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Operating cash flow opacity and stock price crash risk

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ABSTRACT

We examine the relation between operating cash flow (OCF) opacity and stock price crash risk. We find that OCF opacity is positively associated with future stock price crash risk after controlling for accruals opacity and other determinants known to influence crash risk. This finding suggests that OCF opacity facilitates bad news hoarding and enables managerial resource diversion, which in turn increases crash risk. We also find that the positive relation between OCF opacity and crash risk is more pronounced when external monitoring is weak, information asymmetry is high, OCF importance is low, and cost of accruals management is high. Overall, our evidence highlights the severe consequence of OCF opacity in that it boosts crash risk; our study should alert the researchers, investors, and regulators to pay more attention to OCF management.

1. Introduction

Operating cash flow (OCF) is crucial to assessing firm performance. Standard textbooks on financial statement analysis recommend paying attention to OCF, because discretion permitted by accrual accounting in the form of estimates and reporting alternatives can potentially obfuscate performance measured by earnings (see books by Libby et al., 2008; Dyckman et al., 2010). Studies have long documented that investors pay attention to OCF (Ali and Zarowin, 1992; Cheng et al., 1996). Moreover, capital market participants give an increasing emphasis to OCF as evidenced by a growing and significant number of analysts and firms forecasting cash flow (DeFond and Hung, 2003; Wasley and Wu, 2006; Call et al., 2013; Mohanram, 2014). Recently, Barth et al. (2018) show that the value relevance of OCF increases over time.

Given the importance of OCF to market participants, managers have incentives to manage OCF (Lauricella, 2008; Lee, 2012), which in turn increases firm opacity. But many prior studies (e.g., Dechow et al., 1998; Badertscher et al., 2012) do not consider the possibility that OCF can be managed, and often use OCF as a benchmark for assessing earnings predictability or earnings quality (e.g., Dechow and Dichev, 2002). Not surprisingly, there is little research on the consequence of OCF management and opacity caused by OCF management (i.e., OCF opacity).[1] Nevertheless, examples of OCF management and stock price crash abound.[2] Therefore, to bridge this gap in the literature, we examine the effect of OCF opacity on firm-specific stock price crash risk.

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[1] Jin and Myers (2006, 281) define opacity as "the lack of information that would enable investors to observe operating cash flow and income and determine firm value".

[2] As an anecdotal example, during the four quarters of 2001 and the first quarter of 2002, WorldCom overstated cash flow by $3.8 billion through misreporting operating costs such as basic network maintenance as capital investments (Romero and Berenson, 2002). On June 26, 2002, WorldCom announced that they overstated cash flow and the stock price dropped by as much as 76 percent in the after-hour session following the announcement.

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We focus on firm-specific stock price crash risk for several reasons. By examining extreme return outcomes, one can learn more about the value implications of OCF opacity. Extreme outcomes, such as crashes, can have an extraordinary cumulative effect, providing invaluable information on the true nature of a phenomenon (Taleb, 2007). Moreover, firm-specific crash risk can be attributed to a firm-specific factor, such as OCF opacity, because it removes the return component driven by the market-wide factors. It is also critical to understand factors contributing to crashes or even forecast them, because crash risk cannot be reduced through diversification (Sunder, 2010), and crashes can have severe consequences for investor welfare.

OCF opacity has the potential to influence stock price crash risk because it could facilitate managerial bad news hoarding and resource diversion. As a critical source of firm-specific information, OCF informs market participants about firm performance. But when there is OCF opacity, it is more difficult for market participants to understand the true firm performance, which in turn enables managers to hide bad news. When such bad news accumulates for too long and is then suddenly released, a stock price crash occurs (e.g., Jin and Myers, 2006; Hutton et al., 2009). Moreover, OCF opacity can enable insiders to capture firm resources, cash flows in particular, for an extended period by providing a shield against their rent-extracting activities. The sudden revelation of extensive resource diversion could cause stock prices to drop dramatically as well (Kim et al., 2011a). To the extent that OCF opacity facilitates bad news hoarding and resource diversion for an extended period, we predict OCF opacity to be positively associated with future crash risk.

While this prediction is plausible, there are at least two reasons why it may not hold empirically. First, OCF opacity may not be large enough to increase crash risk because OCF is not an estimate and OCF management is more constrained than accruals management. Second, activities contributing to OCF opacity, such as delaying payments to suppliers and accelerating collections from customers, may be a good corporate practice that increases cash flows and reduces firms' reliance on external capital, which can enable firms to pursue value-enhancing projects. Thus, the relation between OCF opacity and crash risk remains an empirical question.

To test our prediction, we follow Hutton et al. (2009) and Kim et al. (2011a) to construct two proxies for firm-specific crash risk. One proxy is the likelihood that extremely negative, firm-specific weekly returns will occur in the future. The second one is the negative skewness of firm-specific weekly returns. Consistent with Jin and Myers (2006), we derive a nondirectional measure of OCF opacity. Specifically, we first construct abnormal OCF, the proxy for OCF management, using the residual from the model developed by Dechow et al. (1998) and implemented by Lee (2012) and Zhang (2018). Similar to Hutton et al. (2009), we then use the moving sum of the absolute value of abnormal OCF over the prior three years to capture OCF opacity, where higher values imply higher OCF opacity. We conduct tests to validate the OCF opacity measure. We find that our OCF opacity metric is associated with higher likelihood of OCF restatements (either upward or downward) and abnormal OCF is less persistent than normal OCF, suggesting that the measure captures the underlying construct.[3]

OCF opacity stems from two types of OCF management. One type only affects OCF but does not change earnings. For example, firms can delay (accelerate) payments to suppliers or accelerate (delay) collections from customers (i.e., timing). Firms can also increase (decrease) reported OCF by simply shifting items among the statement of cash flow categories (i.e., classification shifting). The other type influences both OCF and earnings. For instance, when discretionary expenses are reduced, both earnings and OCF increase (Dechow and Sloan, 1991; Roychowdhury, 2006). Given that our goal is to shed light on the overall effect of OCF opacity on crash risk, we do not distinguish between the two types of OCF management in the primary analyses.

Using a sample of U.S. public firms between 1992 and 2014, we find that OCF opacity is positively associated with one-year-ahead stock price crash risk beyond accruals opacity. This finding holds after controlling for several determinants known to influence crash risk. In addition, our results are robust to alternative measures of OCF opacity and alternative measures of crash risk. Our results also hold when we focus on OCF opacity stemming from the type of OCF management that does not change earnings (e.g., timing and classification shifting).

We also run several analyses to address the endogeneity concern, including additional controls and firm fixed effects. The baseline results of a positive association between OCF opacity and crash risk continue to hold across these analyses. Nonetheless, we acknowledge that the results we show may not imply causality. To better understand how OCF opacity affects crash risk, we explore the mechanisms underlying the OCF opacity – crash risk relation. We provide evidence that OCF opacity influences crash risk through bad-news hoarding and resource diversion by showing that OCF opacity increases the probability of very bad news release during the crash period and corresponds to more resource diversion. Although accruals opacity exhibits a quadratic relation with the probability of very bad news release during the crash window, it is not associated with resource diversion in an expected manner. The evidence suggests that OCF opacity and accruals opacity do not appear to affect crash risk through the exactly same mechanisms.

To further corroborate the argument that the OCF opacity – crash risk relation is built on the agency conflicts between shareholders and managers, we also examine whether this relation varies cross-sectionally. We predict and find that the relation between OCF opacity and crash risk is more pronounced when external monitoring is weak, information asymmetry is high, OCF importance is low, and cost of accruals management is high. However, we do not find the accruals opacity – crash risk relation to vary with external monitoring, information asymmetry, or OCF importance. The differential results for OCF opacity and accruals opacity may arise because market participants impose more scrutiny over managerial oppor-

[3] Using a sample of firms with downward OCF restatements, Lee (2012) and Zhang (2018) confirm the validity of the upward OCF management measure. However, since our OCF opacity measure is nondirectional in nature, we conduct the validation test by focusing on a more complete OCF restatement sample that includes both upward and downward OCF restatements.

tunistic behavior in the presence of OCF opacity than in the presence of accruals opacity. We also show that the relation between OCF opacity (accruals opacity) and crash risk is stronger when firms face higher (lower) cost of accruals management, suggesting that OCF management and accruals management can be used as substitutes in financial reporting manipulation.

Our paper provides several contributions to the literature. First, we extend the emerging research on OCF management. Lee (2012) demonstrates that firms manage reported OCF, especially when OCF is important to investors. However, there is little evidence on the consequences of OCF management with the exception of Zhang (2018), who documents that OCF management is associated with better firm ratings for firms with rating-based debt contracts. In contrast with Zhang (2018) that focuses on the impact of OCF management in debt markets, this study centers on the impact of OCF management in equity markets. Given that extreme events like crashes could have exceptional cumulative effects, the evidence on the impact of OCF management on crash risk is more likely to shed light on the true nature of OCF management. Our findings should be of interest to investors and regulators, because crash risk has a devastating impact on investor welfare. Our results also confirm that it is necessary for the regulators to provide further guidance on the preparation of the statement of cash flows (Deloitte & Touche LLP, 2017).

Second, this study has implications for research on earnings management and earnings quality. Although we do not systematically examine the relation between OCF management and accruals-based earnings management, we provide some evidence that OCF management and accruals management can be used as substitutes in financial statement manipulation. In this case, drawing inferences about accruals management without controlling for OCF management can be problematic. Moreover, in addition to many financial accounting textbooks and investment advisors, multiple prior studies use OCF as a benchmark for evaluating earnings quality (e.g., Dechow and Dichev, 2002). Our results also point to exercising caution in using OCF as a benchmark for assessing earnings quality.

Lastly, our study extends the recent research on the relation between opacity and stock price crash risk. Jin and Myers (2006) show that several cross-country indicators of firm opacity predict crash risk. Using U.S. data, Hutton et al. (2009) show that accruals-based earnings opacity predicts crash risk. Our study extends this line of research by documenting a new source of firm opacity that contributes to crash risk and by providing nuanced evidence on the underlying mechanisms that are not exactly the same for other sources of firm opacity. Hutton et al. (2009, 70) argue that "in contrast to earnings, cash flows do not represent the creation of value but rather the distribution of value and thus they are not the primary measure of firm performance." However, we confirm the importance of OCF as a performance indicator and document the adverse impact of OCF opacity on shareholder value as reflected in crash risk. Sunder (2010) states that risk of extreme losses due to crashes cannot be reduced via diversification but only via screening. In this sense, our research contributes to the literature by offering a potential screening tool.

Moreover, our evidence shows that OCF opacity plays a more important role in influencing crash risk than accruals opacity. The effect of OCF opacity is monotonic, while the effect of accruals opacity tails off after accruals opacity reaches a certain point. The impact of OCF opacity is also more severe than that of accruals opacity in an economic sense. The marginal effect of OCF opacity is 11.1 percent of the average crash risk, while the marginal effect of accruals opacity is 6.4 percent of the average crash risk. The more severe effect of OCF opacity could arise because investors give more credence to OCF and stay more skeptical about accruals due to higher degrees of subjectivity and manipulation associated with accruals (e.g., Aboody et al., 2005; Francis et al., 2005).[4] The more severe effect of OCF opacity could also arise because, compared to accruals opacity, OCF opacity can make it easier for managers to divert firm resources like cash flows, which Jin and Myers (2006) claim to be the resource that insiders always want to capture.

Our paper is related to but distinct in several ways from Khurana et al. (2018), who examine the impact of real earnings smoothing on stock price crash risk. First, OCF opacity stemming from OCF management is fundamentally different from real earnings smoothing. OCF management focuses on cash flows, which may or may not influence earnings, while real earnings smoothing focuses on earnings. For instance, when firms use classification shifting and timing strategy to manage cash flows, there is no influence on earnings. Also, OCF management targets a cash flow level (the first moment of cash flow distribution) and is more likely to be used as a short-term strategy, while real earnings smoothing targets earnings volatility (the second moment of earnings distribution) and tends to be used over several years as a long-term strategy. There could be different reasons and incentives underlying OCF management and real earnings smoothing.[5]

Second, OCF opacity and real earnings smoothing influence investors' perception differently. The reason is that OCF management influences investors' perception about the mean of the firm's economic performance, while real earnings smoothing influences investors' perception about the variance of the firm's economic performance. Third, OCF opacity and real earnings smoothing do not influence crash risk through the exactly same channels. Since cash flow is the most important and direct resource that managers want to capture privately, it is likely that resource diversion is a more important channel for OCF opacity to influence crash risk than for real earnings smoothing to do so. On the other hand, since real earnings smoothing

[4] Similarly, Rountree et al. (2008) show that investors value smooth earnings through cash flow components, rather than smooth earnings through accrual components.

[5] When the uncertainty over the variance of earnings distribution dominates the uncertainty over the mean of earnings distribution, firms are more likely to smooth earnings (Goel and Thakor, 2003). In contrast, when the uncertainty over the mean of cash flow distribution dominates the uncertainty over the variance of cash flow distribution, firms are more likely to use cash flow management.

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influences investors' ex ante perception of risk, it could enable managers to engage in ineffective risk management, which could in turn increase crash risk. But this channel does not apply to the impact of OCF opacity on crash risk.

Another related paper is Francis et al. (2016), who examine the relation between stock price crash risk and abnormal business operation to which one main contributor is real earnings management. Our paper differs from their study along multiple dimensions. First, OCF opacity originates from OCF management, which is conceptually different from real earnings management. OCF management targets OCF, while real earnings management targets earnings. For instance, firms can use OCF management to meet or beat analyst cash flow forecasts (e.g., Zhang, 2007; Lee, 2012). In contrast, firms use real earnings management to avoid reporting a loss or an earnings decline, or to meet or beat analyst earnings forecasts (e.g., Roychowdhury, 2006; Francis et al., 2016). Not surprisingly, the underlying incentives for OCF management and real earnings management can be different. As shown by Lee (2012), four firm characteristics are uniquely associated with stronger incentive to manage OCF, rather than earnings.[6]

Furthermore, investors could respond differently to OCF management relative to real earnings management. For instance, they may rely more on cash flow, but be more skeptical towards earnings. In this case, OCF management is more likely to mislead investors than real earnings management. Indeed, we find that when real earnings management stems from the component that has ambiguous or opposite effect on OCF (e.g., overproduction), it is largely not associated with crash risk. In contrast, when OCF opacity stems from OCF management that does not change earnings (i.e., timing and classification shifting), it is still positively associated with future crash risk. Also, we conduct mechanism tests, which Francis et al. (2016) do not, and show that OCF opacity affects crash risk through increasing the probability of very bad news release during the crash window and facilitating resource diversion, while real earnings management does not have similar effects except when it unambiguously influences earnings and OCF in the same direction. Nevertheless, we use various alternative measures to control for real earnings management and find that our results continue to hold.[7]

The rest of the paper is structured as follows. Section 2 reviews prior research and develops the hypothesis. In Section 3, we describe the sample, research design, and the variables used to test our hypothesis. Section 4 presents the major empirical findings. Section 5 provides additional analyses and robustness tests. Section 6 concludes the paper.

2. Related literature and hypothesis development

The importance of OCF to assessing firm performance is noteworthy for several reasons. First, standard textbooks on financial statement analysis recommend paying attention to OCF, because discretion permitted by accrual accounting can potentially obfuscate performance captured by earnings. Second, academic studies suggest that OCF possesses incremental information content beyond earnings, especially at the extreme earnings levels (e.g. Ali and Zarowin, 1992; Cheng et al., 1996). Moreover, recent studies (e.g., Dichev and Tang, 2008; Bushman et al., 2016) suggest that earnings quality has been declining over time; thus, it is possible that investors place a bigger emphasis on cash flows in recent years. Indeed, Barth et al. (2018) document that the value relevance of OCF is increasing over time, while that of earnings is decreasing. Their results, together with the evidence of an increasing number of cash flow forecasts issued by analysts and managers (e.g., DeFond and Hung, 2003; Wasley and Wu, 2006; Call et al., 2013), suggest that investors are paying more attention to OCF.

Given the importance of OCF to investors, firms have incentives to manage OCF, thereby increasing OCF opacity.[8] Lee (2012) cites several examples of firms managing reported OCF. For example, Dynegy used a special-purpose entity to masquerade a loan as operating cash flow, increasing its OCF by $300 million (this was offset by a non-cash loss; hence, it had no effect on earnings). There are also many cash flow restatement cases revealing that companies shift the classification of cash flow from financing and/or investing activities to cash flow from operating activities or vice versa.[9] Lee (2012) also provides large-sample evidence that firms manage OCF upward when they are near financial distress, have a long-term credit rating near the investment/noninvestment-grade cutoff, have analyst cash flow forecasts, or their stock returns have high associations with OCF.

While the above anecdotal and empirical evidence suggests that the emphasis placed on OCF by market participants induces OCF management, which in turn increases OCF opacity, there is little research on the consequences of OCF opacity. One exception is Zhang (2018), which documents that OCF management is associated with more favorable credit ratings for

[6] When it comes down to specific mechanisms of OCF management, some (e.g., managerial discretion over expenses) jointly influence earnings and hence could overlap with real earnings management. We discuss this situation in detail and address the confounding effect of real earnings management in Section 5.5.

[7] Although Francis et al. (2016) have one test that relates *DRO_CFO*, which is equivalent to our *OCFOPQ*, to future crash risk, they fail to find a significant coefficient on *DRO_CFO* (Table 4). We delve into the different results between their paper and our study and find that the difference arises from their failure to control for lagged *ROA*. In this sense, we complement Francis et al. (2016) by identifying a flaw in their research design and documenting different results after correcting this flaw.

[8] OCF management is conceptually different from real earnings management. The former targets OCF, while the latter targets earnings. As a result, the underlying incentives for OCF management and real earnings management can be different. Section 5.5 provides more details on the difference and the relation between OCF management and real earnings management.

[9] This evidence is analogous to other evidence showing that firms manage the presentation of items in financial reports even when total earnings are not affected at all (e.g., Bowen et al., 2002; McVay, 2006; Fan et al., 2010; Barua et al., 2010; Robinson, 2010). To illustrate this, Fan et al. (2010) document that managers inflate core earnings by shifting expenses from core expenses to noncore expenses.

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firms with ratings-based debt contracts. But to our knowledge, prior studies have not examined the consequences of OCF management and OCF opacity for equity markets.

Our paper fills the void by examining the relation between OCF opacity and future stock price crash risk. Our investigation is built upon the recent theories on the relation between firm opacity and stock price crash risk. Assuming that insiders always want to increase their private capture of their firm's cash flow, Jin and Myers (2006) argue that firm opacity allows insiders to reduce information flow to the market, hence increasing crash risk. Specifically, opacity enables insiders to expropriate more cash flow when good news arrives, because outsiders have imprecise perception of an opaque firm's cash flow. On the other hand, when bad news arrives, opacity reduces insiders' capture of cash flow since insiders still pay minimal dividends sufficient to forestall investors' intervention. After bad news has accumulated for too long, it is either too costly or impossible for insiders to continue withholding bad news; so they release all the bad news at once, triggering stock price crashes.

OCF opacity can increase stock price crash risk because it can provide masks for managers to withhold bad news and make it easier for them to divert corporate resources. As a very important source of firm-specific information, reported OCF informs market participants about firm performance. But when there is opacity involved with OCF, market participants face more difficulty in understanding the true firm performance, which in turn facilitates managers to conceal bad news. After bad news accumulates to a certain threshold, it is released all at once, which in turn causes stock price crashes (e.g., Jin and Myers, 2006; Kim et al., 2011a). In addition, OCF opacity can affect crash risk via its impact on insiders' resource diversion. The pursuit of private interest can drive insiders to increase their capture of cash flows (Dittmar and Mahrt-Smith, 2007; Bates et al., 2009) and other firm resources. OCF opacity provides insiders with a shield against outright cash flow diversion, as well as diversion of other resources, for extended periods, which, once detected, could cause stock price to drop sharply (Kim et al., 2011a).[10]

In summary, OCF opacity could increase future stock price crash risk by facilitating bad news hoarding and resource diversion for an extended period. This leads to the following hypothesis (stated in the alternative form):

H1. *OCF opacity is positively associated with future stock price crash risk, ceteris paribus.*

However, it is possible that OCF opacity does not have an adverse impact on future stock price crash risk. One reason is that OCF opacity may not be high enough to trigger a crash because OCF is not an estimate and firms face more constraints in managing OCF than in managing accruals. Another reason is that OCF management underlying OCF opacity, for instance, delaying payments to suppliers and accelerating collections from customers, may be a good corporate practice that increases cash flows and reduces firms' reliance on external capital, allowing firms to pursue value-increasing projects. Thus, the relation between OCF opacity and crash risk remains an empirical issue.

3. Sample and research design

3.1. Sample and data

The initial sample is drawn from the intersection of firms' weekly common stock return data from the Center for Research in Security Prices (CRSP) with annual financial data from Compustat. The sample selection begins with all firms that have data available on Compustat from 1988 through 2013. The time period starts at 1988, due to data availability of OCF from cash flow statements. We then impose the following selection criteria. First, we exclude firm-years with non-positive total assets and non-positive sales. Second, we require firms to be incorporated in the U.S. Third, the stock price at the fiscal year end should be at least one U.S. dollar. Also, following Kim et al. (2011a, b), we require that each firm has at least 26 weekly returns for a 12-month period ending three months after the fiscal year end. Finally, we exclude financial institutions (SIC: between 6000 and 6799) and firm-years with missing data for variables used in the empirical models described below. After applying these selection criteria, we obtain a final sample of 52,626 firm-year observations spanning the period 1992–2014.[11] To mitigate the influence of any extreme observations, we winsorize all continuous variables (except stock returns) that lie in the upper or lower 0.5 percent of the distribution.

3.2. Measuring firm-specific crash risk

Following Hutton et al. (2009) and Kim et al. (2011a), we use firm-specific weekly returns to compute two measures of firm-specific crash risk. Specifically, we first estimate the expanded market model below:

$$r_{i,\tau} = \alpha_i + \beta_{1i} r_{m,\tau-2} + \beta_{2i} r_{m,\tau-1} + \beta_{3i} r_{m,\tau} + \beta_{4i} r_{m,\tau+1} + \beta_{5i} r_{m,\tau+2} + \varepsilon_{i\tau}, \tag{1}$$

where $r_{i,\tau}$ is the return of firm i in week τ, and $r_{m,\tau}$ is the CRSP value-weighted market return in week τ. The lead and lag terms for the market index are used to allow for nonsynchronous trading (Dimson, 1979). We then define firm-specific weekly return, denoted by W, as the natural log of one plus the residual return from Eq. (1).

[10] For example, the revelation of Tyco management's resource diversion from 1997 through 2002 caused its stock price to decline from about $95 in early 2002 to $14 in mid-2002.

[11] We omit years 1988 through 1991 because all independent variables are lagged values, and the OCF opacity measure (explained in Section 3.3) requires three years of data. Our sample period ends at 2014 since our dependent variable is measured one year ahead of all the independent variables.

Our first measure of crash risk, *CRASH*, captures the crash likelihood of a firm in each year based on the presence of crash weeks. Following Hutton et al. (2009), we define crash weeks for a given firm-year as those weeks during which the firm experiences firm-specific weekly returns of 3.09 standard deviations below the mean firm-specific weekly returns over the entire fiscal year, with 3.09 chosen to generate a frequency of 0.1 percent in the normal distribution. We then set *CRASH* as an indicator variable that equals one for a firm-year that experiences one or more crash weeks during a fiscal year, and zero otherwise.[12]

Our second measure of crash risk, *NCSKEW*, is the skewness of residual returns. Following Chen et al. (2001), we calculate *NCSKEW* by taking the negative of the third moment of firm-specific weekly residual returns during a fiscal year, and then dividing it by the standard deviation of firm-specific weekly residual returns raised to the third power. Specifically, for each firm *i* in year *t*, we compute *NCSKEW* as follows:

$$NCSKEW_{it} = -\left[\mathrm{n}(\mathrm{n}-1)^{3/2}\sum \mathrm{W}^3{}_{it}\right] / \left[(\mathrm{n}-1)(\mathrm{n}-2)\left(\sum \mathrm{W}_{it}{}^2\right)^{3/2}\right], \tag{2}$$

where n is the number of firm-specific weekly returns during the fiscal year *t*, and other variables are as previously defined. Higher values of *NCSKEW* indicate that a firm's stock return distribution is more left-skewed, and hence its stock price is more prone to crash.

3.3. Measuring OCF opacity

We capture OCF opacity based on abnormal OCF (a measure for OCF management) estimated using a model developed by Dechow et al. (1998) and implemented by Lee (2012). Specifically, we use data for each industry-year with at least 10 observations to estimate the model below:[13]

$$OCF_t/TA_{t-1} = \lambda_0 + \lambda_1(1/TA_{t-1}) + \lambda_2(SALE_t/TA_{t-1}) + \lambda_3(\Delta SALE_t/TA_{t-1}) + \varepsilon, \tag{3}$$

where *OCF* is the operating cash flow for period *t*, *TA* is the total assets for period *t*-1, *SALE* is the sales during period *t*, and $\Delta SALE$ is the change in sales during period *t*. We then use the parameter estimates from Eq. (3) to measure abnormal OCF (*AOCF*) as the deviation from normal OCF (i.e. the residual from the estimation model). Finally, we use the moving sum of the absolute value of abnormal cash flow over the prior three years (*OCFOPQ*) to capture OCF opacity.[14] Higher values of *OCFOPQ* imply high OCF opacity.

The basic idea behind this measure is that firms with consistently large absolute values of abnormal OCF are more likely to be managing reported OCF, which increases firm opacity. The characteristic pattern of OCF management—large positive (negative) followed by large negative (positive) abnormal OCF due to the reversing nature of OCF management—would result in a high measure of OCF opacity, as both the positive and negative abnormal values would contribute to the moving sum of absolute abnormal OCF. Similar to Hutton et al. (2009), we therefore use a three-year moving sum of absolute abnormal OCF, rather than a one-year absolute value or a three-year moving sum of signed value, to identify OCF opacity. This approach is more likely to reflect an underlying policy of the firm to manage OCF because it can capture the multiyear effects of OCF management and positive and negative abnormal OCF over multiple years will not cancel each other out.

3.4. Research design

To test hypothesis H1, we estimate the following models that link our measures of crash risk in year *t* to our proxy for OCF opacity in year *t*-1, as well as a set of control variables used in previous research (e.g., Chen et al., 2001; Hutton et al., 2009):

$$\begin{aligned} CRASH_t = \alpha_0 + \beta_1 ACCOPQ_{t-1} + \beta_2 OCFOPQ_{t-1} + \sum_{q=3}^{m} \beta_q (q^{th} Control\ Variables_{t-1}) \\ + \sum Industry + \sum Year + \varepsilon_t \end{aligned} \tag{4}$$

$$\begin{aligned} NCSKEW_t = \alpha_0 + \beta_1 ACCOPQ_{t-1} + \beta_2 OCFOPQ_{t-1} + \sum_{q=3}^{m} \beta_q (q^{th} Control\ Variables_{t-1}) \\ + \sum Industry + \sum Year + \varepsilon_t \end{aligned} \tag{5}$$

[12] To further check robustness, we follow Kim et al. (2011a) to construct an alternative crash indicator variable by imposing 3.2 standard deviations to define the crash weeks. The untabulated result using this variable is similar to those reported in Table 3.

[13] The cross-sectional version of the Dechow et al. (1998) model is chosen over its time-series counterpart as used in Lee (2012) for the reasons similar to Zhang (2018).

[14] We acknowledge that abnormal OCF has been used as a measure of real earnings management by Roychowdhury (2006) and some follow-up studies. However, recent studies (e.g., Gunny 2010; Zang 2012; Zhang 2018) have been shifting away from using abnormal OCF to capture real earnings management due to the mixed implications of different real earnings management activities on OCF. That is, real activities manipulation impacts abnormal OCF in either direction and the net effect is ambiguous (Zang 2012). We follow recent studies to use abnormal OCF to identify OCF management and then capture OCF opacity based on OCF management.

where *ACCOPQ* is accruals-based earnings opacity; other variables are defined as previously or in the appendix. We include *ACCOPQ* to investigate whether OCF opacity has incremental power to predict future crash risk beyond accruals opacity. Eq. (4) is estimated using a logit regression, while Eq. (5) is estimated using an ordinary least squares (OLS) regression. Hypothesis H1 predicts that the more opaque the OCF, the more likely the future stock price is prone to crash. Hence, we expect $\beta_2 > 0$ under H1.

To separate the effect of OCF opacity on future crash risk from the effect of other variables, we include several control variables that affect crash risk. Similar to Chen et al. (2001), Hutton et al. (2009), and Kim et al. (2011a, b), we include the following control variables: detrended share turnover ($DTURN_{t-1}$), negative skewness of firm-specific weekly returns ($NCSKEW_{t-1}$), standard deviation of firm-specific weekly returns ($SIGMA_{t-1}$), firm-specific average weekly returns (RET_{t-1}), firm size ($SIZE_{t-1}$), market-to-book ratio (MB_{t-1}), leverage (LEV_{t-1}), and return on assets (ROA_{t-1}).

We include $DTURN_{t-1}$ as a control variable, because Chen et al. (2001) indicate that this variable reflects differences in shareholders' opinions, and is positively related to crash risk. $NCSKEW_{t-1}$ is meant to control for the possibility that firms with high negative return skewness in year *t*-1 are prone to have high negative return skewness in year *t*. $SIGMA_{t-1}$ is added to control for the possible impact of stock-return volatility in year *t*-1 on crash likelihood in year *t*. Chen et al. (2001) show that negative skewness is larger in stocks that have had positive stock returns over the prior 36 months. To control for this possibility, we include past one-year weekly returns (RET_{t-1}). We add $SIZE_{t-1}$ because negative return skewness is higher in stocks with greater market capitalization (Chen et al. 2001). We also include MB_{t-1} because both Chen et al. (2001) and Hutton et al. (2009) show that growth stocks are more prone to experience crashes in the future. LEV_{t-1} is added due to the documented negative association of leverage with future crash risk (Kim et al., 2011a, b). We also include ROA_{t-1} since Hutton et al. (2009) show that operating performance is related to crash risk.

4. Empirical results

4.1. Descriptive statistics

Table 1 presents descriptive statistics for several variables used in the empirical models. $CRASH_t$ ranges from zero to one with a mean value of 0.171, suggesting that 17.1 percent of our observations experience at least one crash event in a given fiscal year, similar to the result of Kim et al. (2011b). The mean value of *NCSKEW* is −0.010, which is larger than that reported by Kim et al. (2011a), suggesting that our sample of firm-years is more crash-prone than that used in Kim et al. (2011a).[15] The mean and median values of $OCFOPQ_{t-1}$ are 0.407 and 0.306, respectively. The mean and median values of $ACCOPQ_{t-1}$ are 0.249 and 0.187, respectively, similar to those reported by Hutton et al. (2009). The statistics of other variables are largely similar to those reported in prior studies.

Table 2 presents Pearson correlations.[16] $OCFOPQ_{t-1}$ is significantly and positively associated with both measures of crash risk, $CRASH_t$ and $NCSKEW_t$, providing preliminary support for H1. Similarly, $ACCOPQ_{t-1}$ exhibits a positive and significant correlation with $CRASH_t$ and $NCSKEW_t$. In addition, $OCFOPQ_{t-1}$ and $ACCOPQ_{t-1}$ are positively correlated as indicated by the correlation coefficient of 0.351, implying that OCF opacity may subsume accruals opacity in explaining future crash risk and vice versa.

4.2. Test of hypothesis H1

Table 3 presents the regression results for testing H1, which predicts that as OCF becomes more opaque, future crash risk increases. To alleviate the concern about potential cross-sectional and time-series dependence in the data, we report z-values (t-values) on an adjusted basis, using robust standard errors corrected for firm clustering (Cameron et al., 2011; Gow et al., 2010; Petersen, 2009).

Panel A of Table 3 uses *CRASH* as the dependent variable. In column (1), we first replicate Hutton et al. (2009) with our sample and find that crash risk increases with accruals opacity up to a certain level, and then declines with accruals opacity. We then examine the association between OCF opacity and crash risk without controlling for accruals opacity in column (2). As shown in this column, the coefficient on *OCFOPQ* is highly significant with an expected positive sign (0.145 with z = 3.58), consistent with H1.

In column (3), we add the non-linear form of accruals opacity as documented by Hutton et al. (2009) into the model in column (2) to check whether OCF opacity affects crash risk beyond accruals opacity. We continue to find a significantly positive coefficient on *OCFOPQ* (0.137 with z = 3.28). To examine whether there is a non-linear relation between OCF opacity and crash risk, we next add a quadratic term of *OCFOPQ*. As shown in column (4), the coefficient on *OCFOPQ* remains significantly positive, while the coefficient on $OCFOPQ^2$ is insignificant. To gain a better comparison of the effect of OCF opacity relative to that of accruals opacity, we also use the linear form for both OCF opacity and accruals opacity. Column (5) presents the results – the coefficient on *OCFOPQ* continues to be significantly positive, while the coefficient on *ACCOPQ* is insignificant. In summary, these results consistently indicate that as OCF becomes more opaque, future crash risk increases monotonically. In contrast, future crash risk first increases and then decreases as accruals opacity increases.

[15] The sample period of Kim et al. (2011a) is 1995–2008.

[16] The untabulated Spearman correlations are similar to the Pearson correlations with regard to the sign, the magnitude, and the level of significance.

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Table 1
Descriptive statistics.

	Mean	Std	5%	25%	Median	75%	95%
Dependent variables							
$CRASH_t$	0.171	0.376	0.000	0.000	0.000	0.000	1.000
$NCSKEW_t$	−0.010	0.840	−1.258	−0.452	−0.040	0.383	1.369
Test variable							
$OCFOPQ_{t-1}$	0.407	0.352	0.094	0.193	0.306	0.500	1.057
Control variables							
$ACCOPQ_{t-1}$	0.249	0.214	0.047	0.107	0.187	0.320	0.670
$DTURN_{t-1}$	0.004	0.101	−0.110	−0.020	0.000	0.023	0.129
$NCSKEW_{t-1}$	0.000	0.798	−1.184	−0.439	−0.037	0.376	1.321
$SIGMA_{t-1}$	0.058	0.031	0.021	0.035	0.051	0.073	0.116
RET_{t-1}	−0.215	0.335	−0.668	−0.265	−0.129	−0.062	−0.022
$SIZE_{t-1}$	5.889	2.025	2.769	4.391	5.777	7.284	9.442
MB_{t-1}	2.819	5.503	0.641	1.253	1.95	3.211	7.637
LEV_{t-1}	0.176	0.167	0.000	0.011	0.146	0.289	0.489
ROA_{t-1}	0.017	0.072	−0.116	0.010	0.031	0.048	0.085

The sample contains 52,626 firm-year observations for the period from 1992 to 2014. All variables are defined in Appendix.

Table 2
Correlations.

		A	B	C	D	E	F	G	H	I	J	K	L
$CRASH_t$	*A*	**1**											
$NCSKEW_t$	*B*	**0.608**	**1**										
$OCFOPQ_{t-1}$	*C*	**0.017**	**0.015**	**1**									
$ACCOPQ_{t-1}$	*D*	**0.016**	**0.010**	**0.351**	**1**								
$DTURN_{t-1}$	*E*	**0.023**	**0.038**	**0.015**	−0.006	**1**							
$NCSKEW_{t-1}$	*F*	**0.040**	**0.051**	−0.001	0.004	**0.025**	**1**						
$SIGMA_{t-1}$	*G*	**−0.027**	**−0.034**	**0.248**	**0.294**	**0.142**	**0.017**	**1**					
RET_{t-1}	*H*	**0.026**	**0.034**	**−0.182**	**−0.207**	**−0.143**	**0.017**	**−0.839**	**1**				
$SIZE_{t-1}$	*I*	**0.047**	**0.108**	**−0.269**	**−0.251**	0.005	**0.124**	**−0.479**	**0.31**	**1**			
MB_{t-1}	*J*	**0.016**	**0.040**	**0.165**	**0.064**	**0.056**	−0.000	**0.024**	**−0.027**	0.000	**1**		
LEV_{t-1}	*K*	**−0.018**	−0.008	**−0.207**	**−0.151**	**0.013**	0.001	**−0.120**	**0.078**	**0.341**	**−0.015**	**1**	
ROA_{t-1}	*L*	**0.025**	**0.047**	**−0.373**	**−0.194**	**0.050**	**0.012**	**−0.364**	**0.280**	**0.250**	**−0.065**	**0.083**	**1**

The sample period is from 1992 through 2014. Boldface text indicates significance at the 1% level or lower (two-sided). All variables are defined in Appendix.

To assess the economic significance of the results, we use the model specification in column (3) to estimate the marginal effect of OCF opacity on crash risk, which is the expected increase in crash probability as a function of OCF opacity, holding all other variables at their sample mean. The marginal effect of OCF opacity is 1.9 percent, which is 11.1 percent of the mean of crash risk. In contrast, the marginal effect of accruals opacity is 1.1 percent, which is 6.4 percent of the mean of crash risk. These results suggest that the association between OCF opacity and crash risk is economically significant as well; moreover, OCF opacity is more important in explaining crash risk than accruals opacity in terms of economic significance.

The coefficients on the control variables are generally consistent with the results of prior studies. First, the coefficient on $DTURN_{t-1}$ is significantly positive across columns (1) through (5). This finding is consistent with the evidence documented by Chen et al. (2001) and Kim et al. (2011a, b) that investor heterogeneity increases crash likelihood. Second, the coefficients of $NCSKEW_{t-1}$, RET_{t-1}, and $SIZE_{t-1}$ are all significantly positive, consistent with the results of Chen et al. (2001) and Kim et al. (2011a, b). Third, consistent with Hutton et al. (2009) and Kim et al. (2011a, b), we find negative coefficients for LEV_{t-1} but positive coefficients for $SIGMA_{t-1}$ and MB_{t-1}.[17] Finally, we find that the coefficient on ROA_{t-1} is significantly positive, consistent with Callen and Fang (2013) and Kim and Zhang (2016).[18]

[17] Since our crash risk measure, *CRASH*, identifies a negative firm-specific return in excess of 3.09 standard deviations as a crash event, it is likely that the scalar (i.e. the standard deviations of returns at the firm level) creates a mechanical relation between stock price variance (*SIGMA*) and stock price crash risk. To mitigate this concern, we control for $SIGMA_{t-1}$ throughout the analyses. But if the effect of $SIGMA_{t-1}$ persists in the next period, then there could be a mechanical negative correlation between $SIGMA_t$ and $CRASH_t$. To address this concern, we further control for $SIGMA_t$ and find that our inferences remain unchanged.

[18] The finding of a positive coefficient on lagged ROA is consistent with that of prior studies that use lagged ROA as a control variable (e.g., Callen and Fang, 2013; Kim et al., 2016). In contrast, some other studies use contemporaneous ROA (or ROE) as a control variable and document a negative coefficient. When we alternatively control for contemporaneous ROA, we find that our results still hold and the coefficient on ROA is significantly negative. The negative coefficient on the concurrent ROA is consistent with the intuition that when a firm's profitability is high, the stock price is less likely to crash in the concurrent period. The positive coefficient on the lagged ROA is consistent with the view that the market may have overvalued firms with high ROA in the last period, and thus stock prices of these firms are more likely to decline sharply in the current period.

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Table 3
The impact of OCF opacity on stock price crash risk (H1).

Panel A: Dependent variable - $CRASH_t$

	$CRASH_t$				
	(1)	(2)	(3)	(4)	(5)
$ACCOPQ_{t-1}$	0.538***		0.492***	0.478***	0.059
	(3.56)		(3.24)	(3.10)	(0.95)
$ACCOPQ^2_{t-1}$	−0.411***		−0.412***	−0.401***	
	(−3.01)		(−3.00)	(−2.89)	
$OCFOPQ_{t-1}$		**0.145*** **	**0.137*** **	**0.191** **	**0.136*** **
		(3.58)	**(3.28)**	**(2.28)**	**(3.27)**
$OCFOPQ^2_{t-1}$				**−0.029**	
				(−0.75)	
$DTURN_{t-1}$	0.622***	0.604***	0.612***	0.613***	0.607***
	(5.23)	(5.11)	(5.17)	(5.18)	(5.13)
$NCSKEW_{t-1}$	0.081***	0.080***	0.081***	0.080***	0.080***
	(5.18)	(5.11)	(5.14)	(5.13)	(5.11)
$SIGMA_{t-1}$	1.997	2.494	1.992	1.974	2.365
	(1.10)	(1.36)	(1.09)	(1.08)	(1.28)
RET_{t-1}	0.431*	0.473**	0.435*	0.435*	0.466**
	(1.89)	(2.04)	(1.89)	(1.89)	(2.01)
$SIZE_{t-1}$	0.059***	0.059***	0.062***	0.062***	0.060***
	(6.44)	(6.44)	(6.71)	(6.73)	(6.50)
MB_{t-1}	0.003*	0.002	0.002	0.002	0.002
	(1.68)	(1.42)	(1.39)	(1.37)	(1.42)
LEV_{t-1}	−0.138	−0.111	−0.111	−0.107	−0.112
	(−1.61)	(−1.29)	(−1.28)	(−1.24)	(−1.31)
ROA_{t-1}	1.042***	1.197***	1.179***	1.146***	1.191***
	(4.88)	(5.69)	(5.60)	(5.39)	(5.67)
Constant	−2.730***	−2.725***	−2.780***	−2.789***	−2.729***
	(−9.27)	(−9.33)	(−9.45)	(−9.48)	(−9.32)
N	52,626	52,626	52,626	52,626	52,626
Pseudo R^2	0.024	0.024	0.024	0.024	0.024

Panel B: Dependent variable - $NCSKEW_t$

	$NCSKEW_t$				
	(1)	(2)	(3)	(4)	(5)
$ACCOPQ_{t-1}$	0.191***		0.164***	0.163***	0.043**
	(4.18)		(3.55)	(3.47)	(2.05)
$ACCOPQ^2_{t-1}$	−0.116***		−0.116***	−0.115***	
	(−2.98)		(−2.93)	(−2.87)	
$OCFOPQ_{t-1}$		**0.085*** **	**0.079*** **	**0.083*** **	**0.079*** **
		(5.81)	**(5.25)**	**(2.84)**	**(5.25)**
$OCFOPQ^2_{t-1}$				**−0.002**	
				(-0.14)	
$DTURN_{t-1}$	0.249***	0.241***	0.245***	0.245***	0.243***
	(5.95)	(5.80)	(5.88)	(5.88)	(5.86)
$NCSKEW_{t-1}$	0.031***	0.030***	0.030***	0.030***	0.030***
	(5.79)	(5.73)	(5.74)	(5.73)	(5.72)
$SIGMA_{t-1}$	1.356***	1.484***	1.347***	1.345***	1.420***
	(3.10)	(3.31)	(3.07)	(3.06)	(3.19)
RET_{t-1}	0.107**	0.113**	0.107**	0.107**	0.111**
	(2.35)	(2.39)	(2.34)	(2.34)	(2.40)
$SIZE_{t-1}$	0.053***	0.053***	0.055***	0.055***	0.054***
	(19.23)	(19.28)	(19.61)	(19.60)	(19.47)
MB_{t-1}	0.005**	0.004**	0.004**	0.004**	0.004**
	(2.29)	(2.24)	(2.25)	(2.25)	(2.25)
LEV_{t-1}	−0.163***	−0.148***	−0.148***	−0.148***	−0.148***
	(−6.04)	(−5.45)	(−5.47)	(−5.46)	(−5.48)
ROA_{t-1}	0.517***	0.604***	0.598***	0.596***	0.603***
	(7.54)	(9.09)	(8.99)	(8.85)	(9.08)
Constant	−0.549***	−0.556***	−0.576***	−0.577***	−0.560***
	(−9.95)	(−10.18)	(−10.51)	(−10.49)	(−10.28)
N	52,626	52,626	52,626	52,626	52,626
Pseudo R^2	0.034	0.034	0.034	0.034	0.034

The sample period is from 1992 through 2014. Industry and year fixed effects are included in all regressions. The z-values (t-values) reported in parentheses are based on standard errors for the coefficient estimates that are heteroskedasticity-robust and clustered by firm. Significance levels are based on two-tailed tests: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All variables are defined in Appendix.

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Panel B of Table 3 uses *NCSKEW* as the alternative measure of crash risk, and the results are consistent with those reported in Panel A of Table 3. Overall, the results in Table 3 suggest that OCF opacity is positively associated with future crash risk beyond accruals capacity. Furthermore, this relation between OCF opacity and future crash risk is monotonic, while future crash risk first increases and then decreases at some point as accruals opacity increases. These results are robust to alternative model specifications.

4.3. The impact of OCF opacity on stock price crash risk: cross-sectional analyses

The positive relation between OCF opacity and crash risk under H1 is built on agency conflicts between shareholders and managers, which lead to managerial opportunistic behavior. To further corroborate this agency perspective, we conduct a series of cross-sectional analyses to examine how the OCF opacity–crash risk relation varies with external monitoring, information asymmetry, OCF importance, and cost of accruals management. Strong external monitoring is likely to constrain managerial opportunistic behavior (e.g., Callen and Fang, 2013; Bao et al., 2018) and hence alleviate the influence of OCF opacity on crash risk. An environment with low information asymmetry enables investors to detect hidden bad news and managerial resource diversion and mitigate the impact of OCF opacity on crash risk. When OCF is important, investors may impose more scrutiny over reported OCF and hence undo OCF management, thereby weakening the effect of OCF opacity on crash risk.[19] With high cost of accruals management, firms are likely to shift to OCF management, increasing the effect of OCF opacity on crash risk. Therefore, we predict the effect of OCF opacity on crash risk to be more pronounced when external monitoring is weak, information asymmetry is high, OCF importance is low, and cost of accruals management is high.

To test these cross-sectional predictions, we consider several external monitoring mechanisms: stability of institutional ownership (*IO_STB*) and analyst following (*NANAL*). Prior research suggests that stable institutional investors and financial analysts play a monitoring role (e.g., Callen and Fang, 2013; Yu, 2008).[20] Similar to LaFond and Watts (2008) and Doyle et al. (2007), we measure information asymmetry using bid-ask spread (*BASPREAD*) and firm complexity (*COMPLEX*).[21] We follow Lee (2012) to identify situations when OCF is important using the presence of analyst cash flow forecasts (*CFF*) and financial distress (*DISTRESS*). We follow Zang (2012) to capture cost of accruals management using balance sheet constraints (*NOA*) and whether the period is after the passage of the Sarbanes-Oxley Act (*SOX*).[22] The appendix provides more details on the variable definitions. If the partitioning variable is a continuous (dummy) variable, we then partition the sample on the basis of its industry-year median values (whether its value is one or zero) and re-estimate the model specified in column (3) of Table 3 for each subsample.

Table 4 presents the results of the cross-sectional analyses. Panel A shows that regardless of the choice of crash risk measures, the association between OCF opacity and crash risk is stronger for the subsample of firms with less stable institutional ownership. Also, regardless of crash risk measures used, OCF opacity has a stronger impact on crash risk when analyst coverage is lower (Panel B), bid-ask spread is higher (Panel C), firm complexity is higher (Panel D), analyst cash flow forecast is not present (Panel E), and financial distress is lower (Panel F). In contrast, Panels A through F do not provide consistent results that the association between accruals opacity and crash risk differs significantly across subsamples. Panels G through H show that the association between OCF opacity and crash risk is stronger when there are more balance sheet constraints and when the period is after SOX. In contrast, the association between accruals opacity and future crash risk is stronger when there are fewer balance sheet constraints and when the period is before SOX.

Overall, Table 4 suggests that when external monitoring is strong, information asymmetry is low, or OCF importance is high, the relation between OCF opacity and crash risk is mitigated. But we do not find consistent evidence that the relation between accruals opacity and crash risk is alleviated by external monitoring, information asymmetry, or OCF importance. The differential results for OCF opacity and accruals opacity could arise because market participants impose more scrutiny over managerial opportunistic behavior in the presence of OCF opacity than in the presence of accruals opacity. We also provide evidence that the association between OCF opacity (accruals opacity) and crash risk is stronger when firms face higher (lower) cost of accruals management, suggesting that OCF management and accruals management can be used as substitutes in financial reporting manipulation.

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[19] It is possible that when OCF is important, management have stronger incentive to manage OCF. But as long as investors impose more scrutiny over reported OCF in this case, the relation between OCF opacity and crash risk will be less pronounced when OCF is important.

[20] An alternative possibility is that financial analysts do not play an effective monitoring role for two reasons. First, financial analysts can create excessive pressure on managers and push them to engage in more opportunistic behavior. Second, financial analysts receive pressure from their employers, peers, and major clients of their brokerage houses, which could reduce their incentive to guard against managerial opportunistic behavior.

[21] The complex firms with diverse operations have substantial information asymmetry within the firm (e.g., Habib et al., 1997) or between insiders and outsiders (e.g., Gilson et al., 2001).

[22] To address the potential confounding effect of the financial crisis, in defining *SOX* we exclude observations with fiscal year beginning on/after December 1, 2007 and fiscal year ending before/on June 30, 2009.

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Table 4
OCF opacity and stock price crash risk: cross-sectional analyses.

Panel A: Institutional ownership stability (*IO_STB*)

	$CRASH_t$			$NCSKEW_t$		
	High IO_STB	*Low IO_STB*	*Subsample DIFF*	*High IO_STB*	*Low IO_STB*	*Subsample DIFF*
$ACCOPQ_{t-1}$	0.339	0.449**	[0.360]	0.065	0.201***	[0.089]
	(1.29)	(2.38)		(0.96)	(3.05)	
$ACCOPQ^2_{t-1}$	−0.308	−0.421***	[0.343]	−0.047	−0.158***	[0.113]
	(−1.18)	(−2.71)		(−0.78)	(−2.87)	
$OCFOPQ_{t-1}$	**0.067**	**0.195*** **	**[0.061]**	**0.055*** **	**0.099*** **	**[0.071]**
	(1.06)	**(3.50)**		**(3.01)**	**(4.29)**	
N	24,578	25,161		24,578	25,161	
R^2	0.028	0.025		0.033	0.034	

Panel B: Analyst coverage (*NANAL*)

	$CRASH_t$			$NCSKEW_t$		
	High NANAL	*Low NANAL*	*Subsample DIFF*	*High NANAL*	*Low NANAL*	*Subsample DIFF*
$ACCOPQ_{t-1}$	0.330	0.578**	[0.207]	0.103	0.182***	[0.227]
	(1.59)	(2.56)		(1.46)	(3.02)	
$ACCOPQ^2_{t-1}$	−0.275	−0.497**	[0.208]	−0.066	−0.138***	[0.239]
	(−1.45)	(−2.42)		(−1.05)	(−2.74)	
$OCFOPQ_{t-1}$	**0.073**	**0.192*** **	**[0.081]**	**0.048** **	**0.091*** **	**[0.080]**
	(1.18)	**(3.40)**		**(2.11)**	**(4.75)**	
N	25,085	25,253		25,085	25,253	
R^2	0.027	0.027		0.024	0.035	

Panel C: Bid-ask spread (*BASPREAD*)

	$CRASH_t$			$NCSKEW_t$		
	High BASPREAD	*Low BASPREAD*	*Subsample DIFF*	*High BASPREAD*	*Low BASPREAD*	*Subsample DIFF*
$ACCOPQ_{t-1}$	0.416**	0.646***	[0.234]	0.104*	0.245***	[0.108]
	(2.11)	(2.59)		(1.70)	(3.30)	
$ACCOPQ^2_{t-1}$	−0.342**	−0.604**	[0.190]	−0.068	−0.187***	[0.135]
	(−2.00)	(−2.51)		(−1.34)	(−2.81)	
$CFOOPQ_{t-1}$	**0.196*** **	**0.042**	**[0.038]**	**0.095*** **	**0.048** **	**[0.065]**
	(3.70)	**(0.59)**		**(4.62)**	**(2.06)**	
N	25,751	24,548		25,751	24,548	
R^2	0.028	0.025		0.036	0.033	

Panel D: Firm complexity (*COMPLEX*)

	$CRASH_t$			$NCSKEW_t$		
	High COMPLEX	*Low COMPLEX*	*Subsample DIFF*	*High COMPLEX*	*Low COMPLEX*	*Subsample DIFF*
$ACCOPQ_{t-1}$	0.684***	0.346**	[0.082]	0.206***	0.129*	[0.220]
	(3.32)	(2.54)		(3.23)	(1.86)	
$ACCOPQ^2_{t-1}$	−0.615***	−0.245*	[0.048]	−0.170***	−0.063	[0.116]
	(−3.36)	(−1.85)		(−3.20)	(−1.02)	
$CFOOPQ_{t-1}$	**0.167*** **	**0.065* **	**[0.060]**	**0.085*** **	**0.024**	**[0.018]**
	(3.04)	**(1.74)**		**(4.27)**	**(1.09)**	
N	24,480	24,598		24,480	24,598	
R^2	0.024	0.020		0.036	0.035	

Panel E: Presence of analyst cash flow forecast (*CFF*)

	$CRASH_t$			$NCSKEW_t$		
	Present	Not present	*Subsample DIFF*	Present	Not present	*Subsample DIFF*
$ACCOPQ_{t-1}$	0.046	0.359	[0.128]	0.162*	0.146*	[0.424]
	(0.17)	(1.49)		(1.78)	(1.75)	
$ACCOPQ^2_{t-1}$	−0.203	−0.318	[0.317]	−0.137*	−0.131**	[0.462]
	(−0.85)	(−1.57)		(−1.84)	(−1.99)	
$OCFOPQ_{t-1}$	**−0.030**	**0.359*** **	**[0.011]**	**−0.087**	**0.159*** **	**[0.002]**
	(−0.19)	**(2.59)**		**(−1.12)**	**(3.24)**	
N	13,183	15,480		13,183	15,480	
R^2	0.024	0.020		0.018	0.037	

(*continued on next page*)

Table 4 (*continued*)

Panel F: Financial distress (*DISTRESS*)

	$CRASH_t$			$NCSKEW_t$		
	High DISTRESS	*Low DISTRESS*	*Subsample DIFF*	*High DISTRESS*	*Low DISTRESS*	*Subsample DIFF*

Panel F: Financial distress (*DISTRESS*)

	$CRASH_t$			$NCSKEW_t$		
	High DISTRESS	*Low DISTRESS*	*Subsample DIFF*	*High DISTRESS*	*Low DISTRESS*	*Subsample DIFF*
$ACCOPQ_{t-1}$	0.289	0.669***	[0.115]	0.182**	0.145**	[0.357]
	(1.12)	(3.39)		(2.35)	(2.43)	
$ACCOPQ^2_{t-1}$	−0.321	−0.522***	[0.248]	−0.109	−0.112**	[0.488]
	(−1.27)	(−3.01)		(−1.55)	(−2.28)	
$\boldsymbol{CFOOPQ_{t-1}}$	**0.019**	**0.175*****	**[0.049]**	**0.056****	**0.069*****	**[0.337]**
	(0.24)	**(3.37)**		**(2.01)**	**(3.63)**	
N	23,672	26,651		23,672	26,651	
R^2	0.022	0.033		0.037	0.040	

Panel G: Balance sheet constraint (*NOA*)

	$CRASH_t$			$NCSKEW_t$		
	High NOA	*Low NOA*	*Subsample DIFF*	*High NOA*	*Low NOA*	*Subsample DIFF*
$ACCOPQ_{t-1}$	0.081	0.845***	[0.007]	0.030	0.267***	[0.010]
	(0.37)	(4.02)		(0.45)	(4.09)	
$ACCOPQ^2_{t-1}$	−0.129	−0.697***	[0.020]	−0.019	−0.186***	[0.035]
	(−0.64)	(−3.80)		(−0.34)	(−3.37)	
$\boldsymbol{OCFOPQ_{t-1}}$	**0.246*****	**0.092**	**[0.039]**	**0.129*****	**0.049****	**[0.005]**
	(3.60)	**(1.64)**		**(5.45)**	**(2.44)**	
N	25,115	24,964		25,115	24,964	
R^2	0.025	0.021		0.036	0.034	

Panel H: Pre- vs. post-SOX period (*SOX*)

	$CRASH_t$			$NCSKEW_t$		
	Pre-SOX	Post-SOX	*Subsample DIFF*	Pre-SOX	Post-SOX	*Subsample DIFF*
$ACCOPQ_{t-1}$	0.937***	0.145	[0.006]	0.172***	0.114	[0.284]
	(3.85)	(0.70)		(2.77)	(1.58)	
$ACCOPQ^2_{t-1}$	−0.690***	−0.218	[0.052]	−0.089	−0.097*	[0.464]
	(−2.94)	(−1.24)		(−1.58)	(−1.67)	
$\boldsymbol{OCFOPQ_{t-1}}$	**0.012**	**0.203*****	**[0.013]**	**0.035****	**0.098*****	**[0.018]**
	(0.18)	**(3.59)**		**(1.98)**	**(4.00)**	
N	28,824	22,139		28,824	22,139	
R^2	0.0250	0.0177		0.0538	0.0192	

The sample period is from 1992 through 2014. Industry and year fixed effects are included in all regressions. The z-values (t-values) reported in parentheses are based on standard errors for the coefficient estimates that are heteroskedasticity-robust and clustered by firm. Significance levels are based on two-tailed tests: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. For comparison of coefficients across subsamples, the p-values reported in brackets are based on one-tailed tests. To save space, all the control variables (see Table 3) are suppressed. All variables are defined in Appendix.

5. Additional analyses and robustness tests

5.1. Validity of OCF opacity measure

5.1.1. Using OCF restatement sample to validate OCF opacity measure

The inferences based on our results so far are conditional on the validity of our OCF opacity measure. Although Lee (2012) and Zhang (2018) have conducted multiple tests to validate abnormal OCF as a proxy for OCF management, we wish to further validate our OCF opacity measure by showing that firms with high levels of OCF opacity are less transparent. To do so, we examine the relation between *OCFOPQ* and the frequency of OCF restatements.[23]

Specifically, we obtain a sample of OCF restatements following the procedures similar to those used by Lee (2012). We first identify restatements between 1999 and 2013 that stem from cash flow statement classification errors as reported in Audit Analytics. We then exclude OCF restatements that are unrelated to OCF and are accompanied by earnings restatements. This yields a sample of 820 firm-year observations with classification errors that either overstated or understated OCF in their original cash flow statements. We are able to merge 271 out of 820 firm-year observations with our sample used to test H1. Since restating firms are likely to differ from non-restating firms in their size and growth opportunities, we follow

[23] Hutton et al. (2009) use a similar approach to validate their accruals opacity measure.

Lee's (2012) matching procedure and pair each restating observation with one non-restating observation according to firm size and market-to-book ratio.[24] We then use the matched sample to examine the relation between *OCFOPQ* and the frequency of OCF restatements by comparing the percentage of OCF restatements for firm-years with high and low OCF opacity and the mean of OCF opacity for firm-years with and without OCF restatements.

Panel A of Table 5 presents the results. It first shows that the percentage of OCF restatements increases with OCF opacity, with 46.94 percent for low OCF opacity firms compared with 52.86 percent for high OCF opacity firms. In addition, we compare the mean of OCF opacity for firm-years with and without OCF restatements. We find that for the OCF restatement group, the mean value of OCF opacity (0.346) is significantly bigger than that for the no-restatement group (0.313). Overall, our evidence suggests that OCF opacity is positively associated with the frequency of OCF restatements, supporting our interpretation of the OCF opacity measure.

5.1.2. Test of persistence of cash flows

With OCF management, the observed OCF has two components—the managed component and the unmanaged component. The former is likely to be less persistent than the latter because it does not recur. To test this conjecture, we compare the coefficient on the abnormal OCF with that on the normal OCF estimated from the model below:

$$OCF_{t+1} = \beta_0 + \beta_1 NOCF_t + \beta_2 AOCF_t + \beta_3 ACC_t + \varepsilon_{t+1}, \tag{6}$$

where *AOCF* and *NOCF* are abnormal and normal cash flows, respectively, estimated using Eq. (3) in Section 3. The model includes *ACC* since accruals explain future cash flows over and above current cash flows (Dechow et al., 1998). Panel B of Table 5 presents the results. It shows a smaller coefficient on *AOCF* than that on *NOCF*, in line with the prediction that abnormal OCF is less persistent than normal OCF.

5.2. Alternative measures of OCF opacity

Next, we test the robustness of our baseline results using two alternative measures of OCF opacity. The first alternative measure is the performance-adjusted OCF opacity (*OCFOPQ2*), which aims to address the potential concern that our measure for OCF opacity may capture some effects of real cash flow shocks.[25] Specifically, we follow Kothari et al. (2005)'s performance-adjustment approach and control for current firm performance in estimating Eq. (3) using net income scaled by total assets. The second alternative measure is the standard deviation of abnormal OCF over a rolling window of the prior three years (*OCFOPQ3*). This measure can address the concern that consistently large abnormal OCF may have relatively good quality due to its greater persistence, given that its standard deviation is still low. Higher values of *OCFOPQ2* or *OCFOPQ3* imply high OCF opacity.

Table 6 presents the results of estimating Eqs. (4) and (5) after replacing *OCFOPQ* with one of these alternative measures of OCF opacity. Columns (1) and (3) use performance-adjusted OCF opacity (*OCFOPQ2*) as the test variable. For comparison, we also use performance-adjusted accruals opacity (*ACCOPQ2*). In both columns, as OCF opacity increases, future crash risk increases.[26] Columns (2) and (4) use the volatility of abnormal OCF (*OCFOPQ3*) as the test variable and show similar results. We also run all the tests in columns (2) and (4) using the standard deviation of performance-adjusted abnormal OCF over the prior three years and continue to find similar results (untabulated). Collectively, Table 6 shows that our baseline results are insensitive to alternative measures of OCF opacity, in particular, they are unlikely to be driven by real cash flow shocks, providing further support for H1.[27]

5.3. OCF opacity stemming from OCF management that does not change earnings

Although OCF opacity stems from either OCF management that changes earnings or OCF management that does not, so far we have not distinguished between OCF opacity stemming from different types of OCF management. In this subsection, we investigate the impact of OCF opacity attributed to two mechanisms of OCF management that do not change earnings, timing

[24] Specifically, we first match the firms in the restatement sample to the non-restating firms in our sample used to test H1 by industry and year. Second, we match each sample firm to a set of control firms with total assets between 90 percent and 110 percent of that of the sample firm. Finally, from this subsample of firms, we pair each sample firm with the control firm that has the closest market-to-book ratio. This procedure yields 562 firm-year observations with 271 observations each for the restating sample and the non-restating sample.

[25] However, the relation between a real cash flow shock and future crash risk is not clear. One reason is that the market can over- or underreact to a real cash flow shock, depending on the market's perception of the cash flow shock. If the market overreacts (underreacts) to the real cash flow shock, then it is likely (less likely) to observe a severe stock price drop associated with the unexpectedly low OCF stemming from a real cash flow shock. Even if a real cash flow shock does contribute to the severe stock price decline, this relation will be more likely to be concurrent, rather than the lead-lag relation as specified in this study. Nonetheless, the measurement of OCF opacity based on abnormal OCF over the prior three years, combined with year fixed effects, can mitigate this concern if it exists, because real cash flow shocks are one-time events and should be year-specific.

[26] As sensitivity tests, we conduct performance adjustment using three alternative measures of firm performance respectively, including operating income before depreciation, operating income after depreciation, and income before extraordinary items. Using these alternative performance-adjusted OCF opacity measures one at a time, we consistently find that the coefficients on OCF opacity are similar to those reported in Table 6 (untabulated).

[27] Given that financial crisis can bring real cash flow shocks to firms, we also test the impact of OCF opacity on crash risk for the crisis and non-crisis period separately using subsample analysis. The untabulated results show that OCF opacity has a significant and positive impact on crash risk during both the crisis and non-crisis periods. These results provide further support that it is unlikely that our main results are driven by real cash flow shocks.

Table 5
Validity tests for the OCF opacity measure.

Panel A: Using OCF restatement data to validate the OCF opacity measure		
	Percentage of OCF restatements	Mean of OCF opacity
OCF opacity group		
Low OCF opacity (271 observations)	46.94%	
High OCF opacity (271 observations)	52.86%	
t-stat (p-value) for equality across groups	−1.38 (0.085)	
OCF restatement group		
No OCF restatement (271 observations)		0.313
OCF restatements (271 observations)		0.346
t-stat (p-stat) for equality across groups		−1.41 (0.080)

Panel B: Persistence of abnormal OCF vs. normal OCF	
	OCF_t
$NOCF_{t-1}$	0.731***
	(68.20)
$AOCF_{t-1}$	0.687***
	(67.07)
ACC_{t-1}	0.207***
	(18.32)
Constant	0.023***
	(4.30)
N	52,626
Adjusted R^2	0.489
$NOCF_{t-1} \neq AOCF_{t-1}$	F=57.58 P<0.001

Panel A shows the relation between OCF opacity and the frequency of OCF restatements. Using the sample period from 1999 to 2013, we compare the percentage of OCF restatements for firm-years with high and low OCF opacity, as well as the mean of OCF opacity for firm-years with and without OCF restatements. Panel B presents the results of regressing future operating cash flow on the normal and abnormal components of current operating cash flow to test whether abnormal OCF (*AOCF*) is less persistent than normal OCF (*NOCF*). The sample period is from 1992 through 2014. Industry and year fixed effects are included in the regression. The t-values reported in parentheses are based on standard errors for the coefficient estimates that are heteroskedasticity-robust and clustered by firm. Significance levels are based on two-tailed tests: * p < 0.10, ** p < 0.05, *** p < 0.01. All variables are defined in Appendix.

Table 6
OCF opacity and stock price crash risk: alternative measures of OCF opacity.

	$CRASH_t$		$NCSKEW_t$	
	$OCFOPQ2_t$ (1)	$OCFOPQ3_t$ (2)	*OCFOPQ2* (3)	*OCFOPQ3* (4)
$ACCOPQ_{t-1}$	0.511***	0.477***	0.140***	0.145***
	(2.93)	(3.01)	(2.66)	(3.06)
$ACCOPQ^2_{t-1}$	−0.467***	−0.424***	−0.088*	−0.106**
	(−2.74)	(−2.88)	(−1.75)	(−2.57)
$\boldsymbol{OCFOPQ_{t-1}}$	**0.135****	**0.559*****	**0.057*****	**0.256*****
	(2.26)	**(3.47)**	**(2.64)**	**(4.20)**
N	49,775	52,285	49,775	52,285
R^2	0.024	0.024	0.033	0.034

The sample period is from 1992 through 2014. Industry and year fixed effects are included in all regressions. The z-values (t-values) reported in parentheses are based on standard errors for the coefficient estimates that are heteroskedasticity-robust and clustered by firm. Significance levels are based on two-tailed tests: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. To save space, all the control variables (see Table 3) are suppressed. All variables are defined in Appendix.

and classification shifting. The benefit of these analyses is that they allow us to identify the effect of OCF opacity not obscured by earnings opacity.

To measure OCF opacity stemming from timing, we use the absolute value of the change in cash conversion cycle (days in accounts receivable plus days in inventory minus days in accounts payable) from the fourth quarter in year *t-1* to the first quarter in year t ($|\Delta CC|$).[28] Panel A of Table 7 presents the results for alternative specifications of Eqs. (4) and (5) using $|\Delta CC|$ as the test variable. We find that $|\Delta CC|$ is significantly and positively related to future crash risk.

[28] Managers can reduce days in inventory by not buying additional inventory. This can lead to a decrease in cost of goods sold and an increase in earnings. To address this issue, we use an alternative measure of the change in cash conversion cycle by excluding days in inventory in our calculations. The untabulated results using this measure yield inferences similar to those reported in the paper.

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We also use the OCF restatement sample from Section 5.1 to test whether OCF opacity due to classification shifting contributes to crash risk. Panel B of Table 7 shows that irrespective of the measures for crash risk, firms with OCF restatements due to classification errors have higher crash risk in the future than firms without OCF restatements.[29] Moreover, we fail to find a significant effect of accruals opacity on crash risk using the OCF restatement sample. Overall, the results in Table 7 provide support that OCF opacity resulting from OCF management increases future crash risk, holding earnings constant.

5.4. Possible mechanisms

Our evidence thus far demonstrates that OCF opacity leads to higher crash risk. To gain a better understanding of how OCF opacity influences crash risk, we adopt the research design in prior research (e.g., He and Tian, 2013; Lang et al., 2012; Khurana et al., 2018) to explore possible mechanisms through which OCF opacity affects crash risk. In particular, we center on both an informational mechanism and a real mechanism. For the informational mechanism, we examine whether bad news hoarding is one underlying mechanism through which OCF opacity influences crash risk. Empirically, similar to Chang et al. (2017), we test the existence of very bad news release in the crash period since the hidden bad news will ultimately be released in the crash period and it is ex ante unclear how long a firm withholds bad news. For the real mechanism, we examine whether resource diversion is one economic mechanism through which OCF opacity affects crash risk.

5.4.1. Informational mechanism: Bad news release

Our first mechanism test revolves around the link between OCF opacity and the likelihood of very bad news releases during the crash period. As discussed in Section 2, OCF opacity facilitates bad news hoarding for an extended period; at some point when it becomes either too costly or impossible to withhold bad news, all the hitherto accumulated bad news will be released at once, leading to stock price crashes. To examine this mechanism empirically, we follow Chang et al. (2017) and construct an indicator variable for very bad news release in the crash period, *SURP_UOCF* (see Appendix for more details).[30]

Column (1) of Table 8 presents the results of this analysis. The coefficient on *OCFOPQ* is positive and significant, indicating that OCF opacity increases the likelihood of very bad news release in the subsequent period during which stock price crashes occur. The evidence confirms that OCF opacity results in subsequent release of very bad news, which triggers stock price crashes. The coefficient on *ACCOPQ* is positive and significant, while the coefficient on $ACCOPQ^2$ is significantly negative, suggesting that accruals opacity initially increases the subsequent probability of very bad news release, but the effect dies down later on. In short, these results point to bad news release as a plausible mechanism underlying the relation between OCF (accruals) opacity and stock price crash risk.

5.4.2. Real mechanism: Resource diversion

Resource diversion can be another mechanism through which OCF opacity affects crash risk. As discussed in Section 2, OCF opacity can enable insiders to capture firm resources, cash flow in particular, for an extended period by providing a shield against their rent-seeking activities, which, once revealed, could cause stock prices to drop dramatically (Kim et al., 2011a). Therefore, OCF opacity can affect crash risk through its impact on resource diversion. To test this mechanism, we follow Atwood and Lewellen (2019) and Khurana et al. (2018) to measure resource diversion using shareholder payout (*DIV*) – the scaled total cash dividends paid that is set to zero when missing. Less shareholder payout implies more resource diversion.

Column (2) of Table 8 presents the results. The coefficient on *OCFOPQ* is significantly negative, in line with the argument that OCF opacity reduces payout to shareholders and hence enables managerial resource diversion. When the resource diversion is suddenly revealed to the public, the stock price plunges (Kim et al., 2011a). In contrast, the coefficient on *ACCOPQ* is negative and significant, while the coefficient on $ACCOPQ^2$ is insignificant. This result fails to provide support that resource diversion is a mechanism underlying the observed quadratic relation between accruals opacity and crash risk.

To further check robustness, we repeat the analysis in column (2) using an alternative specification of shareholder payout (*DIV_POS*) – the scaled total cash dividends paid that is not set to zero when missing. As shown in column (3), the coefficient on *OCFOPQ* remains significantly negative, but the coefficient on *ACCOPQ* is significantly positive and the coefficient on $ACCOPQ^2$ is significantly negative. These results indicate that OCF opacity facilitates managerial resource diversion, while accruals opacity initially corresponds to lower resource diversion, but the effect dies down later. Put together, the evidence suggests that resource diversion is an economic mechanism through which OCF opacity increases crash risk, but it is not a plausible mechanism underlying the quadratic relation between accruals opacity and crash risk. In other words, OCF opacity and accruals opacity do not influence crash risk through the exact same mechanisms.[31]

[29] We also conduct the univariate tests on the relation between OCF restatements and crash risk. The untabulated results show that crash risk is higher for firms with OCF restatements than for firms without OCF restatements.

[30] Note that we identify bad news release based on cash flow information due to our focus of OCF opacity and Jin and Myers (2006)'s emphasis of cash flow. But we build our theoretical framework following prior studies on firm opacity and stock price crash risk (e.g., Jin and Myers, 2006; Hutton et al., 2009) and use the construct of general bad news, i.e., negative information. As a robustness check, we identify bad news release through earnings information and repeat the analysis in column (1) of Table 8. The untabulated results are similar to those reported in column (1) of Table 8.

[31] As an alternative way to test the mechanisms, we use path analyses to test whether the existence of very bad news release in the crash period and resource diversion are mediating paths underlying the relation between OCF opacity and crash risk. The untabulated results confirm these two meditating paths.

Table 7
OCF opacity and stock price crash risk: OCF opacity stemming from OCF management that does not change earnings.

Panel A: OCF opacity stemming from timing strategy

	$CRASH_t$ (1)	$NCSKEW_t$ (2)
$ACCOPQ_{t-1}$	0.488***	0.221***
	(2.87)	(4.24)
$ACCOPQ^2_{t-1}$	−0.381**	−0.128***
	(−2.45)	(−2.73)
$\|\Delta CC_{t-1}\|$	**0.002****	**0.0003***
	(2.01)	**(1.73)**
N	47,342	47,342
R^2	0.026	0.034

Panel B: OCF opacity stemming from classification shifting

	$CRASH_t$ (1)	$NCSKEW_t$ (2)
$ACCOPQ_{t-1}$	1.732	0.289
	(0.96)	(0.51)
$ACCOPQ^2_{t-1}$	−1.642	−0.660
	(−1.04)	(−1.28)
$OCF\_RESTATE_{t-1}$	**0.531***	**0.162***
.	**(1.89)**	**(1.87)**
N	542	542
R^2	0.141	0.090

Panel A presents the regression results for the relation between OCF opacity stemming from timing strategy and crash risk, while Panel B presents the regression results for the relation between OCF opacity stemming from classification shifting and crash risk. The sample period for Panel A is from 1992 through 2014, while the sample period for Panel B is from 1999 to 2013. Industry and year fixed effects are included in all regressions. The z-values (t-values) reported in parentheses are based on standard errors for the coefficient estimates that are heteroskedasticity-robust and clustered by firm. Significance levels are based on two-tailed tests: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. To save space, all the control variables (see Table 3) are suppressed. All variables are defined in Appendix.

5.5. OCF opacity, real earnings management, and stock price crash risk

OCF opacity stems from OCF management, which is conceptually different from real earnings management. The former targets OCF, while the latter targets earnings; many transactions and accounting entries influence one and not the other (Standard and Poor's, 2008). Not surprisingly, the underlying incentives for OCF management and real earnings management can be different. But some mechanisms of OCF management jointly influence earnings and could overlap with real earnings management.[32] Specifically, as discussed above, we classify the mechanisms of OCF management into two types. One type only affects OCF but does not affect earnings (e.g., timing and classification shifting) and hence has no overlap with real earnings management. This type of OCF management could face different constraints and have different timing relative to real earnings management.[33] The other type of OCF management influences both OCF and earnings in a similar manner (e.g., managerial discretion over expenses) and thereby overlaps with real earnings management. Fig. 1 illustrates the relation between OCF management and real earnings management. To better understand whether OCF opacity influences crash risk just as real earnings management does and beyond real earnings management, we conduct two sets of analyses below.

The first set of analyses examines whether OCF opacity and real earnings management both affect crash risk through the mechanisms of bad news release and resource diversion. Panel A of Table 9 reports the results. The first two columns present the results of the bad news release mechanism. Using aggregate real earnings management measure estimated following Francis et al. (2016) (*ABAGGREM*), column (1) shows that the coefficient on *OCFOPQ* remains significantly positive, while the coefficient on *ABAGGREM* is insignificant. In column (2), we use disaggregated real earnings management measures (*ABPROD* and *ABEXP*) and repeat the analysis in column (1). The coefficient on *OCFOPQ* continues to be significantly positive, while the coefficient on *ABPROD* is insignificant and the coefficient on *ABEXP* is significantly positive. These results suggest that OCF opacity influences crash risk through the mechanism of bad news release, while real earnings management does not influence crash risk through the similar mechanism except for the real earnings management component that overlaps with OCF management.

[32] On the other side, some real earnings management activities influence earnings, but do not influence operating cash flow (e.g., time asset sales) or even if they do, the impact is either ambiguous or opposite (e.g., overproduction). We consider these real earnings management activities do not overlap with OCF management.

[33] When a firm manages OCF through classification shifting, it is constrained by auditors and regulators, while real earnings management is not subject to the similar constraint. As for the timing, when firms manage OCF through timing strategy, this typically occurs in the last quarter of the fiscal year. When firms manage OCF through classification shifting, they can do so even after the fiscal year end. In contrast, real earnings management must occur during the fiscal year and is realized by the fiscal year end (Zang, 2012).

Table 8
OCF opacity and stock price crash risk: possible mechanisms.

	$SURP\_UOCF_t$	DIV_{t-1}	DIV_POS_{t-1}
	(1)	(2)	(3)
$ACCOPQ_{t-1}$	3.546***	−0.001**	0.006***
	(10.23)	(−2.03)	(2.90)
$ACCOPQ^2_{t-1}$	−1.933***	0.000	−0.004**
	(−6.41)	(1.12)	(−2.34)
$OCFOPQ_{t-1}$	**0.243*** **	**−0.001*** **	**−0.004*** **
	(3.33)	**(−4.06)**	**(−5.36)**
$DTURN_{t-1}$	0.200	0.001**	−0.004
	(1.16)	(2.41)	(−1.37)
$NCSKEW_{t-1}$	−0.065**	−0.000	−0.001***
	(−2.16)	(−1.61)	(−3.39)
$SIGMA_{t-1}$	17.183***	−0.042***	0.026
	(5.89)	(−4.57)	(0.83)
RET_{t-1}	0.978***	−0.002**	−0.006
	(3.54)	(−2.08)	(−1.13)
$SIZE_{t-1}$	−0.256***	0.000*	−0.001***
	(−11.61)	(1.95)	(−9.13)
MB_{t-1}	0.005***	−0.000	−0.000
	(2.66)	(−1.41)	(−0.47)
LEV_{t-1}	−2.282***	−0.000	0.002
	(−10.20)	(−0.65)	(1.33)
ROA_{t-1}	2.574***	0.000	−0.024***
	(7.53)	(0.02)	(−3.50)
Constant	−2.934***	0.004**	0.016***
	(−5.24)	(2.18)	(7.24)
N	52,239	52,626	18,494
R^2	0.133	0.074	0.074

The sample period is from 1992 through 2014. Industry and year fixed effects are included in all regressions. The z-values (t-values) reported in parentheses are based on standard errors for the coefficient estimates that are heteroskedasticity-robust and clustered by firm. Significance levels are based on two-tailed tests: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All variables are defined in Appendix.

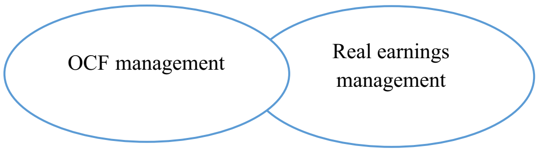

Fig. 1. Relation between OCF management and real earnings management.

Columns (3) and (4) present the results of the resource diversion mechanism. Using aggregate real earnings management measure (*ABAGGREM*), column (3) shows that the coefficient on *OCFOPQ* is significantly negative, while the coefficient on *ABAGGREM* is negative, but barely significant. In column (4), we use disaggregated real earnings management measures (*ABPROD* and *ABEXP*) and repeat the analysis in column (3). The coefficient on *OCFOPQ* continues to be significantly negative, while the coefficient on *ABPROD* is insignificant and the coefficient on *ABEXP* is significantly negative. These results suggest that OCF opacity influences crash risk through resource diversion, while real earnings management does not influence crash risk through the similar mechanism except for the real earnings management component that overlaps with OCF management. Together, Panel A of Table 9 suggests that OCF opacity does not influence crash risk through the same mechanisms as real earnings management does except for its component that overlaps with OCF management.

The second set of analyses examines whether the effect of OCF opacity on crash risk holds after controlling for real earnings management. Panel B of Table 9 reports the results. Column (1) uses aggregate real earnings management measure (*ABAGGREM*) and shows that the coefficient on *OCFOPQ* remains significantly positive and the coefficient on *ABAGGREM* is significantly positive as well. In column (2), we use disaggregated real earnings management measures (*ABPROD* and *ABEXP*) and repeat the analysis in column (1). The coefficient on *OCFOPQ* continues to be significantly positive, while the coefficient on *ABPROD* is insignificant and the coefficient on *ABEXP* is significantly positive. Columns (3) and (4) repeat the analyses in columns (1) and (2) using *NCSKEW* as the crash risk measure and show largely similar results. Overall, Panel B of Table 9 suggests that OCF opacity influences crash risk beyond real earnings management and the effect of real earnings management on crash risk largely stems from its component that overlaps with OCF management.

Table 9
OCF opacity, real earnings management, and stock price crash risk.

Panel A: The impact of OCF opacity and real earnings management on bad-news release and resource diversion				
	$SURP\_UOCF_t$		DIV_{t-1}	
	(1)	(2)	(3)	(4)
$ACCOPQ_{t-1}$	3.089***	3.088***	−0.002**	−0.001**
	(11.00)	(11.05)	(−2.45)	(−2.20)
$ACCOPQ^2_{t-1}$	−1.631***	−1.629***	0.001	0.001
	(−6.89)	(−6.93)	(1.51)	(1.39)
$OCFOPQ_{t-1}$	**0.243*** **	**0.227*** **	**−0.001*** **	**−0.001*** **
	(3.91)	**(3.61)**	**(−4.19)**	**(−3.16)**
$ABAGGREM_{t-1}$	−0.041		−0.0003*	
	(−0.87)		(−1.82)	
$ABPROD_{t-1}$		−0.030		0.0002
		(−0.46)		(1.53)
$ABEXP_{t-1}$		0.121**		−0.0005***
		(2.57)		(−5.47)
N	52,239	52,239	52,626	52,626
R^2	0.153	0.154	0.0677	0.0682
Panel B: The impact of OCF opacity on crash risk after controlling for real earnings management				
	$CRASH_t$		$NCSKEW_t$	
	(1)	(2)	(3)	(4)
$ACCOPQ_{t-1}$	0.497***	0.478***	0.165***	0.161***
	(3.28)	(3.15)	(3.56)	(3.48)
$ACCOPQ^2_{t-1}$	−0.413***	−0.403***	−0.116***	−0.115***
	(−3.00)	(−2.94)	(−2.93)	(−2.91)
$OCFOPQ_{t-1}$	**0.133*** **	**0.079** *	**0.079*** **	**0.059*** **
	(3.18)	**(1.75)**	**(5.24)**	**(3.79)**
$ABAGGREM_{t-1}$	0.124**		0.011	
	(1.98)		(0.53)	
$ABPROD_{t-1}$		0.022		0.016*
		(0.89)		(1.86)
$ABEXP_{t-1}$		0.061***		0.015**
		(3.37)		(2.30)
N	52,626	52,626	52,626	52,626
R^2	0.0244	0.0245	0.0341	0.0340

The sample period is from 1992 through 2014. Industry and year fixed effects are included in all regressions. The z-values (t-values) reported in parentheses are based on standard errors for the coefficient estimates that are heteroskedasticity-robust and clustered by firm. Significance levels are based on two-tailed tests: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. To save space, all the control variables (see Table 3) are suppressed. All variables are defined in Appendix.

5.6. Addressing endogeneity concern

The main endogeneity concern in our setting is correlated omitted variables. OCF opacity could occur in response to firm-specific factors. To address this concern, we use various approaches. First, we control for several firm-level variables that are documented in prior research as determinants of crash risk. Second, a series of cross-sectional analyses help alleviate the concern for omitted variables. For an omitted variable to explain our cross-sectional results, it has to affect both OCF opacity and crash risk in a certain way conditional on a partitioning variable. For instance, for an omitted variable to explain the results that analyst coverage mitigates the adverse impact of OCF opacity on crash risk, it has to influence both OCF opacity and crash risk to a lesser extent when analyst coverage is high. To further alleviate the endogeneity concern, we use two alternative approaches as discussed below.

5.6.1. Firm fixed effects regressions

Although we have controlled for various variables that prior research documents to be associated with crash risk, it is possible that our analysis omits some time-invariant variables, particularly those unobservable ones, which are correlated with both explanatory variables and future crash risk. To mitigate this concern, we re-estimate model (3) in Table 3 using firm fixed effects regressions. Panel A of Table 10 shows that as OCF opacity increases, future crash risk increases, irrespective of the crash risk measures. The evidence suggests that the baseline results are unlikely to be driven by correlated, omitted time-invariant variables.

5.6.2. Additional control variables

We also investigate whether our results are robust to the inclusion of additional variables that recent studies show to be associated with crash risk (Kim et al., 2011a, b; Kim and Zhang, 2016; Callen and Fang, 2013). The additional control variables include tax avoidance (*LRETR*), firm-level conservatism (*CSCORE*), institutional ownership stability (*IO_STB*), and

Table 10
OCF opacity and stock price crash risk: endogeneity concern.

Panel A: Firm fixed effects

	$CRASH_t$ (1)	$NCSKEW_t$ (2)
$ACCOPQ_{t-1}$	0.295	0.179***
	(1.61)	(3.22)
$ACCOPQ^2_{t-1}$	−0.312**	−0.131***
	(−2.03)	(−2.79)
$\boldsymbol{OCFOPQ_{t-1}}$	**0.218*****	**0.063*****
	(3.27)	**(3.12)**
N	41,578	52,626
R^2	0.027	0.091

Panel B: Additional control variables

	$CRASH_t$					$NCSKEW_t$				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$ACCOPQ_{t-1}$	0.485***	0.481***	0.412***	0.503***	0.405***	0.160***	0.155***	0.131***	0.166***	0.120***
	(3.20)	(3.15)	(2.72)	(3.31)	(2.66)	(3.46)	(3.36)	(2.83)	(3.72)	(2.66)
$ACCOPQ^2_{t-1}$	−0.411***	−0.401***	−0.360***	−0.411***	−0.348**	−0.115***	−0.108***	−0.095**	−0.116***	−0.085**
	(−2.99)	(−2.88)	(−2.63)	(−2.99)	(−2.51)	(−2.90)	(−2.73)	(−2.39)	(−3.01)	(−2.20)
$\boldsymbol{OCFOPQ_{t-1}}$	**0.134*****	**0.107****	**0.130*****	**0.137*****	**0.106****	**0.077*****	**0.059*****	**0.075*****	**0.079*****	**0.057*****
	(3.21)	**(2.54)**	**(3.14)**	**(3.30)**	**(2.52)**	**(5.17)**	**(4.03)**	**(5.09)**	**(5.99)**	**(4.29)**
$LRETR_{t-1}$	−0.134**				−0.112*	−0.072***				−0.046**
	(−2.13)				(−1.75)	(−3.73)				(−2.43)
$CSCORE_{t-1}$		−0.743***			−0.631***		−0.487***			−0.443***
		(−6.69)			(−5.47)		(−13.76)			(−12.80)
IO_STB_{t-1}			−0.506***		−0.485***			−0.241***		−0.232***
			(−10.86)		(−10.36)			(−16.99)		(−17.08)
OPT_INC_{t-1}				0.481*	0.405				0.148*	0.100
				(1.90)	(1.60)				(1.75)	(1.18)
STK_INC_{t-1}				−1.068**	−0.861				−0.285*	−0.183
				(−1.99)	(−1.60)				(−1.76)	(−1.13)
$BONUS_{t-1}$				−0.036	−0.035				−0.009	−0.008
				(−1.09)	(−1.06)				(−1.10)	(−0.96)
$SP1500_{t-1}$				0.167***	0.146***				0.039***	0.030***
				(4.54)	(3.98)				(3.62)	(2.80)
N	52,626	52,327	51,997	52,626	51,723	52,626	52,327	51,997	52,626	51,723
R^2	0.024	0.026	0.027	0.025	0.029	0.034	0.038	0.040	0.034	0.043

The sample period is from 1992 through 2014. Industry and year fixed effects are included in all regressions. The z-values (t-values) reported in parentheses are based on standard errors for the coefficient estimates that are heteroskedasticity-robust and clustered by firm. Significance levels are based on two-tailed tests: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. To save space, all the control variables (see Table 3) are suppressed. The pre-SOX period is defined as the period with fiscal year end before June 30, 2002, and the post-SOX period is defined as the period with fiscal year end after July 25, 2002. All other variables are defined in Appendix.

executive equity incentive (*OPT_INC* and *STK_INC*). For executive equity incentive, we control for CFO equity incentive since Kim et al. (2011a) document that CFO equity incentive, not CEO equity incentive, is associated with crash risk. Given that ExecuComp data are limited to S&P1500 firms, we set the values of CFO equity incentive variables to be zero for non-S&P1500 firms and include an indicator variable (*SP1500*) that distinguishes S&P1500 firms from other firms.

Panel B of Table 10 presents the results. In columns (1) through (5), we use *CRASH* as the dependent variable. We add the additional control variables one at a time in columns (1) through (4) and include all of them in column (5). The coefficients on these additional control variables are generally consistent with those reported in prior research. More important, our results on OCF opacity continue to hold after controlling for these additional variables. In columns (6) through (10), we replace *CRASH* with *NCSKEW* as the dependent variable and re-estimate the models in columns (1) through (5). We find similar results. Overall, Panel B suggests that our results are robust to the inclusion of additional control variables.

6. Conclusion

This study examines the relation between OCF opacity and stock price crash risk. Given the importance of OCF to the market, firms have incentives to manage OCF, which in turn increases OCF opacity. With high OCF opacity, insiders are more likely to withhold bad news and divert firm resources, in particular cash flows, thereby increasing stock price crash risk. Thus, we predict that stock price crash risk increases with OCF opacity. However, OCF opacity may not be positively associated with crash risk partly because OCF opacity may not be severe enough to induce crash risk due to the costs and constraints of OCF management. Therefore, we examine the effect of OCF opacity on crash risk empirically.

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Using a sample of U.S. public firms between 1992 and 2014, we find that future crash risk increases with OCF opacity. As we delve into the mechanisms underlying the relation between OCF opacity and crash risk, we find that OCF opacity increases the probability of very bad news release during the crash period and corresponds to more resource diversion. In addition, we find that the relation between OCF opacity and crash risk becomes more pronounced when firms have weak external monitoring, high information asymmetry, low OCF importance, and high cost of accruals management. Overall, our evidence highlights the severe consequence of OCF opacity in that it increases stock price crash risk; our study should alert the researchers, investors and regulators to pay more attention to OCF management.

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Appendix. Variable definitions

Variable	Definition
1. Dependent variables	
CRASH	An indicator variable that takes the value of 1 for a firm-year that experiences one or more firm-specific weekly returns falling 3.09 standard deviations below the mean firm-specific weekly returns over the fiscal year *t*, with 3.09 chosen to generate frequencies of 0.1% in the normal distribution during the fiscal year *t*, and 0 otherwise
NCSKEW	The negative skewness of firm-specific weekly returns over the fiscal year period. It is equal to the negative of the third moment of firm-specific weekly returns during a fiscal year, weighted by the standard deviation of firm-specific weekly returns raised to the third power
2. Test variables	
OCFOPQ	The prior three years' moving sum of the absolute value of abnormal operating cash flow (hereinafter "OCF"), where abnormal OCF is the difference between actual and normal OCF. The normal OCF is estimated by running the Dechow et al. (1998) model cross-sectionally for industry-years with at least 10 observations
OCFOPQ2	The performance-adjusted OCF opacity estimated by controlling for current firm performance when running the Dechow et al. (1998) model, where current firm performance is measured as net income scaled by total assets
OCFOPQ3	The standard deviation of abnormal OCF over a rolling window of prior three years, where the abnormal OCF is the difference between actual and normal OCF. The normal OCF is estimated by running the Dechow et al. (1998) model cross-sectionally for industry-years with at least 10 observations
$\lvert\Delta CC\rvert$	The absolute value of the change in cash conversion cycle from the fourth quarter in year *t-1* to the first quarter in year *t*, where the cash conversion cycle is defined as days in accounts receivable plus days in inventory minus days in accounts payable
3. Control variables	
ACCOPQ	The moving sum of the absolute value of discretionary accruals over the prior three years, where discretionary accruals are estimated from the modified Jones (1991) model in the cross section by each industry-year, where industry is based on two-digit SIC codes. The modified Jones model used is $ACC/\text{Assets} = \lambda\,(1/\text{Assets}) + \beta_1\,(\Delta Sales - \Delta AR)/\text{Assets} + \beta_2(PPE/\text{Assets})$, where *ACC* is total accruals, $\Delta SALES$ is the change in sales revenue, ΔAR is the change in accounts receivable, and *PPE* is gross property and equipment
ACCOPQ2	The performance-adjusted accruals-based earnings opacity estimated by controlling for current firm performance in the modified Jones (1991) model, where current firm performance is measured as net income scaled by total assets
DTURN	The average monthly share turnover in the current fiscal year minus the average monthly share turnover in the last fiscal year, where monthly share turnover equals the monthly trading volume divided by the total number of shares outstanding during that month
SIGMA	Standard deviation of firm-specific weekly returns over the fiscal year
RET	The mean of firm-specific weekly returns over the fiscal year, multiplied by 100
SIZE	Log of market value of equity
MB	Market value of equity divided by book value of equity
LEV	Total long-term debts divided by total assets
ROA	Income before extraordinary items divided by lagged total assets

Appendix (*continued*)

Variable	Definition
4. Partitioning variables	
IO_STB	Institutional ownership stability, measured following Callen and Fang (2013) as the negative value of the average standard deviation of institutional shareholding proportions across all investors in a firm over the most recent 5 years (i.e., 20 quarters)
NANAL	Analyst coverage, measured as the natural logarithm of one plus the number of analysts issuing earnings forecasts for a given firm at a specific year
BASPREAD	Bid-ask spread scaled by the midpoint of the spread, obtained from CRSP. The firm-year bid-ask spread is the average of the daily spreads of that firm-year
COMPLEX	Firm complexity, measured following Doyle et al. (2007), as the sum of the number of business and geographic segments
CFF	An indicator variable that takes the value of 1 if the firm has at least one analyst cash flow forecast for the fiscal year, and 0 otherwise.
DISTRESS	Financial distress, measured as the probability of bankruptcy estimated from the default hazard model based on Shumway (2001).
NOA	Net operating assets, measured following Barton and Simko (2002), as shareholders' equity less cash and marketable securities plus total debt scaled by lagged sales
SOX	An indicator variable that takes the value of 1 for observations with fiscal year end after July 25, 2002 and 0 for observations with fiscal year end before June 30, 2002
5. Other variables	
OCF	Operating cash flow, scaled by lagged total assets
ACC	Total accruals, measured as income before extraordinary items less cash flows from operations, scaled by lagged total assets
NOCF	The normal *OCF*, estimated by running the Dechow et al. (1998) model cross-sectionally for industry-years with at least 10 observations
AOCF	The abnormal *OCF*, calculated as the difference between *OCF* and *NOCF*
OCF_RESTATE	An indicator variable that takes the value of 1 for a firm-year that experiences one or more OCF restatements, and 0 otherwise
SURP_UOCF	An indicator variable that takes the value of 1 if the unexpected operating cash flow is in the lowest decile for the fiscal year *t*, and 0 otherwise, where the unexpected operating cash flow is defined as operating cash flow in the fiscal year t minus operating cash flow in the fiscal year t-1 (scaled by lagged total assets)
DIV	Cash dividends yield, measured as cash dividends paid divided by the fiscal year-end stock price. If the firm pays no dividends, *DIV* equals 0
DIV_POS	Cash dividends yield for the subsample of firms that pay cash dividends
ABPROD	The prior three years' moving sum of the absolute value of the abnormal production cost, where the abnormal production cost is estimated following Roychowdhury (2006)
ABEXP	The prior three years' moving sum of the absolute value of the abnormal discretionary expenditure, where the abnormal discretionary expenditure is estimated following Roychowdhury (2006)
ABAGGREM	The aggregated measure of real earnings management, measured as the sum of *ABPROD* and *ABEXP*
LRETR	The long-run cash effective tax rate, measured following Kim et al. (2011b) as the sum of income tax paid over the previous five years divided by the sum of a firm's pre-tax income less special items
CSCORE	Conditional conservatism, measured following Khan and Watts (2009)
OPT_INC	The incentive ratio of CFO option holdings, measured following Kim et al. (2011a) as *ONEPCT_OPT/(ONEPCT_OPT + SALARY + BONUS)*, where *ONEPCT_OPT* is the dollar change in the value of CFO option holdings resulting from a one percent increase in the firm's stock price
STK_INC	The incentive ratio of CFO stock holdings, measured following Kim et al. (2011a) as *ONEPCT_STK/(ONEPCT_STK + SALARY + BONUS)*, where *ONEPCT_STK* is the dollar change in the value of CFO stock holdings resulting from a one percent increase in the firm's stock price
BONUS	CFO bonus divided by salary
SP1500	An indicator variable that takes the value of 1 for a firm that is in the S&P 1500 index, and 0 otherwise
DOWN	The number of crash weeks during the fiscal year, where crash week is defined as when firm-specific weekly returns falling 3.09 standard deviations below the mean firm-specific weekly returns over the fiscal year
COUNT	The number of crash weeks during the fiscal year minus the number of jump weeks during the fiscal year
DUVOL	The natural logarithm of the ratio of standard deviation of down weeks to that of up weeks
JUMP	An indicator variable that takes the value of 1 for a firm-year that experiences one or more firm-specific weekly returns rising 3.09 standard deviations above the mean firm-specific weekly returns over the fiscal year *t*, and 0 otherwise

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Appendix A. Supplementary material

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jaccpubpol.2020.106717.

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