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Has Section 404 of the Sarbanes–Oxley Act Discouraged Corporate Investment? New Evidence from a Natural Experiment

Ana Albuquerque,[a,b] Julie Lei Zhu[c]

[a] Questrom School of Business, Boston University, Boston, Massachusetts 02215; [b] Católica-Lisbon School of Business & Economics, 1649-023 Lisbon, Portugal; [c] Fanhai International School of Finance, Fudan University, Shanghai 200433, China

Contact: albuquea@bu.edu, http://orcid.org/0000-0003-3486-0415 (AA); juliezhuqian@gmail.com (JLZ)

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Abstract. Prior studies conclude that an unintended consequence of firms complying with the Sarbanes–Oxley Act is lower levels of risk-taking activities, including investment. We first show that prior studies cannot isolate the effects of SOX from other contemporaneous events. We then use the implementation requirements of SOX404 to construct a natural experiment that isolates the effects of SOX404 for a sample of small firms. We do not find a reduction in investment and other risk-taking activities for firms that had to comply with SOX404, relative to other firms. Because small firms are expected to be the most adversely affected by the regulation, our results cast doubt on the notion that SOX404 had a negative impact on larger firms.

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Keywords: SOX • Section 404 • corporate investment • risk taking • regulation • natural experiment

1. Introduction

The U.S. Congress's passage of the Sarbanes–Oxley Act (SOX) in 2002, following a string of high-profile corporate scandals, resulted in the most significant change in securities regulation since the Securities Act of 1933. One of the most important components of SOX is Section 404 (SOX404), which is arguably the most contentious and onerous section of the act (Zhang 2007, Coates and Srinivasan 2014). Congress's objective in creating SOX404 was to increase the reliability of financial statements to prevent accounting fraud. Section 404 requires that companies document, test, and assess procedures for monitoring their internal systems; that managers file a special "management report," in which they evaluate the firm's internal control system on financial reporting; and that the outside auditor attest to the management's assessment of the companies controls.[1] Commentators and empirical evidence suggest that an unintended consequence of SOX, and SOX404 in particular, was a reduction in investment and risk taking (e.g., Bargeron et al. 2010, Kang et al. 2010).[2] According to these authors, investing in risky projects increases the likelihood that SOX-compliant firms compromise their internal control systems and disclose material weakness in their management reports, which can trigger a stock price decline or litigation.

However, the argument that a financial-reporting burden such as SOX404 would induce a CEO to pass up valuable investment opportunities sharply contrasts with the management objective of promoting shareholder value and firm growth. In fact, some studies suggest that SOX404 could have had a *positive* impact on corporate investment, as investors benefit from greater transparency conferred by improved disclosure, which can lead to lower cost of capital for firms (e.g., Coffee 2007). Indeed, Coates and Srinivasan (2014) cite evidence, from surveys of corporate officers and investors, that SOX404 may have created positive net benefits for firms. The objective of this paper is to reexamine prior evidence and shed new light on the debate of the impact of SOX, in particular Section 404, on corporate investment using a quasi-natural experiment (Meyer 1995) as the main identification strategy.

An important challenge for studies of SOX is to isolate the "SOX effect" from other confounding factors around the same time (Leuz 2007). SOX was passed amid major changes in the business environment and other events with far-reaching economic effects (e.g., the burst of the tech bubble in 2000/2001, 9/11, the 2001 recession, new NYSE and NASDAQ rules, and the Enron and WorldCom scandals), any of which could have affected firms' investment decisions (see, e.g., Coates 2007, Leuz 2007, Ball 2009, Hochberg et al. 2009). Prior studies that analyze the impact of SOX on corporate investment rely on non-U.S. (e.g., UK and

Canadian) firms as controls. Using non-U.S. firms as the benchmark to examine the effects of SOX may impose two limitations. First, it ignores possibly different trends affecting U.S. firms during the period leading up to SOX. Second, it does not account for the fact that U.S. and non-U.S. firms are exposed to different contemporaneous economic and regulatory events.

When we reexamine the Bargeron et al. (2010) results showing that SOX had a strong negative impact on corporate investment and other risk-taking activities for U.S. firms (compared with a control group of UK and Canadian firms), we find that the documented "post-SOX" decrease in investment and other risk-taking behavior actually starts in 1999, and not 2003—the year when SOX became effective. Hence, the decline in capital expenditures and total investment (capital expenditures plus R&D) is consistent with U.S. firms starting to reduce investment in 1999 to adjust to an economic and legal environment that had gone through substantial changes *prior* to SOX's introduction.[3]

To revisit the question of SOX's impact on corporate investment, we use the requirements during the implementation of SOX404 and a sample of small U.S. firms as a "quasi-natural experiment" that isolates the impact of the regulation from contemporaneous events. The use of U.S. firms as a control group allows for a better identification strategy than in prior studies. Specifically, firms with a public float above $75 million in 2002 had to comply with Section 404 in 2004, whereas firms with a public float below $75 million in 2002, 2003, and 2004 did not have to comply until the end of 2007.[4] We compare the behavior of a sample of small firms with a public float that is just above $75 million (the "filers" or "treatment group") in 2002 to the behavior of firms with a public float just below this threshold (the "control group"). This allows us to benchmark the changes in investment activities, from the pre- to post-SOX404 periods, made by similar firms that were forced to comply with the section. The $75 million threshold was not known in 2002, so there is little risk of firms manipulating their public float—an endogeneity concern—at that time.[5] This provides for a natural experiment and allows for a differences-in-differences research design, which mitigates potential biases from unobservable factors that might be correlated with corporate investment and risk-taking activities.

With a sample of 455 unique firms over the period from 2002 to 2005, we find that, relative to control firms, filers do not decrease investment as a result of the enhanced disclosure quality mandated by SOX404. In fact, we find that filers increase total investment (the sum of capital expenditure and R&D) more during the post-SOX404 period than the control group does. There is also no evidence that filer firms increase their cash holdings or that the volatility of their stock returns decreases following SOX404. These results are *inconsistent* with the argument that the level of risk-taking activities by filers becomes lower relative to the control firms because of SOX404. In addition, we find that filer firms receive better terms on their bank loans—greater loan size and lower collateral requirements relative to the control group—following SOX404. These findings are consistent with filer firms benefitting from enhanced transparency following SOX404. Exploiting cross-sectional variations among the sample firms, we find that those likely to benefit more from the regulation (e.g., financially constrained firms and firms facing less litigation risk) invest more following SOX404. Furthermore, we provide a number of robustness tests, including the use of a regression discontinuity design, and obtain similar results. Overall, our tests and results reject the notion that SOX404 had adverse effects on investment and other risk-taking activities.

This paper contributes to the debate on the effects of SOX404, a centerpiece of the set of regulations that represent the most significant economic regulatory actions since the 1930s. By focusing on investment—a core activity and a defining characteristic of any firm—our tests and results shed new light on the impact of SOX404. Bargeron et al. (2010) and Kang et al. (2010), among others, argue that compliance with SOX (and Section 404) may deter companies from making risky investments. These authors did not have the benefit of having a sample of U.S. firms as a control group. They find that compared with non-U.S. firms, large U.S. firms decreased investment more after SOX.[6] We provide evidence that the documented decrease in investment and other risk-taking behavior in Bargeron et al. (2010) could have been caused by concurrent business-cycle trends specific to U.S. firms, rather than SOX. These results call for identification strategies that isolate the effects of SOX from contemporaneous events. By taking advantage of a quasi-natural experiment design that alleviates concerns about confounding events and uses firms in the same legal, regulatory, and economic environment, we show that SOX404 did not lead firms to decrease their level of corporate investment and other risk-taking activities.

Our sample of small firms should help us better understand the effects of SOX404 on corporate investment, for three reasons. First, several studies show that small firms bear disproportionately higher costs, relative to large firms, because of the fixed component of the total compliance costs of SOX404 (e.g., Holmstrom and Kaplan 2003, Chhaochharia and Grinstein 2007, Engel et al. 2007, Piotroski and Srinivasan 2008, Iliev 2010, Alexander et al. 2013). Second, small firms typically have high levels of investment activities and growth opportunities.

These first two considerations suggest that small firms are likely to be the most negatively affected by the SOX404 regulation, both in terms of compliance costs and opportunity costs of missed investment opportunities. Our finding that investment and risk-taking activities do not decrease post-SOX404 in these firms provides important evidence against the presumably negative effect of SOX404 on other, larger firms. A third reason why the use of small firms is instructive is that they play a crucial role in both economic and employment growth (e.g., Dennis et al. 1988, Brown et al. 1990, Haltiwanger et al. 2012).

Any findings on how regulation impacts investment by small firms could have important policy implications. For example, the Securities Exchange Commission (SEC) took several actions, including the creation of an advisory committee, to understand the implications of SOX requirements for small firms (Kamar et al. 2007).

The rest of the paper is organized as follows. In Section 2, we present graphical evidence of corporate investment and risk-taking activities for U.S. and non-U.S. firms overtime, and replicate the results in Bargeron et al. (2010) whereas accounting for concurrent business cycles and time trends. In Section 3, we review the institutional background of SOX404. Section 4 presents the empirical results. Section 5 offers sensitivity analyses and discusses additional robustness tests. Section 6 concludes.

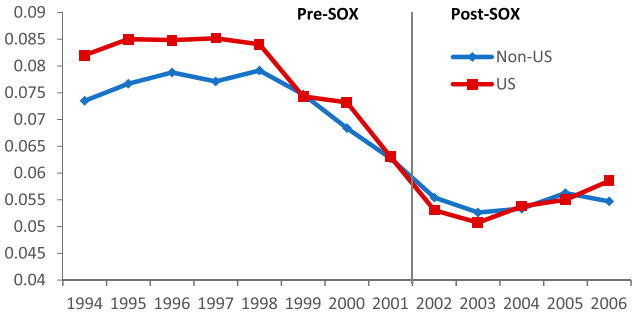
2. Corporate Investment and Risk Taking Over Time for U.S. and Non-U.S. Firms

2.1. Univariate Analysis

Bargeron et al. (2010) compare U.S. firms to a group of non-U.S. firms (from the United Kingdom and Canada) over the sample period of 1994–2006 and find that SOX had a strong negative impact on corporate investment and risk-taking behavior of U.S. firms. However, U.S. firms were exposed to several significant events that were concurrent with SOX, which makes it hard to disentangle whether the decrease in risk-taking behavior was because of SOX or other events. To investigate the impact of SOX on U.S. firms, we first perform a univariate analysis by plotting the mean levels of the variables used to capture investment and risk-taking activities in Bargeron et al. (2010) throughout the 1994–2006 sample period. Figures 1–5 show how the mean of each variable—CAPEX, R&D, INVEST, CASH, and STD—changes through the sample period for U.S. and non-U.S. firms. The figures plot the unconditional behavior of these variables through time. All variables are defined in the appendix.

Figure 1 shows that *CAPEX* moves in tandem for U.S. and non-U.S. firms, decreasing significantly between 1999 and 2003 and then increasing between 2003 and 2006. This figure shows that the lower level of

Figure 1. (Color online) Corporate Investment and Risk-Taking over Time for U.S. and Non-U.S. Firms: Capital Expenditures Scaled by Total Assets



CAPEX for U.S. firms during the post-SOX period (2003–2006) compared with the pre-SOX period (1996–2002) is because of a significant decline in CAPEX investment immediately after 1999 (but before SOX), a period that coincides with the bursting of the high-tech bubble, 9/11, and the 2001 recession. In fact, CAPEX increases following SOX (2003–2006), relative to its pre-SOX level (2000–2002). Figure 2 shows *R&D* for U.S. and non-U.S. firms. The mean *R&D* for the non-U.S. firms shows a declining trend over the sample period, but most of the decline occurs between 1999 and 2002. The mean *R&D* for the U.S. firms shows a decline after 1999 although it does not change significantly throughout the sample period. Figure 3 shows the results for total investment (INVEST), defined as the sum of CAPEX and R&D. Consistent with Figures 1 and 2, the decline in total investment for U.S. and non-U.S. firms starts in 1999 and not in 2002, when SOX was signed into law. Total investment increases rather than decreases during the post-SOX period for the U.S. firms. Although the total investment trend is similar for non-U.S. firms, the fact that the difference in the mean level of investment before and after SOX is larger for the U.S. firms than for the non-U.S. firms explains the results in Bargeron et al. (2010). Figure 4 reports the trend in cash holdings. For U.S. firms, the level of cash holdings decreases between 1994 and 1999

Figure 2. (Color online) Corporate Investment and Risk-Taking over Time for U.S. and Non-U.S. Firms: R&D Expenditures Scaled by Total Assets

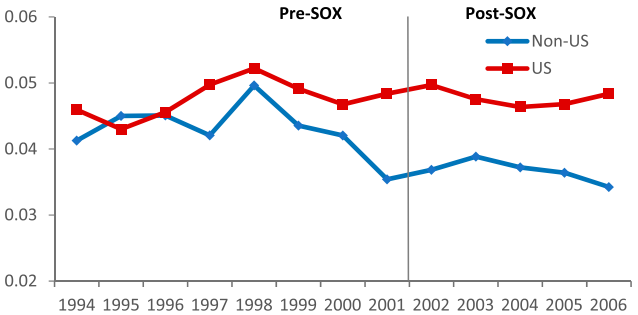
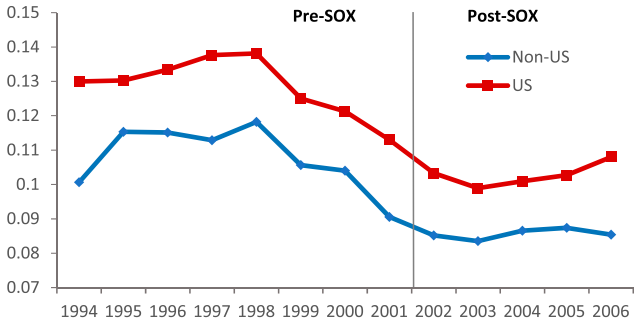


Figure 3. (Color online) Corporate Investment and Risk-Taking over Time for U.S. and Non-U.S. Firms: Total Investment Scaled by Total Assets



and then increases throughout the rest of the sample period (2000–2006), though the rate of increase slows in 2003 and slightly declines after 2004. This evidence is inconsistent with the argument that firms, having become more risk-averse, hold more cash in the post-SOX period. Finally, Figure 5 shows how the average firm's stock volatility evolved throughout the sample period. Volatility increases until 2000, the year when the tech bubble burst, then steadily declines for both U.S. and non-U.S. firms. The fact that the decrease in volatility starts in 2000—before SOX was enacted—calls into question whether risk aversion attributable to SOX was the sole cause of it.

In summary, the above analysis suggests that U.S. firms exhibit a significant decline in investment level and risk-taking activities starting in 1999, well before the 2002 implementation of SOX. The change in investment and risk-taking activities from the pre- to post-SOX period is smaller for non-U.S. firms, which leads to the results in Bargeron et al. (2010).

2.2. Replication of Bargeron et al. (2010)

In this subsection, we first replicate the main results in Bargeron et al. (2010). We then account for time trends affecting U.S. firms by adding time fixed effects to their analysis. Lastly, we redefine the post-SOX period

Figure 4. (Color online) Corporate Investment and Risk-Taking over Time for U.S. and Non-U.S. Firms: Cash Holdings Scaled by Total Assets

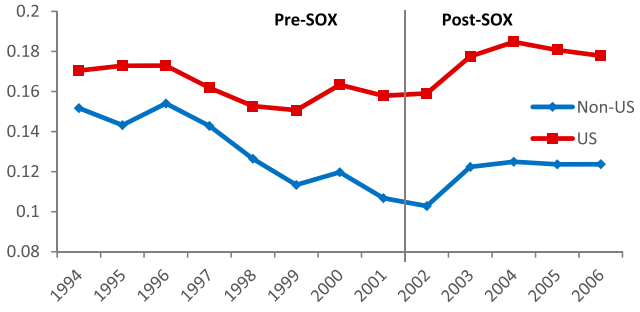
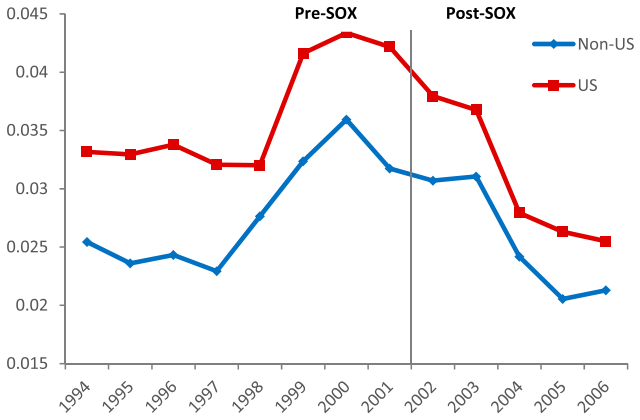


Figure 5. (Color online) Corporate Investment and Risk-Taking over Time for U.S. and Non-U.S. Firms: Standard Deviation of Returns



to start in 1999, so that the effects of the high-tech bubble burst in 2000/2001, 9/11, the 2001 recession, the new NYSE and NASDAQ rules, and the Enron and WorldCom scandals are included. If the decline is attributable to SOX and not to the concurrent events, we would not expect a significant decline in investment and risk-taking activities immediately after 1999.

We replicate the results in Bargeron et al. (2010, table 2) by estimating the following model:

$$\begin{aligned} Y_{i,t} = {} & \alpha + \beta_0 * US\ Post\text{-}SOX_t + \beta_1 * non-US\ Post\text{-}SOX_t \\ & + \beta_2 * Index\ Return_t + \beta_3 * GDP\ Growth_t \\ & + \sum_{m=4}^{n} \beta_m * Control_{i,t-1} \\ & + \sum_{m=4}^{n} \gamma_m * Control_{i,t-1} * US_i + \eta_i + \varepsilon_{i,t}. \end{aligned} \quad (1)$$

Bargeron et al. (2010) define $Y_{i,t}$, a proxy for a firm's investment decisions and willingness to take risks, using the following variables: *CAPEX*, *R&D*, *INVEST* (the sum of *CAPEX* and *R&D*), *CASH*, and *STD*. All variables are defined in the appendix. The variable *US Post-SOX* (*non-US Post-SOX*) is an indicator variable that takes the value of one for U.S. firms (non-U.S. firms) in the years 2003–2006. As in Bargeron et al. (2010), we include GDP growth and an index return to control for the impact of the U.S. and non-U.S. economies' growth on the variables of interest.[7] Following Bargeron et al. (2010), the control variables included are *EBIT* and *MTB* to account for any variation in investment that is related to a firm's profitability or growth opportunities. The *STD* regression includes *EBIT*, *MTB*, and *DEBT* as controls to account for the effects of a firm's profitability, growth prospects, and debt on the volatility of its stock returns. All the control variables are lagged one year. As in Bargeron et al. (2010), all of the continuous variables used in the tests are winsorized

Table 1. Replication of Bargeron et al. (2010)

Panel A. Summary Statistics (modification of panel A of table 1 in Bargeron et al. 2010)

	Means			Medians		
Variable	U.S.	Non-U.S.	*p*-value	U.S.	Non-U.S.	*p*-value
CAPEX	0.0618	0.0655	0.002	0.0415	0.0507	0.000
XRD	0.0771	0.0405	0.000	0.0336	0.0203	0.000
INVEST	0.1338	0.0989	0.000	0.0976	0.0819	0.000
CASH	0.1676	0.1274	0.000	0.0759	0.0889	0.013
STD	0.0343	0.0269	0.000	0.0293	0.0213	0.000
EBIT	0.0586	0.0810	0.000	0.0831	0.0871	0.000
MTB	1.9681	1.6719	0.000	1.4440	1.2511	0.000
DEBT	0.1646	0.2321	0.000	0.1172	0.1762	0.000

Panel B. Multivariate Regressions Analysis When Post-SOX Period Is Years 2003–2006 (columns (1)–(5) replicate the results in modified table 2 of Bargeron et al. 2010)

	(1) *CAPEX*	(2) *R&D*	(3) *INVEST*	(4) *CASH*	(5) *STD*	(6) *CAPEX*	(7) *R&D*	(8) *INVEST*	(9) *CASH*	(10) *STD*
US Post-SOX	−0.017***	−0.005***	−0.022***	0.021***	−0.006***	−0.001	−0.004**	−0.005**	0.019***	−0.004***
	(−29.39)	(−10.06)	(−28.25)	(13.62)	(−39.58)	(−0.82)	(−2.52)	(−2.14)	(4.04)	(−6.00)
Non-US Post-SOX	−0.016***	−0.001	−0.017***	0.000	−0.002***					
	(−10.18)	(−0.62)	(−8.06)	(0.03)	(−4.05)					
Index Return	0.024***	0.009***	0.033***	−0.011***	−0.009***	0.006	0.009	0.016	−0.047*	−0.007**
	(16.69)	(7.75)	(17.27)	(−2.91)	(−21.79)	(0.70)	(1.28)	(1.30)	(−1.88)	(−2.47)
GDP Growth	0.269***	0.063**	0.333***	0.154	0.166***	0.077	−0.085	−0.008	0.083	0.088***
	(7.19)	(2.12)	(6.71)	(1.52)	(14.94)	(0.93)	(−1.28)	(−0.07)	(0.37)	(3.07)
EBIT	0.052***	−0.051***	0.001	0.214***	−0.016***	0.048***	−0.051***	−0.003	0.212***	−0.013***
	(5.84)	(−7.18)	(0.08)	(8.91)	(−5.55)	(5.44)	(−7.25)	(−0.28)	(8.84)	(−5.06)
*US*EBIT*	−0.008	−0.076***	−0.084***	−0.086***	−0.003	−0.006	−0.077***	−0.083***	−0.087***	−0.004
	(−0.84)	(−10.36)	(−6.88)	(−3.45)	(−1.07)	(−0.66)	(−10.39)	(−6.79)	(−3.47)	(−1.57)
MTB	0.004***	0.006***	0.010***	0.006**	−0.001***	0.004***	0.006***	0.010***	0.006***	−0.001***
	(4.50)	(9.60)	(9.19)	(2.57)	(−3.54)	(4.54)	(9.82)	(9.37)	(2.79)	(−3.10)
*US*MTB*	0.001	0.003***	0.004***	0.012***	0.001***	0.001	0.002***	0.004***	0.011***	0.001***
	(1.41)	(3.92)	(3.43)	(4.96)	(4.56)	(1.19)	(3.60)	(3.08)	(4.71)	(3.85)
DEBT					0.028***					0.023***
					(10.72)					(9.53)
*US*DEBT*					0.001					−0.001
					(0.21)					(−0.44)
Constant	0.044***	0.029***	0.073***	0.113***	0.027***	0.041***	0.033***	0.074***	0.121***	0.025***
	(32.35)	(26.32)	(40.29)	(30.59)	(62.97)	(13.84)	(13.95)	(18.90)	(15.08)	(25.12)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Observations	30,267	30,267	30,267	30,267	28,273	30,267	30,267	30,267	30,267	28,273
Number of firms	2,363	2,363	2,363	2,363	2,238	2,363	2,363	2,363	2,363	2,238
Adjusted R^2	0.01	0.08	0.04	−0.03	0.10	0.02	0.09	0.05	−0.03	0.19
F-test	0.24	8.42***	4.48**	22.18***	37.90***					
U.S. = non-U.S., *p*-value	0.628	0.004	0.034	0.000	0.000					

Table 1. (Continued)

Panel C. Multivariate Regressions Analysis When Post-SOX Period Is Years 1999–2002										
	(1) *CAPEX*	(2) *R&D*	(3) *INVEST*	(4) *CASH*	(5) *STD*	(6) *CAPEX*	(7) *R&D*	(8) *INVEST*	(9) *CASH*	(10) *STD*
US Post-SOX	−0.008***	−0.004***	−0.012***	−0.016***	0.009***	−0.004	−0.004*	−0.008**	0.032***	0.000
	(−8.95)	(−6.16)	(−10.53)	(−6.76)	(36.87)	(−1.64)	(−1.93)	(−2.40)	(4.70)	(0.24)
Non_US Post-SOX	−0.006***	−0.001	−0.007***	−0.036***	0.007***					
	(−3.19)	(−0.70)	(−2.87)	(−7.49)	(11.24)					
Index Return	0.014***	0.004**	0.018***	−0.040***	0.003***	−0.008	−0.003	−0.012	0.067	−0.004
	(6.67)	(2.36)	(6.52)	(−7.34)	(5.43)	(−0.52)	(−0.27)	(−0.55)	(1.60)	(−0.83)
GDP Growth	0.338***	0.080**	0.418***	−0.072	0.158***	0.141	−0.036	0.105	−0.017	0.063**
	(7.88)	(2.44)	(7.51)	(−0.65)	(13.44)	(1.47)	(−0.49)	(0.85)	(−0.07)	(1.99)
EBIT	0.029***	−0.061***	−0.033**	0.253***	−0.017***	0.030***	−0.060***	−0.030**	0.247***	−0.017***
	(2.58)	(−7.20)	(−2.26)	(8.76)	(−5.47)	(2.67)	(−7.05)	(−2.10)	(8.54)	(−5.39)
*US*EBIT*	0.013	−0.050***	−0.037**	−0.092***	0.002	0.012	−0.052***	−0.040***	−0.088***	0.001
	(1.16)	(−5.66)	(−2.45)	(−3.07)	(0.49)	(1.02)	(−5.80)	(−2.63)	(−2.90)	(0.28)
MB	0.004***	0.006***	0.010***	0.001	−0.001***	0.004***	0.007***	0.011***	0.001	−0.001***
	(3.76)	(8.16)	(7.71)	(0.36)	(−3.40)	(3.90)	(8.30)	(7.90)	(0.50)	(−3.11)
*US*MB*	0.000	0.001	0.001	0.015***	0.001***	−0.000	0.000	0.000	0.014***	0.001***
	(0.23)	(0.66)	(0.57)	(5.36)	(3.26)	(−0.05)	(0.45)	(0.23)	(5.14)	(2.75)
DEBT					0.023***					0.022***
					(6.86)					(6.57)
*US*DEBT*					0.002					0.002
					(0.46)					(0.69)
Constant	0.048***	0.033***	0.081***	0.133***	0.023***	0.040***	0.034***	0.074***	0.110***	0.031***
	(27.27)	(24.66)	(35.55)	(29.26)	(45.91)	(10.36)	(11.65)	(14.86)	(11.05)	(26.00)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Observations	21,016	21,016	21,016	21,016	19,569	21,016	21,016	21,016	21,016	19,569
Number of firms	2,362	2,362	2,362	2,362	2,236	2,362	2,362	2,362	2,362	2,236
Adjusted R^2	−0.06	0.01	−0.03	−0.07	0.09	−0.06	0.01	−0.03	−0.07	0.10
F-test:	1.23	4.88**	4.65**	16.56***	3.23*					
U.S. = non-U.S., *p*-value	0.267	0.027	0.031	0.000	0.072					

Notes. The sample includes firms with at least 12 years of data available from Compustat and CRSP for all U.S. firms and from Datastream for non-U.S. firms during the 1994–2006 period. We exclude financial firms and those non-U.S. firms that are not primarily listed in each home country (Canada and the United Kingdom). The dependent variables are *CAPEX* (columns (1) and (6)), *R&D* (columns (2) and (7)), *INVEST* (columns (3) and (8)), *CASH* (columns (4) and (9)), and *STD* (columns (5) and (10)). *US Post-SOX* (*non-US Post-SOX*) is an indicator variable equal to one for U.S. firms (non-U.S. firms) in the years 2003–2006 (1999–2006) in panel B (panel C). Following Bargeron et al. (2010), (i) *Index Return* is the return on the S&P500 index for U.S. firms, the return on the FTSE100 index for UK firms, and the return on the TSX Composite index for Canadian firms; (ii) *GDP Growth* is the growth in GDP for the respective firm's country; (iii) control variables other than the *Post-SOX* indicators are lagged one year; and (iv) variables other than *Index Return* and *GDP Growth* are winsorized at the 1% and 99% levels. The remaining variables are defined in the appendix. The F-tests test whether the coefficient on *US Post-SOX* equals the coefficient on *Non-US Post-SOX*. *t*-statistics are in brackets and are based on two-sided tests. Panel A presents summary statistics for the main variables used in the tests across the U.S. sample and the non-U.S. sample and tests for differences between the two samples. Panel B (C) presents the results of estimating Equation (1) and post-SOX defined as years of 2003–2006 (1999–2002). Columns (1)–(5) ((6)–(10)) present results without (with) year fixed effects.

***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

at the 1% and 99% levels, and the regression models are estimated using firm fixed effects, η_i, to control for omitted firm characteristics that are time invariant.

We gather U.S. financial data from Compustat and CRSP, and UK and Canada financial data from Datastream, for the sample period 1994–2006.[8] The GDP data are gathered from the International Monetary Fund's website. Similar to Bargeron et al. (2010), (i) we require at least 12 years of data for asset, cash holding, and capital expenditure for both U.S. firms and non-U.S. firms; (ii) we exclude financial firms; (iii) we keep only non-U.S. firms that are primarily listed in each home country (Canada and the United Kingdom) and not cross-listed; (iv) we define STD as the standard deviation of returns over the January to December calendar year; and (v) we measure GDP growth over a calendar basis. Like Bargeron et al. 2010, we define each fiscal year as August to July (i.e., the 2003 fiscal year begins on August 1, 2002, and ends on July 31, 2003 (see Bargeron et al. 2010), as SOX was passed in late July 2002. In other words, firms with fiscal year ends between July 31, 2003, and June 30, 2004, are assigned a fiscal year of 2003.[9]

Table 1 presents the results. Panel A reports summary statistics for the variables used in the tests across the U.S. sample and the non-U.S. sample and for tests for differences between the two samples. As in Bargeron et al. (2010), the U.S. sample has higher levels of investment (INVEST), CASH (mean only), STD, and MTB. The mean and median values are also similar to those in Bargeron et al. (2010).

Panel B (columns (1)–(5)) replicates the main results in Bargeron et al. (2010). The results show that, compared with the non-U.S. firms, the level of investment and risk-taking activities decreased significantly for U.S. firms in the post-SOX period. In particular, the coefficients on the variable *US Post-SOX* in the CAPEX, R&D, INVEST, CASH, and STD regressions are remarkably close to those in Bargeron et al. (2010).[10] Columns (6)–(10) add year fixed effects to the analysis. Note that the variable *non-US Post-SOX* drops out because of the inclusion of the year fixed effects. After controlling for the time trend of the variables, the coefficient on *US Post-SOX* becomes statistically insignificant for CAPEX (coefficient = −0.001, *t*-value = −0.82) and of smaller magnitude and much less statistically significant for INVEST and STD.

Because Figures 1–5 suggest that the decline in investment and risk taking actually starts in 1999, we next replicate the above analysis defining the post-SOX period to be between 1999 and 2002 to exclude the impact of SOX. Panel C presents the results. Columns (1)–(5) ((6)–(10)) present the results without (with) year fixed effects. Confirming the univariate results, we find that the results are similar to those in panel B.

In sum, these results cast doubt on the premise that the decrease in investment and risk-taking behavior cannot be solely attributed to SOX. To uncover new information on the impact of SOX on U.S. firms' willingness to invest and to engage in risk-taking behavior, we thus focus on the implementation of SOX404. This allows us to use U.S. firms, rather than non-U.S. firms, as a control group.

3. Institutional Background

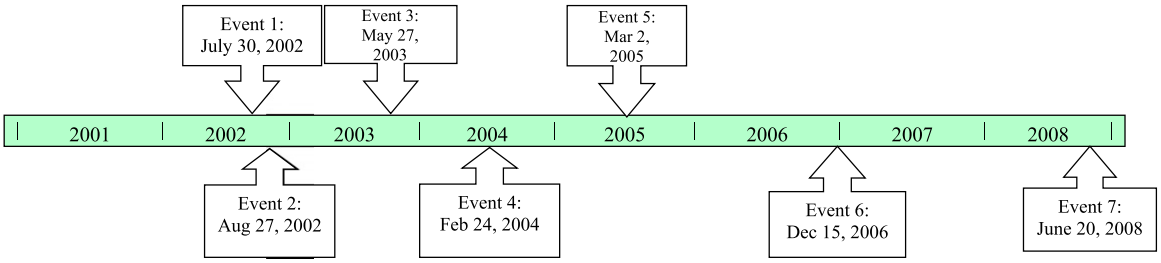
In this section, we provide a brief description of the institutional background and sequence of key events regarding Section 404 of SOX, and SOX more generally. We then explain why this implementation provides an empirical setting that allows us to isolate the effects of SOX404 from those of other, potentially confounding events.

Shortly after SOX was signed into law on July 30, 2002, the SEC required publicly listed companies to declare accelerated filer status in their 2002 annual reports, based on the size of their public float in the second (fiscal) quarter of 2002 (see Figure 6), which for most firms was June 30, 2002. The public float is the fraction of the common stock *not* held by insiders such as managers, employees, and board members, and is reported on the first page of the company's 10-K. Firms with a float over the $75 million threshold became "accelerated filers" and had to keep that status going forward (see Exchange Act Rule 12b-2).[11] Accelerated filers had to complete their 10-K filing within 75 days of their fiscal year end. (The previous deadline had been 90 days.) In May 2003, under the SOX404 guidelines (SEC release 33-8238), the SEC announced that all accelerated filers had to include in their annual reports a management report on internal controls over financial reports beginning in 2004. The rule mandated that management assess the effectiveness of the company's internal control, identify the framework used to evaluate the effectiveness of the company's internal control, and include in the management report a statement that the company's auditor issued an attestation report on management's assessment of the company's internal control over financial reporting. However, firms with a public float under the $75 million threshold in 2002, 2003, and 2004 did not have to comply with SOX404 and could file the management report at a later date. At the time, these firms were also exempted, until June 2010, from having to provide an auditor's attestation of the management report.[12]

We develop our empirical tests based on this sequence of events. First, we use companies' public floats, as reported in their 2002 10-Ks, to define the treatment and control groups. The key to our empirical strategy is that firms declared their accelerated filer status at the end of the second quarter of fiscal year 2002. Because, at that time, firms did not know that a declaration of accelerated filer status would require them to file a management report in 2003 (SOX404 compliance), our classification of the treatment and control groups

Figure 6. (Color online) Corporate Investment and Risk-Taking over Time for U.S. and Non-U.S. Firms: Key Events and Timeline

- **Event 1:** The Sarbanes-Oxley Act is signed into law.
- **Event 2:** SEC Press Release No. 2002-128: Accelerated filer status definition and acceleration schedule.
- **Event 3:** The SEC votes to adopt rules concerning management's report on internal control (Section 404). Accelerated filers are expected to comply for fiscal years ending on or after June 15, 2004. All other issuers are required to comply for their fiscal years ending on or after April 15, 2005.
- **Event 4:** The SEC approves an extension of the original compliance dates. For accelerated filers, the compliance dates are extended to fiscal years ending on or after November 15, 2004, for non-accelerated filers, to fiscal years ending on or after July 15, 2005.
- **Event 5:** The SEC extends Section 404 compliance dates for non-accelerated filers and foreign private issuers to the firm's first fiscal year ending on or after July 15, 2006.
- **Event 6:** The SEC proposes providing further relief for non-accelerated filers regarding Section 404 compliance dates. The compliance date is moved to the firm's first fiscal year ending on or after December 15, 2007. The compliance date to provide an auditor's attestation report on interval control is moved to the fiscal year ending on or after December 15, 2008.
- **Event 7:** The SEC approves an additional one-year extension of the compliance date for smaller public companies to meet Section 404's auditor attestation requirement. Smaller companies will now be required to provide the auditor's attestation report for fiscal years ending on or after December 15, 2009.



based on the 2002 public float avoids concerns about manipulation of, or endogeneity in, the public float. This follows from Iliev (2010, figure 1), who finds no evidence consistent with manipulation of public float in 2002, but finds evidence that after 2003—and especially in 2004—firms manipulated their public float to evade compliance. Similarly, Gao (2016) shows that small firms had an incentive to manipulate their public float to avoid complying with SOX404 in 2004 (see also Gao et al. 2009, Nondorf et al. 2012).

Specifically, we classify firms with a public float over the $75 million threshold in 2002 as mandatory filers, since they must comply with SOX404 starting in 2004 (regardless of the size of their float in 2003 or 2004). These firms represent the treatment group. Firms with a public float below the threshold in 2002 represent the control group; their changes from the pre- to post-SOX404 periods are used as the counterfactual to measure expected changes in the treatment group. The choice of control group in our empirical setting is *conservative* because any firms whose float grew past the $75 million threshold in 2003 or 2004 ultimately had to comply with SOX404. The inclusion of such firms in the control group biases against finding evidence that SOX404 had any differential effect on the filers' risky investment activities relative to the control. In our main tests, we do not differentiate between these firms and the rest of the firms in the control group, because the control firms that ultimately had to comply with SOX404 revealed their actions *after* the SOX404 threshold requirements were announced.

Because firms above the threshold were required to file their first management report in 2004 (for the fiscal year ending on or after June 15, 2004, and later postponed for fiscal years ending on or after November 15, 2004), we define the pre-SOX404 period to include fiscal years 2002 and 2003 and the post-SOX404 period to include fiscal years 2004 and 2005.[13] We follow the Compustat definition of fiscal year, where firms with fiscal year ends between June 1, 2004, and May 31, 2005, as an example, are assigned a fiscal year of 2004.[14] Focusing on this period of analysis has three main advantages. First, the fixed costs of compliance are arguably expected to have the strongest impact on firms' actions in the early years of SOX404's implementation, as firms adapt to the new regulation. Second, the pre-SOX404 period coincides with the enactment of the other SOX provisions, which were implemented immediately after SOX's 2002 enactment and remained in effect in both the pre- and post-SOX404 periods. This allows us to distinguish the effects of SOX404 from the effects of those provisions. Third, focusing on a short time span around the event alleviates concerns regarding serial correlation of the residuals in differences-in-differences estimates (Bertrand et al. 2004).

4. Empirical Results

4.1. Sample Selection

Our sample consists of 455 unique firms with a public float between \$50 million and \$150 million in 2002, excluding financial firms (with SIC codes between 6000 and 6900) and regulated firms (with SIC codes between 4900 and 4939).[15] The public float equals the value of the common stock owned by outside investors, which we hand-collect from the cover page of each firm's 10-K report. Accounting and financial information is obtained from Compustat. Several studies show that many small firms deregister their common stock ("go dark") or go private to avoid the costs of complying with SOX (see Engel et al. 2007, Leuz et al. 2008). To alleviate concerns about survivorship biasing the results toward finding a positive effect of SOX404 on filers relative to the control group, we use a constant sample of filer and control firms with available data for the full sample period of 2002–2005.[16]

Table 2. Sample Description and Summary Statistics

Panel A: Summary Statistics for Filers and Control Firms in 2002

	Means			Medians		
	Filer (N = 292)	Control (N = 163)	p-value	Filer (N = 292)	Control (N = 163)	p-value
Float	105.68	61.99	0.00	101.80	62.09	0.00
CAPEX	0.043	0.047	0.35	0.027	0.029	0.65
R&D	0.087	0.130	0.02	0.020	0.033	0.33
INVEST	0.131	0.177	0.01	0.091	0.098	0.18
CASH	0.261	0.256	0.85	0.176	0.207	0.66
STD	0.047	0.045	0.42	0.045	0.043	0.31
EBIT	–0.045	–0.121	0.01	0.026	0.009	0.10
DEBT	0.180	0.156	0.33	0.062	0.058	0.70
MTB	1.434	1.882	0.10	0.970	0.971	0.86
ASSETS	242.54	130.46	0.00	152.73	96.13	0.00
MKT CAP	134.28	87.70	0.00	125.30	80.46	0.00
REVENUES	262.24	145.73	0.00	122.89	86.35	0.00
STKRET	–0.103	–0.117	0.71	–0.248	–0.207	0.72
ASSET Growth	–0.057	–0.077	0.54	–0.029	–0.043	0.49
MKT CAP Growth	–0.324	–0.316	0.91	–0.218	–0.183	0.45

Panel B: Summary Statistics for Filers and Control Firms Across the Pre-SOX404 and Post-SOX404 Periods

	Filers				Control firms				
	Pre-SOX404 (N = 292 mean)	Post-SOX404 (N = 292 mean)	Difference	p-value	Pre-SOX404 (N = 163 mean)	Post-SOX404 (N = 163 mean)	Difference	p-value	Filer vs. control (difference of differences p-value)
CAPEX	0.038	0.041	0.003	0.150	0.039	0.040	0.001	0.673	0.604
R&D	0.069	0.067	–0.002	0.682	0.083	0.077	–0.006	0.415	0.137
INVEST	0.114	0.117	0.003	0.632	0.133	0.129	–0.003	0.696	0.298
CASH	0.271	0.270	–0.002	0.897	0.272	0.288	0.016	0.411	0.117
STD	0.042	0.032	–0.010	0.000	0.041	0.033	–0.008	0.000	0.101
EBIT	–0.016	0.008	0.024	0.007	–0.049	–0.024	0.025	0.068	0.614
DEBT	0.149	0.125	–0.024	0.018	0.132	0.099	–0.033	0.006	0.251
MTB	1.673	1.837	0.164	0.019	1.818	2.043	0.225	0.031	0.424

Notes. The sample includes firms with public floats between \$50 million and \$150 million in 2002 and with data available from Compustat during the 2002–2005 period. The public float equals the value of the company owned by outside investors as reported in the 10-K. Filers are firms with a public float above \$75 million; control firms have a public float below \$75 million in 2002. Panel A reports summary statistics for filers and control firms in 2002 (the year SOX was signed into law). Panel B reports summary statistics for filers and control firms in the pre- (2002–2003) and post-SOX404 (2004–2005) periods, as well as summary statistics for the differences between the pre- and post-SOX404 periods for the two groups of firms. *CAPEX* is capital expenditures divided by average assets for the year. *R&D* is research and development expenditures divided by average assets for the year. *INVEST* is the sum of *CAPEX* and *R&D*. *CASH* is the year-end level of cash and short-term investments divided by average assets. *STD* is the standard deviation of the returns for the year. *EBIT* is the earnings before interest and taxes divided by average assets. *MTB* is the year-end market value of assets divided by the year-end book value of assets (*ASSETS*). *DEBT* is the average total debt divided by the average market value of total assets. *MKT CAP* is the year-end market capitalization. *REVENUES* is total sales revenues. *STKRET* is the annual stock returns. *ASSET Growth* (*MKT CAP Growth*) represents the percentage annual growth in total assets (market capitalization). These variables are winsorized at the 1% and 99% levels. N represents the number of firms. The remaining variables are defined in the appendix. p-values are based on the results from difference in means tests and from Wilcoxon rank-sum tests for differences in medians.

4.2. Descriptive Statistics

Table 2 reports descriptive statistics for the sample firms. Panel A presents summary statistics for the firm characteristics across both groups for the fiscal year 2002—just before SOX took effect—as well as the *p*-values for the difference in means and medians across the two groups. The average 2002 public float is \$106 million for the filers and \$62 million for the control group. Sixty-four percent (292/[292+163]) of the firms are mandatory filers and represent the treatment group; the remaining 36% are nonfilers and represent the control group. Total investment scaled by total assets (*INVEST*) is statistically significantly higher for the control group than for the filers, mainly because of their investment in research and development (*R&D*); the mean values for *INVEST* are 0.177 and 0.131 for the control group and filers, respectively.[17] The mean control firm is relatively less profitable; mean earnings before interest and taxes scaled by assets (*EBIT*) are –0.121 for control firms and –0.045 for filers. When comparing the median filer to the median control firm, we do not find differences in investment levels or profitability measures.

As expected, and by construction, the mean control firm is relatively smaller than the mean filer firm: the mean market capitalization (*MKT CAP*) is \$88 million for control firms and \$134 million for filers. Although the control and filer firms differ in size, both groups belong to the lowest value decile as determined by the NYSE breakpoints, based on their market capitalization (Fama and French 1992). Filers and control firms do not seem to exhibit different levels of growth opportunities in 2002; for example, in terms of means, the two groups are not statistically different ($p < 0.10$) in market-to-book ratio of assets (*MTB*) or the asset growth (*ASSET Growth*). Bargeron et al. (2010) argue that if a firm engages in less risk-taking behavior because of SOX, then the amount of cash—the riskless asset—held by the firm would increase and the volatility of the firm's stock returns would decrease. Panel A also shows that cash and cash equivalents (short-term investments) scaled by total assets (*CASH*) and the standard deviation of returns (*STD*) are also not statistically different across the two groups. In summary, the average filer exhibits relatively lower levels of investment and higher accounting performance prior to the enactment of SOX404. However, the growth prospects across the two groups do not seem significantly different.

Panel B provides descriptive statistics for the variables of interest across the pre-SOX404 (2002–2003) and post-SOX404 (2004–2005) periods for control firms and filers. From the pre-SOX404 period to the post-SOX404 period, filers exhibit an increase in capital expenditures (*CAPEX*) and total investment (*INVEST*) and a decrease in R&D, but the differences are not statistically significant at conventional levels. For the control firms, the change in investment (*CAPEX* and *R&D*) from the pre-SOX404 period to the post-SOX404 period is similar to that of the filer firms, but the difference is also not statistically significant. Following SOX404, volatility (*STD*) decreases and profitability (*EBIT*) and growth opportunities (*MTB*) increase for both the filer group and the control group. In the last column of panel B, we test for differences between the filers and control firms in the observed changes from the pre-SOX404 period to the post-SOX404 period. None of the univariate changes between filers and control firms are statistically different at common significance levels. In sum, at the univariate level, the change in investment and risk-taking behavior from the pre-SOX404 period to the post-SOX404 period seems to be similar for both filer and control firms, suggesting that SOX404 did not negatively impact filer firms.

To understand the potential impact of SOX404 for both the filer and the control firms and to evaluate parallel trends prior to SOX404, we plot the mean levels of the variables capturing investment and risk-taking activities from 1994 to 2006, while emphasizing the pre-SOX404 (2002–2003) and post-SOX404 (2004–2005/6) periods. Figures 7–11 show how each of the variables—*CAPEX*, *R&D*, *INVEST*, *CASH*, and *STD*—change for filers, control firms, and large firms (for comparison purposes), with the latter defined as all Compustat firms with market capitalization above \$150 million.

Figure 7 shows that, for both filer and nonfiler firms, *CAPEX* decreases between 1996 and 2003, with a more accentuated decline immediately after 2000, then increases between 2003 and 2006. Thus, compared with the pre-SOX404 period, CAPEX seems to increase following SOX404 for both groups. Figure 8 shows that R&D increases from 1995 to 1998 for both groups, declines in 1999 and 2000 for both groups, increases slightly from 2000 to 2006 for the filers, and increases

Figure 7. (Color online) Corporate Investment and Risk Taking over Time for Small Filer Firms, Control Group, and Large Filer Firms: Capital Expenditures Scaled by Total Assets

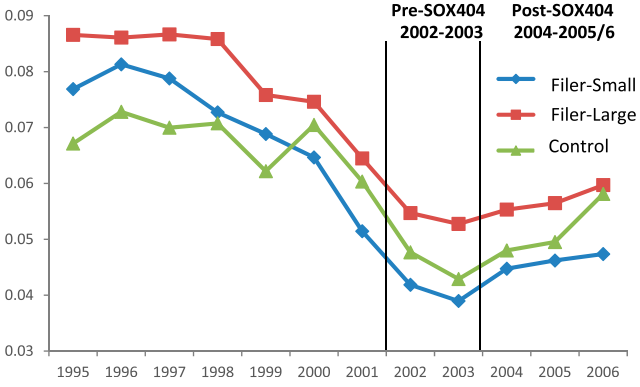
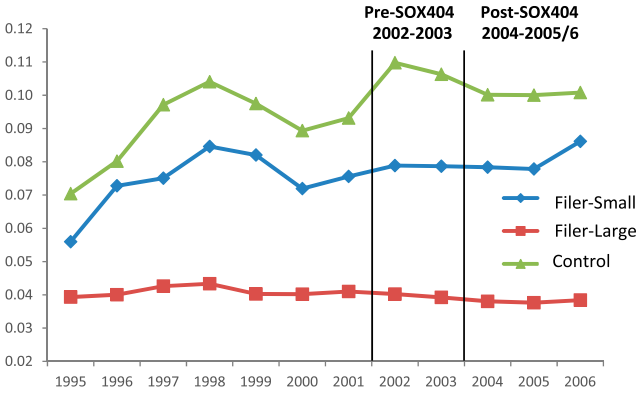


Figure 8. (Color online) Corporate Investment and Risk Taking over Time for Small Filer Firms, Control Group, and Large Filer Firms: R&D Expenditures Scaled by Total Assets



from 2000 to 2002 and declines slightly thereafter for the control group. These results are consistent, at a univariate level, with filer firms not decreasing their R&D expenditure in the post-SOX404 period. However, the different pattern of R&D for the two groups from 2002 to 2006 raises a concern about the preevent parallels trend assumption in a difference-in-difference design (see Roberts and Whited 2013). Therefore, in Section 5.3 we perform a robustness test by using a regression discontinuity design that is not sensitive to the parallels trend assumption, and draw similar conclusions. Consistent with Figure 7, the decline in investment for all three groups in Figure 9 starts in 1999 and not when SOX404 became effective in 2004. During the post-SOX404 period, investment *increases* for both filer and control firms. Figure 10 reports the trend in cash holdings. For all three groups, the level of cash holding exhibits an upward time trend throughout the entire period but declines (especially among filers) in the post-SOX404 period. This evidence is inconsistent with the argument that filer firms become more risk

Figure 9. (Color online) Corporate Investment and Risk Taking over Time for Small Filer Firms, Control Group, and Large Filer Firms: Total Investment Scaled by Total Assets

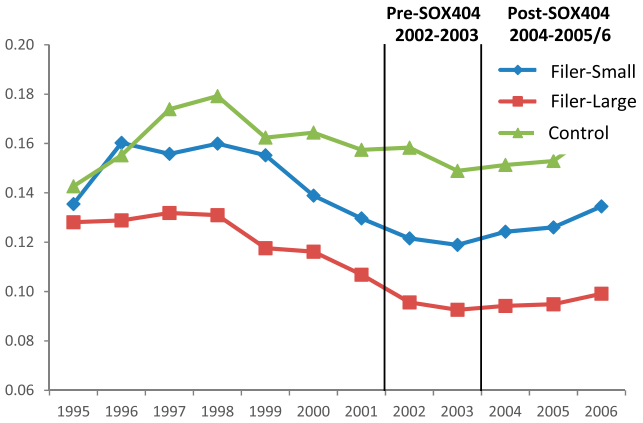
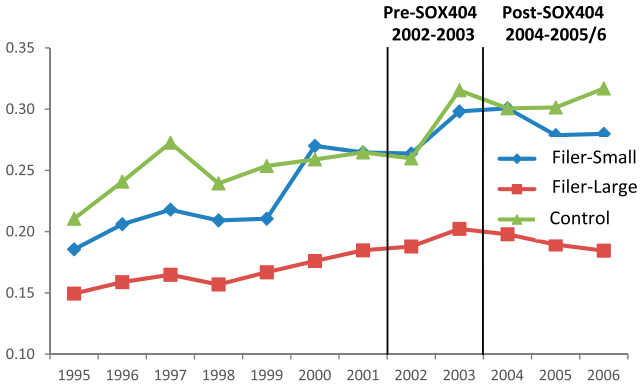


Figure 10. (Color online) Corporate Investment and Risk Taking over Time for Small Filer Firms, Control Group, and Large Filer Firms: Cash Holdings Scaled by Total Assets



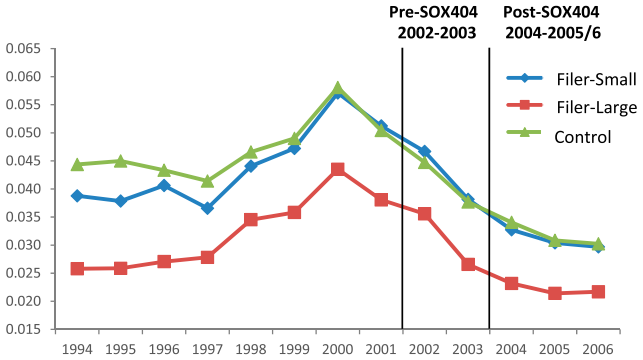
averse and hold more cash in the post-SOX404 period. Finally, Figure 11 shows how the average firm's stock volatility evolved throughout the sample period. Volatility increases until 2000—when the tech bubble burst—then steadily declines. Volatility moves in tandem for filer and control firms (as well as larger firms) both during the pre- and post-SOX404 periods, suggesting that SOX404 had no impact on firms' volatility.

4.3. Impact of Section 404 on Filers vs. Control Firms Using a Differences-in-Differences Methodology

4.3.1. Investment and Risk-Taking Activity. To test whether compliance with SOX's Section 404 impacted investment and risk-taking decisions differently for filers than for the control group, we estimate the following regression model:

$$Y_{i,t} = \alpha + \beta_0 * Post\text{-}SOX404_t + \beta_1 * Post\text{-}SOX404_t * Filer_i + \sum_{m=2}^{n} \beta_m * Control_{i,t-1} + \sum_{m=1}^{n} \gamma_m * Control_{i,t-1} * Filer_i + \nu_t + \eta_i + \varepsilon_{i,t}, \quad (2)$$

Figure 11. (Color online) Corporate Investment and Risk Taking over Time for Small Filer Firms, Control Group, and Large Filer Firms: Standard Deviation of Returns



where $Y_{i,t}$ is any of the following variables: *CAPEX*,[18] *R&D*, *INVEST* (the sum of *CAPEX* and *R&D*),[19] *CASH*, and *STD*. The control variables are the same as mentioned in Section 2. *Post-SOX404* is an indicator variable that takes the value of one for the period from 2004 to 2005. *Filer* is an indicator variable that takes the value of one if a firm's public float is above the $75 million threshold in 2002. The regression models are estimated using year and firm fixed effects.[20] The year fixed effects, ν_t, control for any unobservable time-aggregate effect or time trend in the dependent variables.[21] The firm fixed effects, η_i, control for any omitted firm characteristics that are time invariant. Note that we do not include the variable "*Filer*" by itself in Equation (2), because it drops out because of the inclusion of the firm fixed effects.

Equation (2) also adds interaction terms between the control variables and a dummy variable for the filers to capture any differential marginal impact of the control variables on the variable of interest for these firms. The coefficient β_0 captures the change in the outcome variable between the pre- and post-SOX404 periods for all firms. However, documenting such a change is not sufficient to show causality, because the change in Y_{it} could have been caused by concurrent events that affect all firms. The empirical model specified in Equation (2) allows for a differences-in-differences test, where the coefficient β_1 captures the change in the outcome variable between the pre- and post-SOX404 periods for the filers, benchmarked against the change for the control group during the same period. If SOX404 caused firms to invest less or engage in less risk-taking behavior, as prior studies suggest, we would expect $\beta_1 < 0$. If, instead, the level of corporate investment or risk-taking behavior is because of general economic conditions, or if greater transparency and lower cost of capital (or better credit terms) because of SOX404 lead to higher investment by filers, then $\beta_1 \geq 0$. When the Y_{it} variable is *CASH*, the expectation is different because of the negative association between CASH and risk-taking behavior. Thus, if firms engage in less (more) risk-taking behavior following SOX404, then we would expect a higher (lower) level of the riskless asset—cash—and $\beta_1 > 0$ ($\beta_1 < 0$). If SOX404 has no impact on risk-taking behavior, we would expect $\beta_1 = 0$.

Table 3 presents the results of estimating Equation (2) for each of the five dependent variables. The results in column (1) show that the level of *CAPEX* is *higher* in the post-SOX404 period than it is in the pre-SOX404 period (coefficient of 0.005), though the change is not statistically significant (*t*-value is 1.62). The results also show that the post-SOX404 change in *CAPEX* for filer firms is not statistically significantly different from the change for the control group. However, the level of *CAPEX* is higher for more profitable firms (the coefficient on *EBIT* is 0.033, $p < 0.001$) and for filer firms with more growth opportunities (the coefficient on *MTB*Filer* is 0.010, $p < 0.01$).

Column (2) presents the results for the level of *R&D*. The level of *R&D* is lower during the post-SOX404 period (coefficient of −0.01, $p < 0.10$) than during the pre-SOX404 period (as suggested by Figure 8), providing some evidence that firms decrease their investment in risky R&D projects following SOX404. However, the filer firms increase their investment in R&D relative to the control group during the post-SOX404 period (the coefficient on *Post-SOX404*Filer* of 0.013, $p < 0.05$). The results in column (3) for total investment show that filer firms increase their corporate investment in the post-SOX404 period compared with the control group (the coefficient on *Post-SOX404*Filer* is 0.012, $p < 0.10$). Thus, the results suggest that compliance with SOX404 did not lead filer firms to invest less than the control group.

Column (4) presents the results for *CASH*. The results show that firms decrease their level of cash assets following SOX404, and that the decrease is stronger for firms complying with SOX404 than for the control group (coefficient on *Post-SOX404*Filer* of −0.028, $p < 0.05$). The fact that post-SOX404 filer firms hold less of the riskless asset suggests that these firms engage in more risk-taking activities, not fewer. Column (5) presents the results for stock volatility. The results show that stock-return volatility decreases following SOX404 for both the filer firms and the control group (the coefficient on *Post-SOX404* is −0.005 ($p < 0.01$), but the coefficient on *Post-SOX404*Filer* is not significant). That the decline in volatility occurs in both groups suggests that SOX404 did not cause the decline in filer firms.

Overall, we find that, compared with the control group, firms complying with SOX404 did not decrease their level of investment or stock return volatility or increase their cash holdings in the post-SOX404 period. These results are inconsistent with SOX404 negatively affecting firms' corporate investment and risk-taking behavior.

Next, we investigate whether the compliance costs of SOX404 impact the sample firms' level of investment more strongly in 2004, as firms adjust to the new regulation. Panel B of Table 2 replicates the results from panel A using the year 2004 as the post-SOX404 period.[22] The results show that both the magnitude and statistical significance of the decrease in the level of *R&D* and *INVEST* are stronger in 2004 (e.g., the coefficient on *Post-SOX404* in the R&D regression in panel B is now −0.019 ($p < 0.01$), which is larger than the coefficient on the same variable from panel A of −0.010 ($p < 0.10$)). More importantly, the results still show that, compared with the control group, firms complying with SOX404 exhibit higher levels of R&D and total investment and lower levels of CASH in 2004.[23] These results are inconsistent with filer firms decreasing their investment level and risk-taking behavior because of SOX404 compliance, and consistent with filer firms investing more, possibly because of

Table 3. The Impact of SOX404 on a Filer's Corporate Investment

Panel A. Pre-SOX = 2002–2003, Post-SOX404 = 2004–2005

	(1) *CAPEX*	(2) *R&D*	(3) *INVEST*	(4) *CASH*	(5) *STD*
Post-SOX404	0.005	−0.010*	−0.005	−0.022*	−0.005***
	(1.62)	(−1.81)	(−0.73)	(−1.94)	(−5.21)
*Post-SOX404*Filer*	−0.001	0.013**	0.012*	−0.028**	−0.002
	(−0.37)	(2.24)	(1.70)	(−2.27)	(−1.63)
EBIT	0.033***	−0.115***	−0.086***	−0.152***	−0.010***
	(3.56)	(−7.11)	(−4.52)	(−4.51)	(−3.46)
*EBIT*Filer*	−0.021	0.077**	0.060	0.033	−0.021***
	(−1.11)	(2.27)	(1.52)	(0.47)	(−3.39)
MTB	0.001	0.013***	0.013***	0.007***	−0.001***
	(0.80)	(11.45)	(10.01)	(3.01)	(−3.36)
*MTB*Filer*	0.010***	−0.013***	−0.004	0.031***	0.001*
	(4.91)	(−3.97)	(−0.95)	(4.44)	(1.75)
DEBT					0.005
					(1.00)
*DEBT*Filer*					−0.016*
					(−1.95)
Constant	0.033***	0.075***	0.108***	0.270***	0.038***
	(14.65)	(18.96)	(23.15)	(32.57)	(38.85)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1,768	1,771	1,768	1,771	1,766
Adjusted R^2	0.05	0.18	0.14	0.10	0.31

Panel B. Pre-SOX404 = 2002–2003, Post-SOX404 = 2004

	(1) *CAPEX*	(2) *R&D*	(3) *INVEST*	(4) *CASH*	(5) *STD*
Post-SOX404	0.003	−0.019***	−0.016**	−0.013	−0.004***
	(0.81)	(−3.19)	(−2.35)	(−1.08)	(−3.62)
*Post-SOX404*Filer*	−0.000	0.023***	0.022***	−0.026*	−0.002
	(−0.10)	(3.16)	(2.64)	(−1.73)	(−1.46)
EBIT	0.014	−0.118***	−0.104***	−0.175***	−0.006
	(1.17)	(−5.51)	(−4.15)	(−3.95)	(−1.65)
*EBIT*Filer*	0.001	0.074	0.075	0.046	−0.012*
	(0.05)	(1.64)	(1.42)	(0.50)	(−1.68)
MTB	0.001	0.016***	0.017***	0.003	−0.000
	(0.94)	(11.43)	(10.16)	(0.92)	(−0.62)
*MTB*Filer*	0.007***	−0.020***	−0.013***	0.027***	0.000
	(3.32)	(−4.92)	(−2.66)	(3.16)	(0.62)
DEBT					−0.010*
					(−1.79)
*DEBT*Filer*					−0.004
					(−0.47)
Constant	0.036***	0.082***	0.118***	0.215***	0.048***
	(12.59)	(15.51)	(18.97)	(19.63)	(41.02)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1,315	1,316	1,315	1,316	1,314
Adjusted R^2	0.04	0.20	0.16	0.09	0.33

Notes. The sample includes 455 firms with public floats between \$50 million and \$150 million in 2002 and with data available from Compustat during the 2002–2005 period. The public float equals the value of the company owned by outside investors as reported in the 10-K. Filers are firms with a public float above \$75 million in 2002; control firms have a public float below \$75 million in 2002. Each column in this table reports the regressions of investment variables on a set of firm fixed effects, a set of year indicators (not reported), an indicator variable equal to one for the years 2004 through 2005 (*Post-SOX404*), and interactions between an indicator for *Post-SOX404* and filers (*Post-SOX404*Filer*). All variables are defined in the appendix. The control variables are winsorized at the 1% and 99% levels and are lagged one year in the regressions. Panel A shows the results of estimating Equation (1) using the sample period of 2002–2005. Panel B presents the results when the year 2004 is used for the post-SOX404 period. Panel C presents the results when the sample period includes the years 1994–2006, following Bargeron et al. (2010), and *Post-SOX404* is an indicator variable equal to one for the years of 2004 through 2006.

***, **, and * denote two-sided statistical significance at the 1%, 5%, and 10% levels, respectively.

more reliable financial statements, greater transparency, and better credit terms under SOX404.

4.3.2. Section 404 and Credit Terms from Bank Loan. We next investigate whether filer firms receive better credit terms because of the enhanced disclosure quality mandated by SOX404 (e.g., Ogneva et al. 2007, Ashbaugh-Skaife et al. 2009, Dhaliwal et al. 2011, Kim et al. 2011, Andrade et al. 2012). Better terms of credit, including lower interest rates on loans, would make it easier for firms, especially those facing financing constraints, to raise capital and render more positive net present value projects, leading to an increase in investment. We test this hypothesis by examining terms of bank loans extended to the sample firms. We focus on bank loans because small firms are more likely to obtain financing from banks rather than by issuing equity or bonds (e.g., Petersen and Rajan 1994, Bolton and Freixas 2000).[24]

Loan information comes from the Loan Pricing Corporation's Dealscan database, which provides the identification of which banks lend to which firms in a given year, and detailed information on the terms of the loans at origination, including the interest rate (loan pricing measured as a basis point spread over LIBOR, inclusive of all fees), whether the loan is secured, and the loan's maturity and size. Because interest rates are not always the best tool to control borrower risk (e.g., Stiglitz and Weiss 1981) while collateral requirements constrain firms' ability to raise capital and grow (e.g., Rampini and Viswanathan 2010), we follow Qian and Strahan (2007) and use both pricing and nonpricing terms as our main variables of interest. We thus test whether SOX404 had an impact on the loan characteristics of filer firms compared with the control group. The dependent variable *Spread* is calculated as the natural logarithm of the basis point spread of the loan interest rate over the LIBOR. We define secured lending (*Secure*) as an indicator variable that equals one if the loan is secured, and zero otherwise. *Maturity* is defined as the natural logarithm of the loan maturity measured in years, and *Loan Size* is defined as the natural logarithm of the loan size measured in millions of dollars. We include controls for loan type and loan purpose, using indicator variables, as these loan characteristics can affect the loan contract terms. In addition, we include specific borrower characteristics (*EBIT*, *MTB*, and *DEBT*) and further controls. Finally, we include firm (year) fixed effects to control for any time-invariant borrower characteristics (for any common economy-wide factors).

We test whether SOX404 has an effect on filer firms' credit terms. Because firms issue bank loans sporadically, and because only 40% of our sample firms have data on bank loans during the sample period, the number of observations is significantly smaller than for our other tests. The sparseness of the data decreases the power of our tests, particularly because we continue to incorporate both firm and year fixed effects in all models. Table 4 presents the results. Column (1) shows the results on the determinants of the loan spread. We find some evidence that following SOX404, the spread decreased for all firms (coefficient of −0.318, $p < 0.01$), but there is no difference in the interest rates on loans between the mandatory filers and the control group. Column (2) shows that after SOX404 took effect, the amount of collateral required for firms increased (the coefficient on *Post-SOX404* is 0.147, $p < 0.10$), but the collateral requirements for filer firms decreased compared with the control group (the coefficient on *Post-SOX404*Filer* is −0.178, $p < 0.05$). Column (3) shows no change in loan maturity in the post-SOX404 period for both filers and control firms (the coefficient on *Post-SOX404* is 0.166 but is statistically insignificant), and no change in loan maturity for filers relative to the control group. The results for loan size, presented in column (4), show that, relative to the control group, filer firms enjoy an increase in loan size in the post-SOX404 period (the coefficient on *Post-SOX404*Filer* is 0.370, $p < 0.05$).

In summary, we find evidence that following SOX404, loan size is significantly higher and the number of loans that have collateral is significantly lower for filer firms relative to the control group. This suggests that filer firms enjoy better loan terms, possibly allowing them to invest more in growth opportunities.

4.3.3. Cross-Sectional Analysis. In this section, we test whether the filer firms that are expected to benefit most from the increase in transparency and higher reporting quality of SOX404 invest more after the regulation. In particular, we test whether filer firms that are financially constrained and less exposed to litigation increase their investment level following SOX404.

Financially constrained firms are exposed to greater information asymmetry between insiders and outsiders (e.g., Fazzari et al. 1988). If the SOX404 requirement that companies disclose any internal controls weaknesses leads to increases in the credibility and quality of financial reports, this should strongly benefit firms with more information asymmetry (e.g., Arping and Sautner 2013).[25] Therefore, we expect financially constrained firms to face the largest reduction in information asymmetry, leading to lower financing costs and allowing them to invest more in growth opportunities.[26]

Bargeron et al. (2010) suggest that firms are reluctant to invest in risky projects following SOX404 because such investment can increase the probability that firms disclose material weaknesses in their internal financial controls, which can trigger litigation. Consistent with this, we expect firms that are less exposed to litigation concerns to be less affected by this concern—and to invest more than others.

Table 4. The Impact of SOX404 on Filers' Credit Terms

	(1) *Spread*	(2) *Secure*	(3) *Maturity*	(4) *Loan Size*
Post-SOX404	−0.318***	0.147*	0.166	0.125
	(−3.51)	(1.69)	(1.41)	(0.77)
*Post-SOX404*Filer*	0.139	−0.178**	−0.072	0.370**
	(1.50)	(−2.01)	(−0.59)	(2.24)
Loan Size	0.070**	0.102***	0.136***	
	(2.00)	(3.57)	(3.47)	
Line of Credit	−0.185***	0.089**	−0.168***	0.119*
	(−5.03)	(2.47)	(−3.45)	(1.76)
Secure	0.139**		0.139*	0.354***
	(2.44)		(1.89)	(3.57)
Purpose	−0.004*	0.011***	−0.004	−0.006
	(−1.75)	(5.52)	(−1.30)	(−1.52)
Maturity	−0.001	0.075*		0.252***
	(−0.02)	(1.89)		(3.47)
EBIT	0.651	−0.835	0.881	1.161
	(1.17)	(−1.50)	(1.16)	(1.12)
*EBIT*Filer*	−2.378***	1.556*	−1.177	0.458
	(−2.80)	(1.87)	(−1.03)	(0.29)
MTB	−0.041	−0.243**	0.071	0.253
	(−0.35)	(−2.16)	(0.46)	(1.20)
*MTB*Filer*	−0.105	0.293**	−0.093	−0.575**
	(−0.67)	(2.03)	(−0.47)	(−2.14)
DEBT	0.189	−0.302	0.204	−0.042
	(0.61)	(−1.03)	(0.51)	(−0.08)
*DEBT*Filer*	0.288	0.528	−0.963	0.560
	(0.55)	(1.04)	(−1.39)	(0.59)
Constant	5.399***	−0.023	0.694***	3.566***
	(29.05)	(−0.14)	(3.21)	(15.59)
Firm and year fixed effects	Yes	Yes	Yes	Yes
Observations	510	567	567	567
Adjusted R^2	0.26	0.18	0.17	0.17

Notes. *Pre-SOX404* = 2002–2003, *Post-SOX404* = 2004–2005. The sample includes firms with public floats between $50 million and $150 million in 2002 and with data available from Compustat during the 2002–2005 period. The public float equals the value of the company owned by outside investors as reported in the 10-K. Filers are firms with a public float above $75 million in 2002; control firms have a public float below $75 million in 2002. Each column in this table reports the regressions of the loan characteristic variables (the log of spread over LIBOR, whether the loan is secured, the log of loan maturity, and the log of loan size) on an indicator variable equal to one for the years 2004–2005 (*Post-SOX404*), an interaction between an indicator for *Post-SOX404* and filers (*Post-SOX404*Filer*), control variables, and a set of year indicators (not reported). All variables are defined in the appendix. The control variables are lagged one year in the regressions.

***, **, and * denote two-sided statistical significance at the 1%, 5%, and 10% levels, respectively.

To test these predictions, we estimate the following difference-in-difference model:

$$\begin{aligned} Y_{i,t} = {} & \alpha + \beta_0 * Post\text{-}SOX404_t \\ & + \beta_1 * Post\text{-}SOX404_t * Filer_i \\ & + \beta_2 * Post\text{-}SOX404_t * Filer_i * Variable\ of\ Interest \\ & + \sum_{m=2}^{n} \beta_m * Control_{i,t\text{-}1} \\ & + \sum_{m=1}^{n} \gamma_m * Control_{i,t\text{-}1} * Filer_i + \nu_t + \eta_i + \varepsilon_{i,t}, \end{aligned} \tag{3}$$

where *Variable of Interest* assumes the value of one if the firm is either financially constrained or in a low-litigation industry, and zero otherwise. We measure financially constrained firms using the Hadlock and Pierce (2010) financial constraint (or SA) index. The SA index is derived based on a firm's assets and age—two variables that can be regarded as exogenous in examining firms' investment. We define firms to be financially constrained if their SA index values are in the top one-third of the distribution of index values of all firms.[27] The SA index is calculated with data in fiscal year 2002 to avoid endogeneity concerns; it thus measures a firm's ex ante level of financial constraints. High-litigation industries include the biotechnology (SIC codes 2833–2836 and 8731–8734), computer (3570–3577 and 7370–7374), electronics (3600–3674), and retail (5200–5961) industries (see Francis et al. 1994a, b). All other industries are considered low-litigation industries. All remaining variables are defined in the appendix. In Equation 3, β_2 captures the change in investment following SOX404 for filer firms

with a high level of the variable of interest, compared with that of filer firms with low level of the variable of interest. We expect $\beta_2 > 0$ for firms with financial constraints and firms in low-litigation industries.

Table 5 presents the results. Panel A presents the results when the variable of interest is financial constraint. In columns (1)–(3), the coefficient on the triple interaction (*Post-SOX404*Filer*Variable of Interest*) is positive and statistically significant, whereas the coefficient on *Post-SOX404*Filer* is statistically insignificant. The sum of coefficients on *Post-SOX404*Filer* and *Post-SOX404*Filer*Variable of Interest* remains positive and statistically significant (untabulated). This finding suggests that filer firms with ex ante high levels of financial constraints increase their level of investment following SOX404, whereas non–financially constrained firms do not.[28]

Panel B presents the results for low-litigation industries. In column (1) (*CAPEX*), the coefficient on the triple interaction is positive (0.009) and statistically significant ($p < 10\%$), while the coefficient on *Post-SOX404*Filer* is not statistically significant. The sum of coefficients of *Post-SOX404*Filer* and the triple interaction remains positive and statistically significant (untabulated). This finding suggests that filer firms in low-litigation industries increase *CAPEX* following SOX404, whereas firms in high-litigation industries remain cautious and do not change their level of investment. We do not find differences between low- and high-litigation industries in the level of investment in *R&D* and *INVEST* following SOX404.

5. Additional Results and Sensitivity Analyses

5.1. The Impact of Section 404 on Filers vs. Control Firms Using a Regression Discontinuity Design

To assure that the assignment of firms into treatment and control groups is exogenous, we assign firms to the

Table 5. Cross-Sectional Partitions

Panel A. Cross-Sectional Test for Financial Constraint Firms

	(1) *CAPEX*	(2) *R&D*	(3) *INVEST*	(4) *CASH*	(5) *STD*
Post-SOX404	0.003	−0.015**	−0.012	0.006	−0.008***
	(0.86)	(−2.03)	(−1.57)	(0.57)	(−6.62)
*Post-SOX404*Filer*	−0.007	0.004	−0.003	−0.026	−0.001
	(−1.63)	(0.42)	(−0.28)	(−1.51)	(−0.97)
*Post-SOX404*Filer*Fin_Constraint*	0.014***	0.018**	0.032***	0.019	−0.000
	(3.00)	(2.42)	(3.71)	(1.31)	(−0.33)
Controls	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1,768	1,771	1,768	1,771	1,766
Adjusted R^2	0.05	0.18	0.14	0.10	0.31

Panel B. Cross-Sectional Test for High-Litigation-Risk Industries

	(1) *CAPEX*	(2) *R&D*	(3) *INVEST*	(4) *CASH*	(5) *STD*
Post-SOX404	0.003	−0.015**	−0.012	0.006	−0.008***
	(0.86)	(−2.03)	(−1.57)	(0.57)	(−6.62)
*Post-SOX404*Filer*	0.002	0.007	0.009	−0.018	−0.001
	(0.47)	(0.81)	(0.95)	(−1.26)	(−0.66)
*Post-SOX404*Filer*Low_Litigation*	0.009**	0.012	0.003	−0.002	−0.002
	(2.22)	(1.26)	(0.30)	(−0.11)	(−1.03)
Controls	Yes	Yes	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1,768	1,771	1,768	1,771	1,766
Adjusted R^2	0.05	0.18	0.14	0.10	0.31

Notes. The sample includes 455 firms with public floats between \$50 million and \$150 million in 2002 and with data available from Compustat during the 2002–2005 period. The public float equals the value of the company owned by outside investors as reported in the 10-K. Filers are firms with a public float above \$75 million in 2002; control firms have a public float below \$75 million in 2002. Each column in this table reports the regressions of investment variables on a set of firm fixed effects, a set of year indicators, control variables (not reported), an indicator variable equal to one for the years 2004–2005 (*Post-SOX404*), interactions between an indicator for *Post-SOX404* and filers (*Post-SOX404*Filer*), and triple interactions among *Post-SOX404*, filers, and variables of interest based on which we partition the sample. All variables are defined in the appendix. The control variables are winsorized at the 1% and 99% levels and are lagged one year in the regressions. Panel A shows the results of estimating Equation (3) when the variable of interest is financial constraint. Panel B presents the results for low litigation industries.

*** and ** denote two-sided statistical significance at the 1% and 5% levels, respectively.

treatment and control groups based on their public float in 2002. However, it is possible that some of the firms in the control group had to comply with SOX404 in 2004 because their float crossed the $75 million threshold in 2003 or 2004. To account for the potential misclassification of filers as nonfilers, we use a regression discontinuity design (RDD) (similar to Iliev 2010), which estimates the impact of treatment when the treatment is based on a well-defined threshold or the probability of receiving treatment. When self-selection is suspected, Angrist and Pischkle (2009) recommend the use of fuzzy RDD (FRDD), which assigns firms into treatment groups based on the probability of receiving treatment or the probability of being above the threshold. Thus, FRDD is used when one suspects that firms in the control group ultimately receive treatment (i.e., "cross-overs") (see Roberts and Whited 2013). One of the main advantages of the RDD is that even without having to assume random assignment of firms to the treatment and control groups around the cutoff discontinuity, the estimator has good interval validity (i.e., is robust to various functional forms and bandwidth choices) and finite sample properties (e.g., Angrist and Pischkle 2009, Wooldridge 2009, Roberts and Whited 2013).

This approach is similar to a two-stage least square instrumental variables approach under certain assumptions (e.g., control variables need to be included in the first stage regression) (see Imbens and Lemieux 2008, p. 627, or Angrist and Pischkle 2009, p. 259). In the first stage, we estimate whether a firm will become a filer in 2004 (or receive treatment). As in Iliev (2010), we use the exogenous variable *Float*—the firm's public float in 2002—as an instrument to estimate whether the firm will become a filer. Once again, firms did not know that their public float in 2002 would determine their filer status (hence, the public float is exogenous), and the $75 million threshold indicates that float is a strong predictor of the future filer status. In the second stage, we use the estimated filer status obtained from the first stage to examine the difference in corporate investment and risk-taking activities between filers and nonfilers. Specifically, we estimate the following two-stage least squares model:

$$Filer_{i,2004} = \alpha + \beta_0 * Float_{i,2002} + \sum_{m=1}^{n} \beta_m * Control_{i,2003} + \eta_i + u_i, \quad (4)$$

$$Y_{i,2004} = \alpha + \beta_0 * Predicted\ Filer_{i,2004} + \sum_{m=1}^{n} \beta_m * Control_{i,2003} + \eta_i + \varepsilon_i. \quad (5)$$

Table 6 presents the results of this model. Panel A presents the results that estimate whether the variables of interest in 2004 differ between filers and the control group. Panel B presents the results for 2005. Becoming a filer in 2004 is highly correlated with the size of the firm's public float in 2002. The first-stage regressions in column (1) shows that the coefficient on float in 2002 is 0.089 ($p < 0.01$). The second-stage regression results presented in panel A show that the estimated filers invest *more* in 2004, the first year of SOX implementation, than the control firms do. Columns (5) and (6) show that the estimated filers tend to hold more cash and have lower volatility of returns, but the difference is not statistically significant at the common levels. In conclusion, using an alternative, quasi-experimental technique, the results are inconsistent with firms avoiding risky investments because of SOX404; this reinforces the differences-in-differences results. The findings in panel B that replicate the second-stage results using *CAPEX*, *R&D*, *INVEST*, *CASH*, and *STD* in 2005 are qualitatively similar.

5.2. Disentangling Results Driven by Control vs. Treatment Groups

The key assumption in a difference-in-difference research design is that the control firms' outcome is the correct counterfactual for the treatment firms' expected outcome. In our case, the investment by the control firms is taken as the expected investment by the treatment firms. The assumption is that the control groups' outcome is not affected by the regulation, so the regulation's impact can be assessed by comparing the change in the treatment firms' outcome to the change in the control firms' outcome. However, when analyzing whether firms take actions to avoid complying or delaying the compliance with SOX, Gao et al. (2009) focus on annual net investment growth measured over the period of June 2003 and December of 2005 and find that the growth in net investment for firms with a public float of less than $75 million (the non-accelerated filers) is lower than for a group of firms with a public float higher than $75 million (the accelerated filers);[29] they interpret this as evidence that firms decrease investment to avoid having to comply with SOX. Thus, a potential concern for our study is that a relative *increase* in the level of investment for the treatment firms versus the control firms following SOX404 could reflect a *decrease* in investment levels by control firms that are seeking to avoid compliance—as opposed to SOX-related benefits, such as a better credit terms, accruing to the treatment firms. Similarly, results showing no difference in investment between the filer and the control firms may reflect the net effect of filer firms decreasing investment because of SOX404 and control firms decreasing investment to avoid complying with SOX404.

To explore whether and how SOX404 impacted the control group, we inspect the data along several dimensions. First, as mentioned before, the univariate analyses in Figures 7–9 suggest that, although the average level of *R&D* (*CAPEX*) decreased for the control group during the years 2003 to 2004 (2003), which may

Table 6. The Impact of SOX on a Filers' Corporate Investment Using RD Regressions

Panel A. Regression Results for 2004

	(1) *Filer_04*	(2) *CAPEX_04*	(3) *R&D_04*	(4) *INVEST_04*	(5) *CASH_04*	(6) *STD_04*
Float 2002	0.089***					
	(11.30)					
Predicted FILER_04		0.029**	0.030	0.077**	0.101	−0.006
		(2.01)	(1.02)	(2.12)	(1.09)	(−1.46)
EBIT_03	1.068***	0.015**	−0.144***	−0.149***	−0.212***	−0.015***
	(3.76)	(2.30)	(−10.96)	(−9.18)	(−5.09)	(−8.29)
MTB_03	0.201***	0.004***	0.003***	0.007***	0.017***	0.000
	(5.15)	(6.23)	(2.63)	(4.52)	(3.94)	(0.72)
DEBT_03						0.001
						(0.26)
Constant	−5.217***	0.010	−0.025	−0.026	0.176*	0.033***
	(−8.19)	(0.62)	(−0.80)	(−0.69)	(1.78)	(7.48)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	432	432	432	432	432	432
Adjusted R^2	0.52	0.35	0.54	0.44	0.37	0.32

Panel B. Regression Results for 2005

	(1) *Filer_04*	(2) *CAPEX_05*	(3) *R&D_05*	(4) *INVEST_05*	(5) *CASH_05*	(6) *STD_05*
Float 2002	0.089***					
	(11.55)					
Predicted FILER_04		0.029**	0.048*	0.091**	0.010	0.003
		(2.06)	(1.68)	(2.43)	(0.10)	(0.59)
EBIT_04	2.111***	0.016**	−0.153***	−0.165***	−0.093**	−0.019***
	(6.40)	(2.37)	(−10.72)	(−9.31)	(−2.01)	(−8.90)
MTB_04	0.319***	0.004***	0.002	0.004**	0.013***	−0.000**
	(5.54)	(5.44)	(1.24)	(2.56)	(2.87)	(−1.97)
DEBT_04						0.002
						(0.80)
Constant	−5.305***	0.014	−0.018	−0.007	0.252**	0.029***
	(−8.17)	(0.95)	(−0.60)	(−0.17)	(2.52)	(6.18)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	432	432	432	432	432	432
Adjusted R^2	0.55	0.36	0.54	0.43	0.28	0.26

Notes. This table presents the results of estimating *CAPEX*, *R&D*, *CASH*, *INVEST*, and *STD* using RD regressions implemented using the equivalent two-stage least square instrumental variables approach when all the control variables from the second-stage are included in the first stage regression (see Imbens and Lemieux 2008). Panel A (B) presents the results estimating the variables of interest in 2004 (2005). The sample includes all Compustat firms with public floats between $50 million and $150 million in 2002 and with data available for the 2002–2005 period. The public float equals the value of the company owned by outside investors as reported in the 10-K. We use the firm's public float in 2002 as an instrument to estimate whether a firm will become a filer in 2004. Columns (1)–(5) present the results for the second-stage OLS regression, which uses the estimated *Filer_04* from the first stage as one of the independent variables. Each column in this table reports regression results of investment and risk-taking variables on a set of industry fixed effects based on two-digit SIC codes. All variables are defined in the appendix. The control variables are winsorized at the 1% and 99% levels.

***, **, and * denote two-sided statistical significance at the 1%, 5%, and 10% levels, respectively.

explain an increase in investment for the filer firms relative to the control group during those years, we do *not* observe a decrease in the average level of investment for the filer firms or the control firms following SOX404. This is inconsistent with SOX404 having a negative impact on corporate investment. Second, we separate the control firms into two subgroups: (i) firms whose public float remains below the $75 million threshold (what we call the nonfilers) during the 2002–2004 period, and (ii) firms whose public float crosses the threshold in 2003 or 2004, which makes them filer firms as of 2004. We call the second group of firms "voluntary filers" because they knew the threshold value and could have manipulated their public float to delay SOX404 compliance, but chose otherwise. Given that firms had the option of delaying compliance by staying small until 2004, we would expect only a small percentage of firms to cross the $75 million threshold before then *if* the benefits of compliance with SOX404 are lower than the costs. However, we find that about 75% of the control firms in our sample become

voluntary filers. Hayes (2009) also points out that a high percentage (59.6%) of firms with public float below $75 million in the pre-SOX control period in Gao et al. (2009) become accelerated filers. This outcome is consistent with these firms perceiving that the benefits of SOX404 compliance exceed the costs. As previously mentioned, one such benefits of SOX404 compliance is better credit terms, which can be critical for financially constrained firms with high growth opportunities.

Third, we analyze whether the investment levels across nonfilers and voluntary filers over the sample period are associated with their growth opportunities. Panel C of Table 7 shows that, following SOX404, *voluntary filers* exhibit statistically significantly higher levels of capital expenditures, investment growth, and asset growth, as well as higher MTB, compared with the nonfiler group. The higher growth exhibited by the voluntary filers is consistent with these firms having relatively better growth opportunities than nonfilers and being able to extract net benefits from SOX404 compliance. Therefore, the inclusion of these firms in the control group actually biases our tests against finding evidence that SOX404 had a positive impact on filers' level of investment activities. Overall, the evidence above is inconsistent with SOX404 having a negative impact on investment.

5.3. Vega and Delta

Cohen et al. (2013) examine the effects of SOX on compensation-based incentives to take risky investments and argue that CEO pay-for-performance sensitivity decreased after SOX, leading firms to reduce their investment and risk-taking activities. We examine whether managerial incentives to engage in risky investments also changed following SOX404 for our sample firms. If indeed SOX404 led boards to increase (decrease) managerial incentives to engage in risk-taking behavior, then that would be consistent with managerial compensation being a mechanism through which SOX404 could affect investment and risk-taking behavior. Specifically, we analyze whether filer firms changed CEOs' incentives to engage in risk-taking behavior differently from the control firms in the post-SOX404 period compared with the pre-SOX404 period. We measure CEOs' incentives to engage in risk-taking behavior by pay convexity (vega) and sensitivity of pay to firm value (delta). Vega is the dollar change in CEOs' wealth tied to the firm for a 1% change in the standard deviation of stock returns. Delta is the dollar change in CEOs' wealth for a 1% change in stock price.

In untabulated analysis, we find that, compared with the pre-SOX404 period, vega increases for filer firms following SOX404, whereas the change in vega for the

Table 7. Voluntary Filers vs. Nonfilers

Fiscal Year	*CAPEX*	*R&D*	*INVEST*	*INVEST Growth*	*MTB*	*Asset Growth*	*Stock Returns*
Panel A. Summary Statistics for the Voluntary Filers (N = 121)							
2002	0.043	0.143	0.186	−0.052	1.934	−0.072	−0.080
2003	0.041	0.117	0.158	−0.075	2.867	0.113	1.512
2004	0.045	0.109	0.154	0.058	2.954	0.122	0.401
2005	0.051	0.117	0.168	0.108	2.637	0.090	0.173
2005–2002	0.008	−0.026	−0.018	0.160***	0.704**	0.162***	0.253***
Panel B. Summary Statistics for the Nonfilers (N = 38)							
2002	0.055	0.092	0.147	−0.011	1.666	−0.091	−0.242
2003	0.043	0.086	0.129	−0.101	1.782	−0.019	0.437
2004	0.044	0.077	0.121	−0.008	1.689	−0.020	0.064
2005	0.040	0.095	0.127	0.006	1.706	−0.127	0.040
2005–2002	−0.015**	0.003	−0.020	0.017	0.040	−0.036	0.282**
Panel C. Summary Statistics for the Difference Between the Voluntary Filers and the Nonfilers (N = 38)							
2002	−0.013*	0.051	0.039	−0.041	0.268	0.018	0.161*
2003	−0.003	0.031	0.029	0.026***	1.086**	0.132***	1.076***
2004	0.001	0.032	0.033	0.067	1.265***	0.142***	0.337***
2005	0.011	0.021	0.040	0.102	0.931*	0.216***	0.132
2005–2002	0.024***	−0.030	0.002	0.143**	0.664***	0.198***	−0.029

Notes. The sample includes firms with public floats between $50 million and $150 million in 2002 and with data available from Compustat during the 2002–2005 period. The public float equals the value of the company owned by outside investors as reported in the 10-K. Voluntary filers are firms with a public float below $75 million in 2002, but with a public float above $75 million in 2003 and 2004. Nonfilers have a public float below $75 million in 2002, 2003, and 2004. This table provides descriptive statistics of the mean values of the firm characteristics for the voluntary filers and nonfilers for each of the sample years. The last row provides the difference in mean values (along with the statistical significance) between the years 2002 and 2005. The variable *INVEST_Growth* is defined as the percentage annual growth in *INVEST*. *N* represents the number of firms. All remaining variables are defined in the appendix. The control variables are winsorized at the 1% and 99% levels and are lagged one year.

***, **, and * denote two-sided statistical significance at the 1%, 5%, and 10% levels, respectively.

control group is not statistically significant. The difference in vega is statistically different between filer and control firms. We also find that delta increases following SOX404 for both filer and control firms, and that the difference in delta is not statistically different between filer and control firms. These results suggest that managerial compensation following SOX404 is not consistent with boards decreasing managerial risk-taking incentives to reduce investment and risk-taking behavior.[30] All of the results are available in the online appendix.

5.4. Other Robustness Tests

To analyze whether the estimated effect of SOX404 on the variables of interest is not spurious or caused by confounding events, we follow the suggestion of Roberts and Whited (2013) and perform a falsification test using a different event period. We thus repeat the tests presented in Table 3 assuming that the impact of SOX404 occurred two years before it actually did. If the impact on corporate investment for the filer firms is indeed because of SOX404, as opposed to some alternative force, then we should not observe a difference in corporate investment between the filer firms and the control group during the falsification test period. Assuming that the pre-event period is 2000–2001, and the postevent period is 2002–2003, we find (untabulated) that the investment behavior of filer firms is not statistically distinguishable from that of the control group. This result provides further evidence that the observed change in the corporate investment of filer firms is more likely because of SOX404 compliance than to some other alternative force.

Although the focus of our study is on the impact of Section 404 on corporate investment and risk-taking activities, one could argue that other corporate governance provisions that were adopted around the time of the SOX enactment could have impacted firms' behavior. Both Bargeron et al. (2010) and Kang et al. (2010) show that the post-SOX decline in investment levels for U.S. firms, (compared with non-U.S. firms) is related to corporate governance characteristics prior to SOX. Although SOX requires the board to have an independent auditing committee, the New York Stock Exchange (NYSE) and NASDAQ revised their exchange listing requirements, around the time that SOX was enacted, to increase the required percentage of independent directors on corporate boards and board committees, namely auditing, nominating, and compensation. It is important to recognize that these board independence requirements were enforced for all firms (filer and control alike) and thus are not expected to affect the investment behavior of filer firms, relative to the control group.

Nevertheless, to account for the effects of changes in board structure and CEO compensation contracts on a firm's corporate investment, we rerun the tests in Table 3, adding the following controls: the percentage of insiders on the board of directors and the percentage of equity in the CEO's total pay. To capture the level of monitoring of the CEO's actions by the board, we include board size as an additional control variable. [Our rationale for this is the suggestion, by some studies, that larger boards can provide more effective monitoring when the CEO's opportunity to consume private benefits is high (e.g., Boone et al. 2007, Harris and Raviv 2008).] We hand collect these data for the years 2001 and 2006 and use the values of 2001 (2006) for all the pre-SOX404 (post-SOX404) values. We estimate an expanded version of Equation (1) that includes the three control variables mentioned above and their interactions with the filer indicator variable to capture whether these firm characteristics affect corporate investment and risk-taking behavior differently in filer firms. After we control for these other concurrent corporate governance mechanisms, the (untabulated) results show no evidence that filer firms decreased their level of investment and risk-taking activities more following SOX404 than the control group did.

The sample in this study covers firms with a float between \$50 million and \$150 million. In choosing this range, we sought to generate a sample that was large enough for statistical purposes yet consisting of similar firms. However, one can question whether our results are because of a lack of power to detect statistically significant differences between the filers and the control group because of the lower proportion of control firms in the main sample. To address this concern, we redefine the sample so that it is composed of firms with a public float between \$50 million and \$100 million. In (untabulated) results, we find that the filer firms continue to invest more (*R&D* and *INVEST*) than the control group does in 2004; although the coefficient on CASH is still negative, it is no longer statistically significant at the common significance levels.

6. Conclusion

The impact of the Sarbanes–Oxley Act, which became effective in 2002, on corporate risk taking and investment has attracted significant attention from regulators, practitioners, and academics. Prior studies argue that managers are less willing to take risks because of heightened litigation and compliance costs associated with SOX. This argument, if validated by empirical evidence, would lead to the conclusion that SOX poses a severe obstruction for firms, as investment is key for growth. Bargeron et al. (2010) and Kang et al. (2010), among others, use non-U.S. firms as a benchmark and find that large U.S. firms decreased investment more after SOX; these authors attribute the decrease in investment in the United States to SOX. We show that the documented decrease in investment and other risk-taking behavior in these studies cannot be solely attributed to SOX as it is also related to other concurrent events in the United States.

Our results also indicate that better identification strategies are needed, to isolate the effects of SOX from the many contemporaneous events of that period. We use a quasi-natural experiment to identify a control group for isolating the effects of Section 404 on U.S. firms. Specifically, we compare SOX404's impact on a sample of small firms with a public float just above the $75 million threshold (filer firms) with its impact on firms with a public float just below that threshold (nonfiler firms). Filers are required to comply with Section 404, but nonfilers are not, leading to natural treatment (filers) and control (nonfiler) groupings. With the two groups of firms and both a difference-in-difference and a regression-discontinuity methodology, we do not find that filers invest less than control firms in the post-SOX404 period (2004–2005), relative to the pre-SOX404 period (2002–2003). In fact, we find that filers increase total investment—the sum of capital expenditure and R&D—more during the post-SOX period than the control group does. We also find evidence that filer firms receive better credit terms than the control group in the period following SOX404. In cross-sectional tests, we find that firms likely to benefit more from the regulation—financially constrained firms and firms facing less litigation risk—are the ones that invest the most following SOX404.

In summary, our results reject the hypothesis that the high compliance cost and regulatory burden of SOX404 leads to a reduction in corporate investment and other risk-taking activities. Our use of small firms is particularly relevant for the debate on SOX, as these firms' growth levels depend on their ability to invest. Presumably, they would be more adversely affected than large firms by the costly regulation associated with SOX. Hence, our results also cast doubt on the notion that such regulation has had a negative impact on larger firms.

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Appendix. Variable Definitions

Float	Public float reported on the company's annual report
CAPEX	Capital expenditures divided by average assets for the last two years
R&D	Research and development expenditures for the year divided by average assets for the last two years. Missing values for R&D are set to zero.
INVEST	The sum of *CAPEX* and *R&D*
CASH	The year-end level of cash and short-term investments divided by average assets for the last two years
STD	The standard deviation of the returns for the year
EBIT	The earnings before interest and taxes divided by average assets
MTB	The year-end market value of assets divided by the year-end book value of assets
DEBT	The average total debt divided by the average market value of total assets
ASSETS	The year-end book value of assets
MKT CAP	Fiscal year-end stock price multiplied by the number of shares outstanding
REVENUES	Total sales for the year
STKRET	Buy-and-hold stock return for the year
Pre-SOX404	An indicator variable equal to one for the years 2002 and 2003, zero otherwise
Post-SOX404	An indicator variable equal to one for the years 2004 and 2005, zero otherwise
Large	An indicator variable equal to one if a firm has market cap greater than $150 million at the end of 2002, zero otherwise
ASSET Growth	The percentage annual growth in book value of assets
INVEST Growth	The percentage annual growth in total INVEST
Loan Size	Log of loan size (in millions of dollars)
Line of Credit	An indicator variable equal to one if the type of loan is a line of credit, zero if it is a term loan
Secure	An indicator variable equal to one if the loan is secured with collateral
Purpose	An ordinal variable indicating the purpose of the loan (e.g., debt repayment, commercial paper backup line of credit, general corporate purpose, etc.)
Maturity	Natural logarithm of the loan maturity (in years)
Spread	Natural logarithm of the basis point spread of the loan interest rate over LIBOR
Fin_Constraint	An indicator variable equal to one if a firm's financial constraint (or SA) index is in the top tercile of the distribution among all firms, zero otherwise—following Hadlock and Pierce (2010), financial constraint (or SA) index equals -0.737*(total assets) + 0.043*(total assets)^2 – 0.040**Age*, where *Age* equals the current year minus the first year that the firm was listed
Low_Litigation	An indicator variable equal to one if a firm does not belong to high litigation industries which include: biotechnology (SIC codes 2833–2836 and 8731–8734), computer (3570–3577 and 7370–7374), electronics (3600–3674), and retail (5200–5961) industries (see Francis et al. 1994a, b)
Vega	Dollar change in CEO's wealth tied to the firm for a one percent change in the standard deviation of stock returns
Delta	Dollar change in CEO's wealth for a one percent change in stock price

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Endnotes

[1] The Securities Exchange Commission's (SEC's) final rule on the management report on internal control over financial reporting and the certification of disclosures following Section 404 can be found at http://www.sec.gov/rules/final/33-8238.htm#iiid. Section 302, which became effective before SOX404, states that the CEO and CFO are directly responsible for the accuracy of financial statements and for establishing and maintaining internal controls. However, Section 404 mandates that firms include their management report on internal controls over financial reporting in their 10-K filings, that outside auditors file a report on management's assessment of the company's internal controls, and it lead to further amendments to Section 302.

[2] For example, a year after enactment, William Donaldson, former chairman of the SEC, stated that SOX would lead to a "loss of risk-taking zeal" because of a "huge preoccupation with the dangers and risks of making the slightest mistake." See Testimony Concerning the Impact of the Sarbanes–Oxley Act by the U.S. SEC Chairman William Donaldson before the House Committee on Financial Services (April 21, 2005): https://www.sec.gov/news/testimony/ts042105whd.htm. As another example, the American Enterprise Institute for Public Policy Research sponsored a conference entitled "Is Sarbanes-Oxley Impairing Corporate Risk-Taking?"

[3] The Long-Term Capital Management crisis occurred in 1998–1999, and the U.S. Fed started raising interest rates in June 1999.

[4] Securities and Exchange Act Rule 12b-2 defines "public float" as the aggregate market value of the issuer's outstanding voting and nonvoting common equity held by nonaffiliates. An "affiliate" is a person who, either directly or indirectly through one or more intermediaries, controls, is controlled by, or is under common control with the person specified. The term "control" means the direct or indirect possession of the power to direct or cause the direction of the management and policies of a person, whether through the ownership of voting securities, by contract, or otherwise.

[5] Iliev (2010) finds evidence that after 2003—and especially in 2004—firms manipulated their public float to avoid complying with SOX, but no evidence consistent with manipulation of public float is found in 2002.

[6] Bargeron et al. (2010) mention two other measures of SOX that could have led to a decrease in investment: the increased role of independent directors and the increased liabilities and penalties faced by officers and directors. However, the former had already been imposed by the NYSE and NASDAQ corporate rules and became effective for fiscal year 2003, before SOX. In the robustness section, we control for the percentage of independent directors on a firm's board of directors and find similar results. We thus focus on SOX's most demanding requirement, Section 404.

[7] Following Bargeron et al. (2010), we include (i) the percentage change in GDP growth for U.S. firms, UK firms, and Canadian firms separately; and (ii) the one-year return on the S&P500 Index for U.S. firms, the FTSE100 Index for UK firms, and the TSX Composite Index for Canadian firms.

[8] Bargeron et al. (2010) get data from the Thomson One Banker Database. However, none of our schools have access to that database.

[9] Our results are similar if we follow the Compustat definition of fiscal year as June to May (i.e., the 2003 fiscal year begins June 1, 2003, and ends May 31, 2004).

[10] The coefficients on *US Post-SOX* in the *CAPEX*, *R&D*, *INVEST*, *CASH*, and *STD* regressions in table 2 of Bargeron et al. (2010) are 0.0171, −0.0028, −0.0199, 0.0247, and –0.008, respectively, all statistically significant at the 0.01 level.

[11] A company with accelerated filer status can become a nonaccelerated filer only if its revenues and public float are smaller than $25 million for two consecutive years (SEC release 33-8182). Firms that do not have public equity but have public debt are, by definition, nonaccelerated filers, as they do not meet the criteria for an accelerated filer.

[12] Dodd–Frank permanently excluded nonaccelerated filers from SOX 404(b)—the auditor's certification of the management report.

[13] In a robustness test presented below, we define the post-SOX404 period to include the fiscal years 2004–2006 and obtain similar results.

[14] Because firms had to comply with SOX404 only after November 15, 2004, we perform a robust check where we assign a fiscal year of 2003 to all firms that had fiscal year end between June 1, 2004, and November 15, 2004. Only 70 observations are affected by this change. Our results are qualitatively the same.

[15] Our sample size is similar to that of other studies. Iliev (2010) uses a quasi-experiment to analyze the effect of SOX404 on the market value of small firms using a sample of 301 companies. Cortes (2013) also uses a quasi-experiment to analyze the role of SOX404 on the impact of informational frictions in financial markets on asset liquidity using a sample of 397 firms. In the robustness section, we replicate our tests using a sample with a public float between $50 million and $100 million centered on the $75 million threshold value.

[16] The results are qualitatively similar if we do not require a constant sample of firms and allow firms to delist during the sample period. In our sample, 100 firms delist, 55 (45) of which are filer (control) firms.

[17] Consistent with prior research, we set missing values of R&D to zero (e.g., Bargeron et al. 2010). The justification for this approach is that both the SEC and the Financial Accounting Standards Board require publicly traded firms to report "material" R&D expenditures in the year the expenditure occurred. Chauvin and Hirschey (1993) test whether missing values of R&D correspond to a value of zero, and confirm the validity of this assumption.

[18] Following SOX404 implementation, the amount of controls and processes in place to (a) identify major capital requirements needed, (b) evaluate different possible investments, (c) monitor projects and actual costs incurred, and (d) report on capital expenditures occurred, as examples, increased substantially.

[19] We also have considered other measures of investment. When investment is defined as (i) $CAPEX + ACQUISITION - Sale\ of\ PPE$ or (ii) $CAPEX + R\&D - Sale\ of\ PPE$, our main results do not change. We also use M&A acquisitions as another measure of investment level. In (untabulated) results, we find that filers in the post-SOX404 period do not exhibit significantly lower levels of acquisition than control firms. This outcome can be because of the fact that our sample firms are more likely to be targets than acquirers and do not engage in many acquisitions. In fact, only 30% of our sample firms engage in acquisitions during the sample period.

[20] The results are qualitatively similar if we exclude the year fixed effects.

[21] Bargeron et al. (2010) also include GDP growth and an index return to control for the impact of the UK and U.S. economies' growth on the variables of interest. We do not include these variables because our sample consists only of U.S. firms; GDP growth and the index return are subsumed by the inclusion of the year fixed effects. However, the inclusion of the S&P500 index and GDP growth yields qualitatively similar results.

[22] We further extend the post-SOX404 period to include the years 2004–2006 to account for the fact that firms' investment decisions,

especially in capital expenditures, may require some planning ahead of time making it hard for firms to make large changes in their investment plans in the short term (e.g., building a new factory requires that firms decide on location, size of the factory, hiring employees, and which technology to use, as examples). The untabulated results remain qualitatively similar to those in panel A, except that the level of CAPEX increases following SOX404 for both filer and nonfiler firms (coefficient on *Post-SOX404* is 0.007, with $p < 0.1$).

[23] In a concurrent study of the impact of SOX404 on capital market informational frictions, Cortes (2013) also finds that the levels of R&D (cash holdings) are higher (lower) for firms complying with SOX404 in the post-SOX404 period of 2005–2010.

[24] We also analyze the changes in stock market liquidity following SOX404 for filer firms versus the control group. The advantage of focusing on the impact of SOX404 on market liquidity rather than on the cost of equity is that market liquidity is less anticipatory in nature (see Christensen et al. 2013). We use three widely used liquidity measures: (i) the log of the number of shares traded in the market over the year (*Trade Volume*), (ii) the log of the dollar value of the shares traded (*$Trade Volume*) (Brennan et al. 1998, Amihud et al. 2005), and (iii) the relative number of trading days with zero daily returns (*Zero Returns*) to all potential trading days per year (Lesmond et al. 1999). Untabulated results suggest that the increase disclosure and reporting quality associated with SOX404 leads to an increase in market liquidity for the filer firms compared with the control group following SOX404.

[25] Some studies argue that SOX404 increased the accuracy of financial reports. Alexander et al. (2013, table 6) report a positive market reaction around the events related to SOX404 implementation, suggesting that investors' ex ante perception of SOX404 was, on average, beneficial. Ashbaugh-Skaife et al. (2009) provide evidence that SOX404 reduces the information asymmetry between a firm and its investors (see also Ogneva et al. 2007, Kim et al. 2011, Andrade et al. 2012). Several papers show that Section 404 contributed to an increase in the accuracy of financial reports through increased firm-level earnings quality (see Iliev 2010, Singer and You 2011) and higher accuracy of analysts' earnings forecasts (see Arping and Sautner 2013). Because small firms are subject to higher degrees of information asymmetry and have lower reporting quality than larger firms, they can potentially benefit the most from SOX404's implementation (Kamar et al. 2007). The availability of financial information for small firms is also lower, as these firms use significantly less external financing (Beck et al. 2008), are less likely to have management accounting systems that are separate from financial accounting (e.g., Drury and Tayles 1995), and are more opaque than larger firms (Yu 2012). Thus, the role of SOX404 in enhancing the quality of financial reporting can be particularly beneficial to these firms.

[26] Financially constrained firms would not necessarily disclose more information before SOX404, because theories, such as Verrecchia (1983), show that voluntary disclosure is costly (e.g., competitors may gain an advantage after learning proprietary information). However, once a new regulation such as SOX404, which increases the threshold for information disclosure, is introduced, a new equilibrium level of enhanced disclosure, in the framework of Verrecchia (1983), can be reached.

[27] Results are similar if we define financially constrained firms as those with an SA index above or below the median across all firms.

[28] We also test whether filer firms that are more financially constrained benefit from better credit terms following SOX404. We replicate the tests on the terms of bank loans presented in Table 4 for a subsample of firms that are financially constrained (i.e., SA index value is in the top one-third of the sample firms) and a subsample that are less financially constrained (all remaining firms). In untabulated results, we find that collateral requirements fell significantly more, and that loan size increases significantly more, for financially constrained firms following SOX404, which makes it easier for these small firms to borrow from banks. We do not find interest rates on loans to be significantly different following SOX404 for each group.

[29] Gao et al. (2009) define the growth in net investment measure as the one-year change in investment scaled by lagged total assets, where investment is defined as the sum of capital expenditures (excluding acquisitions expenses), research and development, and advertising expenses, minus the sale of property, plant, and equipment.

[30] These results are not inconsistent with the results of Cohen et al. (2013), which focus on a sample of large firms for the sample period 1992–2006. To compare our results to those in Cohen et al. (2013), we focus on the same period of 2002–2005. In untabulated results, we find that (i) the average values of delta and vega are much larger for the Cohen et al. (2013) sample than for our sample firms, as expected; and (ii) the average values of vega and delta increase following SOX404 (2004–2005) for *both* our sample firms and the Cohen et al. (2013) sample. Moreover, the increase in vega and delta are statistically significantly different from zero for both our sample firms and the Cohen et al. (2013) sample. Therefore, even though the two samples differ greatly in size, the change in the compensation characteristics vega and delta around SOX404 seem to be similar.

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