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Are financially constrained firms susceptible to a stock price crash?

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ABSTRACT

This study investigates whether and how financial constraints on firms affect the risk of their stock price crashing. We find strong evidence that financial constraints increase future stock price crash risk. This finding is robust to using two quasi-natural experiments to control for potential endogeneity. We also provide evidence to suggest that bad news hoarding and default risk explain the crash risk of financially constrained firms. Cross-sectional analysis reveals that the positive relation between financial constraints and future crash risk is more prominent for firms with weak corporate governance. Our study is of interest to investors as well as other stakeholders concerned about firms' creditworthiness and viability.

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Financial constraints; crash risk; bad news hoarding; default risk; corporate governance

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1. Introduction

The objective of our study is to examine whether and how firm financial constraints affect future stock price crash risk. Financial constraint is defined as frictions that prevent a firm from funding its desired investments (e.g. Lamont, Polk, and Saá-Requejo 2001). Previous studies (e.g. Fazzari et al. 1988; Lamont, Polk, and Saá-Requejo 2001; Livdan, Sapriza, and Zhang 2009; Denis and Sibilkov 2010) have examined the association of financial constraints with capital investments, firm value, risks, and expected returns, but none has evaluated the stock price crash risk of financially constrained firms. We seek to fill this gap in the literature. Given that stock price crashes have material impacts on investor welfare, our study on financially constrained firms' crash risk should be of interest to investors making portfolio investment decisions, and relevant to creditors, suppliers, customers, and other stakeholders, who are concerned about corporate creditworthiness and viability.

We posit that bad news hoarding and default risk are two potential mechanisms that make financially constrained firms susceptible to stock price crashes.[1] First, the literature (e.g. Chen, Hong, and Stein 2001; Jin and Myers 2006; Hutton, Marcus, and Tehranian 2009; Kim, Li, and Zhang 2011a, 2011b; Andreou, Louca, and Petrou 2017) regards bad-news hoarding as a fundamental cause of stock price crashes. Compared with financially healthy firms, financially constrained firms are likely to have more bad news on their investments and operations due to cash inadequacy. Because bad news might increase the costs of issuing equity and debt, managers in financially troubled firms are particularly prone to hide bad news for an extended period to secure external funds. However, though the amount of bad news that managers are able to hide is limited (Jin and Myers 2006), managers often cannot *anticipate* and thus *control* when such a limit is reached (He 2015), given constant and unforeseeable changes in the business environments. Once that limit is reached, all the bad news will become uncontainable, resulting in a sudden, dramatic price drop, that is, a stock price crash. In essence, with strong incentives to secure external finance, firms in financial constraints are more likely to withhold bad news and thus have higher future crash risk, compared with unconstrained firms.

Second, financially constrained firms need more cash to fund necessary investments and avoid default. Because external financing is often too expensive for such firms, they have to rely on limited internal funds

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and hence are more susceptible to default and a stock price crash resulting from corporate failure. Therefore, it follows that financially constrained firms have a high risk of stock price crashes. Furthermore, firms facing financial constraints have an incentive to forego positive net-present-value projects; such underinvestment and debt overhang problem would further exacerbate their potential default risk and associated crash risk. The default-risk mechanism is related to the foregoing bad-news-hoarding mechanism in the sense that default risk pertains to a type of bad news for a firm. Nonetheless, corporate default is generally perceived as severe bad news that is difficult to withhold by firms. Therefore, given that financially constrained firms are inherently probable to be subject to default risk, we propose it as a separate mechanism that explains stock price crash risk of the firms.

A counter argument plausibly holds when we take into consideration the investors' abilities to decipher the implications of financial constraints for future crash risk. If investors can decipher such implications and discount the financially constrained firms' stocks promptly, stock prices will be likely to decline on a timely basis over time without triggering a crash, thereby lowering future stock price crash risk. In such a case, the association between financial constraints and future stock price crash risk could be negative. To the extent that the relation of financial constraints with future crash risk is ambiguous, an empirical analysis of the relationship is warranted.

As with previous studies (e.g. Chen, Hong, and Stein 2001; Jin and Myers 2006; Hutton, Marcus, and Tehranian 2009), we focus solely on firm-level stock price crashes; crash risk that is attributed to market-wide factors is not within the scope of our study. Following Hutton, Marcus, and Tehranian (2009), we measure crash risk based on the likelihood of extreme negative firm-specific weekly stock returns for a fiscal year. For robustness checks, we use four other proxies for crash risk as well: (i) the number of crash weeks with negative extreme weekly returns, (ii) the negative skewness of firm-specific weekly stock returns, (iii) the 'down-to-up' volatility of firm-specific weekly returns, and (iv) the minimum value of firm-specific weekly returns, as per prior research (e.g. Chen, Hong, and Stein 2001; Hutton, Marcus, and Tehranian 2009; Kim, Li, and Zhang 2011a, 2011b; Callen and Fang 2013, 2015; Andreou, Louca, and Petrou 2017; Chang, Chen, and Zolotoy 2017; Lobo et al. 2017). We measure financial constraints by the SA index developed by Hadlock and Pierce (2010), and use KZ index (Kaplan and Zingales 1997) and cash dividends as the alternative financial-constraint measures. Using a sample of 32,661 firm-year observations from U.S. listed firms for the period 1995–2019, we find that financial constraints are positively associated with one-year-ahead stock price crash risk. This association is both economically and statistically significant, suggesting that investors might not be capable of appreciating the risks and prospects of financial-constraint firms.

There are two plausible sources of endogeneity. One is potential measurement error in our financial-constraint proxy, and the other is correlated omitted variable(s), either of which might bias our results and inferences. Such endogeneity is addressed in two quasi-natural experimental settings. First, following Almeida, Hsu, and Li (2013) and Kim (2018), we use the collapse of the junk bond market in 1989 as an exogenous shock to firm financial constraints and conduct a difference-in-differences (DID) regression analysis. The exogenous events in 1989 restricted the supply of credit to speculative-grade firms, thereby considerably tightening up their financial constraints (Lemmon and Roberts 2010). Accordingly, we define the treatment firms as those that receive a speculative grade from the Standard and Poor's (S&P) credit rating agency, and the control firms as those without an S&P credit rating.[2] Our DID regression results suggest that an increase in crash risk for the treatment firms, which are subject to tightened financial constraints during the post-collapse period, is significantly higher than that for the unrated control firms, of which the financial constraint statuses are much less affected by the junk-bond-market collapse. This result elicits a causal inference that financial constraints lead to higher future crash risk.

The second quasi-experimental setting involves the Internet bubble, which exogenously relaxed financial constraints for non-technology (henceforth, non-tech) firms (Campello and Graham 2013). With the rapidly increasing use of the Internet for commerce in the 1990s, the technology (hereafters, tech) profession thrived; tech firms soared up, with their stock prices increasingly overvalued by the market. This overvaluation had significant spill-over effects on the non-tech stocks, making their prices generally inflated as well (Caballero, Farhi, and Hammour 2006; Anderson, Brooks, and Katsaris 2010). The market optimism and excess supply of capital in the U.S. stock market gave rise to a stock price bubble, which started in 1995 and persisted until 2000. A firm's financial constraint status hinges critically on the supply of funds to the firm *vis-à-vis* its demand for funds, the latter of which is determined by firms' investment needs. Conditional on the investment needs being unaffected by the bubble, such a bubble would exogenously decrease the financial constraint, if any, of a firm, because the firm can ease the financial constraint by raising more funds from equity issuances in the bubble period. Whereas tech firms had significantly increased investment opportunities during the bubble, non-tech firms did not (Jorgenson and Stiroh 1999; Gordon 2000; Stiroh 2002) and hence are well suited for use in our natural-experimental setting. Consistent with Campello and Graham (2013), non-tech firms that are (are not) in financial constraints during the pre-bubble period are used as our treatment (control) firms. We implement a coarsened-exact-matching approach, per Iacus, King, and Porro (2012), to match the treatment firms with the control firms based on the determinants of financial constraints. Using a difference-in-differences research design, we find that non-tech firms that face financial constraints in the pre-bubble period experience significantly larger decreases in crash risk, as a result of the ease of financial constraints, during the bubble period, compared with the control firms. This again corroborates the causal, positive relationship between financial constraints and future crash risk. The natural experiments that use the exogenous events to exploit the variation in financial constraints also obviate the need to directly use the financial constraint variables for the regression analysis, thus allaying the concern of potential measurement error associated with the variables.

To offer more insights into our main analyses, we further conduct three tests. First, we test the underlying mechanism through which financial constraints are associated with future stock price risk. We find that abnormal accruals and distress risk are higher for financially constrained firms, thereby lending support to our conjecture that bad news hoarding and default risk are two mechanisms that explain the high crash risk of financially constrained firms. Second, we analyze our baseline regression results cross-sectionally and find that the positive association between financial constraints and future crash risk is stronger for firms with weak corporate governance. In the last test, we examine the association between financial constraints and longer-term future crash risk. Our results show that financial constraints remain positively correlated with future crash risk on the two-year and three-year horizons, respectively.

Our paper makes two major contributions to the literature. First, we contribute to the literature on economic consequences of financial constraints. Prior research focuses on the impact of financial constraints on firm performance (e.g. Lamont, Polk, and Saá-Requejo 2001; Livdan, Sapriza, and Zhang 2009; Campello and Chen 2010; Li 2011), cost of capital (Gomes, Yaron, and Zhang 2006; Campbell, Dhaliwal, and Schwartz 2012), corporate policies (Denis and Sibilkov 2010; Hovakimian 2011), and real business activities (Campello, Graham, and Harvey 2010). Our study investigates the impact of financial constraints from a different angle by examining the role of financial constraints in information management and focusing on the extreme future returns of financially constrained firms. We employ rigorous identification strategies as to quasi-experimental designs to establish a causal effect of financial constraints on future stock price crash risk. To the best of our knowledge, this study is the first to examine crash risk of financially constrained firms.

Second, there are three key drivers of firm-specific stock price crash risk: (i) managerial bad-news hoarding; (ii) firms' fundamental risk profiles, which generate unexpected, egregious bad news impossible for managers to withhold once it occurs; (iii) market frictions that hinder investors' abilities to discern the bad news hoarding or a high risk of the egregious bad news. The vast literature on crash risk (e.g. Kim, Li, and Zhang 2011a, 2011b; He 2015; Kim, Wang, and Zhang 2016; Andreou, Louca, and Petrou 2017; Chang, Chen, and Zolotoy 2017; Hong, Kim, and Welker 2017; He, Bai, and Ren 2019) focuses predominantly on the first driver of crash risk. Our study complements this literature by shedding light on the other two drivers as well. Specifically, we offer insight that financially constrained firms' high crash risk is also attributable to their high risk of corporate failure, and that investors are unlikely to infer the implications of financial constraints for future crash risk.

A recent study by Andreou, Andreou, and Lambertides (2021) documents a positive association between distress risk and stock price crash risk. Distress risk (or default risk) refers to the probability that a firm fails to meet its financial obligations (Vassalou and Xing 2004; Campbell et al., 2008; Garlappi, Shu, and Yan 2008) and is conceptually different from financial constraint which is defined as the constraint a firm confronts in funding its desired investments. Kaplan and Zingales (2000, 710) argue that '*financial distress is a form of being financially constrained*', implying that financial constraint is an important aspect in determining a firm's default risk but not necessarily vice versa. The key variable explaining crash risk in our study is financial constraints, whereas the key explanatory variable in Andreou, Andreou, and Lambertides (2021) is default risk. That said, our results for the test of the default-risk mechanism are consistent with Andreou, Andreou, and Lambertides's (2021) finding that financially distressed firms are more prone to stock price crashes.

The remainder of the paper is structured as follows. Section 2 develops our hypotheses. Section 3 describes our sample, measurements of key variables, and research design. Section 4 presents our empirical results. Section 5 conducts further tests, and Section 6 concludes.

2. Hypothesis development

Prior research has proposed a number of explanations for firm-level stock price crashes, among which managerial bad news hoarding is considered as a fundamental cause of stock price crashes (e.g. Jin and Myers 2006; Bleck and Liu 2007; Hutton, Marcus, and Tehranian 2009; Benmelech, Kandel, and Veronesi 2010; Kim, Li, and Zhang 2011a, 2011b; Kim and Zhang 2014, 2016; Chang, Chen, and Zolotoy 2017; Hong, Kim, and Welker 2017). Withholding one piece of bad news entails a low risk of detection by outsiders, because it is difficult for them to discern whether managers are withholding the bad news or unaware of it. However, as withheld bad news accumulates, it would become increasingly hard for insiders to continually hoard it. The occurrence of a stock price crash is attributed to a sudden overrun of a bad-news-hoarding limit, a threshold point at which managers can no longer withhold any unfavorable information. At that point, all the hidden news would come out at once, resulting in a sudden stock price plunge. The maximum amount of bad news that managers can withhold varies unforeseeably and constantly with a firm's changing environments, making it difficult for managers to *anticipate* by themselves when the threshold point will be reached and to prevent a stock price crash from occurring (He 2015). As such, the incidence of a stock price crash depends on how much bad news managers withhold. The greater the extent to which managers camouflage their firm's unfavorable information, the higher the future crash risk. Since funds are crucial for a firm's survival and development, a financially constrained firm is likely to have more bad news on its business than an unconstrained firm. Given the limited amount of internal funds available for investments and operations, financially constrained firms are in great need of external funds. To facilitate external financing, they are more likely to withhold bad news and have a high risk of future stock price crashes.

On the other hand, managers may opt not to withhold bad news, such that stock prices are less likely to be inflated and crash in the future. Managers' decisions to withhold bad news depend on their trade-off between the benefits of securing enough external finance and the costs associated with potential reputational losses and threat of litigation. Prior studies suggest that early revelation of bad news might reduce the likelihood of being sued and the expected costs of litigation (Skinner 1994; Skinner 1997; Field, Lowry, and Shu 2005; Donelson et al. 2012). If the legal and reputational costs are expected to be high, managers may choose not to hide bad news. However, bad news hoarding is unlikely to detect by outsiders who generally do not have access to private corporate information. Therefore, we posit that managers in financially constrained firms are inclined to withhold bad news since the associated detection risk is low.

The potentially high default risk of financially constrained firms provides yet another explanation for their high future crash risk. Fazzari et al. (1988), Almeida, Campello, and Weisbach (2004), and Acharya, Almeida, and Campello (2007) document that the investment spending by financially constrained firms is more sensitive to cash flows than that by unconstrained firms; this is primarily because constrained firms are subject to restrictions in accessing external finance. Whereas cash adequacy helps financially healthy firms to avoid default, cash shortages that often beset financial-constraint firms are likely to induce their corporate default (Davydenko 2007). Thus, a financially constrained firm is inherently more likely to default than an unconstrained firm. Consistent with this notion, the survey research of Campello, Graham, and Harvey (2010) suggests that a firm's inability to fund investments, which manifests itself in high financial constraints, would lead to higher distress risk. Because firms with high default risk are more likely to fail and experience crashes at the point of default (Zhu 2016; Andreou, Andreou, and Lambertides 2021), it follows that financially constrained firms are more prone to stock price crashes. Furthermore, to avoid, or delay the realization of, a default, financially constrained firms have incentives to bypass some positive net-present-value projects. This gives rise to the debt overhang problem (Smith and Warner 1979), aggravating future default risk and associated crash risk.

Both the bad-news-hoarding and default-risk mechanisms predict that financial constraints are positively associated with future crash risk. Nonetheless, if investors are able to discover a financial constraint and infer its implications for bad news hoarding and default probability, financially constrained stocks will be discounted by investors promptly, such that the stock price will not be inflated in a way that likely plunges significantly at a particular point in time. Therefore, we propose the following null hypothesis for empirical tests:

H1. Financial constraints are related to future stock price crash risk.

3. Data and variable measurements

3.1. Data sources and sample selection

We obtain data primarily from four sources, the Center for Research in Security Prices (CRSP), Compustat, Factset, and Institutional Shareholder Services (ISS). The crash risk variables are constructed using stock returns data from the CRSP database. Firms' financial information is collected from the Compustat database. The institutional ownership data are taken from the Factset database. Given that our crash risk measure is one-year ahead of the financial-constraint index and control variables in our regressions, the sample period for our crash risk variables (financial constraint variable) ranges from 1996 (1995) to 2019 (2018). We require that firms have necessary data available for constructing the variables of interest for our empirical analyses. In dealing with potential outliers, we winsorize all of the continuous variables at the top and bottom 1% levels. Our final sample comprises 32,661 firm-year observations corresponding with 7,335 unique firms. Table 1 presents descriptive statistics of the main variables used in our main multivariate tests. The Spearman correlations among the variables used in our baseline regression are reported in Table 2. It shows that the correlation between financial constraints (*SA*) and future crash risk (*crashrisk*) is positive and statistically significant at the 5% level.

3.2. Crash risk measures

In line with prior literature (Chen, Hong, and Stein 2001; Hutton, Marcus, and Tehranian 2009; Kim, Li, and Zhang 2011a, 2011b; Callen and Fang 2013, 2015; Kim and Zhang 2016; Andreou, Louca, and Petrou 2017;

Table 1. Descriptive statistics.

Variables	No. of firm-years	Mean	Std. dev.	25th	Median	75th
$crashrisk_{t+1}$	32,661	0.1933	0.3949	0	0	0
$ncrash_{t+1}$	32,421	0.3211	0.6516	0	0	0
$ncskew_{t+1}$	32,312	5.2603	19.9235	−4.3637	4.1126	12.8863
$duvol_{t+1}$	32,648	−0.1578	0.4147	−0.4002	−0.1338	0.1123
$minreturn_{t+1}$	32,661	2.4883	0.7022	2.0020	2.3626	2.8441
SA_t	32,661	−1.1592	1.2557	−2.1299	−0.5319	−0.1252
KZ_t	26,820	0.9652	0.7015	0.4348	0.8771	1.3585
$dividend_t$	32,184	1.6674	2.1762	0	0	3.3842
$lnequity_t$	32,661	6.4554	2.0278	5.0680	6.5121	7.8328
btm_t	32,661	0.7061	0.7947	0.2788	0.5021	0.8323
roa_t	32,661	−0.0205	0.2098	−0.0174	0.0290	0.0671
$lanacov_t$	32,661	2.7335	1.5750	1.7918	3.0910	3.9120
$insti_t$	32,661	0.5010	0.3447	0.1678	0.5429	0.8050
$opacity_t$	32,661	−1.0781	2.8100	−2.9440	−1.6828	0.1217
$stdret_t$	32,661	0.0645	0.0393	0.0368	0.0541	0.0802
$qtrret_t$	32,661	0.0095	0.5483	−0.3025	−0.0521	0.2032
$shareturnover_t$	32,661	1.5179	1.4935	0.5233	1.0699	1.9736
$debt_t$	32,661	0.2011	0.1996	0.0006	0.1596	0.3425
$abaccruals_t$	16,869	1.3410	6.8583	−0.0784	0.0509	0.2322
$salesgrowth_t$	19,396	0.1664	0.6102	−0.0523	0.0683	0.2245
$intangible_t$	19,396	0.0175	0.0741	0	0	0
$tangible_t$	11,783	0.9647	0.0998	1	1	1
$auditfee_t$	19,396	13.7310	1.3116	12.8558	13.7832	14.5936
$rating_t$	11,783	2.5216	0.2673	2.3026	2.5649	2.7081

Notes: This table presents descriptive statistics for the variables used in the multivariate tests. The sample contains firm-year observations for the period 1995–2019. All the variables are defined in Appendix 1.

Table 2. Spearman correlations.

Variables	crashrisk$_{t+1}$	SA$_t$	lnequity$_t$	btm$_t$	roa$_t$	lanacov$_t$	insti$_t$	opacity$_t$	stdret$_t$	qtrret$_t$	shareturnover$_t$	ncskew$_t$	debt$_t$
crashrisk$_{t+1}$	1												
SA$_t$	0.0111**	1											
	(0.045)												
lnequity$_t$	−0.0043	−0.8612***	1										
	(0.442)	(< 0.001)											
btm$_t$	0.0003	−0.0403***	−0.3378***	1									
	(0.958)	(< 0.001)	(< 0.001)										
roa$_t$	−0.0239***	−0.2440***	0.3514***	−0.1994***	1								
	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)									
lanacov$_t$	0.0556***	−0.6566***	0.7419***	−0.2308***	0.1953***	1							
	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)								
insti$_t$	0.0579***	−0.4063***	0.4815***	−0.1700***	0.2060***	0.5070***	1						
	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)							
opacity$_t$	0.0480***	0.1878***	−0.0676***	−0.1522***	−0.1004***	−0.0598***	−0.0466***	1					
	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)						
stdret$_t$	0.0800***	0.5153***	−0.4762***	0.0068	−0.3768***	−0.1878***	−0.2173***	0.1802***	1				
	(< 0.001)	(< 0.001)	(< 0.001)	(0.218)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)					
qtrret$_t$	−0.1831***	−0.1210***	0.2594***	−0.3178***	0.2609***	0.0620***	0.1519***	−0.0073	−0.1462***	1			
	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(0.188)	(< 0.001)				
shareturnover$_t$	0.1274***	−0.2222***	0.3559***	−0.2384***	0.0120**	0.5426***	0.4779***	0.0850***	0.2364***	0.0645***	1		
	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(0.030)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)			
ncskew$_t$	−0.3999***	0.1598***	−0.1637***	−0.0073	−0.0476***	−0.2027***	−0.1448***	−0.0186***	0.0677***	0.1751***	−0.1745***	1	
	(< 0.001)	(< 0.001)	(< 0.001)	(0.186)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)		
debt$_t$	−0.0416***	−0.4086***	0.2013***	0.1187***	−0.0122**	0.1006***	0.0408***	−0.1061***	−0.2461***	−0.0007	−0.0863***	−0.0319***	1
	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(0.028)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(0.893)	(< 0.001)	(< 0.001)	

Notes: This table reports the results for the Spearman correlations among the variables used in Model (4). The sample consists of 32,661 firm-year observations and covers the years 1995–2019. All the variables are defined in Appendix 1. The *p*-values in parentheses are based on robust standard errors clustered by firm. *, **, and *** indicate the two-tailed statistical significance at the 10%, 5%, and 1% levels, respectively.

Chang, Chen, and Zolotoy 2017; Lobo et al. 2017; He, Ren, and Taffler 2021a), we employ five measures of firm-specific stock price crash risk: (i) the likelihood of negative extreme firm-specific weekly returns over a fiscal year (*crashrisk*); (ii) the number of crash weeks with negative extreme firm-specific weekly returns (*ncrash*); (iii) the negative of the third-moment of firm-specific weekly returns (*ncskew*); (iv) the down-to-up volatility of firm-specific weekly returns (*duvol*); and (v) the negative of the minimum weekly return over a fiscal year (*minreturn*). The weekly stock returns are all adjusted for market-wide factors.

As per Hutton, Marcus, and Tehranian (2009) and Kim, Li, and Zhang (2011a, 2011b), a stock price crash is defined as a situation in which a firm experiences a firm-specific weekly return falling 3.2 standard deviations below the mean firm-specific weekly return for a fiscal year.[3] *crashrisk* equals 1 if a firm experiences one or more stock price crashes in a fiscal year and 0 otherwise. *ncrash* is equal to the number of crash weeks, in which a firm experiences a negative extreme weekly return, over a fiscal year. *ncskew* is defined as the third moment of firm-specific weekly returns for a stock and is expressed as follows:

$$ncskew_{it} = -\frac{n(n-1)^{3/2}\sum R_{it}^3}{(n-1)(n-2)\left(\sum R_{it}^2\right)^{3/2}} \tag{1}$$

duvol is calculated based on the standard deviation of 'down'-week firm-specific weekly returns relative to the standard deviation of 'up'-week firm-specific weekly returns and is expressed as follows:

$$duvol_{it} = \frac{(n_u-1)\sum_{DOWN} R_{it}^2}{(n_d-1)\sum_{UP} R_{it}^2} \tag{2}$$

where the standard deviation of 'down' ('up')-week firm-specific weekly returns is scaled by the number of 'down' ('up') weeks ($n_d(n_u)$) minus one. A 'down' ('up') week is defined as a week in which firm-specific weekly stock return is below (above) the mean weekly return for a fiscal year. The last crash risk variable, *minreturn*, is computed as -1 times the minimum value of firm-specific weekly returns, less the mean firm-specific weekly return, and divided by the standard deviation of firm-specific weekly returns, for a fiscal year.

Our empirical analysis is based mainly on the *crashrisk* variable, which is consistent with Hutton, Marcus, and Tehranian (2009); the other four crash risk variables (i.e. *ncrash*, *ncskew*, *duvol*, *minreturn*) are used for robustness checks.[4] 16.61% of our sample observations (corresponding with 5,385 firm-years) experience one crash ($ncrash = 1$), 5.18% (corresponding with 1,678 firm-years) have two crashes ($ncrash = 2$), and 1.72% (corresponding with 557 firm-years) undergo more than two crashes. These statistics are close to those reported by Hutton, Marcus, and Tehranian (2009). As reported in Table 1, the mean of *crashrisk* in our sample is 0.1933, indicating that the firm-specific stock price crash risk is, on average, 19.33% for a fiscal year. This is in line with the figures reported in prior research (e.g. Hutton, Marcus, and Tehranian 2009; Kim, Li, and Zhang 2011a, 2011b).

3.3. *Financial constraint measures*

The SA index constructed by Hadlock and Pierce (2010) is used as our primary measure of financial constraints and is defined as follows:

$$SA = -0.737 \times \text{size} + 0.043 \times \text{size}^2 - 0.040 \times \text{age} \tag{3}$$

where *size* is the natural logarithm of the book value of total assets, and *age* is the number of years for which a firm has been listed. More financially constrained firms have higher SA indices (*SA*). For robustness check, we use Kaplan and Zingales (1997) (KZ) index and cash dividends as alternative proxies for financial constraints. Higher KZ index (*KZ*) and lower cash dividends (*dividend*) indicate higher financial constraints (e.g. Fazzari et al. 1988; Denis and Sibilkov 2010). These measures of financial constraints might be subject to measurement errors, inducing an endogeneity problem to our multivariate analysis. We address this concern in Sections 4.2 and 4.3 by conducting two natural experiments in which the collapse of the junk bond market in 1989 and the Internet bubble in the late 1990s, respectively, are used as exogenous shocks to firms' financial-constraint statuses.

4. Research design and empirical results

4.1. Baseline regression analysis of the hypothesis H1

We estimate the following pooled logit regression model to test the hypothesis H1:

$$Crashrisk_{i,t+1} = \alpha_0 + \alpha_1 SA_{i,t} + \sum_k \alpha_k Controls_{i,t}^k + Industry\text{-}fixed\text{-}effects + Year\text{-}fixed\text{-}effects + \varepsilon_{i,t} \quad (4)$$

crashrisk and *SA* are defined as previously. Following prior literature (e.g. Chen, Hong, and Stein 2001; Jin and Myers 2006; Hutton, Marcus, and Tehranian 2009; Kim, Li, and Zhang 2011a, 2011b; Callen and Fang 2013), we include a broad set of control variables to ensure that our results are not driven by correlated omitted variable(s). We control for the market value of a firm's equity (*lnequity*) because Chen, Hong, and Stein (2001) and Hutton, Marcus, and Tehranian (2009) show that stock price crashes are more likely to occur among large firms. We control for the book-to-market ratio (*btm*), a proxy for a firm's growth opportunities, since Harvey and Siddique (2000) and Chen, Hong, and Stein (2001) find that growth firms are more prone to future stock price crashes. As per Kim, Li, and Zhang (2011a), we include return on assets (*roa*) to control for the effect of firm performance on crash risk. Previous studies (Chen, Hong, and Stein 2001; Callen and Fang 2013, 2015) document that analyst coverage might pressure managers into meeting and beating analyst forecasts, thereby exacerbating managerial myopia and increasing stock price crash risk. Hence, we control for analyst coverage (*lanacov*) and expect it to be positively correlated with future crash risk. Callen and Fang (2013) find supportive evidence that high institutional stock ownership curbs bad news hoarding and reduces future crash risk. Therefore, we also include institutional stock holdings (*insti*) as a control for crash risk.

Hutton, Marcus, and Tehranian (2009) find that firms with high financial opacity are more likely to experience future stock price crashes. Therefore, we control for financial opacity (*opacity*) and predict it to be positively correlated with future crash risk. Chen, Hong, and Stein (2001) find that highly volatile stocks are more likely to crash in their stock prices. Hence, we include stock return volatility (*stdret*) in the regression. We control for abnormal stock returns (*qtrret*), as stocks with high abnormal returns are more likely to crash in their prices in the future (Chen, Hong, and Stein 2001; Kim, Li, and Zhang 2011a). High trading volume is associated with high stock liquidity and hence with a higher likelihood of stock price crashes (Chang, Chen, and Zolotoy 2017). Thus, we control for trading volume (*shareturnover*) and predict its positive association with future crash risk. Prior literature (Chen, Hong, and Stein 2001; Jin and Myers 2006; Kim and Zhang 2014) finds that firms with high negative skewness in their weekly stock return distributions are more likely to have stock price crashes in future periods. Therefore, we include the negative weekly return skewness (*ncskew*), lagged one-year, as a control in our regression. To the extent that high financial leverage is associated with higher default risk (e.g. Edwards, Schwab, and Shevlin 2016; Andreou, Andreou, and Lambertides 2021; Campbell, Goldman, and Li 2021), we also include financial leverage (*debt*) as a control for crash risk in the regression. All the control variables are defined in detail in Appendix 1. Lastly, as with previous research (e.g. Denis and Sibilkov 2010; Campbell, Dhaliwal, and Schwartz 2012; Callen and Fang 2015; Chang, Chen, and Zolotoy 2017), we include industry-fixed effects and year-fixed effects in the crash-risk regression.[5]

Table 3 presents the regression results. Column (1) of Panel A reports the results for Model (4), where $\mathrm{crashrisk}_{t+1}$ is the dependent variable. The coefficient for SA_t is positive and statistically significant at the 0.1% level. A one-unit increase in SA_t leads to an increase in the probability of a stock price crash ($\mathrm{crashrisk}_{t+1}$) by 14.86 percentage points. This result indicates that financial constraint is positively associated with one-year-ahead stock price crash risk, and is consistent with our argument that outside investors are not able to deduce the implications of financial constraints for bad news hoarding and default risk. We use the foregoing alternative measures of financial constraints (i.e. KZ and dividend) and of crash risk (i.e. ncrash, ncskew, duvol and minreturn) to check the robustness of our baseline results. In Columns (2) and (3) of Panel A, the coefficient for KZ (dividend) is statistically significant at the 1% level with the expected positive (negative) sign. In Panel B of Table 3 which reports the results for the regressions of the alternative crash-risk measures, the coefficients for SA are all positive and statistically significant. In addition, we conduct a test of variance inflation factors (VIF)

Table 3. Tests of the hypothesis H1: The association between financial constraints and future stock price crash risk.

Panel A: Baseline regression analysis

		Dependent Variable $= crashrisk_{t+1}$		
Variables	Predicted sign	(1)	(2)	(3)
Intercept	?	−1.5609***	−1.0360*	−1.2485**
		(−2.906)	(−1.948)	(−2.319)
$\boldsymbol{SA_t}$	+	**0.1486*** **		
		(5.961)		
$\boldsymbol{KZ_t}$	+		**0.1068*** **	
			(3.219)	
$\boldsymbol{dividend_t}$	−			**−0.0475*** **
				(−4.646)
$lnequity_t$	+	0.0370*	−0.0530***	−0.0117
		(1.916)	(−3.563)	(−0.736)
btm_t	−	−0.0591**	−0.1057***	−0.1042***
		(−2.214)	(−3.773)	(−3.971)
roa_t	−	0.0862	0.1605*	0.1147
		(0.976)	(1.741)	(1.284)
$lanacov_t$	+	0.0674***	0.0797***	0.0647***
		(3.913)	(4.205)	(3.682)
$insti_t$	−	−0.0668	−0.0193	−0.0514
		(−1.121)	(−0.300)	(−0.857)
$opacity_t$	+	0.0185***	0.0182***	0.0197***
		(2.823)	(2.684)	(2.998)
$stdret_t$	+	−2.9755***	−3.1659***	−3.2044***
		(−4.837)	(−4.862)	(−5.151)
$qtrret_t$	+	0.1080***	0.1103***	0.1231***
		(3.746)	(3.541)	(4.281)
$shareturnover_t$	+	0.0684***	0.0595***	0.0638***
		(5.720)	(4.785)	(5.236)
$ncskew_t$	?	−0.0024***	−0.0012	−0.0026***
		(−2.606)	(−1.178)	(−2.714)
$debt_t$	+	−0.0140	−0.3880***	−0.1000
		(−0.152)	(−3.188)	(−1.124)
Industry-fixed effects		included	included	included
Year-fixed effects		included	included	included
No. of observations		32,661	26,820	32,184
Pseudo R-squared		0.0309	0.0269	0.0306

Panel B: Alternative measures of stock price crash risk measures

		Dependent variables			
Variables	Predicted sign	(1) $ncrash_{t+1}$	(2) $ncskew_{t+1}$	(3) $duvol_{t+1}$	(4) $minreturn_{t+1}$
Intercept	?	−1.0296**	10.8153***	−0.3919***	2.5959***
		(−2.286)	(3.457)	(−3.160)	(9.751)
$\boldsymbol{SA_t}$	+	**1.2554*** **	**4.8545*** **	**0.2355*** **	**0.5362*** **
		(5.212)	**(2.736)**	**(6.573)**	**(8.358)**
$lnequity_t$	+	0.0511***	−1.0001***	0.0454***	0.0312***
		(2.652)	(−7.323)	(15.438)	(6.133)
btm_t	−	−0.0056	0.2908*	−0.0135***	−0.0208***
		(−0.212)	(1.842)	(−3.535)	(−3.612)
roa_t	−	0.0485	0.5515	0.1484***	0.1239***
		(0.517)	(1.145)	(10.251)	(5.166)
$lanacov_t$	+	0.0672***	−0.5622***	0.0122***	0.0152***
		(3.931)	(−4.550)	(4.835)	(3.464)
$insti_t$	−	−0.1038*	1.6836***	0.0361***	−0.0081
		(−1.716)	(4.040)	(4.184)	(−0.511)
$opacity_t$	+	0.0274***	−0.0427	0.0022**	0.0065***
		(3.936)	(−0.852)	(2.053)	(3.348)
$stdret_t$	+	−1.9016***	−14.2674***	−1.5020***	−2.4505***
		(−3.023)	(−3.467)	(−15.659)	(−15.761)

(*continued*).

Table 3. Continued.

Panel B: Alternative measures of stock price crash risk measures

		Dependent variables			
Variables	Predicted sign	(1) $ncrash_{t+1}$	(2) $ncskew_{t+1}$	(3) $duvol_{t+1}$	(4) $minreturn_{t+1}$
$qtrret_t$	+	0.0703**	−0.8438***	0.0595***	0.0560***
		(2.413)	(−4.657)	(13.330)	(7.231)
$shareturnover_t$	+	0.0447***	0.0742	0.0045**	0.0158***
		(3.593)	(0.949)	(2.294)	(4.593)
$ncskew_t$	?	−0.0019*	0.0140*	−0.0006***	−0.0007***
		(−1.958)	(1.658)	(−4.291)	(−2.745)
$debt_t$	+	−0.0915	1.4485**	0.0031	0.0443*
		(−0.993)	(2.038)	(0.213)	(1.800)
Industry-fixed effects		included	included	included	included
Year-fixed effects		included	included	included	included
No. of observations		32,421	32,312	32,648	32,661
Pseudo R-squared		0.1535			
Adjusted R-squared			0.0461	0.1641	0.0608

Notes: This table presents the regression results for the tests of the association between financial constraints and future crash risk. The sample period for the independent variables covers the years 1995–2019. In Panel A, the logit regression is used in the test. The dependent variable, $crashrisk_{t+1}$, equals 1 if a firm experiences one or more firm-specific weekly returns falling 3.2 standard deviations below the mean firm-specific weekly return over the fiscal year t+1, and 0 otherwise. The treatment variables are SA_t, KZ_t, and $dividend_t$ from Column (1) to Column (3). In Panel B, the ordinary least squares (OLS) regression is used in the tests. The dependent variables are $ncrash_{t+1}$, $ncskew_{t+1}$, $duvol_{t+1}$, and $minreturn_{t+1}$ from Column (1) to Column (4), and are the alternative measures of stock price crash risk, while the treatment variable is SA_t for all the columns. All the variables are defined in Appendix 1. Industry dummies (constructed based on the first two digits of SIC codes) and year dummies are included in all the regressions, but their results are not reported for simplicity. The t-statistics in parentheses are based on robust standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

for all the regressors. The results, not tabulated for simplicity, reveal that VIF values are less than 10 for all the explanatory variables, indicating that multicollinearity is unlikely to be an issue with our regression analysis.

Overall, the baseline regression results suggest that investors are unable to decipher the implications of financial constraints for future crash risk, probably because the amount of hidden bad news and the probability of default can hardly be appraised by outsiders who generally do not have access to private information (Dye 1985; Jung and Kwon 1988; Dichev 1998; Griffin and Lemmon 2002; Campbell et al., 2008). As investors fail to discern the bad news hoarding, and/or a high risk of default, of financially constrained firms, their stocks would be overvalued, resulting in higher future stock price crash risk.

4.2. *Control for endogeneity – a collapse of the junk bond market and crash risk*

Outside investors, who generally do not have access to private information, are unlikely to appraise the amount of bad news withheld in a firm or extrapolate future crash risk from current default risk. Therefore, it is hard for investors to predict a firm's future stock price crash risk. On this basis, reverse causality is less of a concern in our study. That said, it is possible that either correlated omitted variable(s) or measurement error(s) in the financial-constraint index bias the coefficient estimates in our multivariate tests. To mitigate this concern, we follow Almeida, Hsu, and Li (2013) and Kim (2018) to conduct a quasi-experiment in which the collapse of bond market in 1989 is used as an exogenous shock that increased financial constraints of speculative-grade firms. Lemmon and Roberts (2010) argue that three unexpected events in 1989 led to a substantial decline in the supply of credit to speculative-grade firms. These events include (i) the collapse of Drexel Burnham Lambert, Inc., which caused a substantial reduction in funds available to speculative-grade firms; (ii) the passage of the Financial Institutions Reform, Recovery, and Enforcement Act of 1989 (FIRREA), which resulted in a forced sell-off of all junk bonds by Savings and Loans (S&Ls); and (iii) a change in the National Association of Insurance Companies (NAIC) credit rating guideline, which led to a sharp decrease in the life-insurance companies' commitments to purchase bonds from speculative-grade issuers. As a result of these events, speculative-grade firms, which used to rely heavily on junk bond issuances to secure external funds, became more financially constrained (e.g. Almeida, Hsu, and Li 2013). Therefore, the junk-bond-market collapse offers a nice experimental setting to examine the causal effect of financial constraints on crash risk. If the casual effect is positive, the increase in financial constraints of speculative-grade firms following the junk-bond-market collapse should lead to a more significant increase in crash risk, compared with nonrated firms that do not rely on bond financing.

Using the collapse of the junk bond markets as an exogenous event, we conduct a difference-in-differences (DID) test for the period 1987–1992, in which 1987–1989 (1990–1992) is designated as the pre- (post-) collapse period. The treatment firms are defined as those rated with a speculative grade (i.e. a grade of BB+ or lower) by the S&P credit rating agency in 1989 (i.e. the year prior to the collapse); the control firms are defined as those without an S&P credit rating in 1989.[6] The DID regression is specified below.

$$Crashrisk_{i,t+1} = \alpha_0 + \alpha_1 Junk_i + \alpha_2 PostCollapse_t + \sum_k \alpha_k Controls^k_{i,t} + Industry\text{-}fixed\text{-}effects + Year\text{-}fixed\text{-}effects + \varepsilon_{i,t} \quad (5)$$

$PostCollapse_t$ equals 1 if a firm is in the post-collapse period and 0 otherwise. $Junk_i$ equals 1 (0) if a firm pertains to a treatment (control) firm. The interaction term, $PostCollapse_t \times Junk_i$, captures the change in crash risk from the pre-collapse period to the post-collapse period for the treatment firms, relative to the control firms. The control variables included in Model (5) are similar to those in Model (4). The sample size decreases to 1,217 firm-years after clearing missing values for the control variables.[7]

The parallel trends assumption underlying our difference-in-differences regression analysis requires similar trends in crash risk for both the treatment and control firms over the pre-collapse period. To test the validity of this assumption, we follow Roberts and Whited (2013) to rerun our DID regression model by using 1988 and 1989 (as well as 1987 and 1988), respectively, as the pre- and post-'event' periods, respectively. As shown in Panel A of Table 4, we find no evidence of a substantive change in crash risk for the treatment firms relative to the control firms. This suggests that our DID regression estimation is not biased by potential violation of the parallel trends assumption. Table 5 reports the DID regression results. The coefficient on the interaction term, $PostCollapse_t \times Junk_i$, is positive (1.2820) and statistically significant at the 5% level, indicating that the treatment firms, which suffered from tightened financial constraints after the collapse of the junk bond markets, experienced higher crash risk than the control firms, which were not affected by the collapse event.[8]

4.3. Control for endogeneity – the Internet bubble and crash risk

The Internet bubble of the late 1990s, which generated exogenous variation in firms' financial constraints, is employed as our second quasi-experimental setting to examine the causal effect of financial constraint on crash risk. In the late 1990s, due to the prevalent use of computers, investors were keen on investing in tech firms, making technology stocks highly priced and yield over 1,000-percent returns (Ofek and Richardson 2003). The rise in technology stocks also fueled a run-up in non-tech firms' equity prices, thereby leading to a stock price bubble in the whole equity market. This bubble was argued to be driven by irrational euphoria among retail investors (Shiller 2000), speculative trading by hedge funds (Brunnermeier and Nagel 2004; Griffin et al. 2011), and limits of arbitrages (Morck, Shleifer, and Vishny 1990; Shleifer and Vishny 1997; Ofek and Richardson 2003). Financially constrained firms could take advantage of the stock price bubble by issuing equities to ease their financial constraints. In this sense, the bubble exogenously decreased firms' financial constraints. Nonetheless, the technological innovations that triggered the Internet bubble also brought a good deal of investment opportunities to tech firms, raising such firms' demand for funds and thereby engendering and/or amplifying their financial constraints; this offset the foregoing, attenuating effect that the bubble *per se* exerted on the tech firms' financial constraints. Therefore, we expect that only financially constrained *non-tech* firms experienced a substantial decrease in financial constraints during the bubble, when external funds became cheaper for the non-tech firms but their investment opportunities and demand for funds remained largely unchanged (Jorgenson and Stiroh 1999; Gordon 2000; Stiroh 2002).

On the above basis and in line with Campello and Graham (2013), our treatment (control) firms are defined as non-tech firms that faced high (low) financial constraints during the pre-bubble period 1990–1994; the bubble

Table 4. Tests of the parallel trends assumption

Panel A: The effect of the junk-bond-market collapse

	Dependent Variable $= crashrisk_{t+1}$	
Variables	(1) 1987 vs. 1988	(2) 1988 vs. 1989
$\boldsymbol{PostCollapse_t \times Junk_i}$	**1.2986**	**−0.9356**
	(1.133)	**(−1.158)**
Industry-fixed effects	included	included
Year-fixed effects	included	included
No. of observations	533	604
Pseudo R-squared	0.1061	0.0996

Panel B: The effect of the Internet bubble.

	Dependent Variable $= crashrisk_{t+1}$				
Variables	(1) 1990 vs. 1991	(2) 1991 vs. 1992	(3) 1992 vs. 1993	(4) 1993 vs. 1994	(5) 1994 vs. 1995
$\boldsymbol{Bubble_t \times FC_i}$	**−3.0068**	**−0.0525**	**0.4807**	**0.1277**	**−0.3416**
	(−1.488)	**(−0.047)**	**(0.659)**	**(0.409)**	**(−1.623)**
firm-fixed effects	included	included	included	included	included
Year-fixed effects	included	included	included	included	included
No. of observations	122	127	443	564	696
Pseudo R-squared	0.3162	0.1391	0.1449	0.1798	0.1521

Notes: This table presents the results from testing the parallel trends assumption. Panel A reports the multivariate results of the test of the treatment effect of the junk-bond-market collapse. 1987 and 1988 as well as 1988 and 1989 are used as the pre- and post-treatment periods, respectively, for the estimation of DID regression model (5). For simplicity, only the results of the coefficients on the interaction term, $PostCollapse_t \times Junk_i$, are reported. Panel B reports the multivariate results of the test of the treatment effect of the Internet bubble. 1990 and 1991, 1991 and 1992, 1992 and 1993, 1993 and 1994, and 1994 and 1995 are used as the pre- and post-treatment periods, respectively, for the estimation of DID regression model (6). For simplicity, only the results for the coefficients on the interaction term, $Bubble_t \times FC_i$, are reported. All the variables are defined in Appendix 1. The t-statistics in parentheses are based on robust standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

period is defined to cover the years 1995–1999.[9] The pre-bubble financial constraint statuses of non-tech firms are measured by the standardized mean of the SA indices over the five-year pre-bubble period.

We implement coarsened exact matching (CEM) to reduce the imbalance in pre-treatment covariates between the treatment and control groups (Blackwell et al. 2009). The idea of CEM is to temporarily coarsen each covariate into meaningful strata, exactly match on these coarsened data, and retain only the un-coarsened values of the matched data. Specifically, we match the treatment firms with the control firms based on the pre-bubble firm characteristics as to equity value (*lnequity*), the book-to-market ratio (*btm*), the leverage ratio (*debt*), return on assets (*roa*), earnings volatility (*stdearnings*), and financial opacity (*opacity*), which are arguably related to firms' financial constraints. Unlike commonly used matching techniques such as propensity-score matching (PSM), CEM does not require checking *ex post* the covariate balance, as the coarsening levels are chosen *ex ante* (Iacus, King, and Porro 2012; King and Nielsen 2019). After an automated coarsening k-to-k match, our matched data contain the same number of treated and control units in all strata.

The following DID regression model is specified to carry out the experimental test.

$$Crashrisk_{i,t+1} = \alpha_0 + \alpha_1 Bubble_t \times FC_i + \sum_k \alpha_k Controls_{i,t}^k + Firm\text{-}fixed\text{-}effects + Year\text{-}fixed\text{-}effects + \varepsilon_{i,t} \quad (6)$$

$Bubble_t$ equals 1 (0) if a firm is in the Internet bubble (pre-bubble) period 1995–1999 (1990–1994). FC_i is equal to 1 (0) if a firm is a treatment (control) firm, defined as having a pre-bubble standardized mean of the SA indices that is higher (lower) than its sample median.[10] The interaction term, $Bubble_t \times FC_i$, captures the DID estimate of crash risk between the treatment and matched control firms across the pre-bubble and bubble periods. We maintain similar control variables as those included in Model (4). It is possible that the Internet bubble also caused exogenous changes in some unobserved firm-specific factors that influence crash risk. Accounting

Table 5. Tests of the hypothesis H1: The effect of the junk-bond-market collapse on stock price crash risk

Variables	Dependent Variable = $crashrisk_{t+1}$
Intercept	1.4703
	(1.508)
$Junk_i$	−0.5918
	(−1.484)
$PostCollapse_t \times Junk_i$	**1.2820****
	(2.384)
$lnequity_t$	−0.4208***
	(−3.896)
	(0.909)
roa_t	1.0482*
	(1.943)
$lanacov_t$	0.1693
	(1.612)
$stdret_t$	−1.8767
	(−0.593)
$qtrret_t$	−0.0855
	(−0.429)
$shareturnover_t$	0.2580
	(1.370)
$ncskew_t$	−0.0022
	(−0.442)
$debt_t$	0.1338
	(0.256)
Industry-fixed effects	included
Year-fixed effects	included
No. of observations	1,217
Pseudo R-squared	0.0899

Notes: This table reports the logit regression results of the difference-in-differences test for the effect of the junk-bond-market collapse on stock price crash risk. The sample period for the independent variables is 1987–1992. The dependent variable is $crashrisk_{t+1}$, as defined previously. The indicator variable, *Junk*, equals 1 if a sample firm is rated with a speculative grade (BB+ or lower) by the S&P credit rating agency in a year, and 0 if a firm does not receive an S&P credit rating in a year. The interaction term, $PostCollapse_t \times Junk_i$, is the DID estimator. All the variables are defined in Appendix 1. Industry dummies (constructed based on the first two digits of SIC codes) and year dummies are included in the regression, but their results are not reported for simplicity. The t-statistics in parentheses are based on robust standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

for this possibility, we include firm-fixed effects in the regression.[11] If the causal effect implied by the hypothesis H1 holds, the coefficient on $Bubble_t \times FC_i$ will be negative and statistically significant at a conventional level.

We first conduct a multivariate test of the parallel trends assumption for the DID analysis, as per Roberts and Whited (2013) and He, Ren, and Taffler (2022). Specifically, we re-run Model (6) by using 1990 and 1991 (as well as 1991 and 1992, 1992 and 1993, 1993 and 1994, or 1994 and 1995), respectively, as the pre-'event' and 'event' periods. In the results reported in Panel B of Table 4, none of the DID estimators are statistically significant, which signifies that the parallel trends assumption is tenable. Table 6 reports the DID regression results. As expected, the coefficient on the interaction term, $Bubble_t \times FC_i$, is significantly negative at the 5% level.[12] This indicates that non-tech firms faced with high financial constraints have significantly larger declines in crash risk during the Internet bubble when compared with non-tech firms that are less subject to financial constraints. The general inflation of stock prices during the bubble might imply higher crash risk for our treatment firms, but we still find the significantly lower crash risk of such firms. This reinforces our causal inference that the eases in financial constraints lead to lower stock price crash risk. By and large, the results for our second quasi-experiment speak strongly to the positive, causal relationship between financial constraints and future crash risk.

Table 6. Tests of the hypothesis H1: The effect of the Internet bubble (1995–1999) on stock price crash risk

Variables	Dependent Variable = $crashrisk_{t+1}$
Intercept	−4.2119**
	(−2.077)
$\boldsymbol{Bubble_t \times FC_i}$	**−0.4973****
	(−2.035)
$lnequity_t$	0.5667***
	(3.301)
btm_t	−0.0019
	(−0.008)
roa_t	2.0240
	(1.368)
$lanacov_t$	−0.1244
	(−1.025)
$insti_t$	−0.3806
	(−0.697)
$opacity_t$	−0.0288
	(−0.411)
$stdret_t$	−4.3672
	(−0.931)
$qtrret_t$	0.2248
	(1.509)
$shareturnover_t$	−0.0055
	(−0.025)
$ncskew_t$	−0.0114***
	(−3.823)
$debt_t$	0.2923
	(0.434)
Year-fixed effects	included
Firm-fixed effects	included
No. of observations	2,262
Pseudo R-squared	0.1105

Notes: This table reports the logit regression results of the difference-in-differences tests for the effect of the Internet bubble on stock price crash risk. The sample period for the independent variables is 1990–1999. Non-tech firms are those that do not have the first three digits of SICs of 355, 357, 366, 367, 369, 381, 382, or 384. The dependent variable is $crashrisk_{t+1}$, as defined previously. The interaction term, $Bubble_t \times FC_i$, is the DID estimator. All the variables are defined in Appendix 1. Firm-fixed effects and year dummies are included in the regression, but their results are not reported for simplicity. The t-statistics in parentheses are based on robust standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

5. Further tests

5.1. Tests of the mechanisms through which financial constraints affect future stock price crash risk

This section tests whether bad news hoarding and default risk are the two channels through which financial constraints are correlated with future stock price crash risk.

5.1.1. Test of the bad-news-hoarding mechanism

Under an accrual-accounting system, a firm's performance is based on earnings, which comprise accruals and cash flows. Firm management is responsible for giving shareholders earnings estimates, and the inherent subjectivity of these estimates provides managers with a tool to hide bad news (He, Li, and Shen 2021b). Prior studies (Hutton, Marcus, and Tehranian 2009; Zhu 2016) find evidence that earnings management is associated with a larger extent of bad news hoarding and with higher future crash risk, which supports the notion that managers tend to make aggressive accrual estimates to withhold bad news.

One type of accruals that managers can use to disguise bad news is working capital accruals, which involve balance sheet items such as inventories, accounts receivable, accounts payable, and provisions for liabilities. For example, by understating the provision for bad debt, managers can withhold customer-related bad news, which arises from deteriorating financial health of customers or worsening customer relationship. Other bad-news-hoarding strategies include understatements of an obligation to clean up polluted production sites or to provide warranty coverage for low-quality products sold, both of which would lead to a future outflow of cash for a firm. Appendix 2 shows more examples of managers using accruals to withhold bad news. In essence, aggressive recognition of accruals makes it difficult for outside investors to discern related corporate bad news. Accruals management thereby serves as a device for managers to conceal bad news. We thus use abnormal accruals as the proxy for managerial bad-news hoarding.

Firms could engage in accruals manipulation for one to three years before it is possibly reversed or detected (e.g. Dechow, Sloan, and Sweeney 1996; Hutton, Marcus, and Tehranian 2009). Therefore, we follow Hutton, Marcus, and Tehranian (2009) to construct the three-year moving sum of abnormal accruals (*opacity*) to capture accruals management and associated information hoarding. To test the bad-news-hoarding mechanism, we regress abnormal accruals (*opacity*) on financial constraints (*SA*) and a range of control variables. These variables include firm size (*lnequity*), growth (*btm* and *salesgrowth*), profitability (*roa*), intangible assets (*intangible*), analyst coverage (*lanacov*), institutional stock ownership (*insti*), business risk (*stdret*), and audit quality (*auditfee*), which are chosen based on prior literature (e.g. Ayers, Ramalingegowda, and Yeung 2011; He 2015). The definitions of all variables are given in Appendix 1. Table 7 reports the regression results. The coefficient for *SA* is positive and statistically significant ($p < 0.001$), suggesting that managers in financially constrained firms are likely to manipulate accruals to withhold bad news.[13] This explains why the future crash risk of a financially constrained firm is higher.

5.1.2. Test of the default-risk mechanism

A firm's credit rating reflects a credit rating agency's opinion about the firm's creditworthiness and its ability to meet financial obligations (Standard & Poor's 2009). A low credit rating implies a shorter distance to default. Therefore, financially constrained firms with low credit ratings should be more likely to default and to encounter stock price crashes. Moreover, low-credit-rating firms often find it difficult and costly to access external funds (Kisgen 2006; Manso 2013; He 2018). As a result, they tend to face high risks of default and of stock price crashes. Thus, we posit that default risk measured by credit rating is the second mechanism through which financial constraints affect future stock price crash risk. To test this mechanism, we regress credit rating on financial constraints as well as an array of the determinants of distress risk. Consistent with prior research (e.g. Campbell et al., 2008), the determinant variables include firm size (*lnequity*), financial leverage (*debt*), firm performance (*roa* and *qtrret*), business risk (*stdret*), stock liquidity (*shareturnover*), and asset tangibility (*tangible*). We report the regression results in Table 8. The coefficient for SA_t is positive and significant at the 1% level. This result indicates that default risk is indeed higher for financially constrained firms, and offers support to our supposition that default risk is the underlying mechanism that drives the crash risk of financially constrained firms.

5.2. Are the crash risk of financially constrained firms lower for firms that have stronger corporate governance?

Bad news is more likely to arise when there is an agency conflict between shareholders and firm management. Such bad news might be attributed to managerial rent extraction or other managers' self-interested behaviors. Concerns about job prospects, personal reputation, the value of option grants, and bonus plans (Graham, Harvey, and Rajgopal 2005; Kothari, Shu, and Wysocki 2009; Jiang, Kim, and Pang 2013; Baginski et al. 2018) give managers an incentive to withhold the bad news. Strong corporate governance puts managers under intense monitoring (Ashbaugh-Skaife, Collins, and LaFond 2006) and reduces their ability to hoard bad news (Ajinkya, Bhojraj, and Sengupta 2005; Karamanou and Vafeas 2005; Andreou et al. 2016; He, Ren, and Taffler 2021a), thereby mitigating future crash risk (Kim, Li, and Zhang 2011a, 2011b; Callen and Fang 2013; Andreou et al. 2016; Chang, Chen, and Zolotoy 2017). On this basis, we expect that managers in a well-governed, financially constrained firm are less likely to withhold bad news, and hence that their firm's future crash risk tends to be lower. Put differently, the positive association between financial constraints and future stock price crash risk should be weaker for firms with strong corporate governance.

Table 7. Test of the bad-news-hoarding mechanism: The association between financial constraints and abnormal accruals

Variables	Dependent Variable = $opacity_t$
Intercept	−5.2984***
	(−7.394)
$\boldsymbol{SA_t}$	**0.2235*** **
	(5.813)
$lnequity_t$	0.1285***
	(4.228)
btm_t	0.0043
	(0.137)
roa_t	0.1583
	(1.270)
$salesgrowth_t$	0.0829**
	(2.523)
$intangible_t$	0.1098
	(0.319)
$lanacov_t$	−0.0224
	(−0.813)
$insti_t$	−0.2410**
	(−2.571)
$stdret_t$	3.7873***
	(5.157)
$auditfee_t$	0.0014
	(0.039)
Year-fixed effects	included
Year-fixed effects	included
Industry-fixed effects	included
No. of observations	19,396
Adjusted R-squared	0.4247

Notes: This table presents the OLS regression results for the tests of the association between financial constraints and abnormal accruals. The data on audit fees ($auditfee_t$) for the years prior to 1999 are not available for us in our universities, so the sample period for the independent variables covers the years 1999–2019. The dependent variable is $opacity_t$, the natural logarithm of the three-year moving sum of the absolute value of annual discretionary accruals, while the treatment variable is SA_t. All the variables are defined in Appendix 1. Industry dummies (constructed based on the first two digits of SIC codes) and year dummies are included in all the regressions, but their results are not reported for simplicity. The t-statistics in parentheses are based on robust standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Building on previous studies (e.g. Byrd and Hickman 1992; Petra 2005; Callen and Fang 2013; Andreou et al. 2016), we employ nine corporate governance measures for our analysis. These measures are outside directors' stock ownership (*directorownership*) (e.g. Ayers, Ramalingegowda, and Yeung 2011), the proportion of independent directors on board (*indp*) (e.g. Laksmana 2008; Hoitash, Hoitash, and Bedard 2009; Li and Srinivasan 2011; Hazarika, Karpoff, and Nahata 2012; Masulis, Wang, and Xie 2012; Morellec, Nikolov, and Schurhoff 2012; Wintoki, Linck, and Netter 2012), board size (*boardsize*) (e.g. Core, Holthausen, and Larcker 1999; Laksmana 2008; Hoitash, Hoitash, and Bedard 2009; Li and Srinivasan 2011; Chen, Lu, and Sougiannis 2012; Hazarika, Karpoff, and Nahata 2012; Hoechle et al. 2012; Masulis, Wang, and Xie 2012; Wintoki, Linck, and Netter 2012; Andreou et al. 2016), CEO-chair duality (*CEOduality*) (e.g. Hazarika, Karpoff, and Nahata 2012; Masulis, Wang, and Xie 2012; Andreou et al. 2016), the percentage of busy independent directors (*indpbusy*) (e.g. Laksmana 2008; Hoitash, Hoitash, and Bedard 2009; Hoechle et al. 2012; Masulis, Wang, and Xie 2012; Andreou et al. 2016), the percentage of directors who age over 64 (*olddirector*) (e.g. Armstrong, Balakrishnan, and Cohen 2012; Hoechle et al. 2012), the percentage of female independent directors (*indpfemale*) (e.g. Shrader 1997; Carter, Simkins, and Simpson 2003; Erhardt, Werbel, and Shrader 2003; Adams and Ferreira 2009), the independence of the chairman of board (*directorchair*) (e.g. Armstrong, Balakrishnan, and Cohen 2012), and staggered board (*staggered*) (e.g. Zhao and Chen 2008). Detailed definitions of the corporate governance variables are provided in Appendix

Table 8. Test of the default-risk mechanism: The association between financial constraints and credit rating

Variables	Dependent Variable = $rating_t$
Intercept	1.8316***
	(47.523)
$\boldsymbol{SA_t}$	**−0.1686*** **
	(−4.080)
$lnequity_t$	0.0758***
	(23.614)
roa_t	0.3046***
	(8.990)
$debt_t$	−0.2172***
	(−12.041)
$qtrret_t$	−0.0647***
	(−16.593)
$stdret_t$	−2.2772***
	(−18.030)
$shareturnover_t$	−0.0211***
	(−8.439)
$tangible_t$	−0.0297
	(−1.092)
Year-fixed effects	included
Industry-fixed effects	included
No. of observations	11,783
Adjusted R-squared	0.6906

Notes: This table presents the OLS regression results for the tests of the association between financial constraints and credit rating. The data on the Compustat S&P ratings, which are subscribed by our university, are not available for the period starting from the year 2018, and hence our sample period for the independent variables covers the years 1995–2017. The dependent variable is $rating_t$. It is calculated as the natural logarithm of the Standard & Poor's long-term domestic issuer credit ratings, which range from 22 (AAA) to 0 (D/SD). The treatment variable is SA_t. All the variables are defined in Appendix 1. Industry dummies (constructed based on the first two digits of SIC codes) and year dummies are included in all the regressions, but their results are not reported for simplicity. The t-statistics in parentheses are based on robust standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

1. Low (high) values of *directorownership*, *indp*, *olddirector*, *directorchair*, *boardsize*, and *staggered* (*indpbusy*, *indpfemale*, and *CEOduality*) indicate weak corporate governance.

The corporate governance variables are constructed using data mainly from the ISS database, where the data are available only for the period 2007–2019. To ease the presentation of our results, we use common factor analysis to construct a composite measure of the nine corporate governance variables (*CGcomposite*). Its higher value indicates stronger corporate governance. We partition our sample into two groups based on the sample median of *CGcomposite*, and estimate Model (4) separately for the two subsamples. The results are reported in Table 9. Panel A presents descriptive statistics for the nine corporate governance variables that compose *CGcomposite*. Panel B presents the subsample regression results. As expected, the positive relation between financial constraints and future crash risk is statistically significant ($p = 0.043$) in the low-*CGcomposite* subsample but not in the high-*CGcomposite* subsample. These results support our conjecture that the positive link between financial constraints and future crash risk is more pronounced for firms with weak corporate governance.

5.3. The association between financial constraints and longer-term future crash risk

Our main test concerns the association between financial constraints and one-year-ahead crash risk. However, if the difficulty in raising external funds induces financially constrained firms to withhold bad news for an extended period (say, two to three years), financial constraints would have an impact on longer-term future crash risk. To test this conjecture, we extend the measurement windows of crash risk to two years and three years ahead of our

Table 9. Tests of the moderating effect of corporate governance on the baseline regression results.

Panel A: Descriptive statistics of corporate governance measures

CG variables	Obs.	Mean	Std. dev.	25th	Median	75th
directorownership	7,983	0.0119	0.0312	0.0013	0.0034	0.0081
indp	7,983	0.8024	0.1014	0.7500	0.8333	0.8889
boardsize	7,983	9.1679	2.2394	8	9	10
CEOduality	7,983	0.4920	0.5000	0	0	1
indpbusy	6,438	0.2298	0.1320	0.1111	0.2222	0.3333
olddirector	7,730	0.4335	0.1855	0.2857	0.4286	0.5714
directorchair	7,983	0.1476	0.3547	0	0	0
indpfemale	7,983	0.1338	0.1030	0	0.1250	0.2000
staggered	7,983	0.3996	0.4898	0	0	1
CGcomposite	6,283	0	1	−0.6657	0.0991	0.7341

Panel B: Subsample regression analysis based on a composite measure of corporate governance

	Dependent Variable = $crashrisk_{t+1}$	
	Corporate governance (*CGcomposite*)	
Variables	Low	High
Intercept	0.0755	0.2751
	(0.056)	(0.211)
$\boldsymbol{SA_t}$	**1.6376****	**−1.3682**
	(1.719)	**(−1.552)**
$lnequity_t$	0.0071	−0.1231**
	(0.076)	(−1.653)
btm_t	0.0107	−0.5740***
	(0.072)	(−3.232)
roa_t	−0.0429***	−0.0238
	(−2.216)	(−0.980)
$lanacov_t$	0.0386	0.0006
	(0.433)	(0.005)
$insti_t$	−0.1748	0.2315
	(−0.752)	(0.711)
$opacity_t$	0.0003	0.0001
	(1.139)	(0.330)
$stdret_t$	−12.6076***	2.0047
	(−3.711)	(0.535)
$qtrret_t$	0.4010***	−0.1843
	(3.201)	(−0.948)
$shareturnover_t$	0.0128	−0.0249
	(0.337)	(−0.642)
$ncskew_t$	−0.0011	−0.0007
	(−0.383)	(−0.289)
$debt_t$	0.0948	−0.1546
	(0.253)	(−0.380)
Industry-fixed effects	included	included
Year-fixed effects	included	included
No. of observations	2,788	2,771
Pseudo R-squared	0.0536	0.0511

Notes: Panel A presents descriptive statistics for the corporate governance variables as to the moderating effect of corporate governance on the relation between financial constraints and future stock price crash risk. The corporate governance variables are constructed using the data from Institutional Shareholder Services (ISS) database, where the data cover the period starting from 2007. The sample period for the financial constraints (crash risk) variable ranges from 2007 (2008) to 2018 (2019). Panel B presents the logit regression results for the moderating effect of corporate governance on the relation between financial constraints and future stock price crash risk. The dependent variable is the indicator variable, $crashrisk_{t+1}$. The treatment variable is the SA index (SA_t). The moderator variable used in Panel B is *CGcomposite,* a composite measure of corporate governance. *CGcomposite* is constructed based on 9 corporate governance measures: *directorownership, indp, boardsize*, *CEOduality*, *indpbusy*, *olddirector*, *directorchair, indpfemale, staggered.* Our sample is separated into two subsamples based on whether an observation has a value of *CGcomposite* higher than the sample median of *CGcomposite*. The high (low) *CGcomposite* subsample represents strong (weak) corporate governance group. All the variables are defined in Appendix 1. Industry dummies (constructed based on the first two digits of SIC codes) and year dummies are included in all the regressions, but their results are not reported for simplicity. The t-statistics in parentheses are based on robust standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (one-tailed), respectively.

Table 10. Additional test: The association between financial constraints and two-year- and three-year-ahead stock price crash risk.

Variables	(1) Dependent variable = $crashrisk_{t+2}$	(2) Dependent variable = $crashrisk_{t+3}$
Intercept	−1.4963***	−1.6493***
	(−5.817)	(−6.264)
SA_t	**0.1210*** **	**0.0970*** **
	(5.036)	**(3.667)**
$lnequity_t$	0.0452**	0.0403*
	(2.351)	(1.906)
btm_t	−0.0287	−0.0275
	(−1.030)	(−0.916)
roa_t	−0.0040	−0.0902
	(−0.044)	(−0.951)
$lanacov_t$	0.0650***	0.0623***
	(3.730)	(3.345)
$insti_t$	−0.0851	−0.1219*
	(−1.414)	(−1.925)
$opacity_t$	0.0074	−0.0058
	(1.111)	(−0.855)
$stdret_t$	−2.0251***	−1.6727**
	(−3.258)	(−2.547)
$qtrret_t$	0.0682**	0.0733**
	(2.375)	(2.465)
$shareturnover_t$	0.0527***	0.0476***
	(4.331)	(3.684)
$ncskew_t$	−0.0023**	−0.0013
	(−2.437)	(−1.291)
$debt_t$	−0.1024	0.0461
	(−1.114)	(0.478)
Year-fixed effects	included	included
Industry-fixed effects	included	included
No. of observations	32,400	27,438
Pseudo R-squared	0.0268	0.0294

Notes: Column (1) ((2)) of this table reports the logit regression results for the test of the association between financial constraints and two-year-(three-year-) ahead stock price crash risk. For the results in Column (1) ((2)), the sample period for the independent variables covers the years 1995–2018 (1995–2017), and the dependent variable is $crashrisk_{t+2}$ ($crashrisk_{t+3}$). The key independent variable is the SA index (SA_t). All the variables are defined in Appendix 1. Industry dummies (constructed based on the first two digits of SIC codes) and year dummies are included in both regressions, but their results are not reported for simplicity. The t-statistics in parentheses are based on robust standard errors clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

financial constraint measure (SA_t) and re-estimate Model (4). Specifically, we replace the one-year-ahead crash risk, $crashrisk_{t+1}$, with the two-year and three-year lead measures of crash risk, $crashrisk_{t+2}$ and $crashrisk_{t+3}$, respectively, as the dependent variable for our regression estimations.

Column (1) ((2)) of Table 10 reports the results as to the association between financial constraints and the two-year-ahead (three-year-ahead) crash risk. The coefficients on SA_t are both positive and statistically significant at the 1% level, which suggests that financial constraints can predict crash risk as far as two years and three years ahead, respectively. A one-unit increase in SA_t leads to an increase in the probability of a stock price crash $crashrisk_{t+2}$($crashrisk_{t+3}$) by 12.10 (9.70) percentage points. In results not tabulated for brevity, SA_t is also positively associated with the alternative crash risk variables, *ncrash*, *duvol*, and *minreturn*, which are measured on the two-year-ahead and three-year-ahead horizons, respectively. Also, this finding is not only statistically but also economically significant. Overall, our results imply that financial constraints are strongly predictive of future crash risk as far as three years ahead, thereby buttressing the bad-news-hoarding mechanism.

6. Conclusion

This study examines whether financial constraints are associated with future stock price crash risk. On the one hand, financially constrained firms have strong incentives to withhold bad news for an extended period to secure external funds. As withheld bad news accumulates, stock prices become increasingly overvalued, leading to a higher risk of future stock price crashes. On the other hand, financially constrained firms are subject to higher default risk and are more likely to undergo a stock price crash when they default. Consistent with these reasonings, we find strong evidence that financial constraints are positively correlated with the one-year-ahead stock price crash risk. This finding is robust to controlling for potential endogeneity in two quasi-experimental settings including the collapse of the junk bond market in 1989 and the Internet bubble in the late 1990s. In the quasi-natural experiments, crash risk was significantly higher (lower) in periods when firms' financial constraints were exogenously exacerbated (eased) by the collapse of the junk bond market (by the Internet bubble). These corroborate our causal inference that financial constraints lead to high future stock price crash risk, suggesting that outside investors are unlikely to extrapolate the implications of financial constraints for future stock price crash risk.

We also find that financially constrained firms tend to have larger abnormal accruals and higher default risk than unconstrained firms. This finding lends support to the bad-news-hoarding and default-risk mechanisms through which financial constraints lead to higher crash risk. These two mechanisms are not mutually exclusive and could jointly contribute to the positive effect of financial constraints on future crash risk. Further analysis reveals that this positive effect is stronger for firms with weak corporate governance, and that financial constraints can be associated with future crash risk as far as three years ahead.

Overall, our results shed light on the stock price crash risk of financially constrained firms and should have important implications for not only companies *per se* but also their stakeholders, including investors, creditors, suppliers, and customers concerned about the companies' creditworthiness, viability, and prospects. On the other hand, to mitigate crash risk, it is important for a financially constrained firm to build up strong corporate governance and to increase creditworthiness as well as information transparency to the public.

Notes

1. We refer to default risk as the probability of default, financial distress, economic distress, or bankruptcy, which are often used interchangeably in the literature (Campbell et al., 2008).
2. In this study, we use the S&P's long-term domestic issuer credit ratings to classify firms into investment-grade firms versus speculative-grade firms.
3. Our inferences remain qualitatively the same, if we re-define a stock price crash as a firm-specific weekly return falling 3.1, or 3.3, standard deviations below the mean firm-specific weekly return to do our empirical analysis.
4. *ncskew*, *duvol*, and *minreturn* might be relatively less powerful in measuring a stock price crash, compared with *crashrisk*. Suppose that stock price decreases slowly to a considerably low level in response to a firm's gradual release of bad news and then is maintained continually low for an extended period. In this case, the stock price decline features large negative skewness (*ncskew*), high down-to-up return volatility (*duvol*), and extreme low returns (*minreturn*) but should not be regarded as a stock price crash. The values of *ncrash* do not proportionally reflect the distinction in crash risk across different levels. For instance, the differential in crash risk, as indicated by the difference between $ncrash = 1$ and $ncrash = 2$, is far smaller than the differential in crash risk, as indicated by the difference between $ncrash = 0$ and $ncrash = 1$. Moreover, conceptually speaking, the *ncrash* variable measures more of the frequency, rather than the incidence, of stock price crashes, and hence is a relatively weak measure of crash risk.
5. We do not include firm-fixed effects in our model because they are multicollinear with industry dummies.
6. To reduce potential multivariate imbalance in covariates between the treatment and control groups, we apply coarsened-exact matching (CEM, the same approach used in Section 4.3), a monotonic imbalance bounding approach. Specifically, an automated coarsening k-to-k match is done between the treatment firms and control firms. We then repeat our DID analysis using the matched data, and obtain qualitatively the same results. However, the number of observations after the matching drops to 183 firm-years, reducing the power of the test. Hence, the results from the test need to be interpreted with caution. Likewise, when we include firm-fixed effects in Model (5), firms that have no time-series variation are removed from the regression estimation, reducing our sample to only 372 firm-years. Due to the lack of power of the test, we do not provide our firm-fixed-effects regression analysis.
7. To ensure sufficient observations for the test, the *opacity* variable, which has many missing values, is not included in Model (5). *Insti* is not included either, because none of the control firms in the period 1987–1992 have an institutional ownership greater than zero.
8. We also use alternative crash risk measures to run our DID regression. The results show that, when using *ncrash* as the dependent variable, the coefficient on $PostCollapse_t \times unk_i$ is positive (1.2819) and statistically significant at the 5% (p-value $= 0.017$).
9. We obtain qualitatively identical results, when using a bubble period 1996–1999 and a pre-bubble period 1992–1995 for the DID test. We do not include the year 2000 in our bubble period, because the bubble had burst, with stock price crashes occurring among a large number of firms, during that year.

10. Following previous literature (e.g., Bond and Cummins 2000; Campello and Graham 2013), we classify tech firms as those with the first three digits of SIC codes of 355, 357, 366, 367, 369, 381, 382, and 384. These codes correspond to special industry machinery, computer and office equipment, communications equipment, electric components and accessories, electric transmission and distribution equipment, electric industrial apparatus, miscellaneous electrical equipment, search and navigation equipment, measuring and controlling devices, and medical instruments, respectively. The non-tech firms refer to those not in these sectors.
11. The internet bubble had affected the non-tech firms in all industries, while the crash risk of the non-tech firms in our DID sample does not vary as substantially across industries as does the crash risk of the sample used in our baseline regression analysis. Thus, the control of industry effects is less important for the natural experiment, and we include firm-fixed effects, in lieu of the industry effects, in the DID regression.
12. Using the alternative crash risk measure, *minreturn*, to repeat our DID test, we obtain similar result: the coefficient on $Bubble \times C_i$ is negative -0.0939 and statistically significant at the 5% level.
13. We also use a one-year measure of abnormal accruals (*ab_accruals*) to test the bad-news-hoarding mechanism, and obtain similar result: the coefficient for *SA* is positive and statistically significant at the 5% level ($p = 0.034$).

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Disclosure statement

No potential conflict of interest was reported by the author(s).

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Appendices

Appendix 1. Summary of variable definitions

Variables	Definitions
crashrisk	1 if a firm experiences one or more firm-specific weekly returns falling 3.2 standard deviations below the mean firm-specific weekly return over a fiscal year, and 0 otherwise. The firm-specific weekly returns measure follows Kim, Li, and Zhang (2011a).
ncrash	The natural logarithm of 1 plus the number of firm-specific weekly returns that fall 3.2 standard deviations below the mean firm-specific weekly return over a fiscal year.
duvol	The standard deviation of 'down'-week firm-specific weekly returns (scaled by the number of 'down'-weeks minus one), divided by the standard deviation of 'up'-week firm-specific weekly returns (scaled by the number of 'up'-weeks minus one) over a fiscal year. The firm-specific weekly returns measure follows Kim, Li, and Zhang (2011a).
minreturn	The minimum value of firm-specific weekly returns over a fiscal year, times −1, less the mean firm-specific weekly return, divided by the standard deviation of firm-specific weekly returns over a fiscal year. The firm-specific weekly returns measure follows Kim, Li, and Zhang (2011a).
ncskew	The negative of the third moment of firm-specific weekly returns. The firm- specific weekly returns measure follows Kim, Li, and Zhang (2011a).
SA	A financial constraint index (*SA*) developed by Hadlock and Pierce (2010). $SA = -0.737 \times size + 0.043 \times size^2 - 0.040 \times age$, where *size* is the natural logarithm of total assets capped at $4.5 billion, and *age* is the number of years for which a firm has been listed. *SA* index is re-scaled by dividing 1,000.
KZ	A financial constraint index (*KZ*) developed by Kaplan and Zingales (1997). $KZ = -1.002^*(cf/ta) - 39.368^*(div/ta) - 1.315^*(ca/ta) + 3.139^*lev + 0.283^*mtb$, where *cf/ta* is the ratio of cash flows to the lagged book value of assets, *div/ta* is the ratio of cash dividends to the lagged book value of assets, *ca/ta* is the ratio of cash and cash equivalent to the lagged book value of assets, *lev* is the ratio of total debt to the current book value of assets, and *mtb* is the market-to-book ratio.
dividend	The natural logarithm of 1 plus cash dividends paid to common shareholders in a fiscal year.
lnequity	The natural logarithm of the market value of a firm's equity at the end of a fiscal year.
btm	The book value of firm equity divided by the market value of firm equity at the end of a fiscal year.
insti	Institutional investors' stock ownership as a percentage of total outstanding shares of a firm at the end of a fiscal year.
lanacov	The natural logarithm of 1 plus the number of analysts that make at least one annual earnings per share (EPS) forecast for a firm over a fiscal year.
roa	Return on assets at the end of a fiscal year.
stdret	The standard deviation of firm-specific weekly returns for a fiscal year. The firm-specific weekly returns measure follows Kim, Li, and Zhang (2011a).
qtrret	Buy-and-hold abnormal stock returns of a firm for a fiscal year.
stdearnings	The standard deviation of income before extraordinary items in the current and previous four fiscal years.
shareturnover	The average of monthly trading volume for a firm over a fiscal year, scaled by total shares outstanding of the firm at the end of the year.
opacity	The natural logarithm of the three-year moving sum of the absolute value of annual discretionary accruals, a measure of financial opacity developed by Hutton, Marcus, and Tehranian (2009).
ab_accruals	The abnormal accruals of a firm for a fiscal year, which is estimated using industry-specific modified Jones model per Dechow, Sloan, and Sweeney (1995).
debt	The sum of short-term debt and long-term debt divided by total assets of a firm for a fiscal year.
PostCollapse	1 if a firm is in the three-year period (i.e. 1990–1992) after the collapse of junk bond market in 1989, and 0 if a firm is in the three-year period (i.e. 1987–1989) as of the 1989 junk bond collapse.
Junk	1 if a firm is rated at BB+ or lower by the S&P credit rating agency, and 0 if a firm does not have an S&P credit rating, in the years (i.e. 1987–1989) prior to the collapse of the junk bond market. Credit ratings used in this study are the Standard & Poor's long-term domestic issuer credit ratings reported by Compustat.
FC	1 (0) if a firm is a financially constrained (unconstrained) non-tech firm that has the standardized mean of the SA indices higher (lower) than its sample median. The standardized mean of the SA indices is calculated based on the pre-bubble period 1990–1994.
Bubble	1 if a firm is in the Internet bubble period 1995–1999, and 0 if a firm is in the pre-bubble period 1990–1994.
salesgrowth	The difference between sales revenue for the current fiscal year and sales revenue for the previous fiscal year, divided by that for the previous fiscal year.
intangible	The ratio of intangible assets to total assets of a firm at the end of a fiscal year.
tangible	The ratio of tangible assets to total assets of a firm at the end of a fiscal year.
auditfee	The natural logarithm of audit fees incurred by a firm for a fiscal year.
rating	The natural logarithm of the Standard & Poor's long-term domestic issuer credit ratings. The ratings range from 22 (AAA) to 0 (D/SD).
directorownership	The outside directors' equity ownership as a percentage of total shares outstanding of a firm at the end of a fiscal year.
indp	The number of the independent outside directors on the board of a firm, divided by the number of all the directors on the board, at the end of a fiscal year.

(*continued*).

Variables	Definitions
boardsize	The number of directors on the board of a firm at the end of a fiscal year.
CEOduality	1 if the CEO and the chairman of the board are the same person for a firm for a fiscal year and 0 otherwise.
indpbusy	The number of the independent outside directors who hold two or more board directorships, divided by the number of the independent outside directors, for a firm as of the end of a fiscal year.
olddirector	The number of directors who are older than 64, divided by the number of all the directors on the board of a firm, at the end of a fiscal year.
directorchair	1 if the chairman of the board is an independent outside director for a firm for a fiscal year and 0 otherwise.
indpfemale	The number of the female independent outside directors, divided by the number of all the directors on the board of a firm, at the end of a fiscal year.
staggered	1 if a firm's board is a staggered board for a fiscal year and 0 otherwise.

Appendix 2. Examples of using accruals to withhold bad news

Strategies of manipulating accruals	Examples of corporate bad news
Understating impairment loss on inventories	Obsolescence or physical damage of products;
	Significant decline in some major customers' demand for products due to worsening customer relationship; deteriorating financial health of customers, or changes in customers' tastes, preferences, and needs on products;
	Emergence and increase in substitute products made by a competitor, which undermine the potential sales outlet and market value of existing products in stock.
Delaying or underestimating write-off of assets	A warehouse fire that impaired assets such as inventories, building, equipment, and machinery;
	Discontinued operations or disposals of a subsidiary, which reduce the values of currently operated assets;
	Changes in technologies, markets, or regulations which engendered adverse impacts that reduce the value of brands, goodwill, and other intangible assets.
Understating bad debt provisions	Deteriorating financial health of customers;
	Uncollectable payments due to bankruptcy or other cash-inadequacy issues of customers.
Understating other provisions or putting the provisions off balance sheet	Obligations to clean up polluted production sites;
	Obligations to provide warranty coverage for products sold due to the discovered malfunction of operating appliances;
	Obligations to pay expenses incurred from a lawsuit.