The Effects of Firm-initiated Clawback Provisions on Bank Loan Contracting

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ABSTRACT: Although firm-initiated clawbacks reduce accounting manipulation, they also induce managers to engage in sub-optimal activities (e.g., reduce R&D) to achieve earnings targets. To assess the effectiveness of clawback provisions, we examine their impact from debtholders' point of view. We find that banks use more financial covenants and performance pricing provisions in the loan contracts and decrease interest rates after firms initiate clawbacks. Moreover, we also find that loan maturity increases and loan collateral decreases subsequent to clawback adoption. Taken together, our findings indicate that firm-initiated clawback provisions enhance financial reporting quality, thereby reducing the information uncertainty that financing providers face.

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I. INTRODUCTION

Accounting scandals in recent years suggest that earnings manipulation driven by managers' self-dealing behavior is prevalent among listed firms (Bergstresser et al. 2006; Efendi et al. 2007; Cornett et al. 2008). To deter this behavior, the Dodd-Frank Wall Street Reform and Consumer Protection Act of 2010 includes a section (Section 954) allowing firms' boards of directors to recoup managers' incentive compensation and profits, if any, from the sale of stock in the event of accounting restatements (hereafter, DFA 954). Since the Securities and Exchange Commission (SEC) still has not released the guidelines for implementing DFA 954 up to now, it is still not certain when the "clawback" provisions will become mandatory.[1] It is thus not yet possible to estimate the effect of DFA 954 on financial reporting integrity. The effectiveness of this legislation can be inferred, however, by studying firm-initiated clawbacks.

Similar to DFA 954, firm-initiated clawback provisions are triggered by accounting restatements and designate a firm's board as the enforcer of the provisions. However, while firm-initiated clawbacks focus mainly on accounting irregularities caused by managerial misconduct (i.e., intentional restatements), DFA 954 covers both intentional and unintentional restatements. DFA 954 is thus stricter than firm-initiated clawbacks. As such, if firm-initiated clawbacks constrain managers from distorting financial reports, DFA 954 can be expected to have similar, if not stronger, effects.

The above rationale has motivated a number of studies on the effectiveness of voluntary clawbacks. Chan et al. (2012a) and Dehaan et al. (2012) find that the adoption

[1]DFA 954 directs the SEC to set up guidelines requiring all listed companies to adopt and implement a policy to recover executive compensation that is awarded based on erroneous financial information. However, as of September 2012, the development of the guidelines is still listed as a pending action item by the SEC. Other than DFA 954, Section 304 of the Sarbanes-Oxley Act of 2002 also includes a clawback provision, which will be discussed in Section 2.

of the voluntary clawback provisions is associated with a reduction in the occurrence of accounting restatements, suggesting that clawbacks may be an effective deterrent of financial misreporting. However, this finding may not necessarily indicate that clawback provisions enhance financial reporting quality. Instead, the reduced occurrence of financial misstatements may be attributed to auditors' erroneous belief that clawback adopters have better earnings quality, so they reduce effort in auditing such firms and therefore are less likely to discover a material misstatement (Dennis 2012). Indeed, Chan et al. (2012a) find that auditors spend less time auditing clients with clawbacks. The reduced likelihood of misstatements may also be driven by managers' lower incentives to disclose accounting irregularity to avoid compensation recovery once they are subject to clawbacks. Moreover, as Ewert and Wagenhofer (2005) argue, accounting standards designed to constrain earnings manipulation may lead firms to replace accounting manipulation with real activity management (e.g., discretionary reduction in R&D expenses), working against the standard-setters' objective. Consistent with this view, Chan et al. (2012b) find that clawback adopting firms switch from using abnormal accruals to cutting discretionary expenses to avoid missing earnings benchmarks, with total earnings management not decreasing subsequent to clawbacks. Thus, the question of whether firm-initiated clawbacks lead to an overall improvement in financial reporting integrity still requires further examination.

In this study we examine the effectiveness of firm-initiated clawbacks from another stakeholders' point of view, namely, that of debtholders. We focus on changes in bank loan contracts surrounding clawback initiation for several reasons. First, bank loans represent one of the most important sources of debt financing (Myers 2003) and bank

loan contracts are closely tied to accounting numbers (Drucker and Puri 2009). Second, the terms of a bank loan contract, such as interest rate or financial covenants, are subject to fewer measurement errors than abnormal accruals or ERC. They therefore provide cleaner evidence on the usefulness of clawback provisions. Finally, Bharath et al. (2008) find that both the price and non-price terms in *bank loan* contracts are sensitive to changes in accounting quality, while only the price term is affected by accounting quality in *bond* contracts. These findings suggest that focusing on bank loans enables us to examine how clawback provisions affect various dimensions of debt contracts. Moreover, in comparison to public bondholders, banks have more direct access to the borrowing firms (Rajan 1992), allowing banks to more effectively judge whether clawbacks have a real impact on financial reporting quality.

If lenders perceive clawback provisions useful to improve the quality of financial reporting, we hypothesize that there should be changes in the lending contracts initiated after the clawback adoption. The improved quality of financial reporting will affect lending contracts directly and indirectly. The "direct" effect is expected because accounting measures are explicitly used as inputs in financial covenants (Smith and Warner 1979) or as the basis of performance-pricing provisions (which adjust interest rates based on accounting performance measures, e.g., Asquith et al. 2005). Lenders are found to include more financial covenants and accounting-based performance-pricing provisions in lending contracts if they consider financial reporting more reliable (e.g., Ball et al. 2008; Costello and Wittenberg-Moerman 2011). As such, we would observe an increase in the use of financial covenants or accounting-based performance pricing if firm-initiated clawbacks indeed enhance financial reporting integrity.

Clawback provisions also affect lending contracts "indirectly" because lenders rely on publicly available accounting information contained in financial reporting to assess the credit risks. The uncertainty in the reliability of financial reporting creates an information risk to the lenders (Duffie and Lando 2001; Easley et al. 2002; Easley and O'Hara 2004). Prior studies show that financial reporting quality is negatively associated with firms' cost of debt (Ashbaugh et al. 2009; Bharath et al. 2008). To the extent that clawback adoption reduces the likelihood of accounting misstatements and enhances financial reporting integrity, lenders should face less information risk. We therefore hypothesize that firm-initiated clawbacks lead lenders to demand lower interest rate and loosen other credit terms such as maturity and collateral.

To test our predictions, we use the difference-in-differences methodology to examine 1,566 loan facilities issued to 147 pairs of clawback adopters and non-adopters listed in the Russell 3000 index (where non-adopters are identified based on propensity-score matching to mitigate the self-selection associated with the decision to adopt clawbacks). We first show that banks are more likely to use financial covenants in loan contracts after clawback adoption. In addition, banks also increase the use of accounting-based performance pricing provisions subsequent to clawback adoption. Both results suggest that banks consider borrowers' accounting information to be more reliable after clawbacks.

Next, we show that banks respond favorably to firm-initiated clawbacks by lowering the interest rates on loans. In particular, interest rates are 33 basis points lower on average after clawback initiation, which represents an economically significant 24% decrease in the cost of debt capital. Moreover, bank loans issued after clawback adoption have longer

maturities and are less likely to be collateralized. These results suggest that, due to the improved quality of financial reporting associated with clawbacks, debtholders do not need to monitor clawback adopters as frequently and seek additional protection following clawback adoption (Harris and Raviv 1990; Stulz 2000; Diamond 1991, 1993; Berger et al. 2005).

To ascertain the robustness of our results, other than using the propensity-score-matching, we also apply a two-stage selection model to control for *unobservable* differences across clawback adopters and control firms. We find that our results remain similar.

In additional analysis we investigate the cross-sectional differences in the impact of firm-initiated clawback on debt contracts. While clawbacks may be initiated by firms with weak financial reporting quality as impetus for improvement, they may also be initiated by firms with good reporting quality to signal their good reporting quality to outsiders (e.g., Spence 1973). Since banks have more access to firms' private information than equity-holders, the signaling effect is expected to be less pronounced for bank loan providers. We therefore predict that banks react to firm-initiated clawbacks only when these provisions effectively improve financial reporting quality. After partitioning clawback adopters into high-restatement-risk firms and low-restatement-risk firms using Dechow et al.'s (2011) *F-score*, we find that the changes in debt contract terms that we document above concentrate in the former group. For example, clawback adopters with high restatement risk experience a larger reduction in the cost of debt capital and are less likely to face collateral requirements than those with low restatement risk. Moreover, after initiating clawbacks, banks are more likely to introduce financial covenants and

accounting-based performance pricing in loan contracts issued to high risk firms. This result lends further support to the view that firm-initiated clawbacks enhance financial reporting quality and reduce the degree of information asymmetry between lenders and borrowers and are not considered by lenders as just a signaling device.

This study contributes to at least two literatures. First, we extend the literature on the effectiveness of firm-initiated clawbacks from the perspective of equity investors to that of debtholders. Our results suggest that mandatory clawbacks as required by the Dodd-Frank Act are likely to be beneficial in increasing financial reporting quality, at least from the perspective of creditors. Our study also contributes to the growing literature on the association between accounting quality and credit terms. Graham et al. (2008) show that, after a financial restatement, the firm faces higher interest rates and lower debt maturity to compensate for the higher information costs to banks. Costello and Wittenberg-Moerman (2011) and Kim et al. (2011) find similar effects after a firm discloses internal control weaknesses according to the requirement of Section 302 or 404 of the Sarbanes-Oxley Act (SOX). We extend this literature by showing that when borrowing firms take measures to enhance financial reporting quality, banks respond favorably.

The rest of the paper is organized as follows. Section II provides background on clawback provisions and develops our testable hypotheses. Section III discusses the sample. Section IV discusses our research design and presents the results. Section V concludes the paper.

II. BACKGROUND AND HYPOTHESIS DEVELOPMENT

The practice of and research on clawabck of compensation

Executive compensation tends to be closely linked to company performance. As a result, managers have strong incentives to manipulate financial reports (Burns and Kedia 2006; Efendi et al. 2007), notwithstanding the fact that restatements of financial reports often lead to a significant decline in stock prices, substantial legal liabilities, and economic losses (Palmrose et al. 2004; Kedia and Philippon 2009). In an attempt to help alleviate this phenomenon, Section 304 of the 2002 Sarbanes-Oxley Act (SOX) authorized the SEC to recover bonus or other incentive compensation earned by CEOs and CFOs of firms that experienced restatements due to *managerial misconduct*. However, SOX 304 was rarely enforced by the SEC largely due to ambiguities in the legislation and the SEC's lack of resources (e.g., Morgenson 2011).

The 2010 Dodd-Frank Act expands the reach of clawback provisions. Specifically, Section 954 of the Dodd-Frank Act requires listed companies to adopt and implement clawback policies that allow the board of directors to recoup incentive compensation based on misstated earnings numbers. In contrast to SOX 304, which designates enforcement authority to the SEC, DFA 954 requires corporate boards to enforce the clawbacks. Moreover, DFA 954 does not limit clawbacks to restatements involving intentional accounting irregularities (i.e., managerial misconduct) – unintentional accounting irregularities can also trigger clawbacks. However, the implementation of DFA 954 is still pending while the SEC is developing guidelines for listed firms to follow.

While legislators have been working to mandate the use of clawbacks, firms have

increasingly chosen to voluntarily adopt clawback provisions authorizing the board of directors to recover compensation from executives in the event of accounting restatements. For example, Corporate Library indicates by 2010, 194 firms in the S&P 500 (39.8%) had clawback provisions in place. Typically, firm-initiated clawbacks authorize the board to recoup the *excess* portion of executives' cash bonuses, equity compensation, or both awarded based on misstated financial numbers.

Several extant studies examine various aspects of firm-initiated clawbacks. Addy et al. (2009), Brown et al. (2011), and Babenko et al. (2012) investigate the determinants of clawbacks and find that clawback adoption is mostly driven by firm size and prior financial reporting quality (e.g., restatement history). Chan et al. (2012a) and Dehaan et al. (2012) examine the consequences of clawbacks and show that the frequency of accounting restatements decreases, and the credibility of earnings quality perceived by investors increases, after clawback initiation. Gao et al. (2011) and Babenko et al. (2012) further show that investors react positively to announcements of firm-initiated clawbacks.

Although the findings above seem to suggest that firm-initiated clawbacks are beneficial, they are subject to alternative interpretations as well. For example, the reduced likelihood of financial misstatements after clawback adoption may actually be driven by auditors' erroneous belief that clawback adopters have better financial reporting quality, thereby paying less attention to such clients and therefore are less likely to discover financial misstatements (Dennis 2012). Indeed, Chan et al. (2012a) find that auditors spend less time auditing firms with clawback provisions. Moreover, there are also costs associated with clawback provisions. As Ewert and Wagenhofer (2005) argue, accounting regulations intended to constrain earnings manipulation may actually encourage

managerial myopia – that is, managers may engage in suboptimal operating decisions to achieve earnings targets (e.g., discretionary reduction in R&D investment). Supporting this view, Chan et al. (2012b) find that firm-initiated clawbacks induce CEOs to switch from accruals manipulation to real transaction management to meet or beat earnings targets, and that this phenomenon is more pronounced for firms with greater growth opportunities (as these firms face greater pressure to achieve earnings benchmarks). As such, whether firm-initiated clawbacks lead to an overall improvement in financial reporting integrity remains an open question.

The effects of clawbacks on bank loan contracts

If firm-initiated clawbacks indeed lead to an improvement in financial reporting quality, we expect that they may cause both "direct" and indirect" effects on bank loan contracts. There is a *direct* impact because accounting measures are explicitly used as inputs in certain contractual terms. For example, loan contracts typically include financial covenants that are based on accounting ratios to provide a signal of deteriorating credit quality to loan providers or to prohibit managers from taking actions that are detrimental to lenders (such as paying out large amounts of dividend to shareholders) (Smith and Warner, 1979). Except for financial covenants, loan contracts may also contain performance-pricing provisions stipulating adjustments to interest rates based on accounting performance measures (Asquith et al., 2005).

Previous studies have shown that when the accounting reporting quality is in doubt, such as when firms disclose material weaknesses in internal controls, lenders reduce the number of financial covenants (Costello and Wittenberg-Moerman 2011). Conversely,

when accounting information is more relevant in lenders' assessment of credit risk, lenders increase the use of accounting-based relative to rating-based performance-pricing provisions (Ball et al. 2008). As such, we expect that the initiation of clawback provisions is associated with an increased use of financial covenants and performance-pricing provisions if it enhances financial reporting integrity. More formally, we propose hypothesis H1 as follows:

H1: The adoption of clawbacks increases the use of financial covenants and accounting-based performance-pricing provisions in bank loan contracts.

Firm-initiated clawbacks will also affect loan contracts *indirectly* through the reduction in the information risk faced by lenders. To assess a borrower's default risk and reduce the agency costs arising from the contracting process, debtholders have to rely on information provided in financial reports to assess a borrower's future cash flow risk (e.g., Smith and Warner 1979). Better accounting information reduces banks' information uncertainty and allows banks to charge lower interest rates on loans (Duffie and Lando 2001; Easley and O'Hara 2004). Recent empirical studies have shown that higher financial reporting quality is associated with a decrease in the cost of debt capital (e.g., Bharath et al. 2008; Ashbaugh et al. 2009). Conversely, events that raise concerns about financial reporting quality, such as financial misstatements or the revelation of internal control weaknesses, increase borrowers' loan spread (Graham et al 2008; Costello and Wittenberg-Moerman 2011; Dhaliwal et al. 2011; Kim et al. 2011).

As discussed above, the objective of firm-initiated clawbacks is to mitigate financial misstatements by directly linking executive compensation to financial reporting behavior. We conjecture that banks view this contractual arrangement favorably. As Chan et al. (2012a) argue, clawback provisions impose not only monetary penalties but also substantial reputational costs on managers involved in financial misstatements. Indeed, companies that enforce clawback provisions tend to explicitly describe their disciplinary actions in corporate proxy statements, and such enforcement actions are often followed by the media (Morgenson 2008). As such, to the extent that clawbacks induce managers to be more truthful in financial reports, decreasing banks' information risk, we expect clawback adoption to be associated with a decrease in the interest rates charged on loans (e.g., Diamond and Verrecchia 1991).

Clawback adoption may also affect other terms in a bank loan contract. Existing literature on loan maturity finds that short-term debt forces managers to disclose information on a timely basis, mitigating the agency problems arising from conflicts between equity- and debtholders (Harris and Raviv 1990; Stulz 2000). In line with this view, Diamond (1991, 1993), Flannery (1986), and Berger et al. (2005) document that when information asymmetry is lower, debt maturity is longer. These findings suggest that loan maturity is positively related to information quality. Accordingly, to the extent that clawback provisions increase financial reporting quality and reduce the degree of information asymmetry between borrowers and lenders, we expect clawback adoption to be associated with an increase in loan maturity.

Another term in the loan contracts that is likely to be affected by clawback initiations is collateral requirement. Rajan and Winston (1995) argue that loans are more likely to be

collateralized when borrowers require more monitoring, that is, when agency problems stemming from the conflicts between equity- and debtholders are more severe. Several studies provide supportive evidence that collateralized loans are used to reduce the information risk borne by lenders (Graham et al. 2008; Costello and Wittenberg-Moerman2011; Kim et al. 2011). Thus, again, to the extent that clawback provisions reduce the information uncertainty faced by banks, we expect clawback adoption to be associated with a lower likelihood of collateral requirements.

In short, whether firm-initiated clawbacks lead to a reduction in information asymmetry between lenders and borrowers is ultimately an empirical question. This leads to the following hypothesis, stated in alternative form:

H2: In comparison to bank loans extended before clawback adoption, those extended after clawback adoption are associated with lower interest rates, longer maturities, and a lower likelihood of being collateralized.

Notwithstanding the above arguments, firm-initiated clawbacks may not reduce the degree of information asymmetry between banks and adopting firms. As we discuss earlier, clawbacks may lead managers to switch from accounting manipulation to real transactions management to meet or beat earnings targets, reducing companies' long-term performance (Chan et al. 2012b). Hence, to the extent that banks expect managers to substitute earnings management tools, they may not consider clawbacks useful in mitigating their information risk. In short, we would not observe any change in loan contracts if clawback provisions do not enhance financial reporting integrity.

The relation between restatement risk and the effects of clawbacks on bank loan contracts

There are two plausible motivations for clawback adoption: clawback provisions may be adopted by firms with poor financial reporting quality to mitigate the occurrence of accounting restatements, or they may be adopted by firms with good reporting quality to distinguish themselves from firms with poor reporting quality (e.g., Spence 1973). In other words, firm-initiated clawbacks may be effective deterrents of financial misstatements, or they may simply signal adopters' financial reporting quality. We expect firm-initiated clawbacks to have more pronounced effects on bank loan contracts when they are adopted by firms with poor financial reporting quality. Bharath et al. (2008) indicate that relative to other types of lenders or equityholders, banks tend to have more private information concerning their clients. As such, banks are likely able to determine the intention of clawback adoption, and hence should respond more favorably to clawback adoption when such provisions are able to reduce the information uncertainty they face. Of course, even if clawback adoption is merely a signal of financial reporting quality, we still may observe changes in banks loan contracts to the extent that banks have imperfect information about the borrower.

In short, we expect that the aforementioned effects of clawbacks on the terms of bank loan contracts mainly concentrate on clawback adopters with a higher *ex ante* likelihood of accounting restatements before they adopt clawbacks. This leads to our third hypothesis:

H3: Clawback adoption has a more pronounced impact on a bank loan's interest rate, maturity, collateral requirements, and use of financial covenants in firms with higher restatement risk (as proxied by Dechow et al.'s (2011) *F*-score) than in firms with lower restatement risk.

III. SAMPLE

Following prior studies (e.g., Addy et al. 2009; Chan et al. 2012a), our initial sample of clawback adopters comes from Corporate Library. At the beginning of 2010, Corporate Library examined the financial reports of 3,380 firms and found that 638 of them had clawback provisions in place. As shown in Table 1, after excluding financial firms (which were mandated by the Department of Treasury to have clawbacks in place in 2009) and firms without financial and governance data in Compustat and Corporate Library, we identify 343 firms with clawback provisions and 1,840 firms without such provisions as of fiscal year 2009. Table 1 provides descriptive statistics for clawback adopters and non-adopters. As can be seen from the table, relative to firms without clawbacks, clawback adopters have on average larger firm size, better profitability, and higher leverage.

To avoid potential endogeneity associated with the decision to adopt clawbacks, for our empirical tests we form samples of clawback adopters and non-adopting control firms using propensity-score matching (LaLonde 1986).[2] Specifically, we estimate a logit model in which we regress clawback adoption on a set of firm characteristics and

[2]Instead of using propensity-score matching to select control firms, one could use all firms that never adopt clawbacks during our sample period. However, as Chan et al. (2012a) and Dehaan et al. (2012) indicate, clawback adopters are different from non-adopters across many dimensions such as firm size, growth opportunities, and profitability. Thus, to address the self-selection problem while also facilitating execution of our difference-in-differences approach, we choose the matched sample as our main research design.

governance variables including firm size, accounting profitability, leverage ratio, growth opportunities, number of segments, prior restatements, board independence, institutional ownership, size of audit committee, number of board meetings, institutional ownership, insider ownership, and CEO tenure. We also include indicator variables for each fiscal year in our sample and 2-digit SIC codes. This matching process yields a set of control firms with the closest probability of having clawbacks and assigns an "artificial" adoption year to control firms (even though they do not adopt clawbacks). We further require a caliper of 0.1, and a common support range of 0.1 to 0.9 (Caliendo and Kopeinig 2008). This one-to-one matching procedure results in 248 pairs of clawback adopters and non-adopters.

After forming samples of clawback adopters and control firms, we further match these firms to DealScan. More specifically, to test for changes in bank loan contracts while avoiding changes in sample composition, we require that clawback adopters and control firms have at least one loan contract issued during the pre-adoption period and at least one loan contract issued during the post-adoption period. This requirement yields 147 pairs of clawback adopters and control firms that together have 1,566 loan facilities issued during the 2000 to 2009 period. Following prior studies on bank loan contracts (e.g., Costello and Wittenberg-Moerman 2011), we perform empirical analysis at the individual facility level.[3]

Table 2 provides descriptive statistics for the 147 pairs of clawback adopters and control firms, as well as for the 1,566 loan facilities. We find that relative to control firms, clawback adopters are less leveraged, and they have less tangible assets and inventory as

[3]A typical loan contract tends to include several loan facilities with different terms (maturities, interest rates, collateral requirements, and covenants).

a percentage of total assets. In terms of corporate governance, clawback adopters have less independent boards of directors, have higher institutional ownership, hold more directors meetings per year, and have larger audit committees.

Turning to loan terms, we find that loans borrowed by clawback adopters are larger, have lower interest rates, are less likely to be collateralized, and are less likely to have performance-pricing provisions.[4]

IV. EMPIRICAL DESIGN

The effect of clawbacks on the use of financial covenants in bank loan contracts

To examine the effects of firm-initiated clawbacks on bank loan contracts, we adopt the difference-in-differences methodology following extant literature on clawbacks (Chan et al. 2012a; Dehaan et al. 2012).[5] We first estimate the following equation using a Poisson model to examine the change in the use of financial covenants surrounding the adoption of clawback provisions, where we conduct the regression at the individual facility level following Graham et al (2008) and Costello and Wittenberg-Moerman (2011):

[4]Based on univariate comparison, we find some preliminary evidence that clawbacks cause changes in loan terms issued to clawback adopters. For example, for clawback adopters, the mean of loan interest rate is 145.07 during pre-clawback period and 135.28 during post-clawback periods, with the difference significant (t = -1.80). Moreover, for loan contracts issues to clawback adopters, the mean of number of financial covenants 1.1 before initiating clawbacks, and 1.3 after having clawbacks (t-stat for the difference = 1.37). In addition, for clawback adopters, 50.29% of loan terms in pre-clawback period include performance-pricing provisions and 53.23% in post-clawback period (t-stat for the difference = 0.774). The mean of loan maturity is 41.68 during pre-clawback period and 50.56 during post-clawback periods for clawback adopters, with the difference significant (t = 4.82). In pre-clawback period, 70.45% of loan terms of clawback adopters require collateral while in post-clawback period, 67.300% require collateral (t-stat for the difference = 1.62).

[5]The difference-in-differences methodology is commonly used in finance and economic studies to investigate the effects of specific events on variables of interest. See, for instance, Bertrand and Mullainathan (1999, 2003) and Low (2009).

$$\begin{aligned} Financial\ Covenant_{it} = {} & \alpha + \beta_1 Claw_{it} + \beta_2 PostClaw_{it} + \beta_3 Relationship_{it} + \beta_4 InstituteLoan_{it} \\ & + \beta_5 Revolver_{it} + \beta_6 InterestRate_{it} + \beta_7 Syndication_{it} + \beta_8 Collateral_{it} \\ & + \beta_9 Dealsize_{it} + \beta_{10} Maturity_{it} + \beta_{11} Rating_{it} + \beta_{12} No\_Lender_{it} \\ & + \beta_{13} PPricing_{it} + \beta_{14} PrimaryPurpose_{it} + \beta_{15} Firmsize_{it} + \beta_{16} Profit_{it} \\ & + \beta_{17} Leverage_{it} + \beta_{18} Tangibility_{it} + \beta_{19} MB_{it} + \beta_{20} Current\_Ratio_{it} \\ & + \beta_{21} Log\_Coverage_{it} + \beta_{22} Indep_{it} + \beta_{23} InstituteOwn_{it} \\ & + \beta_{24} BoardMeet_{it} + \beta_{25} AuditCommitee_{it} + Year\ Dummy \\ & + Industry\ Dummy + \varepsilon_{it}. \end{aligned} \quad (1)$$

Financial Covenant is the logarithm of the number of financial covenants included in the loan contract. *Claw* is a dummy variable equal to one if the company is a clawback adopter and equal to zero if the company is a non-adopter. As discussed above, the performance-matching procedure assigns an "artificial" adoption year to each control firm even though these firms do not adopt clawbacks. In equation (1) *PostClaw* measures the change in interest rates across pre- and post-adoption periods for a clawback adopter relative to the change over the same interval for a control firm. *Relationship* is a dummy variable equal to one if at least one of the loan's lead banks was a lead bank for the borrower's loans over the five years prior to the present loan, and zero otherwise. *InstituteLoan* is a dummy variable equal to one if the loan is an institutional term loan, and zero otherwise. *Revolver* is a dummy variable equal to one if the loan is a revolver loan, and zero otherwise. *InterestRate* is the All-in-Drawn-Spread reported by DealScan, which is equal to the amount the borrower pays in basis points over LIBOR for each dollar drawn. *Syndication* is a dummy variable equal to one if the loan is a syndicated loan, and zero otherwise. *Collateral* is a dummy variable equal to one if the loan is collateralized, and zero otherwise. *Dealsize* is the log of the loan amount. *Maturity* is the number of months between the facility issue date and the end date of the loan. *Rating* is a dummy variable equal to one if the company has a credit rating, and zero otherwise.

No_Lender is the number of participating lenders in the syndicated loan. *PPricing* is a dummy variable equal to one if the loan has a performance-pricing provison, and zero otherwise. *PrimaryPurpose* is a dummy variable equal to one if the loan's primary purpose is a takeover, LBO/MBO, or recapitalization. *Firmsize* is the natural log the company's total assets. *Profit* is the ratio of EBITDA to total assets. *Leverage* is long-term debt divided by total assets. *Tangibility* is the sum of property, plan, and equipment plus inventory divided by total assets. *MB* is the ratio of market value of equity to book value of equity. *Current_Ratio* is the ratio of current assets to current liabilities. *Log_Coverage* is the natural logarithm of one plus the ratio of EBITDA to interest expenses. *Indep* is the percentage of independent directors on the board. *InstituteOwn* is the percentage of institutional ownership. *BoardMeet* is the number of board meetings held per year. *AuditCommittee* is the number of audit committee members. The *t*-statistics are adjusted for heteroskedasticity and firm-level clustering.

The results for equation (1) are presented in column (1) of Table 3. We find that the number of financial covenants in loan contracts is similar for clawback adopters and control firms prior to the adoption of clawbacks, as the coefficient on *Claw* is insignificant (-0.035, t = -0.56). However, banks include more financial covenants in loan agreements after clawbacks are adopted, as the coefficient on *PostClaw* is significantly positive (0.196, t = 1.96). The magnitude of the coefficient on *PostClaw* indicates that the number of financial covenants increases by 1.22 (= exponential$^{(0.196)}$) per loan contract following clawback adoption, which translates into a 102% increase, as the average number of financial covenants per loan contract prior to clawback adoption is 1.20. Consistent with prior literature (e.g., Costello and Wittenberg-Moerman 2011),

institutional loan is positively related to number of financial covenants as these loans are more risky and the lenders are more likely to rely on financial covenants to monitor the borrower. In addition, the coefficient on *Interest Rate* is positively significant, suggesting that those loans with higher interest rate would include more financial covenants in the loan contracts. The coefficient on *PPricing* is positively significant, indicating that loans which include performance-pricing provisions are more likely to include more financial covenants. *Firmsize* and *Tangibility* are negatively related to number of financial covenants, suggesting that lenders include fewer financial covenants in the loan contracts for larger firms and firms with more tangible assets, due to their lower agency costs of debt. However, we find that the signs of the coefficients on *Collateral* and *Dealsize* are opposite to the prediction. This is possible due to our requirement that a firm must issue a loan both during pre- and post-adoption periods, and we find that the two coefficients would retain the predicted signs when we relax the data requirement in later analysis.

Before moving on to the next test, we conduct several tests to verify the robustness of the above results. First, we modify equation (1) by dropping *Claw* but including firm fixed effects to control for time-invariant unobservables that differ across clawback adopters and non-adopters. We note that the number of observations for this analysis is smaller than the one presented in column (1) (1418 versus 1566). The decrease in sample size is because for certain firms, the number of financial covenants does not change across the sample period, and these firms are dropped during the statistical analysis as they are perfectly correlated with the individual firm indicator. The results, presented in column (2) of Table 3, show that the coefficient on *PostClaw* remains significantly positive (0.278, $t = 2.66$), suggesting that the inclusion of firm-fixed effects

does not affect our finding. Second, instead of using a matched-sample design, we repeat the analysis using the entire sample - that is, we repeat the difference-in-differences model with firm fixed effects using the entire sample of clawback adopters and control firms. Naturally, this sample is much larger than the matched sample (7,512 versus 1,566 facilities). The results are presented in column (3) of Table 3. We find that the coefficient on *PostClaw* continues to be significantly positive (0.203, t = 2.56), suggesting that our results are not driven by the matching procedure. Third, instead of using a continuous dependent variable, we also repeat the estimation of equation (1) with an indicator dependent variable using a Probit model. That is, we construct a dummy variable, *DFinancial*, which equals one if the loan facility has any financial covenants and zero otherwise. Untabulated results suggest that our finding remains unaffected (the coefficient on *PostClaw* = 0.367 with t = 1.92). In short, after performing various robustness tests, the results continue to support our prediction that firm-initiated clawbacks increase the use of financial covenants in the loan contracts.

The effect of clawback provisions on the use of performance pricing provisions in loan contracts

We next estimate the following Probit specification to examine the effect of clawback provisions on the use of performance pricing provisions.

$$\begin{aligned} PPricing_{it} = {} & \alpha + \beta_1 Claw_{it} + \beta_2 PostClaw_{it} + \beta_3 Relationship_{it} + \beta_4 InstituteLoan_{it} \\ & + \beta_5 Revolver_{it} + \beta_7 Syndication_{it} + \beta_8 Collateral_{it} + \beta_9 Dealsize_{it} + \beta_{10} Maturity_{it} \\ & + \beta_{11} Rating_{it} + \beta_{12} No\_Lender_{it} + \beta_{14} PrimaryPurpose_{it} + \beta_{15} Firmsize_{it} + \beta_{16} Profit_{it} \\ & + \beta_{17} Leverage_{it} + \beta_{18} Tangibility_{it} + \beta_{19} MB_{it} + \beta_{22} Indep_{it} + \beta_{23} InstituteOwn_{it} \\ & + \beta_{24} BoardMeet_{it} + \beta_{25} AuditCommitee_{it} + Year\ Dummy + Industry\ Dummy + \varepsilon_{it}. \end{aligned} \quad (2)$$

Where *PPricing* is an indicator variable which equals one if the loan facility includes a performance-pricing provision and zero otherwise. The other variables are the same as previously defined. The *z*-statistics are adjusted for heteroskedasticity and firm-level clustering.

The estimation of equation (2) is presented in column (1) of Panel B, Table 3. We find that *Claw* is insignificant, suggesting that loan contracts issued to clawback adopters have the same likelihood of containing a performance-pricing provision as those issued to non-adopters during pre-adoption periods. On the other hand, we find the coefficient on *PostClaw* is significantly positive (0.648, $z = 2.33$), indicating that clawback adoption leads to an increased use of performance-pricing provisions in bank loan contracts.

Asquith et al. (2005) indicate that performance pricing provisions can be based on either financial statement numbers (e.g., EBITDA) or credit rating. We expect that the increased use of performance pricing caused by clawback adoption would be attributed to those based on financial ratios as they are explicitly linked to accounting numbers. To see if this is the case, we then re-estimate equation (2) by replacing *PPricing* with *ACCTPricing*, where *ACCTPricing* is an indicator variable which equals one if the loan facility includes a performance-pricing provision based on financial numbers and zero otherwise. The number of observations for this analysis is smaller than the one presented in column (1) (1322 versus 1566). The decrease in sample size is because for certain industries, accounting-based performance pricing is never used in their loan contracts, and firms in these industries are dropped during the statistical analysis as they are perfectly correlated with the industry indicators. From the result provided in the second column of Panel B of Table 3, we find that consistent with our expectation, the increased

use of performance pricing subsequent to clawback adoption is indeed driven by accounting-based performance pricing. Specifically, the coefficient on *PostClaw* is significantly positive (0.752, t = 2.15), and the marginal probability associated with it is 0.102. The coefficients on *Revolver* are significantly positive (0.728; z = 4.72) for both *PPricing* and *ACCTPricing* (0.374; z= 1.91), suggesting that lenders are more likely to impose these provisions for more risky loans. Also, because smaller sized and higher leveraged firms have higher agency costs of debt, *Firmsize* is negatively related and *Leveraged* is positively related to the likelihood of having performance-pricing provisions in loan contracts. However, the coefficient on *Relationship* has an opposite sign to the predictions.

To conclude, the results in Table 3 support our proposition that clawback provisions enhance financial reporting integrity, thereby leading to an increased use of financial covenants and accounting-based performance pricing.

The effect of clawback provisions on loan interest rates and other contractual terms

We estimate the following ordinary least squares (OLS) specification to examine the change in bank loan interest rates surrounding the adoption of clawback provisions.

$$\begin{aligned} InterestRate_{it} = {} & \alpha + \beta_2 PostClaw_{it} + \beta_3 Relationship_{it} + \beta_4 InstituteLoan_{it} \\ & + \beta_5 Revolver_{it} + \beta_6 FinancialCovenant_{it} + \beta_7 Syndication_{it} + \beta_8 Dealsize_{it} \\ & + \beta_9 Maturity_{it} + \beta_{10} Rating_{it} + \beta_{11} No\_Lender_{it} + \beta_{12} PPricing_{it} \\ & + \beta_{13} PrimaryPurpose_{it} + \beta_{14} Firmsize_{it} + \beta_{15} Profit_{it} + \beta_{16} Leverage_{it} \\ & + \beta_{17} Tangibility_{it} + \beta_{18} MB_{it} + \beta_{19} Current\_Ratio_{it} + \beta_{20} Log\_Coverage_{it} \\ & + \beta_{21} Indep_{it} + \beta_{22} InstituteOwn_{it} + \beta_{23} BoardMeet_{it} + \beta_{24} AuditCommitee_{it} \\ & + Year\ Dummy + Firm\ Dummy + \varepsilon_{it}. \end{aligned} \quad (3)$$

InterestRate is the All-in-Drawn-Spread reported by DealScan, which is equal to the

amount the borrower pays in basis points over LIBOR for each dollar drawn. The other variables are the same as previously defined. The *t*-statistics are adjusted for heteroskedasticity and firm-level clustering.

The regression results for equation (3) are presented in Table 4, column (1). We find that the coefficient on *PostClaw* is significantly negative (-33.246, t = -3.43), suggesting that clawback adoption leads to a reduction in interest rates. Given the average interest rate for clawback adopters prior to clawback adoption is 137 basis points, the 33 basis point decrease represents a 19% decrease in interest rate relative to the change in the interest rate experienced by control firms. The other control variables take the predicted signs whenever significant.[6]

The effect of clawbacks on loan maturity

To examine the effect of firm-initiated clawbacks on loan maturity, we estimate the following ordinary least squares regression, which is similar to the one used in Bharath et al. (2009) and Costello and Wittenberg-Moerman (2011):

$$\begin{aligned} Maturity_{it} = {} & \alpha + \beta_1 Claw_{it} + \beta_2 PostClaw_{it} + \beta_3 Relationship_{it} + \beta_4 InstituteLoan_{it} \\ & + \beta_5 InterestRate_{it} + \beta_6 FinancialCovenant_{it} + \beta_7 Collateral_{it} + \beta_8 Syndication_{it} \\ & + \beta_9 Dealsize_{it} + \beta_{10} Rating_{it} + \beta_{11} No\_Lender_{it} + \beta_{12} PPricing_{it} \\ & + \beta_{13} PrimaryPurpose_{it} + \beta_{14} Firmsize_{it} + \beta_{15} Profit_{it} + \beta_{16} Leverage_{it} \\ & + \beta_{17} Tangibility_{it} + \beta_{18} MB_{it} + \beta_{19} Indep_{it} + \beta_{20} InstituteOwn_{it} + \beta_{21} BoardMeet_{it} \\ & + \beta_{22} AuditCommitee_{it} + Year\ Dummy + Firm\ Dummy + \varepsilon_{it}. \end{aligned} \quad (4)$$

Maturity is the logarithm of the number of months between the facility issue date and the end date of the loan. *Collateral* is a dummy variable equal to one if the loan is

[6] We repeat the estimation of equation (3) using the natural logarithm of interest rates as the dependent variable. In untabulated results we find that the coefficient on *PostClaw* is still significantly negative (-0.169, t = -1.97).

collateralized, and zero otherwise. The other variables are as previously defined in the estimation of equation (1). The *t*-statistics are adjusted for heteroskedasticity and firm-level clustering.

The results for equation (4) are provided in Table 4, column (2). We find that the coefficient on *PostClaw* is significantly positive (0.169, t = 2.49), suggesting that clawback adoption leads to longer loan maturities. More specifically, firm-initiated clawbacks increase loan maturity by 1.2 months, which represents a 3% increase (1.2 / 40 months, which is the average loan maturity for clawback adopters before adopting clawbacks). The other control variables take the predicted signs whenever significant.

The effect of clawbacks on the likelihood of loan collateral

To examine the effect of firm-initiated clawbacks on the likelihood of collateral requirements, we estimate the following Logit regression, which is similar to the one used in Bharath et al. (2009):

$$\begin{aligned} Collateral_{it} = {} & \alpha + \beta_1 Claw_{it} + \beta_2 PostClaw_{it} + \beta_3 Relationship_{it} + \beta_4 InstituteLoan_{it} \\ & + \beta_5 Revolver_{it} + \beta_6 Syndication_{it} + \beta_7 Dealsize_{it} + \beta_8 Maturity_{it} + \beta_9 Rating_{it} \\ & + \beta_{10} No\_Lender_{it} + \beta_{11} PPricing_{it} + \beta_{12} PrimaryPurpose_{it} + \beta_{13} Firmsize_{it} \\ & + \beta_{14} Profit_{it} + \beta_{15} Leverage_{it} + \beta_{16} Tangibility_{it} + \beta_{17} MB_{it} + \beta_{18} Indep_{it} \\ & + \beta_{19} InstituteOwn_{it} + \beta_{20} BoardMeet_{it} + \beta_{21} AuditCommitee_{it} \\ & + Year\ Dummy + Industry\ Dummy + \varepsilon_{it}. \end{aligned} \quad (5)$$

Collateral is a dummy variable equal to one if the loan is collateralized, and zero otherwise. The other variables are as defined in the estimation of equation (1). The *z*-statistics are adjusted for heteroskedasticity and firm-level clustering.

The results for equation (5) are provided in Table 5, column (3). We find that the coefficient on *Claw* is insignificant (0.228, *z*= 1.07), suggesting that clawback adopters are as likely to face collateral requirements on loans prior to clawback adoption as control firms. The coefficient on *PostClaw* is significantly negative (-0.699, *z*= -2.16), however, suggesting that the likelihood of collateralized loans is lower after clawback adoption for clawback adopters. More specifically, the marginal probability associated with *PostClaw* is 0.115, indicating that clawbacks decrease the likelihood of loan collateral by around 16% (0.115/0.709, where 0.709 is the average percentage of collateralized loans during pre-adoption periods). The other control variables take the predicted signs whenever significant.

Robustness test – Heckman selection model

Before moving to the test of hypothesis H3, we address the concern raised by Tucker (2010), namely, that propensity-score matching only controls for *observed* differences across clawback adopters and control firms (e.g., clawback adopters have larger firm size). To the extent that this procedure does not fully account for the differences between clawback adopters and non-adopters, our results may still suffer from self-selection problems (although the inclusion of firm-fixed effects may overcome these unobservable differences). To further account for the differences between (in our context) clawback adopters and control firms, Tucker (2010) suggests that one perform both propensity-score matching and the Heckman test. We therefore confirm the robustness of our results using the Heckman model (also commonly known as the treatment effect model). An important feature of the Heckman model is the "excluding

restriction": we need to identify a variable that is correlated with clawback adoption but that does not affect loan pricing except through clawback provisions (Lennox et al. 2012; Larcker and Rusticus 2010). As we mention earlier, several studies examine the determinants of clawback adoption, but these studies do not find consistent results (e.g., Addy et al. 2009; Babenko et al 2012; Chen et al. 2012).[7] The only consensus emerging from these studies seems to be that larger firms are more likely to adopt clawbacks. However, because firm size is also an important determinant of loan pricing, it cannot be used to satisfy the excluding restriction. We therefore need to find an alternative variable. Toward this end, we employ the industry-level variable *PeerAdoption*, which we define as the prevalence of clawback provisions among peer firms in the same industry (2-digit SIC codes) before the specific company decides to adopt clawbacks. We expect that firms are more likely to adopt clawbacks when such provisions are popular among peer firms, that is, we expect a positive association between *PeerAdoption* and clawback adoption. More importantly, we do not expect *PeerAdoption* to be correlated with the adopting firm's loan terms.

To summarize, we estimate a logit model with clawback adoption as the dependent variable, and *PeerAdoption* together with the other variables (see page 14) used in the

[7]For example, Chan et al. (2012a), Babenko et al. (2012), and Chen et al. (2012) do not find that prior restatement history is correlated with clawback adoption, suggesting that financial reporting quality does not affect the decision to adopt clawbacks. In contrast, Dehaan et al. (2012) find that firms with a lower ERC are more likely to adopt clawbacks, and Addy et al. (2009) find that firms with more discretionary accruals are less likely to adopt clawbacks. Thus, empirical evidence on the association between clawback adoption and earnings quality is, at best, mixed. Moreover, Chan et al. (2012a) find that firms with more insider ownership are less likely to adopt clawbacks, while the other governance mechanisms do not account for clawback adoption. Addy et al. (2009) also document mixed evidence on the relation between clawback adoption and corporate governance. Specifically, Addy et al. (2009) show that firms with more CEO influence are *less* likely to adopt clawbacks, while firms with better governance are *more* likely to adopt clawbacks. In short, the evidence on the association between corporate governance and clawback adoption is mixed. Brown et al. (2011) and Chan et al. (2012a) conclude that firm size is the most important determinant of clawback adoption.

propensity-score matching procedure as independent variables to determine clawback adoption. Under the Heckman model, an inverse Mills' ratio (*IMR*) is produced from the choice model, which should be added to equations (1) to (5) to mitigate the self-selection problem associated with clawback adoption. We present this analysis in Table 5. In panel A, we first present the results of the first-stage selection model. Consistent with prior studies (e.g., Brown et al. 2011; Chan et al. 2012a), we find that clawback adoption is positively related to firm size and number of segments, and is negatively related to CEO tenure and insider ownership. More importantly, we find that clawback adoption is positively associated with our instrument – *PeerAdoption* (0.253, z = 7.52), suggesting that a company is more likely to initiate clawbacks if its industry peers do so. Next, we present the results of the second-stage treatment effect model in panel B of Table 5. To save space, we only present results on the key variable of interest, *PostClaw*, and the inverse Milles ratio (*IMR*). The results generally are consistent with those presented in Tables 3 and 4 – that is, the coefficient on *PostClaw* is significant with predicted signs in all columns. As indicated in Bharath ret al. (2009), it is difficult to examine the power of the instruments when the first-stage selection model concerns a binary indicator (in our case, *Claw*). Thus, as suggested in Lennox et al. (2012), we check the Variance-Inflation-Factors (VIFs) associated with the inverse Mills ratio (*IMR*), and *PostClaw*. We find that VIF with the two variables is all below 5, indicating that multicollinearity is not a concern in our model and our instruments are appropriate.

To conclude, our main conclusions are unaffected after using the Heckman model to control for unobservable differences across clawback and control firms.

The relation between restatement risk and the effects of clawbacks on bank loan contracts

To test hypothesis H3, which posits that clawback provisions have stronger effects on loan contracts if adopting firms have a higher *ex ante* likelihood of financial misstatements, we first rely on the *F*-score developed by Dechow et al. (2011) to proxy for the likelihood of accounting restatements. Specifically, borrowing the coefficients from the model of accounting restatements provided by Dechow et al. (2011), we compute *F*-scores for each of the clawback adopters prior to clawback adoption and then categorize these firms into high-risk adopters and low-risk adopters based on their *F*-score. In particular, clawback adopters with an above-median *F*-score are classified as having a high ex ante likelihood of financial misstatements prior to clawback adoption, while those with a below-median *F*-score are classified as low-risk adopters.

We next modify equations (1) to (5) by splitting *Claw* into *ClawHigh* and *ClawLow*, and *PostClaw* into *PostClawHigh* and *PostClawLow*. *ClawHigh* (*ClawLow*) is an indicator variable equal to one if the company is a clawback adopter and has a high (low) likelihood of accounting restatements, and *PostClawHigh* (*PostClawLow*) is an indicator variable equal to one for firm-years in which clawback adopters and control firms have clawback provisions in place and high (low) restatement risk. We then repeat the estimation of equations (1) to (5). To save space, we only present coefficients on the key variables of interest in Table 6.

In the first column of Table 6, we find that clawback provisions increase the use of financial covenants in bank loan contracts for high-risk adopters, but not for low risk adopters as only the coefficient on *PostClawHigh* is significantly positive. The difference

is statistically significant (*F*=3.89), indicating that banks consider clawback provisions more useful in improving accounting quality when the adopting firms have a higher likelihood of restatements. Turning to the result in columns (2) and (3), we find that although clawback provisions increase the use of performance pricing (or accounting-based performance pricing) for both high- and low-risk adopters, the effect is more pronounced for high-risk adopters. That is, firm-initiated clawbacks are found to significantly increase the use of accounting-based performance pricing for high-risk adopters, but not for low-risk adopters (the difference between the two groups is significant with a F-stat of 2.80). We also find that clawbacks reduce the interest rates charged on bank loans for high--risk adopters more significantly than for low-risk adopters. Again, the difference is statistically significant (*F*=3.90), indicating that banks consider clawback provisions more useful when the adopting firms have a higher likelihood of restatements. In the sixth column of the table, we find that clawback provisions reduce the likelihood of loan collateral for adopters with a higher likelihood of financial misstatements, but not for low-risk adopters, as the coefficient on *PostClawHigh* is significantly negative while that on *PostClawLow* is insignificantly positive. The difference is again statistically significant (*F* = 7.57). The results in column (5) provide no evidence that clawback provisions have different effects on loan maturity across high- and low-risk adopters.

To confirm the robustness of these results, instead of using the *F*-score we use restatement history to partition clawback adopters. In particular, if a clawback adopter issues a restatement during the three-year period prior to clawback adoption, it is classified as a high-risk firm; otherwise, it is classified as a low-risk firm. Among the

clawback adopters, 31% have at least one restatement during the three-year period prior to clawback adoption. Results of this analysis are presented in Panel B of Table 6. The conclusions are similar to those in Panel A. In short, high-risk adopters observe more pronounced increases in the use of financial covenants and accounting-based performance pricing, larger decreases in the interest rate charged on loans and in the likelihood of loan collateral following clawback adoption than low-risk adopters, while there is no significant difference in loan maturity between the two groups.

To summarize, the results in Table 6 support our prediction that banks regard clawback provisions as more useful for firms with a higher likelihood of financial misstatements.

V. Conclusion

In recent years a number of public firms in the U.S. have adopted "clawback" provisions allowing the board of directors to recoup incentive compensation from executives in the event of restatements. Several prior studies examine the effectiveness of firm-initiated clawbacks from equityholders' point of view (e.g., Addy et al. 2009; Gao et al. 2011; Chan et al. 2012a). These studies do not find consistent evidence on the effects of clawbacks on financial reporting quality. In particular, while clawbacks are found to deter accounting restatements, they are also shown to induce managers to switch from accruals manipulation to real transactions management to achieve earnings targets (Chan et al. 2012a; Chan et al. 2012b; Dehaan et al. 2012). In this study we add to the above literature by investigating the effects of firm-initiated clawbacks from the perspective of another group of stakeholders, namely, debtholders.

We find that firm-initiated clawbacks reduce the information asymmetry between borrowers and lenders. More specifically, using a difference-in-differences research design, we find that interest rates charged on loans are lower subsequent to clawback adoption, suggesting that banks consider clawback adopters as having better information quality following clawback adoption. Further, after adopting clawbacks, banks are less likely to request that borrowing firms provide collateral, and are more likely to lengthen loan maturity. Consistent with the notion that the use of financial covenants is increasing in the quality of accounting numbers (e.g., Costello and Wittenberg-Moerman 2011), we also observe an increase in number of accounting-based covenants in loan contracts after clawback adoption. Finally, we find that clawback provisions are more beneficial for adopters with higher likelihood of restatements as our findings aforementioned are mainly attributed to these firms. That is, relative to adopting firms with low risk of financial misstatements, high-risk adopters experience a more pronounced increase in the use of financial covenants and accounting-based performance pricing, and they also enjoy a greater decrease in loan interest rates and are less likely to be required to provide loan collaterals after initiating clawbacks.

In sum, our results demonstrate that banks perceive firm-initiated clawbacks as useful in promoting financial reporting integrity. Importantly, as debt financing is far more popular than equity financing, the findings in our study have important implications for the mandatory adoption of clawback provisions under the Dodd-Frank Act. In particular, even without access to proprietary financial information, clawbacks are still useful for debt holders. Together with existing studies, we therefore expect mandatory clawback provisions to lead to an overall improvement in financial reporting integrity.

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Table 1
Sample construction

Total firms in the Corporate Library dataset	3112
Minus: Financial institutions	660
Minus: Firms without necessary financial and governance data	269
	2183
Firms with clawback provisions as of year 2009	343
Firms without clawback provisions as of year 2009	1840

	Clawback firms (n=343)	Control firms (n = 1870)	Difference *t*-stat.
Firmsize	8.127	6.821	7.34***
Profit	0.156	0.124	3.29***
Leverage	0.211	0.194	1.88*

Table 2
Descriptive statistics – 147 pairs of clawback adopters and control firms based on propensity-score matching

	Clawback firms	Control firms	Mean t-test	Clawback firms	Control firms	Median z-test
Firmsize	8.280	8.075	1.63	8.163	8.016	1.35
Profit	0.148	0.138	1.16	0.134	0.133	1.08
Leverage	0.219	0.268	-2.12**	0.197	0.232	-2.22**
Tangibility	0.702	0.776	-2.49**	0.664	0.825	-2.54**
Rating	0.835	0.786	1.42	1.000	1.000	1.46
Indep	1.544	1.652	-1.63	1.383	1.518	-1.71*
InstituteOwn	2.430	2.238	1.86*	2.320	2.153	2.25**
BoardMeet	0.865	0.810	2.04**	1.000	1.000	2.03**
AuditCommittee	0.696	0.676	1.59	0.700	0.667	1.37
Bank loan terms						
Interest Rate	144.792	172.135	-2.80***	100.000	125.000	-2.62***
Maturity	43.548	44.555	-0.85	48.000	51.000	-1.15
Collateral	0.669	0.732	-2.32**	1.000	1.000	-2.36**
Financial Covenants	1.191	1.338	-1.60	1.000	1.000	-1.63
Relationship	0.841	0.866	-1.55	1.000	1.000	-1.54
InstituteLoan	0.077	0.100	-1.62	0.000	0.000	-1.61
Revolver	0.536	0.525	0.45	1.000	1.000	0.45
Syndication	0.972	0.964	0.68	1.000	1.000	0.68
Dealsize ($M)	1,135.991	836.502	2.09**	450.000	436.000	1.15
No_Lender	10.44	10.127	0.72	8.000	8.000	1.08
PPricing	0.489	0.537	-2.01**	1.000	1.000	-2.01**
ACCTPricing	0.223	0.205	0.85	0.000	0.000	0.85
No. of Obs.	742	824		742	824	

*, **, ***, represent significance at the 10%, 5%, and 1% levels, respectively, based on *t*-statistic for difference in means or based on *z*-statistic for difference in medians. See Appendix for variable definitions.

Table 3
Panel A: The effects of firm-initiated clawbacks on financial covenants

Dependent variable = *Financial Covenants*				
	Predicted sign	***(1)***	***(2)***	***(3)***
Claw	?	-0.035	-	-
		(-0.56)		
PostClaw	**+**	**0.196****	**0.278*****	**0.203*****
		(1.96)	**(2.66)**	**(2.56)**
Relationship	+	0.258	0.638***	0.100
		(1.33)	(3.12)	(1.47)
InstituteLoan	+	0.293***	0.294***	0.210***
		(2.99)	(2.98)	(5.49)
Revolver	?	-0.066	-0.074	-0.013
		(-1.19)	(-1.15)	(-0.48)
Interest Rate	+	0.001***	0.001***	0.001***
		(4.70)	(3.32)	(6.12)
Syndication	?	0.173	0.335	0.008
		(0.65)	(1.18)	(0.08)
Collateral	+	-0.308***	-0.362***	0.577***
		(-5.37)	(-4.44)	(16.07)
Dealsize	-	0.164**	0.142**	-0.072*
		(2.41)	(2.19)	(-1.82)
Maturity	+	0.001	-0.001	-0.001
		(0.35)	(-0.39)	(-0.94)
Rating	+	0.061	-	-
		(0.62)		
No_Lender	?	0.002	0.006	0.003***
		(0.51)	(1.48)	(2.90)
PPricing	+	1.039***	0.923***	0.737***
		(12.34)	(12.29)	(24.78)
PrimaryPurpose	+	0.004	0.136	-0.042
		(0.04)	(1.37)	(-1.05)
Firmsize	-	-0.317***	-0.096	-0.022
		(-8.76)	(-1.20)	(-0.55)
Profit	-	-0.665	-1.246	0.501*
		(-1.51)	(-1.54)	(1.65)
Leverage	+	0.257	-0.576**	-0.070
		(1.54)	(-1.97)	(-0.55)
Tangibility	-	-0.301***	-0.206	-0.196
		(-2.87)	(-0.80)	(-1.63)
MB	-	-0.004	0.005	0.002
		(-0.29)	(0.36)	(0.56)

Current_Ratio	-	-0.034	0.018	-0.004
		(-0.75)	(0.28)	(-0.21)
Log_Coverage	-	-0.058*	-0.058	-0.241***
		(-1.87)	(-1.26)	(-4.70)
Indep	-	0.110	0.677*	0.331
		(0.49)	(1.87)	(1.17)
InstituteOwn	-	0.091	0.164**	0.040
		(1.25)	(2.25)	(0.71)
BoardMeet	-	0.001	0.005	0.008
		(0.01)	(0.47)	(0.99)
AuditCommittee	-	-0.019	0.026	0.014
		(-0.74)	(0.57)	(0.40)
Industry fixed effects		*Yes*	*No*	*No*
Firm fixed effects		*No*	*Yes*	*Yes*
Year fixed effects		*Yes*	*Yes*	*Yes*
Pseudo-R^2		0.21	0.25	0.32
n		1,566	1,418	6,718

The regressions are estimated using Poisson model. Column (1) presents the Poisson regression results with industry and year fixed effects of 147 matched pairs of clawback adopters and non-adopters (based on the propensity-score-matching) which have at least one loan contract issued during the pre-adoption period and at least one loan contract issued during the post-adoption period. Column (2) presents results suing the same sample as in column (1), but with the inclusion of firm- and year-fixed effects. Column (3) presents the Poisson regression results with firm and year fixed effects based on the entire sample of clawback adopters and non-adopters with available data in DealScan. The *z*-statistics based on robust standard errors clustered by firm are reported in parentheses. ***, **, * indicates significance at the 0.01, 0.05, and 0.10 level, respectively. See Appendix for variable definitions.

Panel B: The effects of firm-initiated clawbacks on performance pricing provisions

	Predicted sign	***(1) PPricing***	***(2) ACCTPricing***
Claw	?	-0.135	0.096
		(-0.70)	(0.39)
PostClaw	**+**	**0.648****	**0.752****
		(2.33)	**(2.15)**
Relationship	-	1.267***	1.322**
		(2.76)	(2.16)
InstituteLoan	+	-0.762	-1.079
		(-1.34)	(-1.37)
Revolver	+	0.728***	0.374*
		(4.72)	(1.91)
Syndication	?	0.486	-
		(0.71)	
Collateral	+	-2.579	-0.376
		(-1.38)	(-1.56)
Dealsize	?	0.241**	0.220*
		(2.54)	(1.67)
Maturity	+	0.003	0.012**
		(0.69)	(2.11)
Rating	+	0.166	-1.002***
		(0.58)	(-3.45)
No_Lender	-	0.104***	0.038**
		(7.66)	(2.20)
PrimaryPurpose	+	0.252	0.526
		(0.87)	(1.57)
Firmsize	-	-0.801***	-1.130***
		(-8.42)	(-2.73)
Profit	+	-0.585	2.116
		(-0.34)	(1.11)
Leverage	+	0.828*	1.336***
		(1.82)	(2.64)
Tangibility	-	-0.432	-1.135***
		(-1.42)	(-2.73)
MB	+	-0.023	-0.019
		(-0.55)	(-0.45)
Indep	-	1.757**	0.385
		(2.74)	(0.47)
InstituteOwn	+	0.144	0.252
		(0.75)	(1.08)
BoardMeet	+	-0.015	-0.081*

		(-0.66)	(-1.91)
AuditCommittee	+	-0.021	-0.237*
		(-0.24)	(-1.61)
Industry fixed effects		*Yes*	*Yes*
Firm fixed effects		*No*	*No*
Year fixed effects		*Yes*	*Yes*
Pseudo-R^2		0.34	0.34
n		1,566	1,322

The regressions are estimated using Probit model with industry and year fixed effects of 147 matched pairs of clawback adopters and non-adopters (based on the propensity-score-matching) which have at least one loan contract issued during the pre-adoption period and at least one loan contract issued during the post-adoption period. Column (1) presents the results of adoption of clawback provisions on the likelihood of including a performance pricing option in loan contract. Column (2) presents the results of adoption of clawback provisions on the likelihood of including an accounting based performance pricing provision in loan contract. The *t*-statistics based on robust standard errors clustered by firm are reported in parentheses. ***, **, * indicates significance at the 0.01, 0.05, and 0.10 level, respectively. See Appendix for variable definitions.

Table 4
The effects of firm-initiated clawbacks on bank loan interest rates, loan maturity and the likelihood of having collateralized loans

	(1) ***Interest Rate***	***(2)*** ***Maturity***	***(3)*** ***Collateral***
Claw	-	-	0.228
			(1.07)
PostClaw	**-33.246*** **	**0.169** **	**-0.699** **
	(-3.43)	**(2.49)**	**(-2.16)**
Relationship	-39.319***	0.192*	0.402
	(-2.71)	(1.88)	(0.72)
InstituteLoan	26.078**	0.297***	2.046***
	(2.45)	(4.44)	(3.09)
Interest Rate	-	0.001	-
		(0.30)	
Revolver	-21.099***	-	0.005
	(-3.30)		(0.03)
Financial Covenant	12.422***	0.005	-
	(3.94)	(0.20)	
Syndication	-20.269	0.027	-1.042
	(-1.01)	(0.45)	(-1.54)
Dealsize	-7.439**	0.518***	0.126
	(-2.10)	(3.66)	(1.09)
Maturity	0.087	-	0.015***
	(0.59)		(2.97)
Rating	-	-	0.481
			(1.54)
No_Lender	-1.779***	0.003	-0.037***
	(-4.16)	(0.89)	(-2.70)
PPricing	-42.968***	0.134***	-2.853***
	(-6.07)	(2.69)	(-11.19)
PrimaryPurpose	31.462***	-0.159**	0.368*
	(3.19)	(-2.30)	(1.02)
Firmsize	-4.984	-0.149	-0.457***
	(-0.58)	(-1.50)	(-4.34)
Profit	-198.700**	-0.002	-5.331***
	(-2.60)	(-0.00)	(-2.71)
Leverage	19.682	0.237	3.194***
	(0.64)	(1.15)	(5.17)
Tangibility	-49.169	-0.432	-0.893**
	(-1.60)	(-1.42)	(-2.34)
MB	1.420	-0.012	-0.015
	(0.97)	(-1.13)	(-0.25)

Current_Ratio	-21.638***	-	-
	(-3.28)		
Log_Coverage	-1.579	-	-
	(-0.37)		
Indep	-29.667	0.428*	0.125
	(-0.86)	(1.79)	(0.17)
InstituteOwn	12.657	-0.069	0.220
	(1.62)	(-1.29)	(0.78)
BoardMeet	2.018	0.001	0.035
	(1.50)	(0.01)	(1.16)
AuditCommittee	2.497	0.010	-0.039
	(0.67)	(0.39)	(-0.44)
Industry fixed effects	*No*	*No*	*Yes*
Firm fixed effects	*Yes*	*Yes*	*No*
Year fixed effects	*Yes*	*Yes*	*Yes*
Pseudo-R^2/Adj-R^2	0.53	0.55	0.37
n	1,566	1,566	1,566

The regressions are estimated using 147 matched pairs of clawback adopters and non-adopters (based on the propensity-score-matching) which have at least one loan contract issued during the pre-adoption period and at least one loan contract issued during the post-adoption period. Column (1) presents the OLS regression results of adoption of clawback provisions on interest rate with firm and year fixed effects. Column (2) presents the OLS regression results of adoption of clawback provisions on loan maturity with firm and year fixed effects. Column (3) presents the Logit regression results of adoption of clawback provisions on the likelihood of requiring collateral in the loan contract with industry and year fixed effects. The *t*-statistics (*z*-statistics) based on robust standard errors clustered by firm are reported in parentheses. ***, **, * indicates significance at the 0.01, 0.05, and 0.10 level, respectively. See Appendix for variable definitions.

Table 5
Treatment effects model

Panel A *1st stage model - Selection model*	
	Claw
Size	0.539***
	(11.04)
Leverage	0.014
	(0.04)
Profit	0.227
	(0.42)
MB	0.001
	(0.80)
Ln_segment	0.207***
	(2.79)
Prior_restate	0.196
	(1.38)
Indep	0.480
	(0.94)
InstituteOwn	0.044
	(0.47)
BoardMeet	0.019
	(1.32)
AuditCommittee	0.015
	(0.25)
Insiderown	-1.475***
	(-3.30)
CEO Tenure	-0.281*
	(1.84)
PeerAdoption	0.253***
	(7.52)
Industry fixed effects	*Yes*
Firm fixed effects	*No*
Year fixed effects	*Yes*

Panel B *2nd stage model - Treatment effects model*						
	Financial Covenant	***PPricing***	***ACCTPricing***	***Interest Rate***	***Maturity***	***Collateral***
PostClaw	0.334**	0.782***	0.806**	-32.602**	0.172**	-0.700**
	(1.96)	(3.08)	(2.52)	(-2.56)	(2.00)	(-2.23)
IMR	0.001	0.289	0.295	-3.083	-0.139	1.035
	(0.01)	(1.57)	(1.61)	(-0.42)	(-1.40)	(0.20)
Industry fixed effects	*No*	*Yes*	*Yes*	*No*	*No*	*Yes*
Firm fixed effects	*Yes*	*No*	*No*	*Yes*	*Yes*	*No*
Year fixed effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Control Variables	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Pseudo-R^2/Adj-R^2	0.20	0.34	0.41	0.46	0.52	0.43
n	6,718	6,718	6,024	6,718	6,718	6,718

Pseudo-R^2/Adj-R^2	0.22
n	15,012

Panel A presents the first stage Logit regression model with industry and year fixed effects on the determinants of adopting clawback provisions for the entire sample of clawback adopters and non-adopters. Panel B presents the second stage regression model including the inverse Mills ratio (*IMR*) estimated from the first stage based on the entire sample of clawback adopters and non-adopters with available data in DealScan. The *z*-statistics (*t*-statistics) corrected for heteroskedasticity and firm-level clustering of standard errors are reported in parentheses. *, **, ***, represent significance at the 10%, 5%, and 1% levels, respectively. See Appendix for variable definitions.

Table 6

Clawback adopters with higher *ex ante* restatement risk versus those with lower risk

Panel A: Restatement risk as proxied by F-score

	Dependent variable =					
	Financial Covenant	*PPricing*	*ACCTPricing*	*Interest Rate*	*Maturity*	*Collateral*
ClawHighRisk	-0.118	-0.387*	-0.213	17.232	-0.169	-0.359
	(-1.50)	(-1.64)	(-0.68)	(1.09)	(-0.27)	(-0.92)
ClawLowRisk	0.054	-0.682**	-0.084	-2.868	0.051	-0.246
	(0.61)	(-2.04)	(-0.20)	(-0.21)	(0.74)	(-0.67)
PostClawHighRisk	0.325***	0.932**	1.192**	-38.501***	0.267**	-1.403**
	(2.72)	(2.27)	(2.30)	(-2.53)	(3.32)	(-2.32)
PostClawLowRisk	0.103	0.881**	0.726	-18.205	0.155	0.551
	(1.15)	(2.07)	(1.27)	(-1.08)	(1.58)	(1.12)
Control variables	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Industry fixed effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Year fixed effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Test for PostClawHighRisk =PostClawLowRisk	3.89**	0.01	2.80*	3.90**	1.01	7.57***
Pseudo-R^2/Adj-R^2	0.24	0.36	0.37	0.57	0.34	0.41
n	1,566	1,566	1,322	1,566	1,566	1,566

Panel B: Restatement risk as proxied by prior restatement history

	Dependent variable =					
	Financial Covenant	*PPricing*	*ACCTPricing*	*Interest Rate*	*Maturity*	*Collateral*
ClawRestate	0.028	-0.052	0.297	30.081	0.066	-0.096
	(0.34)	(-0.18)	(0.83)	(1.54)	(0.82)	(-0.22)
ClawNoRestate	-0.096	-0.407*	-0.086	-12.253	0.005	-0.524
	(-1.16)	(-1.61)	(-0.28)	(-1.54)	(0.08)	(-1.57)
PostClawRestate	0.279**	0.769*	0.899**	-46.381**	0.253***	-1.519***
	(2.11)	(1.91)	(1.96)	(-2.30)	(2.62)	(-2.88)
PostClawNoRestate	0.104	0.353	0.378	5.689	0.169*	-0.076
	(0.40)	(0.98)	(0.71)	(0.43)	(1.95)	(-0.18)
Control variables	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Industry fixed effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Year fixed effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Test for PostClawRestate =PostClawNoRestate	3.85**	0.71	3.95**	7.65***	0.58	5.33**

Pseudo-R^2/Adj-R^2	0.22	0.34	0.36	0.57	0.37	0.42
n	1,566	1,566	1,322	1,566	1,566	1,566

The regressions are estimated with industry and year fixed effects of 147 matched pairs of clawback adopters and non-adopters (based on the propensity-score-matching) which have at least one loan contract issued during the pre-adoption period and at least one loan contract issued during the post-adoption period. The *z*-statistics (*t*-statistics) corrected for heteroskedasticity and firm-level clustering of standard errors are reported in parentheses. *, **, ***, represent significance at the 10%, 5%, and 1% levels, respectively. See Appendix for variable definitions.

Appendix: Variable definitions

Claw	1 if firm is in the treatment group (clawback adopters) and 0 if firm is instead in the control group (non-adopters)
PostClaw	1 for firm-years in which clawback adopters have clawback provisions in place, and 0 otherwise
ClawHighRisk	1 if firm is in the treatment group (clawback adopters) and has high restatement risk, i.e. high *F*-score, and 0 otherwise
ClawLowRisk	1 if firm is in the treatment group (clawback adopters) and has low restatement risk, i.e. low *F*-score, and 0 otherwise
PostClawHighRisk	1 for firm-years in which clawback adopters have clawback provisions in place and have high restatement risk, i.e. high *F*-score, and 0 otherwise
PostClawLowRisk	1 for firm-years in which clawback adopters have clawback provisions in place and have low restatement risk, i.e. low *F*-score, and 0 otherwise
ClawRestate	1 if firm is in the treatment group (clawback adopters) and has any earnings being restated during prior three years, and 0 otherwise
ClawNoRestate	1 if firm is in the treatment group (clawback adopters) and has no earnings being restated during prior three years, and 0 otherwise
PostClawRestate	1 for firm-years in which clawback adopters have clawback provisions in place and haveany earningsbeing restated during prior three years, and 0 otherwise
PostClawNoRestate	1 for firm-years in which clawback adopters have clawback provisions in place and haveno earnings being restated during prior three years, and 0 otherwise
Financial covenant	The number of financial covenants included in the loan contract
PPricing	1 if the loan has a performance pricing option, and 0 otherwise
ACCTPricing	1 if the loan has an accounting based performance pricing option, and 0 otherwise
Interest Rate	The amount the borrower pays in basis points over LIBOR for each dollar drawn down. It includes the spread of the loan with any annual (or facility) fee paid to the bank group
Maturity	The number of months between the facility's issue date and the end date of the loan
Collateral	1 if the loan is collateralized, and 0 otherwise
Relationship	1if at least one of the loan's lead banks had been a lead bank of the borrower's previous loans in the last five years before the present loan and zero otherwise.
InstituteLoan	1 if the loan's type is an institutional term loan, and 0 otherwise
Revolver	1 if the loan's type is a revolver loan, and 0 otherwise
Syndication	1 if the loan's type is a syndication loan, and 0 otherwise
Dealsize	Natural logarithm of the loan's amount
Rating	1 if the firm had senior debt rating by S&P at the time of the loan's issuance, and 0 otherwise
No_Lender	Number of participating lenders in the loan, including the arranger
PrimaryPurpose	1 if the loan's primary purpose is used to takeover, LBO/MBO or recapitalization

Firmsize	Natural logarithm of total assets in the year prior to entering into a loan contract
Profit	EBITDA to total assets in the year prior to entering into a loan contract
Leverage	Long-term debt divided by total assets in the year prior to entering into a loan contract
Tangibility	Net PPE and inventory to total asset in the year prior to entering into a loan amount
MB	Ratio of market value of equity to book value of equity in the year prior to entering into a loan contract
Current_Ratio	Ratio of current assets to current liabilities in the year prior to entering into a loan contract
Log_Coverage	Natural logarithm of 1+ the ratio of EBITDA to interest expense in the year prior to entering into a loan contract
Indep	Percentage of independent directors on the board
InstituteOwn	Percentage of institutional ownership
BoardMeet	Number of board meetings held per year
AuditCommittee	Number of audit committee members
Ln_Segment	Natural logarithm of 1 + number of segments
Prior_Restate	1 if the firm has restated its earnings in any of the previous three years, and 0 otherwise
InsiderOwn	Percentage of insider ownership
CEO Tenure	Log of CEO tenure
PeerAdoption	Percentage of peer firms in the same industry (2 digit SIC codes) having clawback provisions before the firm adopts clawbacks
IMR	Inverse Mills' ratio calculated from the Heckman model.