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# The Asymmetric Effect of Reporting Flexibility on Priced Risk

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Abstract

Most firms covary more positively with downmarkets than upmarkets—a phenomenon I refer to as "risk asymmetry." I predict and find that risk asymmetry is caused, at least in part, by a firm's ability to selectively obfuscate poor performance. Risk asymmetry decreases significantly when firms are required to adhere to the more stringent auditing standards mandated under Section 404 of the Sarbanes-Oxley Act, however this decrease is more muted for firms with weak internal controls. Consistent with my predictions, these patterns are stronger for more market-sensitive firms and weaker for firms that include relative performance evaluation in their CEOs' pay packages. Taken together with prior literature (which documents that risk asymmetry is priced), my

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results suggest that a firm can lower its cost of capital by credibly reducing its ability to obfuscate value-relevant information.

**JEL codes:** G14, M41, M42, M49

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## 1. *Introduction*

In this study, I develop and test predictions regarding the link between a manager's ability to obfuscate value-relevant information, hereafter "reporting flexibility," and systematic risk. In particular, I examine how reporting flexibility creates asymmetric systematic risk exposure, whereby the firm covaries more positively with downmarkets than with upmarkets—a phenomenon I refer to as "risk asymmetry."

Motivating my predictions are the notions that: (1) poor information quality causes assessed systematic risk exposure to amplify (e.g., Yee [2006], Lambert, Leuz, and Verrecchia [2007]; and (2) managers prefer to be less transparent with bad news than with good news (e.g., Verrecchia [1983], Dye [1985], Jung and Kwon [1988], Lang and Lundholm [1993], Bloomfield [2002], Miller [2002], Li [2008], Kothari, Shu, and Wysocki [2009], Bertomeu, Ma, and Marinovic [2015]). Coupled with (1), managers' asymmetric desire to obfuscate will result in greater systematic risk in bad states than in good states—but only insofar as obfuscation is feasible. If obfuscation is sufficiently costly (e.g., because of stringent audit practices), then management's asymmetric incentives to obfuscate will not translate into asymmetric information quality. Thus, ceteris paribus, I expect firms with more (less) reporting flexibility, to exhibit greater (lower) risk asymmetry.[1]

Risk asymmetry is a feature of asset return comovements and cannot be diversified away in large economies, and is therefore of potential hedging concern to investors. Prior work by Ang, Chen, and Xing [2006], hereafter "ACX," demonstrates that risk asymmetry is positively priced in American equity markets, even after controlling for common determinants of the cost of capital ($\beta$, size, book-to-market, momentum, liquidity, and coskewness risk). Thus, empirical support for my predictions would suggest that reporting flexibility contributes to a firm's cost of capital beyond what is currently captured by common multifactor asset pricing models.

I measure risk asymmetry as the difference between a firm's downmarket $\beta$ and its upmarket $\beta$.[2] For my primary measure of reporting flexibility, I exploit variation in treatment by Section 404 of the Sarbanes-Oxley Act ("SOX 404"), which requires firms to have their internal controls assessed and reported on by their auditors. I find that firms that are mandated to comply with SOX 404 requirements ("SOX 404 reporters") exhibit

[1] I provide an analytical derivation of my predictions in the online appendix.

[2] A detailed description of the construction of this measure can be found in section 3.

significantly less risk asymmetry than do firms that are not mandated to comply with SOX 404 requirements ("SOX 404 non-reporters").

To justify a causal interpretation, I follow Iliev [2010] and use a regression discontinuity design ("RDD") to implement an event study based on plausibly exogenous variation in firms' exposures to the SOX 404 mandate. For fiscal years ending after November 15, 2004, domestic firms with public floats in excess of $75 million ("accelerated filers") are subject to more stringent auditing requirements, whereas firms with smaller public floats ("non-accelerated filers") are left unaffected. For firms within a narrow bandwidth of the $75 million treatment threshold, the assumption that treatment is "as good as random" allows the estimated difference-in-differences to be attributed to SOX 404.[3] I find that, compared to otherwise similar firms, being an accelerated filer is associated with a roughly 0.3 (0.27 standard deviation) decrease in the firm's risk asymmetry—but only after SOX 404 comes into effect. I caveat that the estimated magnitude should be interpreted as a "local average treatment effect" and is plausibly larger than for the average public firm in the economy.

Consistent with my predictions, I further find that the effect of SOX 404 is greater for more market-sensitive firms (i.e., firms whose market $\beta$s are large in magnitude). The estimated post–SOX 404 decrease in risk asymmetry is roughly five times greater for firms of above-median market sensitivity, compared to firms of below-median market-sensitivity.

Many of the firms in the RDD sample switch from being non-accelerated filers to accelerated filers, after the initial SOX 404 adoption date. By design, the RDD specifications disregard these endogenous switches, relying instead on instrumented filer status based on 2002 public floats. However, in subsequent analyses, I exploit this variation and analyze changes in firms' risk asymmetry as they transition from being SOX 404 non-reporters to SOX 404 reporters. I find that the switch to being a SOX 404 reporter coincides with a substantial drop in risk asymmetry relative to comparably sized nontransitioning firms.

Lastly, I explore variation in reporting flexibility and risk asymmetry among SOX 404 reporters. I winnow the sample to include only the firm-year observations for which an auditor reported an opinion on the firm's internal controls, and exploit variation in the auditors' opinions. I document that risk asymmetry is greater for firm-year observations in which the auditor assessed the internal controls to be weak. With respect to the magnitude of the effect, weak internal controls appear to undo most of the benefits from SOX 404 reporting, vis-à-vis reducing risk asymmetry. On average, SOX 404 reporters with internal control weaknesses exhibit risk asymmetry on par with SOX 404 non-reporters. The same is true of a within-firm comparison; on average SOX 404 reporters with internal control weaknesses

[3] Prior work by Gao, Wu, and Zimmerman [2009] and Iliev [2010] suggests that some firms manage their 2004 public floats to avoid treatment—a violation of random assignment. To address this concern, I use the IV approach from Iliev [2010].

exhibit risk asymmetry similar to their own levels from their pre–SOX 404 years. Consistent with my predictions, I find that the exacerbating effects of internal control weaknesses on risk asymmetry vanish among firms that use relative performance-based executive pay.

In addition to examining risk asymmetry, I also analyze two other measures of asymmetric risk: relative downside risk and relative upside risk. I find that SOX 404 and internal control weaknesses have fairly symmetric effects on the two types of risk. SOX 404 reporting is associated with decreased relative downside risk and increased relative upside risk. In contrast, internal control weaknesses are associated with increased relative downside risk and decreased relative upside risk.

Throughout my analyses, I employ a battery of control variables to address observable heterogeneity, and dense fixed effect structures (firm and year) to address unobservable heterogeneity. The use of year fixed effects allows for arbitrary sample-wide time trends, whereas the use of firm fixed effects mitigates concerns about persistent firm-level endogeneities (e.g., time-invariant business model, operating volatility) because coefficient estimates are identified exclusively from within-firm time-series variation in risk asymmetry and reporting flexibility. Collectively, the empirical patterns documented in this study provide consistent and robust evidence in support of my predictions: A firm's ability to selectively obfuscate adverse value-relevant information appears to create an asymmetric risk profile, whereby the firm covaries more positively with downmarkets and less positively with upmarkets.

This study contributes to the literature along a number of dimensions. First and foremost, this paper is the first to provide evidence that a firm's ability to obfuscate value-relevant information (i.e., "reporting flexibility") affects the firm's systematic risk profile asymmetrically. Using a variety of measures and methodologies, including an RDD, I provide evidence that reporting flexibility causes risk asymmetry to increase. Prior work by ACX demonstrates that an increase in risk asymmetry carries substantial cost of capital implications. Taken together with this prior evidence, my findings suggest that reporting flexibility may represent a significant source of variation in firms' costs of capital. Risk asymmetry is not included in conventional multifactor models, which may explain the apparent "abnormal" pricing of accounting quality documented in prior empirical studies—further research is needed to conclusively establish what role risk asymmetry plays in these findings.

Relatedly, this is the first study to connect the reporting quality literature to the returns-based risk asymmetry measure developed by ACX. In so doing, this study complements prior work connecting firm transparency to other types of downside risk, such as stock price "crash risk" and "earnings downside risk." Prior studies find that crash risk is more prevalent among more opaque firms (e.g., Lin and Myers [2006], Hutton, Marcus, and Tehranian [2009]), less conservative firms (e.g., Kim and Zhang [2016]; Kim, Wang and Zhang [2016]), firms with higher (signed) accruals (e.g., Zhu, [2010]), firms with less comparable financial statements (e.g., Kim et al. [2016]), and poorly monitored firms (e.g., Kim, Li, and Zhang [2011], An and Zhang [2013]). This study complements the existing work on crash risk by examining the distinct but related phenomenon of "risk asymmetry," and providing evidence that it is causally affected by the firm's information environment.[4]

In another related paper, Konchitchki et al. [2016] examine an alternative form of asymmetric risk: earnings downside risk ("EDR"). They use a "root lower partial moment framework"[5] to examine the relative variability of below-expectation and above-expectation earnings. They find that earnings are often asymmetric in that below-expectation earnings are more volatile. Moreover, firms with relatively more volatile below-expectation earnings are more sensitive to negative macroeconomic shocks and face a significantly higher cost of capital. Their work corroborates ACX, further providing evidence that investors have a greater distaste for downside risk than for upside risk. Although Konchitchki et al. [2016] do not examine the relation between reporting flexibility and EDR, they do document that EDR is associated with earnings' statistical properties such as "smoothness," persistence, and predictability.

Lastly, this paper contributes to the literature on the consequences of reporting flexibility, and specifically internal controls and SOX 404. Ample prior literature examines the role of internal controls as a disciplinary mechanism, which improves financial reporting quality by disciplining managements' ability to obfuscate value-relevant information. For example, Caplan [1999] argues that weak internal controls help managers "hide" fraud, whereas Doyle, Ge, and McVay [2007a], as well as Ashbaugh-Skaife et al. [2009], show that internal control weaknesses are associated with poor accrual quality. Furthermore, Iliev [2010] shows that SOX 404 compliance causes firms to report more conservatively. Related structural work by Bertomeu et al. [2020] as well as Bird, Karolyi, and Ruchti [2018] shows that SOX 404 increased the cost of manipulation.

Additionally, several prior studies examine the link between internal controls and the cost of equity. For example, Ogneva, Subramanyam, and Raghunandan [2007] find that, among first-time SOX 404 filers, internal control weaknesses were associated with significantly higher implied costs

[4] On their surface, crash risk and risk asymmetry seem closely related, but there are several significant conceptual and empirical differences. First, crash risk is not a measure of asymmetric risk. Crash risk reflects the likelihood of an unusually negative stock return. It does not reflect the relative likelihoods of very negative versus very positive stock returns. Second, crash risk does not reflect comovement with the market. In fact, crash risk is intentionally designed to be firm specific—the measure of returns typically used in calculating crash risk is carefully constructed using residuals to purge the effect of market-wide downturns from the measure. Thus, a firm's high degree of crash risk does not imply that the firm's crashes tend to coincide with market downturns. And third, as an empirical matter, crash risk and risk asymmetry are not positively associated.

[5] See Stone [1973] and Fishburn [1977].

of equity—26 to 126 basis points higher. Similarly, Ashbaugh-Skaife et al. [2009] find that internal control weaknesses are associated with greater indiosyncratic and systematic risk, and that auditor-confirmed changes in internal control status are associated with significant changes in cost of capital—50 to 150 basis points, on average. Relatedly, Beneish, Billings, and Hodder [2008] document significant negative returns around internal control weakness announcements, especially for unaudited Sarbanes-Oxley Section 302 disclosures. My study offers a potential explanation for these patterns by providing evidence that reporting flexibility affects systematic risk in a manner that is positively priced (as per ACX), but not captured by commonly used multifactor models.

The remainder of the paper is organized as follows: in section 2, I develop and formally state my empirical predictions; in section 3, I detail the data sources, sample selection process, and variable construction underlying my empirical analysis; in section 4, I present my empirical analyses and discuss my findings; and in section 5, I conclude. Lastly, in appendix A, I summarize the motivating literature on the relation between reporting quality and cost of capital; in appendix B, I present variable definitions; and in the online appendix, I provide the stylized model from which my predictions derive, as well as additional tabulated empirical results.

## 2. *Hypothesis Development*

I combine two distinct branches of the accounting and finance literatures to form my prediction that reporting flexibility causes systematic risk asymmetry. I first rely on the estimation risk literature, and in particular Lambert, Leuz, and Verrecchia [2007], who demonstrate analytically that greater residual uncertainty regarding the value of a firm can cause the firm's assessed covariation with the market factor, $\beta$, to amplify.

Second, I look to the literature on managerial obfuscation, and the notion that firm managers who care about current stock price have a heightened incentive to obfuscate value-relevant information when current performance and/or future outlook is poor (e.g., Bloomfield [2002]). Consistent with this notion, Lang and Lundholm [1993], Walther [2000], and Miller [2002] find that managers of well-performing firms are more likely to be transparent in their disclosures. Similarly, Li [2008] finds that more opaque disclosures (as measured by the length and fog index of the MD&A section of the 10-K) are associated with lower earnings and less persistent positive earnings, whereas Kothari, Shu, and Wysocki [2009] find that managers tend to delay the release of bad news. Collectively, this evidence corroborates the notion that managerial incentives to obfuscate value-relevant information are negatively associated with firm performance.

Combining these literatures, I develop a stylized model built upon two crucial assumptions: (1) managers wish to obfuscate value-decreasing information, and promote value-increasing information; and (2) investors rely more heavily on aggregate information (e.g., the market factor) when firm-specific information is less precise. This stylized model, presented in the online appendix, provides an analytical derivation for each of my predictions. In what follows, I provide an intuitive explanation as to how these two forces, coupled with sufficient reporting flexibility, combine to generate risk asymmetry.

Investors typically have multiple sources of information to value a firm. Although firm-generated information (e.g., financial statements, earnings announcements, press releases, conferences calls, social media posts) is often the most relevant, it is not always the most reliable, as interested insiders can manipulate, withhold, or otherwise deprecate the information content for private gain. When a firm provides less useful information, rational investors must rely more heavily on other signals in order to value the firm. In effect, obfuscation causes the weights on other signals to amplify (e.g., Yee [2006], Lambert, Leuz, and Verrecchia [2007]).

Ample prior literature shows that firms selectively obfuscate when their own performance is poor (e.g., Lang and Lundholm [1993], Schrand and Walther [2000], Miller [2002], Li [2008], Kothari, Shu, and Wysocki [2009]). If a firm behaves this way, then factor exposures will tend to amplify when the firm is performing poorly. This amplification will, in turn, cause the firm to have a higher $\beta$ during downmarkets than during upmarkets, on average. The intuition for this effect is as follows:

For ease of exposition, consider a firm with a positive market exposure (i.e., $\beta > 0$). When the firm obfuscates, the market exposure amplifies to become more positive. Given the firm's positive market exposure, its own poor performance will, on average, tend to coincide with poor market performance. Thus, if the firm obfuscates its own poor performance, its market exposure will be more likely to amplify (i.e., become more positive) during downmarkets than during upmarkets. Hence, if the firm has the reporting flexibility necessary to obfuscate its own bad news, it will tend to exhibit risk asymmetry with downmarket $\beta >$upmarket $\beta$.

A similar effect holds for a firm with a negative market exposure (i.e., $\beta < 0$). In this case, the firm's poor performance will, on average, tend to coincide with strong market performance. Moreover, when the negative-$\beta$ firm obfuscates and its exposure amplifies, the firm's exposure to the market becomes more negative. Thus, if the firm obfuscates its own poor performance, its market exposure will be more likely to amplify (i.e., become more negative) during upmarkets than during upmarkets. Hence, if the firm has the reporting flexibility necessary to obfuscate its own bad news, a negative-$\beta$ firm will also exhibit risk asymmetry with downmarket $\beta >$upmarket $\beta$.[6]

[6] Suppose, for example, that a firm's $\beta$ amplifies by 100% when it obfuscates. Consider if this firm has a $\beta$ of $-1$, when not obfuscating. In this case, the $\beta$ decreases from $-1$ to $-2$ when the firm obfuscates. Given the firm's negative exposure to the market factor, the firm will tend to perform well (and therefore be less likely to obfuscate) when the market performs

Risk asymmetry vanishes as $\beta$ approaches zero. If $\beta \approx 0$, a rational investor will never rely on the market signal, no matter how imprecise other information sources become. Thus, although the sign of a firm's $\beta$ is irrelevant, the magnitude of the firm's $\beta$ is an important moderator of the relation between reporting flexibility and risk asymmetry; the greater the firm's market sensitivity (in either direction), the stronger the effect of reporting flexibility on risk asymmetry. Accordingly, my first two predictions are as follows:

P1: Reporting flexibility causes risk asymmetry to increase.
P2: P1 is stronger when $\boldsymbol{\beta}$ is greater in magnitude.

A more formal derivation of these predictions can be found in the online appendix. In forming these predictions, I do not assume that market performance enters into the firm's reporting decisions. Quite the opposite—I assume that firms' reporting policies are based solely on their own absolute performance, without regard to the market, or their performance relative to it.[7] Under this assumption, reporting precision and market performance correlate only insofar as the firm's performance is correlated with market performance.

However, one could imagine a firm with a different reporting incentive. For example, some firms may have greater incentives to obfuscate when their relative performance is poor. If firms benchmark against the market, and only wish to obfuscate when their performance is poor relative to expectations given the market, then the above logic will not hold. By design, a firm's relative performance is uncorrelated with market performance. Therefore, a firm that obfuscates on the basis of relative performance will not comove differentially with down- versus upmarkets. Under the assumption that managers are more likely to pay attention to relative performance when their compensation is tied to relative performance, I predict the following:

P3: Reporting flexibility is less positively associated with risk asymmetry when the CEO is paid on the basis of relative performance evaluation (RPE).

Prior literature documents that strong market performance leads firms to obfuscate (e.g., Povel, Singh, and Winton [2007], Wang, Winton, and

poorly. Therefore, on average, the firm's $\beta$ will be higher (i.e., less negative) during periods of poor market performance. For a graphical depiction of this intuition, see figures OA1, OA2, and OA3 of the online appendix.

[7] In the context of performance evaluation, "absolute performance" does not refer to the absolute value of performance, but rather the level of performance, absent any benchmark comparison. In contrast, "relative performance" refers to performance compared to a benchmark. For example, suppose a firm has a 10% total shareholder return ("TSR"), whereas its benchmark (e.g., the S&P 500 index) has a contemporaneous 8% TSR, the firm's absolute TSR performance would be 10% whereas its relative TSR performance would be $10\% - 8\% = 2\%$.

Yu [2010]).[8] On its surface, this may seem at odds with my assumptions and corresponding empirical predictions. However, this is not the case. The crucial assumption underlying my predictions is that managers' incentives to obfuscate decrease in their firms' absolute performance, as opposed to their firms' relative performance. If their incentives to obfuscate also increase in market performance, this would not affect my predictions. For example, consider the following reporting policy: Obfuscate if own performance is poor, or if market performance is strong. Such a reporting policy would generate risk asymmetry in qualitatively the same manner I predict, and explain the prevalence of fraud during market booms; the two are wholly consistent.[9]

## 3. Data, Sample, Constructs, and Proxies

In this section, I describe all the key constructs and define their proxies. Definitions for all variables, including the controls, can be found in appendix B.

### 3.1 DATA AND SAMPLE

My sample comes from nonfinancial firms in the intersection of the CRSP daily return files and the Compustat Annual Industrial files, over the period from 1987 to 2017. I drop firm-year observations if any of the following cannot be calculated: risk asymmetry, market value of equity, book-to-market ratio, debt-to-equity ratio, length of time since the firm first listed on a major exchange, trading volume, return volatility, return on assets, crash risk, and earnings downside risk. This yields a final sample of 129,261 firm-year observations from 14,681 unique firms. For the event study, I further employ hand-collected public float data, generously provided by Peter Iliev—the same data set used by Iliev [2010]. In some analyses, I further incorporate data on executive incentives from the Incentive Lab data set.

### 3.2 REPORTING FLEXIBILITY

I define the construct "reporting flexibility" as management's ability to obfuscate value-relevant information. This definition is related, but not identical, to similar terms from the prior literature. For example, Hann, Lu, and Subramanyam [2011] define "accounting flexibility" to be the discretion that is explicitly afforded to managers by GAAP (e.g., managerial discretion in estimating bad debt expense). I define reporting flexibility more broadly. My definition includes the discretion allowed by GAAP, but

[8] Strobl [2013] further provides an agency framework to explain, analytically, why some firms might prefer to manipulate earnings during strong market conditions.

[9] In the online appendix, I provide numerical simulations of risk asymmetry arising from such a policy. The simulated results are qualitatively equivalent to the case where the manager cares only about absolute own-firm performance, ignoring the market altogether.

further includes additional sources of flexibility such as management's ability to commit outright fraud, without being detected (e.g., because of weak audit oversight and/or poor internal controls). Simply put, reporting flexibility includes any source of leeway that allows the manager to obfuscate value-relevant information, whether legal or not. Barton and Simko [2002] present another related construct: managers' ability to optimistically bias earnings. The key conceptual difference is that Barton and Simko [2002] are concerned with bias, whereas I am interested in obfuscation. The two need not coincide.[10] From a capital market perspective, it is obfuscation, not bias, that affects information risk.

Conceptually, reporting flexibility is distinct from many commonly used information quality constructs (e.g., accruals quality, earnings quality, fraud, conditional conservatism) and their associated measures (e.g., "discretionary"/"abnormal" accruals, earnings-response coefficients, AAER issuances, earnings-return relation asymmetry) in that reporting flexibility takes an ex ante perspective on information quality. Reporting flexibility relates to how costly it would be to provide low-quality information to the capital market—whether or not the firm actually chooses to do so, ex post.

The advantage of this ex ante perspective is that it allows me to better separate the firm's reporting system from the underlying economic events on which the firm reports. Although ample reporting flexibility may—and likely does—lead to adverse reporting consequences (e.g., earnings management, fraud and unconservative accounting), examinations of these ex post outcomes often conflate the firm's reporting decisions and economic circumstances. The inability to separate the two has been a major criticism of the existing reporting quality literature (e.g., Dechow, Ge, and Schrand [2010], Gerakos [2012], Leuz and Wysocki [2016]).[11]

Consistent with prior literature (e.g., Cohen, Dey, and Lys [2008], Iliev [2010], Gallemore and Labro [2015], Bertomeu et al. [2020], Bird, Karolyi, and Ruchti [2018]), I assume that more stringent reporting/auditing requirements, as mandated by SOX 404, constitute a positive shock to the firm's information environment. In particular, I assume that treatment by SOX 404 impinges upon management's ability to obfuscate value-relevant information—that is, it reduces reporting flexibility. As my primary measure of reporting flexibility, I use *SOX 404*, a dummy variable that indicates whether or not the firm is a SOX 404 reporter. It takes a value of one if an

[10] As a canonical example, Stein [1989] models a firm facing price pressure from an efficient capital market. The firm biases, but does not obfuscate, its performance—investors are perfectly able to unwind the bias.

[11] For example, highly volatile accruals can arise because of earnings management or high operating volatility (e.g., Nichols [2006], Gerakos [2012]). Similarly, an earnings-response coefficient can reflect both the quality of the firm's reports and the persistence of the firm's economic performance (e.g., Dechow, Ge, and Schrand [2010]), whereas an asymmetric earnings-return relation can reflect conditionally conservative accounting practices (e.g., Basu [1997]) or the abandonment/curtailment of low NPV ventures (e.g., Hayn [1995], Lawrence, Sloan, and Sun [2018]).

auditor offered an opinion regarding the firm's internal controls (Compustat variable $AUOPIC$) and zero otherwise.[12]

Being a SOX 404 non-reporter does not imply that the firm is flouting its regulatory requirements. For the vast majority of cases in my sample, a firm-year observation without an audit opinion regarding internal controls corresponds to a firm that faces no such mandate, either because the regulation has not yet come into force (e.g., years prior to 2004), or because the firm is small enough to be exempt (e.g., firms with public floats below \$75 million).

I also use internal control strength as an additional measure of reporting flexibility. Consistent with prior literature, I posit that internal control weaknesses make it easier for management to obfuscate value-relevant information. Conditional on being a SOX 404 reporter, $ICW$ is a dummy variable that takes a value of zero if an auditor assesses the internal controls as "effective" ($AUOPIC = 1$) and one otherwise. This measure can only be constructed for SOX 404 reporters.

### 3.3 RISK ASYMMETRY

Using CRSP's daily stock return data, I estimate three $\beta$'s for each firm-fiscal year observation: $\hat{\beta}$, $\hat{\beta}^+$, and $\hat{\beta}^-$, defined by:

$$\hat{\beta} \equiv \frac{Cov(r_i, r_m)}{Var(r_m)}, \tag{1}$$

$$\hat{\beta}^+ \equiv \frac{Cov(r_i, r_m | r_m \geq 0)}{Var(r_m | r_m \geq 0)}, \tag{2}$$

$$\hat{\beta}^- \equiv \frac{Cov(r_i, r_m | r_m < 0)}{Var(r_m | r_m < 0)}, \tag{3}$$

where $r_i$ is the firm's excess stock return and $r_m$ is the contemporaneous excess return on the value-weighted market portfolio. I define $Risk\ Asymmetry$ as the difference,

$$Risk\ Asymmetry_{i,t} \equiv \beta^-_{i,t} - \beta^+_{i,t}. \tag{4}$$

In supplemental tests, I further decompose $Risk\ Asymmetry$ into two component parts: $Relative\ Downside\ Risk$ and $Relative\ Upside\ Risk$. Following ACX, I define them as follows:

$$Relative\ Downside\ Risk_{i,t} \equiv \hat{\beta}^-_{i,t} - \hat{\beta}_{i,t}, \tag{5}$$

$$Relative\ Upside\ Risk_{i,t} \equiv \hat{\beta}^+_{i,t} - \hat{\beta}_{i,t}. \tag{6}$$

As the estimates of upmarket and downmarket $\beta$'s can be unduly influenced by a handful of extremely good or bad market return days, I exclude

[12] As firms differ with respect to their SOX 404 adoption dates, with some firms never adopting, this variable is not subsumed by year fixed effects.

the highest 1% and lowest 1% of market return days, prior to estimating these parameters. This trimming procedure improves the reliability of the parameter estimates, and ensures that any documented asymmetry is not attributable to asymmetries in tail events.[13] Because of the kurtosis of the estimates, I winsorize $\hat{\beta}$, $\hat{\beta}^{+}$, and $\hat{\beta}^{-}$ at 1% and 99% before constructing my measures of risk asymmetry, relative downside risk and relative upside risk.

### 3.4 MARKET SENSITIVITY

The predicted relation between reporting flexibility and risk asymmetry depends on the strength of the relation between the market and the firm. If the firm's performance is uncorrelated with that of the market, I do not expect reporting flexibility would have any bearing on risk asymmetry. In contrast, if the firm's performance is highly sensitive to the market, I predict reporting flexibility will manifest in risk asymmetry. I measure market sensitivity as $\hat{\beta}^2$. In some analyses, I use sensitivity only as a partitioning variable. When I include it as a regressor, I use the natural logarithm, because of its skewness.[14]

### 3.5 REPORTING INCENTIVES: ABSOLUTE VERSUS RELATIVE

The predicted relation between reporting flexibility and risk asymmetry requires that management considers absolute performance, as opposed to relative performance, in deciding whether or not to obfuscate. If management considers performance relative to expectations given the market, no asymmetry is predicted to arise. I proxy for these reporting incentives with the indicator variable *RPE*, which takes a value of one if the CEO has compensation explicitly tied to relative performance objectives, as indicated by Incentive Lab (i.e., $performancetype =$ "*Rel*").[15]

## 4. Empirical Analyses

I divide my empirical analyses into three primary categories. In section 4.1, I examine whether having an auditor assessment of internal controls (i.e., being a SOX 404 reporter) is related to risk asymmetry. In section 4.2, I restrict the sample to include only SOX 404 reporters, and

[13] My results are not particularly sensitive to this design choice. In tables OA1 through OA6 of the online appendix, I replicate my regression analyses without any trimming, as well as with a more aggressive 5% trim level, and document largely similar results.

[14] In my analyses, $\hat{\beta}^2$ and $|\hat{\beta}|$ are functionally equivalent. As partitioning variables, there is no difference between the two, as $\hat{\beta}^2$ and $|\hat{\beta}|$ are monotonically related. As log-transformed regressors, the two are linearly dependent, so converting between the two approaches is as simple as scaling the estimated coefficients by 2 or 1/2. I choose $\hat{\beta}^2$ rather than $|\hat{\beta}|$ because it arises most naturally from the model, as presented in the online appendix.

[15] Some firms use hybridized grants which are partially relative incentives and partially absolute incentives (i.e., $performancetype =$ "*AbsRel*", in Incentive Lab). I do not consider such grants when constructing the *RPE* variable, as their joint use of absolute and relative incentives makes the predictions unclear.

examine whether variation in assessed internal control strength explains variation in risk asymmetry. In section 4.3, I present sensitivity tests and supplemental analyses.

### 4.1 SOX 404 REPORTING AND RISK ASYMMETRY

I first examine the empirical relation between SOX 404 reporting and risk asymmetry. I tabulate descriptive statistics for all variables used in these analyses in table 1. Panel A summarizes the distributions of all the variables; panel B presents the means and standard deviations of each variable, split by SOX 404 reporting status; and panel C presents a correlation matrix of all variables examined with Pearson (Spearman) correlations below (above) the diagonal.

On average, firms in my sample exhibit a risk asymmetry of 0.243. Risk asymmetry is most pronounced among SOX 404 non-reporters, with an average risk asymmetry of 0.310, compared to only 0.062 among SOX 404 reporters. A related pattern emerges for relative upside and downside risks; SOX 404 reporters exhibit significantly less relative downside risk and significantly more relative upside risk.

Although these univariate comparisons align with my predictions, there are many omitted time-series and cross-sectional characteristics that could explain these patterns. In what follows, I address these potential issues first with ordinary least squares ("OLS") regressions, in section 4.1.1, and then with an event study "RDD," in section 4.1.2.

*4.1.1. Tests of Association.* I use OLS regressions to examine the statistical association between SOX 404 reporting and risk asymmetry, with variants on the following regression specification:

$$Risk\ Asymmetry_{i,t} = \phi SOX\ 404_{i,t} + \boldsymbol{\lambda} X_{i,t} + \tau_t + \mu_i + \varepsilon_{i,t}, \tag{7}$$

where $SOX\ 404_{i,t}$ is a dummy variable indicating if firm $i$ is a SOX 404 reporter in year $t$, $X_{i,t}$ are firm-fiscal year controls for firm size, book-to-market, capital structure, firm age, liquidity, volatility, profitability, crash risk, and earnings downside risk, and $\tau$ and $\mu$ are year and cross-sectional fixed effects (either industry or firm). In all of my regression analyses, the year fixed effects are defined based on the combination of fiscal year, and fiscal year-end month. In this way, all observations corresponding to the same year fixed effect also correspond to the same 12-month period.

In latter specifications, I augment the specification by interacting $SOX404$ with market sensitivity, $log(\hat{\beta}^2_{i,t})$:

$$\begin{aligned} Risk\ Asymmetry_{i,t} = {} & \phi_1 SOX\ 404_{i,t} + \phi_2 SOX\ 404_{i,t} \times log(\hat{\beta}^2_{i,t}) \\ & + \boldsymbol{\lambda} X_{i,t} + \tau_t + \mu_i + \varepsilon_{i,t}. \end{aligned} \tag{8}$$

I use a variety of fixed effect structures in these analyses: year, year + industry (defined by the Fama and French 48 industry classification), and year + firm (defined by PERMCO). Although these analyses are primarily descriptive, the inclusion of firm fixed effects aids in identification by

TABLE 1
*Descriptive Statistics*

Panel A: Summary statistics

| Variable | $N$ | Mean | $SD$ | Q1 | Med. | Q3 |
|---|---|---|---|---|---|---|
| *SOX 404* | 129,261 | 0.272 | 0.445 | 0.000 | 0.000 | 1.000 |
| *Risk Asymmetry* | 129,261 | 0.243 | 1.114 | −0.315 | 0.149 | 0.730 |
| *Relative Downside* | 129,261 | 0.116 | 0.705 | −0.230 | 0.065 | 0.418 |
| *Relative Upside Risk* | 129,261 | −0.127 | 0.730 | −0.443 | −0.079 | 0.223 |
| $log(\hat{\beta}^2)$ | 129,261 | −1.022 | 2.041 | −1.869 | −0.558 | 0.346 |
| *logSize* | 129,261 | 12.149 | 2.248 | 10.520 | 12.068 | 13.693 |
| *logAge* | 129,261 | 2.124 | 1.207 | 1.365 | 2.270 | 3.017 |
| *logBTM* | 129,261 | −0.565 | 1.192 | −1.203 | −0.605 | −0.043 |
| *logDTE* | 129,261 | −7.060 | 1.203 | −7.816 | −6.994 | −6.322 |
| *logLiq.* | 129,261 | 9.648 | 2.085 | 8.222 | 9.677 | 11.090 |
| *logVol.* | 129,261 | −2.070 | 0.610 | −2.477 | −2.073 | −1.674 |
| *ROA* | 129,261 | −0.041 | 0.276 | −0.052 | 0.029 | 0.074 |
| *Crash* | 129,261 | 0.143 | 0.351 | 0.000 | 0.000 | 0.000 |
| *EDR* | 129,261 | 0.009 | 0.078 | −0.027 | 0.000 | 0.017 |

Panel B: Variable means, split by SOX 404 reporting

| | SOX 404 Non-reporters | | SOX 404 Reporters | | |
|---|---|---|---|---|---|
| | Obs. = 94,128 | | Obs. = 35,133 | | Difference |
| | (1) | (2) | (3) | (4) | (5) |
| Variable | Mean | SD | Mean | SD | (3)–(1) |
| *Risk Asymmetry* | 0.310 | 1.228 | 0.062 | 0.697 | −0.248*** |
| *Relative Downside Risk* | 0.158 | 0.779 | 0.005 | 0.433 | −0.153*** |
| *Relative Upside Risk* | −0.153 | 0.805 | −0.056 | 0.465 | 0.097*** |
| $log(\hat{\beta}^2)$ | −1.417 | 2.156 | 0.038 | 1.153 | 1.455*** |
| *logSize* | 11.529 | 2.088 | 13.812 | 1.766 | 2.283*** |
| *logAge* | 1.944 | 1.235 | 2.608 | 0.975 | 0.664*** |
| *logBTM* | −0.520 | 1.220 | −0.685 | 1.104 | −0.165** |
| *logDTE* | −7.089 | 1.221 | −6.982 | 1.152 | 0.107** |
| *logLiq.* | 9.018 | 1.883 | 11.334 | 1.610 | 2.316*** |
| *logVol.* | −1.994 | 0.619 | −2.274 | 0.536 | −0.280*** |
| *ROA* | −0.057 | 0.300 | 0.002 | 0.195 | 0.059*** |
| *Crash* | 0.118 | 0.322 | 0.212 | 0.409 | 0.094*** |
| *EDR* | 0.012 | 0.084 | 0.001 | 0.060 | −0.011*** |

Panel C: Correlations

| | | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|---|
| (1) | *SOX 404* | | −0.117 | −0.114 | 0.065 | 0.369 | 0.461 | 0.240 |
| (2) | *Risk Asymmetry* | −0.099 | | 0.734 | −0.752 | −0.050 | −0.123 | −0.105 |
| (3) | *Relative Downside Risk* | −0.096 | 0.768 | | −0.201 | −0.052 | −0.089 | −0.085 |
| (4) | *Relative Upside Risk* | 0.059 | −0.785 | −0.206 | | 0.026 | 0.097 | 0.074 |
| (5) | $log(\hat{\beta}^2)$ | 0.317 | −0.027 | −0.042 | 0.000 | | 0.411 | −0.007 |
| (6) | *logSize* | 0.452 | −0.103 | −0.083 | 0.077 | 0.374 | | 0.298 |
| (7) | *logAge* | 0.245 | −0.096 | −0.082 | 0.068 | 0.003 | 0.290 | |
| (8) | *logBTM* | −0.062 | −0.036 | −0.021 | 0.035 | −0.151 | −0.284 | 0.078 |

(*Continued*)

**TABLE 1**—*(Continued)*

Panel C: Correlations

| | | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|---|
| (9) | *logDTE* | 0.040 | −0.028 | −0.020 | 0.023 | −0.033 | 0.087 | 0.179 |
| (10) | *logLiq.* | 0.494 | −0.045 | −0.042 | 0.029 | 0.525 | 0.760 | 0.119 |
| (11) | *logVol.* | −0.204 | 0.116 | 0.091 | −0.088 | 0.135 | −0.404 | −0.304 |
| (12) | *ROA* | 0.095 | −0.058 | −0.042 | 0.048 | 0.004 | 0.271 | 0.194 |
| (13) | *Crash* | 0.120 | −0.024 | −0.026 | 0.012 | 0.022 | 0.071 | 0.062 |
| (14) | *EDR* | −0.062 | 0.057 | 0.045 | −0.043 | 0.022 | −0.178 | −0.054 |

| | | (8) | (9) | (10) | (11) | (12) | (13) | (14) |
|---|---|---|---|---|---|---|---|---|
| (1) | *SOX 404* | −0.077 | 0.035 | 0.505 | −0.214 | 0.104 | 0.120 | −0.034 |
| (2) | *Risk Asymmetry* | −0.021 | −0.027 | −0.073 | 0.117 | −0.056 | −0.027 | 0.044 |
| (3) | *Relative Downside Risk* | −0.011 | −0.022 | −0.059 | 0.087 | −0.033 | −0.025 | 0.032 |
| (4) | *Relative Upside Risk* | 0.022 | 0.019 | 0.053 | −0.087 | 0.050 | 0.012 | −0.033 |
| (5) | $log(\hat{\beta}^2)$ | −0.208 | −0.065 | 0.579 | 0.167 | 0.036 | 0.032 | 0.042 |
| (6) | *logSize* | −0.313 | 0.099 | 0.760 | −0.411 | 0.371 | 0.080 | −0.117 |
| (7) | *logAge* | 0.095 | 0.180 | 0.122 | −0.360 | 0.191 | 0.055 | −0.071 |
| (8) | *logBTM* | | 0.000 | −0.277 | −0.052 | −0.117 | 0.031 | −0.029 |
| (9) | *logDTE* | −0.063 | | 0.030 | −0.128 | −0.086 | 0.016 | 0.169 |
| (10) | *logLiq.* | −0.248 | 0.032 | | −0.026 | 0.091 | 0.104 | 0.051 |
| (11) | *logVol.* | −0.080 | −0.104 | −0.008 | | −0.372 | −0.040 | 0.194 |
| (12) | *ROA* | 0.145 | 0.020 | 0.019 | −0.324 | | 0.013 | −0.489 |
| (13) | *Crash* | 0.018 | 0.017 | 0.089 | −0.045 | 0.027 | | −0.033 |
| (14) | *EDR* | −0.110 | 0.108 | 0.027 | 0.265 | −0.466 | −0.045 | |

This table presents descriptive statistics for all the variables included in the SOX 404 regressions. The sample spans fiscal years 1987–2017 and includes all nonfinancial firms in the intersection of CRSP and Compustat for which all the summarized variables can be constructed. Panel A presents summary statistics for the entire sample; panel B presents variable means and standard deviations split by status as a SOX 404 Reporter versus Non-reporter; and panel C presents Pearson correlations (below the diagonal) and Spearman correlations (above the diagonal).

ruling out any time-invariant firm-level endogeneity concerns (e.g., time-invariant business model, or operating volatility), because coefficients are identified purely from intrafirm time-series variation in reporting flexibility, risk asymmetry, and the control variables. However, firm fixed effects will not address the problems created by time-varying correlated omitted variables, thus the estimated coefficients do not necessarily reflect the true causal effect of SOX 404 reporting on risk asymmetry.[16] The results from these OLS analyses can be found in table 2.

Across all six analyses, I find that SOX 404 reporting is significantly negatively associated with risk asymmetry. Without controls, SOX 404 reporting is associated with 0.208 less risk asymmetry. The coefficient remains largely unaffected by the inclusion of industry fixed effects, but shrinks by a little over half with the addition of control variables. With firm fixed effects, the coefficient attenuates somewhat more, but remains economically and statistically significant.

[16] For tighter causal attribution, see the event study in section 4.1.2.

TABLE 2

*Association Between SOX 404 Reporting and Risk Asymmetry*

| | | Outcome = *Risk Asymmetry* | | | | | |
|---|---|---|---|---|---|---|---|
| Variables | Pred. | (1) | (2) | (3) | (4) | (5) | (6) |
| *SOX 404* | – | −0.208*** | −0.188*** | −0.088*** | −0.059** | −0.093*** | −0.066*** |
| | | (−12.820) | (−10.031) | (−4.239) | (−2.741) | (−4.482) | (−3.065) |
| *SOX 404* × $log(\hat{\beta}^2)$ | – | | | | | −0.044*** | −0.031** |
| | | | | | | (−4.028) | (−2.534) |
| $log(\hat{\beta}^2)$ | | | | −0.021*** | −0.022*** | −0.017*** | −0.019*** |
| | | | | (−5.118) | (−4.810) | (−4.567) | (−4.411) |
| *logSize* | | | | −0.037*** | −0.039*** | −0.038*** | −0.040*** |
| | | | | (−5.908) | (−3.639) | (−6.004) | (−3.660) |
| *logAge* | | | | −0.032*** | −0.058*** | −0.032*** | −0.057*** |
| | | | | (−3.275) | (−2.900) | (−3.191) | (−2.836) |
| *logBTM* | | | | −0.024*** | −0.056*** | −0.024*** | −0.056*** |
| | | | | (−3.567) | (−4.239) | (−3.575) | (−4.239) |
| *logDTE* | | | | −0.015*** | −0.030*** | −0.014** | −0.030*** |
| | | | | (−2.838) | (−3.091) | (−2.728) | (−3.074) |
| *logLiq.* | | | | 0.044*** | 0.063*** | 0.044*** | 0.064*** |
| | | | | (8.517) | (7.383) | (8.554) | (7.346) |
| *logVol.* | | | | 0.156*** | 0.143*** | 0.158*** | 0.143*** |
| | | | | (8.339) | (6.612) | (8.598) | (6.723) |
| *ROA* | | | | 0.027 | 0.011 | 0.029 | 0.013 |
| | | | | (0.872) | (0.391) | (0.949) | (0.444) |
| *Crash* | | | | −0.032*** | −0.007 | −0.031*** | −0.008 |
| | | | | (−4.189) | (−1.080) | (−4.176) | (−1.097) |
| *EDR* | | | | 0.282*** | 0.203** | 0.281*** | 0.205** |
| | | | | (3.241) | (2.453) | (3.214) | (2.461) |
| Fixed effects | | Year | Year, Industry | Year, Industry | Year, Firm | Year, Industry | Year, Firm |
| Observations | | 129,261 | 129,261 | 129,261 | 129,261 | 129,261 | 129,261 |
| *R*-squared | | 0.026 | 0.030 | 0.046 | 0.184 | 0.046 | 0.184 |

This table presents OLS analyses on the association between SOX 404 reporting and risk asymmetry, for the full sample. The sample spans fiscal years 1987–2017. Specification 1 includes year fixed effects; specification 2 includes year and industry fixed effects; specification 3 includes year and industry fixed effects and firm-year controls; and specification 4 includes year and firm fixed effects and firm-year controls. Specifications 5 and 6 are identical to specifications 3 and 4, but for the inclusion of an interaction term between *SOX* 404 and $log(\hat{\beta}^2)$. Across all specifications, the dependent variable is risk asymmetry. All significance levels are based on two-tailed tests. Below each coefficient is a *t*-statistic, in parentheses, calculated based on standard errors clustered by industry and fiscal year.

To put these magnitudes in perspective, ACX document that, after controlling for size, book-to-market, and momentum, a one-unit difference in *Risk Asymmetry* is associated with a 250–640 basis point difference in the cost of capital (ACX table 2, specification 3). With a back-of-the-envelope calculation, taking ACX's findings as given, a 0.059 decrease in *Risk Asymmetry* (as documented in table 2, specification 4) might be associated with a roughly 15–38 basis point per annum reduction in the cost of capital. However, I caution that this is not an explicit test of any cost of capital effect.

In specifications 5 and 6, I replicate the analyses from specifications 3 and 4, but further include an interaction between *SOX 404* and $log(\hat{\beta}^2)$. I find that the association between SOX 404 reporting and risk asymmetry

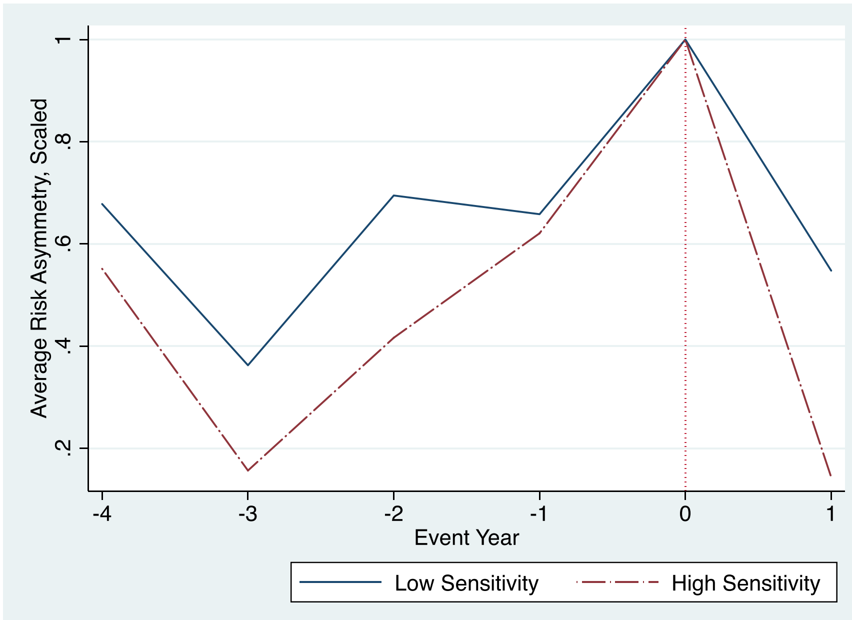


FIG 1.—Time series of risk asymmetry around SOX 404 adoption, by sensitivity. This figure plots the time series of risk asymmetry, in event time, around firms' SOX 404 adoption dates, split at the median by market sensitivity (i.e., $\hat{\beta}^2$) at year $t = 1$. Along the $y$-axis, risk asymmetry is scaled by the year $t = 0$ level. The faint, vertical line represents the transition to SOX 404 reporting. To avoid contamination from changes in sample composition, firms are only included if they are present in the sample for each event year from $t = -4$ to $t = 1$.

is significantly more pronounced among more market-sensitive firms. For the average firm in my sample, a one standard deviation increase in market sensitivity is associated with a roughly 100% increase in the slope of the relation between *SOX* 404 and *Risk Asymmetry*.

In figure 1, I present a graphical representation of these results, using an event-time plot of *Risk Asymmetry* around each firm's SOX 404 adoption year. I split firms, at the median, into "high sensitivity" and "low sensitivity" groups, based on estimates of $\beta^2$ from firms' first year after SOX 404 adoption. I then plot the average level of risk asymmetry for the years surrounding SOX 404 adoption. To facilitate easy comparison, I scale risk asymmetry by each groups' year 0 level. As shown in figure 1, risk asymmetry moves roughly in parallel across high- and low-sensitivity groups, in the years leading up to SOX 404 adoption. From year 0 to year 1, risk asymmetry drops substantially across both groups, but the effect is more pronounced among the more market-sensitive firms. Firms of below-median (above-median) market sensitivity experience a ~35% (~85%) drop in *Risk Asymmetry*, in the first year of SOX 404 reporting.

Collectively, the regression analyses and the figure convey a consistent narrative: SOX 404 reporting is associated with decreased risk asymmetry, especially among more market-sensitive firms. However, I caveat that these analyses do not exploit any type of plausibly exogenous variation in SOX 404 reporting, and should therefore be interpreted cautiously as evidence of an association, but not necessarily a causal relation.

*4.1.2. Main Analysis: Sarbanes-Oxley Section 404 Event Study.* In order to sidestep concerns that the results from the OLS analyses can be attributed to correlated omitted variables, I exploit plausibly exogenous variation in firms' exposure to SOX 404 requirements and examine how risk asymmetry changes in response. Naïve pre/post analyses of one-off events (like regulatory changes) without a sensible control group are easily confounded by contemporaneous shocks. Moreover, changes in capital market regulation are typically endogenous responses to market conditions—SOX 404, itself, was a response to a spate of high-profile accounting scandals, including those of Enron and WorldCom. In order to defend the claim of "plausible exogeneity," I follow Iliev's [2010] regression discontinuity methodology and use a difference-in-differences design to identify the causal effect of reporting flexibility on risk asymmetry. Specifically, I exploit SOX 404's differential impact on firms on either side of the $75 million public float threshold.

Firms with public floats in excess of $75 million ("accelerated filers") were subject to much more stringent audit requirements, beginning for year-ends on or after November 15, 2004. Firms with smaller public floats ("non-accelerated filers") were not. This cutoff separating treated and untreated firms provides a convenient research design—the firms just under the threshold become control firms, whereas the firms just above become the treatment firms. With a tight enough bandwidth, differential outcomes between treatment and control groups can be plausibly attributed to the treatment, rather than contemporaneous confounding events (which would arguably affect both groups uniformly). This econometric technique is commonly referred to as a "regression discontinuity design." The key assumption behind such a design is that treatment and control firms are "as good as randomly" assigned. That is, treatment does not systematically befall a firm based on relevant characteristics.

Such an assumption is easily violated. In particular, Gao, Wu, and Zimmerman [2009] and Iliev [2010] document that after the announcement of the $75 million public float threshold, the concentration of firms with public floats of just under $75 million rises dramatically, whereas the concentration of firms with public floats of just over $75 million drops precipitously. This evidence suggests that certain firms are selecting into being non-accelerated filers by managing their public floats, thereby invalidating the assumption of 'as good as random' assignment. In order to address this, I follow Iliev [2010] and use firms' 2002 public floats—before the threshold announcement—as an instrument for whether or not the firm

**TABLE 3**
*Descriptive Statistics by Accelerated Filer Status*

| | Non-Accelerated Filers | | Accelerated Filers | | Difference |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Variable | Mean | *SD* | Mean | *SD* | (3)–(1) |
| *Risk Asymmetry* | 0.186 | 0.916 | 0.235 | 0.882 | 0.049 |
| *Relative Downside Risk* | 0.140 | 0.666 | 0.144 | 0.582 | 0.004 |
| *Relative Upside Risk* | −0.046 | 0.624 | −0.091 | 0.563 | −0.045 |
| $log(\hat{\beta}^2)$ | −2.632 | 2.537 | −1.066 | 2.187 | 1.566* |
| *logSize* | 10.927 | 0.797 | 11.636 | 0.635 | 0.709 |
| *logAge* | 2.165 | 0.928 | 1.982 | 1.050 | −0.183 |
| *logBTM* | −0.439 | 1.073 | −0.526 | 0.890 | −0.087 |
| *logDTE* | −7.304 | 1.273 | −7.691 | 1.306 | −0.387* |
| *logLiq.* | 8.527 | 1.473 | 9.423 | 1.514 | 0.896* |
| *logVol.* | −1.794 | 0.620 | −1.770 | 0.603 | 0.024 |
| *ROA* | −0.118 | 0.324 | −0.126 | 0.279 | −0.008 |
| *Crash* | 0.092 | 0.290 | 0.186 | 0.390 | 0.094 |
| *EDR* | 0.043 | 0.115 | 0.038 | 0.118 | −0.005 |

This table presents pretreatment descriptive statistics for the treatment and control groups used in the RDD sample. The sample is restricted to only include firms whose 2004 public floats were within $20 million of the $75 million public float threshold. Firms whose 2002 public floats are above (below) $75 million are considered to be accelerated (non-accelerated) filers. Summary statistics come from fiscal years 2001–03.

will become treated at the end of 2004. Arguably, 2002 public floats are not managed, because there was no incentive to keep public floats below $75 million at that time. In what follows, I refer to firms as "accelerated filers" ("non-accelerated filers") if their 2002 public float was above (below) $75 million. To minimize the differences between the two groups, I restrict the sample to the small subset of firms within a bandwidth of $20 million of the $75 million threshold.

To assess the pretreatment comparability of the accelerated and non-accelerated filers, I examine the pretreatment distributions of risk asymmetry, relative downside and upside risks, and the control variables. I find that they are nearly identical with respect to risk asymmetry, relative downside risk and relative upside risk, and largely homogenous across all of the control variables. For 3 of the 10 control variables (market sensitivity, debt-to-equity, and liquidity), accelerated and non-accelerated filers are marginally different at the 10% level, but the remaining seven variables show no significant differences. Compared to the stark differences documented in table 1, panel B, these descriptives suggest that the RDD setting dramatically reduces the differences between treatment and control observations. These results are presented in table 3.

I use variants on the following difference-in-differences specification to estimate the causal effect of SOX 404 reporting on risk asymmetry:

$$Risk\ Asymmetry_{i,t} = \phi Acc.\ Filer_i \times Post_t + \boldsymbol{\lambda} X_{i,t} + \tau_t + \mu_i + \varepsilon_{i,t}, \qquad (9)$$

where $Acc.\ Filer$ is a dummy variable equal to one if the firm's 2002 public float exceeds $75 million, *Post* is an indicator variable that takes a value of one if SOX 404 has already come into effect for accelerated filers and zero otherwise, and $X_{i,t}$ are firm-year controls. Lastly, $u_i$ and $\tau_t$ represent firm and year fixed effects.[17] Results are presented in table 4.

I present 10 regressions, which differ across two dimensions: (1) the inclusion of control variables; and (2) the sample. In panel A (panel B), I exclude (include) all of the control variables. Within each panel, specifications differ with respect to the sample. In specification 1, I include the full sample. In specification 2 (specification 3), I restrict the sample by including only observations of above-median (below-median) market sensitivity, as measured by $\hat{\beta}^2$. Specification 4 presents a statistical test of differences between specifications 2 and 3. Specifications 5 through 7 are identical to specifications 2 through 4, but use industry-level average market sensitivity (defined based on Fama and French 48 industries) to assign firms to high- versus low-sensitivity samples.

I find that being an accelerated filer is associated with a statistically and economically significant post–SOX 404 reduction in risk asymmetry. The magnitude of the estimated treatment effect is substantial, at around 0.3 ($\sim$0.27 standard deviations). Moreover, as predicted, the estimated effect is greater among more market-sensitive firms. In specification 2 (specification 3) of panel A, I find that the estimated treatment effect is roughly 70% larger (smaller) than for the full sample. Moreover, absent control variables, the differences across high- versus low-sensitivity samples are statistically significant at the 1% level. With the inclusion of control variables in panel B, the estimated differences across high- versus low-sensitivity samples remain economically large, but lose statistical significance at conventional inference levels ($t$-statistics of $-1.491$ and $-1.545$). Collectively, these results provide support for my predictions by showing that: (1) relative to otherwise similar firms, risk asymmetry drops when firms become SOX 404 reporters; and (2) the posttreatment drop in risk asymmetry is more pronounced among more market-sensitive firms.

To put the magnitude of the effect into perspective, I look to ACX who document that a one-unit change in risk asymmetry is associated with a 250–640 basis point change in the cost of capital. Using the same back-of-the-envelope calculation as before, a 0.30 decrease in risk asymmetry might be associated with a 75–192 basis point reduction in the cost of capital. Although fairly large, this is quite comparable to the cost of capital effects of poor internal controls documented by Ogneva, Subramanyam, and Raghunandan [2007] and Ashbaugh-Skaife et al. [2009].

I caveat that my analyses do not constitute an explicit estimate of any cost of capital effects. Rather, they demonstrate a plausibly causal link between

[17] Note that *Post* and $Acc.\ Filer$ main effects are implicitly included in the regression specification via the firm and year fixed effects.

**TABLE 4**
*Effect of SOX 404 on Risk Asymmetry*

Panel A: No Controls

| Variables | Pred. | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|---|
| | | Outcome = Risk Asymmetry | | | | | | |
| | | Full Sample | High $\hat{\beta}^2$ | Low $\hat{\beta}^2$ | (2)–(3) | High $\hat{\beta}^2$ | Low $\hat{\beta}^2$ | (5)–(6) |
| *Acc. Filer* × *Post* | – | −0.302*** | −0.516*** | −0.096 | −0.420*** | −0.416*** | −0.069 | −0.347*** |
| | | (−4.298) | (−4.803) | (−1.009) | (−5.951) | (−3.592) | (−0.577) | (−4.322) |
| Fixed Effects | | Year, Firm | Year, Firm | Year, Firm | | Year, Firm | Year, Firm | |
| Observations | | 1,855 | 928 | 927 | | 907 | 948 | |
| *R*-squared | | 0.231 | 0.317 | 0.361 | | 0.261 | 0.255 | |

Panel B: With Controls

| VARIABLES | Pred. | (1) Full Sample | (2) High $\hat{\beta}^2$ | (3) Low $\hat{\beta}^2$ | (4) (2)–(3) | (5) High $\hat{\beta}^2$ | (6) Low $\hat{\beta}^2$ | (7) (5)–(6) |
|---|---|---|---|---|---|---|---|---|
| | | Outcome = *Risk Asymmetry* | | | | | | |
| *Acc. Filer* × *Post* | – | −0.287** | −0.516*** | −0.168 | −0.347 | −0.359* | −0.069 | −0.290 |
| | | (−2.816) | (−3.549) | (−1.200) | (−1.491) | (−1.949) | (−0.477) | (−1.545) |
| $log(\hat{\beta}^2)$ | | −0.036* | −0.079 | −0.031** | | −0.034 | −0.044** | |
| | | (−2.273) | (−0.936) | (−2.746) | | (−1.390) | (−2.371) | |
| *logSize* | | 0.024 | 0.061 | −0.017 | | 0.023 | 0.063 | |
| | | (0.354) | (0.544) | (−0.123) | | (0.238) | (1.130) | |
| *logAge* | | −0.158 | −0.132 | −0.231 | | −0.332** | −0.019 | |
| | | (−1.550) | (−1.136) | (−1.180) | | (−2.535) | (−0.256) | |

(*Continued*)

**TABLE 4**—*(Continued)*

Panel B: With Controls

| | | Outcome = *Risk Asymmetry* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| VARIABLES | Pred. | Full Sample | High $\hat{\beta}^2$ | Low $\hat{\beta}^2$ | (2)−(3) | High $\hat{\beta}^2$ | Low $\hat{\beta}^2$ | (5)−(6) |
| *logBTM* | | 0.030 | 0.010 | 0.005 | | 0.080 | −0.038 | |
| | | (0.417) | (0.131) | (0.033) | | (0.657) | (−0.746) | |
| *logDTE* | | −0.011 | 0.002 | −0.180 | | 0.047 | −0.082 | |
| | | (−0.179) | (0.022) | (−1.651) | | (0.511) | (−1.640) | |
| *logLiq.* | | 0.026 | 0.045 | −0.038 | | 0.016 | 0.009 | |
| | | (0.704) | (0.561) | (−0.482) | | (0.332) | (0.146) | |
| *logVol.* | | 0.307*** | 0.338*** | 0.353*** | | 0.251** | 0.346*** | |
| | | (4.988) | (8.984) | (5.305) | | (3.223) | (3.948) | |
| *ROA* | | 0.467** | 0.209 | 0.750** | | 0.442** | 0.105 | |
| | | (2.736) | (1.067) | (2.537) | | (2.446) | (0.178) | |
| *Crash* | | −0.019 | −0.147 | 0.059 | | −0.092 | −0.024 | |
| | | (−0.197) | (−1.251) | (0.516) | | (−0.700) | (−0.192) | |
| *EDR* | | 0.778 | 0.658 | 1.558 | | 0.544 | 0.004 | |
| | | (1.218) | (0.631) | (1.194) | | (0.812) | (0.004) | |
| Fixed Effects | | Year, Firm | Year, Firm | Year, Firm | | Year, Firm | Year, Firm | |
| Observations | | 1,855 | 928 | 927 | | 907 | 948 | |
| *R*-squared | | 0.255 | 0.335 | 0.415 | | 0.281 | 0.282 | |

This table presents the event study difference-in-differences results. I exploit the differential impact of SOX 404 on firms with public floats just above versus below $75 million and use a regression discontinuity design to identify the effect of SOX 404 on risk asymmetry. The sample spans fiscal years 2001−09 and is restricted to only include firms whose 2004 public floats were within $20 million of the $75 million public float threshold. Panel A (panel B) excludes (includes) control variables. Within each panel, specifications differ with respect to sample. Specification 1 includes the full sample. In the remaining specifications, the sample is restricted based on market sensitivity, $\hat{\beta}^2$. Specification 2 (specification 3) includes only the firm-year observations of above-median (below-median) market sensitivity. Specification 4 presents a statistical test of differences in coefficients across specifications 2 and 3. Specification 5 (specification 6) includes only the firm-year observations from Fama-French 48 industries of above-median (below-median) average market sensitivity. Specification 7 presents a statistical test of differences in coefficients across specifications 5 and 6. All significance levels are based on two-tailed tests. Below each coefficient is a t-statistic, in parentheses, calculated based on standard errors clustered by industry and fiscal year.

SOX 404 reporting and a risk characteristic that prior literature has shown to be priced. I further caveat that, because of the highly selected nature of the sample, the generalizability of these results is difficult to assess. These estimated magnitudes should be interpreted as “local average treatment effects,” and I caution against extrapolating these results to the entire cross-section of public firms. The treatment effect is plausibly much larger for the small firms included in this difference-in-differences analysis, than for the average public company. Although not directly comparable, the magnitude of the analogous OLS treatment effect estimate weighs in at the markedly smaller 0.059 (table 2, specification 4).[18]

Regarding causal attribution, the key identifying assumption in any difference-in-differences analysis is the parallel trends assumption. In the context of this RDD, the parallel trends assumption is: “absent treatment by SOX 404, risk asymmetry would have followed parallel trends for accelerated and non-accelerated filers.” If this assumption is violated, then the estimated causal effect of the treatment can be biased, potentially resulting in spurious inferences. This assumption is not directly testable, but it is possible to assess its plausibility graphically. To that end, I plot average risk asymmetry, by year, across the two groups. Although I cannot definitely rule out a violation of the parallel trends assumption, the evidence in the graph offers no cause for concern. I find that the two move almost perfectly in parallel from 2001 to 2004, as well as from 2006 to 2009. The two diverge only in 2004 to 2006, when accelerated filers are first required to adopt the new SOX 404 provisions and experience a dramatic decline in risk asymmetry. These patterns are shown in figure 2.

I caution that, even if the treatment is randomly assigned, the RDD analyses can, at best, demonstrate a causal relation between SOX 404 and risk asymmetry. To the extent that SOX 404 affects firms in ways beyond reducing reporting flexibility, the analyses cannot demonstrate definitively that reporting flexibility played any role. For example, suppose SOX 404 helped managers make better business decisions, allowing them to mitigate the harm caused by sudden economic downturns. In this case, it is conceivable that SOX 404 would impact risk asymmetry through a governance channel. Although I cannot conclusively rule this possibility out, I note that my measure of risk asymmetry excludes the effects of extreme market events (best and worst 1% of market days), which are likely when the governance channel would be most pronounced. Moreover, I control for earnings downside risk, which would likely capture risk asymmetries driven by firm fundamentals.

[18] In untabulated analyses, I find that firm size moderates the relation between SOX 404 reporting and risk asymmetry to a significant degree. Thus, it is not surprising that the estimated treatment effect would be much larger among the firms in the RDD sample, which are an order of magnitude smaller than the SOX 404 reporters from the full sample, on average.


FIG 2.—Time series of *Risk Asymmetry* around SOX 404 adoption, by accelerated filer status. This figure plots the time series of risk asymmetry around the SOX 404 adoption event, for a balanced panel of accelerated and non-accelerated filers. The sample is restricted to only include firms whose 2004 public floats were within $20 million of the $75 million public float threshold. The sample window spans fiscal years 2001–2009. To avoid contamination arising from changes in sample composition, firms are only included in the sample if they are present in the data for each year from 2001 to 2009. Firms whose 2002 public floats are above (below) $75 million are considered to be accelerated (non-accelerated) filers. To facilitate easy comparison, both time series are level-shifted by subtracting the 2002 level of *Risk Asymmetry*. The faint vertical line represents the start of SOX 404 reporting for accelerated filers.

*4.1.3. Placebo Event Study.* As a falsification test, I replicate the main analysis using a different time window and a placebo coding of the *Post* variable. I construct a pre–SOX 404 sample, such that the sample period and "treatment date" are shifted backward by seven years—no firms adopt the SOX 404 requirements during this window, as it takes place entirely before the initial SOX 404 adoption date. I then replicate the RDD analyses on the placebo sample and present the results in table 5. I find that the estimated treatment effect is economically small and statistically insignificant across all specifications. Moreover, I observe no statistical differences between the above-median and below-median market sensitivity samples. These null results reinforce the prior findings by alleviating concerns that systematic differences in the treatment and control groups (other than the treatment, itself) drive the results.

*4.1.4. Reconvergence Analyses.* In the main analysis, I leveraged the institutional fact that SOX 404 differentially affected firms on either side of a

**TABLE 5**
*Placebo Tests*

Panel A: No controls

| | | Outcome = *Risk Asymmetry* | | | |
|---|---|---|---|---|---|
| Variables | Pred. | (1) Full Sample | (2) High $\hat{\beta}^2$ | (3) Low $\hat{\beta}^2$ | (4) (2)−(3) |
| *Acc. Filer × Post (placebo)* | 0 | 0.088 | 0.080 | −0.129 | 0.209 |
| | | (0.652) | (0.254) | (−0.962) | (0.577) |
| Fixed Effects | | Year, Firm | Year, Firm | Year, Firm | |
| Observations | | 1,661 | 831 | 830 | |
| *R*-squared | | 0.267 | 0.350 | 0.353 | |

Panel B: With controls

| | | Outcome = *Risk Asymmetry* | | | |
|---|---|---|---|---|---|
| Variables | Pred. | (1) Full Sample | (2) High $\hat{\beta}^2$ | (3) Low $\hat{\beta}^2$ | (4) (2)−(3) |
| *Acc. Filer × Post (placebo)* | 0 | 0.074 | 0.108 | −0.112 | 0.220 |
| | | (0.537) | (0.251) | (−0.671) | (0.422) |
| *log($\hat{\beta}^2$)* | | −0.013 | −0.049 | −0.037 | |
| | | (−0.527) | (−0.438) | (−1.387) | |
| *logSize* | | 0.075 | −0.041 | 0.125 | |
| | | (0.553) | (−0.143) | (0.588) | |
| *logAge* | | −0.097 | 0.052 | 0.001 | |
| | | (−1.485) | (0.278) | (0.007) | |
| *logBTM* | | 0.027 | −0.073 | 0.103 | |
| | | (0.160) | (−0.271) | (0.506) | |
| *logDTE* | | −0.081 | −0.113 | −0.232* | |
| | | (−0.671) | (−0.488) | (−1.878) | |
| *logLiq.* | | 0.020 | 0.068 | 0.026 | |
| | | (0.205) | (0.340) | (0.316) | |
| *logVol.* | | 0.104 | 0.250 | 0.166 | |
| | | (1.189) | (1.184) | (1.325) | |
| *ROA* | | 0.045 | 0.041 | 0.202 | |
| | | (0.267) | (0.140) | (0.636) | |
| *Crash* | | 0.137 | −0.048 | 0.189 | |
| | | (1.755) | (−0.246) | (1.290) | |
| *EDR* | | 0.550 | 0.707 | −0.099 | |
| | | (0.695) | (0.770) | (−0.119) | |
| Fixed Effects | | Year, Firm | Year, Firm | Year, Firm | |
| Observations | | 1,661 | 831 | 830 | |
| *R*-squared | | 0.274 | 0.357 | 0.386 | |

This table presents placebo event study difference-in-differences results. The analysis replicates table 4, but shifts the sample window and "treatment" date back by seven years. The sample is restricted to only include firms whose 2004 public floats were within \$20 million of the \$75 million public float threshold. The sample spans 1994−2002. Panels differ only with respect to the inclusion of control variables; panel A (panel B) excludes (includes) all the control variables. Within each panel, specifications differ with respect to sample. Specification 1 includes the full sample. In the remaining specifications, the sample is restricted based on market sensitivity, $\hat{\beta}^2$. Specification 2 (specification 3) includes firm-year observations of above-median (below-median) market sensitivity. Specification 4 presents a statistical test of differences in coefficients across specifications 2 and 3. All inferences are two tailed. Below each coefficient is a *t*-statistic, in parentheses, calculated based on standard errors clustered by industry and fiscal year.

$75 million public float threshold. To do so, I coded firms as treatment versus control observations based on 2002 public floats. In these next analyses, I examine what happens to firms that were not accelerated filers at the time of the initial SOX 404 adoption date, but subsequently came under the mandate. I use variants on the following specification:

$$Risk\ Asymmetry_{i,t} = \phi SOX\ 404_{i,t} + \boldsymbol{\lambda} X_{i,t} + \tau_t + \mu_i + \varepsilon_{i,t}, \tag{10}$$

which exactly mirrors that of specification 4 from table 2, but uses a restricted sample, which I detail below.

For this analysis, I use the firms from the RDD sample (i.e., the firms with 2004 public floats within $20 million of the $75 million threshold), but shift the time window forward, covering 2006 to 2013. Thus, the sample window for this analysis starts after the initial impact of SOX 404 adoption on some firms' risk asymmetries. I further exclude from the sample any firms that never become SOX 404 reporters at any point over this sample window.

In this analysis, "treatment" firms are the firms that first become SOX 404 reporters at some point between 2006 and 2013. Their changes in risk asymmetry around their SOX 404 adoption dates are benchmarked jointly against two control groups: (1) firms that were already SOX 404 reporters at the start of the sample window; and (2) other treatment firms, which become treated at a different point in time during the sample window. Results from this analysis are presented in table 6.

The presentation of table 6 results mirrors that of tables 4 and 5. In panel A (panel B), I exclude (include) all of the control variables. Within each panel, specifications differ with respect to the sample. Specification 1 includes the full sample. Specification 2 (specification 3) includes only observations of above-median (below-median) market sensitivity, as measured by $\hat{\beta}^2$. The fourth column presents a statistical test of differences across specifications 2 and 3.

I find that firms experience a significant decrease in risk asymmetry as they become SOX 404 reporters. That is, risk asymmetry of previously non-accelerated filers appears to converge to that of accelerated filers, as the previously non-accelerated filers become accelerated filers themselves. Moreover, this pattern appears to be more pronounced among more market-sensitive firms. However, with the inclusion of the control variables, this difference is not statistically significant.

Collectively, these results corroborate the evidence from the RDD analyses (i.e., table 4), by showing that relative to otherwise comparable firms, becoming a SOX 404 reporter coincides with a large decrease in risk asymmetry. However, I caveat that the identifying variation underlying this analysis is not as clean as that of table 4; the realized changes in SOX 404 reporting are endogenous to changes in firm size. The results in table 6 should therefore be interpreted as supporting descriptive evidence, rather than additional causal evidence.

**TABLE 6**
*Reconvergence Tests*

Panel A: No controls

| | | Outcome = *Risk Asymmetry* | | | |
|---|---|---|---|---|---|
| Variables | Pred. | (1) Full Sample | (2) High $\hat{\beta}^2$ | (3) Low $\hat{\beta}^2$ | (4) (2)–(3) |
| *SOX 404* | – | −0.194*** | −0.514** | 0.098 | −0.612* |
| | | (−4.668) | (−2.702) | (0.638) | (−1.789) |
| Fixed effects | | Year, Firm | Year, Firm | Year, Firm | |
| Observations | | 974 | 538 | 436 | |
| *R*-squared | | 0.237 | 0.361 | 0.354 | |

Panel B: With controls

| | | Outcome = *Risk Asymmetry* | | | |
|---|---|---|---|---|---|
| Variables | Pred. | (1) Full Sample | (2) High $\hat{\beta}^2$ | (3) Low $\hat{\beta}^2$ | (4) (2)–(3) |
| *SOX 404* | – | −0.144** | −0.392 | 0.114 | −0.506 |
| | | (−3.459) | (−1.525) | (0.555) | (−1.182) |
| *log($\hat{\beta}^2$)* | | −0.065** | −0.159 | −0.046 | |
| | | (−3.006) | (−1.643) | (−1.081) | |
| *logSize* | | −0.093 | −0.068 | −0.307 | |
| | | (−0.905) | (−0.480) | (−1.207) | |
| *logAge* | | 0.032 | 0.066 | −1.394 | |
| | | (0.061) | (0.111) | (−1.658) | |
| *logBTM* | | −0.033 | −0.049 | −0.149 | |
| | | (−0.329) | (−0.475) | (−0.872) | |
| *logDTE* | | −0.055 | −0.103 | 0.073 | |
| | | (−0.730) | (−1.516) | (0.521) | |
| *logLiq.* | | 0.118 | 0.130 | 0.125 | |
| | | (1.631) | (0.968) | (0.835) | |
| *logVol.* | | 0.274* | 0.358 | 0.216 | |
| | | (2.079) | (1.673) | (1.013) | |
| *ROA* | | 0.564*** | 0.288** | 0.705*** | |
| | | (3.722) | (2.684) | (5.382) | |
| *Crash* | | −0.017 | −0.027 | −0.097 | |
| | | (−0.148) | (−0.177) | (−0.667) | |
| *EDR* | | 0.338* | 1.616 | −2.171*** | |
| | | (2.210) | (1.465) | (−4.234) | |
| Fixed effects | | Year, Firm | Year, Firm | Year, Firm | |
| Observations | | 974 | 538 | 436 | |
| *R*-squared | | 0.277 | 0.406 | 0.407 | |

This table presents results from the reconvergence analysis. The sample is restricted to only include firms whose 2004 public floats were within $20 million of the $75 million public float threshold. The sample is further restricted to only include firms that are SOX 404 reporters by fiscal 2013. The sample spans 2006–2013. Panels differ only with respect to the inclusion of control variables; panel A (panel B) excludes (includes) all the control variables. Within each panel, specifications differ with respect to sample. Specification 1 includes the full sample. In the remaining specifications, the sample is restricted based on market sensitivity, $\hat{\beta}^2$. Specification 2 (specification 3) includes firm-year observations of above-median (below-median) market sensitivity. Specification 4 presents a statistical test of differences in coefficients across specifications 2 and 3. All significance levels are based on two-tailed tests. Below each coefficient is a *t*-statistic, in parentheses, calculated based on standard errors clustered by industry and fiscal year.

### 4.2 INTERNAL CONTROL STRENGTH AND RISK ASYMMETRY

In the prior analyses, I examine the relation between SOX 404 reporting and risk asymmetry. In this subsection, I restrict the sample to include only SOX 404 reporters, and examine whether heterogeneity in assessed internal control strength can explain variation in risk asymmetry. For these analyses, I expand the set of control variables to include additional variables documented by Doyle, Ge, and McVay [2007a] to predict internal control weaknesses. I further winnow the sample by dropping observations for which the additional control variables cannot be constructed.

In table 7, I tabulate descriptive statistics for all variables used in the section 4.2 tests. Panel A summarizes the distributions of all the variables; panel B presents means and standard deviations for each variable, split by internal control strength; and panel C presents a correlation matrix of all variables examined with Pearson (Spearman) correlations below (above) the diagonal.[19]

On average, firms in this sample exhibit a risk asymmetry of 0.056—far lower than the sample mean of 0.243 tabulated in table 1. This discrepancy is likely because of the fact that all observations in the table 7 sample are SOX 404 reporters. Among SOX 404 reporters, risk asymmetry is most pronounced among firms with internal control weaknesses, with an average risk asymmetry of 0.194, compared to only 0.047 among firms with strong internal controls. I further document that relative downside (upside) risk is significantly greater (lower) among firms with internal control weaknesses.

*4.2.1. Tests of Association.* I next confirm that ICWs are associated with risk asymmetry in a multivariate context. I replicate the design of table 2, but use *ICW* instead of *SOX 404* as the variable of interest:

$$Risk\ Asymmetry_{i,t} = \phi ICW_{i,t} + \boldsymbol{\lambda} X_{i,t} + \tau_t + \mu_i + \varepsilon_{i,t}. \tag{11}$$

I tabulate the results from these regressions in table 8.

Across all specifications, I find that internal control weaknesses are associated with greater risk asymmetry. The inclusion of the control variables cuts the association by almost half, but it remains statistically and economically significant across all specifications. The fourth specification includes firm fixed effects. I find that, in years with an internal control weakness, firms exhibit an average of 0.051 more risk asymmetry than they do in years without an internal control weakness. Notably, every firm-year observation in the sample is a SOX 404 reporter, so these results provide evidence of an association between reporting flexibility and risk asymmetry among SOX 404 reporters: weaker internal controls are associated with greater risk asymmetry.

[19] The descriptive statistics in table 7, panel A, do not exactly match those of columns 3 and 4 from table 1, panel B, because the section 4.2 sample is restricted slightly based on the constructibility of the additional Doyle, Ge, and McVay (2007a) control variables.

**TABLE 7**

*Descriptive Statistics for Internal Controls Tests*

| Panel A: Summary statistics | | | | | | |
|---|---|---|---|---|---|---|
| Variable | *N* | Mean | *SD* | Q1 | Med. | Q3 |
| *ICW* | 32,830 | 0.059 | 0.235 | 0.000 | 0.000 | 0.000 |
| *Risk Asymmetry* | 32,830 | 0.056 | 0.686 | −0.317 | 0.018 | 0.384 |
| *Relative Downside Risk* | 32,830 | 0.004 | 0.427 | −0.220 | −0.009 | 0.207 |
| *Relative Upside Risk* | 32,830 | −0.052 | 0.459 | −0.269 | −0.032 | 0.185 |
| $log(\hat{\beta}^2)$ | 32,830 | 0.072 | 1.127 | −0.363 | 0.238 | 0.752 |
| *logSize* | 32,830 | 13.823 | 1.773 | 12.538 | 13.701 | 14.975 |
| *logAge* | 32,830 | 2.676 | 0.910 | 2.131 | 2.741 | 3.324 |
| *logBTM* | 32,830 | −0.685 | 1.097 | −1.264 | −0.726 | −0.221 |
| *logDTE* | 32,830 | −6.992 | 1.140 | −7.694 | −6.958 | −6.316 |
| *logLiq.* | 32,830 | 11.357 | 1.619 | 10.346 | 11.353 | 12.432 |
| *logVol.* | 32,830 | −2.274 | 0.531 | −2.635 | −2.279 | −1.922 |
| *ROA* | 32,830 | 0.004 | 0.187 | −0.009 | 0.040 | 0.082 |
| *Crash* | 32,830 | 0.215 | 0.411 | 0.000 | 0.000 | 0.000 |
| *EDR* | 32,830 | 0.001 | 0.061 | −0.029 | −0.005 | 0.017 |
| *Agg. Loss* | 32,830 | 0.188 | 0.390 | 0.000 | 0.000 | 0.000 |
| *B. Risk* | 32,830 | 5.281 | 2.727 | 3.000 | 5.000 | 8.000 |
| *Foreign* | 32,830 | 0.623 | 0.485 | 0.000 | 1.000 | 1.000 |
| *ACQ* | 32,830 | 0.063 | 0.135 | 0.000 | 0.003 | 0.059 |
| *logSegs* | 32,830 | 0.343 | 1.309 | 0.000 | 0.000 | 0.000 |
| *Ex. Growth* | 32,830 | 0.146 | 0.353 | 0.000 | 0.000 | 0.000 |
| *Restruc.* | 32,830 | 0.009 | 0.022 | 0.000 | 0.000 | 0.006 |
| *RPE* | 10,893 | 0.152 | 0.359 | 0.000 | 0.000 | 0.000 |

(*Continued*)

**TABLE 7**—*(Continued)*

Panel B: Variable means, split by internal controls strength

| Variable | Strong Internal Controls Obs. = 30,901 (1) Mean | (2) SD | Weak Internal Controls Obs. = 1,929 (3) Mean | (4) SD | Difference (5) (3)–(1) |
|---|---|---|---|---|---|
| *Risk Asymmetry* | 0.047 | 0.673 | 0.194 | 0.855 | 0.147*** |
| *Relative Downside Risk* | −0.001 | 0.416 | 0.075 | 0.568 | 0.076*** |
| *Relative Upside Risk* | −0.048 | 0.454 | −0.119 | 0.539 | −0.071*** |
| *log*($\hat{\beta}^2$) | 0.077 | 1.103 | −0.015 | 1.455 | −0.092 |
| *logSize* | 13.881 | 1.775 | 12.898 | 1.451 | −0.983*** |
| *logAge* | 2.690 | 0.910 | 2.443 | 0.883 | −0.247*** |
| *logBTM* | −0.691 | 1.098 | −0.594 | 1.085 | 0.097** |
| *logDTE* | −6.994 | 1.137 | −6.961 | 1.179 | 0.033 |
| *logLiq.* | 11.384 | 1.617 | 10.911 | 1.577 | −0.473*** |
| *logVol.* | −2.285 | 0.531 | −2.102 | 0.501 | 0.183*** |
| *ROA* | 0.007 | 0.186 | −0.034 | 0.189 | −0.041*** |
| *Crash* | 0.212 | 0.409 | 0.249 | 0.432 | 0.037*** |
| *EDR* | 0.001 | 0.060 | 0.015 | 0.072 | 0.014*** |
| *Agg. Loss* | 0.181 | 0.385 | 0.285 | 0.452 | 0.104*** |
| *B. Risk* | 5.238 | 2.722 | 5.972 | 2.722 | 0.734*** |
| *Foreign* | 0.621 | 0.485 | 0.653 | 0.476 | 0.032* |
| *ACQ* | 0.063 | 0.135 | 0.071 | 0.146 | 0.008 |
| *logSegs* | 0.341 | 1.305 | 0.384 | 1.383 | 0.043 |
| *Ex. Growth* | 0.144 | 0.351 | 0.180 | 0.385 | 0.036*** |
| *Restruc.* | 0.009 | 0.022 | 0.011 | 0.026 | 0.002*** |
| *RPE* | 0.153 | 0.354 | 0.109 | 0.306 | −0.044 |

(*Continued*)

**TABLE 7**—*(Continued)*

Panel C: Correlations

| | | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| (1) | *ICW* | | 0.044 | 0.033 | −0.039 | 0.007 | −0.134 | −0.065 | 0.025 | 0.003 | −0.067 | 0.082 |
| (2) | *Risk Asymmetry* | 0.050 | | 0.728 | −0.764 | −0.039 | −0.030 | −0.064 | −0.032 | 0.004 | 0.020 | 0.057 |
| (3) | *Relative Downside Risk* | 0.042 | 0.755 | | −0.199 | −0.061 | −0.001 | −0.035 | −0.018 | 0.024 | 0.027 | 0.043 |
| (4) | *Relative Upside Risk* | −0.036 | −0.793 | −0.198 | | 0.007 | 0.046 | 0.061 | 0.030 | 0.016 | −0.004 | −0.045 |
| (5) | $log(\hat{\beta}^2)$ | −0.019 | −0.031 | −0.053 | −0.003 | | 0.004 | −0.084 | −0.027 | 0.001 | 0.182 | 0.355 |
| (6) | *logSize* | −0.131 | −0.047 | −0.023 | 0.048 | 0.102 | | 0.292 | −0.326 | 0.245 | 0.689 | −0.489 |
| (7) | *logAge* | −0.064 | −0.062 | −0.039 | 0.057 | −0.046 | 0.300 | | 0.014 | 0.159 | 0.107 | −0.330 |
| (8) | *logBTM* | 0.021 | −0.031 | −0.018 | 0.030 | −0.056 | −0.303 | −0.002 | | −0.082 | −0.176 | 0.105 |
| (9) | *logDTE* | 0.007 | −0.013 | 0.012 | 0.030 | 0.036 | 0.207 | 0.156 | −0.155 | | 0.214 | −0.123 |
| (10) | *logLiq.* | −0.069 | 0.021 | 0.014 | −0.018 | 0.271 | 0.687 | 0.107 | −0.175 | 0.186 | | −0.040 |
| (11) | *logVol.* | 0.081 | 0.086 | 0.067 | −0.066 | 0.249 | −0.489 | −0.324 | 0.055 | −0.091 | −0.020 | |
| (12) | *ROA* | −0.051 | −0.069 | −0.023 | 0.082 | 0.011 | 0.345 | 0.165 | 0.043 | 0.001 | 0.038 | −0.337 |
| (13) | *Crash* | 0.021 | 0.009 | −0.008 | −0.021 | −0.013 | −0.010 | 0.003 | −0.004 | 0.003 | 0.037 | 0.025 |
| (14) | *EDR* | 0.054 | 0.068 | 0.040 | −0.064 | 0.063 | −0.132 | −0.048 | −0.064 | 0.147 | 0.082 | 0.211 |
| (15) | *Agg. Loss* | 0.062 | 0.098 | 0.043 | −0.106 | 0.029 | −0.337 | −0.208 | −0.059 | −0.034 | −0.025 | 0.376 |
| (16) | *B. Risk* | 0.063 | 0.053 | 0.027 | −0.053 | −0.022 | −0.164 | −0.035 | 0.311 | 0.531 | 0.070 | 0.111 |
| (17) | *Foreign* | 0.016 | −0.041 | −0.022 | 0.040 | 0.079 | 0.213 | 0.101 | 0.028 | 0.053 | 0.131 | −0.124 |
| (18) | *ACQ* | 0.015 | −0.029 | −0.012 | 0.033 | −0.003 | −0.083 | −0.038 | 0.281 | 0.158 | −0.045 | 0.020 |
| (19) | *logSegs* | 0.008 | 0.010 | 0.017 | 0.001 | −0.010 | 0.010 | 0.119 | −0.018 | −0.016 | 0.004 | −0.021 |
| (20) | *Ex. Growth* | 0.025 | 0.045 | 0.037 | −0.032 | 0.092 | −0.045 | −0.172 | −0.069 | −0.041 | 0.025 | 0.152 |
| (21) | *Restruc.* | 0.025 | −0.004 | 0.011 | 0.017 | 0.035 | −0.119 | 0.056 | 0.227 | 0.183 | 0.030 | 0.125 |
| (22) | *RPE* | −0.021 | 0.001 | −0.004 | −0.005 | −0.041 | 0.104 | 0.154 | 0.078 | 0.074 | 0.061 | −0.098 |

*(Continued)*

**TABLE 7**—*(Continued)*

Panel C: Correlations

| | | (12) | (13) | (14) | (15) | (16) | (17) | (18) | (19) | (20) | (21) | (22) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| (1) | *ICW* | −0.103 | 0.021 | 0.055 | 0.062 | 0.063 | 0.016 | −0.001 | 0.007 | 0.025 | 0.013 | −0.021 |
| (2) | *Risk Asymmetry* | −0.036 | 0.005 | 0.057 | 0.080 | 0.050 | −0.032 | −0.038 | 0.009 | 0.037 | −0.012 | 0.005 |
| (3) | *Relative Downside Risk* | 0.000 | −0.009 | 0.035 | 0.027 | 0.029 | −0.013 | −0.015 | 0.017 | 0.026 | 0.005 | −0.009 |
| (4) | *Relative Upside Risk* | 0.060 | −0.022 | −0.051 | −0.097 | −0.048 | 0.033 | 0.044 | 0.001 | −0.024 | 0.017 | −0.012 |
| (5) | $log(\hat{\beta}^2)$ | −0.072 | −0.031 | 0.098 | 0.088 | 0.010 | 0.046 | −0.007 | −0.007 | 0.125 | 0.039 | −0.032 |
| (6) | *logSize* | 0.397 | −0.006 | −0.050 | −0.345 | −0.149 | 0.219 | 0.115 | 0.018 | −0.044 | 0.075 | 0.113 |
| (7) | *logAge* | 0.156 | −0.001 | −0.009 | −0.215 | −0.050 | 0.100 | 0.025 | 0.130 | −0.162 | 0.142 | 0.166 |
| (8) | *logBTM* | −0.248 | 0.012 | 0.030 | −0.029 | 0.359 | −0.003 | 0.106 | −0.011 | −0.075 | 0.100 | 0.090 |
| (9) | *logDTE* | −0.127 | 0.002 | 0.239 | −0.042 | 0.548 | 0.045 | 0.121 | −0.013 | −0.044 | 0.180 | 0.091 |
| (10) | *logLiq.* | 0.083 | 0.036 | 0.116 | −0.037 | 0.080 | 0.126 | 0.023 | 0.013 | 0.028 | 0.125 | 0.063 |
| (11) | *logVol.* | −0.348 | 0.029 | 0.128 | 0.371 | 0.104 | −0.127 | −0.117 | −0.029 | 0.163 | −0.014 | −0.096 |
| (12) | *ROA* | | −0.006 | −0.502 | −0.632 | −0.514 | 0.112 | 0.053 | 0.021 | 0.032 | −0.154 | −0.042 |
| (13) | *Crash* | 0.002 | | −0.014 | −0.023 | −0.016 | 0.040 | 0.064 | 0.000 | −0.047 | 0.061 | −0.027 |
| (14) | *EDR* | −0.543 | −0.022 | | 0.410 | 0.335 | 0.041 | −0.035 | 0.002 | 0.022 | 0.228 | 0.000 |
| (15) | *Agg. Loss* | −0.610 | −0.023 | 0.462 | | 0.281 | −0.118 | −0.165 | −0.024 | 0.056 | 0.060 | −0.059 |
| (16) | *B. Risk* | −0.349 | −0.015 | 0.302 | 0.291 | | −0.051 | 0.037 | −0.024 | −0.014 | 0.158 | 0.097 |
| (17) | *Foreign* | 0.142 | 0.040 | 0.000 | −0.118 | −0.053 | | 0.212 | −0.009 | −0.061 | 0.266 | −0.032 |
| (18) | *ACQ* | −0.003 | 0.033 | −0.008 | −0.050 | 0.186 | 0.078 | | −0.005 | 0.039 | 0.141 | −0.028 |
| (19) | *logSegs* | 0.014 | −0.002 | 0.007 | −0.021 | −0.028 | −0.012 | −0.011 | | −0.020 | 0.015 | −0.024 |
| (20) | *Ex. Growth* | −0.011 | −0.047 | 0.060 | 0.056 | −0.013 | −0.061 | 0.103 | −0.019 | | −0.121 | −0.035 |
| (21) | *Restruc.* | −0.119 | 0.040 | 0.195 | 0.160 | 0.218 | 0.137 | 0.119 | −0.001 | −0.084 | | 0.005 |
| (22) | *RPE* | 0.002 | −0.027 | −0.014 | −0.059 | 0.094 | −0.032 | −0.017 | −0.027 | −0.035 | −0.013 | |

This table presents descriptive statistics for all the variables included in the ICW regressions. The sample spans 2004–17 and is restricted to include only SOX 404 reporters. Panel A presents summary statistics for all variables; Panel B presents variable means split by assessed internal control strength; and panel C presents Pearson correlations (below the diagonal) and Spearman correlations (above the diagonal).

**TABLE 8**

*Internal Controls and Risk Asymmetry*

| | | Outcome Variable = *Risk Asymmetry* | | | |
|---|---|---|---|---|---|
| Variables | Pred. | (1) | (2) | (3) | (4) |
| *ICW* | + | 0.109*** | 0.109*** | 0.059*** | 0.051** |
| | | (4.543) | (4.588) | (3.072) | (2.115) |
| $log(\hat{\beta}^2)$ | | | | −0.057*** | −0.041** |
| | | | | (−4.195) | (−2.534) |
| *logSize* | | | | −0.018 | −0.004 |
| | | | | (−1.472) | (−0.111) |
| *logAge* | | | | −0.015 | 0.028 |
| | | | | (−1.307) | (0.817) |
| *logBTM* | | | | −0.013 | −0.041 |
| | | | | (−0.836) | (−1.669) |
| *logDTE* | | | | −0.012 | −0.006 |
| | | | | (−0.593) | (−0.257) |
| *logLiq.* | | | | 0.038*** | 0.039 |
| | | | | (3.106) | (1.684) |
| *logVol.* | | | | 0.149*** | 0.115** |
| | | | | (3.701) | (2.631) |
| *ROA* | | | | 0.094 | 0.161* |
| | | | | (1.022) | (1.839) |
| *Crash* | | | | 0.233 | 0.229 |
| | | | | (1.219) | (1.320) |
| *EDR* | | | | 0.003 | 0.010 |
| | | | | (0.234) | (0.647) |
| *Agg. Loss* | | | | 0.060*** | 0.048** |
| | | | | (3.790) | (2.583) |
| *B. Risk* | | | | 0.007 | −0.001 |
| | | | | (1.292) | (−0.130) |
| *Foreign* | | | | −0.014 | 0.019 |
| | | | | (−1.158) | (0.754) |
| *ACQ* | | | | −0.079 | −0.053 |
| | | | | (−1.302) | (−0.739) |
| *logSegs* | | | | 0.005 | 0.013 |
| | | | | (1.596) | (1.482) |
| *Ex. Growth* | | | | 0.034 | 0.017 |
| | | | | (1.661) | (1.026) |
| *Restruc.* | | | | −0.335 | −0.258 |
| | | | | (−1.713) | (−1.054) |
| Fixed effects | | Year | Year, Industry | Year, Industry | Year, Firm |
| Observations | | 32,830 | 32,830 | 32,830 | 32,830 |
| *R*-squared | | 0.037 | 0.045 | 0.069 | 0.229 |

This table presents OLS results for the association between internal control weaknesses and risk asymmetry. The sample spans 2004–17 and is restricted to include only SOX 404 reporters. Specification 1 includes year fixed effects; specification 2 includes year and industry fixed effects; specification 3 includes year and industry fixed effects and firm-year controls; and specification 4 includes year and firm fixed effects and firm-year controls. All significance levels are based on two-tailed tests. Below each coefficient is a *t*-statistic, in parentheses, calculated based on standard errors clustered by industry and fiscal year.

*4.2.2. Relative Performance Evaluation.* One of the crucial assumptions underlying Predictions 1 and 2 is that firms' incentives to obfuscate are dictated, in large part, by poor absolute performance (as opposed to poor relative performance). Although this assumption has strong empirical backing, it need not be the case for all firms. For instance, some managers may prefer to obfuscate based on performance relative to the market. In this case, I do not expect reporting flexibility to have any effect on risk asymmetry.

I posit that firms' reporting choices are more likely to be affected by relative (as opposed to absolute) performance when the CEOs are compensated on the basis of RPE. If so, then I would expect the relation between reporting flexibility and risk asymmetry to be diminished by the use of RPE-based CEO pay.

With this intuition in mind, I examine whether the relation between internal controls and risk asymmetry differs depending on the usage of RPE in the CEO's pay package.[20] I test for an interactive effect using variants on the following regression specification:

$$Risk\ Asymmetry_{i,t} = \phi_1 ICW_{i,t} \times RPE_{i,t} + \phi_2 ICW_{i,t} + \phi_3 RPE_{i,t} + \boldsymbol{\lambda} X_{i,t} + \tau_t + \mu_i + \varepsilon_{i,t}, \tag{12}$$

where $RPE_{i,t}$ is an indicator variable equal to one if the CEO of firm $i$ in year $t$ has any incentive pay tied to relative performance objectives. The results of this analysis are tabulated in table 9. With respect to fixed effects and the inclusion of controls, the specifications mirror those of table 8.

As predicted, internal control weaknesses are significantly less positively associated with risk asymmetry when the CEO has incentive pay tied to relative performance objectives. Among observations that include RPE, I find no evidence of a positive relation between internal control weaknesses and risk asymmetry. Notably, *RPE* carries no main effect; the coefficient is both economically small and statistically insignificant. This suggests that *RPE* has no bearing on risk asymmetry, but for its moderating effect on the importance of reporting flexibility.

### 4.3 SENSITIVITY AND SUPPLEMENTAL ANALYSES

In this subsection, I describe my sensitivity checks and supplemental analyses. I tabulate the results from these tests in the online appendix.

*4.3.1. Sensitivity Analysis: Measurement of Risk Asymmetry.* I measure risk asymmetry as the difference between downmarket and upmarket $\beta$'s, where downmarket (upmarket) $\beta$'s are firm-year–specific slope coefficients estimated from negative (positive) market return days. Conditioning on the sign of the market's return results in a highly skewed and kurtotic left-hand

[20] I cannot use variation in SOX 404 reporting to test this prediction. Broad sample, high-quality data on RPE are only available for very large firms in the post-CD&A period; for this sample, nearly all firms are SOX 404 reporters.

**TABLE 9**

*Relative Performance Evaluation and Risk Asymmetry*

| | | Outcome Variable = *Risk Asymmetry* | | | |
|---|---|---|---|---|---|
| Variables | Pred. | (1) | (2) | (3) | (4) |
| *ICW* × *RPE* | – | −0.195*** | −0.172*** | −0.147** | −0.223** |
| | | (−4.889) | (−3.840) | (−2.691) | (−2.829) |
| *ICW* | | 0.105** | 0.100** | 0.063 | 0.050 |
| | | (2.535) | (2.250) | (1.239) | (0.943) |
| *RPE* | | 0.010 | −0.014 | −0.007 | −0.014 |
| | | (0.419) | (−0.797) | (−0.360) | (−0.449) |
| $log(\hat{\beta}^2)$ | | | | −0.026 | −0.016 |
| | | | | (−0.884) | (−0.563) |
| *logSize* | | | | −0.021 | 0.016 |
| | | | | (−1.315) | (0.286) |
| *logAge* | | | | −0.020* | 0.014 |
| | | | | (−1.824) | (0.194) |
| *logBTM* | | | | −0.005 | 0.011 |
| | | | | (−0.195) | (0.254) |
| *logDTE* | | | | 0.009 | 0.039 |
| | | | | (0.513) | (1.074) |
| *logLiq.* | | | | 0.026 | 0.027 |
| | | | | (1.431) | (0.772) |
| *logVol.* | | | | 0.058 | 0.008 |
| | | | | (1.354) | (0.155) |
| *ROA* | | | | 0.268 | 0.543** |
| | | | | (1.058) | (2.434) |
| *Crash* | | | | 0.007 | 0.017 |
| | | | | (0.381) | (0.733) |
| *EDR* | | | | 0.153 | 0.245 |
| | | | | (0.625) | (1.176) |
| *Agg. Loss* | | | | 0.061 | 0.029 |
| | | | | (1.386) | (0.480) |
| *B. Risk* | | | | 0.008 | −0.002 |
| | | | | (1.392) | (−0.184) |
| *Foreign* | | | | 0.016 | 0.050 |
| | | | | (0.577) | (0.987) |
| *ACQ* | | | | −0.069 | −0.105 |
| | | | | (−0.694) | (−0.693) |
| *logSegs* | | | | 0.002 | 0.011 |
| | | | | (0.453) | (0.937) |
| *Ex. Growth* | | | | 0.065** | 0.040 |
| | | | | (2.353) | (1.383) |
| *Restruc.* | | | | −0.141 | 0.406 |
| | | | | (−0.254) | (0.475) |
| Fixed Effects | | Year | Year, Industry | Year, Industry | Year, Firm |
| Observations | | 10,893 | 10,893 | 10,893 | 10,893 |
| *R*-squared | | 0.044 | 0.059 | 0.070 | 0.197 |

This table presents evidence regarding the moderating role of executive incentives on the relation between reporting flexibility and risk asymmetry. The sample spans 2004–17 and is restricted to include only SOX 404 reporters. The sample is further restricted to only include firms covered by the Incentive Lab data set. Each specification is identical to the corresponding specification from table 8, but for the inclusion of an *RPE* main effect, and its interaction with *ICW*. Specification 1 includes year fixed effects; specification 2 includes year and industry fixed effects; specification 3 includes year and industry fixed effects and firm-year controls; and specification 4 includes year and firm fixed effects and firm-year controls. All significance levels are based on two-tailed tests. Below each coefficient is a *t*-statistic, in parentheses, calculated based on standard errors clustered by industry and fiscal year.

side variable, which is known to be problematic for regression analysis. For this reason, I trim the sample prior to estimating the firm-year downmarket and upmarket $\beta$'s by dropping the 1% most positive and 1% most negative market return days. This adjustment reduces the excess kurtosis of the left-hand side variable by roughly 90%.

To verify that my inferences are not driven by this design choice, I replicate all of the regression analyses using two alternative formulations of the asymmetry variables: (1) untrimmed versions, whereby I estimate the firm-year downmarket and upmarket $\beta$'s without trimming the sample at all; and (2) more aggressively trimmed versions, whereby I estimate the firm-year downmarket and upmarket $\beta$'s after excluding the 5% most positive and 5% most negative market return days.

Results using these alternative dependent variables are presented in the online appendix, in tables OA1 through OA6.[21] I find that the results are largely consistent across all three measurement approaches, suggesting that my inferences are not sensitive to my handling of extreme market return days.

*4.3.2. Decomposing Risk Asymmetry into Relative Downside and Upside Risks.* For supplemental analyses, I decompose risk asymmetry into two component parts: relative downside risk ($\hat{\beta}^- - \hat{\beta}$) and relative upside risk ($\hat{\beta}^+ - \hat{\beta}$). I then explore how SOX 404 reporting and internal control weaknesses relate to these two measures of asymmetric risk.

I first examine the effects of SOX 404, by replicating the OLS analyses from table 2 and the RDD analyses from table 4. These results are tabulated in the online appendix, table OA7. I present 12 specifications in total. The first (latter) six specifications use $Relative\ Downside\ Risk$ ($Relative\ Upside\ Risk$) as the outcome variable. Within each set of six specifications, the first four mirror specifications 1–4 from table 2 whereas the last two specifications mirror the full sample RDD tests from table 4, with and without controls. I find that SOX 404 reporting is associated with higher relative downside risk and lower relative upside risk. However the results are not statistically significant across all specifications.

Lastly, I examine how internal control weaknesses associate with relative downside and upside risks. I tabulate these analyses in the online appendix, table OA8. I present eight specifications in total. The first (latter) four specifications use $Relative\ Downside\ Risk$ ($Relative\ Upside\ Risk$) as the dependent variable. Within each set of four regressions, the specifications mirror those of table 8, with respect to fixed effects and control variables. I find that internal control weaknesses are associated with greater relative downside risk and lower relative upside risk. This result in statistically significant for relative downside risk across all four specifications. However, for relative

[21] For brevity, I do not report coefficient estimates on the control variables, but simply note whether or not they are included in the estimation.

upside risk, the result loses significance once the control variables are included.

Collectively, the results in tables OA7 and OA8 suggest that reporting flexibility is associated with increased relative downside risk and decreased relative upside risk. However, I note that this interpretation is not uniformly supported by all specifications in tables OA7 and OA8. In particular, for the full panel OLS results, reporting flexibility is significantly associated with relative downside risk across all eight specifications (table OA7, specifications 1–4 and table OA8, specifications 1–4), but significantly associated with relative upside risk in only five out of eight specifications (table OA7, specifications 7–10, and table OA8, specifications 5–8). In the RDD analyses, this pattern reverses; reporting flexibility is significantly associated with relative upside risk in both RDD specifications (table OA7, specifications 11 and 12), but significantly associated with relative downside risk in only one out of two specifications (table OA7, specifications 5 and 6). Across these two tables, all 20 specifications are entirely consistent with each other with respect to estimated coefficient signs, and fairly consistent with respect to estimated coefficient magnitudes, suggesting these discrepancies may be a matter of statistical power.

## 5. *Conclusion*

In this study, I predict and find an association between firms' ability to obfuscate value-relevant information—"reporting flexibility"—and asymmetric systematic risk exposure. Using an RDD, I further provide evidence to suggest that a causal relation exists between the two, whereby greater reporting flexibility causes risk asymmetry. The economic magnitude of the observed causal relation is substantial. For firms with public floats in the neighborhood of \$75MM, treatment by SOX 404 leads to a decrease in risk asymmetry of around 0.30 (0.27 standard deviations), relative to otherwise similar untreated firms. However, I caution that this finding be interpreted as a "local average treatment effect." For larger firms, the magnitude is likely to be substantially lower.

In addition, I examine heterogeneity in internal control strength among SOX 404 reporters. I find that, on average, an internal control weakness almost entirely offsets the benefits of SOX 404 reporting, vis-à-vis reducing risk asymmetry. SOX 404 reporters with internal control weaknesses exhibit risk asymmetry to a similar degree as SOX 404 non-reporters.

Coupled with the findings of ACX, who document that risk asymmetry is positively priced in American equity markets, my results suggest that firms with greater reporting flexibility will, ceteris paribus, have higher costs of equity capital. Moreover, the cost of capital effect of risk asymmetry is not captured by commonly used multifactor models, thus the inclusion of an additional factor mimicking portfolio, designed to capture the effects of reporting flexibility, may be warranted.

## APPENDIX A: REPORTING QUALITY AND COST OF CAPITAL

The link between reporting quality and the cost of capital is a central but contentious issue in the accounting and finance literatures.[22] Traditional asset pricing theory (e.g., Fama [1991]) takes the position that the diversifiabilty of information risk precludes it from affecting a firm's cost of capital. Indeed, if poor reporting quality is a purely idiosyncratic risk then it should not be expected-return relevant at all (Sharpe [1964], Lintner [1965]).[23]

More recent theoretical work suggests that a firm's reporting quality results in risk that is not purely idiosyncratic, and thus can have cost of capital implications. Lambert, Leuz, and Verrecchia [2007] explore a variant of the Capital Asset Pricing Model ("CAPM") to demonstrate that reporting quality influences a firm's systematic risk, and thus can affect a firm's cost of equity capital. Lambert, Leuz, and Verrecchia [2007] show that improved reporting quality has a "direct effect" on a firm's cost of equity capital by mitigating "estimation risk," thereby lowering a firm's $\beta$. Consistent with earlier work by Yee [2006], who notes that "[poor] earnings quality magnifies fundamental risk," the effect of information risk is not linearly separable from systematic risk, but is instead a systematic risk multiplier. Although Lambert, Leuz, and Verrecchia [2007] provide a compelling and affirmative answer to the question of whether or not reporting quality is pricing relevant, they do not suggest that reporting quality is a separate pricing-relevant risk factor—the resulting systematic risk implications are entirely captured by the CAPM.

However, ample empirical evidence suggests that firms with lower reporting quality have abnormally high costs of equity capital (e.g., Aboody, Hughes, and Liu [2003], Francis et al. [2004]. Ogneva, Subramanyam, and Raghunandan [2007], Francis, Nanda, and Olsson [2008], Ashbaugh-Skaife et al. [2009], Barth, Konchitchki, and Landsman [2013]). One explanation for these findings is that reporting quality is, itself, a separate systematic risk factor of hedging concern, although no widely accepted theory supports this notion.[24] Despite a lack of supporting theory, empirical researchers frequently construct and employ atheoretical multifactor specifications based purely on their ability to explain the cross-section of expected returns.[25] In line with this approach, Francis et al. [2005] demonstrate that adding a reporting quality hedge portfolio allows standard multifactor pricing models to better explain the cross-section of realized returns.

[22] See Botosan [1997], Botosan and Plumlee [2002];, Penman [2003], and Aboody, Hughes, and Liu [2005].

[23] Recent work by Taylor and Verrecchia [2015] challenges this notion, showing that, with a single risky asset, imperfectly competitive equity markets can lead to the pricing of idiosyncratic risk, even as the market grows large.

[24] Another alternative explanation is that market prices simply fail to efficiently reflect the information contained in low-quality disclosures. See Fama [1970] for a discussion of the "joint hypothesis problem."

[25] For the most frequently cited examples, see Fama and French [1993] and Carhart [1997].

Core, Guay, and Verdi [2008] caution that significant factor loadings (i.e., an asset's "exposure" to a factor) do not imply that a factor is priced. Using the two-stage method of Fama and Macbeth [1973], Core, Guay, and Verdi [2008] find no evidence that reporting quality is a priced risk factor.

More recently, Kim and Qi [2010] and Ogneva [2012] revisit and reconcile the work of Francis et al. [2005] and Core, Guay, and Verdi [2008]. Specifically, Kim and Qi [2010] show that with minor research design adjustments (controls for low-price stocks, different cross-sorts, and a decomposition of accrual quality into innate and discretionary components), the reporting quality hedge portfolio carries a robustly significant risk premium. This result is consistent with prior empirical work, which found reporting quality to be positively associated with abnormal costs of equity capital, but is inconsistent with the current state of asset pricing theory.

Another possible explanation for the link between reporting quality the cost of capital pertains to illiquidity risk—low transparency exacerbates information asymmetries, forcing rational investors to price protect, reducing liquidity, and increasing the cost of equity capital (e.g., Glosten and Milgrom [1985], Kyle [1985], Amihud and Mendelson [1986], Welker [1995], Healy, Hutton, and Palepu [1999], and Leuz and Verrecchia [2000]). This explanation has both solid theoretical grounding and substantial empirical support. For example, Daske et al. [2008] and Lang, Lins, and Maffett [2012] both document that higher transparency is associated with greater liquidity, higher valuations, and a lower cost of capital. However, the relation between reporting quality and the cost of capital persists, and remains economically large, even after taking information asymmetry/illiquidity risk into account. Thus, it seems unlikely that illiquidity entirely explains the pricing effects of transparency (e.g., Kim and Qi [2010], Bhattacharya et al. [2012]).

In this study, I predict and find empirical support for an additional explanation: that reporting flexibility leads to (priced) systematic risk, which is distinct from illiquidity risk, and not captured by standard multifactor models: "risk asymmetry." If reporting flexibility causes firms to exhibit greater risk asymmetry, this would likely lead to apparent "abnormal" returns for low reporting quality firms, which does not dissipate after controlling for illiquidity risk, or other commonly used systematic risk factors (e.g., the market, SMB, HML, UMD).

## APPENDIX B: VARIABLE DEFINITIONS

| Construct | Measure | Definition |
|---|---|---|
| Panel A: Market comovement variables | | |
| Market $\beta$ | $\hat{\beta}$ | The covariance between a firm's daily excess return $(r_i)$ and the value-weighted market portfolio's contemporaneous excess daily return $(r_m)$, scaled by the variance of the firm's daily returns: $\hat{\beta} = \frac{Cov(r_i, r_m)}{Var(r_m)}$, estimated separately for each firm-fiscal year observation |
| Downmarket $\beta$ | $\hat{\beta}^-$ | Identical to $\hat{\beta}$ but estimated exclusively from days where the value-weighted market portfolio's excess daily return is negative: $\hat{\beta}^- = \frac{Cov(r_i, r_m \| r_m < 0)}{Var(r_m \| r_m < 0)}$ I discard the worst 1% of market return days before estimating. |
| Upmarket $\beta$ | $\hat{\beta}^+$ | Identical to $\hat{\beta}$ but estimated exclusively from days where the value-weighted market portfolio's excess daily return is weakly positive: $\hat{\beta}^+ = \frac{Cov(r_i, r_m \| r_m \geq 0)}{Var(r_m \| r_m \geq 0)}$ I discard the best 1% of market return days before estimating. |
| Risk asymmetry | $\hat{\beta}^- - \hat{\beta}^+$ | The difference between $\hat{\beta}^-$ and $\hat{\beta}^+$ |
| Relative downside risk | $\hat{\beta}^- - \hat{\beta}$ | The difference between $\hat{\beta}^-$ and $\hat{\beta}$ |
| Relative upside risk | $\hat{\beta}^+ - \hat{\beta}$ | The difference between $\hat{\beta}^+$ and $\hat{\beta}$ |
| Market sensitivity | $log(\hat{\beta}^2)$ | The natural logarithm of $\hat{\beta}^2$ |
| Panel B: Reporting flexibility variables | | |
| SOX 404 reporter | *SOX 404* | An indicator equal to one for firm-years in which an auditor issued an opinion regarding the firm's internal controls (i.e., $AUOPIC$ is nonmissing and not equal to zero) |
| Accelerated filer status | *Acc. Filer* | An indicator equal to one for firms with 2002 public floats greater than $75 million |
| Internal control weakness | *ICW* | An indicator equal to one for firm-years in which $SOX$ 404 equals one, and the audit opinion is averse (i.e., $AUOPIC$ not equal to one) |
| Panel C: Incentive variables | | |
| RPE-based pay | *RPE* | An indicator equal to one for firm-years in which the CEO has performance pay tied to "Rel" objectives, according to Incentive Lab |
| Panel D: Control variables | | |
| Firm size | *logSize* | The natural logarithm of the firm's market value of equity |
| Firm age | *logAge* | The natural logarithm of the number of years since a firm first listed in CRSP Monthly Stock File |

| Panel D: Control variables | | |
|---|---|---|
| Growth options | *logBTM* | The natural logarithm of ratio between a firm's book value of equity ($SEQ$) and market value of equity |
| Capital structure | *logDTE* | The natural logarithm of the ratio between book liabilities ($LT$) by book equity ($SEQ$) |
| Liquidity | *logLiq.* | The natural logarithm of average monthly trading volume |
| Idiosyncratic volatility | *logVol.* | The natural logarithm of the standard deviation of monthly returns |
| Profitability | *ROA* | Income before extraordinary items ($IB$) scaled by average total assets |
| Crash risk | *Crash* | Based on Hutton, Marcus, and Tehranian [2009], an indicator equal to one for firm-year observations in which market-adjusted weekly returns are more than three standard deviations below the firm's average at least one time during the fiscal year, where market-adjusted weekly returns are residuals from a regression of weekly excess firm returns on contemporaneous excess returns of the value-weighted market portfolio |
| Earnings downside risk | *EDR* | As described by Konchitchki et al. [2016], the natural logarithm of the ratio of unexpected earnings' lower partial moment to its upper partial moment, with one added to both numerator and denominator: $EDR = log(\frac{1+lower\ partial\ moment}{1+upper\ partial\ moment})$ |

| Panel E: Additional control variables for ICW tests | | |
|---|---|---|
| Loss-making firm | *Agg. Loss* | An indicator equal to one if earnings has been negative in the current and prior fiscal year |
| Bankruptcy risk | *B. Risk* | The decile value for the firm's Altman Z-score, for the current fiscal year |
| Foreign transactions | *Foreign* | An indicator equal to one for firm-years with non-zero foreign currency translations |
| Acquisitions | *ACQ* | The natural logarithm of total acquisition spending from the current and prior fiscal years, scaled by beginning of year market value of equity (MVE): $log(1 + \frac{ACQ_t + ACQ_{t-1}}{MVE_t})$ |
| Number of segments | *logSegs* | The natural logarithm of the number of distinct segments in the Compustat Segments file |
| Extreme growth | *Ex. Growth* | An indicator equal to one for firm-years in which year-over-year revenue growth is in the top quintile for the firm's Fama and French 48 industry |
| Restructuring | *Restruc.* | The natural log of one plus the total restructuring charges from the current and prior fiscal years, scaled by beginning of year market value of equity: $log(1 - \frac{RCP_t + RCP_{t-1}}{MVE_t})$ |

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