management forecast during quarter *t*. α_i is firm-fixed effects. $After_t$ is the indicator for the second half of the specified two-year period. The indicator variable $ExpoMBK_i$ identifies whether borrower *i*'s main bank was exposed to the crisis areas. Since $ExpoMBK_i$ is time-invariant for each firm, its main effect is subsumed by the firm fixed effect. Thus, the model does not include $ExpoMBK_i$ separately. All tests include control variables ($Controls_{it-1}$), measured at one lag. See Appendix E for the definition of these variables. For simplicity, the coefficients on control variables are not reported. The regressions are based on available firm-quarters in the specified two-year period. Section 3.4.5 provides more information about each of these additional tests. The test statistics reported in parentheses are computed using robust standard errors adjusted for firm-level clustering.

		Post-crisis reversal period		Non-crisis periods					
		Two years around July 1 of 1999 (1)		Two years around July 1 of 1996 (2)		Two years around July 1 of 2003 (3)		Two years around July 1 of 2004 (4)	
	Predict	Coef.	z-stat.	Coef.	z-stat.	Coef.	z-stat.	Coef.	z-stat.
After	?	0.116	(1.110)						
After × ExpoMBK	**−**	**-0.262**	**(-1.966)**						
After	?			0.118	(1.347)	-0.093	(-0.604)	-0.166	(-0.953)
After × ExpoMBK	**0**			**-0.053**	**(-0.445)**	**-0.280**	**(-1.491)**	**-0.062**	**(-0.289)**
Firm Controls		Yes		Yes		Yes		Yes	
Firm fixed effects		Yes		Yes		Yes		Yes	
Number of firm-quarters		6,622		7,907		4,452		4,217	
Wald χ^2 (p-value)		54.04	(0.001)	87.08	(0.001)	51.22	(0.001)	27.21	(0.001)

Table 3.6: Results of robustness checks

This table reports the regression results of three robustness checks for the main findings shown in Table 3.3. Section 3.4.5 provides more information about each of these additional tests. Regression (1) is a logit regression that excludes firm-fixed effects. Regression (2) uses a more restricted sample of 865 borrowers. This sample excludes all firms with aggregate sales in specific regions (Asia, Europe, Pacific Basin, and South America) greater than 5 percent of total sales in the fiscal year prior to the crisis events. Finally, rather than using the issuance of management forecasts as the dependent variable, regression (3) uses the frequency of management forecasts during the quarter. The test is based on a Poisson specification. All tests include control variables, measured at one lag. See Appendix E for the definition of these variables. For simplicity, the coefficients on control variables are not reported. The test statistics reported in parentheses are computed using robust standard errors adjusted for firm-level clustering.

		No firm fixed effects (1)		Restricted sample (2)		Forecast frequency (3)	
	Predict	Coef.	z-stat.	Coef.	z-stat.	Coef.	z-stat.
Crisis	?	0.082	(1.424)	-0.072	(-1.013)	-0.056	(-1.120)
Crisis × ExpoMBK	**+**	**0.336**	**(4.368)**	**0.291**	**(3.043)**	**0.355**	**(5.560)**
ExpoMBK	?	0.026	(0.474)				
Intercept	?	-1.466	(-6.188)				
Firm Controls		Yes		Yes		Yes	
Firm fixed effects		No		Yes		Yes	
Number of firm-quarters		15,328		12,399		15,328	
Wald χ^2 (p-value)		387.69	(0.001)	178.94	(0.001)	126.47	(0.001)

Table 3.7: Summary statistics for management forecasts

This table reports summary statistics for the 4,650 forecasts issued by the sample firms during 1995:Q3 – 1999:Q2. *Horizon* equals the number of calendar days between the forecast issuance date and the forecasting fiscal period-end dates. It is a negative number if the forecast is issued after the fiscal period end but before the earnings announcement date (i.e., an earnings preannouncement). MF_DUM_{Long} is an indicator variable that equals one if the forecast is a long-run forecast with an horizon greater than 92 calendar days (about the length of a quarter). *Precision* equals 3 if the forecast is a point forecast, 2 if the forecast is a range forecast, 1 if the forecast is an open-ended forecast, and 0 if the forecast is a general impression forecast. *Point forecast* is an indicator variable that equals one if the forecast is a point forecast. There are three separate indicator variables for range forecasts (*Range forecast*) open-ended forecasts, (*Open-ended forecast*), and general impression forecasts (*General impression forecast*). $MF\_DUM_{GoodNews}$ is an indicator variable that equals one if the forecast is a good news forecast with the abnormal return around the forecast issuance date greater than one percent. The abnormal return (*MF_CAR*) is computed as the sum of size-adjusted daily returns in the three-day window [-1,1] around the forecast.

Panel A: By borrower type

	Forecasts from borrowers of exposed banks (N=2,766)					Forecasts from other borrowers (N=1,884)					p-values of the difference	
	Mean	Median	Q1	Q3	Std.	Mean	Median	Q1	Q3	Std.	Mean	Median
Forecast horizon												
Horizon	94	27	-3	161	155	68	16	-8	89	136	0.001	0.001
Long-run forecast (MF_DUM_{Long})	0.325	0	0	1	0.469	0.248	0	0	0	0.432	0.001	0.001
Forecast precision												
Precision	1.992	2	1	3	1.009	1.903	2	1	3	1.010	0.003	0.001
Point forecast	0.385	0	0	1	0.487	0.335	0	0	1	0.472	0.001	0.003
Range forecast	0.341	0	0	1	0.474	0.364	0	0	1	0.481	0.098	0.098
Open-ended forecast	0.155	0	0	0	0.362	0.170	0	0	0	0.376	0.169	0.169
General impression forecast	0.119	0	0	0	0.324	0.131	0	0	0	0.338	0.231	0.231
Forecast news content *												
Good news forecast ($MF\_DUM_{GoodNews}$)	0.346	0	0	1	0.492	0.331	0	0	1	0.484	0.116	0.116
Abnormal return (*MF_CAR*)	-0.035	-0.013	-0.077	0.026	0.113	-0.057	-0.024	-0.128	0.025	0.141	0.001	0.001

Summary statistics for management forecasts

Panel B: By borrower type, and by time period

	Forecasts from borrowers of exposed banks					Forecasts from other borrowers				
	Pre-crisis period		Crisis period		p-values of the change	Pre-crisis period		Crisis period		p-values of the change
	N	Mean (Median)	N	Mean (Median)	Mean (Median)	N	Mean (Median)	N	Mean (Median)	Mean (Median)
Forecast horizon										
Horizon	1,095	65 (13)	1,671	114 (59)	0.001 (0.001)	905	57 (10)	979	78 (20)	0.001 (0.001)
Long-run forecast (MF_DUM_{Long})	1,095	0.247 (0.000)	1,671	0.376 (0.000)	0.001 (0.001)	905	0.225 (0.000)	979	0.270 (0.000)	0.026 (0.026)
Forecast precision										
Precision	1,095	2.065 (2.000)	1,671	1.944 (2.000)	0.002 (0.043)	905	2.043 (2.000)	979	1.773 (2.000)	0.001 (0.001)
Point forecast	1,095	0.383 (0.000)	1,671	0.387 (0.000)	0.810 (0.810)	905	0.359 (0.000)	979	0.313 (0.000)	0.032 (0.032)
Range forecast	1,095	0.376 (0.000)	1,671	0.317 (0.000)	0.001 (0.001)	905	0.394 (0.000)	979	0.336 (0.000)	0.009 (0.009)
Open-ended forecast	1,095	0.164 (0.000)	1,671	0.148 (0.000)	0.256 (0.256)	905	0.177 (0.000)	979	0.163 (0.000)	0.441 (0.441)
General impression forecast	1,095	0.077 (0.000)	1,671	0.147 (0.000)	0.001 (0.001)	905	0.070 (0.000)	979	0.188 (0.000)	0.001 (0.001)
*Forecast news content **										
Good news forecast ($MF\_DUM_{GoodNews}$)	932	0.313 (0.000)	1,236	0.371 (0.000)	0.005 (0. 005)	786	0.307 (0.000)	750	0.357 (0.000)	0.045 (0.045)
Abnormal return (*MF_CAR*)	932	-0.042 (-0.015)	1,236	-0.030 (-0.011)	0.013 (0.012)	786	-0.065 (-0.030)	750	-0.049 (-0.020)	0.020 (0.027)

*Forecast news assignment is based on the stock market reactions to the forecast issued. Because the stock market effects of coincidental forecasts cannot be separated, multiple forecasts by the same firm on the same day are treated as one single forecast. Thus, the summary statistics for the forecast news variables are based on only 2,168 and 1,536 forecasts for the sample of borrowers of exposed banks and other borrowers, respectively.

Table 3.8: Differential changes in forecast horizon for borrowers of exposed banks following the crisis events

This table reports the regression results of the following firm-fixed effects model:

$$Dep\_Var_{it} = \alpha_i + \beta\ Crisis_t + \gamma\ Crisis_t \times ExpoMBK_i + \delta\ Controls_{it-1} + \varepsilon_{it} \quad (2)$$

Models (1) and (2) are logit models. The dependent variable (Dep_Var_{it}) is an indicator variable ($MF\_DUM_{Long_{it}}$) that equals one if the forecast is a long-run forecast with at least 92 calendar days (about the length of a quarter) between the forecast issuance date and the forecasting period-end date. Model (3) is estimated by OLS. The dependent variable equals the log of the number of calendar days between the forecast issuance date and the forecasting fiscal period-end dates ($Log(Horizon)_{it}$). To enable a log transformation, $Horizon$ is recoded to a small positive value (0.5) if the forecast is an earnings preannouncement (i.e., a forecast issued after the fiscal period end but before the earnings announcement date. In this case, *Horizon* is a negative number). The sample includes 4,650 individual management forecasts issued by the sample firms in the period of 1995:Q3 – 1999:Q2. See notes to Table 3.3 for other details about the model specification. The test statistics reported in parentheses are computed using robust standard errors adjusted for firm-level clustering.

Dependent variable (Model)		MF_DUM_{Long} (1)		MF_DUM_{Long} (2)		*Log(Horizon)* (4)	
	Predict	Coef.	z-stat	Coef.	z-stat	Coef.	t-value
Crisis	?	-0.120	(-0.628)	-0.072	(-0.434)	0.080	(0.578)
Crisis × ExpoMBK	**+**	**0.672**	**(3.319)**	**0.615**	**(3.142)**	**0.469**	**(2.795)**
Firm Controls							
Ln(MV)				0.146	(1.001)	0.218	(1.720)
MTB				0.004	(0.101)	0.006	(0.160)
ATGROWTH				-0.182	(-0.360)	0.469	(1.049)
ROA				4.438	(1.908)	7.248	(3.810)
LOSSDUM				0.098	(0.753)	0.103	(0.899)
BHAR				-0.007	(-0.026)	-0.162	(-0.755)
BETA				0.288	(1.629)	0.285	(1.640)
CFVOL				3.826	(1.127)	-1.192	(-0.414)
RETVOL				-0.272	(-0.049)	10.025	(1.638)
Firm fixed effects		Yes		Yes		Yes	
Number of forecasts		4,650		4,408		4,408	
Likelihood ratio χ^2 (p-value)		24.71	(0.001)	32.36	(0.001)	-	-
Adjusted R^2		-		-		0.442	

Table 3.9: Differential changes in forecast precision for borrowers of exposed banks following the crisis events

Regression (1) and (2) are based on the following ordered logit model:

$$Precision_{it} = \alpha_0 + \beta\ Crisis_t + \gamma\ Crisis_t \times ExpoMBK_i + \delta\ Controls_{it-1} + \varepsilon_{it} \quad (3)$$

$Precision$ equals 3 if the forecast is a point forecast, 2 if the forecast is a range forecast, 1 if the forecast is an open-ended forecast, and 0 if the forecast is a general impression forecast. Regression (3) differs from the others in two aspects. First, it is a logit model; the dependent variable is an indicator variable that equals one if the forecast is a quantitative forecast in the form of a point forecast or a range forecast (MF_DUM_{Quan}). Second, regression (3) controls for firm-fixed effects. Since $ExpoMBK_i$ is time-invariant for each firm, it is subsumed by the firm fixed effect. The sample includes 4,650 individual management forecasts issued by the sample firms during the period of 1995:Q3 – 1999:Q2. See notes to Table 3.3 for other details about the model specification. The test statistics reported in parentheses are computed using robust standard errors adjusted for firm-level clustering.

Dependent variable (Model)		$Precision$ (1)		$Precision$ (2)		MF_DUM_{Quan} (3)	
	Predict	Coef.	z-stat	Coef.	z-stat	Coef.	z-stat
Crisis	?	-0.404	(-4.805)	-0.433	(-4.915)	-0.934	(-5.561)
Crisis × ExpoMBK	+	**0.260**	**(2.383)**	**0.286**	**(2.507)**	**0.742**	**(3.676)**
Firm Controls							
Ln(MV)				0.039	(1.767)	-0.029	(-0.199)
MTB				0.009	(0.659)	0.001	(0.017)
ATGROWTH				0.495	(1.749)	0.563	(1.042)
ROA				2.538	(2.231)	4.409	(2.031)
LOSSDUM				-0.109	(-1.661)	-0.184	(-1.380)
BHAR				0.505	(3.553)	0.388	(1.580)
BETA				0.021	(0.295)	-0.066	(-0.388)
CFVOL				-2.026	(-2.062)	0.162	(0.049)
RETVOL				-5.056	(-1.894)	-10.205	(-1.952)
ExpoMBK		0.059	(0.712)	-0.044	(-0.492)		
Intercept 1		2.062	(29.044)	2.154	(12.199)		
Intercept 2		1.021	(15.984)	1.111	(6.406)		
Intercept 3		-0.460	(-7.367)	-0.405	(-2.347)		
Firm fixed effects		No		No		Yes	
Number of forecasts		4,650		4,408		4,408	
Likelihood ratio χ^2 (p-value)		38.05	(0.001)	128.96	(0.001)	76.27	(0.001)

Table 3.10: Differential changes in forecast news content for borrowers of exposed banks following the crisis events

This table reports the regression results of the following firm-fixed effects model:

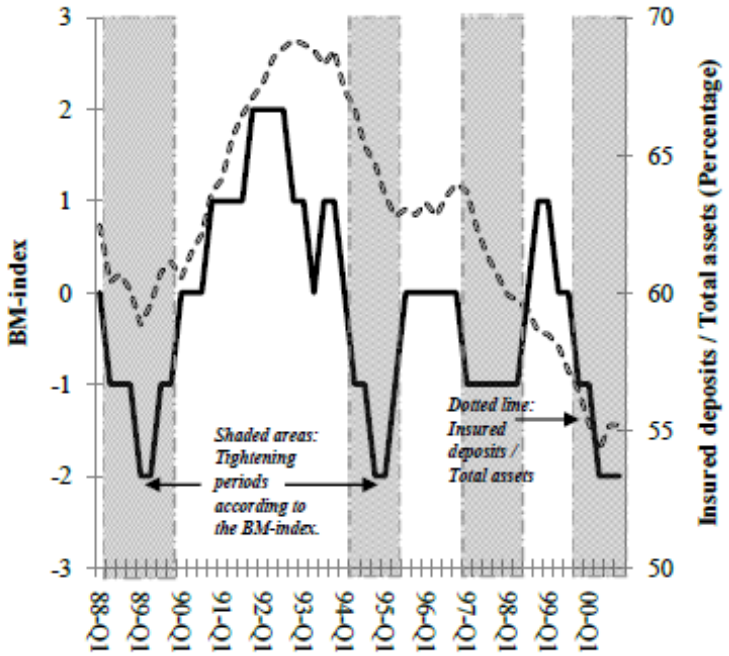
$$Dep\_Var_{it} = \alpha_0 + \beta\ Crisis_t + \gamma\ Crisis_t \times ExpoMBK_i + \delta\ Controls_{it-1} + \varepsilon_{it} \quad (4)$$

Models (1) and (2) are logit models. The dependent variable (Dep_Var_{it}) is an indicator variable ($MF\_DUM_{GoodNews_{it}}$) that equals one if the forecast is a good news forecast with the abnormal return around the forecast issuance date greater than one percent. The abnormal return (*MF_CAR*) is computed as the sum of size-adjusted daily returns in the three-day window [-1,1] around the forecast issuance date. Model (3) is estimated using OLS. The dependent variable is the forecast abnormal return (*MF_CAR*). Because the stock market effects of coincidental forecasts cannot be separated, multiple forecasts by the same firm on the same day are treated as one single forecast. Accordingly, the sample reduces from 4,650 to 3,704 individual management forecasts issued by the sample firms in the period of 1995:Q3 – 1999:Q2. See notes to Table 3.3 for other details about the model specification. The test statistics reported in parentheses are computed using robust standard errors adjusted for firm-level clustering.

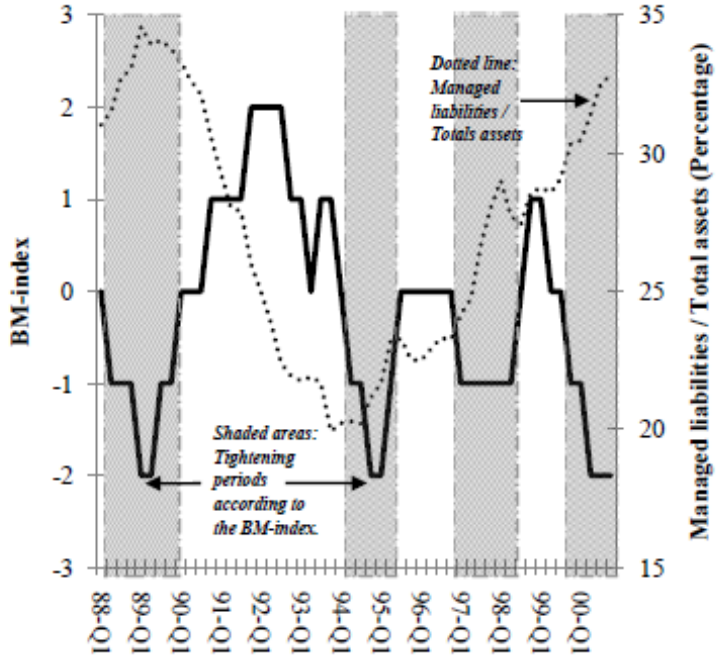
Dependent variable (Model)		$MF\_DUM_{GoodNews}$ (1)		$MF\_DUM_{GoodNews}$ (2)		*MF_CAR* (3)	
	Predict	Coef.	z-stat	Coef.	z-stat	Coef.	t-value
Crisis	?	-0.094	(-0.721)	-0.070	(-0.451)	0.002	(0.268)
Crisis × ExpoMBK	**?**	**0.325**	**(1.484)**	**0.323**	**(1.508)**	**0.006**	**(0.704)**
Firm Controls							
Ln(MV)				-0.588	(-4.167)	-0.051	(-7.345)
MTB				0.085	(2.031)	0.004	(1.786)
ATGROWTH				0.354	(0.695)	0.024	(0.952)
ROA				7.123	(3.301)	0.346	(3.383)
LOSSDUM				0.215	(1.684)	0.002	(0.381)
BHAR				0.968	(4.071)	0.059	(5.034)
BETA				-0.414	(-2.501)	-0.034	(-4.296)
CFVOL				0.972	(0.302)	0.502	(3.243)
RETVOL				14.101	(2.665)	1.099	(4.275)
Firm fixed effects		Yes		Yes		Yes	
Number of forecasts		3,704		3,599		3,654	
Likelihood ratio χ^2 (p-value)		4.76	(0.093)	66.31	(0.001)	-	-
Adjusted R^2		-		-		0.457	

Figure 2.1: Boschen-Mills (BM) index of monetary policy stance versus shares of liabilities in total assets

Panel A: BM-index versus insured deposits to total asset



Panel B: BM-index versus managed liabilities to total assets

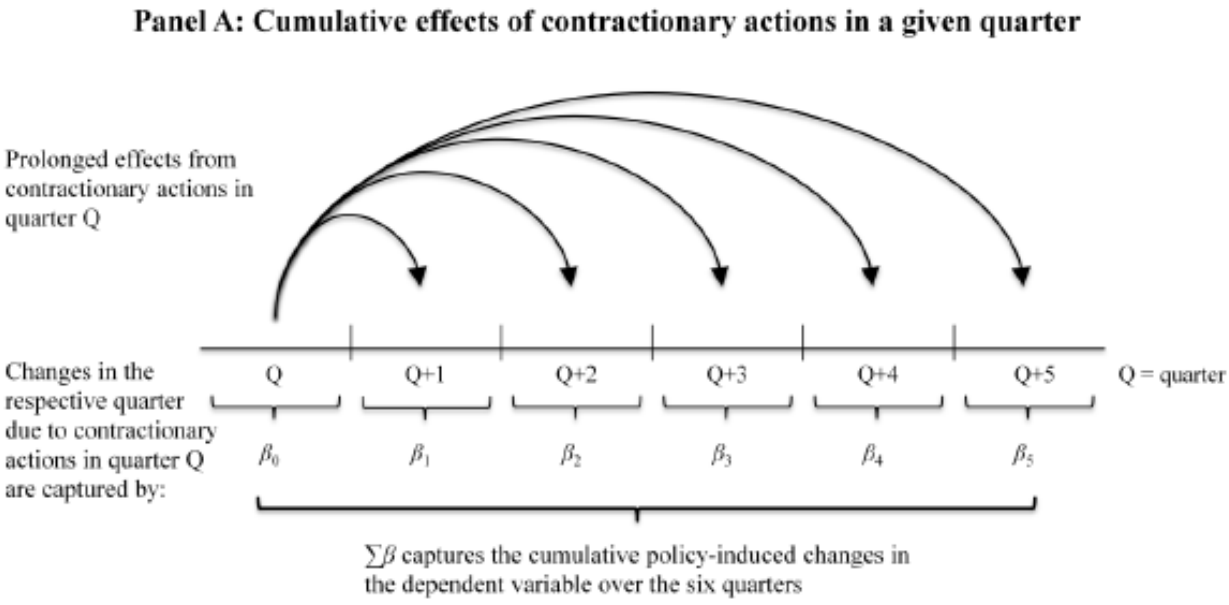


Notes: Figure 2.1 charts the value of the Boschen-Mills (BM) index at each quarter end throughout the sample period. Boschen and Mills (1995) peruse the policy records of the Federal Open Market Committee (FOMC) and classify the stance of policy into five categories from "strongly contractionary" (coded -2) to "strongly expansionary" (coded 2). A "neutral" policy is coded a zero value. Figure 2.1 also depicts the share of selected liabilities in total assets in levels (dotted line; right axis). The share information is calculated using aggregate balance sheet data provided by the Federal Reserve for all small domestically chartered commercial banks (Series H.8). The Federal Reserve defines small domestically chartered commercial banks as all domestically chartered commercial banks outside of the largest 25.

Seasonally adjusted item 1110 (Total deposits except large time deposits) is used to approximate insured deposits (Panel A). The sum of item 1072 (Large time deposits) and item 1094 (Borrowings) is used to approximate managed liabilities (Panel B). Total assets are based on Item 1151.

Figure 2.2: Bank responses to monetary tightening with lags

The inclusion of policy lags in Equation (1), Section 2.3.2 allows delays in banks' responses to monetary tightening. There are two interpretations of the sum of the coefficients on *TightMP* (i.e., $\sum \beta$). First, assume that the Fed adopts a contractionary policy for one given quarter (quarter Q in Panel A). $\sum \beta$ then captures the cumulative effect on a bank of the contractionary policy over the subsequent five quarters.



Bank responses to monetary tightening with lags

Alternatively, $\sum \beta$ can be interpreted from the perspective of a given quarter that follows prior contractionary actions. Specifically, consider the final quarter of a six-quarter contractionary cycle (quarter Q in Panel B). Under this alternative interpretation, $\sum \beta$ captures changes in a bank's managed liabilities (or total loans) in quarter Q that are attributable to successive contractionary actions throughout the current and the past five quarters. In Section 2.4.3.2, I assess the economic relevance of the results using both interpretations.

Panel B: Changes in the current quarter that are attributable to successive contractionary actions

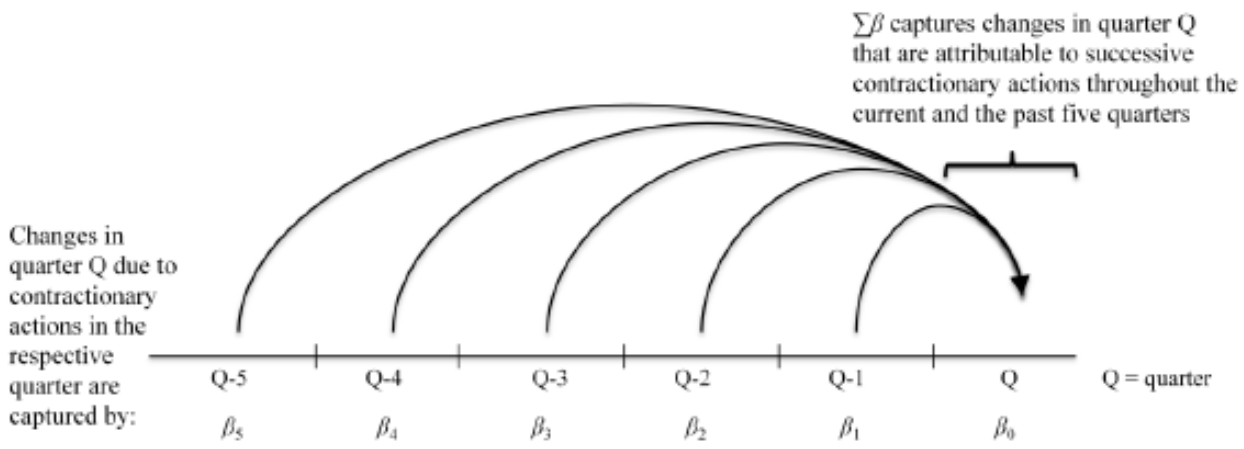
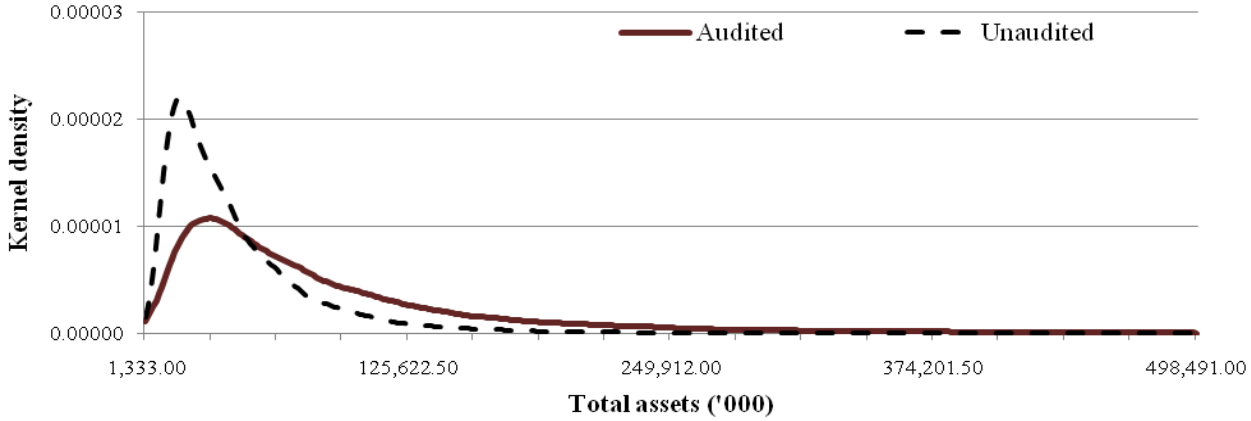


Figure 2.3: Size distribution of audited and unaudited banks

Figure 2.3 shows the size distribution for audited and unaudited banks in the full sample and in the three subsamples of similarly sized banks. For each set of banks, the graphs present Gaussian kernel densities of total assets. Sheather-Jones-plug-in bandwidth selection and a bandwidth multiplier of 1 are used.

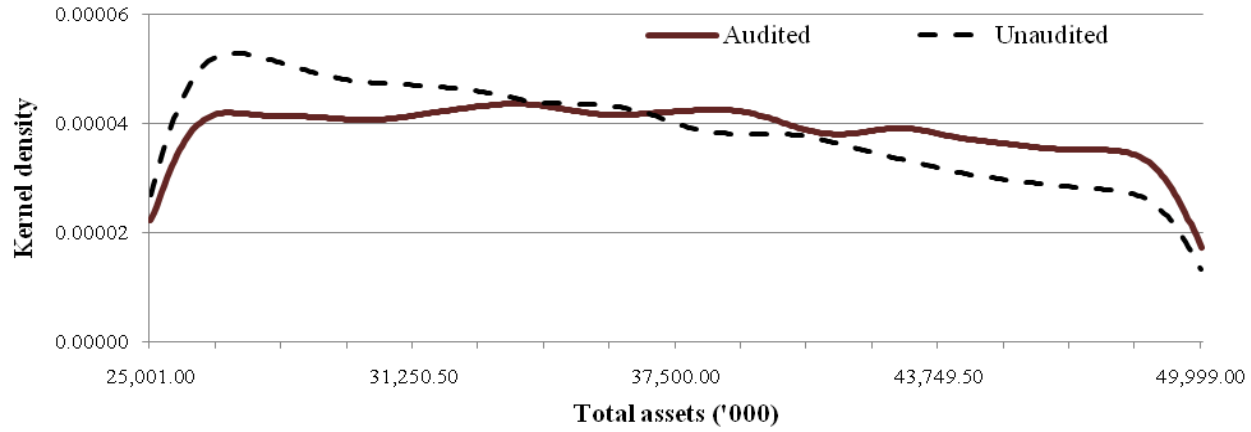
Full sample (N=327,084; 54% audited)

	Mean	Std Dev	Q1	Median	Q3
Total assets ($M)					
Audited	90.46	80.59	33.98	60.88	108.53
Unaudited	45.97	39.64	18.87	32.24	54.96



Size category A: $25 M - 50 M (N= 95,150; 47% audited)

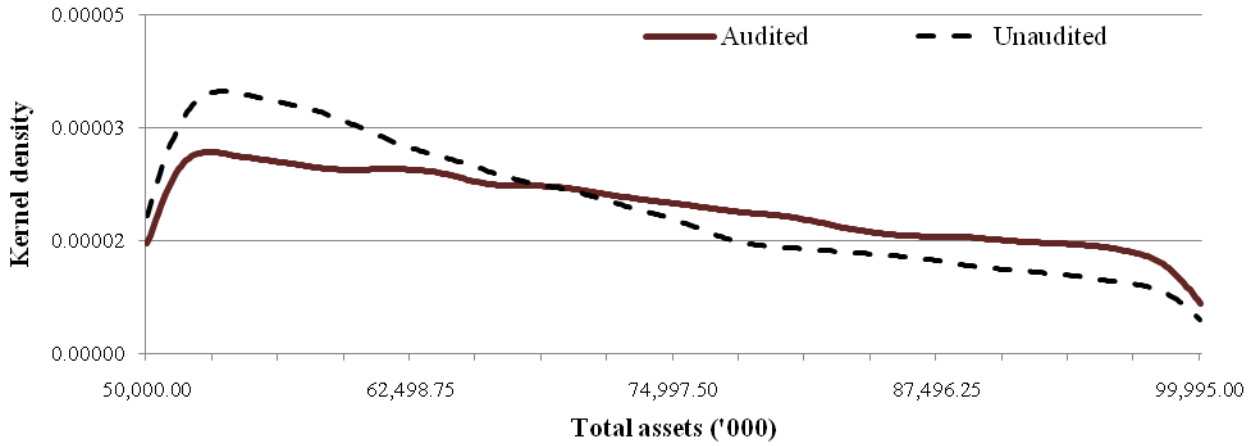
	Mean	Std Dev	Q1	Median	Q3
Total assets ($M)					
Audited	37.20	7.05	31.14	34.08	43.16
Unaudited	36.06	7.03	29.91	33.13	41.69



Size distribution of audited and unaudited banks

Size category B: $50 M - 100 M (N= 91,975; 61% audited)

	Mean	Std Dev	Q1	Median	Q3
Total assets ($M)					
Audited	72.24	14.17	59.92	65.91	83.88
Unaudited	69.02	13.71	57.24	62.38	79.19



Size category C: $100 M - 300 M (N= 60,336; 81% audited)

	Mean	Std Dev	Q1	Median	Q3
Total assets ($M)					
Audited	160.03	50.11	118.78	138.27	191.64
Unaudited	140.51	39.48	111.48	121.96	155.32

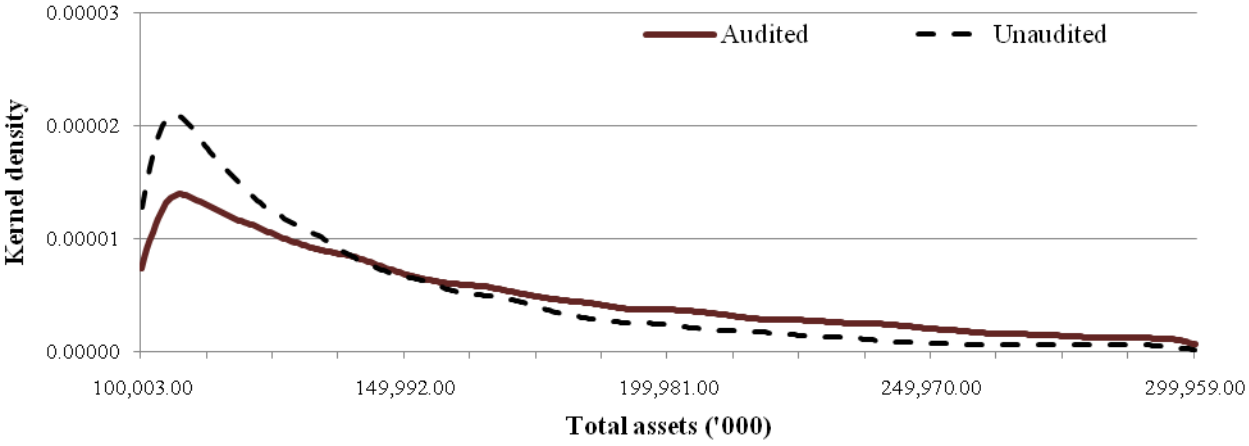
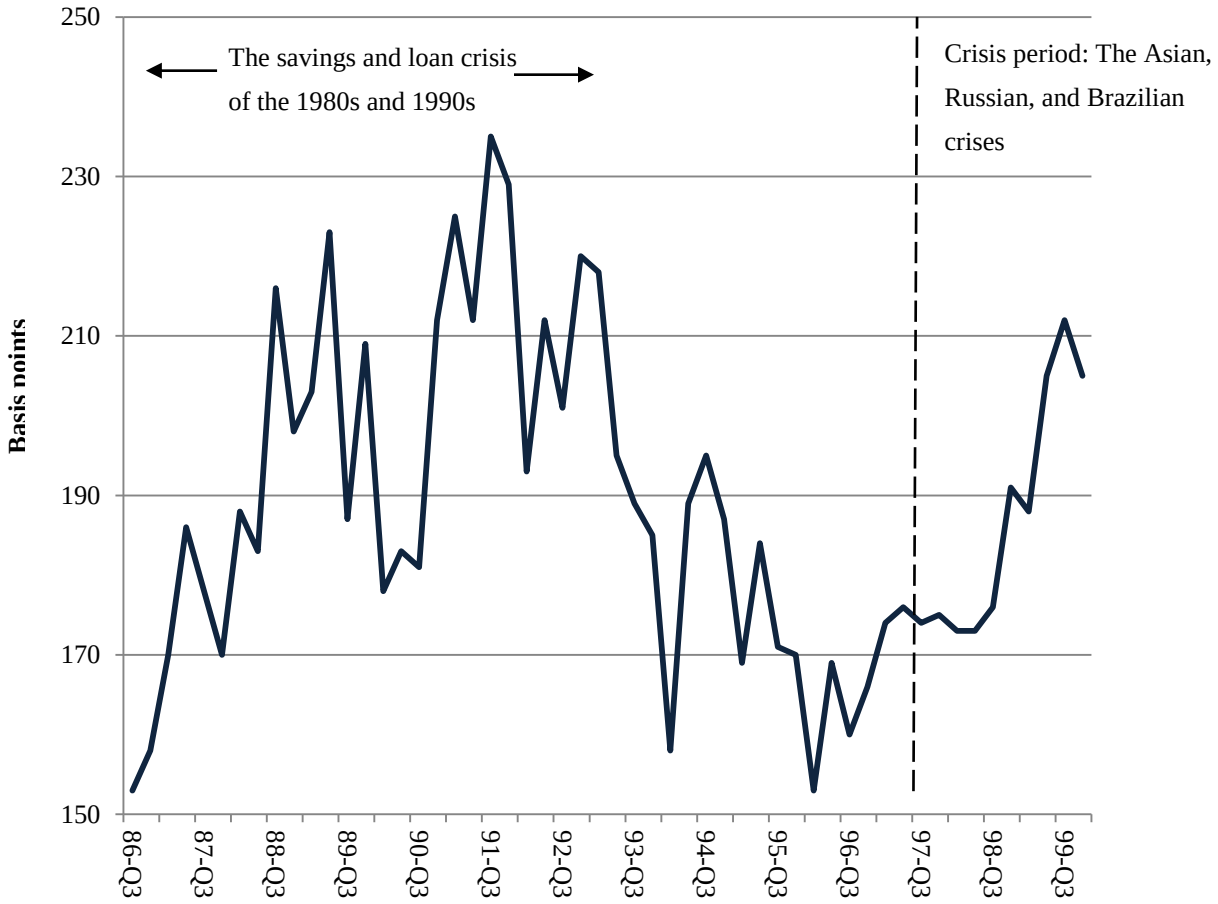
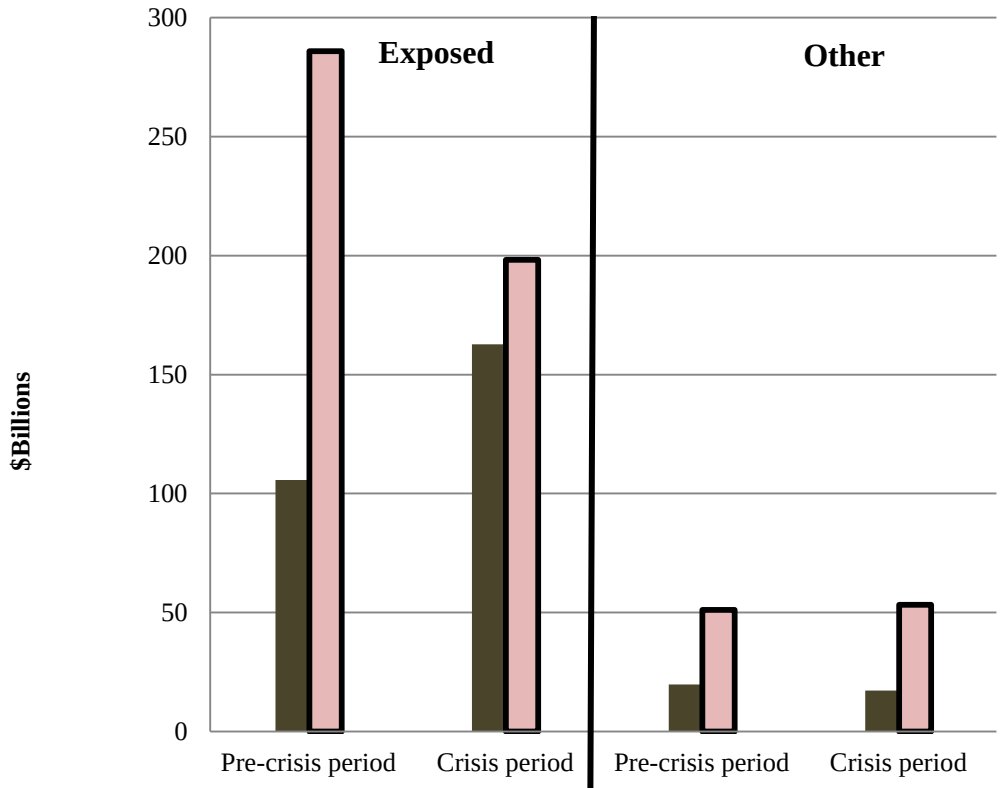


Figure 3.1: Spread between the average commercial and industrial (C&I) loan rate and the intended federal funds rate for all C&I loans



Notes: Figure 3.1 shows the spread between the average commercial and industrial (C&I) loan rate and the intended federal funds rate for all new C&I loans made by domestic banks from 1986:Q3 to 1999:Q4. The data are weighted by loan volume. The data are from Federal Reserve Board, Statistical Release E.2, "Survey of Terms of Business Lending".

Figure 3.2: New issuances of public financing and bank loans by borrower type and by period

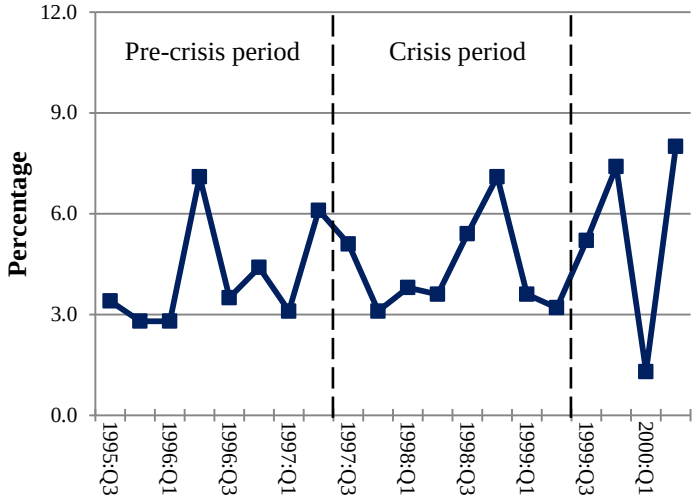


Public financing (% of total)	105.7 (27%)	162.8 (45%)	19.8 (28%)	17.2 (24%)
Public debt	90.3	145.3	11.7	12.6
Equity	15.4	17.5	8.1	4.6
Bank loans (% of total)	285.8 (73%)	198.1 (55%)	51.1 (72%)	53.2 (76%)
Total	391.5	360.9	70.9	70.4

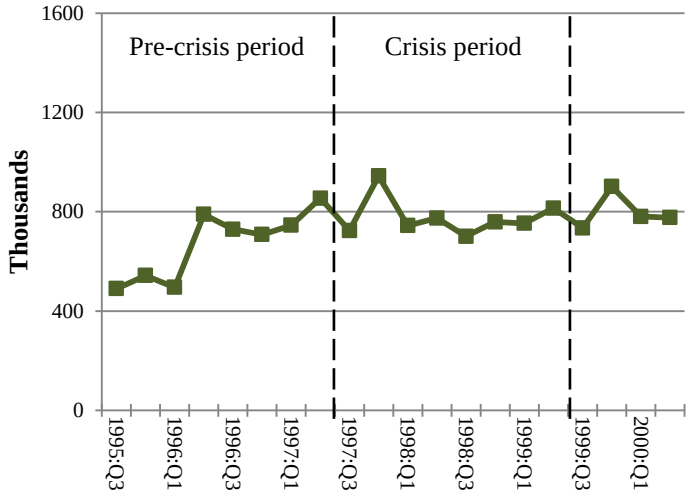
Notes: Figure 3.2 shows the new issuances of public financing (including public debt and equity) and bank loans for both the pre-crisis (1995:Q3 to 1997:Q2) and crisis periods (1997:Q3 to 1999:Q2) individually for borrowers of exposed banks and for other borrowers. There are 592 (519) borrowers in the group of "Exposed" ("Other"). The classification of borrower types, and the sample selection procedures are explained in Section 3.4 and 4.1, respectively. Bank loan data are from DealScan. Public debt and equity offerings data are from the Securities Data Corporation (SDC). For each form of financing in a given period, information on individual transactions occurred in that period is collected and then aggregated across firms in the same borrower group.

Figure 3.3: Economic indicators of the conditions of the U.S. economy

Panel A: Percent change in real gross domestic product (GDP) from the preceding quarter



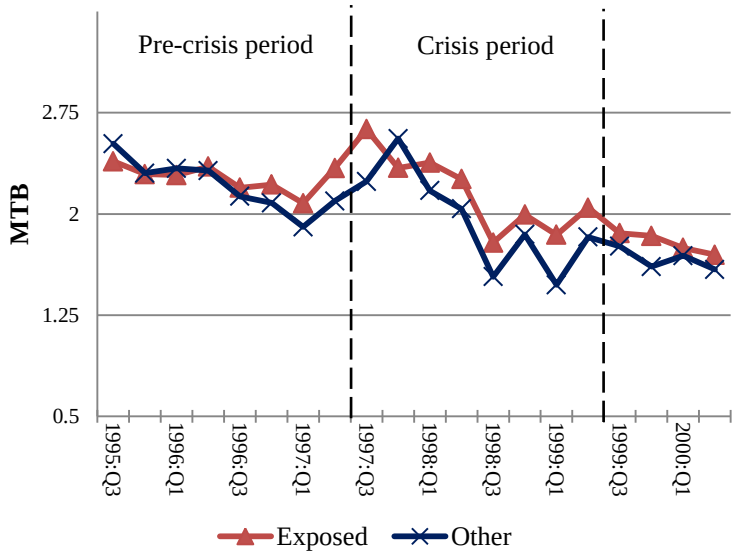
Panel B: Quarterly change in the number of nonfarm payroll jobs



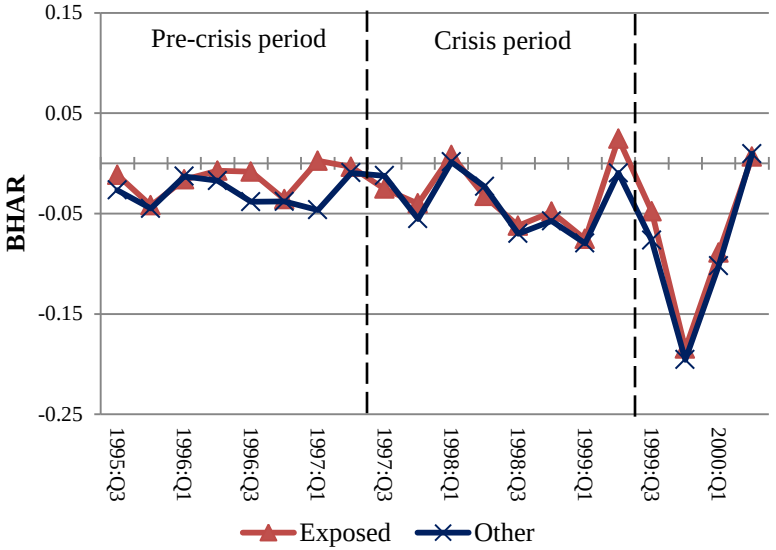
Notes: Panel A displays the percent change in real gross domestic product (GDP) from the preceding quarter (seasonally adjusted; annualized) throughout 1995:Q3 – 2000:Q2. Real GDP data are from the Federal Reserve Bank of St. Louis, FRED® (Series ID: GDPC96; Units: Compounded Annual Rate of Change). Panel B depicts the quarterly change in the number of nonfarm payroll jobs throughout the same period of time. Nonfarm payroll employment data are from the U.S. Department of Labor, Bureau of Labor Statistics (Series ID: CES0000000025).

Figure 3.4: Firm-specific growth opportunities

Panel A: Market-to-book value of equity ratio (MTB)



Panel B: Quarterly buy-and-hold size-adjusted return (BHAR)



Notes: Panel A displays the median market-to-book value of equity ratio in the contemporaneous quarter separately for borrowers of exposed banks and for other borrowers from 1995:Q3 to 2000:Q2. There are 592 (519) borrowers in the group of "Exposed" ("Other"). The classification of borrower types, and the sample selection procedures are explained in Section 3.4 and 4.1, respectively. Panel B depicts the median buy-and-hold size-adjusted return in the contemporaneous quarter throughout the same period of time. The size adjustment is based on the return of a portfolio of NYSE/AMEX/NASDAQ stocks in the same size-decile (market capitalization) as the sample firm. Medians are computed using available observations.

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Appendices

Appendix A: Simple illustration of how monetary tightening affects bank liquidity and lending

Monetary tightening is typically implemented through a large-scale open market sale of government securities by the Federal Reserve (Fed). To demonstrate the intuition behind how a contraction of monetary policy reduces bank liquidity, and ultimately the supply of bank loans, a hypothetical case where the Fed sells $10 million government securities is considered.[72] Assume that the required reserve ratio is 10 percent, and that the bank of the purchasing parties initially holds no excess reserves. The original position of the hypothetical bank is shown in the following simplified balance sheet:

Fig 1a. The initial financial position of the bank of the purchasing parties before the Fed sales

Assets		Liabilities and equity	
Reserves	$ 10 million	Reservable Deposits	$100 million
Securities	$ 5 million	Bank capital	$ 10 million
Loans	$ 95 million		

As a result of the open market sale operation, the hypothetical bank loses $10 million reservable deposits. This occurs because the purchasing parties pay for the securities with checks written on their checkable deposit account at the bank. Hence, the Fed deducts the proceeds from the hypothetical bank's deposits with it, reducing its holding of reserves by $10 million. The bank's immediate financial standing thus becomes:

[72] In reality, all Fed security transactions are conducted with a selected few primary dealers in U.S. government securities. See Mishkin (2006, Chapter 13) for further details on how open market operations affect bank reserves, and how reserve deficits lead banks to hold back lending (Chapter 9).

Fig 1b. The immediate financial position of the bank of the purchasing parties following the Fed sales

Assets		Liabilities and equity	
Reserves	$ 0 million	Reservable Deposits	$ 90 million
Securities	$ 5 million	Bank capital	$ 10 million
Loans	$ 95 million		

After the $10 million deposit outflow, the bank has a $9 million required reserves deficit (or 10 percent of $90 million). To make up for this shortfall, the bank can raise funds from uninsured, nonreservable forms of finance such as large certificates of deposit (CDs). Alternatively, it can liquidate securities holdings and/or hold back lending. Suppose that the bank can only raise $2 million large CDs due to restricted market conditions, the bank inevitably has to shrink its assets side of the balance sheet in order to meet its obligatory level of reserves. But, in the context here, even if the bank is willing to sell all its holdings of liquid securities, it still falls short of the required reserves. The bank therefore has to contract loans by at least $2 million to avoid costly penalty from reserve deficits. This links the initial reserve shock by a tightening of monetary policy to an ultimate contraction of loan supply. The final position of the bank is as follows:

Fig 1c. The final financial position of the bank of the purchasing parties following the Fed sales

Assets		Liabilities and equity	
Reserves	$ 9 million	Reservable Deposits	$ 90 million
Securities	$ 0 million	Uninsured large CDs	$ 2 million
Loans	$ 93 million	Bank capital	$ 10 million

Appendix B: Variable descriptions for Chapter 2

Panel A: Data source for monetary policy measures

MP measures	
The Boschen-Mills index	The Boschen-Mills index is available from the website of Prof. Charles L. Weise (http://www.gettysburg.edu/academics/economics/char_weisehomepage/charles_weise.dot, retrieved 2 Oct 2010), who updated the index through 2000:Q4. See Weise (2008) for more details.
FF-Bond	*FF-Bond* is computed as the difference between the federal funds rate and the rate on 10-year Treasury bills (in decimal form). The interest rate variables are available from the FRED data bank (Series ID: FEDFUNDS and GS10).

Panel B: Variables reported in Table 2.2 and those used in the financing (Table 2.3) and the lending (Table 2.4) response tests

Variables are listed in the order as in Table 2.2. Unless otherwise stated, all variables are taken from the Federal Reserve's *Report of Condition and Income (Call Reports).*

Variable name		Call reports item name (item number)
Total assets	=	Total assets (RCFD2170)
Log of total assets (Ln(TA))	=	Natural log of *Total assets*
Liquid Assets	=	For the period 1988:Q1 to 1993:Q2 (Campello 2002) Total investment securities (RCFD0390) + total assets held in trading accounts (RCFD2146) + federal funds sold and securities purchased under agreements to resell (RCFD1350) From 1993:Q3 onwards RCFD1350 + total held-to-maturity securities (RCFD1754) + total trading assets (RCFD3545) In the regressions, *Liquid Assets* is scaled by end of period *Total assets*.
Total loans	=	Total loans and leases, net (RCFD2125) [73]
C&I loans	=	Commercial and industrial loans (RCFD1600)
Real estate loans	=	Loans secured by real estate (RCFD1410)

[73] Using gross total loans and leases (RCFD1400) provides qualitatively similar results.

Variable name		Call reports item name (item number)
Other loans	=	*Total loans - C&I loans - Real estate loans*
Nonperforming loans(NPL)	=	Total loans and lease finance receivables: nonaccrual (RCFD1403) + past due 90 days or more and still accruing (RCFD1407). In the regression tests, *NPL* is scaled by end of period gross total loans and leases (RCFD1400).
Total Liabilities	=	Total Liabilities, net of subordinated debt (RCFD2950) + subordinated debt (RCFD3778)
Core deposits	=	Total deposits (RCFD2200) - Amount of deposit accounts of more than $100,000 (RCON2710)
Managed liabilities	=	Total time deposits of $100,000 or more (RCON2604) + subordinated debt (RCFD3778) + Other borrowed money (RCFD3850) + total brokered deposits (RCON2365) - total brokered retail deposits issued in denominations of less than $100,000 (RCON2343)
Large CDs	=	Total time deposits of $100,000 or more (RCON2604)
Equity	=	Total equity capital (RCFD3210)
Quarterly loan growth (Loan_Growth)	=	The quarterly first difference of the natural log of *Total loans* (i.e., $\Delta Ln(Total\ loans)$)
Quarterly CD growth (ML_Growth)	=	The quarterly first difference of the natural log of *Managed liabilities* (i.e., $\Delta Ln(Managed\ liabilities)$)
Audit indicator (Audited)	=	1 if the bank is audited (RCFD6724 = 1 or 2) [74]
OBHC	=	1 if the bank is controlled by a one-bank holding company. One-bank holding company affiliation is identified if the bank is owned by a direct (RSSD9379) or regulatory holder (RSSD9348), and that holder owns only one bank.
MSA	=	1 if the bank is located in a Metropolitan Statistical Area (RSSD9180 > 0)
LowCap	=	1 if the bank's *Equity* -to- *Total assets* ratio is below six percent (Ashcraft 2006)
Loss	=	1 if the bank made losses in the previous quarter. Losses are measured based on income before extraordinary items and other adjustments (RIAD4300)
Internal CF	=	the sum of income before extraordinary items and other adjustments (RIAD4300) and provision for loan and lease losses (RIAD4230) scaled by beginning of period *Total loans* (Houston et al. 1997)

[74] Each year, the audit indicator is reported only in the March Call Report (except in 1988, it was reported in the June Report) regarding the most comprehensive level of external auditing work a bank obtained in the previous year. Hence, the value of the indicator is extrapolated to the other quarters of the same fiscal year.

Variable name		Call reports item name (item number)
Other variables used in the regressions of large CDs or total loans		
GDP_Growth	=	The quarterly change of the natural log of national GDP. National GDP is taken from FRED (*Series ID: GDP*)
CPI_Growth	=	The quarterly change of the natural log of the consumer price index (CPI). CPI is taken from FRED (Series ID: CPIAUCNS)
Basel	=	A dummy variable for the time period from 1988 – 92
State	=	A set of state dummies (RSSD9200)
Trend	=	A linear time trend, defined as the distance (in years) of observation period from 1988
Quarter	=	A set of three quarter dummies

Panel C: Regression variables in the audit prediction model reported in Appendix D

Variable name		Call reports item name (item number)
Audited	=	1 if the bank is audited (RCFD6724 = 1 or 2)
PastAudit	=	1 if the bank was audited five years ago
Branches	=	1 if the bank is with branches. This information is taken from either the Summary of Deposits from the FDIC (Unit), or Research Information System (Unit).
Non-interest income	=	The ratio of non-interest income (RIAD4079) to beginning of year total assets (RCFD2170)
ROA volatility	=	The standard deviation of return on assets over the past five years. Return on assets is based on the ratio of income before extraordinary items and other adjustments (RIAD4300) to beginning of year total assets (RCFD2170)
Ln(TA)	=	Natural log of total assets (RCFD2170)
OBHC	=	1 if the bank is controlled by a one-bank holding company. One-bank holding company affiliation is identified if the bank is owned by a direct (RSSD9379) or regulatory holder (RSSD9348), and that holder owns only one bank.
MSA	=	1 if the bank is located in a Metropolitan Statistical Area (RSSD9180 > 0)
LowCap	=	1 if equity to total assets ratio is below six percent (Ashcraft 2006). Equity is based on (RCFD3210).
Loss	=	1 if the bank made losses in the previous quarter

Variable name		Call reports item name (item number)
Nonperforming loans(NPL)	=	Total loans and lease finance receivables: nonaccrual (RCFD1403) + past due 90 days or more and still accruing (RCFD1407). *NPL* is scaled by end of year gross total loans and leases (RCFD1400).
Liquid Assets	=	See Panel B for definition. Liquid Assets is scaled by end of year total assets (RCFD2170).
Internal CF	=	The sum of income before extraordinary items and other adjustments (RIAD4300) and provision for loan and lease losses (RIAD4230) scaled by beginning of year total loans (RCFD2125) (Houston et al. 1997)
C&I loans	=	Commercial and industrial loans (RCFD1600) scaled by end of year total loans (RCFD2125).
Real estate loans	=	Loans secured by real estate (RCFD1410) scaled by end of year total loans (RCFD2125).
Individual loans	=	Loans to individuals (RCFD1975) scaled by end of year total loans (RCFD2125).
Loan commitments	=	Unused commitments (RCFD3423) scaled by end of year total loans (RCFD2125).

Panel D: Regression variables in the loan loss timeliness tests reported in Table 2.8

Variable name		Call reports item name (item number)
LLP	=	The ratio of provision for loan and lease losses (RIAD4230) for the year to beginning of year gross total loans and leases (RCFD1400)
ΔNPL	=	The ratio of *NPL* (see Panel B) to gross total loans and leases minus the same ratio for the previous year
NCO	=	The ratio of charge-offs on allowance for loan and lease losses (RIAD4635) for the year to beginning of year gross total loans and leases (RCFD1400)
Ln(TA)	=	Natural log of total assets (RCFD2170)
LLA	=	The ratio of allowance for loan and lease losses (RCFD3123) to gross total loans and leases (RCFD1400)
HOM	=	The proportion of small or infrequently renegotiated loans in gross total loans and leases (RCFD1400)
		Small or infrequently renegotiated loans are defined as the sum of loans to individuals for household, family, and other personal expenditures (RCFD1975), all other loans (RCON1564), loans for purchasing or carrying securities (RCFD1545), and loans to depository institutions (RCFD1489).
Loan growth	=	End of year gross total loans and leases (RCFD1400) divided by beginning of year gross total loans and leases minus one (Nichols et al. 2009)

Variable name		Call reports item name (item number)
Equity	=	Total equity capital (RCFD3210) scaled by total assets (RCFD2170)
Audit indicator (Audited)	=	1 if the bank is audited (RCFD6724 = 1 or 2)

Further details on the control variables used in the loan loss timeliness tests reported in Table 2.8

The choice of the control variables follows prior research. First, the change in non-performing loans in the previous year (ΔNPL_{t-1}) is included because it is positively related to LLP_t for private banks (Nichols et al. 2009). Such a relation may reflect a delay in recogni tion of past credit losses or, conversely, timely revision of contemporaneous loan loss expectations when new information about the previous default likelihood becomes available in the current period. Nichols et al. (2009) also find that current period and future net loan chargeoffs (NCO_t, NCO_{t+1}, and NCO_{t+2}) are positively associated with LLP_t for private banks, so I include these variables as well. The test allows the coefficients on these three loan-related variables to differ between audited and unaudited banks, but no prediction is made about their signs.

To control for bank size, the natural log of total assets ($Ln(TA)_t$) is included. Next, past period loan loss allowance (LLA_{t-1}) is used to adjust for the impact of previous loss recognition decisions. Homogenous loans (HOM_{t-1}) are to control for banks' differential ability to exercise discretion over provisions for different types of loans. These two variables are expected to be negatively correlated with LLP_t (e.g., Liu and Ryan, 2006). Banks with rapid loan growth can indicate increases in credit risk taking. At the same time, banks that take greater risk in lending will hold higher levels of capitalization to absorb potential credit losses. Hence, following Nichols et al. (2009), I introduce current period loan growth ($Loan\ growth_t$), and equity ratio ($Equity_t$) to account for differences in credit risk in the loan portfolio. They are expected to be positively correlated with LLP_t. Finally, a set of state and year indicators are included to adjust for differences in economic conditions across bank locations and time.

Appendix C: Predicted bank responses to monetary tightening

This table shows the computation of the predicted cumulative responses of a bank five quarters after a given contractionary quarter (including the effects in quarters Q to Q+5; see Panel A of Figure 2.2). The estimates in columns (B) and (C) are taken from the results reported in column (4) of Tables 2.3 and 2.4. All bank-level control variables are evaluated at their median values (see Table 2.2 Panel A).

			Quarterly growth of managed liabilities (*ML_Growth*)		Quarterly growth of total loans (*Loan_Growth*)	
		Evaluated at (A)	Sum of coeffi-cients (B)	(A) × (B)	Sum of coeffi-cients (C)	(A) × (C)
1	TightMP	1.000	0.0409	0.0409	-0.0552	-0.0552
2	TightMP × Audited	0.000	0.0193	0.0000	0.0052	0.0000
	Bank-level controls	*Sample median*				
3	TightMP × Ln(TA)	10.71	0.0075	0.0803	0.0023	0.0246
4	TightMP × OBHC	1.000	-0.0042	-0.0042	0.0010	0.0010
5	TightMP × MSA	0.000	0.0204	0.0000	0.0056	0.0000
6	TightMP × LowCap	0.000	-0.0296	0.0000	-0.0082	0.0000
7	TightMP × Loss	0.000	-0.0167	0.0000	-0.0052	0.0000
8	TightMP × NPL	0.010	0.1056	0.0011	0.0144	0.0001
9	TightMP × Liquid assets	0.268	-0.0603	-0.0162	0.0177	0.0047
10	TightMP × Internal CF	0.014	-0.2543	-0.0036	-0.0184	-0.0003
11	***Cumulative policy responses for an unaudited bank (Sum of rows 1 to 10)***			**0.0984**		**-0.0249**
12	*Differential responses for an audited bank (row 2)*			0.0193		0.0052
	Cumulative policy responses for an audited bank (Sum of rows 11 and 12)			**0.1177**		**-0.0197**

An audited bank responding to contractionary actions in the current quarter is predicted to have a cumulative 11.77 percent increase in managed liabilities over the subsequent five quarters. At the same time, there is a predicted cumulative 1.97 percent decrease in total loans for the bank over the same period.

Appendix D: Predicting external audit status

This table reports results for the probit regression of banks' audit status (*Audited*).

Variable	Coeff. estimate	Wald χ^2	P-value
Past audit status and bank complexity			
Intercept	-4.785	581.471	(0.001)
PastAudit	**1.790**	**9914.307**	**(0.001)**
Branches	0.072	10.564	(0.001)
Non-interest income	4.606	50.391	(0.001)
ROA volatility	-5.913	1.704	(0.192)
Bank-level controls used in Equation (1), Section 2.3.2			
Ln(TA)	0.383	616.918	(0.001)
OBHC	-0.043	4.195	(0.041)
MSA	0.124	31.436	(0.001)
LowCap	-0.127	3.111	(0.078)
Loss	0.008	0.011	(0.918)
NPL	1.419	5.226	(0.022)
Liquid assets	-0.127	2.367	(0.124)
Internal CF	-2.298	58.360	(0.001)
Loan portfolio information used in Section 2.5.1			
C&I loans	0.285	6.261	(0.012)
Real-estate loans	0.235	8.292	(0.004)
Individual loans	0.307	8.348	(0.004)
Loan commitments	0.896	18.775	(0.001)
State and year indicators		Yes	
Number of observations		36,676	
Proportion audited		0.52	
	χ^2		P-value
Likelihood ratio test	25545.831		(0.001)
Score test	20988.121		(0.001)
Wald test	15764.872		(0.001)
Percent Concordant (discon-cordant)		90.6 (9.3)	
Pseudo R^2		0.502	

Notes: The regression is estimated using annual data taken from the December Call Reports. The sample period starts in 1992 when information about a bank's audit status five years ago is available (*PastAudit*).

It ends in 1999, corresponding to the last period when the audit status of a bank is measured in the main tests (see Tables 2.3 and 2.4). The other predictors are defined in Panel C of Appendix B and are lagged one period relative to *Audited*. After applying the same sample selection criteria as that reported in Table 2.1, 36,676 bank-years had the required data. Two sided p-values based on robust standard errors adjusted for bank-level clustering are reported in parentheses. As discussed in Section 2.5.3, estimates from this probit regression are used to compute the inverse Mills' ratio (*IMR*) for each sample bank-year.

Further details on the explanatory variables used in the audit choice model

The choice on the explanatory variables follows prior research. Kohlbeck (2005) argues that bank complexity increases the demand for third-party expertise and induces banks to hire external auditors. I therefore include three variables that are expected to be positively associated with bank complexity. First, an indicator variable for banks with branches (*Branches*) is included to assess the impact of more geographically-dispersed banking operations. Second, the ratio of non-interest income to total assets (*Non-interest income*) is included to assess the impact of bank operations other than lending. Third, the volatility of return on assets (*ROA volatility*) is included as complex bank operations can result in volatile operational results.

In addition to these complexity proxy variables, the audit choice model includes all the bank-level controls used in Chapter 2 Equation (1). As discussed in Section 2.3.2, these variables are expected to be associated with banks' audit decisions due to the effects of bank size (*Ln(TA)*), organizational structure (*OBHC*), metropolitan business settings (*MSA*), managers' concerns due to capital inadequacy (*LowCap*) and difficult business environments (*Loss*), the ability to generate cash flows (*internal CF*), and the bank's liquidity management policy (*Liquid assets*). To assess the impact of different lending operations, I also include four variables related to the characteristics of the bank's loan portfolio as discussed in Section 2.5.1: *C&I loans*, *Real-estate loans*, *Individual loans*, and *Loan commitments*. Finally, I include a set of state and year indicator variables to control for the effects of different geographical regions and time periods.

Appendix E: Variable descriptions for Chapter 3

MF_DUM = an indicator variable set to one for firms that issue at least one management forecast in the current quarter;

Crisis = an indicator variable set to one for the financial crisis period 1997:Q3 – 1999:Q2;

ExpoMBK = an indicator variable set to one for firms whose main bank was exposed to the crisis areas (per Kho et al. 2000);

Ln(MV) = natural logarithm of beginning of quarter market value of firm equity;

MTB = market-to-book ratio measured at the beginning of the fiscal quarter;

ATGROWTH = growth of total assets in the previous quarter;

ROA = return on assets, computed as the ratio of earnings before extraordinary items to total assets at the beginning of the fiscal quarter;

LOSSDUM = an indicator variable set to one for firms that report losses in the previous quarter;

BHAR = buy-and-hold size adjusted return measured over the 3 months prior to the beginning of the fiscal quarter. The size adjustment is based on the return of a portfolio of NYSE/AMEX/NASDAQ stocks in the same size-decile (market capitalization) as the sample firm;

RESTRUCT = an indicator variable equal to one if the firm recognized restructuring charges in the previous quarter;

BETA = equity beta for the previous fiscal period;

CFVOL = cash flow volatility, computed as the standard deviation of quarterly operating cash flows over the prior 2 years;

RETVOL = stock return volatility, computed as the standard deviation of daily gross stock returns over the 3 months prior to the beginning of the quarter.