Does Voluntary Adoption of a Clawback Provision Improve Financial Reporting Quality?

ED DEHAAN, *University of Washington*

FRANK HODGE, *University of Washington*

TERRY SHEVLIN[†], *University of Washington*

Michael G. Foster School of Business
PACCAR HALL, Box 353226
4295 E. Stevens Way NE
University of Washington
Seattle, WA 98195-3226

December 27, 2011

ABSTRACT
We examine whether financial reporting quality improves after firms voluntarily adopt a compensation clawback clause. Clawback clauses allow companies to recoup excess incentive pay in the event of an accounting restatement, and are intended to *ex ante* deter managers from publishing misstated accounting information and *ex post* penalize managers who do so. For the period 2007 – 2009, our difference-in-difference analysis reveals significant improvements in both actual and perceived financial reporting quality following clawback adoption, relative to a propensity-matched set of control firms. We also find an increase in compensation for CEOs subject to new clawback provisions, as well as an increase in the sensitivity of cash compensation to accounting performance. Overall, our findings suggest that voluntarily adopting a clawback provision tends to improve a firm's financial reporting quality. In the cross-section, our findings indicate clawback policies that apply to restatements involving intentional and unintentional restatements are incrementally more effective than policies that apply only to restatement that involve fraud.

Keywords: financial reporting quality, executive compensation, clawback provisions.

JEL Descriptors: K22, M41, M48, M52

---

†Contact author: shevlin@uw.edu.
We thank Weili Ge, Michelle Hanlon, Jared Jennings, David Maber, Rick Mergenthaler, Mort Pincus, Shivaram Rajgopal, and Ryan Wilson for their helpful suggestions. We also acknowledge helpful feedback from workshop participants at the Foster School of Business, University of California-Irvine, and the University of Southern California on an earlier version of this paper. Frank Hodge acknowledges support of the Herbert O. Whitten Professorship. Terry Shevlin acknowledges financial support from the Paul Piggot/PACCAR Business Administration Professorship. Data support kindly provided by The Corporate Library. Thanks to Equilar, an executive compensation research firm, for providing complimentary use of their reports.

1. Introduction

We examine how voluntarily adopting a compensation clawback clause affects a firm's financial reporting quality.[1] Clawback clauses allow firms to recoup compensation from executives upon occurrence of a pre-defined trigger event. Clawbacks became increasingly prevalent following the Sarbanes-Oxley Act of 2002 (SOX), and are intended to more directly link an executive's compensation to his or her behavior. As such, clawbacks are a type of corporate governance mechanism designed to ex ante *deter* executives from publishing misstated accounting information, and ex post *penalize* executives who do so. While voluntarily adopted clawback clauses can have various trigger events, ranging from ethical misconduct to executive fraud, clawback provisions that are triggered in the event of an accounting restatement are the most common and the focus of this study. We examine changes in a sample of firms that voluntarily adopt such clawbacks prior to 2010 against a propensity-matched sample of control firms to provide insights into whether adopting a restatement-triggered clawback provision improves a firm's financial reporting quality.

Following the financial reporting failures of the late 1990s and the financial crisis in the 2000s, many have criticized how executives are compensated and the incentives such compensation packages introduce (Bebchuk and Fried 2003, 2004; Devers et al. 2007). While stock-based and performance-based compensation is a means of mitigating effort aversion, such compensation also encourages managers to manipulate accounting information in order to maximize their pay. The unobservable nature of executive effort and ability incentivizes executives to manipulate metrics used by shareholders to judge performance. Jensen (2005) argues that stock-based compensation is an impetus for aggressive financial reporting because it provides added incentive for managers to increase short-term stock prices. Examining Jensen's conjecture, academic research has shown a positive relation between stock-based compensation and accounting restatements (Efendi, *et al.* 2007; Burns and Kedia 2006), and the cost of restatement to shareholder wealth has been documented in the billions (Karpoff et al. 2008b). Given that restatements are

[1] We define "financial reporting quality" as the extent to which financial statements represent a diligent and unbiased application of financial reporting standards.

significant economic events with well-documented negative consequences, and that there appears to be a link between restatements and executive compensation, firms face a strong incentive to structure an executive's compensation contract to reduce the risk of a restatement. Clawback provisions are intended to reduce the risk of restatement by increasing the probability that a manager will repay ill-gotten performance-based compensation, thereby reducing the expected benefit to executives from misreported financial statements.

Prior to 2005, just three percent of Fortune 100 companies disclosed having a clawback clause in their executive compensation contracts. By 2010, that number had jumped to 82 percent (Equilar 2009, 2011). This rapid increase in the number of firms that disclose clawback policies is likely, at least in part, driven by SOX, which requires the Securities and Exchange Commission (SEC) to pursue CEOs and CFOs of listed companies to pay back excess incentive compensation in the event of a restatement due to misconduct. Given the SEC's limited resources and the difficulty in proving that a restatement was the result of executive misconduct, very few clawback cases have made it to court since SOX was adopted (Fried and Shilon 2011). Soon, however, all listed firms will be required by the Dodd-Frank Act to adopt a clawback provision that is triggered by a restatement, regardless of fault (Dodd-Frank Act 2010). The Dodd-Frank Act instructs the SEC to provide guidance to firms on the details of such policies and whom they must cover. The SEC expects to issue its guidance in late 2011 (Thomson Reuters 2011).

Though mandated clawback adoption is forthcoming, hundreds of firms voluntarily adopted clawbacks between 2006 and 2010 – long before the Dodd-Frank Act was drafted and finalized. We develop four hypotheses relating to the expected effects of voluntarily adopting a clawback provision. Our hypotheses predict that voluntarily adopting a clawback provision will influence managers, their compensation, board members, and external financial statement users. We first predict that voluntarily adopting a clawback provision will improve the quality of a firm's financial reporting. Consistent with our prediction, we find evidence of a decrease in meet-or-beat behavior and unexplained audit fees for clawback adopters relative to propensity-matched control firms. We also document a decrease in restatements following clawback adoption, though this result should be interpreted with some caution as adopting a clawback provision simultaneously decreases managers' incentives to file amended financial statements. We also predict that voluntarily adopting a clawback provision will improve perceptions of financial statement quality, both by external users and Boards of Directors. Our results indicate that voluntary adopters experience a significant increase in earnings response coefficients and a significant decrease in analyst forecast dispersion relative to control firms. These results are consistent with investors and analysts perceiving financial statements to be of higher quality after clawback adoption. Consistent with Boards of Directors also perceiving earnings as a higher quality measure of firm performance after adoption, we find a significant increase in the responsiveness of CEO cash compensation to accounting performance (i.e., pay-for-performance) among adopting firms.

Finally, we predict that managers, having assumed additional risk as a result of their firm adopting a restatement-triggered clawback, will demand additional pay to compensate for the increased risk. Consistent with our prediction, we find an increase in total CEO compensation following clawback adoption relative to control firms. The increase in total compensation is primarily driven by higher base salary. Overall, our results are consistent with voluntary adoption of a clawback provision improving corporate governance by improving both actual and perceived financial reporting quality.

We also investigate whether a key cross-sectional characteristic of clawback provisions affects the relations examined above: some clawback provisions state that compensation will be repaid in the event of any restatement, while others only require repayment for restatements involving intentional misconduct. The former, which we refer to as a "robust" clawback, is a more stringent provision that should incent managers to reduce both intentional and unintentional financial statement error. The latter, referred to as a "misconduct" clawback, should discourage intentional error but is less likely to impact unintentional misstatements. Cross-sectional tests provide some evidence that the robust clawback provision has an incremental effect on financial reporting quality and compensation than a misconduct clawback provision.

Our study examines the consequences of a firm voluntarily adopting a new governance mechanism. Like other studies that examine voluntary adoptions, there is a concern that our results may be influenced by omitted variables that are correlated with both clawback adoption and the dependent variables of interest (Armstrong et al. 2010; Roberts and Whited 2011). We employ several empirical design methods to mitigate this risk. First, we use a difference-in-differences design for analyzing changes in financial reporting and compensation before/after clawback adoption. The difference-in-differences design reduces concerns that unobserved time-invariant variables or market-wide forces drive the observed changes in the dependent variables. Second, we select our control firms for the difference-in-differences analysis using a propensity-matching method that allows us to control for a range of observable firm condition, performance, compensation, and governance variables that determine clawback implementation. The propensity match also mitigates concerns over violations of the "parallel trends" assumption that is critical to the difference-in-differences design (Lemmon and Roberts 2010; Roberts and Whited 2011). Third, we include a number of control variables in our difference-in-differences regressions to reduce the possibility of bias from observable, time-varying firm characteristics. Finally, we perform supplementary analysis to: (i) ensure that the difference-in-differences treatment and control firms have similar trends in the dependent variables prior to clawback adoption, and (ii) reduce concerns that our results are driven by other governance measures that might be implemented in conjunction with clawback provisions.

Our study makes several contributions. First, our study contributes to the literature that links corporate governance and executive compensation to financial reporting quality and the use of accounting information in capital markets. This broad stream of literature shows that a large portion of top executives' compensation at public firms comes from incentive compensation in the form of bonuses and equity (Gao *et al*. 2011), that incentive compensation is often linked to accounting numbers (Healy 1985; Bebchuk and Fried 2004), and that accounting numbers are sometimes managed to achieve compensation targets (Healy 1985; Guidry *et al*., 1999), Our findings that clawback provisions improve both actual and perceived financial reporting quality compliment this literature by providing further evidence that: (i) managers have the ability to manipulate financial statements in accordance with their personal incentives, (ii) investors recognize and respond to this agency problem by altering their reliance on financial statement numbers, and (iii) boards of directors have the ability to reduce incentive misalignments with relatively simple governance measures.

Second, we inform organizations and individuals who desire to better understand the consequences of clawback adoption. For example, the Dodd-Frank Act directs the SEC to provide guidance to firms on what their clawback provisions should contain and which employees they should cover. In addition, the Office of Superintendent of Financial Institutions (OSFI), which regulates financial institutions in Canada, recommends clawback adoption as a "best practice." Better understanding the effects of clawback adoption allows parties such as the SEC and OSFI to make more informed decisions about the guidance they provide. While we cannot unequivocally predict that the effects we observe for voluntary adopters will apply to all mandatory adopters, our findings do indicate that clawbacks improve actual and perceived financial reporting quality in our sample of firms. Other parties we inform include organizations not required by Dodd-Frank to adopt clawback clauses: non-public companies, government entities, not-for-profit organizations, etc. Our results should help these organizations make more informed decisions about whether they should perhaps consider adoption.

We are aware of two concurrent working papers that examine the determinants of clawback implementation, and one paper that examines the consequences of voluntarily adopting a clawback. Using logit regressions, Brown et al. (2011) and Addy et al. (2011) both find that the likelihood of clawback implementation is increasing with size and decreasing with CEO influence on the Board of Directors. Unlike these papers, our paper focuses on the consequences of clawback adoption, not on the determinants of adoption. More closely related to our study is a working paper by Chan et al. (2011) who also examine the consequences of clawback adoption and find that clawback adoption is associated with: (i) a reduction in accounting restatements, (ii) an increase in earnings response coefficients, and (iii) improved audit outcomes. In addition to several methodology improvements to our overlapping tests (discussed where relevant throughout the paper), our paper contributes beyond Chan et al. (2011) by investigating two additional effects of clawback adoption. First, we examine the effects of clawback provisions on the amount and composition of CEO compensation, as well as the pay-for-performance sensitivity. Given that clawback policies are a compensation contract modifications that can significantly alter a CEO's risk and expected (net) pay, understanding how clawbacks impact CEO compensation is essential to understanding the overall impact of the adopting a clawback provision. Second, we examine a significant source of cross-sectional variation within clawback provisions: whether compensation must be repaid in the event of any restatement or only those involving fraud.

The remainder of our paper is organized as follows. In section 2 we discuss theory and present our hypotheses. In Section 3 we describe our empirical proxies. In Section 4 we discuss how we constructed our sample. Section 5 includes our hypothesis testing, and Section 6 includes our cross-sectional tests. We conclude in section 7.

2. Hypothesis Development

An executive's most direct incentive for misreporting financial statements is the increase in personal wealth to be gained from performance-based compensation and inflated stock price.[2] The executive's disincentive is that, if caught, he or she will likely suffer reputational damage, career penalties, and potentially significant SEC fines (Karpoff et al. 2008a). Without a clawback provisions, executives will likely retain ill-gotten bonuses even after their dismissal; for example, Franklin Raines of Fannie Mae kept millions of dollars in bonuses that were received on $6.3 billion of overstated earnings (Fried and Shilon 2011). An effective clawback clause increases the cost of being caught by increasing the likelihood that the firm's shareholders and board of directors will be able to recapture erroneously awarded performance-based compensation. Thus, a clawback clause triggered by a restatement is intended to

[2] As discussed by Erickson et al. (2006), there is conflicting empirical evidence on whether accounting misstatements are linked to executive compensation. Still, other than to increase personal wealth via monetary compensation or less direct means (e.g. enhanced career prospects), there are few compelling alternative theories for why CEOs engage in earnings management.

decrease executives' incentives to manipulate financial statements. Such clawback provisions do not directly penalize operational decisions; rather, they decrease the expected gain from misreporting those decisions.

An effective clawback provision should serve as a deterrent to intentional or unintentional misreporting of accounting information. As such, we expect financial statement quality to increase post adoption. We define "quality" as the extent to which financial statements represent a diligent and unbiased application of financial reporting standards.[3] Low quality can be a result of either intentional manipulation or unintentional errors. If a clawback increases a manager's incentive to produce high-quality accounting information, then financial reporting quality should increase following clawback adoption.

H_1: Financial reporting quality increases following voluntary clawback adoption.

Improving actual financial reporting quality is one possible reason a firm may choose to voluntarily adopt a clawback provision. Another is to signal to market participants that a firm's financial statements are already reliable and free from manipulation. For example, the manager of a firm with inherently volatile cash flows and accruals may be concerned that investors doubt the veracity of the firm's financial statements relative to an operationally similar firm with more stable cash flows and accruals. If clawbacks are thought to be effective, adopting a clawback provision is a way for the manager to signal the quality of his financial statements. Our second prediction focuses on market participants' perceptions of reporting quality. If market participants perceive that reporting quality has improved, or they interpret voluntary adoption as a signal that a firm's financial statements are less subject to manipulation or error, we expect market participants to perceive the firm's financial statements as being of higher quality.

H_2: Market participants' perceptions of financial reporting quality improve following voluntary clawback adoption.

[3] Note that our definition of financial reporting quality is different than the definition of "earnings quality" provided in Dechow et al. (2010). As per Dechow et al., "higher quality earnings provide more information about the features of a firm's financial performance that are relevant to a specific decision made by a specific decision-maker" (page 344). Our definition of financial reporting quality does not require that the accounting information be relevant for predicting outcomes or decision-making, but simply that it be a diligent and unbiased application of GAAP.

Our third hypothesis focuses on the Board of Directors' reliance on accounting numbers in compensating executives. If board members perceive earnings as a higher quality measure of firm performance post adoption, they should place additional weight on earnings (i.e., increase the pay-earnings performance sensitivity) in determining incentive-based compensation.

H_3: Pay-for-earnings performance sensitivity increases following voluntary clawback adoption.

Our final hypothesis focuses on the costs to a manager of agreeing to a clawback clause. An effective clawback provision will increase the probability of a manager having to return compensation for reasons beyond his control, thereby imposing additional risk on the manager.[4] Assuming an efficient labor market, a manager of a clawback-adopting firm will demand a change in compensation such that he has the same expected net pay before and after adoption. One method of accomplishing this is to increase the amount of compensation that is not subject to a clawback. This will result in higher gross compensation to the executive (although expected net compensation is unchanged).

A second way to accomplish the same net pay pre- versus post-adoption is to shift from incentive-based compensation to base pay that is not subject to the clawback, in which case there will be a change in compensation composition but potentially no change in total compensation. A third option is to increase the amount of compensation that is subject to the clawback such that the expected net incentive pay, after impounding the probability of clawback enforcement, is the same post-adoption as it was pre-adoption. In practice, it is likely that combinations of these three options are used, with the balance depending on the executives' and boards' preferences and expectations about the probability of restatement. Note that these predictions are not inconsistent with the preceding hypothesis. Here we are predicting intercept shifts in the levels of salary and/or incentive pay. In H3 we predict a change in the slope of incentive pay with respect to accounting performance.

Thus, we test three, non-mutually-exclusive hypotheses regarding compensation levels:

---

[4] Even though some clawback provisions only require repayment in the event of manager culpability (see Section 6 for further discussion), it is likely impossible to definitively determine exactly who was and who was not responsible for a restatement. Thus, all types of clawback impose some additional risk on the manager.

H_{4a}: Total CEO compensation increases following clawback adoption.

H_{4b}: CEO base pay increases following clawback adoption.

H_{4c}: CEO incentive pay increases following clawback adoption.

We might not observe results consistent with our four hypotheses for several reasons. First, prior literature documents that restatements are associated with managers suffering reputational damage, career penalties, and significant SEC fines (Karpoff et al 2008a; Hennes et al. 2008). One could argue that these costs likely overwhelm any incremental cost associated with the adoption of a clawback clause. Second, it is possible that managers do not perceive clawbacks as a viable threat, and therefore do not see a reason to change their behavior upon adoption. Third, even if managers do perceive adoption as a viable threat, it is possible that investors, analysts, and board members will not view the clawback as a credible signal, and will therefore not immediately alter their views on financial reporting quality.

3. Empirical Proxies

3.1. Proxies for financial reporting quality

As noted, we define "quality" financial reporting as producing financial statements that represent a diligent and unbiased application of the financial reporting standards on which they are based. Dechow et al. 2010) discuss the multitude of measures that have been used as proxies for similar constructs. For our purposes, suitable proxies must satisfy two criteria: (i) we must be able to make an unambiguous directional prediction regarding the effect of clawback adoption on the proxy; and (ii) as we have just four quarters of post-adoption data for firms that implement a clawback provision in 2009, the proxy must be able to identify changes in financial reporting quality over a short period of time. Few empirical proxies satisfy these criteria. We identify two suitable proxies for our analysis of H1: meet/beat frequency and unexplained audit fees.

3.1.1. Meet/beat frequency

A common proxy for financial reporting quality first explored by Burgstahler and Dichev (1997) is the existence of benchmark "meet and beat" behavior. In short, Burgstahler and Dichev, among others, argue that the discontinuity of earnings distributions around certain benchmarks is evidence of financial statement manipulation (Burgstahler and Chuk 2011; Burgstahler and Dichev 1997; Degeorge et al. 1999). While there is considerable debate over whether discontinuities around zero profit and negative growth benchmarks are evidence of earnings management (Durtschi and Easton 2005, 2009 versus Burgstahler and Chuk 2011), Dechow et al. (2010) conclude that the evidence regarding earnings management around analyst forecasts is more convincing. A recent paper by Harris et al. (2011) also finds that meet/beat behavior is more prevalent among restating firms relative to a control sample and can be an early indicator of accounting fraud.

In accordance with criteria (i), there is no obvious reason to predict that clawbacks will increase meet/beat frequency. Meet/beat behavior can be altered immediately following clawback adoption, satisfying criteria (ii). One limitation is that meet/beat behavior is a result of intentional earnings manipulation and will not capture any improvements in financial reporting quality due to a reduction of unintentional accounting errors. Another potential limitation of using meet/beat is that small changes in EPS are easier to make than large changes, implying that meet or beat behavior is likely only available to firms that are otherwise quite close to the consensus forecast. Despite its drawbacks, we expect meet/beat frequency to be a reasonable proxy for intentional earnings management and include it as one of our two measures. We use a binary variable, *BEAT*, to identify firms with a $0.00 or $0.01 deviation from the median of analysts' consensus EPS forecast.[5]

3.1.2. Unexplained audit fees

Hribar et al. (2011) develop a new financial reporting quality proxy based on unexplained audit fees, where larger unexplained audit fees are indicative of lower quality financial statements and higher risk of misstatement. The underlying theory is that these higher fees are driven in part by the increased effort required to audit low quality accounting, and by the higher risk premium required to cover the potential

[5] Our results are robust to scaling the deviation by price. However, we argue that unscaled BEAT is most appropriate as Cheong and Thomas (2010) find that deviations from consensus do not vary by price.

costs (e.g., litigation costs, regulatory penalties, reputation loss, etc.) associated with auditing a risky client. Hribar et al. (2011) calculate unexplained audit fees as the residual from regressions of fees on known determinants. The authors find that their audit fee measure helps predict fraud, restatements, and SEC comment letters. The measure is also correlated with Dechow and Dichev's (2002) measure of accruals quality.

Unexplained audit fees (*UAF*) satisfies both of our criteria for a financial reporting quality proxy; effective clawbacks should unambiguously reduce audit fees, and audit fees are typically negotiated annually and thus can respond quickly to changes in financial reporting quality. An additional advantage of *UAF* over *BEAT* is that lower audit fees can capture reductions in both intentional and unintentional misstatements. We calculate *UAF* using regressions by year and size decile of logged audit fees on firm and auditor characteristics that are known to determine fees. The residuals represent a firm-year measure of unexplained audit fees (see Appendix 2 for additional details).

3.1.3. Discussion of unsuitable proxies

Restatements: We do not consider restatements to be a suitable proxy for financial reporting quality because restatements fail to satisfy our proxy selection criteria. Regarding our first criteria, a decrease in restatements after clawback adoption could be indicative of either firms producing higher quality financial statements, or executives simply being less willing to file amended financial statements post adoption because they have more to lose after clawback adoption. The first outcome would be consistent with an improvement in financial statement quality. The latter outcome will actually reflect an overall decrease in the quality of financial statements. Srinivasan et al. (2011) also discuss the offsetting effects of accounting errors versus manager incentives on restatement frequency, and find evidence consistent with lower restatement frequency being indicative of lower quality financial reporting in countries with weak legal institutions. Thus, we believe it is not possible to draw unambiguous inference about changes in financial reporting quality from observing a decline in restatement frequency after clawback adoption.

Regarding our second criteria, restatements often occur several years after the incorrect financial statements are originally published, which means that several years of post-implementation data are necessary to gain a full understanding of the effect of clawbacks on restatement rates.[6] The implications of this lag are immediately apparent in our data; a total of 218 firm-years have restated financial statements among control and clawback firms in the pre-adoption period (representing a 9.4% restatement rate), but just 20 firm-years are restated in the post-adoption period (representing a 3.1% restatement rate). This reduction is unlikely to be a result of a drastic decrease in financial statement errors, but rather to the fact that restatements relating to the post-adoption period have not yet been filed. This data issue would not necessarily be a concern if it equally impacted clawback adopters and control firms. However, as noted above, clawback adoption likely decreases an executive's incentive to voluntarily report an error in the financial statements, which could exasperate the lag between the time of the violation and the restatement for clawback firms relative to control firms.

A concurrent working paper by Chan et al. (2011) use restatement frequency as their sole proxy for financial reporting quality. The authors find a significant decline in restatements following clawback adoption relative to control firms. Given the concerns noted above, we do not believe this finding can be unequivocally interpreted as an improvement in financial reporting quality. Though we do not include restatements as one of our primary proxies for financial reporting quality, we report results for restatements to allow for comparisons between our paper and Chan et al. (2011). Our metric, *RESTATE,* is a binary variable equal to one if the firm-year financial statements have been amended from the original filing, as identified in the Audit Analytics database.

Abnormal Accruals: One of the most common proxies used in the literature to represent intentional accounting manipulation is the extent to which a treatment firm's discretionary accruals differ

[6] In a large sample of SEC enforcement actions from 1990 through mid-2006 used in Karpoff et al. (2008a, 2008b) and kindly provided by Gerald Martin, the average lag between the mid-point of the accounting violation period and the date of the related accounting restatement is 517 days. The average lag between the start of the accounting violation and the restatement date is 990 days.

from those of a control group of firms. A difficulty with using abnormal accruals as a proxy for measuring changes in financial reporting quality around clawback adoption is that we have no *apriori* expectation about whether clawback adopters (and their matched control firms) were managing accruals upwards or downwards relative to the universe of firms in the pre-adoption period, so it is impossible to make an unambiguous directional prediction about changes in signed abnormal accruals after adoption (violating criteria (i). Using unsigned abnormal accruals avoids this issue but introduces an additional set of econometric concerns (see Hribar and Nichols (2007). Further, it is possible that clawback adoption is followed by an "unwinding" of accumulated accruals from prior period manipulation, in which case the clawback could drive an increase in absolute abnormal accruals in the years immediately following implementation (again violating criteria (i) from above). As such, abnormal accruals is not a suitable proxy for identifying post-clawback changes in financial reporting quality.

Other unsuitable proxies: Finally, we consider and reject several other accounting quality proxies. First, we do not examine a time series measure of accruals quality (AQ), such as that introduced by Dechow and Dichev (2002), because such AQ measures are typically based on five years of data and therefore will likely not capture changes in accounting properties over a one or two-year period. Second, we do not examine earnings properties such as persistence and smoothness as noted in Dechow et al. (2010) because these measures are intended to capture the earnings relevance and decision-usefulness of information, rather than the extent to which financial statements comply with GAAP.

3.2. Proxies for perceptions of financial reporting quality

Hypothesis 2 predicts that market participants' perceptions of financial statement quality will improve following clawback adoption. In this section we motivate the two empirical proxies we use to evaluate Hypothesis 2: earnings response coefficients and analyst forecast dispersion.

3.2.1. Earnings Response Coefficients

Holthausen and Verrecchia (1988) discuss the theoretical relation between accounting signal credibility and price changes. In short, the authors present a model where the market reaction is inversely related to the perceived precision of newly released accounting data. Following this logic, numerous authors have used earnings response coefficient (ERC) magnitude as a proxy for market participants' beliefs about financial statement quality. For instance, Francis et al. (2005) and Wang (2006) find higher ERCs among firms with better governance, and Teoh and Wong (1993) and Francis and Ke (2006) find an association between ERCs and auditor quality. Similarly, Wilson (2008) finds that ERCs decrease substantially following an accounting restatement but predictably recover over the subsequent two years. These among several other papers summarized by Dechow et al. (2010) interpret the higher ERCs as evidence that investors believe a firm's financial statements contain less error.

A limitation of using ERCs is that they are known to be associated with earnings persistence, growth, risk, macroeconomic conditions, and other factors (see Kothari (2001) for discussion). As a result, we follow Wilson (2008) and use a number of variables to control for these determinants in order to isolate the portion of the ERC due to perceptions about earnings quality. We discuss these controls in the next section. In addition, we use a difference-in-differences design that controls for time invariant ERC determinants.

3.2.2. Analyst Forecast Dispersion

Lang and Lundholm (1996) argue that belief dispersion between analysts is increasing in the extent to which analysts use private information in the forecasting process. If clawbacks improve market participants' beliefs about financial statement quality, we expect that analysts will make greater use of this common information source in their analysis. In turn, we expect that analyst forecast dispersion decreases following clawback adoption. Similar to Lang and Lundholm (1996), our measure of forecast dispersion, *DISP*, is the standard deviation of quarterly analyst forecasts scaled by end-of-quarter price. The standard deviation of analyst forecasts is as reported by I/B/E/S (variable *STDEV*).

3.3. Proxies for pay-for-performance sensitivity

We construct all of our CEO compensation variables using annual data available from the Execucomp database. Similar to Core (2002), our pay-for-performance models regresses change in the log of cash compensation (*Δlog(CASHPAY)*) on change in return on assets (*ΔROA*) and abnormal returns (*RET*). As recent literature has demonstrated an asymmetric relation between CEO compensation and positive versus negative performance (Garvey and Milbourn 2006), we further disaggregate *ΔROA* into positive and negative changes (ΔROA^+ and ΔROA^-, respectively). *CASHPAY* is defined as the sum of salary and bonus per Execucomp. *RET* is the firm's annual return less the value-weighted market return.

3.4. Compensation levels variables

We examine three individual components of executive compensation: log of base salary (*SALARY*); log of non-equity incentive compensation (*BONUS*); and log of equity incentive compensation in the form of stock option grants (*GRANTS).* We also examine the log of the sum of base salary, bonus, option grants, and all other compensation (*TOTALCOMP*). See Appendix 1 for variable specifications.

4. Empirical Approach, Sample, and Propensity-Score Matching

We use the same empirical approach to address all four of your hypotheses: we match each clawback firm to a non-adopting control firm using propensity-score matching, and then perform difference-in-differences analysis to assess pre/post-adoption changes in our variables of interest. We next discuss the strengths of our empirical approach, describe our sample, and present the results of the propensity-score matching.

4.1. Empirical approach

The difference-in-differences design effectively controls for time-invariant differences between treatment and control firms, as well as for temporal trends that are common to both groups. Though a powerful approach for eliminating many correlated omitted variables, the internal validity of the difference-in-differences design depends heavily on the "parallel trends" assumption; i.e., that absent the clawback, the changes in the dependent variable would have been the same for both the adopter and non-adopter firms (Roberts and Whited 2011). Violations of the parallel trends assumption typically arise for one of two reasons. The first is that, without the treatment or any other changes, the trends in the dependent variables would not have been parallel through the sample period. We refer to this as the "dissimilar control firms" problem. The second is that the treatment is correlated with an unobserved event that is driving the observed change in the dependent variable; i.e. the "correlated omitted events" problem.

We employ four additional measures to alleviate these concerns. The first is that we identify the control firms via propensity score matching rather than using the entire sample of non-clawback firms. Propensity score matching reduces concerns about dissimilar control firms. It also helps ensure that the observable characteristics that affect both the probability of clawback implementation and the probability of correlated omitted events are controlled in our pre/post-analysis (see Lemon and Roberts 2010) for further discussion). Second, we further reduce concerns over dissimilar control firms by repeating the difference-in-differences analysis for just 2005 through 2007 to ensure that the trends in the dependent variables were similar prior to clawback adoption (see Bertrand and Mullainathan (2003) for an example of this technique and Roberts and Whited (2011) for further discussion). The combination of these two techniques gives us reasonable assurance that we have identified a suitable group of control firms.

Third, we include a number of time-varying control variables in our main regressions to further reduce the risk that changes in observable variables caused by correlated omitted events influence our findings. Though the variables included in the propensity-score model control for time-invariant characteristics, including these additional controls in the main regressions controls for characteristics that do change over time. Finally, we perform supplementary analysis to reduce concerns that clawbacks are implemented as part of a suite of governance changes, the collection of which drives our findings. Though it is impossible to completely eliminate the risk of endogeneity bias in financial research, the combination of a difference-in-differences design with propensity score matching, control variables, pre-treatment trends assessment, and supplementary investigation of other governance changes provides us reasonable assurance that our results are not driven by correlated omitted variables.

4.2. Sample

Our tests employ data from Compustat, CRSP, I/B/E/S, Execucomp, Audit Analytics, and Risk Metrics. The meet/beat, earnings response coefficient, and analyst dispersion tests are conducted using quarterly data from 2005 through 2010, whereas the audit fees and compensation tests use annual data for the same period (these data are only available on an annual basis). Data on voluntary adoption of clawback provisions are from The Corporate Library, which maintains a database of compensation and governance variables that are gathered from firms' annual proxy statements. We use extracts of this database taken in mid-2008, 2009, and 2010 to identify firms that had voluntarily implemented a restatement-triggered clawback provision based on the most recently available proxy at each date (all prior to mandatory adoption as required by the Dodd-Frank Act). We restrict our analysis to only those clawback provisions that are triggered by an accounting restatement as these are the most-prevalent triggering event and are the type of clawback provision required in the Dodd-Frank Act. As proxy statements are typically released a few months following a firm's fiscal year-end, our clawback data coincides with most firms' fiscal years 2007 – 2009.

The Corporate Library's data includes 3,112 firms (see Table 1). Because our empirical analysis is focused on the effects of *voluntarily* adopting a clawback provision, we eliminate 660 financial services firms because firms receiving funds under the Troubled Asset Relief Program (TARP) were required to implement a clawback provision in their executive compensation plans.[7] As described below, we propensity-match clawback adopters with control firms using annual data as of 2006. Table 1 shows that 126 firms lack the requisite Compustat and CRSP data to estimate the propensity-scoring model. 2,326 firms from the Corporate Library's database remain. Of these, 281 have a clawback provision in all years 2007 – 2009 and 1,746 firms do not have a clawback in any year. 108 adopt a clawback provision in 2008 and another 191 adopt a clawback in 2009.

4.3. Propensity-score matching control firms

[7] Examining the effects of mandatory clawback adoption by financial firms subject to TARP versus voluntary clawback adopters would be interesting. However, a lack of control firms that are similar to the TARP adopters precludes us from making effective difference-in-difference assessments. We leave any investigation of mandatory TARP adopters for future research.

The difference-in-differences design relies on the assumption that the treatment and control firms would have had similar trends in our dependent variables had the treatment firm not adopted a clawback provision. One approach in difference-in-differences analysis is to simply use all non-treatment firms as the control group. However, selecting control firms that closely resemble the treatment firms reduces the risk of violating the parallel trends assumption. Propensity score matching allows us to identify the control firms over a range of observable firm and executive characteristics.[8]

We match using annual data as of 2006, prior to the "adopter" firms implementing a clawback provision. Matching prior to clawback adoption is essential so as to avoid simultaneity between the clawback provision and determinants of clawback adoption that are affected by the presence of a clawback provision.[9] The matching model is a logit with a binary dependent variable equal to one for firms that have adopted a clawback by the end of our sample period (*CLAW*). All 2,326 firms with available data are included in this regression.[10] Conditional probabilities of clawback adoption, i.e., "propensity scores," are calculated as the fitted values from the logit model. These propensity scores are used to match each clawback adopter in the financial reporting sample to its closest non-adopting counterpart, with replacement.[11] The same propensity scores are used to identify control firms for all test samples.

---

[8] Propensity score matching is used in a variety of empirical approaches. One common approach is to propensity match a treatment to a control firm and perform analysis of paired-differences on variables of interest. We use propensity score matching in a different context: to identify control firms to be used in difference-in-differences analysis. Our difference-in-differences approach has the advantage of easily incorporating multiple pre- and post-treatment firm-quarters or years.

[9] For example, we predict that governance-improving clawbacks reduce earnings management. Concerns about earnings management may also influence the board's decision to implement a clawback. Conducting propensity matching after the clawback is in place introduces simultaneity between the earnings management proxy and dependent clawback indicator variable. We conduct the match in 2006 to avoid this source of endogeneity.

[10] As noted earlier, 281 firms on the Corporate Library database have clawbacks as of 2007. As very few firms had a clawback provision prior to 2006, the majority of these plans were likely adopted during 2006 or 2007 (Equilar 2010). We include these observations in our propensity score model to increase the size of our treatment group in this model. Excluding these observations results in only 15% of our the firms having clawbacks, the logit model has trouble converging, the r-square is low, and we get poor propensity score matches compared to when we include these observations in the matching model. Note that these 281 firms are NOT included in our hypothesis tests because we cannot definitively assign an adoption year/quarter for the AFTER test variable.

[11] Roberts and Whited (2011) note that matching with replacement increases the similarity of the control firms and can be preferable to matching without replacement. In an empirical accounting research setting, Armstrong et al. (2010) note that the choice of matching with our without replacement depends on the similarity of matches that can be

Though an in-depth analysis of the determinants of clawback implementation is not the objective of this paper, propensity score matching requires that we identify characteristics that are associated with clawback adoption. We start with several measures of firm condition and performance that have been used in previous compensation-related literature (see Armstrong et al. (2010) and Core et al. (1999), among others): three-year averages of leverage (*AVGLEV*), log of market value of equity (*AVGLNMVE*), market-to-book (*AVGMTB*), return on assets (*AVGROA*), and log of total revenue (*AVGLNREV*). Standard deviation of the previous two years' returns (*SDRET*) is included as a measure of risk. A binary variable for the existence of a restatement in the last two years (*RESTATE_PRE*) is included as it is logical that restatement-triggered clawback provisions might be introduced in response to an actual misstatement. We include binary variables for each of the Fama and French 48 industry classification codes in order to capture industry differences.

If boards tend to adopt clawbacks for governance-improving purposes and CEOs tend to resist clawback adoption, the probability of implementation is likely decreasing with the CEO's ability to influence the board members. Indeed, current working papers by Brown et al. (2011) and Addy et al. (2011) find evidence consistent with clawback implementation decreasing with CEO entrenchment. Bebchuk et al. (2010) find that the ratio of the CEO's compensation to the aggregate compensation of the top-five executive team (*PAYSLICE*) is an effective proxy for the CEO's ability to influence compensation and firm outcomes. We include this measure as well as the log of CEO tenure (*LNTENURE*) and the CEO's ownership percentage of the firm (*OWNPERC*) as proxies for the CEO's ability to resist an unwanted clawback provision.

For completeness, we include pre-treatment values of several of the dependent variables examined in our hypothesis testing. *AVGBEAT* and *AVGDISP* are the two-year averages of *BEAT* and *DISP*, as

---

obtained without replacement. Approximately 20% of the control firms are repeated in our samples. Our primary results using a matched sample without replacement are similar, although our sensitivity analysis to pre-treatment parallel trends indicates, as one would expect, that the "with replacement" samples more closely resemble the treatment samples. Clustering standard errors by firm in tests of H1 through H4 mitigates the bias introduced by repeated control firms.

previously defined. *AVGERC* is the two-year average ERC coefficient from quarterly regressions of three-day abnormal returns (*CAR*) on unexpected earnings (*UE*). *TOTALCOMP* is the CEO's total compensation as of 2006. We also include two-year average of bid-ask spread, *AVGSPREAD*, as an additional measure of uncertainty and belief dispersion between market participants.

A total of 2,326 firms have the requisite accounting and returns data described above. Of these, 59% (45%) have information on CEO turnover (CEO pay and ownership). To prevent significant loss of data, we create binary variables for firms with non-missing CEO data (*HAVETURNOVER*, *HAVEPAY*, and *HAVEOWN*) and set the missing data for the remaining observations equal to zero.[12] We follow the same process to identify the roughly 10% of observations that are missing information on average *DISP*, *BEAT*, and *ERC* (*HAVEDISP*, *HAVEBEAT*, and *HAVEERC*). The complete model is as follows:

$$CLAW = \alpha + \beta_1 AVGLEV + \beta_2 AVGLNMVE + \beta_3 AVGMTB + \beta_4 AVGROA + \beta_5 AVGLNREV + \beta_6 LNTENURE + \beta_7 HAVETENURE + \beta_8 PAYSLICE + \beta_9 HAVEPAY + \beta_{10} OWNPERC + \beta_{11} HAVEOWN + \beta_{12} SDRET + \beta_{13} RESTATE + \beta_{14} TURNOVER + \beta_{15} HAVETURNOVER + \beta_{16} AVGSPREAD + \beta_{17} AVGDISP + \beta_{18} HAVEDISP + \beta_{19} AVGBEAT + \beta_{20} HAVEBEAT + \beta_{21} AVGERC + \beta_{22} HAVEERC + \beta_{23} TOTALCOMP + \Sigma\beta_k FFIND48 + \varepsilon \quad \textbf{(1)}$$

All variables are as discussed above and are defined in Appendix 1. We make no predictions about the coefficient signs as evaluating clawback determinants is beyond the scope of this paper. Results are tabulated in Table 2. The pseudo and rescaled R-Squared statistics are 0.229 and 0.339, respectively, which is on par with the propensity model fit in other compensation studies (e.g., Armstrong et al. (2010) have a pseudo R-Squared of 0.273). The likelihood of clawback adoption is increasing with leverage, market value of equity, and revenue. The findings for the proxies relating to CEO power are mixed. The likelihood of clawback adoption is decreasing with tenure, which would be consistent with a more entrenched manager resisting a governance-improving clawback. However, the probability of clawback

[12] An alternate approach is to set missing data to the sample averages and then include HAVETURNOVER, HAVEPAY, HAVEOWN, and the other HAVE binary variables along with interactions between each binary and the main variable. Results using this alternate approach are essentially unchanged.

adoption is increasing with CEO pay slice. The probability of clawback adoption is significantly decreasing with average ERC. Coefficients on *AVGBEAT, AVGDISP*, and *TOTALCOMP* are insignificantly different from zero.

The data requirements differ substantially between tests, so we create three separate samples for analyzing quarterly financial statement data (used to assess *BEAT, ERC,* and *DISP*), annual financial statement data (used to assess *UAF*), and annual compensation data (for all compensation analysis). Clawback adopters must have at least one valid observation before and after their clawback adoption year so as to enable effective pre/post-adoption analysis. For consistency, we require that the non-adopting firms have at least one valid observation in or before 2007 and one observation in or after 2009. Table 1 details our sample refinement. Of the 2,326 firms used in the propensity score matching, 408 firms lack the requisite Compustat, CRSP and IBES data to complete the quarterly financial reporting tests described below. 280 of the remaining 1,918 firms adopt a clawback during our sample period. Thus, our quarterly financial reporting tests will assess these 280 adopter firms in comparison to a propensity-matched sample of 280 control firms that do not have a clawback through the sample period. The 263 firms with clawbacks already in place in 2007 are excluded from the hypothesis testing analysis.

Turning to the annual financial reporting sample, from the base 2,326 firms in the propensity model we eliminate 396 firms for which insufficient Compustat, CRSP, and IBES data are available. We are left with 258 clawback firms and an available pool of 1,422 control firms. In the compensation analysis sample, we eliminate 957 firms from the base 2,326 firms in the propensity model that do not have requisite Execucomp, Compustat and CRSP data for the tests described below (the primary limiting factor is Execucomp). The final sample for the compensation tests consists of 1,369 firms, of which 228 adopt a clawback during the sample period.

The difference-in-differences analysis requires that we use a binary variable, *AFTER*, to identify periods after clawback adoption in which we expect the effect of the clawback provision exist.[13] We assume that the "after" date for all tests starts with the fiscal quarter (for the financial reporting tests) and fiscal year (for the compensation tests) following its announcement in a proxy statement. For instance, a firm's proxy statement for fiscal 2008 will be released during early 2009, so we assume that a newly announced clawback policy is effective and known by the market for 2009 onward. Errors in identifying the pre- versus post-adoption periods erroneously assigns data from one period to the other, which add noise to our sample and biases against identifying changes before/after clawback adoption.

Panel A of Table 3 presents summary information for the 280 firms in the quarterly financial reporting sample along with their matched controls. The average propensity score for both adopters and controls is 0.384. *T*-tests of differences in means indicate that none of the variables used in the propensity score model are significantly different between the matched adopter and control firms. Panel B presents similar information for the annual financial reporting sample; just two of the propensity score variables differ at 10% between the clawback adopters and controls. Panel C presents similar information for the annual compensation sample; several of the propensity score determinants differ between the control and adopter firms at 5%. One of the advantages of propensity score matching is that it does not require that the matched firms are identical across all determinant variables. As such, these differences are not a concern for the validity of the remaining analysis. Finally, Panel D details the distribution of difference in propensity scores between each adopter and its matched control firm. The minimum (maximum) deviation is -0.011 (0.024), indicating that we were able to identify high-quality matches.

Panel A of Table 4 shows that within the quarterly financial reporting sample there are a total of 6,224 and 6,199 firm-quarter observations for the clawback and control samples, respectively. Panels B

[13] This *AFTER* variable of the difference-in-differences design is excluded in the models in Chan et al. (2011). Thus, the results in Chan et al. (2011) effectively compare a change in the dependent variable among clawback firms to the average of the dependent variable for the control firms over the sample period. Macroeconomic trends common to both clawback and control firms (such as the decline in restatement frequency observed in our data) are not eliminated if the *AFTER* variable is excluded from the difference-in-differences design.

and C of Table 4 show that the annual financial reporting and compensation samples are also evenly balanced between control and clawback adopter firm-year observations.

5. Hypotheses Results

5.1. Hypothesis 1: Financial reporting quality

As previously described, we use two proxies to capture changes in post-clawback financial reporting quality. We also include an analysis of restatements for comparison with Chan et al. (2011). We employ the same general model for each, but with different control variables and logit versus ordinary least squares specification as appropriate:

$$Quality\ Proxy = \alpha_1 + \beta_1 AFTER + \beta_2 CLAW + \beta_3 (CLAW*AFTER) + \Sigma\beta_k CNTRLS + \Sigma\beta_k (CNTRLS*AFTER) + \Sigma\beta_k (CNTRLS*CLAW) + \Sigma\beta_k (CNTRLS*CLAW*AFTER) + \Sigma\beta_k (QTR\ or\ YEAR) + \varepsilon \quad \textbf{(2)}$$

AFTER is a binary variable equal to one for the period following clawback adoption. For control firms, *AFTER* is equal to one for the same periods as *AFTER* for the matched clawback firm. *CLAW* is a binary variable equal to one for clawback adopter firms. The coefficient of interest, β_3, can be interpreted as the difference-in-differences in the dependent variable following clawback adoption between the adopter and control firms.[14] Hypothesis 1 predicts $\beta_3 < 0$ for our two proxies.

CNTRLS vary depending on the dependent variable. Though not required in a difference-in-differences model, for completeness we include interactions between each control and *AFTER, CLAW,* and *CLAW*AFTER.* Calendar quarter or year fixed effects, *QTR* or *YEAR,* are an additional control for macroeconomic effects and reduce concerns over cross-sectionally correlated residuals. We cluster standard errors by firm to mitigate bias from repeated control firms and serial correlation. All significance

[14] Two of our regressions are based on logit models rather than ordinary least squares (OLS). Ai and Norton (2003) show that interaction coefficients in logit models do not always have the same sign or significance as the cross-partial derivative with respect to the probability of the dependent variable. The authors also present an alternative measure of the magnitude and standard error of interaction effects in nonlinear models. Greene (2010) contends that the Ai and Norton (2003) modified statistics do not provide meaningful interpretations. On the other hand, Kolasinski and Siegel (2011) use statistics literature (e.g., Le 1998) to show that the standard logit interaction coefficient is appropriate for practical research of non-extreme probabilities. Still, we compute the Ai and Norton (2003) interaction effects as a robustness test below.

levels discussed below are based on one-tailed tests where a directional prediction is provided, or two-tailed tests otherwise.

5.1.1. Meet/beat analysis

A binary variable, *BEAT*, is set equal to 1 for firms that have \$0.00 or \$0.01 deviation from analyst EPS consensus forecast each quarter.[15] Of the total 12,423 firm-quarter observations, 11,226 (90%) are within +/- \$0.20 of the analyst forecast. A histogram of the deviations within this range is provided in Figure 1. 3.7% of the observations have a deviation of -\$0.02, 5.6 percent have -\$0.01, 10.9 percent have \$0.00, 11.3 percent have \$0.01, and 9.9 percent have \$0.02. Thus, there is some indication of a discontinuity around \$0.00. Panel A of Table 5 shows that meet/beat frequency declines from 13.6% to 11.5% among control firms between the pre- and post-adoption periods. This coincides with a decrease from 12.6% to 9.2% among the clawback adopters.

Similar to Matsumoto (2002) we use a logit specification of model (2) to evaluate changes in meet/beat frequency around clawback adoption. *CNTRLS* include the log of market value (*SIZE*), market-to-book (*MTB*), an indicator variable for fiscal quarter four (*Q4*), and the number of analyst forecasts included in the consensus figure (*NUMEST).*[16]

We present results in the first column of panel B of Table 5. β_1 on *AFTER* is insignificant at conventional levels, which is consistent with no change in the probability of *BEAT* among control firms over the sample period. β_2 is insignificantly different from zero, which is consistent with there being no difference in meet-or-beat behavior in the pre-adoption period between clawback and control firms. β_3 is significantly negative, as predicted by Hypothesis 1 ($p = 0.018$, one-tailed).[17] The (untabulated) marginal

[15] The results are robust to scaling deviation from consensus forecast by price and defining *BEAT* as the scaled forecast error between 0 and 0.001.

[16] It is possible that BEAT is also a function of forecast dispersion. However, our prediction is that both BEAT and dispersion are influenced by clawback implementation. Including dispersion as a control variable could, therefore, have the effect of "throwing out the baby with the bathwater."

[17] We compute the Ai and Norton (2003) interaction effects as a robustness test: the marginal effects are negative across the range of probabilities, with z-statistics from -0.998 to -2.701.

effect of β_3 evaluated at regressor means indicates that there is a 12.8 point reduction in the probability of meet/beat for clawback adopters relative to control firms. The coefficients on the control variables are omitted for brevity. Untabulated results indicate a negative relation between *SIZE* and *BEAT* ($p = 0.01$, two-tailed), positive relation for *MTB* ($p < 0.000$, two-tailed), positive relation for *NUMEST* ($p < 0.000$, two-tailed), and an insignificant relation for *FQ4* (p = 0. 905, two-tailed).

5.1.2. Unexplained audit fees analysis

Relative to the broader population, panel A of Table 5 shows that both the control and clawback firms have positive unexplained audit fees (*UAF*) in both the pre and post periods. *UAF* for control firms increases from 0.061 to 0.084 during the sample period, while *UAF* for clawback adopters decreases from 0.103 to 0.062. We evaluate the significance of these changes using an ordinary least squares specification of model (2). Additional control variables are unnecessary as the dependent variable is the residual from a first-stage model of audit fees on known covariates. As shown in the second column of panel B of Table 5, the significantly negative coefficient on β_3 is consistent with the prediction that there is a significant decline in *UAF* for clawback adopters relative to control firms.

5.1.3. Restatements analysis

As discussed earlier, we do not consider restatements to be a suitable proxy for financial reporting quality in this setting. We provide results to allow for comparisons between our paper and Chan et al. (2011). Panel A of Table 5 shows that there is an approximately 65% decrease in the prevalence of restated financial statements for both clawback adopters and control firms between the pre and post periods. As discussed in section 3.1, this decrease is likely due to the fact that restatements take several years to occur and our *AFTER* period includes predominantly 2009 and 2010 data. *CNTRLS* in our logit model include *MTB*, *SIZE*, an indicator for negative income (*LOSS*), the square roots of the firm's number of business and geographic segments (*BUSSEG_SR* and *GEOSEG_SR*), leverage (*LEV*), return on assets (*ROA*), a binary variable for having one of the four largest auditors (*BIG4*), and indicator variable for anything other than an unqualified audit opinion (*AUD_OPIN*), an indicator variables if there was a stock or debt issuance during the year (*SEO* and *ISSUANCE*), and an indicator variable for the existence of a restatement in the trailing two years (*RESTATE_PRE*). See Appendix 1 for further detail on the sources and specifications of the control variables.

Results are presented in the third column of panel B of Table 5. The β_3 coefficient on *CLAW*AFTER* is negative and highly significant.[18] Evaluated at the means, the β_3 coefficient indicates that there is a 16.1 point reduction in the probability of restatement among the clawback adopters relative to the control firms between the pre and post periods. Note also that despite observing a sizable reduction in restatement frequency based on the summary statistics in Panel A, the regression β_1 coefficient is significantly positive. This result is primarily due to the inclusion of year fixed effects. Untabulated control variables indicate that the likelihood of restatement is significantly increasing with *MTB* (p = 0.016), *LEV* (p = 0.088), and *RESTATE_PRE* (p = 0.003), and that restatements are decreasing with *SIZE* (p = 0.002).

5.2. Hypothesis 2: Perceived financial reporting quality

5.2.1. Earnings Response Coefficient

Our ERC analysis is based on the following difference-in-differences regression:

$$CAR = \alpha_1 + \alpha_2 AFTER + \alpha_3 CLAW + \alpha_4(CLAW*AFTER) + \beta_1 UE + \beta_2(AFTER*UE) + \beta_3(CLAW*UE) + \beta_4(CLAW*AFTER*UE) + \Sigma\beta_k CNTRLS + \Sigma\beta_k(CNTRLS*UE) + \Sigma\beta_k(CNTRLS*AFTER) + \Sigma\beta_k(CNTRLS*CLAW) + \Sigma\beta_k(CNTRLS*CLAW*AFTER) + \Sigma\beta_k(CNTRLS*AFTER*UE) + \Sigma\beta_k(CNTRLS*CLAW*UE) + \Sigma\beta_k(CNTRLS*CLAW*AFTER*UE) + \Sigma\beta_k QTR + \varepsilon \quad \textbf{(3)}$$

CAR is the three-day market-adjusted return around the quarterly earnings announcement. *UE* is the deviation from analyst consensus forecast scaled by end-of-quarter price. Forecasts greater than 90 days old are eliminated. H2 predicts that ERCs among clawback-adopters increase relative to control firms; i.e., $\beta_4 > 0$.

[18] Ai and Norton (2003) marginal effects for *CLAW*AFTER* are negative across the range of probabilities, with z-statistics from -0.414 to -7.581.

CNTRLS follow Wilson (2008) and are intended to control for the determinants of ERC magnitude that are unrelated to investors' perceptions of financial statement precision. The controls we include are *PREDICT* for earnings predictability, *PERSIST* for earnings persistence, *MTB* for growth, *BETA* as a measure of risk, *LOSS* an indicator variable for loss quarters, *SIZE, Q4* an indicator variable for fourth fiscal quarter, and *NONLINEAR* for nonlinearity in the returns earning relation. Each variable is interacted with UE to control for its effect on the ERC. We also include calendar quarter fixed effects, and cluster standard errors by firm.

Panel B of Table 6 reports the main regression coefficients of interest. β_1 is 5.894 and highly significant ($p < 0.000$).[19] β_2 is negative and insignificant ($p = 0.49$, two-tailed), which is consistent with no change in ERCs among the control firms over the sample period. β_3 is negative but insignificant ($p = 0.18$), indicating that clawback adopters have the same average ERCs as the control firms prior to adoption. β_4, the difference-in-differences coefficient, is 6.381 and significant ($p = 0.023$). Untabulated results show that the signs on the control variables interacted with *UE* are largely of the expected signs. Few of the controls interacted with *CLAW* or *AFTER* are significant at 10 percent.

Panel C reports the aggregate ERC for the treatment and matched firms pre and post clawback adoption. Strictly speaking, the ERC for the matched firms in the pre-period is for Q1 of 2005 as quarterly fixed effects are included in the model. Control firms experience a statistically insignificant decrease in ERC from 5.89 to 4.52 over the sample period, while adopter firms experience an increase from 4.23 to

[19] Kothari (2001) notes that, assuming earnings follow a random walk and a discount rate of 10%, the expected magnitude of annual ERCs is roughly 11 (calculated as 1 + 1/r). Kothari also notes that observed ERCs range from 1 – 3. It is important to recognize that Kothari is specifically referring to annual ERCs using annual earnings surprises and discount rates. One must annualize quarterly unexpected earnings in order to calculate the expected magnitude of quarterly ERCs. Using Kothari's assumptions, the expected quarterly ERC is (1 + (1*4)/.10) = 41. Alternatively, our observed ERC of 5.90 annualizes to 1.48, which is consistent with prior studies of annual ERCs. Using an identical model to our own, Wilson (2008) calculates quarterly ERCs of 5 – 8 among restating firms, which likely have lower ERCs than the general population. Thus, our ERCs of up to 9.24 are not unreasonable.

9.24. In sum, the evidence is consistent with investors perceiving financial statements as being higher quality following clawback adoption, as predicted by Hypothesis 2.[20]

5.2.2. Analyst Forecast Dispersion

We use the following OLS regression to test for differences in analyst forecast dispersion following clawback adoption:

$$DISP = \alpha_1 + \beta_1 AFTER + \beta_2 CLAW + \beta_3(CLAW*AFTER) + \Sigma\beta_k CNTRLS + \Sigma\beta_k(CNTRLS*AFTER) + \Sigma\beta_k(CNTRLS*CLAW) + \Sigma\beta_k(CNTRLS*CLAW*AFTER) + \Sigma\beta_k QTR + \varepsilon \qquad \textbf{(4)}$$

DISP is the standard deviation of analyst forecasts. Firms must have at least two analysts in order to calculate *DISP*. For consistency, we use the same controls as in the ERC model except that *NONLINEAR* is now irrelevant and we add an additional control for the number of analyst forecasts included in the quarterly consensus (*NUMEST*). We cluster standard errors by firm. Quarterly fixed effects should adequately remove cross-sectional correlation. H3 predicts that $\beta_3 < 0$.

The results of model (4) are presented in Panel B of Table 6. The intercept, α_1, is 0.005 and highly significant. β_2 is insignificant, consistent with no difference in *DISP* between clawback adopters and control firms in the pre-period. β_1 is significantly positive (p = 0.033), which is consistent with the non-clawback firms experiencing an increase in *DISP* over the sample period. As predicted, β_3 is significantly negative (p = 0.025, one-tailed), consistent with the clawback firms experiencing a relative decrease in *DISP* as compared to the non-adopters. Consistent with H2, results indicate that analysts' perceptions of financial reporting quality improve after clawback adoption.

5.3. Hypothesis 3: Pay for performance analysis

[20] There is a concern that the increase in ERCs is partially attributable to the decline in meet/beat behavior. There is some evidence that just missing an analyst target has an asymmetrically negative reaction than the positive reaction to just beating a target (Skinner and Sloan 2002). As such, larger ERCs after clawback adoption could be driven by a higher frequency of near-misses due to a reduction in meet/beat behavior. Untabulated analysis confirms that our results are robust to excluding firms with negative earnings surprises up to two cents.

H3 predicts that the sensitivity of CEO compensation to accounting performance increases following clawback adoption. Similar to Core (2002), we implement our pay-for-performance analysis using the following regression.

$$\Delta log(CASHPAY) = \alpha_1 + \alpha_2 AFTER + \alpha_3 CLAW + \alpha_4 (CLAW*AFTER) + \beta_1 \Delta ROA^+ + \beta_2 (\Delta ROA^+ *CLAW) + \beta_3 (\Delta ROA^+ *AFTER) + \beta_4 (\Delta ROA^+ *CLAW*AFTER) + \beta_5 \Delta ROA^- + \beta_6 (\Delta ROA^- *CLAW) + \beta_7 (\Delta ROA^- *AFTER) + \beta_8 (\Delta ROA^- *CLAW*AFTER) + \Sigma\beta_k CNTRLS + \Sigma\beta_k (CNTRLS*AFTER) + \Sigma\beta_k (CNTRLS*CLAW) + \Sigma\beta_k (CNTRLS*CLAW*AFTER) + \Sigma\beta_k YEAR + \varepsilon \quad \textbf{(5)}$$

Δlog(CASHPAY) is the year-over-year change in the log of total salary and bonus. ΔROA^+ is the change in ROA for changes greater than or equal to zero, zero otherwise. Similarly, ΔROA^- is for negative changes in ROA. *CNTRLS* include annual abnormal return (*RET*) and a binary variable for recent CEO turnover (*TURNOVER*). Standard errors are clustered by firm. We expect both β_1 and β_5 to be significantly positive if cash compensation is, on average, sensitive to changes in ROA. H3 predicts that β_4 and $\beta_8 > 0$. However, given that recent studies have found the sensitivity of compensation to be lower to negative performance than positive (Garvey and Milbourn 2006), we expect any changes in sensitivity to be more concentrated in the positive performance variables.

Results of estimating model (5) are presented in Table 7. As expected, both β_1 and β_5 are significantly greater than zero. In accordance with H3, β_4 is significantly positive, which indicates that pay-for-performance sensitivity to good news increases for clawback adopters relative to control firms over the sample. β_8 is insignificantly different from zero, indicating that there is not a similar effect relating to negative changes in performance.

5.4. Hypothesis 4: Compensation composition changes

Hypothesis 4a predicts that total compensation increases following clawback adoption, Hypothesis 4b predicts that base compensation increases, and Hypothesis 4c predicts that incentive compensation increases. As defined in Section 3.4, the variable we examine in relation to Hypothesis 4a is log of total compensation (*TOTALCOMP*), the variable we examine in relation to Hypothesis 4b is log of salary (*SALARY*), and the variables we examine in relation to Hypothesis 4c are log of bonus (*BONUS*) and log of stock option grants (*GRANTS*). For completeness, we also examine the log of all other compensation (*OTHERCOMP*), which is defined as total compensation less salary, bonus, and option grants.[21] We assess each before and after clawback adoption relative to control firms using the following difference-in-difference regression:

$$COMP = \alpha_1 + \beta_1 AFTER + \beta_2 CLAW + \beta_3 (CLAW*AFTER) + \Sigma\beta_k CNTRLS + \Sigma\beta_k (CNTRLS*AFTER) + \Sigma\beta_k (CNTRLS*CLAW) + \Sigma\beta_k (CNTRLS*CLAW*AFTER) + \Sigma\beta_k YEAR + \varepsilon \quad \textbf{(6)}$$

COMP is one of the five compensation variables discussed above. *YEAR* are yearly fixed effects. *CNTRLS* are the three-year averages of leverage (*AVGLEV*), log of market value of equity (*AVGLNMVE*), market-to-book (*AVGMTB*), return on assets (*AVGROA*), and log of total revenue (*AVGLNREV*). See Appendix 1 for further discussion of the variable sources and specifications. We also include an indicator variable, TURNOVER, that is equal to one if the CEO position has changed within two years. We once again cluster standard errors by firm. H4a – H4c predict $\beta_3 > 0$.

Prior literature has found a wide array of variables to be correlated with compensation, including measures of governance and operational risk (see Bushman and Smith (2001) and Armstrong et al. (2010) for reviews). We opt for a parsimonious model for several reasons. First, the difference-in-differences design controls for any time-invariant compensation determinants. The difference-in-differences design also eliminates the effects of governance or risk trends that are common to both clawback and control firms. Second, the propensity score model includes several measures of risk and governance and, therefore, reduces the risk that temporal trends in governance or risk variables that are correlated with both clawback implementation and compensation are driving our results. Finally, including any variables that are influenced by clawback implementation would introduce simultaneity bias to our equation. For example,

[21] Per Execucomp, this portion of compensation includes the value of restricted stock grants, long-term incentive payouts, retirement contribution, and all other compensation.

including *PAYSLICE* from the propensity score model in the compensation regression is problematic as we predict that clawback implementation will result in higher CEO compensation

Results are presented in panel B of Table 8. As predicted by H4a, the significantly positive coefficient on β_3 for *TOTALCOMP* (column 1) is consistent with an increase in total compensation for clawback adopters relative to control firms over the sample period. The significantly positive coefficient on β_3 for *SALARY* (column 2) indicates an increase in base pay for clawback adopters relative to control firms, which is consistent with H4b. The results provide little support for H4c: there is an insignificant change in *BONUS* or *GRANTS* among clawback adopters (see β_3 in columns 3 and 4). Though we make no specific prediction about changes in *OTHERCOMP*, there is no significant increase in this compensation component over the sample (column 5). In sum, the results indicate that there is an increase in total compensation for clawback adopters relative to controls, and that this increase is largely driven by higher base salary as opposed to incentive compensation.[22]

5.5. Robustness test: pre-clawback parallel trends analysis

Following Bertrand and Mullainathan (2003) and the advice of Roberts and Whited (2011), we repeat the difference-in-differences analysis in the pre-clawback years to ensure that the clawback and control firms had similar trends in the dependent variables prior to clawback implementation. The original difference-in-differences analysis includes years 2005 – 2010 with "after" periods starting as of 2009 or 2010, depending on the clawback implementation date. We repeat the analysis limiting the data for 2005 – 2007 (prior to any clawback adoption) and with "after" periods starting in 2006 or 2007 (defined as the original "after" date minus three years). The difference-in-differences variables should be insignificantly different than zero if the treatment and control firms have similar trends in the dependent variables prior to clawback adoption. Untabulated results confirm that this is the case in all cases, giving us further confidence in the validity of our parallel trends assumption.

[22] A concern is that these compensation changes are exogenous to clawback adoption and are actually responsible for the results observed in H1 through H3. Section 5.7 discusses robustness tests that include compensation as an additional regressor in the models testing the first three hypotheses.

5.6. Robustness test: correlated governance changes

It is possible that our difference-in-differences and propensity score matching techniques have not adequately controlled for clawback provisions being adopted as part of a suite of governance changes, the collection of which are responsible for the observed changes in dependent variables. We reduce this concern by examining whether clawback firms experienced changes in other observable governance characteristics over the sample period. Specifically, we estimate the following difference-in-differences model for all sample firms with available governance data:

$$GOV = \beta_0 + \beta_1 AFTER + \beta_2 CLAW + \beta_3 (CLAW*AFTER) + \varepsilon \qquad \textbf{(7)}$$

GOV is one of 19 governance characteristics we examine. The models are logit regressions for binary *GOV* variables and ordinary least squares otherwise. Heteroskedasticity-robust standard errors are clustered by firm.. $\beta_3 = 0$ is consistent with no difference in *GOV* changes between control and treatment firms over the sample period. We obtain data on 19 governance provisions for all available firm-years from Risk Metrics for 2005 – 2010; see Appendix 1 for variables and definitions. All variables are coded such than an increase is considered governance-reducing, as per Gompers et al. (2003).

We estimate model (7) a total of 19 times each for the financial reporting sample – one for each *GOV* variable (results untabulated). Of the total of 19 regressions, β_3 is negative in 11 and positive in 8. β_3 is significant at a 10% level of confidence in two regressions. At a 10% level, we expect 1.9 of the 19 regressions to produce spuriously significant results. The results are similar for the annual financial reporting and compensation samples. Further, the coefficients are not of consistent signs or significance across the three samples. Thus, there is no evidence of a systematic difference in governance changes between the clawback and control firms, or that the clawback provisions were adopted as part of a suite of other observable governance measures.

5.7. Robustness test: controlling for compensation changes

One concern with our compensation analysis is that compensation is itself a governance mechanism. If the compensation changes we observe are exogenous to clawback adoption, the results in H1 through H3 could be driven by the compensation changes and not the clawback provisions.

As a robustness test, we re-perform all of the analysis for H1 through H3 including the logs of *TOTALCOMP*, *BONUS,* and *SALARY* as additional regressors, along with interactions between these variables and *CLAW*, *AFTER*, and *CLAW*AFTER*. In the ERC and pay-for-performance analysis, the compensation levels are also interacted with *UE* and *ROA*, respectively. As the compensation variables as well as the dependent variables in our tests of H1 through H3 are both predicted to be influenced by clawback adoption, we expect that including *TOTALCOMP*, *BONUS*, and *SALARY* as regressors will bias downwards our test coefficients of interest. However, observing that the test coefficients are significant even after including compensation will reduce concerns that changes in compensation are driving our results.

Untabulated analysis confirms that the results presented in Tables 5 through 7 are quantitatively and qualitatively the same after including *TOTALCOMP*, *BONUS,* and *SALARY.* With one exception, the difference-in-difference estimators are attenuated but remain significant at 10%. The exception is that the decline in *RESTATE* is no longer significant once the compensation variables are included. In sum, the results of H1 through H3 are robust to controlling for changes in compensation.

6. Cross-Sectional Analysis: "Robust" versus "Misconduct" Clawbacks

In this section we assess whether a key cross-sectional characteristic of clawback provisions affects the relations examined in H1 through H4: some clawback provisions require recovery of excess compensation by all executives whether or not there was misconduct, while others only require recovery from the specific executives who committed the misconduct. Consistent with Fried and Shilon (2011), we refer to the former as "robust" clawbacks and the latter as "misconduct" clawbacks. Robust clawbacks should incent managers to reduce both intentional and unintentional reporting errors. Misconduct clawbacks should discourage intentional errors but are less likely to impact unintentional errors. We distinguish between "robust" and "misconduct" clawback provisions using a classification code provided in The Corporate Library's clawbacks dataset. Panel A of Table 9 shows that roughly 39% of each sample of clawback provisions are designated as "robust"

It is unclear whether a robust clawback will have a larger effect than a misconduct clawback on the post-clawback changes predicted by H1 through H4. On one hand, robust clawbacks cover intentional and unintentional restatements, and therefore should have a larger impact on financial reporting quality than a misconduct clawback provision. On the other hand, if financial statement users are primarily concerned about fraud rather than simple error, then it is possible that the robust clawback has little or no marginal effect. Or, it is also possible that the risk of serious but unintentional misstatement is sufficiently low among our sample (i.e., public, U.S. Russell 3000 firms in a post-SOX era) that the value of adopting a clawback is primarily in reducing intentional earnings manipulation. Our approach to evaluating the incremental impact of robust clawbacks is to amend the previous regression models to include a new binary variable, *ROBUST*, that is equal to one if a firm's clawback is robust. We then interact *ROBUST* with *AFTER*, the coefficient that represents the incremental impact of *ROBUST* on change in the dependent variable, relative to misconduct clawbacks.

6.1. Robust versus misconduct clawbacks analysis

H1 examines financial reporting quality. As meet/beat is a result of intentional earnings management, a robust clawback covering unintentional errors should have no larger effect on *BEAT* than will a misconduct clawback policy. Unexplained audit fees, on the other hand, capture intentional and unintentional quality issues. Thus, we expect a negative coefficient on the *ROBUST*AFTER* coefficient if a robust clawback is more effective than a misconduct clawback at improving reporting quality.

Results of the H1 regression models including the *ROBUST* and *ROBUST*AFTER* variables are presented in panel B of Table 9. As expected, the β_5 coefficient on *ROBUST*AFTER* in the *BEAT* analysis (column 1) is insignificantly different from zero, though β_3 remains significant as in Table 5. β_3 and β_5 for *UAF* are both negative, but β_3 is only marginally significant while β_5 is insignificant. However, the linear combination of β_3 and β_5 is negative and highly significant, which provides some indication that the reduction in *UAF* is an increasing function of the robustness of a clawback. β_5 is insignificant in column 3, indicating that robust clawbacks have no marginal effect over misconduct clawbacks on restatement frequency.

H2 examines market participants' perceptions of financial reporting quality. We expect all clawbacks to cause an increase (decrease) in ERCs (dispersion). If robust policies are perceived as more effective, then these effects will be greater among the robust subset of firms. Results of the H2 regression models including the *ROBUST* and *ROBUST*AFTER* variables are presented in panel C Table 9. As in Table 6, the β_4 coefficient on *CLAW*AFTER*UE* in the ERC model is significantly positive, indicating that ERCs increase among all clawback adopters. β_8 on *ROBUST*AFTER*UE* is also significantly positive, which is consistent with this effect being greater for robust clawbacks. In the *DISP* model, β_3 is significantly negative, indicating a reduction in dispersion for all clawbacks. However, β_5 is insignificantly different from zero, indicating that there is not an incremental affect to adopting a robust clawback.

H3 examines pay-for-performance. We again expect that all clawbacks will result in an increase in pay-for-performance sensitivity, and that this effect will be even larger among firms implementing robust clawbacks. The results reported in panel D of Table 9 support this prediction: both β_4 and β_8 are positive and significant at 5 percent (one-tailed). Finally, H4a – H4c examine executive compensation. , The results reported in panel E of Table 9 are mixed: β_5 on *ROBUST*AFTER* is positive for both *TOTALCOMP* (column 1) and *SALARY* (column 2), but only the coefficient for *SALARY* is significant at conventional levels. Further, the economic significance of the 0.052 marginal effect on *ROBUST*AFTER* is considerably lower than the 0.268 coefficient on *CLAW*AFTER.*

Overall, our cross-sectional results provide some evidence that adopting a robust clawback provides incremental benefits to adopting a misconduct only clawback. With respect to the dependent measures we use to examine H1 – H4, we find having a robust clawback significantly affects *UAF*, *ERC*, pay-for-performance, and *SALARY*, but not our other measures. However, the economic significance of the incremental effects on *ERC* and *SALARY* are relatively low compared to the misconduct-only provision.

7. Conclusion

Though the Dodd-Frank Act will soon mandate restatement-triggered clawback provisions, hundreds of firms voluntarily adopted such clawbacks between 2006 and 2010 – long before the Dodd-Frank legislation was drafted and finalized. Relative to control firms, we find evidence consistent with clawback-adopters experiencing 1) an improvement in financial reporting quality, 2) an improvement in investors' perceptions about financial reporting quality, 3) an increase in pay-for-performance sensitivity, and 4) an increase in executive compensation over the sample period. In the cross-section, we find that these effects are incrementally amplified among the stricter set of clawbacks that do not require the manager to be culpable in the restatement.

Our results should be of interest to regulators and any organization that desires to better understand the consequences of clawback adoption. Our findings that clawback provisions appear to improve both actual and perceived financial reporting quality also provides several contributions to the literature linking governance and compensation to accounting quality and the use of accounting information in capital markets. Specifically, we provide further evidence consistent with: 1) managers having the intent and ability to manipulate financial statements in accordance with their personal incentives, 2) investors recognizing and responding to this agency problem by reducing their reliance on the manipulated information, and 3) boards of directors having the ability to reduce incentive misalignments with relatively simple governance measures.

APPENDIX 1
Variable Definitions

Financial Reporting and Compensation Test Variables (based on quarterly or annual data, depending on the sample)	
AFTER	Binary variable equal to one for periods following clawback adoption. For control firms, it is equal to one following the matched firm's clawback adoption.
AUD_OPINION	Binary variable for anything other than an unqualified audit opinion, as per Audit Analytics.
AVGLEV	Three-year average of annual leverage. Calculated as total debt divided by market value of assets: (DLTT + DLC) / (PRCC_F * CSHO + DLTT)
AVGLNMVE	Three-year average of annual log of market value of equity. MVE = CSHO * PRCC_F
AVGLNREV	Three-year average of annual log of total revenue (REVT).
AVGMTB	Three-year average of annual market-to-book. MTB = (CSHO * PRCC_F) / SEQ
AVGROA	Three-year average of annual return on assets. ROA = NI / AT
BEAT	Binary variable equal to one for raw unexpected earnings of $0.00 or $0.01. Calculated using IBES actual and consensus EPS figures.
BETA	Market model beta estimated using value-weighted market returns over the year ending five days before the earnings announcement.
BIG4	Binary variable for having one of the four largest audit firms, as per Audit Analytics.
BONUS	Log of total non-equity incentive compensation: Execucomp BONUS + NONEQ_INCENT
BUSSEG_SR	Square root of the firm's number of business segments, as per Compustat.
CASHPAY	Sum of salary and bonus per Execucomp.
CAR	Three-day buy-and-hold market-adjusted returns surrounding quarterly earnings announcements. Market adjustment based on value-weighted CRSP return.
CLAW	Binary variable equal to one for clawback adopters.
DISP	Standard deviation of analyst forecasts as per I/B/E/S, scaled by end-of-quarter price.
FQ4	Binary variable equal to one for the firm's fourth fiscal quarter.
GEOSEG_SR	Square root of the firm's number of geographic segments, as per Compustat.
GRANTS	Black-Scholes value of stock options granted. Calculated as Execucomp TDC1 – (TDC2 - OPT_EXER_VAL) as this calculation provides a significantly larger sample size than if using Execucomp's OPTION_AWARDS_BLK_VALUE variable.
ISSUANCE	Binary variable equal to one if the firm issued new debt during the year. Identified as firms with a current year's total debt (DLTT + DLC) greater than 105% of the prior year's total debt.
LEV	Leverage, calculated as total debt divided by market value of assets
LOSS	Binary variable equal to one for IBES actual EPS less than $0.00.
MTB	Market-to-book: (PRCCQ * CSHOQ) / SEQQ
ROBUST	Binary variable equal to one for the subset of clawback adopters for which the clawback is triggered by any restatement, not just those due to intentional misstatement. As identified by Corporate Library.
NONLINEAR	UE * |UE|
NUMEST	The number of individual equity analyst forecasts included in the consensus figure prior to the earnings announcement. As reported by I/B/E/S.
OTHERCOMP	Total compensation less salary, bonus, and the value of options granted. Per Execucomp, this consists of restricted stock granted, long-term incentive payouts, and all "other" compensation.
PERSIST	Autoregressive coefficient from earnings regressed on seasonally lagged earnings (EPSPXQ), calculated over the preceding two years.
PREDICT	Variance of the absolute value of unexpected earnings over the prior two years, with unexpected earnings based on a seasonal random walk.
RESTATE	Binary variable equal to one if the firm's financial statements are restated for the fiscal year, as reported by Audit Analytics.
RESTATE_PRE	Binary variable equal to one if the firm's financial statements for either of the trailing two years have been restated.

RET	One-year returns less the value-weighted market return, as per CRSP.
ROA	Return on assets: NI / AT
SALARY	Log of base salary. Execucomp SALARY variable.
SEO	Binary variable equal to one if the firm had a seasoned equity offering during the year.
SIZE	Natural log of market value of equity. MVE = PRCCQ * CSHOQ
TOTALCOMP	Log of total salary, bonus, value of restricted stock granted, value of stock options granted, long-term incentive payouts, and all other compensation. Execucomp TDC1.
UE	Unexpected earnings. I/B/E/S actual EPS less analyst consensus, scaled by end-of-quarter price (PRCCQ).
Unexplained Audit Fees (UAF)	The residual of regressions of audit fees on known determinants, run by year and size decile. Closely based on the abnormal audit fee model by Hribar et al. (2011). See Appendix B for further detail.
ΔROA^-	Change in annual ROA for ΔROA less than zero, or equal to zero otherwise.
ΔROA^+	Change in ROA for ΔROA greater than or equal to zero, or equal to zero otherwise.
Propensity Score Variables (data as of firms' fiscal 2006 year-end, unless otherwise noted)	
AVGDISP	Average quarterly analyst forecast dispersion over the previous two years. Set equal to zero if missing.
AVGERC	Average ERC over the previous two years, calculated using quarterly regressions of CAR on UE. Set equal to zero if missing.
AVGLEV	Three-year average of annual leverage. Calculated as total debt divided by market value of assets: (DLTT + DLC) / (PRCC_F * CSHO + DLTT)
AVGLNMVE	Three-year average of annual log of market value of equity. MVE = CSHO * PRCC_F
AVGLNREV	Three-year average of annual log of total revenue (REVT).
AVGMTB	Three-year average of annual market-to-book. MTB = (CSHO * PRCC_F) / SEQ
AVGROA	Three-year average of annual return on assets. ROA = NI / AT
AVGSPREAD	Average bid-ask spread over the previous two years. Calculated using CRSP closing bid and ask prices as in Chung and Zhang (2009).
CLAW	Binary variable equal to one for clawback adopter firms.
FFIND48	Fama-French 48 industry classification codes, based on SIC per Compustat.
HAVEDISP	Binary variable equal to one if AVGDISP is non-missing.
HAVEERC	Binary variable equal to one if AVGERC is non-missing.
HAVEOWN	Binary variable equal to one if OWNPERC is non-missing.
HAVEPAY	Binary variable equal to one if PAYSLICE is non-missing.
HAVETENURE	Binary variable equal to one if LNTENURE is non-missing.
HAVETURNOVER	Binary variable equal to one if TURNOVER is non-missing.
LNTENURE	Log of CEO tenure, as per Execucomp. Set equal to zero if missing.
OWNPERC	CEO's ownership percentage of the firm, as per Execucomp. Set to zero if missing.
PAYSLICE	Proportion of CEO's total pay to the aggregate total pay of the firm's top five executives, as per Execucomp variable TDC2. Set equal to zero if missing.
RESTATE_PRE	Binary variable equal to one if the firm's financial statements for either of the trailing two years have been restated
SDRET	Standard deviation of daily returns, as per CRSP.
TOTALCOMP	Log of total salary, bonus, value of restricted stock granted, value of stock options granted, long-term incentive payouts, and all other compensation. Execucomp TDC1.
TURNOVER	Binary variable equal to one if the firm experienced CEO turnover in the trailing two years, as per Execucomp. Set to zero if missing.

Governance Sensitivity Test Variables (based on annual data from Risk Metrics)	
BLANKCHECK	Binary variable for equal to one for blank check preferred stock. Available 2005 – 2009.
CBOARD	Binary variable equal to one for classified board. Available 2005 – 2009.
CEOCHAIR	Binary variable equal to one if the CEO is also chairman of the board of directors. Available 2007 – 2009. Obtained by combining the

	EMPLOYMENT_CEO and EMPLOYMENT_CHAIRMAN variables in Risk Metrics' Directors file.
CHARTER_AMEND_VOTEPCNT	Vote percentage required to amend charter. Available 2007 – 2009.
CONFVOTE	Binary variable equal to zero for confidential voting. Available 2005 – 2009.
CUMVOTE	Binary variable equal to zero for cumulative voting. Available 2005 – 2009.
DUALCLASS	Binary variable equal to one for dual class common stock. Available 2005 – 2009.
FAIRPRICE	Binary variable equal to one for fair price bidding provision. Available 2005 – 2009.
GPARACHUTE	Binary variable equal to one for golden parachute. Available 2005 – 2009.
LABYLW	Binary variable equal to one for limit ability to amend by-laws. Available 2007 – 2009.
LACHTR	Binary variable equal to one for limit ability to amend charter. Available 2007 – 2009.
LAW_AMEND_VOTEPCNT	Vote percentage required to amend by-laws. Available 2007 – 2009.
LSPMT	Binary variable equal to one for limit ability to call special meeting. Available 2007 – 2009.
LWCNST	Binary variable equal to one for limit ability to act by written consent. Available 2007 – 2009.
PPILL	Binary variable equal to one for poison pill. Available 2005 – 2009.
SPL_MEET_VOTEPCNT	Vote percentage required to call special meeting. Available 2007 – 2009.
SUPERMAJOR_PCNT	Supermajority mergers (percentage required) . Available 2007 – 2009.
UNEQVOTE	Binary variable equal to one for unequal voting. Available 2005 – 2009.
WRITTEN_CONSENT_VOTEPCNT	Vote percentage required for written consent. Available 2007 – 2009.

APPENDIX 2
Abnormal Audit Fees Calculation

Hribar et al. (2011) develop a proxy for measuring accounting quality using the unexplained portion of the firm's audit fees. The theory underlying the proxy is that auditors charge higher fees to firms with lower quality accounting. These higher fees are driven in part by the increased effort required to audit low quality accounting, as well as risk premium for the higher probability of incurring costs relating to a restatement (litigation costs, regulator penalties, etc.). Hribar et al. (2011) calculate unexplained audit fees as the residual from regressions of fees on known determinants. The authors find that this audit fee-based measure of accounting quality is predictive of fraud, restatements and SEC comment letters. The measure is also correlated with existing proxies for accounting quality.

Our audit fee model closely resembles that in Hribar et al. (2011):

$$Ln(FEE) = \alpha_0 + \beta_1 BIG4 + \beta_2 Ln(ASSETS) + \beta_3 BUSSEG\_SR + \beta_4 GEOSEG\_SR + \beta_5 INV + \beta_6 REC + \beta_7 DEBT + \beta_8 INCOME + \beta_9 LOSS + \beta_{10} AUD\_OPIN + \beta_{11} SEO + \beta_{12} ISSUANCE + \beta_{13} CLIENT\_SR + \Sigma_k IND + \varepsilon \quad \textbf{(A1)}$$

FEE is audit fees per Audit Analytics, *BIG4* is a binary variable if the auditor is one of the four largest firms (five prior to Anderson's failure), *ASSETS* is total assets, *BUSSEG_SR* is the square root of the firm's number of business segments per Compustat, *GEOSEG_SR* is the square root of the firm's number of geographic segments, *INV* is inventory scaled by assets, REC is receivables scaled by assets, *DEBT* is total debt scaled by assets, *INCOME* is operating income after depreciation scaled by assets, *LOSS* is a binary variable for income before income before extraordinary items less than zero, *AUD_OPIN* is a binary variable for anything other than an unqualified audit opinion, *SEO* is a binary variable if the firm had a seasoned equity offering during the year, *ISSUANCE* is a binary variable if the firm issued debt during the year, *CLIENT_SR* is the square root of the number of years for which the firm has had the same auditor, *IND* are industry fixed effects based on two-digit SIC codes.

The model is estimated by year and size decile for 2003 through 2010 (for a total of 80 regressions). Untabulated results show that the average adjusted r-squared across all regressions is 0.46. The residuals are considered abnormal audit fees and are used as one proxy for accounting quality, where a higher residual is indicative of lower quality.

REFERENCES

Addy, N. D., Jr., X. Chu, and T. R. Yoder. 2009. Recovering bonuses after restated financials: Adopting clawback provisions. *SSRN eLibrary*.

Ai, C., and E. Norton. 2003. Interaction terms in logit and probit models. *Economics Letters* 80: 123-129.

Armstrong, C. S., A. D. Jagolinzer, and D. F. Larcker. 2010. Chief executive officer equity incentives and accounting irregularities. *Journal of Accounting Research* 48 (2): 225-271.

Bebchuk, L. A., M. Cremers, and U. Peyer. 2010. The ceo pay slice. *Journal of Financial Economics*.

Bebchuk, L. A., and J. M. Fried. 2003. Executive compensation as an agency problem: National Bureau of Economic Research Cambridge, Mass., USA.

Bebchuk, L. A., and J. M. Fried. 2004. *Pay without performance: The unfulfilled promise of executive compensation*: Harvard Univ Pr.

Bertrand, M., and S. Mullainathan. 2003. Enjoying the quiet life? Corporate governance and managerial preferences. *Journal of Political Economy* 111 (5).

Brown, A. B., P. Y. Davis-Friday, and L. Guler. 2011. Economic determinants of the voluntary adoption of clawback provisions in executive compensation contracts. *SSRN eLibrary*.

Burgstahler, D., and E. Chuk. 2011. Correcting disinformation about discontinuity evidence of earnings management.

Burgstahler, D., and I. Dichev. 1997. Earnings management to avoid earnings decreases and losses. *Journal of Accounting and Economics* 24 (1): 99-126.

Burns, N., and S. Kedia. 2006. The impact of performance-based compensation on misreporting. *Journal of Financial Economics* 79 (1): 35-67.

Bushman, R. M., and A. J. Smith. 2001. Financial accounting information and corporate governance. *Journal of Accounting and Economics* 32 (1-3): 237-333.

Chan, L., K. C. Chen, T.-Y. Chen, and Y. YU. 2011. The effects of firm-initiated clawback provisions on earnings quality and auditor behavior. *SSRN eLibrary*.

Cheong, F. S., and J. Thomas. 2010. Why do eps forecast error and dispersion not vary with scale? Implications for analyst and managerial behavior. *Journal of Accounting Research*.

Chung, K. H., and H. Zhang. 2009. A simple approximation of intraday spreads using daily data. *SSRN eLibrary*.

Congress, U. 2010. Dodd-frank act. In *H.R. 4173*. USA.

Core, J. E. 2002. Discussion of the roles of performance measures and monitoring in annual governance decisions in entrepreneurial firms. *Journal of Accounting Research* 40 (2): 519-527.

Dechow, P., W. Ge, and C. Schrand. 2010. Understanding earnings quality: A review of the proxies, their determinants and their consequences. *Journal of Accounting and Economics* 50 (2-3): 344-401.

Dechow, P. M., and I. D. Dichev. 2002. The quality of accruals and earnings: The role of accrual estimation errors. *The Accounting Review* 77 (, Supplement: Quality of Earnings Conference): 35-59.

Degeorge, F., J. Patel, and R. Zeckhauser. 1999. Earnings management to exceed thresholds. *The Journal of Business* 72 (1): 1-33.

Devers, C. E., A. A. Cannella, G. P. Reilly, and M. E. Yoder. 2007. Executive compensation: A multidisciplinary review of recent developments. *Journal of Management* 33 (6): 1016.

Durtschi, C., and P. Easton. 2005. Earnings management? The shapes of the frequency distributions of earnings metrics are not evidence ipso facto. *Journal of Accounting Research* 43 (4): 557-592.

Durtschi, C., and P. Easton. 2009. Earnings management? Erroneous inferences based on earnings frequency distributions. *Journal of Accounting Research* 47 (5): 1249-1281.

Efendi, J., A. Srivastava, and E. P. Swanson. 2007. Why do corporate managers misstate financial statements? The role of option compensation and other factors. *Journal of Financial Economics* 85 (3): 667-708.

Equilar. 2009. Clawback policy report. Redwood Shores.

Equilar. 2011. Clawbacks are here to stay. *C-Suite Insight*, 7/11/2011.

Erickson, M., M. Hanlon, and E. L. Maydew. 2006. Is there a link between executive equity incentives and accounting fraud? *Journal of Accounting Research* 44 (1): 113-143.

Francis, J., R. LaFond, P. Olsson, and K. Schipper. 2005. The market pricing of accruals quality. *Journal of Accounting and Economics* 39 (2): 295-327.

Francis, J. R., and B. Ke. 2006. Disclosure of fees paid to auditors and the market valuation of earnings surprises. *Review of Accounting Studies* 11 (4): 495-523.

Fried, J., and N. Shilon. 2011. Excess-pay clawbacks. *The Journal of Corporation Law* 36 (4): 721-751.

Gao, H., K. Li, and M. L. Lemmon. 2011. A comparison of ceo pay in public and private us firms. *SSRN eLibrary*.

Garvey, G. T., and T. T. Milbourn. 2006. Asymmetric benchmarking in compensation: Executives are rewarded for good luck but not penalized for bad. *Journal of Financial Economics* 82 (1): 197-225.

Gompers, P., J. Ishii, and A. Metrick. 2003. Corporate governance and equity prices. *Quarterly Journal of Economics* 118 (1): 107-155.

Greene, W. 2010. Testing hypotheses about interaction terms in nonlinear models. *Economics Letters* 107 (2): 291-296.

Guidry, F., A. J. Leone, and S. Rock. 1999. Earnings-based bonus plans and earnings management by business-unit managers. *Journal of Accounting and Economics* 26 (1-3): 113-142.

Harris, D. G., L. Shi, and H. Xie. 2010. Is meeting and beating earnings benchmarks associated with accounting fraud. *Working Paper*.

Healy, P., and J. Wahlen. 1999. A review of the earnings management literature and its implications for standard setting. *Accounting Horizons* 13 (4): 365-383.

Hennes, K. M., A. J. Leone, and B. P. Miller. 2008. The importance of distinguishing errors from irregularities in restatement research: The case of restatements and ceo/cfo turnover. *The Accounting Review* 83 (6): 1487-1519.

Holthausen, R., and R. Verrecchia. 1988. The effect of sequential information releases on the variance of price changes in an intertemporal multi-asset market. *Journal of Accounting Research*: 82-106.

Hribar, P., T. D. Kravet, and R. J. Wilson. 2011. A new measure of accounting quality. *SSRN eLibrary*.

Hribar, P., and D. Nichols. 2007. The use of unsigned earnings quality measures in tests of earnings management. *Journal of Accounting Research* 45 (5): 1017-1053.

Jensen, M. C. 2005. Agency costs of overvalued equity. *Financial Management* 34 (1): 5-19.

Karpoff, J. M., D. S. Lee, and G. S. Martin. 2008a. The consequences to managers for financial misrepresentation. *Journal of Financial Economics* 88 (2): 193-215.

Karpoff, J. M., D. S. Lee, and G. S. Martin. 2008b. The cost to firms of cooking the books. *Journal of Financial and Quantiative Analysis* 43 (4): 32.

Kolasinski, A. C., and A. F. Siegel. 2010. On the economic meaning of interaction term coefficients in non-linear binary response regression models. *SSRN eLibrary*.

Kothari, S. 2001. Capital markets research in accounting. *Journal of Accounting and Economics* 31 (1-3): 105-231.

Lang, M. H., and R. J. Lundholm. 1996. Corporate disclosure policy and analyst behavior. *Accounting Review*: 467-492.

Le, C. T. 1998. *Applied categorical data analysis*. New York: John Wiley & Sons inc.

Lemmon, M., and M. R. Roberts. 2010. The response of corporate financing and investment to changes in the supply of credit. *Journal of Financial and Quantitative Analysis* 45 (03): 555-587.

Matsumoto, D. A. 2002. Management's incentives to avoid negative earnings surprises. *The Accounting Review* 77 (3): 483-514.

Roberts, M. R., and T. M. Whited. 2011. Endogeneity in empirical corporate finance. *Handbook of the Economics of Finance* 2.

Skinner, D. J., and R. G. Sloan. 2002. Earnings surprises, growth expectations, and stock returns or don't let an earnings torpedo sink your portfolio. *Review of Accounting Studies* 7 (2): 289-312.

Srinivasan, S., A. S. Wahid, and G. Yu. 2011. Admitting mistakes: An analysis of restatements by foreign firms listed in the us. *Working Paper*.

Teoh, S. H., and T. Wong. 1993. Perceived auditor quality and the earnings response coefficient. *Accounting Review*: 346-366.

Thomson Reuters. 2011. Corporate governance: Dodd-frank's compensation tremors. *Westlaw Business Currents*, 2/23/2011.

Wang, D. 2006. Founding family ownership and earnings quality. *Journal of Accounting Research* 44 (3): 619-656.

Wilson, W. M. 2008. An empirical analysis of the decline in the information content of earnings following restatements. *The Accounting Review* 83 (2): 519-548.

FIGURE 1
Histogram of Unscaled Earnings Surprises

The x-axis is unscaled earnings surprise, defined as actual I/B/E/S EPS minus the most recent I/B/E/S consensus forecast. Only surprises within $0.20 of consensus are included, N=11,226 of a total 12,423 quarterly observations in the financial reporting sample.

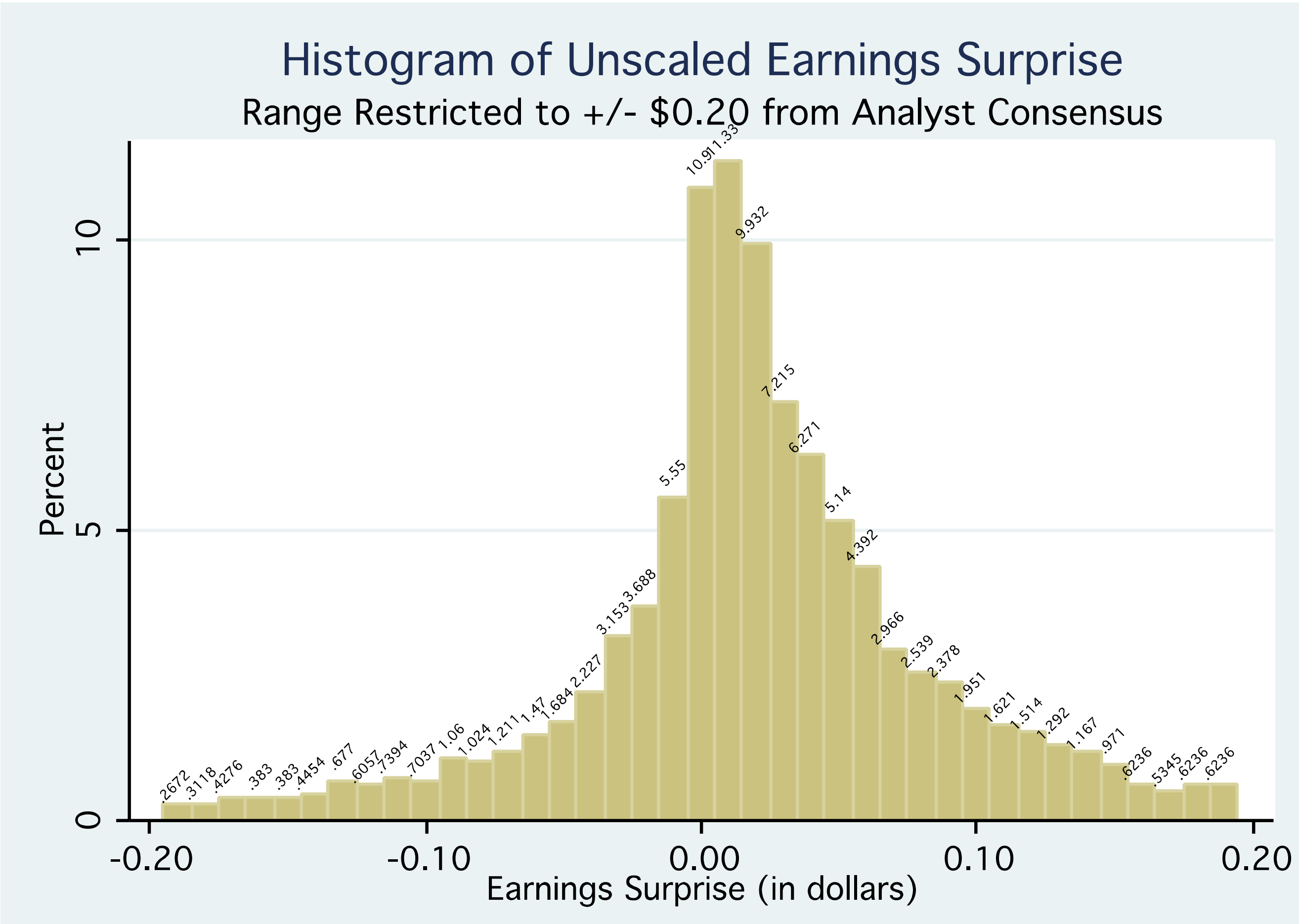

TABLE 1
Sample Refinement

Our sample is obtained from The Corporate Library and identifies firms with and without a restatement-triggered clawback provision as of mid-2007, 2008 and 2009. We apply separate data requirements for the quarterly financial reporting tests, annual financial reporting tests, and annual compensation tests.

Corporate Library Sample	
Total Firms	3,112
Less: Financial services firms	-660
Sample excluding financial firms	**2,452**
Propensity Score Model Restrictions	
Less: Firms without sufficient data	-126
Firms Available for Propensity Matching	**2,326**
Adopter Firms (108 in 2008 and 191 in 2009)	299
Firms with a clawback in all years	281
Firms without a clawback in all years	1,746
Firms Available for Propensity Matching	**2,326**
Quarterly Financial Reporting Sample Restrictions	
Less: Firms without sufficient Compustat, IBES, and CRSP data	-408
Firms with Available Data (2,326 – 408)	**1,918**
Adopter Firms	280
Firms with a clawback in all years	263
Firms without a clawback in all years	1,375
Firms with Available Data	**1,918**
Annual Financial Reporting Sample Restrictions	
Less: Firms without sufficient Compustat, IBES, CRSP, and AuditAnalytics data	-396
Firms with Available Data (2,326 – 396)	**1,930**
Adopter Firms	258
Firms with a clawback in all years	250
Firms without a clawback in all years	1,422
Firms with Available Data	**1,930**
Annual Compensation Sample Restrictions	
Less: Firms without sufficient Compustat, CRSP, and Execucomp data	-957
Firms with Available Data (2,326 – 957)	**1,369**
Adopter Firms	228
Firms with a clawback in all years	232
Firms without a clawback in all years	909
Firms with Available Data	**1,369**

TABLE 2
Propensity Score Matching Results

$CLAW = \alpha + \beta_1 AVGLEV + \beta_2 AVGLNMVE + \beta_3 AVGMTB + \beta_4 AVGROA + \beta_5 AVGLNREV + \beta_6 LNTENURE + \beta_7 HAVETENURE + \beta_8 PAYSLICE + \beta_9 HAVEPAY + \beta_{10} OWNPERC + \beta_{11} HAVEOWN + \beta_{12} SDRET + \beta_{13} RESTATE\_PRE + \beta_{14} TURNOVER + \beta_{15} HAVETURNOVER + \beta_{16} AVGSPREAD + \beta_{17} AVGDISP + \beta_{18} HAVEDISP + \beta_{19} AVGBEAT + \beta_{20} HAVEBEAT + \beta_{21} AVGERC + \beta_{22} HAVEERC + \beta_{23} TOTALCOMP + \Sigma\beta_k FFIND48 + \varepsilon$

The propensity-match logit model uses data as of year-end 2006. CLAW is a binary variable equal to one for firms with a clawback in place by the end of the sample period. All other variables are described in Appendix A. Panel A contains regression results. Panel B provides distributional information on the fitted conditional probabilities (i.e. "propensity scores"). All continuous variables are winsorized at 1% and 99%. ***Indicates significance at 1%; **at 5%; *at 10%, based on two-tailed tests. Standard errors are heteroskedasticity-robust.

Panel A: Regression Results

	Estimate	Wald Chi-Square	Pr > ChiSq	
Intercept	-2.494	8.651	0.003	***
AVGLEV	0.705	2.979	0.084	*
AVGLNMVE	0.256	7.442	0.006	***
AVGMTB	-0.007	0.206	0.650	
AVGROA	0.498	0.447	0.504	
AVGLNREV	0.275	10.619	0.001	***
LNTENURE	-0.418	10.555	0.001	***
HAVETENURE	2.115	3.648	0.056	*
PAYSLICE	1.195	3.495	0.062	*
HAVEPAY	1.168	1.143	0.285	
OWNPERC	-0.029	1.786	0.181	
HAVEOWN	-2.745	3.304	0.069	*
RETSD	-15.915	2.170	0.141	
RESTATE_PRE	0.219	2.424	0.120	
TURNOVER	-0.425	3.414	0.065	*
HAVETURNOVER	0.422	0.882	0.348	
AVGSPREAD	-3.822	0.007	0.936	
AVGDISP	0.033	0.001	0.978	
HAVEDISP	0.387	0.742	0.389	
AVGBEAT	0.019	0.002	0.962	
HAVEBEAT	0.343	0.435	0.510	
AVGERC	-0.005	9.231	0.002	***
HAVEERC	-0.211	0.388	0.534	
TOTALCOMP	-0.008	0.119	0.731	
FFIND48 binaries omitted				
N	2,326			
Pseudo R-Squared	0.229			
Rescaled R-Squared	0.339			

Panel B: Distribution of Fitted Conditional Probabilities ("P-Scores")

N	2326
Mean	0.249
1st percentile	0.010
25th percentile	0.077
median	0.176
75th percentile	0.368
99th percentile	0.922
standard deviaion	0.220

TABLE 3
Firm-Level Summary Information – Propensity Model Variable Differences in Means

Clawback Adopter firms are those that announce a new clawback provision between 2007 and 2009. Each firm is propensity-matched to a control firm. See Appendix A for variable definitions and data sources. The data for each firm is as of its 2006 fiscal year-end. Panel A includes the sample of firms with available data for the quarterly financial reporting analysis. Panel B includes the sample of firms with available data for the annual financial reporting analysis. Panel C includes the sample of firms with available data for the annual compensation analysis. Panel D presents additional distributional information on the difference between propensity scores for each pair of clawback and control firms. All continuous variables except propensity scores are winsorized at 1% and 99%. ***Indicates that the difference in means between Clawback Adopters and Control firms is significant at 1%; **at 5%; *at 10%, based on two-tailed tests (none are significant in panel A).

Panel A: Quarterly Financial Reporting Analysis Sample (Meet/Beat, ERC, and Analyst Dispersion Analysis)

		Clawback Adopters	Control Firms		
	N	Mean	Mean	Difference	T-Stat
Propensity-Score	280	0.384	0.384	0.000	0.000
avglev	280	0.219	0.221	-0.002	-0.140
avglnmve	280	7.884	7.775	0.109	0.960
avgmtb	280	3.179	3.108	0.071	0.210
avgroa	280	0.055	0.044	0.010	1.400
avglnrev	280	7.650	7.468	0.182	1.430
lntenure	280	1.083	1.191	-0.109	-1.290
havetenure	280	0.689	0.736	-0.046	-1.210
payslice	280	0.361	0.397	-0.036	-1.640
havepay	280	0.686	0.736	-0.050	-1.300
ownperc	280	0.859	0.888	-0.029	-0.130
haveown	280	0.689	0.736	-0.046	-1.210
retsd	280	0.020	0.020	0.000	-0.580
restate	280	0.229	0.239	-0.011	-0.300
turnover	280	0.161	0.175	-0.014	-0.450
haveturnover	280	0.825	0.782	0.043	1.280
avgspread	280	0.001	0.001	0.000	-1.180
avgdisp	280	0.047	0.045	0.001	0.260
havedisp	280	0.986	0.996	-0.011	-1.350
avgbeat	280	0.120	0.136	-0.016	-1.200
havebeat	280	0.993	0.996	-0.004	-0.580
avgerc	280	14.232	17.373	-3.141	-1.220
haveerc	280	0.971	0.986	-0.014	-1.170
totalcomp	280	4.809	4.322	0.488	0.800

Panel B: Annual Financial Reporting Analysis Sample (Restatements and Unexplained Audit Fees)

		Clawback Adopters	Control Firms		
	N	Mean	Mean	Difference	T-Stat
Propensity-Score	258	0.372	0.372	0.000	0.000
avglev	258	0.219	0.211	0.008	0.460
avglnmve	258	7.811	7.677	0.135	1.130
avgmtb	258	3.103	3.485	-0.382	-1.150
avgroa	258	0.051	0.050	0.001	0.120
avglnrev	258	7.579	7.365	0.215	1.610
lntenure	258	1.042	1.139	-0.097	-1.120
havetenure	258	0.671	0.713	-0.043	-1.050
payslice	258	0.349	0.387	-0.038	-1.660*
havepay	258	0.667	0.713	-0.047	-1.140
ownperc	258	0.833	0.550	0.282	1.770*
haveown	258	0.671	0.713	-0.043	-1.050
retsd	258	0.020	0.020	0.000	-0.250
restate	258	0.221	0.271	-0.050	-1.330
turnover	258	0.163	0.167	-0.004	-0.120
haveturnover	258	0.798	0.764	0.035	0.960
avgspread	258	0.001	0.002	0.000	-1.690

	N	Clawback Adopters Mean	Control Firms Mean	Difference	T-Stat
avgdisp	258	0.047	0.041	0.006	1.130
havedisp	258	0.942	0.938	0.004	0.180
avgbeat	258	0.108	0.129	-0.021	-1.550
havebeat	258	0.965	0.950	0.016	0.870
avgerc	258	12.773	16.956	-4.183	-1.610
haveerc	258	0.942	0.934	0.008	0.360
totalcomp	258	4.342	3.929	0.413	0.630

Panel C: Annual Compensation Analysis Sample (Pay-for-Performance and Compensation Components Test)

	N	Clawback Adopters Mean	Control Firms Mean	Difference	T-Stat
Propensity-Score	228	0.415	0.415	0.000	0.000
avglev	228	0.215	0.187	0.027	1.550
avglnmve	228	8.045	7.921	0.125	1.030
avgmtb	228	3.183	3.637	-0.454	-1.470
avgroa	228	0.061	0.066	-0.006	-0.990
avglnrev	228	7.841	7.591	0.251	1.970**
lntenure	228	1.284	1.467	-0.184	-2.110**
havetenure	228	0.816	0.908	-0.092	-2.870***
payslice	228	0.426	0.482	-0.056	-2.810***
havepay	228	0.811	0.899	-0.088	-2.680***
ownperc	228	0.965	0.846	0.120	0.630
haveown	228	0.816	0.908	-0.092	-2.870***
retsd	228	0.019	0.019	0.000	-0.020
restate	228	0.232	0.237	-0.004	-0.110
turnover	228	0.193	0.211	-0.018	-0.470
haveturnover	228	0.965	0.974	-0.009	-0.540
avgspread	228	0.001	0.001	0.000	0.640
avgdisp	228	0.046	0.042	0.004	0.650
havedisp	228	0.974	0.974	0.000	0.000
avgbeat	228	0.118	0.136	-0.018	-1.240
havebeat	228	0.991	0.978	0.013	1.140
avgerc	228	13.438	23.435	-9.997	-3.140***
haveerc	228	0.969	0.969	0.000	0.000
totalcomp	228	7.204	7.625	-0.420	-1.190

Panel D: Distribution of Differences in Matched Propensity Scores

	N	Mean	Median	Min	Max	Std. Dev.
Quarterly Financial Reporting Sample	280	-0.00007	-0.00003	-0.0112	0.0243	0.0024
Annual Financial Reporting Sample	258	-0.00007	-0.00004	-0.0112	0.0113	0.0020
Annual Compensation Sample	228	0.00000	-0.00002	-0.0112	0.0243	0.0026

TABLE 4
Observations Before/After Clawback Adoption

Panels A through C show the distribution of observations before and after clawback adoption for each of the three analysis samples. For clawback firms, the "after" period starts with the fiscal year following the clawback announcement in the firm's Proxy Statement. The same "after" period is assigned to the firm's matched control firm.

All samples span calendar 2005 through 2010.

Panel A: Quarterly Financial Reporting Analysis Sample

	Clawback Firms	% of Total	Control Firms	% of Total	All
Before Adoption	4,820	39%	4,805	39%	9,625
After Adoption	1,404	11%	1,394	11%	2,798
	6,224	**50%**	**6,199**	**50%**	**12,423**

Panel B: Annual Financial Reporting Analysis Sample

	Clawback Firms	% of Total	Control Firms	% of Total	All
Before Adoption	1,154	39%	1,159	39%	2,313
After Adoption	333	11%	321	11%	654
	1,487	**50%**	**1,480**	**50%**	**2,967**

Panel C: Annual Compensation Analysis Sample

	Clawback Firms	% of Total	Control Firms	% of Total	All
Before Adoption	970	38%	997	39%	1,967
After Adoption	305	12%	286	11%	591
	1,275	**50%**	**1,283**	**50%**	**2,558**

TABLE 5
Financial Reporting Quality Analysis (Hypothesis 1)

$$Reporting\ Quality\ Proxy = \alpha_1 + \beta_1 AFTER + \beta_2 CLAW + \beta_3(CLAW{*}AFTER) + \Sigma\beta_k CNTRLS + \Sigma\beta_k(CNTRLS{*}AFTER) + \Sigma\beta_k(CNTRLS{*}CLAW) + \Sigma\beta_k(CNTRLS{*}CLAW{*}AFTER) + \Sigma\beta_k(QTR\ or\ YEAR) + \varepsilon$$

This table includes analysis of two proxies for financial reporting quality that are used to assess Hypothesis 1: meet/beat frequency and unexplained audit fees. We also present an analysis of restatements for comparison purposes. Panel A presents summary information. Panel B presents the results of the regression model above. Column (1) is a logit model using quarterly meet/beat data. Included controls are MTB, SIZE, NUMEST, FQ4, and quarterly fixed effects. Column (2) is an OLS model using annual abnormal audit fees, calculated as the residual of regressions by industry, year and size deciles of audit fees on known determinants (based on Hribar et al. 2011). Year fixed effects are included. Column (3) is a logit model using annual restatements data. Included controls are BIG4, SIZE, BUSSEG_SR, GEOSEG_SR, LOSS, AUD_OPIN, SEO, ISSUANCE, MTB, ROA, LEV, RESTATE_PRE, and year fixed effects. All variables are defined in Appendix A. Continuous variables are winsorized at 1% and 99%. Regression standard errors are heteroskedasticity-robust and clustered by firm. The column headed H_0 lists our predictions for the coefficient signs, where applicable. ***Indicates significance at 1%; **at 5%; *at 10%, based on one-tailed tests where directional predictions are provided and two-tailed tests otherwise. Control variable coefficients are omitted for brevity.

Panel A: Summary Information

Number of Meet/Beat = 1	Before	After
Control Firms	652	160
Clawback Firms	609	129

Meet/Beat Rate	Before	After
Control Firms	13.6%	11.5%
Clawback Firms	12.6%	9.2%

Mean Unexplained Audit Fee	Before	After
Control Firms	0.061	0.084
Clawback Firms	0.103	0.062

Number of Restatements	Before	After
Control Firms	136	12
Clawback Firms	83	8

Restatement Rate	Before	After
Control Firms	11.7%	3.7%
Clawback Firms	7.2%	2.4%

Panel B: Regression Results (Controls and Interactions Untabulated)

			1	2	3
		H_0	Meet/Beat	Unexplained Audit Fee	Restatement
Constant	α_1		-0.842	0.059	-1.101
			[-1.74]*	[1.85]*	[-1.21]
AFTER	β_1		0.763	0.030	4.013
			[1.10]	[0.72]	[1.97]**
CLAW	β_2		0.001	0.042	-0.807
			[0.00]	[1.00]	[-0.71]
CLAW*AFTER	β_3	(-)	-2.042	-0.064	-8.046
			[-2.10]**	[-2.05]**	[-2.42]***
Fixed Effects			Quarter	Year	Year
Controls Included			Yes	No	Yes
Observations			12,423	2,967	2,967
Pseudo or Adj. R-squared			0.0267	0.002	0.136

TABLE 6
Perceptions of Financial Reporting Quality Analysis (Hypothesis 2)

ERC Model: $CAR = \alpha_1 + \alpha_2 AFTER + \alpha_3 CLAW + \alpha_4(CLAW*AFTER) + \beta_1 UE + \beta_2(AFTER*UE) + \beta_3(CLAW*UE) + \beta_4(CLAW*AFTER*UE) + \Sigma\beta_k CNTRLS + \Sigma\beta_k(CNTRLS*UE) + \Sigma\beta_k(CNTRLS*AFTER) + \Sigma\beta_k(CNTRLS*CLAW) + \Sigma\beta_k(CNTRLS*CLAW*AFTER) + \Sigma\beta_k(CNTRLS*AFTER*UE) + \Sigma\beta_k(CNTRLS*CLAW*UE) + \Sigma\beta_k(CNTRLS*CLAW*AFTER*UE) + \Sigma\beta_k QTR + \varepsilon$

Dispersion Model: $DISP = \alpha_1 + \beta_1 AFTER + \beta_2 CLAW + \beta_3(CLAW*AFTER) + \beta_4 ROBUST + \beta_5(ROBUST*AFTER) + \Sigma\beta_k CNTRLS + \Sigma\beta_k(CNTRLS*AFTER) + \Sigma\beta_k(CNTRLS*CLAW) + \Sigma\beta_k(CNTRLS*CLAW*AFTER) + \Sigma\beta_k QTR + \varepsilon$

Controls in the ERC model are NONLINEAR, PERSIST, PREDICT, MTB, BETA, SIZE, LOSS, and FQ4. Controls in the DISP model are NUMEST, PERSIST, PREDICT, MTB, BETA, SIZE, LOSS, and FQ4. All variables are defined in Appendix A. QTR are calendar quarter fixed effects. Continuous variables are winsorized at 1% and 99%. Regression standard errors are heteroskedasticity-robust and clustered by firm. The column headed H_0 lists our predictions for the coefficient signs, where applicable. ***Indicates significance at 1%; **at 5%; *at 10%, based on one-tailed tests where directional predictions are provided and two-tailed tests otherwise. Controls and interactions are omitted for brevity. Panel A presents summary DISP information. Panel B presents the regression estimates. Panel C presents coefficient linear combinations providing the conditional average earnings response coefficient in each period and group.

Panel A: Mean Dispersion by Group and Period

	Before	**After**
Control Firms	0.00235	0.00288
Clawback Firms	0.00211	0.00244

Panel B: Regression Results (Controls and Interactions Untabulated)

	ERC Model			**Dispersion Model**		
		H_0	**Estimates**		**H_0**	**Estimates**
Constant	α_1		0.014	α_1		0.005
			[1.84]*			[4.99]***
AFTER	α_2		-0.006	β_1		0.004
			[-0.37]			[2.14]**
CLAW	α_3		0.006	β_2		-0.003
			[0.58]			[-1.51]
CLAW * AFTER	α_4		-0.003	β_3	(-)	-0.005
			[-0.13]			[-1.97]**
UE	β_1	(+)	5.894			
			[8.24]***			
AFTER * UE	β_2		-1.373			
			[-0.70]			
CLAW*UE	β_3		-1.659			
			[-1.35]			
CLAW*AFTER*UE	β_4	(+)	6.381			
			[2.00]**			
Fixed Effects			Quarter			Quarter
Controls Included			Yes			Yes
Observations			12,423			12,423
Adjusted R-squared			0.108			0.347

Panel C: ERCs (Linear coefficient combinations from the ERC model in panel B)

Pre-Period, Control Firms	β_1	5.894**
Post-Period, Control Firms	$\beta_1+\beta_2$	4.521***
Pre-Period, Clawback Adopters	$\beta_1+\beta_3$	4.235***
Post-Period, Clawback Adopters	$\beta_1+\beta_2+\beta_3+\beta_4$	9.244***

TABLE 7
Pay-for-Performance Analysis (Hypotheses 3)

$\Delta log(CASHPAY) = \alpha_1 + \alpha_2 AFTER + \alpha_3 CLAW + \alpha_4(CLAW*AFTER) + \beta_1 \Delta ROA^+ + \beta_2(\Delta ROA^+ *CLAW) + \beta_3(\Delta ROA^+ *AFTER) + \beta_4(\Delta ROA^+ *CLAW*AFTER) + \beta_5 \Delta ROA^- + \beta_6(\Delta ROA^- *CLAW) + \beta_7(\Delta ROA^- *AFTER) + \beta_8(\Delta ROA^- *CLAW*AFTER) + \Sigma\beta_k CNTRLS + \Sigma\beta_k(CNTRLS*AFTER) + \Sigma\beta_k(CNTRLS*CLAW) + \Sigma\beta_k(CNTRLS*CLAW*AFTER) + \Sigma\beta_k YEAR + \varepsilon$

ΔROA^+ is change in ROA for positive or zero change, or zero otherwise. ΔROA^- is change in ROA for negative change, or zero otherwise. Controls include RET and TURNOVER. See Appendix A for variable specifications. Control variables and interaction terms are omitted below for brevity. All continuous variables are winsorized at 1% and 99%. Standard errors are heteroskedasticity-robust and clustered by firm. The column headed H_0 lists our predictions for the coefficient signs, where applicable. ***Indicates significance at 1%; **at 5%; *at 10%, based on one-tailed tests where directional predictions are provided and two-tailed tests otherwise.

		H_0	Δlog(CASHPAY)
Constant	α_1		-0.013
			[-0.45]
AFTER	α_2		0.105
			[1.67]*
CLAW	α_3		0.034
			[1.64]
CLAW*AFTER	α_4		-0.099
			[-1.86]*
ΔROA^+	β_1	(+)	1.171
			[4.09]***
ΔROA^+ *CLAW	β_2		-0.754
			[-1.51]
ΔROA^+ *AFTER	β_3		-0.456
			[-1.04]
ΔROA^+ *CLAW*AFTER	β_4	(+)	1.543
			[2.11]**
ΔROA^-	β_5	(+)	0.552
			[2.16]**
ΔROA^- *CLAW	β_6		0.567
			[1.61]
ΔROA^- *AFTER	β_7		1.017
			[1.54]
ΔROA^- *CLAW*AFTER	β_8	(+)	-1.212
			[-1.19]
Fixed Effects			Year
Controls Included			Yes
Observations			2,558
Adjusted R-squared			0.162

TABLE 8
Compensation Analysis (Hypotheses 4a – 4c)

$COMP = \alpha_1 + \beta_1 AFTER + \beta_2 CLAW + \beta_3(CLAW*AFTER) + \Sigma\beta_k CNTRLS + \Sigma\beta_k(CNTRLS*AFTER) + \Sigma\beta_k(CNTRLS*CLAW) + \Sigma\beta_k(CNTRLS*CLAW*AFTER) + \Sigma\beta_k YEAR + \varepsilon$

The dependent variables in columns 1 through 5 are TOTALCOMP, SALARY, BONUS, GRANTS, and all OTHER compensation, respectively. Included controls are AVGROA, AVGLEV, AVGLNMVE, AVGMTB, AVGLNREV, and TURNOVER. See Appendix A for variable specifications. Control and interaction term coefficients are omitted below for brevity. All continuous variables are winsorized at 1% and 99%. Regression results are presented in Panel B. Standard errors are heteroskedasticity-robust and clustered by firm. The column headed H_0 lists our predictions for the coefficient signs, where applicable. ***Indicates significance at 1%; **at 5%; *at 10%, based on one-tailed tests where directional predictions are provided and two-tailed tests otherwise. Results of Tobit models for regressions 3 and 4 (in which $0 values of the dependent variables are observed) are unchanged.

Panel A: Medians of Nominal Compensation (in Thousands)

	Total		**Salary**		**Bonus**		**Grants**		**Other**	
Nominal Comp.	**Before**	**After**	**Before**	**After**	**Before**	**After**	**Before**	**After**	**Before**	**After**
Control Firms	4,239	4,191	807	839	850	942	537	473	846	1,103
Clawback Firms	4,593	4,711	841	872	900	1,100	860	661	1,082	1,577

Panel B: Regression Results (Compensation in Logs, Control and Interaction Coefficients Untabulated)

		H_0	1 **Log(Total)**	2 **Log(Salary)**	3 **Log(Bonus)**	4 **Log(Grants)**	5 **Log(Other)**
Constant	α_1		4.543	4.957	1.277	-0.668	0.884
			[16.86]***	[23.10]***	[1.22]	[-0.45]	[1.08]
AFTER	β_1		-0.479	-0.164	-0.058	-3.627	-0.434
			[-1.80]*	[-1.50]	[-0.05]	[-1.99]**	[-0.46]
CLAW	β_2		-0.116	-0.045	0.448	0.016	-0.334
			[-0.37]	[-0.19]	[0.37]	[0.01]	[-0.35]
CLAW*AFTER	β_3	(+)	0.707	0.291	1.085	2.539	1.350
			[2.19]**	[2.09]**	[0.73]	[1.19]	[1.23]
Fixed Effects			Year	Year	Year	Year	Year
Controls Included			Yes	Yes	Yes	Yes	Yes
Observations			2,558	2,558	2,558	2,558	2,558
Adjusted R-squared			0.486	0.492	0.118	0.104	0.203

TABLE 9
Cross-Sectional Analysis by Clawback Type (Misconduct vs. Robust)

Below is cross-sectional analysis of the regressions in Tables 5 through 8, based on the distinction between misconduct and ROBUST clawback provisions. A ROBUST clawback is one that does not require intentional misstatement for compensation to be recovered by the firm. The analysis below repeats each of the models in Tables 5 through 8 with the inclusion of a binary variable ROBUST as well as ROBUST*AFTER. Control and fixed effect coefficients are untabulated for brevity. Regression standard errors are heteroskedasticity-robust and clustered by firm. The column headed H_0 lists our predictions for the coefficient signs, where applicable. ***Indicates significance at 1%; **at 5%; *at 10%, based on one-tailed tests where directional predictions are provided and two-tailed tests otherwise.

Panel A: Summary Information – Number of Firms with Misconduct versus Robust Clawback Provisions

	Control Firms	Misconduct Clawback	Robust Clawback
Quarterly Financial Reporting Sample	280	174	106
Annual Financial Reporting Sample	258	155	103
Annual Compensation Sample	228	140	88

Panel B: Financial Reporting Quality Analysis (Hypothesis 1)

			1		2		3
		H_0	**Meet/Beat**	**H_0**	**Unexplained Audit Fee**	**H_0**	**Restate**
Constant	α_1		-0.839		0.060		-1.097
			[-1.73]*		[1.87]*		[-1.20]
AFTER	β_1		0.765		0.028		4.005
			[1.10]		[0.68]		[1.97]**
CLAW	β_2		0.073		0.045		-0.786
			[0.11]		[0.94]		[-0.69]
CLAW*AFTER	β_3	(-)	-2.022	(-)	-0.054	(-)	-9.002
			[-2.06]**		[-1.43]*		[-2.88]***
ROBUST	β_4		-0.254		-0.007		-0.042
			[-1.80]*		[-0.13]		[-0.18]
ROBUST*AFTER	β_5	0	0.026	(-)	-0.053	(-)	1.170
			[0.12]		[-1.25]		[1.19]
Fixed Effects			Quarter		Year		Year
Controls Included			Yes		No		Yes
Observations			12,423		2,967		2,967
Pseudo or Adj. R-squared			0.0277		0.002		0.137
Linear Combination							
(CLAW*AFTER) + (ROBUST*AFTER)	$\beta_3 + \beta_5$	(-)	-1.997	(-)	-0.107	(-)	-7.832
			[-2.04]**		[-2.73]***		[-2.21]**

Panel C: Perceptions of Financial Reporting Quality Analysis (Hypothesis 2)

	ERC Model				Dispersion Model		
		H0	**Estimates**			**H0**	**Estimates**
Constant	α_1		0.014 [1.85]*		α_1		0.005 [4.99]***
AFTER	α_2		-0.006 [-0.36]		β_1		0.004 [2.14]**
CLAW	α_3		0.010 [0.89]		β_2		-0.003 [-1.49]
CLAW * AFTER	α_4		-0.008 [-0.31]		β_3	(-)	-0.005 [-2.02]**
UE	β_1	(+)	5.896 [8.26]***				
AFTER * UE	β_2		-1.383 [-0.70]				
CLAW*UE	β_3		-2.024 [-1.61]				
CLAW*AFTER*UE	β_4	(+)	5.677 [1.76]**				
ROBUST	β_5		-0.007 [-3.07]***		β_4		-0.000 [-0.43]
ROBUST*AFTER	β_6		0.005 [1.19]		β_5	(-)	0.000 [1.07]
ROBUST*UE	β_7		1.727 [3.35]***				
ROBUST*AFTER*UE	β_8	(+)	1.709 [1.72]**				
Fixed Effects			Quarter				Quarter
Controls Included			Yes				Yes
Observations			12,423				12,423
Adjusted R-squared			0.108				0.347
Linear Combination							
(CLAW*AFTER*UE) + (ROBUST*AFTER*UE)	$\beta_4 + \beta_8$	(+)	7.386 [2.27]**	CLAW*AFTER) + (ROBUST*AFTER)	$\beta_3 + \beta_5$	(-)	-0.004 [-1.88]**

Panel C: Pay-For-Performance Analysis (Hypothesis 3)

		H_0	Δlog(CASHPAY)
Constant	α_1		-0.013
			[-0.43]
AFTER	α_2		0.102
			[1.62]
CLAW	α_3		0.021
			[0.93]
CLAW*AFTER	α_4		-0.118
			[-1.98]**
ΔROA^+	β_1	(+)	1.170
			[4.08]***
ΔROA^+*CLAW	β_2		0.022
			[0.05]
ΔROA^+*AFTER	β_3		-0.455
			[-1.03]
ΔROA^+*CLAW*AFTER	β_4	(+)	1.447
			[1.89]**
ROBUST	β_5	(+)	0.039
			[1.49]*
ROBUST*AFTER	β_6		0.051
			[0.75]
ROBUST*ΔROA^+	β_7		-3.022
			[-3.06]***
ROBUST*AFTER*ΔROA^+	β_8	(+)	2.174
			[1.87]**
Fixed Effects			Year
Controls Included			Yes
Observations			2,558
Adjusted R-squared			0.170
Linear Combination			
(CLAW*AFTER*UE) +			
(ROBUST*AFTER*UE)	$\beta_4 + \beta_8$	(+)	3.620
			[3.28]***

Panel D: Compensation (Hypothesis 4)

		H_0	1 Log(Total)	2 Log(Salary)	3 Log(Bonus)	4 Log(Grants)	5 Log(Other)
Constant	α_1		4.544	4.958	1.279	-0.668	0.884
			[16.86]***	[23.09]***	[1.22]	[-0.45]	[1.08]
AFTER	β_1		-0.479	-0.164	-0.058	-3.627	-0.434
			[-1.80]*	[-1.50]	[-0.05]	[-1.99]**	[-0.46]
CLAW	β_2		-0.084	-0.037	0.506	0.019	-0.321
			[-0.27]	[-0.16]	[0.42]	[0.01]	[-0.34]
CLAW*AFTER	β_3	(+)	0.697	0.268	1.055	2.527	1.369
			[2.15]**	[1.92]**	[0.71]	[1.18]	[1.24]
ROBUST	β_4		-0.132	-0.033	-0.241	-0.014	-0.055
			[-2.13]**	[-1.10]	[-1.14]	[-0.04]	[-0.30]
ROBUST*AFTER	β_5	(+)	0.011	0.052	0.086	0.070	-0.128
			[0.19]	[2.10]**	[0.28]	[0.21]	[-0.66]
Fixed Effects			Year	Year	Year	Year	Year
Controls Included			Yes	Yes	Yes	Yes	Yes
Observations			2,558	2,558	2,558	2,558	2,558
Adjusted R-squared			0.488	0.495	0.118	0.104	0.202
Linear Combination							
(CLAW*AFTER) + (ROBUST*AFTER)		(+)	0.834	0.320	1.189	3.141	1.283
			[2.528]***	[2.311]**	[0.796]	[1.525]*	[1.156]