Payoffs to Aggressiveness

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Payoffs to Aggressiveness

1. Introduction

We estimate a model that links aggressive real-actions with aggressive financial reporting and connects each type of behavior to payoffs to investors and to CEOs. We start from the premise that aggressiveness is a (possibly) stable manager and/or firm characteristic that is an external manifestation of a combination of largely unobservable factors, including managerial traits, cognitive biases, self-selection, matching and corporate culture. This premise suggests a linked propensity toward both more aggressive real-action behavior and more aggressive financial-reporting behavior; the latter includes both reporting that complies with US GAAP and reporting that violates US GAAP and results in restatements. The premise does not imply that aggressive behavior is either beneficial or detrimental, nor does it provide predictions about the payoffs to the two kinds of behaviors. That is, our approach to analyzing the payoffs to aggressive behaviors takes the perspective that these behaviors are common, rational, possibly benign and not necessarily extreme, and that it is an empirical question as to whether aggressive behaviors affect payoffs to shareholders and CEOs and if so, how.

In the first part of the paper, we explore the role of within-GAAP reporting aggressiveness using a broad sample of firms covering fiscal years 1992-2014. Specifically, we estimate a structural equations model (SEM) that allows for an association between latent variables for real aggressiveness and reporting aggressiveness and for associations between each latent variable and payoffs to CEOs and shareholders. The use of a SEM approach in this setting has several advantages: it addresses the error-in-variables issue with regard to the inputs to the latent variables; it does not impose constraints on which variables are informative (the estimation determines the proxies most important to defining each latent variable); and it exploits the available information by simultaneously estimating *both* the latent variables and their correlations *and* the paths between each aggressiveness variable and each payoff variable.

In the main model, the latent variable for aggressive real behavior (*RealAgg*) is based on three proxies: acquisition value, proceeds of debt issuance and goodwill; the latent variable for reporting aggression (*ReportAgg*) is based on the choices of inventory method and depreciation method.[1] In line with prior work, we consider both straight-line depreciation and FIFO as aggressive accounting method choices and begin our empirical analysis by showing that they increase not only the level of income but also its over-time volatility. Our tests show *RealAgg* and *ReportAgg* are positively correlated, consistent with the view that aggressiveness is an underlying firm-characteristic that manifests in both real-actions and financial-reporting.

We measure shareholder payoffs as future abnormal returns, which control for changes in risk resulting from aggressive behaviors, and as average returns, which do not. We measure CEO payoffs as total compensation, both unadjusted and industry-year-adjusted. With regard to payoffs, we find that both CEO compensation and shareholder returns are *negatively* associated with *RealAgg* and *positively* associated with *ReportAgg*. Thus, despite the logical inference that both forms of aggressive behavior spring from a stable characteristic, they have different payoff effects. In particular, neither shareholders nor CEOs benefit from aggressive real actions, but they do benefit from aggressive reporting.[2]

In the second part of the paper, we analyze restatement firms, which by definition exhibit outcome-based evidence of GAAP violations in their financial reporting, to explore the consequences of extreme reporting aggressiveness. We begin by noting that the finding of larger (more positive) returns to shareholders of more aggressive reporting firms seems at odds with prior research documenting negative returns to announcements of extremely aggressive accounting, such as restatements. However, our broad-sample measure of aggressive reporting is not intended to focus on extreme behavior and, in particular, on the kind of non-GAAP reporting that leads to restatements; rather, it is intended to capture reasonably stable within-GAAP accounting policy choices. To probe the returns consequences of these different degrees of reporting aggression, we examine returns to restatement firms and non-restatement firms

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[1] Section 3.2 shows that the inclusion of six additional proxies for real aggression (amount of goodwill impairment, proceeds of equity issuances, capital expenditures, advertising expenditures, R&D expenditures and leverage) does not improve the model; in fact, the inclusions worsen model fit. We find a similar issue with model fit when we expand the definition of reporting aggressiveness to include five additional variables from Dichev and Li (2013).

[2] CEOs might also benefit from aggressive real actions in other ways, for example, by moving to a larger firm or being appointed to corporate boards. We do not model these behaviors.

conditional on including the latent variable for *ReportAgg* as well its interaction with pre-restatement and post-restatement indicator variables. Consistent with our prior tests, we find that firms with higher values of *ReportAgg* have more positive returns as compared to other sample firms (coefficient of 0.0108, t = 6.28). For restatement firms, this association is *more* pronounced in the pre-restatement period (incremental coefficient of 0.0056, t = 1.97, two-tailed p-value = 0.05) and *less* pronounced in the post-restatement period (incremental coefficient of −0.0067, t = −1.71, two-tailed p-value = 0.09). The subset of fraudulent restatements shows, as expected, more extreme results. While we verify that shareholders of restating firms benefit in the pre-restatement period and suffer in the post-restatement period, these results are consistent with more aggressive restatement firms showing similar but more extreme returns before and after the event.

To calibrate the *net* economic magnitude of these associations, we perform calendar-time portfolio regressions over a long window around restatements, tracking returns from 72 months before to 36 months after the restatement. For the sample that includes all restatements, shareholders earned an average cumulative abnormal return of 22% in the 69 months leading up to the restatement, lost about 11 percentage points (pp) in the five months surrounding the restatement and experienced no significant abnormal returns thereafter. Thus, long-term shareholders earned about 11% in cumulative abnormal returns, conditional on a restatement event and allowing for returns-reactions during the following two months. For the subset of restating firms with high *ReportAgg* scores, the magnitude of the returns effects increases to 40.5% (pre-restatement) followed by a 13.8pp drop, yielding an average net cumulative abnormal return of 26.8%. The pattern of increasing returns to more aggressive reporting firms applies also to firms with fraudulent restatements and high *ReportAgg* scores, where we find pre-restatement returns of 68.0% followed by a drop of 25.6 pp, yielding a net abnormal return of about 42.4%.

We confirm these results by contrasting returns over the reporting period(s) affected by a restatement (and known only ex-post) with the restatement-announcement return. Depending on the lengths of the event windows, we find overall cumulative returns of about 40.9% (35.9%) for the average restatement (fraudulent restatement) firm. We interpret these results as indicating that long-term positive returns to shareholders of aggressive reporting firms are not eliminated even when the reporting behavior results in a detected GAAP violation and restatement.

We summarize our results as follows. We use a broad set of variables to capture aggressive decisions, both real actions and accounting choices, in a structural equations model to define latent variables for each type of aggressive behavior. The results both identify which input variables are most important for each aggressiveness latent variable, and whether and how each latent variable is associated with payoffs to shareholders and CEOs. While real aggressiveness and reporting aggressiveness are, as expected, positively correlated, they have distinct and opposing effects on payoffs: real-action aggressiveness is associated with lower stock returns and lower CEO compensation and reporting aggressiveness is associated with higher stock returns and higher CEO compensation. We further show that the documented higher returns to reporting aggressiveness are more than sufficient to offset negative short-window stock market reactions to extreme adverse reporting outcomes, specifically, restatements.

The rest of the paper is organized as follows. In Section 2, we summarize findings from related research. Section 3 describes the sample and data. Section 4 presents the structural equations modeling results, and Section 5 presents our analysis of the long-term returns to restatement firms. Section 6 concludes.

2. Background, previous research and our approach

We describe results from three empirical literatures that provide a foundation for our analysis.[3] Section 2.1 analyzes research on aggressive real behavior; Section 2.2 describes research on aggressive reporting behavior including earnings management and GAAP violations; and Section 2.3 describes

[3] A fourth stream of empirical research investigates the real effects of accounting rules. For example, Shroff (2017) finds that certain changes in US GAAP affect corporate investment decisions through a debt-financing channel (reported outcomes under a new standard affect covenant compliance and in turn debt financing) and an information channel (compliance with a new standard requires the collection of new information that in turn affects investment decisions). Kanodia and Sapra (2016) discuss the theoretical underpinnings for this stream of empirical research and suggest avenues for extending it. We do not discuss this research in detail because (1) it focuses mostly on *mandatory* changes in accounting *standards* while we focus on *free-choice* managerial selections of accounting *policies* within GAAP and (2) it tends to focus on cost of capital effects that we try to control for by using abnormal returns as the shareholder-payoff metric.

studies investigating links between the two. Section 2.4 describes how our approach builds on and differs from the approaches in previous research.

2.1 Research on real aggressiveness

Within the literature on determinants of aggressive real behaviors, researchers have examined whether and how managers' traits manifest in investment decisions and financial policies, sometimes capturing unspecified managerial traits in a simple fixed-effects specification (e.g., Bertrand and Schoar, 2003). Motivated by theories rooted in bounded rationality (Simon, 1955), research also links observable and unobservable manager characteristics to corporate actions. For example, upper-echelon theory argues that executive perceptions, cognitions and values manifest themselves in the decision-making process (Hambrick and Mason, 1984).

One well-studied unobservable managerial characteristic is overconfidence, typically viewed as a cognitive bias. An overconfident manager overestimates the accuracy of his knowledge and/or information and, as a consequence, underestimates the risk of a project or decision; that is, he acts less conservatively than an objective consideration would warrant.[4] Research shows that overconfidence leads to greater investment-to-cash flow sensitivity (Malmendier and Tate, 2005a, b), larger capital expenditures (Ben-David, Graham and Harvey, 2013), excessive merger activity (Roll, 1986; Doukas and Petmezas, 2007; Billett and Qian, 2008; Malmendier and Tate, 2008), larger acquisition premia (Hayward and Hambrick, 1997), more use of debt financing (Hackbarth, 2008; Li, 2010; Ben-David et al., 2013), riskier product introductions (Simon and Houghton, 2003), physical plant expansions (Nutt, 1993) and innovation (Staw, 1991).[5]

[4] Overconfidence differs from optimism (expecting better outcomes than are warranted), but both overconfidence and optimism lead to the same behavior – a tendency to take on more risky projects (Gervais, 2010). We do not distinguish between overconfidence and optimism.

[5] Research also links executives' personal behavior to corporate behavior. For example, Cronqvist, Makhija and Yonker (2012) show that personal leverage (as evidenced by house purchases) is associated with corporate leverage. Graham, Harvey and Puri (2013) use psychometric tests to measure executives' underlying psychological traits and attitudes. Among other things, they show that behavioral traits such as optimism and managerial risk-aversion are related to firms' financial policies and to compensation.

With regard to outcomes associated with overconfidence, theory shows that *some* degree of overconfidence is benign or even desirable, in the sense of leading to larger payoffs to shareholders (Goel and Thakor, 2008; Gervais, Heaton and Odean, 2011). The reasoning is that overconfidence mitigates a manager's risk aversion by partly offsetting the manager's natural tendency to make overly cautious investment decisions. Contracting may be more efficient and less expensive because the overconfident manager is more willing to accept risk. Gervais et al. (2011) show analytically that, when overconfidence benefits shareholders, overconfident managers also benefit, and Gervais and Goldstein (2007) show that manager overconfidence is Pareto-optimal for shareholders and the manager. An important qualification to these findings is that, while *some* overconfidence can benefit both managers and shareholders, *too much* overconfidence can be value-destroying (Goel and Thakor 2008, Gervais, et al. 2011). This notion is also reflected in Gervais and Goldstein's finding (2007) that Pareto-optimality is no longer a possible outcome when the overconfident manager is also the leader of the firm.

Empirical research on the consequences of overconfidence shows that overinvestment is generally associated with lower shareholder returns (Gervais, 2010). For example, Lang, Stulz and Walkling (1991) and Malmendier and Tate (2008) find that market responses to acquisition announcements are more negative for acquiring firms led by overconfident CEOs. Titman, Wei and Xie (2004) find that firms that substantially increase capital investments have subsequent negative adjusted returns. This relation is stronger for firms with higher cash flows and lower debt ratios, consistent with Jensen's (1986) agency arguments.

It is generally difficult to distinguish empirically between outcomes caused by managerial traits and outcomes caused by selective hiring (matching) in which firms with certain characteristics selectively hire managers with certain characteristics, for example, overconfidence (Kaplan, Klebanov and Sorensen, 2012).[6] This distinction does not matter for our study, because our focus is on the payoffs to aggressive behavior, not its causes. Specifically, it does not matter whether the behavior arises from overconfidence,

[6] For example, Fee, Hadlock and Pierce (2013) find firm policies to be stable after exogenous CEO departures, and interpret this result as evidence against idiosyncratic managerial effects affecting firms.

any other managerial trait, genetics (Cronqvist and Siegle, 2014), life experiences (such as military experience, Benmelech and Frydman, 2014), matching of firm and manager, corporate culture, hubris (Roll, 1986), corporate empire-building (Jensen, 1986), or some combination of these.

Our research design builds on this research insofar as we use variables capturing financing and investment decisions as manifestations of real-activities aggressiveness. With regard to financing decisions, numerous studies document a negative relation between future returns and both equity and debt financing (Ritter, 1991; Spiess and Affleck-Graves, 1995, 1999; Bradshaw, Richardson and Sloan, 2006). With regard to investment decisions, the evidence depends on the type of investment. For example, Titman, Wei and Xie (2004) document a negative association between capital investments and subsequent returns, and Fairfield, Whisenant and Yohn (2003) find a similar negative association for the change in net operating assets, another measure of investment.[7] With regard to investments in intangible assets, Chan, Lakonishok and Sougiannis (2001) find no relation between future returns and either R&D spending or advertising spending while Eberhart, Maxwell and Siddique (2004) document positive long-term abnormal returns for firms that increase their R&D expenditures.

To summarize, one strand of research on determinants of corporate investment decisions focuses on the possibly benign role of some (unspecified) amount of managerial overconfidence, and shows analytically that a certain amount of overconfidence reduces risk aversion in a beneficial way. Another strand of research documents mixed associations between share returns and corporate investing and financing decisions. We extend this stream of research by focusing on payoffs to both shareholders and CEOs and by considering both real aggression and reporting aggression and a possible link between them.

2.2 Research on financial reporting aggressiveness

Research on aggressive financial reporting behavior examines both determinants and consequences, often but not always focusing on unusual, adverse and extreme reporting outcomes. With regard to determinants, Schrand and Zechman (2012) examine overconfidence and AAER incidence.

[7] Penman and Zhu (2014) consider whether the returns to investment and asset growth are due to market mispricing or rational pricing.

Davidson, Dey and Smith (2015) and Amir, Kallunki and Nilsson (2014) examine personal CEO factors (for example, criminal records) and restatements or financial reporting quality. Jia, van Lent and Zeng (2013) investigate the role of exposure to testosterone, which has been linked to aggression, risk-seeking and misreporting. Within this literature, results are sometimes sensitive to both the choice of proxy for managerial traits and the type of manager studied. For example, Schrand and Zechman find that indirect overconfidence measures (such as managers' portfolio holdings) show the expected association between overconfidence and greater incidence of AAERs, but other proxies for overconfidence (e.g., education-related) do not. Amir et al. report significant effects for CEOs but not for CFOs. Davidson et al. report significant results for executives with a criminal record, but not for non-frugal executives (proxied as ownership of luxury goods).

Proxies for financial reporting aggression range from discretionary accruals, which can change year-by-year, to accounting method choices, which are reasonably stable, to severe and unusual adverse outcomes such as restatements and fraud. Each proxy represents different research design choices, discussed next.

Accruals proxies: The literature on accruals management considers measurement, determinants and outcomes. A common proxy for accruals management, sometimes interpreted as an outcome of aggressive reporting, is discretionary (or abnormal) accruals. Researchers often measure discretionary accruals as the residual (the unexplained portion of total accruals) from a model that uses accounting fundamentals such as change in sales and property, plant and equipment as explanatory variables to capture normal accruals. One common approach is to estimate the model in industry cross sections. Positive (negative) discretionary accruals are interpreted as income-increasing (income-decreasing) earnings management.

Numerous studies examine whether more-positive discretionary accruals are associated with incentives to manage earnings upwards in a specified reporting period, for example, to meet or exceed analyst forecasts or to reach bonus targets. The weight of the evidence from this research supports the existence of managerial responses to the incentives embodied in these benchmarks. Research has also examined the association between discretionary accruals and shareholder returns. Subramanyam (1996) documents a positive association between discretionary accruals and concurrent returns or firm value, and Frank, Lynch, Rego and Zhao (2012) find that discretionary accruals are associated with larger values of Tobin's Q. There is also evidence that discretionary accruals are negatively associated with future returns (Sloan, 1996 and Xie, 2001; however, Francis and Smith, 2005 argue that this result is driven by a flawed measure of accruals).

Accounting method choice: Research on accounting method choice has for the most part focused on its determinants. For example, Skinner (1993) calculates an accounting choice score based on his scoring of three accounting choices (depreciation method, inventory cost-flow assumption and goodwill amortization period) as income increasing versus income-decreasing. He finds that income-increasing choices are associated with firms that are smaller, more levered and more likely to have accounting-based bonus plans, and concludes that a firm's investment opportunity set has only an indirect effect on accounting choice. Using an aggregate score based on inventory valuation method and depreciation method, Bowen, DuCharme and Shores (1995) examine how implicit claims, in the form of arrangements between a firm and its customers, suppliers, employees and short-term creditors, are linked to accounting policy choice. Their results suggest managers respond to these implicit claims by choosing income-increasing accounting methods. Finally, Dichev and Li (2013) investigate the relation between 1-3-year sales growth and several accounting policy choices. They predict that the ability of income-increasing accounting choices to increase income depends on growth,[8] but they find no relation between growth and accounting aggressiveness as proxied by income-increasing policy choices.[9]

We focus on within-GAAP accounting policy choices because they, in contrast to discretionary accruals, for example, capture managerial *inputs* into the reporting process, are directly observable without further assumptions and estimations and are relatively stable over time. Our tests are joint tests of

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[8] For example, the choice between straight-line depreciation and accelerated depreciation will have no effect on income if the balance in gross property, plant and equipment is constant over time.
[9] These null-results may be attributable to the inconsistent correlation pattern across the accounting choices they consider.

(1) the construct validity of our reporting aggressiveness measure and (2) whether reporting aggressiveness is linked to the outcomes we consider. The tests are premised on the view that managers make accounting policy choices within GAAP to convey information about future cash flows, and this information is incrementally informative in the presence of other outcome determinants (e.g., beta, size, or the book-to-market ratio in the returns tests).

Adverse reporting events: Research has analyzed unusual and extreme accounting practices that violate US GAAP. The focus is typically on adverse outcomes, including restatements of financial reports, accounting and auditing enforcement releases (AAERs), allegations of fraud, and shareholder lawsuits alleging defective accounting. Research documents negative reactions to announcements of adverse reporting events (Palmrose, Richardson and Scholz, 2004; Hennes, Leone and Miller, 2008; Dechow, Sloan and Sweeney, 1996). Hribar and Jenkins (2004) document increases in restating firms' costs of equity following the restatement, consistent with shareholders demanding a higher return for some incremental risk. With regard to consequences to managers of adverse reporting events, the evidence is mixed. Beneish (1999) and Agrawal, Jaffe and Karpoff (1999) find little evidence of increased CEO turnover following fraud and/or GAAP violations. However, Desai, Hogan and Wilkins (2006) find that about 60% of restating firms experience some form of top-manager turnover in the two years following the restatement, as compared to 35% turnover for a matched sample of non-restating firms.

2.3. Research on the interaction between real aggressiveness and reporting aggressiveness

Research that posits a connection between aggressive real decisions and aggressive reporting decisions[10] often takes the perspective that these decisions are intended to obfuscate performance, perhaps

[10] Several studies examine the association between investment decisions and reporting quality, concluding that firms with poorer reporting quality overinvest. For example, Biddle, Hilary and Verdi (2009) measure reporting quality using the FOG index which captures readability of the financial statements; Bushman, Piotroski and Smith (2011) use timely recognition of losses; Biddle and Hilary (2006) measure reporting quality using a combination of loss avoidance, earnings smoothing, timeliness and the inverse of a conservatism measure; and Balakrishnan, Core and Verdi (2014) use six measures: bid-ask spread, timely loss recognition, the length of the MD&A section, number of 8-Ks filed, number of management forecasts, and a composite score. We do not link these results to our research

to meet an earnings target or achieve some other reporting objective. For example, Roychowdhury (2006) examines whether managers make short-term reversible operating decisions to manage earnings upward. Zang (2012) finds evidence that managers appear to treat earnings management by making short-term operating decisions and earnings management by accruals manipulations as substitutes, emphasizing the need to explore both simultaneously.

McNichols and Stubben (2008) find that firms experiencing negative reporting events (AAERs, restatements and shareholder lawsuits) over-invest during the period of aggressive reporting behavior. Using discretionary accruals as their proxy for reporting aggressiveness, Frank, Lynch and Rego (2009) find a positive association between tax aggressiveness and reporting aggressiveness. Schrand and Zechman (2012) find an increased probability of receiving an AAER for overconfident CEOs; their proxy for CEO overconfidence is based on variables identified in the finance literature as associated with overconfidence (overinvestment relative to the industry, acquisition activity and leverage).

Some research investigates tax avoidance (aggressive tax reporting) as a specific example of a potentially risky activity. For example, Frank, Lynch, Rego and Zhao (2012) investigate whether firms with greater tax aggressiveness and financial reporting aggressiveness are characterized by more risk-taking environments. They find associations among performance-matched discretionary accruals (their proxy for reporting aggressiveness), a measure of tax aggressiveness (Frank, Lynch and Rego, 2009) and their proxy for risk-taking environments before but not after the passage of the Sarbanes-Oxley Act (SOX). They also find shareholders placed a premium on reporting aggressiveness but not risk-taking environments, also only in the pre-SOX period. Guenther, Matsunaga and Williams (2017) find no evidence that several proxies for risky tax avoidance are linked to risk as measured by stock return volatility and future tax rate volatility; they do find evidence that tax rate volatility and firm risk (measured as stock returns volatility) are linked.

2.4. <u>Relation between our study and previous research</u>

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question because it is not obvious if and how reporting quality as captured by these studies maps into the reporting-aggressiveness construct of interest in our study.

We take the perspective that aggressiveness is a possibly stable firm characteristic, not necessarily specific to an individual manager. This perspective allows for aggressive behavior to arise from any combination of managerial traits and other forces such as self-selection and matching of managers to corporate culture (Van den Steen, 2010). We select a measure of reporting aggressiveness that is directly observable and reasonably stable – a combination of two freely-available accounting method choices -- rather than one that changes frequently, such as discretionary accruals.[11] Our measure of reporting aggressiveness is intentionally based on commonly occurring, within-GAAP policy choices, not on extreme or unusual behavior that demonstrably or allegedly violates GAAP. In contrast to the literature on earnings management, we posit that management makes accounting policy choices to convey information about how management will operate the firm and, relatedly, about expected future cash flows from those operating decisions.[12] This reporting aggressiveness measure is thus aligned with our real aggressiveness behavior which is similarly based on commonly-occurring financing and investing decisions, and not on extreme or unusual corporate behaviors such as bribe-paying, money-laundering or other forms of aggressive (and illegal) behavior. Relative to previous research, we view our approach as more comprehensive, in the sense that we examine how real aggressiveness and reporting aggressiveness are related to each other and how each influences payoffs to shareholders (average monthly returns and abnormal returns) and to CEOs (raw and abnormal compensation) in a single comprehensive structural equations estimation.

We expect real aggressiveness and reporting aggressiveness will be positively correlated, but not that they necessarily affect payoffs to CEOs and shareholders similarly. In fact, prior research suggests real aggressiveness and reporting aggressiveness need not have *any* association with payoffs. It also suggests that results may be sensitive to the choice of proxies. For example, research examining the market consequences of excessive capital expenditures, acquisition behavior and external financing often

[11] Our sample contains 2.2% observations of changes in inventory method and 1.3% observations of changes in depreciation method.

[12] For example, accelerated depreciation would be informative that a firm operates its machinery and equipment close to or even above capacity. The FIFO inventory cost-flow assumption would be informative that (for example) management expects input prices will not increase much and might even decrease.

finds these behaviors are value destroying (Gervais, 2010), whereas research examining the consequences of R&D and advertising spending finds no association with subsequent returns, or sometimes a positive association (Chan, et al. 2001; Eberhart, et al. 2004). Similarly, results of analyses of the link between shareholder returns and aggressive reporting behavior are proxy-dependent. While research reports negative returns at announcements of adverse reporting outcomes such as restatements, studies that measure reporting aggressiveness with accruals sometimes find a positive relation between discretionary accruals and stock price.

Relative to previous research, we aim to create an *ex ante* observable proxy for aggressive reporting behavior that is available for a large sample of firms, is stable over time, is not based on the premise that managers make reporting decisions to obfuscate actual performance and is not derived from adverse reporting events that are extreme, unusual and not known *ex ante*.[13] We measure each aggressiveness construct as a latent variable derived from proxies linked in previous research to aggressive real behavior and aggressive reporting behavior. We are thus able to build on previous research that uses a variety of proxies and to speak to which of the proxies are most important in defining the construct and in influencing payoffs to shareholders and CEOs. We do not use discretionary accruals because they are measured with error, have a forced distribution determined by the regression procedure used to estimate normal accruals, are often viewed by researchers as a mechanism for disguising performance, and, by their nature, are expected to reverse over short horizons. Such a reversal confounds the interpretation that large positive accruals are more aggressive than large negative accruals or that aggressiveness is supposed to be a (relatively) persistent characteristic of firms or managers. Our focus on relatively stable accounting policy choices allows us to avoid measures of aggressiveness derived from returns and thereby allows us to test whether aggressive reporting behavior influences shareholder returns.

[13] In Section 5, we reconcile our broad-sample results with prior work on extreme outcomes and implicitly examine the overlap of our measure and the incidence of extreme reporting outcomes.

Thus, we do not consider inverse measures of conservatism, such as those used by Basu (1997) or Ball, Kothari and Robin (2000).[14]

We use a structural equations model (SEM) to estimate the latent aggressiveness variables, the correlation between the two latent variables and the paths between each of the two latent variables and payoffs. SEM is a powerful approach for detecting the sign, magnitude and significance of these associations, if they exist, because it aims to address the error in the input variables in capturing the underlying latent construct of interest, does not impose constraints on which variables are informative or how they are informative, and maximally uses information by simultaneously estimating the latent variables and the paths. This approach also sheds light on which types of real and reporting decisions have the greatest influence on payoffs.

We estimate the SEM given by equations (1)-(3) using maximum likelihood estimation:

$$Proxy\_RealAgg(k)_{i,t} = Intercept_k + r_k \times RealAgg_{i,t} + v_{k,i,t} \quad (1)$$

$$Proxy\_ReportAgg(j)_{i,t} = Intercept_j + a_j \times ReportAgg_{i,t} + u_{j,i,t} \quad (2)$$

$$Payoff_{i,t} = Intercept + p_1 \times RealAgg_{i,t} + p_2 \times RepAgg_{i,t} + \varepsilon_{i,t} \quad (3)$$

where *k* indexes the set of proxies for the latent variable for real aggression (*RealAgg*) and *j* indexes the proxies for the latent variable for reporting aggression (*ReportAgg*). Equations (1) and (2) describe the measurement components and Equation (3) is the structural part of the SEM. As described in Section 3, *Payoff* is measured as shareholder returns (average monthly return or monthly abnormal return) or CEO compensation (total compensation or industry-year-adjusted total compensation). [15]

In the estimation, one proxy for each latent variable (*RealAgg* and *ReportAgg*) is used as the anchor variable that fixes the scale of the latent variable. For *RealAgg*, the anchor variable is a measure

[14] Penman and Zhang (2002) estimate a measure of conservatism based on a balance sheet approach, which includes information about the firm's LIFO reserve and estimated R&D assets and estimated advertising assets. We do not use this measure because it blends aspects of reporting aggressiveness and real aggressiveness and therefore does not allow for a distinction between the two types of behavior.

[15] Our main results are based on the CEO's total compensation which often includes items linked to share price or share returns. Our inferences are similar when we measure the CEO's payoff as (industry-year-adjusted) salary only. Specifically, the path coefficient from *RealAgg* to (industry-year-adjusted) salary is -0.1143 (t=-7.70); the path coefficient from *ReportAgg* to salary is 0.2426 (t = 6.01); full results not tabulated.

of acquisition activity (*Acquisition*) and for *ReportAgg* the anchor variable is the choice of depreciation method (*DepMethod*). The coefficients on the anchor variables (r_1 and a_1) are set to 1 in the estimation. All other raw coefficient estimates ($r_{k>1}$, $a_{j>1}$, p_1, p_2) depend on the choice of anchor variable. To make the coefficients interpretable we report standardized coefficient estimates. For example, the standardized coefficient (p_1) is calculated as $p_1 = p_1^{raw} \times \frac{\sigma_{RealAgg}}{\sigma_{Payoff}}$, where p_1^{raw} is the raw coefficient estimate, $\sigma_{RealAgg}$ is the standard deviation of the latent *RealAgg* variable, and σ_{Payoff} is the standard deviation of the payoff variable. The standardized coefficients are directly comparable in magnitude; the choice of anchor variable has no influence on the signs and magnitudes of the standardized coefficients.

The SEM approach jointly estimates the two latent variables for real aggression and reporting aggression, as well as the associations between the latent variables themselves and between each latent variable and the payoff variables. This approach differs from a two-stage procedure that first uses factor analysis to independently or jointly identify common factors from a set of manifest variables and then tests for an association between those common factors and a payoff variable, because SEM uses *all* variables in a single estimation of the system of equations, trying to match the empirically observed covariances between variables with model-predicted covariances. Thus, compared to regression analysis, SEM uses a different objective function and simultaneously uses information in all variables.

3. Sample and Data

3.1 Sample description

Our main sample consists of fiscal years 1992 -2014.[16] To be included in the sample, a firm must have data on stock returns for at least a full year preceding and following its fiscal year end and data on lagged and current adjusted total assets, which we use to scale the continuous variables.[17] As shown in

[16] Coverage on the *AuditAnalytics* restatements database is sparse before 1992. The last fiscal years labeled as 2014 end in May 2015; to be able to measure future average and abnormal returns, the underlying monthly returns sample ends in August 2016. In tests on restatement announcements we require monthly returns for up to three years after the event, and therefore extend the database for these tests to the last available month, December 2016.

[17] Following Bowen et al. (1995), adjusted total assets equals total assets plus the LIFO reserve plus accumulated depreciation, depletion and amortization. If either the LIFO reserve or accumulated depreciation/amortization is

Table 1, Panel A, a total of 116,897 firm-years (14,242 firms) meet these criteria. Of these, we eliminate 55,783 firm-years (6,382 firms) with missing data on depreciation method or inventory cost-flow method. Although we do not explicitly eliminate any industries *per se*, these selection criteria eliminate certain industries systematically.[18] The resulting Returns Sample contains 61,114 firm years (7,860 firms); we use this sample to examine shareholder average or abnormal returns as the payoff variables. When we also require CEO compensation data on ExecuComp, the resulting Compensation Sample contains 21,558 firm-years (2,124 firms). We use this perfect subsample of the Returns Sample to examine CEO compensation as the payoff variable. Table 1, Panel B, shows both the Returns Sample and the Compensation Sample are fairly balanced over the sample period.

We report the distribution of observations for three additional subsamples of the Returns Sample. The 'Restatement-Affected Sample' selects observations that are subsequently announced to be subject to a restatement. A fiscal year is included if at least one subperiod (e.g., a quarter) within that fiscal year was affected. The 'Restatement Announcement Sample' includes fiscal years with a restatement announcement within the subsequent 12 months.[19] The 'Options Backdater Sample' includes firms that acknowledge having backdated employee stock options, spanning fiscal 2006 and 2007.[20]

Appendix A defines the manifest variables (proxies) used to estimate the two latent aggressiveness variables. All proxies are measured at the fiscal-year level. We assign a zero value to missing continuous data items underlying the real aggressiveness proxies, such as acquisitions or goodwill impairments, as these events may not occur every (fiscal) year. On the other hand, when data on the reporting aggressiveness proxies are missing, it may not be appropriate to assign zero to missing values for reporting aggressiveness proxies (depreciation method and inventory cost-flow method), as

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missing, we set the values of these variables to zero rather than delete the observation. Conceptually, the adjusted total assets measure is independent of the depreciation method and inventory cost-flow assumption.

[18] For example, few firms in the financial sector (SIC 60-69) have data on both *DepMethod* and *InvMethod* (514 firm-years in the Returns Sample). In addition, the proportion of firms in the business services sector (SIC 73) declines from about 9.1% to 5.0% when we require those data.

[19] In the tests of Table 7, these firm years correspond to the last year of the pre-announcement-event period.

[20] We thank Glass-Lewis for providing the data on option backdaters.

those choices have to be made every year. In these cases, we assume that the data are truly missing and delete the observations.

3.2 Manifest variable proxies for the latent variable estimation

Appendix A describes the manifest variables we use in the latent variable estimation. We consider up to nine proxies in the SEM estimation of the real aggression latent variable (*RealAgg*): acquisition value, additions to goodwill (a proxy for over-payment), goodwill impairment charge, proceeds of debt issuances, proceeds of equity issuances, capital expenditures, advertising expenditures, R&D expenditures and leverage. For the proxies included in the estimation of the reporting aggression latent variable (*ReportAgg*), we follow Bowen et al.'s (1995) scoring of incoming-increasing accounting policy choices. For depreciation method choice, *DepMethod* =1 if the firm uses straight line (most aggressive), 0 if the firm uses accelerated depreciation (least aggressive), and 0.5 if the firm uses a combination of the two. Table 2 shows that 51,742 (84.7%) firm-years in the Returns Sample report using straight-line depreciation, 965 (1.6%) accelerated, and 8,407 (13.8%) a combination of the two. For inventory flow method (*InvMethod*), FIFO is coded as 1 (most aggressive), LIFO as 0 (least aggressive); and average cost as 0.5. In the Returns Sample, 38,956 (63.7%) firm-years report under FIFO, 13.1% under LIFO and 23.1% report average cost.[21] In contrast to Bowen et al., we do not create an equal-weighted score from the sum of *DepMethod* and *InvMethod* in our main tests. Rather, we treat each variable as separate input into a latent variable for reporting aggressiveness, so that we can assess the relative impact of each accounting choice. That is, summing the two variables assumes each accounting choice has equal weight in measuring reporting aggression; we allow the properties of the data to determine the weights.

Appendix A also describes the returns and compensation payoff variables. We measure firm-specific returns over the 12-months following the fiscal year end (m=+4,+15), where m=0 indicates the month of the fiscal year end. For each interval, we calculate the average raw return, *AvgReturn*, and the

[21] The Compensation Sample has an identical distribution for *DepMethod*. For *InvMethod*, the Compensation Sample has a slightly higher percentage of LIFO observations (22.1%) and a slightly lower percentage of FIFO observations (57.4%).

(average) abnormal return, *AbnReturn*, equal to the intercept from firm-specific 3-factor regressions of monthly returns on the market risk premium, *SMB* and *HML*. We estimate the factor betas (and hence the abnormal returns) over the future estimation period; that is, we use the forward-looking factor returns and factor betas to determine the abnormal return and do not restrict the expected return to be available in *m*=0, e.g., through estimating it over some prior period, as we do with accounting and other information. This should result in conservative estimates of abnormal returns. *AvgReturn* and *AbnReturn* are comparable, as monthly return values, except that *AvgReturn* does not control for risk factors and *AbnReturn* does. If aggressive behavior manifests in greater systematic risk as reflected in the 3-factor model, we expect that our aggressiveness constructs will be more strongly associated with *AvgReturn* than with *AbnReturn.*

CEO compensation data are from ExecuComp, which collects information from annual proxy statements (Form DEF14A). We calculate total compensation (*TotalComp*) as the sum of salary, cash bonuses, other annual and deferred compensation, long-term incentive plan payouts, restricted stock grants and stock options grants, scaled by adjusted total assets and multiplied by 100. We measure abnormal total compensation (*AbnTotalComp*) by subtracting from each firm-specific measure the mean value of *TotalComp* for members of that firm's 2-digit SIC industry for the same fiscal year.

Descriptive statistics on the reporting aggressiveness proxies, real aggressiveness proxies and payoff variables are reported in Table 2. Proxies for each aggressiveness construct are in general not markedly different across the Returns and Compensation samples. For example, average capital expenditures are about 4.5% of adjusted total assets for both samples and both samples show roughly similar acquisition activity and external debt financing. Stock issuances for the (on average) larger firms of the Compensation Sample are negative, indicating net stock repurchases. The table also describes the data on the variable for reporting aggressiveness. For both samples, roughly 85% of firms select straight-line depreciation; more Returns Sample firms than Compensation Sample firms choose FIFO (63.7% versus 57.4%). The mean future average (abnormal) return is about 1.28% (0.42%) per month. The CEO payoff variables *TotalComp* (*AbnTotalComp*) are about 0.31% (-0.03%) of adjusted total assets.

3.3 Validation of *DepMethod* and *InvMethod* as measures of aggressive reporting

We validate *DepMethod* and *InvMethod*, the inputs to characterizing a firm's financial reporting aggressiveness, in two ways. First, and in line with prior research, we assume aggressive reporting choices lead to higher *levels* of income. If FIFO is a more aggressive choice than LIFO, then we should observe that FIFO cost of goods sold is less than LIFO cost of goods sold over our 23-year sample period characterized by rising input prices. We compute pro-forma COGS(FIFO) for LIFO firms as (reported) COGS(LIFO) less the change in the LIFO reserve. As expected, for the average LIFO firm, pro-forma COGS(FIFO) are $907,541 lower than the reported COGS (LIFO) (firm-cluster-robust t=5.93). We conclude that the average LIFO firm in our sample reports systematically lower income than under the alternative FIFO choice and interpret LIFO as *un*aggressive reporting.

To verify that the choice of straight-line depreciation has an income-increasing effect, we take a different approach, because a pro-forma calculation for the same firm under the alternative accounting choice, analogous to the FIFO-based COGS above is not feasible. Specifically, we contrast the rate of depreciation, computed as depreciation expense scaled by gross property, plant and equipment of firms with straight-line and accelerated depreciation across firms. Using a regression with firm-fixed effects, we find the straight-line depreciation rate is 2.18 percentage points lower than the accelerated depreciation rate (t=−3.56). We conclude that the average accelerated-depreciation firm in our sample reports systematically lower income than under the alternative straight-line depreciation choice, and interpret accelerated depreciation as *un*aggressive reporting.

The second set of validation tests focuses on the time-series *variability* of income statement line items and income as the aggregate, to the extent these are affected by both accounting policy choices. These tests rest on the premise that greater reporting aggressiveness is associated with both a higher level of income and greater variability on the income statement. We measure the accounting-choice-based variability of income statement line items, scaled by a balance sheet item directly related to, but unaffected by, the accounting choice itself. For the depreciation method choice, we calculate the standard deviation of depreciation expense divided by the gross value of property, plant and equipment; for inventory method choice, we calculate the standard deviation of cost of goods sold divided by the sum of inventory and the LIFO reserve. Standard deviations are calculated using rolling five-year windows:[22]

$$\sigma(Depreciation)_{i,t} = \sigma\left(\frac{Depreciation}{Gross\ PP\&E}\right)_{i,[\ t-4;t]}$$

$$\sigma(COGS)_{i,t} = \sigma\left(\frac{COGS}{Inventory+LIFO\ Reserve}\right)_{i,[\ t-4;t]}$$

Our tests regress $\sigma(Depreciation)_{i,t}$ on *DepMethod*$_t$, and, separately, regress $\sigma(COGS)_{i,t}$ on *InvMethod*$_t$. If more aggressive reporting choices lead to greater variability, we expect to observe positive slope coefficients in these regressions. The results, reported in the columns labeled "Unadjusted Coefficients" in Table 3, show the coefficients are significantly positive (t-statistics of 4.70 and 10.77), implying that larger values of *DepMethod* and *InvMethod* are associated with greater variability in these directly-affected income statement line items.

We also re-estimate the regressions replacing *DepMethod* and *InvMethod* with standardized measures of each accounting choice, calculated by subtracting the mean of each variable across the sample and scaling by the sample standard deviation. The regression coefficients on the standardized accounting method choice variables are now directly comparable to each other. The resulting coefficient estimates, shown in the Table 3 columns labeled "Standardized Coefficients", indicate the choice of inventory method has about twice the impact as the choice of depreciation method (0.0744 versus 0.0368), and both are reliably different from zero at the 0.001 level.

The far right columns of Table 3 show the effect of accounting choice on overall income statement variability; here, the dependent variable is the time-series standard deviation of income before extraordinary items scaled by adjusted total assets, representing the summary variable affected by both accounting choices.

$$\sigma(Income)_{i,t} = \sigma\left(\frac{Income\ before\ extraordinary\ items}{Adjusted\ Total\ Assets}\right)_{i,[\ t-4;t]}$$

[22] This data requirement reduces the sample slightly; see Table 3 for details.

$$= \sigma \left(\frac{Income\ before\ extraordinary\ items}{Total\ Assets + Acc.\ Depreciation + LIFO\ Reserve} \right)_{i,[\ t-4;t]}$$

Results based on the unadjusted accounting choice variables show that accounting choice variables contribute significantly to the standard deviation of profitability (t-statistics of 2.76 for *DepMethod* and 28.03 for *InvMethod*). The standardized coefficients are also significantly (at the 0.01 level) positive and further indicate that more aggressive inventory choices have a substantially greater effect on the variability of profitability as compared to more aggressive depreciation choices (0.1823 versus 0.0226).

Overall, we view these tests as indicating that, despite the seemingly innocuous nature of depreciation and inventory method choices, they have statistically reliable and economically meaningful effects on the level and variability of income statement line items that are directionally consistent with more aggressive choices leading to both higher levels of income and higher variability of income.

4. SEM Tests and Results

The results of estimating Equations (1)-(2), the measurement models, and Equation (3), the structural model, are shown in Tables 4 and 5. The latent variables for each aggressiveness construct are estimated simultaneously with the association of each latent variable with each payoff variable. Table 4 reports the coefficient estimates and t-statistics for each proxy used in the latent variable construction from the SEM, using two models for *RealAgg*. Results in Panel A are based on *Payoff=AbnReturn(+4;+15)* for the Returns Sample, and results in panel B are based on *Payoff=AbnTotalComp* for the Compensation Sample; other payoff variables (unadjusted returns and unadjusted total compensation) yield similar results and are not tabulated.

In the estimation that includes all nine *RealAgg* proxies (columns labeled "Full Model), we find that all nine are significant at the 0.01 level or better in defining the latent variable. The most significant proxies, with t-statistics exceeding 15, are related to acquisition activity, amount of goodwill (a proxy for overpayment), debt issuances and leverage. Goodwill impairments, R&D and advertising spending and stock issuances appear with negative signs, indicating less aggressive behavior. The coefficient estimates for both accounting policy choices are positive and significant (t-statistics approximately 2.7), indicating that income-increasing and income-variability-increasing inventory method choices and depreciation choices contribute to more reporting aggressiveness, as captured by larger values of the latent variable for reporting aggressiveness, *ReportAgg*.

With regard to the CEO payoff variables (Panel B), results for acquisition activity, amount of goodwill (a proxy for overpayment), debt issuances are similar to those shown in Panel A. The significance of leverage drops considerably, but the coefficient remains significant at the 0.01 level. In contrast, the significance level of the Capex (capital expenditures) coefficient drops to the 0.10 level, while R&D and stock issuances no longer significantly contribute to the empirical definition of *RealAgg*.

To determine how well this model characterizes the data, we calculate two measures of goodness of fit. The first is the standardized root mean squared residual (SRMR) equal to the square root of the average squared deviations between the empirical correlations and the model-predicted correlations across all manifest proxies. Smaller values of SRMR indicate better model fit. The second is the root mean squared error of approximation (RMSEA) which adheres to benchmark distributions, and therefore allows for a statistical test of goodness of fit. Conceptually, RMSEA is based on the likelihood ratio test that compares the tested model with a fully saturated model; smaller numbers for RMSEA indicate better model fit. Wang and Wang (2012) propose that a SEM model with a close fit should have an (asymmetric) 90% confidence interval around the RMSEA that does not include the benchmark value of 5%. SRMR and RMSEA values are reported in Table 4. For both abnormal returns (Panel A) and abnormal compensation (Panel B), the evidence shows SRMR values of about 0.06-0.07 and RMSEA values of about 0.08-0.09. These values of RMSEA are unable to reject the hypothesis that the 90% confidence interval does not contain 0.05. We conclude from both measures that the full model does *not* fit the data well.

Given the positive sign and the high and consistent significance of the coefficients on acquisition activity, debt issuance and goodwill in defining the latent variable for *RealAgg*, we repeat the tests in Table 4 using only these three manifest proxies for real aggressiveness. Results, reported in the column labeled "Standard Model," show that the significance of all three manifest proxies for *RealAgg* increases.

In Panel A, the t-statistics on the coefficients on the *DepMethod* and *InvMethod* in the estimation of *ReportAgg* also increase to around 10-11. We interpret these increases as indicative of better model fit, and less noise in the latent variables. More directly, both measures of goodness of fit are much smaller, with tests of RMSEA now rejecting the hypothesis that the 90% confidence interval includes 0.05. These results are interesting in their own right, in that they highlight that more is not always better: in our research setting, adding more proxies for a given construct of interest may not yield a better model fit and can introduce noise.[23]

We also estimate a model that bases the SEM on industry-year-adjusted manifest input variables. This design is motivated by the overconfidence literature, where some researchers measure firm-specific overconfident behavior relative to an industry benchmark (i.e., over-investing firms invest more than other firms in their industries). This adjustment transforms the accounting choice variables into deviations from industry means, so they are no longer categorical. Results are reported in the final set of columns in Table 4, labeled "Standard Model, industry-year-adjusted." The t-statistics of the *ReportAgg* variables drop by approximately 50%, but the 0.01 significance level for all coefficients remains and coefficient estimates remain similar. In addition, this effective introduction of industry and year controls yields a qualitatively comparable model fit to the unadjusted Standard Model (slight increase in Panel A; modest effect in Panel B). Given its significantly better fit compared to the Full Model, we use the Standard Model in our main tests.

Table 5, Panel A (Panel B), reports the results of the structural part of the SEM (Eq. 3) using the unadjusted (industry-year-adjusted) input variables; specifically, the path coefficients between *RealAgg* and *ReportAgg* to the payoff variable (p_1 and p_2) as well as the correlations between the latent variables

[23] We also considered a broader set of seven proxies for reporting aggressiveness, taken from Dichev and Li (2013). In addition to inventory method and depreciation method, the expanded set includes average depreciable life of property, present value of operating lease obligations and three defined-benefit pension variables (rate of compensation increase, expected return on plan assets and discount rate). This expansion reduces the sample markedly, limiting the sample to firms with defined benefit pension plans, which in itself reflects a choice. In our case, the Returns Sample (Compensation Sample) decreases to 12,810 (7,658) firm-years. Second, and empirically, the model fit of this 'expanded model' is comparable to the poor fit of the "Full Model" in Table 4. Specifically, the SRMR on the Returns Sample is slightly higher (worse); for the Compensation Sample, the SRMR is slightly lower, but qualitatively similar.

RealAgg and *ReportAgg* (ρ). We also report the magnitude and significance of p_2 - p_1, as a formal test of whether real aggressiveness and reporting aggressiveness have different associations with four measures of payoffs. Adopting a shareholder perspective, we examine average and abnormal return over Month+4 to Month+15 after the fiscal year end, denoted *AvgReturn* (+4;+15) and *AbnReturn* (+4;+15), respectively. CEO payoff is captured as raw and industry-year-adjusted total CEO compensation (*TotalComp* and *AbnTotalComp*, respectively).

There are three key findings. First, and as reported in the right-most column, ρ is positive and significant at the 0.01 level or better in both payoff specifications, indicating that *RealAgg* and *ReportAgg* are positively correlated at approximately 0.16-0.18 for the unadjusted proxies (Panel A) and at approximately 0.07-0.10 for the industry-year-adjusted proxies (Panel B). This positive correlation is consistent with the view that real aggressiveness and reporting aggressiveness are manifestations of a single (unobservable) firm or manager characteristic.

Second, p_1 on *RealAgg* is, with one exception, significantly negative, at the 0.05 level or better. The exception is the industry-year-adjusted specification with *AbnTotalComp* as payoff; the t-statistic of -1.27 indicates significance at approximately the 0.20 level. Third, p_2 on *ReportAgg* is consistently positive for both payoff variables and consistently significant at the 0.01 level or better. Taken together, these results indicate that shareholder returns measures and CEO compensation measures are *negatively* associated with *RealAgg* and *positively* associated with *ReportAgg*. The difference between the two associations, p_2 - p_1, is reliably positive for all payoff measures at the 0.01 level (Wald-type test statistics, not tabulated). Results also indicate the associations between *RealAgg* and *AbnReturn* (p_1) are generally smaller in magnitude than for the raw *AvgReturn*. We interpret these differences as suggesting that a component of aggressive real behavior is captured by the (future) factor betas in the 3-factor model; that is, it manifests in the firm-specific coefficients on the market risk premium, SMB and HML.

We conduct two additional analyses to determine the sensitivity of the Table 5 results to research design choices. First, we apply an alternative statistical technique for evaluating the significance of accounting choices. Our main tests use the inventory policy choice and the depreciation method choice as defined and coded by Bowen et al. (1995). This approach assumes both the coded values and the distances between values are meaningful. While we believe the values capture an ordinal ranking of reporting aggressiveness (1>0.5>0), the values themselves are *ad hoc* and the distances between values are not interpretable. Furthermore, a difference of 0.5 between adjacent levels of *InvMethod* and *DepMethod* may not be directly comparable. To address these concerns, we re-estimate our standard SEM models using a weighted-least-squares estimation in Mplus that treats these choice variables as ordinal categorical variables (1>0.5>0), but does not use the actual values or the distance between the values.[24] Results from the weighted-least-squares estimation (not tabulated) are generally similar to those reported: Estimates of p_2 are larger in magnitude and equally significant (t-statistics range from 6.12 to 11.21); the path coefficients from *RealAgg* to the payoffs (p_1) are similar in magnitude and significance levels remain the same for the payoff variables *AvgReturn*, *AbnReturn* and *AbnTotalComp* . For *TotalComp*, however, the estimate of p_1 declines to −0.039 and is no longer significant at conventional levels (t = −1.56; p = 0.12).

Second, we augment the structural part of the compensation SEM (Eq. 3) with four control variables: end-of-year market capitalization, book-to-market ratio, CEO age and a CEO gender indicator (results not tabulated). Requiring CEO age data reduces the sample size from 21,558 to 19,580 firm-year observations (from 2,124 to 2,073 distinct firms). As expected, the presence of these control variables reduces the magnitudes of the p_1 and p_2 estimates. The estimates of p_2 which links reporting aggressiveness to payoffs retain their signs and significance levels of at least 0.01 in all four specifications. The coefficient from *RealAgg* on *TotalComp*, p_1, is significant at the 0.10 level; qualitative results and significance levels on *AbnTotalComp* remain unchanged. Importantly, in all cases, the effect

[24] While the weighted least squares approach with categorical input variables is more appealing from a theoretical perspective, we do not report results based on this approach as our main results because the estimation requires additional distributional assumptions which might not be met in our application (Byrne, 2012). In addition, the actual bias in path coefficient estimates when treating ordinal variables as continuous is likely to be small in many applications. The insensitivity of our results to the treatment of ordinal choice variables lends support to this assertion for our data and models.

of including these control variables on the difference between p_1 and p_2 and on the correlation between *RealAgg* and *ReportAgg* is small; all estimates remain significant at the 0.01 level or better.

In summary, we find shareholders and CEOs benefit from reporting aggressiveness in the sense of receiving larger payoffs, but neither stakeholder group benefits from real aggressiveness. The finding that real-action aggressiveness is not rewarded is roughly consistent with previous research that suggests a little, but not too much, overconfidence helps alleviate a natural human tendency to risk aversion but it is easy to overstep. The finding that both CEOs and shareholders benefit from a certain amount of reporting aggressiveness might appear to be inconsistent with prior research showing significant *negative* reactions to announcements of unusual and beyond-GAAP aggressive reporting, such as restatements. However, and as previously noted, our measure of aggressive reporting is intentionally within-GAAP while restatements are, by definition, corrections of material non-GAAP reporting. We explore the relation between our findings in Table 5 and the results from previous research on restatements in the next section.

5. Analysis of Negative Reporting Events

5.1. <u>Likelihood of negative reporting events</u>

We first investigate the association between the latent variable, *ReportAgg*, and the incidence of unusual and extreme aggressive reporting as captured by restatements. Of the 61,114 firm-years in the Returns Sample, 6,343 observations reflect firm-years with GAAP violations that led to 1,562 publicly announced restatement events over the period March 1998 to December 2016. We obtain data on the restatement announcements (to be used in later tests) and data on the fiscal years affected by those restatements from the *AuditAnalytics* database.[25]

In the test of this subsection, we are interested in whether the probability a fiscal year is affected by a restatement is linked to *RealAgg* or *ReportAgg*. As shown in Table 1, Panel B, the 'Restatement-Affected' Sample observations are not balanced across years: there are fewer than 100 restatements per

[25] For a fiscal year to be marked as affected, it is sufficient that one quarter of the fiscal year was subject to a restatement. We eliminate duplicate observations by retaining firm-years affected by multiple distinct restatement announcements only once.

year prior to 1996, over 450 per year during 2001 to 2005, and 274 per year on average following 2005. For the first test, we replace Equation (3) with a logit model that uses an indicator variable for restatement events as the *Payoff* variable: *Restatement*=1 if the firm-year is affected by a restatement and 0 otherwise. Because we expect that more aggressive reporting firms are more likely to experience restatements, we predict that the path coefficient on *ReportAgg* is positive, $p_2 > 0$. We have no prediction about the association between *RealAgg* and the likelihood of being subject to a restatement, p_1, beyond an expectation that real-action aggressiveness would be less strongly linked to restatement than reporting aggressiveness.[26] We continue to predict that *ReportAgg* and *RealAgg* are positively correlated ($\rho > 0$). Table 6, Panel A, shows that *ReportAgg* and *RealAgg* continue to be positively correlated ($\rho > 0$, t=8.15 for unadjusted proxies, t=5.43 for industry-adjusted proxies, with correlation coefficients of approximately 0.18 and 0.11, respectively). As expected, *ReportAgg* is positively associated with restatement likelihood with t-statistics of 4.09 (unadjusted) and 5.23 (industry-year-adjusted); *RealAgg* is not significant at conventional levels in explaining the likelihood of restatement (t-statistics of 1.18 and −0.23, respectively).

In an extension, we explore an alternative adverse outcome – stock option backdating – which combines both real aggression (issuing in-the-money call options to employees) and reporting aggression (accounting for the options as if they are at-the-money and not revealing this). The sample here is limited to fiscal years 2006 and 2007 and includes 4,657 firm-years (2,552 distinct firms). Of these, 105 firms publicly announced they would perform an internal investigation or be subject to an external investigation by the SEC or the Department of Justice as to whether they engaged in stock option backdating. Results in Table 6, Panel B, show significant (at the 0.05 level or better) path coefficients for *ReportAgg* (the unadjusted coefficient is 0.4550, t=4.25; the industry-adjusted coefficient is 0.1140, t=1.98). We continue to find positive correlations between *RealAgg* and *ReportAgg* using unadjusted manifest variables, but, in

[26] On the one hand, real aggressiveness, for example, a high level of acquisition activity, might increase the chance of accounting mistakes, implying a positive association between *RealAgg* and *Restatement*. On the other hand, there is often increased scrutiny around these discrete transactions, which might lead to a decreased likelihood of a restatement.

the specification with industry-year-adjusted input variables, the t-statistic declines to 1.37, significant at approximately the 0.17 level. We do not find an association between *RealAgg* and option backdating: p_1 is insignificant at conventional levels in both specifications (t = -0.92 and t = -1.10, respectively).

We believe the weight of the evidence in Table 6 suggests that the likelihood of extreme accounting outcomes (announcements of restatements and option backdating) is increasing in reporting aggressiveness as proxied by *ReportAgg,* but not necessarily increasing in real-action aggressiveness. We view this finding as corroborating the validity of the latent variable *ReportAgg* in capturing reporting aggressiveness.

5.2. Long-term versus short-term returns of restatement firms

Our final analyses examine shareholder returns preceding and following restatement *announcements*. Specifically, we test for incremental returns effects of both forms of aggressiveness, before and after the announcement. In our first test, we interact indicator variables for pre- and post-restatement periods with proxies for *RealAgg* and *ReportAgg*. To calculate these proxies, we estimate only the measurement model of the SEM (Equations 1 and 2) and extract the factors that are the latent variables for *RealAgg* and *ReportAgg*. We then regress abnormal returns on pre- and post-restatement indicators, latent variables and interaction terms:

$$AbnReturn\ (+4,+15)_{i,t} = a_0 + a_1\, Pre_{i,t} + a_2\, Post_{i,t}$$
$$+\, a_3\, RealAgg_{i,t} + a_4\, Pre_{i,t} \times RealAgg_{i,t} + a_5\, Post_{i,t} \times RealAgg_{i,t}$$
$$+ a_6\, ReportAgg_{i,t} + a_7\, Pre_{i,t} \times ReportAgg_{i,t} + a_8\, Post_{i,t} \times ReportAgg_{i,t} + \varepsilon_{i,t} \qquad (4)$$

where $Pre_{i,t}$ = 1 for all firm-years preceding the year in which firm *i* announced a restatement and zero otherwise; $Post_{i,t}$ = 1 for all firm-years in which firm *i* announced a restatement and all years after, and zero otherwise. Thus, the *Pre* and *Post* variables turn on/off for the subset of the Returns Sample with restatement announcements and are set to zero for all non-restatement observations. *Pre* and *Post* are defined relative to all restatement announcements; 14,346 fiscal years are pre-event firm years (*Pre*=1), 7,945 are post-event firm years (*Post*=1). To be conservative in our estimates of any post-event effects, we consider only those years before the first announcement pre-event and include any subsequent restatement announcements in the post-event period.

Table 7 reports the results of estimating regression (4). The first results, based on all restatement announcements, show that consistent with the results in Table 5, abnormal returns are positively associated with *ReportAgg* and negatively associated with *RealAgg* for the average sample firm, even when controlling for restatement firms in the interaction terms. As expected, restatement firms are characterized by negative abnormal returns after the restatement (t=−2.00), but, perhaps unexpectedly, restatement firms experience *positive* pre-event abnormal returns, exceeding abnormal returns to non-restatement firms by 0.49%, t-statistic = 7.34. The difference between the pre- and post-event coefficients, 0.62%, is highly significant (t=7.51). There is no discernible incremental effect of *RealAgg* prior to the restatement, but *RealAgg* is positively associated with post-event abnormal returns (t=2.64). That is, the negative main effect of restatements is mitigated for firms with higher *RealAgg.*

The incremental effect of aggressive reporting is quite different. The abnormal return in the pre-restatement period is increasing in *ReportAgg* (as evidenced by the positive coefficient on the interaction of *Pre* and *ReportAgg;* t=1.97, significant at the 0.05 level); the negative post-event returns are decreasing in *ReportAgg* (as evidenced by the negative coefficient on the interaction of *Post* and *ReportAgg* (t=−1.71, significant at the 0.10 level). The difference in this incremental effect of *ReportAgg* is significant at the 0.01 level, with a t-statistic of 2.89. Overall, this pattern is consistent with restatement firms having positive pre-event returns and negative post-event returns, and with high-reporting-aggressiveness restatement firms having a similar and more pronounced abnormal returns pattern around the announcement event.

We also examine two subsets of restatements, fraudulent restatements and clerical errors, effectively conditioning our tests on the nature of the GAAP violation, severe in the sense of intentional violations or not-severe in the sense of inadvertent violations. It is possible that high reporting aggressiveness coincides with severe restatement cases and that, once the sample consists only of these severe cases, the effects of *ReportAgg* are captured by the main effects and the incremental effects of *ReportAgg* disappear.

The subset of fraudulent restatements contains 1,301 (1,830) pre-event (post-event) observations. Despite the reduction in sample size, we find larger (not smaller) coefficients with consistent signs on both interaction terms with *ReportAgg*. The difference between these coefficients is much larger than the difference for the restatement sample as a whole (0.0437 for the fraudulent restatements and 0.0123 for the restatement sample as a whole) and continues to be significant at the 0.01 level (t=3.01). The coefficient difference on the *RealAgg* interaction terms is no longer significant at conventional levels (t=−1.35, significant at approximately the 0.18 level). Finally, analysis of the less-severe restatement cases due to clerical errors, containing 751 and 497 pre- and post-restatement observations, yields no reliably non-zero coefficient on any interaction term.[27]

We next analyze the net or overall shareholder returns to restatement firms to provide evidence on whether the positive pre-restatement returns are entirely reversed by the negative market reactions at and/or after the restatement announcements. This analysis provides evidence on the *net* benefits of aggressive reporting by focusing on extreme instances when the aggressive behavior results in a GAAP violation. Put another way, over long holding periods, do shareholders of aggressive-reporting restating firms gain more in returns preceding the restatements than they lose at or after the restatement is announced? We begin by examining all restatements which, by definition, can be identified only after the fact, meaning after the affected fiscal periods, and then examine whether high *ReportAgg*, which is assessable ex ante, has any incremental effect, conditioning on a subsequently-revealed GAAP violation.

[27] We repeat these subsample tests after eliminating all other types of restatements from the set of benchmark firms (reducing the sample). Results for the fraudulent restatements are hardly affected (results not tabulated). For the clerical errors, coefficient estimates on the interaction terms with *ReportAgg* are slightly larger and still insignificant at conventional levels. The pre-event main effect increases from 0.0034 to 0.0043 and is significant at the 0.10 level (t=1.88).

To assess the long-term net abnormal returns associated with aggressive reporting, even if the firm has a negative reporting event (i.e., restates with probability = 100%)[28], we examine the shareholder returns of restating firms over periods prior to and after restatements. These tests use information that cannot be known at the (hypothetical) time of investment, so they do not constitute an implementable trading strategy; a non-insider cannot select (future) restatement firms. We view this analysis as providing descriptive information about the returns earned by firms even if they are identified *ex post* as having made extreme and unusual aggressive reporting decisions.

Our first analysis examines cumulative abnormal returns over researcher-defined intervals before, at and after the restatement announcement. That is, we mimic a hypothetical long-term investor who is agnostic about the fiscal periods for which it is later revealed that financial reports must be restated, meaning that the holding period in these tests is always a mix of affected and unaffected fiscal periods. Our second analysis focuses only on the cumulative returns over the actual reporting periods that are *ex post* identified as affected by the restatement, net of the returns at the restatement announcement itself.

<u>Researcher-defined returns intervals</u>: We use calendar-time-portfolio regressions to examine the monthly abnormal returns, relative to the 3-factor model, to equal-weighted portfolios of firms disclosing restatements *m* months in the future and *n* months after the restatement announcement. These calendar-time tests retrospectively calculate the abnormal returns to a shareholder who invested in the portfolio of firms that will announce a restatement in *m* months.

We construct a monthly returns data base for 4,221 restatement-announcing firms,[29] between January 1992 and December 2016, requiring only monthly CRSP returns for the month of the first

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[28] Recall that, in the overall Returns Sample, unconditional on any later adverse event, higher reporting aggressiveness is associated with higher future abnormal returns (see Table 5).

[29] Those firms made 6,938 restatement announcements. The sample is larger than the announcement sample in the fiscal-annual database because of the much lower data requirements. In particular, for the tests on all restatements and the fraudulent restatements, we do not require data on *DepMethod* and *InvMethod*, but use these data only to separate out the High-*ReportAgg* subsamples. To be conservative, we do not eliminate the announcement returns of subsequent restatements (after the first), which, in expectation, yields lower estimates of future abnormal returns.

restatement (Month 0).[30] The inclusion of a given firm in the calendar-time portfolio is determined relative to Month 0. We require at least 10 firms each month to create a portfolio and to include that month in the final regression sample of monthly portfolio returns. We perform the calendar-time-portfolio tests on four samples: (i) All Restatements; (ii) Restatements with the highest values of *ReportAgg* (i.e., firms that are recorded on Compustat as using only FIFO for their inventories and only straight-line depreciation for their depreciable assets); (iii) Restatements that are fraudulent; and (iv) Restatements that are both fraudulent and have the highest values of *ReportAgg*. We are interested in the effects of both restatements and the latent variable *ReportAgg* variable as indicators of high reporting aggressiveness. To the extent aggressive behavior is not fully captured by a restatement or even a fraudulent restatement, we expect to see increasing abnormal returns for high-*ReportAgg* firms.

For each of the four samples, Table 8 shows the average monthly abnormal return for 10 estimation intervals from 72 months prior to 36 months after the restatement announcement (i.e., the intercept from 10 window-specific 3-factor models).[31] For the 'All Restatements' sample, we document a total cumulative abnormal return of 21.96% from Month −72 to Month −3 (three months prior to the restatement); this compares to 40.53% for the subset of restatements with high values of *ReportAgg*, 23.27% for fraudulent restatements, and 68.01% for fraudulent restatements with high values of *ReportAgg*. Consistent with prior research, we find reliably negative returns around restatement announcements, averaging −1.87% per month over the 5-month interval centered on the announcement for the 'All Restatements' sample (t=−7.20). The 5-month returns are −2.04% (t= -6.25) per month for restatements with high values of *ReportAgg*, −2.95% (t= -5.48) per month for fraudulent restatements and −3.25% (t= -3.54) per month for fraudulent restatements with high values of *ReportAgg*; all results are significant at the 0.01 level). If we extend the holding period up to Month +36, we find no reliably non-

[30] This requirement is, strictly speaking, not necessary. We aim to be conservative, however, by including the expected negative restatement-announcement return for all sample firms.

[31] For brevity, we do not report coefficient estimates and t-statistics for the three factors; market betas are highly significant and generally exceed 1, with very few exceptions. Also as expected, SMB betas are positive and highly significant due to the average small size of restatement firms. HML betas are much less consistent; being positive (negative) [insignificant] at the 0.10 level in 11 (4) [25] of the 40 regressions. The average adjusted R-square from the estimations, across all windows and subsamples, is about 68.4%, ranging from 46% to 84%.

zero monthly abnormal return for any of the four samples (i.e., none of the intercepts is significant at conventional levels).

Table 8 also shows the *cumulative* abnormal return including the announcement event and ending two months after the restatement announcement. These findings indicate that shareholders who invest early in restating firms (for example, 72 months before the restatement), even when holding on to these shares through two months after the restatement, earn positive abnormal returns: 11.0% for All Restatements; 26.75% for restatements with high *ReportAgg* scores; 6.15% for fraudulent restatements; and 42.40% for fraudulent restatements that also have high *ReportAgg* scores.

The cumulative returns calculations reported in Table 8 are based on a hypothetical investment 72 months (6 years) before the restatement announcement. Taking a different perspective, the interval-specific intercept estimates also allow us to approximate the minimum holding period for a hypothetical investor to break even: The monthly average event-period abnormal return of −1.87% for 'All Restatements' translates into a cumulative abnormal return of −8.99% over the five months that end two months after the restatement announcement. Thus, investors would have to hold shares for at least 49 months to earn a cumulative abnormal return of (close to) zero. For restatement firms with high *ReportAgg*, fraudulent restatements, and fraudulent restatements with high *ReportAgg*, the hypothetical investment holding periods to break even would be about 36, 67 and 38 months, respectively[32]

<u>Actual restatement-affected fiscal period returns:</u> The calendar-time portfolio tests are agnostic about the actual (and only *ex-post* known) periods affected by restatements. Table 9 reports results of a different test on the same monthly database that uses the *ex-post* information about restatement-affected periods and directly contrasts returns in those periods with the restatement announcement returns. Information about restatement periods is taken from the *AuditAnalytics* database; we define the restatement period as the period starting with the first month of the earliest restated period and ending with the last month of the last restated period (irrespective of when the restatement was announced). This

[32] This approximation assumes that intercepts in the different intervals are applicable to each event-month of the event time intervals.

test requires returns during the restatement-affected period(s) as well as around the restatement announcement for the same firm, reducing the sample to 6,719 restatements for 4,187 distinct firms. The average restatement firm announces 1.60 restatements; combined, these affect an average of about 32.9 months, approximately 11 quarters. We aggregate these data to the firm level by calculating the buy-and-hold return over all restated periods. Each monthly return can enter the calculation only once, even if affected by multiple restatements or multiple restatement announcements.

For all restatement firms, Table 9 reports an average buy-and-hold return of 44.78% (t=7.69) over the fiscal periods whose results were ultimately restated. The market reaction to the restatement appears to be concentrated in the month of the announcement, where we find an average return of −2.61% (t=−4.35); wider windows show negative returns of smaller magnitudes. "Cumulative return" refers to the mean firm-specific cumulative return over both the restatement period and three announcement windows of different lengths.[33] For all restatement firms, this cumulative return is about 39.79% at a minimum (t = 10.29) and about 40.86% on average across the three announcement windows (average result not tabulated). That is, the market's penalty at the announcement is smaller than the stock return that accrued over the restated fiscal periods. We expect to see more extreme results for the subset of 2,539 restatements with high reporting aggressiveness. Despite the similar length of the restated period, the return is in fact slightly higher, at 48.93% (t= 6.86). The announcement returns are lower, however, so that the net return is only modestly higher, at a minimum of 40.77% (t=8.79).

As in the preceding calendar-time-portfolio tests, we extend our analysis to examine the interaction between our within-GAAP metric of reporting aggressiveness and the incidence of a fraud, i.e., intentional GAAP violations. Table 9 shows a buy-and-hold return of 52.84% (t=4.69) over a comparable length of (fiscal) time compared to the whole sample of restatement firms. The negative reaction to a fraud announcement is −7.42% (t= -5.88) in the announcement month and −10.28% (t= -5.57) for the 3-month window centered on the announcement; both results are significant at the 0.01 level. The cumulative return for this sample is 34.62% (t=3.22) using the most conservative range. In short,

[33] Note that a constant expected return would not affect these results.

conditional on *ex post* fraudulent reporting behavior, the net shareholder return is considerably lower than for firms who report aggressively but remain within-GAAP for financial reporting. The subsample of fraud cases with high *ReportAgg* contains 370 firms and shows the highest restatement-period returns of 67.61% (t= 4.32), while announcement returns are in line with those for the overall fraud sample.

The findings in Tables 7-9 combined suggest that even extremely aggressive reporting firms that violate GAAP and must restate their financial reports earn significantly positive *net* abnormal returns for long-term investors, after including the market's negative reaction to the restatement. Further, the net positive abnormal return of calendar-time portfolios is increasing in aggressiveness of the within-GAAP accounting choices; fraudulent application of GAAP does not explain this result.

6. Summary and Conclusions

We propose and estimate a comprehensive structural equations model that allows for (1) associations between a latent variable for real aggressiveness developed from observable measures of investment and financing decisions and a latent variable for reporting aggressiveness developed from observable income-increasing accounting policy choices and (2) associations between both latent variables and payoffs to two key stakeholder groups: shareholders and CEOs. We find that real aggressiveness and reporting aggressiveness are positively correlated, consistent with the perspective that they are manifestations of a common CEO or corporate characteristic.

Reasoning from results in prior research, we consider the possibility that aggressive behaviors in both the real-action investment/financing and the financial-reporting contexts might be neutral or beneficial with regard to outcomes in the form of payoffs to shareholders and managers. Our reasoning, based on previous research in financial theory, is that a certain degree of decision-aggressiveness helps overcome basic human tendencies toward risk aversion, including a propensity to make overly cautious investment decisions or a reluctance to accept uncertainty in compensation. We find, however, that aggressive real behavior is not associated with higher shareholder returns or with larger CEO compensation. One broad interpretation of this result is that attempts to counteract or entirely overcome managerial risk-aversion might not have the desired effect of increasing payoffs, from the perspective of either the manager or shareholders.

In our main tests, we find consistent evidence that aggressive reporting behavior is associated with higher shareholder returns and larger CEO compensation. The finding that aggressive reporting behavior is associated with higher shareholder returns is, on its surface, at odds with research documenting sharply negative market reactions to announcements of extreme adverse reporting events, specifically, restatements. To reconcile these results, we first provide evidence that the restatement announcement (negative) returns are significantly smaller in magnitude than the (positive) returns a long-term investor would earn either over the restatement-affected period or over a naïvely chosen pre-event period. Stated differently, shareholders benefit from reporting aggressiveness, even in extreme and unusual instances when aggressive reporting behavior violates the boundaries imposed by GAAP and results in restatements. Secondly, the beyond-GAAP accounting practices that led to these restatements, which are of course known only *ex post*, do not explain the effect of our within-GAAP aggressive-reporting that is based on such innocuous accounting choices as depreciation method and inventory cost-flow method.

Our analysis leaves several questions unanswered. For example, we do not investigate the question of whether there is some (desirable) level of real-action aggressiveness that would be associated with increased payoffs to CEOs and shareholders. Also, we cannot shed light on the possibility of an over-time evolution in a CEO's payoff that might be a function of job-change; our essentially cross-sectional analysis assumes substantial stability in real-action aggressiveness and reporting aggressiveness and cannot distinguish, for example, the potential that a CEO might move to a firm with more (or less) aggressive real activities.

Appendix A: Variable Definitions

Reporting Aggressiveness (*RepAgg*) Latent Variable and Proxies

ReportAgg	=	Latent variable for reporting aggressiveness.
DepMethod	=	Depreciation method (DPACT_FN). Accelerated (straight-line) depreciation is assigned a value of 0 (1), and a combination is assigned a value of 0.5.
InvMethod	=	Inventory valuation method (INVVAL). LIFO (FIFO) is assigned a value of 0 (1), and the average cost method is assigned a value of 0.5.
AvgLife	=	Average depreciable life across all property, plant and equipment (PPE) assets, estimated as gross PPE (PPEGT) over the sum of depreciation expense (DP) and amortization expense (AM, set to zero if missing.)
OperLease	=	Present value of operating lease obligations (MRC1 through MRC5).

Real Aggressiveness (*RealAgg*) Latent Variable and Proxies

RealAgg	=	Latent variable for real aggressiveness.
Acquisition	=	Total of all acquisition transaction values from SDC Platinum in a fiscal year, scaled by adjusted total assets.
Debt Issuance	=	Debt issuance proceeds in the fiscal year (DLTIS) net of debt repayments (DLTR), scaled by adjusted total assets.
Goodwill	=	New investments in goodwill, calculated as the difference between the ending balance of goodwill (GDWL) and beginning balance of goodwill (lagged GDWL) plus goodwill amortization expense (GDWLAM, set to zero if missing), scaled by adjusted total assets.
CapEx	=	Capital expenditures (CAPX), scaled by adjusted total assets.
R&D	=	Research and development expense (XRD, set to zero if missing), scaled by adjusted total assets.
Advertising	=	Advertising expense (XAD, set to zero if missing), scaled by adjusted total assets.
Leverage	=	Total debt (DLC + DLTT), scaled by adjusted total assets.
Stock_Issuance	=	Stock issuance proceeds (SSTK) net of stock repurchases (PRSTKC), scaled by adjusted total assets.
GW_Impairment	=	Impairment charges for goodwill (absolute value of GDWLIA, set to zero if missing), scaled by adjusted total assets. If GDWLIA is missing and goodwill investment is negative, we set it to the absolute value of goodwill investment.

Payoff variables

AvgReturn	=	Average monthly return from CRSP over months m−11 to m or over Months +4 to +15, whereby *m*=0 is the fiscal year end month.

AbnReturn	=	Three-factor monthly abnormal return (intercepts) from firm-specific regressions of monthly excess returns on the market risk premium, SMB and HML. The estimation period is concurrent with the returns measurement period, Months +4 to +15 after the fiscal year end.
TotalComp	=	Total CEO compensation from ExecuComp (TDC1), scaled by adjusted total assets and multiplied by 100.
AbnTotalComp	=	*TotalComp* less the average *TotalComp* in that 2-digit SIC code and year.
Restatement	=	Indicator variable for the fiscal years affected by a restatement according to the Audit Analytics database, and 0 otherwise.
Backdater	=	Indicator variable for fiscal years during which firms announced, or were alleged, to have backdated stock options as collected by Glass-Lewis, and 0 otherwise.

<u>Other Variables</u>

Adj. Total Assets	=	Adjusted total assets, equal to total assets (AT) plus accumulated depreciation (DPACT) plus LIFO reserve (LIFR). DPACT and LIFR are set to zero if missing.

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Table 1
Sample Construction and Distribution

Panel A: Selection criteria

	# Firm-years	# Distinct Firms
Firm-years on Compustat North America 1992-2014	265,890	19,914
Lagged total assets, total assets < 1	(67,012)	(3,077)
Missing returns data for months (-11,0) or months (+4;+15)	(81,981)	(2,595)
	116,897	14,242
Missing data on depreciation method or inventory valuation method	(55,783)	(6,382)
Returns Sample	**61,114**	**7,860**
Missing compensation data on ExecuComp	(39,556)	(5,736)
Compensation Sample	**21,558**	**2,124**

Panel B: Distribution of sample observations by year

Year	Returns Sample	Subsamples			
		Compensation Sample	Restatement-Affected Sample	Restatement Announcement Sample	Options Backdater Sample
1992	2,866	712	4		
1993	3,047	824	9		
1994	3,272	886	27		
1995	3,426	940	59		
1996	3,450	947	101		
1997	3,439	969	142		
1998	3,280	956	208	33	
1999	3,074	929	274	77	
2000	2,911	942	360	86	
2001	2,898	982	474	98	
2002	2,727	986	569	128	
2003	2,657	1,005	601	155	
2004	2,529	981	601	237	
2005	2,468	926	452	172	
2006	2,370	937	336	100	82
2007	2,287	1,014	267	61	23
2008	2,266	1,014	243	54	
2009	2,176	999	241	47	
2010	2,090	974	278	50	
2011	2,051	952	312	72	
2012	1,982	920	322	74	
2013	1,939	895	271	67	
2014	1,909	868	192	51	
Total	**61,114**	**21,558**	**6,343**	**1,562**	**105**

Table 2
Descriptive Statistics

	Returns Sample				Compensation Sample			
	# Obs.	Mean	Std. Dev.	Median	# Obs.	Mean	Std. Dev.	Median
Shareholder Payoff Variables								
AvgReturn (+4,+15)	61,114	0.0128	0.0541	0.0104	--	--	--	--
AbnReturn (+4;+15)	61,114	0.0042	0.0667	0.0012	--	--	--	--
CEO Payoff Variables								
TotalComp	--	--	--	--	21,558	0.3052	0.7172	0.1494
AbnTotalComp	--	--	--	--	21,558	-0.0264	0.7001	-0.0935
ReportAgg Proxies:								
DepMethod	61,114	0.9154	0.2074	1.0000	21,558	0.9224	0.1942	1.0000
Accelerated (0)	965				213			
Combination (0.5)	8,407				2,920			
Straight-line (1)	51,742				18,425			
InvMethod	61,114	0.7529	0.3582	1.0000	21,558	0.6766	0.4092	1.0000
LIFO (0)	8,047				4,757			
Average cost (0.5)	14,111				4,428			
FIFO (1)	38,956				12,373			
Additional proxies								
AvgLife	12,810	16.1270	6.6313	15.1242	7,658	16.4628	6.1437	15.4875
OperLease	12,810	0.0378	0.0623	0.0181	7,658	0.0352	0.0585	0.0171
RealAgg Proxies:								
Acquisition	61,114	0.0260	0.0744	0.0000	21,558	0.0301	0.0739	0.0000
Debt_Issuance	61,114	0.0104	0.0682	0.0000	21,558	0.0095	0.0614	0.0000
Goodwill_Investment	61,114	0.0106	0.0343	0.0000	21,558	0.0121	0.0337	0.0000
Additional proxies								
CapEx	61,114	0.0454	0.0442	0.0320	21,558	0.0446	0.0381	0.0337
R&D	61,114	0.0366	0.0686	0.0033	21,558	0.0270	0.0484	0.0046
Advertising	61,114	0.0106	0.0277	0.0000	21,558	0.0118	0.0275	0.0000
Leverage	61,114	0.1743	0.1617	0.1455	21,558	0.1729	0.1432	0.1578
Stock_Issuance	61,114	0.0177	0.0948	0.0000	21,558	-0.0036	0.0599	0.0000
GW_Impairment	61,114	0.0055	0.0295	0.0000	21,558	0.0057	0.0306	0.0000
Other Variables								
Adjusted Total Assets	61,114	5,232	26,462	341	21,558	8,076	32,041	1,459

Variable definitions are in Appendix A. The Returns Sample and Compensation Sample are described in Table 1.

Table 3
Regression of Time-Series Variation in Income Statement Line Items on Accounting Choices

	σ (Depreciation)		σ (COGS)		σ (Income)	
Dependent Variable	Unadjusted Coefficients	Standardized Coefficients	Unadjusted Coefficients	Standardized Coefficients	Unadjusted Coefficients	Standardized Coefficients
Intercept	0.0223***		2.7810***		0.0197***	
	8.17		*11.41*		*4.67*	
DepMethod	0.0141***	0.0368***			0.0127***	0.0226***
	4.70				*2.76*	
InvMethod			3.6992***	0.0744***	0.0593***	0.1823***
			10.77		*28.03*	
Adj. Rsq. (in %)	0.1338		0.5511		3.4607	
N	60,980		60,337		61,098	
# Distinct Firms	7,840		7,703		7,856	

Variable definitions are in Appendix A. This table reports the results of regressing the time-series variability in three income statement line items (scaled measures of depreciation expense, cost of goods sold and net income) on accounting choice. Unadjusted coefficients are based on accounting choice proxies as defined in Appendix A. Results labeled 'Standard Coefficients' are based on regressions with demeaned variables, scaled by the respective sample standard deviation. Standard errors are clustered by firm; *** (**) [*] indicates coefficient significance at the 1% (5%) [10%] level.

Table 4
Measurement Models - Latent Variable Coefficients

Panel A: Returns Sample

		Full Model		Standard Model		Standard Model (industry-year-adjusted)	
Latent Variable	Proxy	Coeff.	*t-stat*	Coeff.	*t-stat*	Coeff.	*t-stat*
ReportAgg	DepMethod	0.1904***	*2.67*	0.2630***	*10.39*	0.2006***	*5.17*
	InvMethod	0.5770***	*2.76*	0.4178***	*11.17*	0.4592***	*5.59*
RealAgg	Acquisition	0.3102***	*15.15*	0.5188***	*40.29*	0.4997***	*39.75*
	Debt_Issuance	0.7143***	*27.01*	0.3812***	*35.68*	0.3857***	*35.83*
	Goodwill_Investment	0.3724***	*16.73*	0.6486***	*45.25*	0.6463***	*44.78*
	GW_Impairment	-0.0608***	*-11.01*				
	CapEx	0.1718***	*12.28*				
	R&D	-0.0940***	*-7.14*				
	Advertising	-0.0298***	*-4.19*				
	Leverage	0.3815***	*49.14*				
	Stock_Issuance	-0.0507***	*-5.68*				
<u>Goodness-of-fit</u>							
SRMR		0.0636		0.0071		0.0047	
RMSEA		0.0820		0.0124***		0.0081***	
Prob (RMSEA < 0.05)		0.00		1.00		1.00	

Panel B: Compensation Sample

		Full Model		Standard Model		Standard Model (industry-year-adjusted)	
Latent Variable	Proxy	Coeff.	*t-stat*	Coeff.	*t-stat*	Coeff.	*t-stat*
ReportAgg	DepMethod	0.2536***	*7.23*	0.2478***	*7.40*	0.2367***	*6.77*
	InvMethod	0.4799***	*8.28*	0.4912***	*8.59*	0.4820***	*8.40*
RealAgg	Acquisition	0.4536***	*17.66*	0.4763***	*22.94*	0.4591***	*22.30*
	Debt_Issuance	0.5192***	*9.35*	0.4513***	*25.11*	0.4539***	*24.79*
	Goodwill_Investment	0.6412***	*11.34*	0.6896***	*29.31*	0.6882***	*28.00*
	GW_Impairment	-0.0589***	*-6.52*				
	CapEx	-0.0511*	*-1.70*				
	R&D	0.0075	*0.26*				
	Advertising	-0.0463***	*-3.25*				
	Leverage	0.2375***	*5.39*				
	Stock_Issuance	-0.0350	*-1.31*				
<u>Goodness-of-fit</u>							
SRMR		0.0713		0.0181		0.0182	
RMSEA		0.0893		0.0332***		0.0347***	
Prob (RMSEA < 0.05)		0.00		1.00		1.00	

Variable definitions are in Appendix A. This table reports the coefficient estimates (t-statistics) for each proxy obtained from estimating the SEM given by equations (1)-(3) for each of the noted *Payoff* variables. Panel A reports results for shareholder payoffs, which use the Returns Sample. Panel B shows results for CEO payoffs, which use the Compensation Sample. Standard errors are clustered by firm; *** (**) [*] indicates coefficient significance at the 1% (5%) [10%] level.

Table 5
Structural Equations Models for Payoffs

Panel A: Path and correlation coefficients (unadjusted proxies)

Payoff Variable	p_1 (*RealAgg -> Payoff*)	p_2 (*ReportAgg -> Payoff*)	$p_2 - p_1$ (Difference)	ρ(*RealAgg* ,*ReportAgg*)
Shareholder Payoff Variables				
AvgReturn (+4;+15)	-0.0861***	0.0614***	0.1475***	0.1638***
	-14.95	*8.40*		*9.71*
AbnReturn (+4;+15)	-0.0600***	0.0668***	0.1268***	0.1630***
	-10.85	*8.14*		*9.62*
CEO Payoff Variables				
TotalComp	-0.0482**	0.4559***	0.5041***	0.1820***
	-2.40	*10.33*		*6.90*
AbnTotalComp	-0.0547***	0.2410***	0.2958***	0.1803***
	-3.63	*7.10*		*6.50*

Panel B: Path and correlation coefficients (industry-year-adjusted proxies)

Payoff Variable	p_1 (*RealAgg -> Payoff*)	p_2 (*ReportAgg -> Payoff*)	$p_2 - p_1$ (Difference)	ρ(*RealAgg* ,*ReportAgg*)
Shareholder Payoff Variables				
AvgReturn (+4;+15)	-0.0761***	0.0325***	0.1086***	0.1013***
	-13.68	*4.46*		*5.50*
AbnReturn (+4;+15)	-0.0552***	0.0268***	0.0820***	0.0985***
	-10.54	*3.67*		*5.04*
CEO Payoff Variables				
TotalComp	-0.0313**	0.2591***	0.2904***	0.0694***
	-2.49	*8.05*		*2.93*
AbnTotalComp	-0.0167	0.2885***	0.3052***	0.0664***
	-1.27	*8.18*		*2.93*

Variable definitions are in Appendix A. This table reports the path coefficients between *RealAgg* and *Payoff* (p_1) and between *ReportAgg* and *Payoff* (p_2). We also report tests of the difference, $p_2 - p_1$, and the correlations between *ReportAgg* and *RealAgg* (ρ) in the last column. The estimates are based on maximum likelihood estimation of the SEM given by Equations (1)-(3). *RealAgg* is the latent variable capturing real aggressiveness and *ReportAgg* is the latent variable capturing reporting aggressiveness; see the 'Simple Model' of Table 4 for the manifest proxies for each latent variable. Panel A reports results based on unadjusted proxies, and Panel B shows results using industry-year-adjusted proxies. Results for shareholder payoff variables are based on the Returns Sample; results for CEO payoff variables use the Compensation Sample. Standard errors are clustered by firm; *** (**) [*] indicates coefficient significance at the 1% (5%) [10%] level.

Table 6
Structural Equations Models for Extreme Reporting Outcomes (LOGIT models)

Panel A: Restatement Events (path and correlation coefficients; N = 61,114, of which 6,343 are event firm-years)

Proxies	p_1 (*RealAgg* -> *Payoff*)	p_2 (*ReportAgg* -> *Payoff*)	p_2 - p_1 (Difference)	ρ(*RealAgg* ,*ReportAgg*)
Unadjusted Proxies	0.0140	0.1320***	0.1180***	0.1810***
	1.18	*4.09*	*3.04*	*8.15*
Industry-year-adjusted Proxies	-0.0030	0.1850***	0.1880***	0.1070***
	-0.23	*5.23*	*4.64*	*5.43*

Panel B: Option Backdating Events (path and correlation coefficients; N = 4,657 from 2006/07, of which 105 are event firm-years)

Proxies	p_1 (*RealAgg* -> *Payoff*)	p_2 (*ReportAgg* -> *Payoff*)	p_2 - p_1 (Difference)	ρ(*RealAgg* ,*ReportAgg*)
Unadjusted Proxies	-0.0530	0.4550***	0.5080***	0.1470***
	-0.92	*4.25*	*3.79*	*3.37*
Industry-year-adjusted Proxies	-0.1140	0.1140**	0.2290*	0.0640
	-1.10	*1.98*	*1.89*	*1.37*

Variable definitions are in Appendix A. This table reports the path coefficients between *RealAgg* and *Payoff* (p_1) and between *ReportAgg* and *Payoff* (p_2). We also report tests of the difference, p_2- p_1, and the correlations between *ReportAgg* and *RealAgg* (ρ) in the last column. *RealAgg* is the latent variable capturing real aggressiveness and *ReportAgg* is the latent variable capturing reporting aggressiveness. We report separate results based on the raw values of the proxies and on the industry-year-adjusted proxies. In Panel A, the estimates are based on maximum likelihood estimation of the SEM given by equations (1)-(3) on the Returns Sample, whereby the structural component, Eq. (3), is estimated as a logit function for *Restatement* $\in$ {0;1}as *Payoff*. *Restatement* =1 for firm-years that are (ex-post) subject to a restatement, and zero otherwise. For a fiscal year to be marked as affected, it is sufficient that a fraction of that year, e.g. one quarter, is affected by a restatement; Table 1, Panel B, shows the distribution of restatement-affected fiscal years over time. Panel B is a similar logit model, except *Restatement* is replaced by *OptionBackdater* =1 if the firm announced to have backdated stock options, 0 otherwise. The sample for this test reduces to the fiscal years 2006 and 2007. Standard errors are clustered by firm; *** (**) [*] indicates coefficient significance at the 1% (5%) [10%] level.

Table 7
Abnormal Returns on Latent Aggressiveness Factors around Restatement Events

Independent variable	All Restatements	Fraudulent Restatements Only	Clerical Errors Only
Intercept	0.0033***	0.0040***	0.0043***
	9.49	*15.06*	*15.68*
Pre (a_1)	0.0049***	0.0101***	0.0034
	7.34	*3.66*	*1.49*
Post (a_2)	-0.0014**	0.0009	-0.0025
	-2.00	*0.44*	*-1.42*
RealAgg (a_3)	-0.1170***	-0.1054***	-0.1055***
	-9.43	*-10.94*	*-11.04*
RealAgg x *Pre (a_4)*	0.0115	-0.0329	-0.0001
	0.53	*-0.58*	*0.00*
RealAgg x *Post (a_5)*	0.0671***	0.0693	0.0849
	2.64	*1.32*	*1.03*
ReportAgg (a_6)	0.0108***	0.0109***	0.0111***
	6.28	*8.33*	*8.49*
ReportAgg x *Pre (a_7)*	0.0056**	0.0239**	0.0032
	1.97	*2.54*	*0.36*
ReportAgg x *Post (a_8)*	-0.0067*	-0.0198*	0.0057
	-1.71	*-1.82*	*0.51*
Adj. Rsq. (in %)	0.3429	0.2804	0.2256
Tests of Coefficient Differences			
Δ (a_1 - a_2) -- Main effects	0.0062***	0.0093***	0.0059*
	7.51	*2.83*	*1.84*
Δ (a_6 - a_3)-- Main effects	0.1278***	0.1163***	0.1165***
	9.72	*11.40*	*11.52*
Δ (a_4 - a_5)	-0.0556*	-0.1022	-0.0850
	-1.96	*-1.35*	*-0.82*
Δ (a_7 - a_8)	0.0123***	0.0437***	-0.0024
	2.89	*3.01*	*-0.17*
# Obs. (Total)	61,114	61,114	61,114
# Obs. with Pre-Event = 1	14,346	1,301	751
# Obs. with Post-Event = 1	7,945	1,830	497

Variable definitions are in Appendix A. This table reports the results of regressing future abnormal returns *AbnReturns*(+4,+15), on the latent variables *ReportAgg* and *RealAgg* as well as their interactions with *Pre*- and *Post*-indicator variables using the Returns Sample. *Pre* equals 1 for firm-year observations prior to a restatement announcement event, and 0 otherwise. *Post* equals 1 for firm-year observations with or after a restatement announcement event, 0 otherwise. Column 1 reports the results for all restatements, Column 2 and 3 reports results for the subsamples of restatements: frauds and clerical errors. Standard errors are clustered by firm; *** (**) [*] indicates coefficient significance at the 1% (5%) [10%] level.

Table 8
Calendar-Time Portfolio Analysis for Restatement Firms

	All Restatements			Restatements with High *ReportAgg*			Fraudulent Restatements			Fraudulent Restatements with High *ReportAgg*		
Event-time period (in months)	# Calendar Months	Intercept	Cumulative Abnormal Return *(Starting Month -72)*	# Calendar Months	Intercept	Cumulative Abnormal Return *(Starting Month -72)*	# Calendar Months	Intercept	Cumulative Abnormal Return *(Starting Month -72)*	# Calendar Months	Intercept	Cumulative Abnormal Return *(Starting Month -72)*
-72 to -61	236	0.0035*	4.23%	233	0.0043*	5.34%	204	0.0069**	8.63%	107	0.0060	7.45%
		1.92	*[Months -72 to -61]*		*1.96*	*[Months -72 to -61]*		*2.31*	*[Months -72 to -61]*		*1.16*	*[Months -72 to -61]*
-60 to -49	246	0.0043***	9.74%	240	0.0070***	14.55%	211	0.0071**	18.31%	126	0.0179***	33.00%
		2.79	*[Months -72 to -49]*		*3.33*	*[Months -72 to -49]*		*2.56*	*[Months -72 to -49]*		*3.52*	*[Months -72 to -49]*
-48 to -37	254	0.0044***	15.70%	242	0.0061***	23.20%	216	0.0016	20.65%	144	0.0037	39.00%
		2.60	*[Months -72 to -37]*		*2.73*	*[Months -72 to -37]*		*0.58*	*[Months -72 to -37]*		*0.85*	*[Months -72 to -37]*
-36 to -25	255	0.0049***	22.66%	246	0.0087***	36.68%	228	0.0065**	30.38%	149	0.0169***	69.94%
		2.92	*[Months -72 to -25]*		*3.85*	*[Months -72 to -25]*		*2.11*	*[Months -72 to -25]*		*2.82*	*[Months -72 to -25]*
-24 to -13	260	0.0015	24.83%	247	0.0025	40.82%	231	-0.0032	25.50%	177	0.0012	72.48%
		0.85	*[Months -72 to -13]*		*1.25*	*[Months -72 to -13]*		*-1.17*	*[Months -72 to -13]*		*0.34*	*[Months -72 to -13]*
-12 to -3	256	-0.0023	21.96%	245	-0.0002	40.53%	222	-0.0018	23.27%	152	-0.0026	68.01%
		-1.13	*[Months -72 to -3]*		*-0.08*	*[Months -72 to -3]*		*-0.42*	*[Months -72 to -3]*		*-0.36*	*[Months -72 to -3]*
-2 to +2	240	-0.0187***	11.00%	239	-0.0204***	26.75%	126	-0.0295***	6.15%	62	-0.0325***	42.40%
		-7.20	*[Months -72 to +2]*		*-6.25*	*[Months -72 to +2]*		*-5.48*	*[Months -72 to +2]*		*-3.54*	*[Months -72 to +2]*
+3 to +12	245	-0.0043	6.33%	237	-0.0025	23.60%	214	-0.0055	0.41%	131	-0.0067	33.15%
		-1.49	*[Months -72 to +12]*		*-0.71*	*[Months -72 to +12]*		*-1.42*	*[Months -72 to +12]*		*-1.00*	*[Months -72 to +12]*
+13 to +24	233	-0.0016	4.31%	226	-0.0014	21.49%	208	0.0013	1.94%	137	0.0057	42.56%
		-0.61	*[Months -72 to +24]*		*-0.51*	*[Months -72 to +24]*		*0.33*	*[Months -72 to +24]*		*1.06*	*[Months -72 to +24]*
+25 to +36	215	0.0001	4.41%	214	0.0022	24.70%	187	-0.0048	-3.78%	103	-0.0055	33.40%
		0.04	*[Months -72 to +36]*		*0.87*	*[Months -72 to +36]*		*-1.50*	*[Months -72 to +36]*		*-1.20*	*[Months -72 to +36]*

This table shows the results of estimating calendar-time portfolio regressions for (monthly) abnormal returns to equal-weighted portfolios of firms that disclosed a restatement. *m* denotes the number of months between the returns measurement and the restatement announcement; a range of values for *m* is used to form the calendar-time portfolios. Abnormal returns are calculated using the 3-factor model as the benchmark for expected returns; the intercept from this regression is the average monthly abnormal return over the event-time period for all calendar months with sufficient data to form a portfolio of at least ten event firms. Cumulative abnormal returns are calculated using the interval-specific intercept estimates, starting in Event Month -72 and ending in the last event month of the most recent interval included in the calculation. Results are based on a sample of 6,938 restatement announcements from April 1995 to September 2016 by 4,221 distinct firms. The entire underlying monthly returns database spans the period from January 1992 to December 2016, from which observations are selected depending on the varying event-time windows.

Table 9
Average Firm-Specific Returns in Restatement Period and Various Announcement Windows

Interval	All Restatements			Restatements with High *ReportAgg*			Fraudulent Restatements			Fraudulent Restatements with High *ReportAgg*		
	Avg. Length (in months)	Buy-and-Hold Return	Cumulative Return	Avg. Length (in months)	Buy-and-Hold Return	Cumulative Return	Avg. Length (in months)	Buy-and-Hold Return	Cumulative Return	Avg. Length (in months)	Buy-and-Hold Return	Cumulative Return
Restatement Period	32.90	0.4478***		32.42	0.4893***		32.33	0.5284***		31.68	0.6761***	
		7.69			*6.86*			*4.69*			*4.32*	
Announcement Windows												
-2 to +2	7.92	-0.0040	0.4290***	7.78	-0.0103	0.4360***	5.33	-0.0985***	0.3537***	5.24	-0.1063***	0.4173***
		-0.31	*10.67*		*-0.62*	*9.13*		*-4.65*	*3.42*		*-3.99*	*3.17*
-1 to +1	4.77	-0.0201*	0.3989***	4.69	-0.0285**	0.4077***	3.22	-0.1028***	0.3462***	3.16	-0.1078***	0.4218***
		-1.89	*10.20*		*-2.12*	*8.79*		*-5.57*	*3.22*		*-5.14*	*3.07*
Event Month only	1.60	-0.0261***	0.3979***	1.57	-0.0329***	0.4175***	1.08	-0.0742***	0.3780***	1.06	-0.0697***	0.4953***
		-4.35	*10.29*		*-4.43*	*8.56*		*-5.88*	*3.28*		*-4.47*	*3.08*
# Event Firms		4,187			2,539			598			370	

This table contrasts the buy-and-hold return for restatement firms over the actual fiscal periods affected by a restatement with buy-and-hold return over three windows around the restatement announcement. 'Cumulative' returns report the mean cumulative return over the restatement period and announcement windows. For each firm, returns are aggregated across all restatement events, if applicable, and thus each firm enters the sample only once. In case of overlapping restatement periods announced at different points in time, each firm-month is only used once in the return calculations. Because this test requires data on the restatement period and returns data in the announcement month, the sample reduces slightly, compared to Table 8, to 6,719 restatement announcement events by 4,187 distinct firms (about 1.60 restatement announcements per average firm), 2,539 of which are classified as high *ReportAgg*. In the subsample of 598 fraudulent restatements, 370 are classified as high *ReportAgg*. All standard errors for the restatement period buy-and-hold returns are clustered by the first restatement-affected return month; standard errors for the announcement and cumulative buy-and-hold returns are clustered by the month of the firm's first announcement.