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Divergence of Cash Flow and Voting Rights, Opacity, and Stock Price Crash Risk: International Evidence

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ABSTRACT

This study investigates whether and how the deviation of cash flow rights (ownership) from voting rights (control), or simply the ownership-control wedge, influences the likelihood that extreme negative outliers occur in stock return distributions, which we refer to as stock price crash risk. We do so using a comprehensive panel data set of firms with a dual-class share structure from 20 countries around the world for the period of 1995–2007. We predict and find that opaque firms with a large wedge are more crash prone than opaque firms with a small wedge. In addition, we predict and find that the positive relation between the wedge and crash risk is less pronounced for firms with more effective external monitoring and for firms with greater growth opportunities. The results of this study are broadly consistent with Jin and Myers's

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theory that agency costs, combined with opacity, exacerbate stock price crash risk.

JEL codes: G12; K22; M41

Keywords: ownership-control wedge; stock price crash risk; information opacity; IFRS

1. Introduction

This study investigates the firm-level relation between corporate ownership structure and stock price crash risk. To do so, we construct a comprehensive panel data set of firms with dual-class equity structures (or simply dual-class firms) from 20 countries for the period of 1995–2007. We draw on the past literature to suggest that dual-class firms may be characterized by agency problems that lead to the consumption of private benefits of control and opacity. These two features of dual-class firms match the two factors that jointly determine crash risk in a theory developed by Jin and Myers [2006, hereafter JM]. Our main empirical results are consistent with JM's main prediction: stock price crash risk is increasing in the severity of agency problems when firms are opaque.

In JM, controlling insiders have the ability to expropriate resources and hide that activity from outsiders through earnings smoothing. When good news arrives, controlling insiders do not release it to the market, and instead report lower earnings, and then capture cash flows that are not anticipated by outsiders. When there is bad news, this information cannot be credibly revealed to outsiders, and the insiders thus absorb (or hide) it by reporting higher earnings and capturing less, possibly negative, cash flows. If the bad news accumulates to a tipping point, insiders no longer conceal it and the sudden revelation of this bad news leads to a stock price crash.[1]

Using cross-country data, JM provide country-level evidence supporting a positive association between *country-level* financial reporting opacity and stock price crash risk. Using U.S. data, subsequent studies provide *firm-level* evidence that financial reporting opacity is positively associated with ex post realized crash risk (Hutton, Marcus, and Tehranian [2009]) and ex ante expected crash risk proxied by options implied volatility smirk (Kim and Zhang [2014], Kim et al. [2016]). However, these studies do not focus on agency conflicts between controlling insiders and outside minority investors as causes of crash occurrence. As a result, we have little evidence relating directly to the JM prediction that this agency conflict, combined with financial reporting opacity, exacerbates firm-level crash risk. Our study aims to provide direct evidence consistent with this prediction.

[1] In the JM model, the opacity occurs independently of the agency conflict, but in practice these two arise jointly and are mutually reinforcing to facilitate insiders' consumption of private control benefits at the expense of outsiders.

For our empirical analysis, we use a sample of dual-class firms from 20 countries for the following reasons. A dual-class ownership structure exists when a firm has at least two classes of shares with different voting rights, creating a wedge between cash flow rights (ownership) and voting rights (control). This ownership-control wedge is prevalent among international firms and is a major source of agency problems in many countries around the world (Shleifer and Vishny [1997], La Porta et al. [1999], Lins [2003]). The use of dual-class firms provides us with a unique opportunity to directly measure the severity of the agency conflicts between the two parties, using the ownership-control wedge that has been commonly used in the literature (e.g., Shleifer and Vishny [1997], La Porta et al. [1999], Zingales [1995]). We discuss measurement of this variable in detail and provide related sensitivity analyses later. Controlling insiders have incentives to consume private control benefits at the expense of outside minority shareholders (Zingales [1994], Nenova [2003], Doidge [2004]). These insiders use the dual-class structure to facilitate extracting private control benefits and then provide opaque reporting to conceal that activity from outside stakeholders (Leuz, Nanda, and Wysocki [2003]).

Our sample of dual-class firms from 20 countries produces measurable variation in the two factors, that is, agency conflicts and opacity, that influence stock price crash risk in the JM model, and also allows us to empirically highlight that having only one of these factors *does not* necessarily engender crash risk. Further, the use of the international sample in this study allows us to examine whether and how country- and firm-level external monitoring mechanisms (which should increase the expected costs of extracting private control benefits) affect the agency conflict-crash risk relation that JM predict.

In our empirical tests, we focus on the relation between the ownership-control wedge and *firm-specific* crash risk (after netting out common risk). Following the literature (e.g., Chen, Hong, and Stein [2001], Jin and Myers [2006], Hutton, Marcus, and Tehranian [2009], Kim, Li, and Zhang [2011a,b]), we proxy for firm-specific crash risk using three distinct measures: (1) negative conditional skewness of firm-specific weekly returns; (2) the ratio of firm-specific weekly return volatility in down markets to that in up markets, simply called the down-to-up volatility; and (3) the likelihood that extreme negative firm-specific weekly returns occur in each year.

To measure the ownership-control wedge (i.e., the deviation of voting rights from cash flow rights), we first identify dual-class firms that issue two classes of shares with the same cash flow rights but with different voting rights in each sample country. The superior voting shares (e.g., having 10 votes per share) have significantly greater voting rights relative to their cash flow rights, compared to the inferior voting shares (e.g., having one vote per share). The superior voting shares are typically owned, in large part, by the controlling insiders (managers and directors) of the firm and create a considerable wedge between their voting and cash-flow rights. The ownership-control wedge is then defined for a dual-class share firm as one minus the ratio of voting rights to cash flow rights for inferior voting shares (Francis, Schipper, and Vincent [2005]).[2]

To empirically test the key prediction of JM's model that the agency conflict, combined with opacity, engenders firm-level crash risk, we condition our analyses of the agency conflict-crash risk relation on opacity by splitting the sample into two subsamples based on the sample median of financial reporting opacity in the past year for each sample country. Following Leuz, Nanda, and Wysocki [2003], Lang, Raedy, and Yetman [2003], and Barth, Landsman, and Lang [2008], we use the extent to which a firm engages in earnings smoothing as our proxy for a firm's financial reporting opacity. Earnings smoothing is the reporting behavior predicted in JM; managers smooth out firm-specific information to hide their consumption of private control benefits.

Our main results show that, for all three measures of crash risk, the association between the size of the ownership-control wedge and crash risk is positive and significant for high-opacity firms, while the same association is insignificant for low-opacity firms. We conduct a variety of additional tests to see if the above wedge-crash risk association is conditioned upon certain country- and firm-level factors that potentially affect the agency conflict or opacity. External monitoring, if effective, could constrain the ability of controlling insiders to consume private control benefits (Leuz, Nanda, and Wysocki [2003], Gopalan and Jayaraman [2012]). Strong external monitoring increases the expected cost of extracting private control benefits, including detection risk or penalties, and the strength of external monitoring varies across firms and countries. Our additional tests take advantage of this variation to identify agency costs and opacity as the underlying mechanisms that contribute to increasing stock price crash risk. These additional tests thus help us strengthen our identification strategy.

Specifically, a country's anti-self-dealing rules are expected to play a salient role in our setting because they are intended to limit asset expropriation or diversion through related party transactions. Consistent with our expectation, we find that country-level external monitoring as reflected in a country's anti-self-dealing rules moderates the positive wedge-crash risk association for high-opacity firms. We also find that firm-level external monitoring, proxied by analyst coverage, also moderates the positive relation between the wedge and crash risk for high-opacity firms. In addition, we find that growth opportunities reduce the positive wedge-crash risk association.

[2] The wedge between cash flow rights and voting rights therefore approaches zero as the voting and cash flow rights of the inferior shares become more equal, while it approaches one as the voting rights attached to inferior shares approach zero. While we focus on firms with a dual-class share structure, we acknowledge that a difference between ownership and control can arise in the absence of two classes of shares with differential voting rights. As will be further explained in section 5, even in a single-class firm, insiders who own 51% of total shares exercise full (100%) control over the firm. This ownership concentration creates a control-ownership wedge for single-class firms, which is similar to that for dual-class firms.

This result is consistent with the view that the opportunity costs of consuming private control benefits are higher for firms facing better growth opportunities (Johnson et al. [2000], Gopalan and Jayaraman [2012]).

We also find that our results are robust to a variety of sensitivity tests and supplemental analyses. To further strengthen our identification strategy, we also examine whether and how the observed positive relation between the wedge and crash risk for high-opacity firms is affected by the mandatory adoption of International Financial Reporting Standards (IFRS) in 2005 and concurrent regulatory changes associated therewith. These concurrent events are an exogenous change to financial reporting opacity at the firm level to the extent that the IFRS mandate and the concurrent regulatory changes, individually or jointly, reduce opacity. We find that the positive relation between the wedge and crash risk that we observe for high-opacity firms in the pre-IFRS-adoption period is attenuated in the post-IFRS-adoption period for mandatory IFRS adopters.[3]

Our results are robust to an alternative measure of agency conflicts. We use the market value of the voting premium as an alternative proxy for the severity of agency conflicts in dual-class firms. While the ownership-control wedge captures the severity of potential agency conflicts induced by dual-class ownership structure, the voting premium directly captures the size of managerial consumption of private control benefits based on the market price differential between superior and inferior voting shares in the public equity market.[4,5] Our results continue to hold using alternative measures of stock price crash risk and using an alternative econometric specification that controls for the impact of past crash history on future crash risk.

[3] Mandatory IFRS adoption has been combined in some countries with their concurrent efforts to strengthen corporate governance mechanisms and the enforcement of corporate and securities laws. Recent studies (e.g., Christensen, Hail, and Leuz [2013, 2016]) note other concurrent changes in corporate governance, enforcement, and securities laws around mandatory IFRS adoption. For the purpose of this test, it is not critical, however, whether other concurrent changes are responsible for the shift in the relation between the wedge and crash risk rather than mandatory IFRS adoption, to the extent that these changes result in an exogenous change to opacity around IFRS adoption. We do admit, however, that it is possible that contemporaneous events that do not affect opacity could impact the wedge-crash risk relation.

[4] The examples of private control benefits include the power to elect a related party to the board of directors or to appoint a family member as CEO or CFO. They may also include opportunities to engage in empire building, tunneling, self-dealing, and the expropriation of the firm's growth opportunities (Grossman and Hart [1988], Barclay and Holderness [1989], Zingales [1994], Shleifer and Vishny [1997], La Porta et al. [1997, 1998], Claessens, Djankov, and Lang [2000], Johnson et al. [2000], Bertland, Mehta, and Mullainathan [2002], Faccio and Lang [2002], Leuz, Nanda, and Wysocki [2003], Nenova [2003], Doidge [2004], Dyck and Zingales [2004], Djankov et al. [2008], Hong [2013]).

[5] The voting premium measures the value that market participants place on the additional votes attached to superior shares. It provides a lower bound on the private benefits of control that the controlling shareholders with superior voting rights can enjoy, because the market participants who buy superior voting shares but do not gain control of the company will realize the value of their superior votes only in the event of a future control contest.

We perform supplemental analyses using a sample of both dual-class firms and single-class firms matched with dual-class firms based on observable firm characteristics. We also present the results of regressions after controlling for a wide range of country-level and firm-level variables that could be related to the dual-class structure or opacity. These tests help us mitigate a concern that our results could be driven by omitted variables that are correlated with the dual-class structure and/or opacity. Our results continue to hold in these tests. We also use alternative measures of external monitoring and the opportunity costs of expropriation, and find similar results.

Our study contributes to the existing literature in the following ways. First, to our knowledge, this study is the first that provides direct evidence supporting JM's main prediction that agency conflicts between insiders and outsiders and opacity combine to increase firm-level crash risk. Second, our study uses international data and provides evidence that the wedge-crash risk relation is moderated by stronger investor protection at the country level and more effective external monitoring at the firm level. We show that the wedge-crash risk relation is moderated by higher opportunity costs of consuming private control benefits. These tests support the JM prediction that stronger investor protection, more effective monitoring, and higher costs of resource diversion reduce the impact of the wedge-induced agency conflict on crash risk in high-opacity firms.

2. *Background and Hypotheses*

2.1 FIRM-SPECIFIC DETERMINANTS OF STOCK PRICE CRASH RISK

JM develop an analytical model in which an agency conflict between corporate insiders and outside investors, when combined with opacity, can lead to a stock price crash. In JM, inside managers have information that outside investors do not have, that is, the firm is at least partly opaque.[6] In their model, in each period, the insiders receive better information than investors about firm performance. This allows them to potentially divert resources away from outside investors for their own benefit. Outside investors may take collective action to seize the firm and dismiss inside managers. However, this collective action could be too costly to justify the associated expected benefit. Better investor protection reduces the cost of this

[6] As we noted earlier, this opacity arises independently of the agency conflict in JM, but JM do note that in practice they go together and are likely to be mutually reinforcing to hide and facilitate insider mangers' consumption of private control benefits. Our study tests this mutually reinforcing effect of the agency conflict and opacity using dual-class firm data, even though JM model these as two independent forces driving stock price crashes. Testing the JM model requires identifying firms in which both agency conflicts between insiders and outsiders and opacity exist, and JM note that in most circumstances these will arise jointly, as in our data. As we explain in more detail later, there are reasons why dual-class firms would have only one of agency conflicts or opacity, and our research design exploits this variation to focus on the conditions jointly identified as contributing to crash risk in JM.

action and therefore reduces insiders' asset diversion. The collective action is taken only if, based on outside investors' information, inside managers report earnings and pay dividends that are not sufficient to satisfy the outside investors. The resulting equilibrium has managers choosing to report earnings and pay dividends necessary to forestall action by investors.

When the private information of the insiders is good news, the insiders steal more and do not reveal the good news. When the private information of the insiders is bad news, the private information cannot be credibly revealed to outsiders without cost because the insiders have incentives to always claim that their private information is bad news to lower reported earnings and dividend payments and increase their stealing. As a result, when their private information is bad news, the insiders will absorb this bad news and subsidize the payment of dividends based on reported earnings that do not incorporate their private bad news.[7] Thus, they continue to operate the firm in the future and enjoy private benefits when there is good news. Stated differently, the insiders smooth earnings and hide unfavorable private information from outside investors, and accumulate it over time.[8] But the insiders are only able to do this up to a certain threshold point beyond which the cost of hiding additional bad news exceeds the associated benefit. If the total amount of hidden bad news accumulated over time crosses over this point, then the controlling insiders exercise an abandonment option rather than absorbing further bad news. This causes the accumulated bad news to be released all at once, creating an abrupt, large-scale decline in stock price, that is, a stock price crash.

2.2 MAIN PREDICTION

A central prediction of the JM theory is that the agency conflict, combined with opacity, drives a firm's crash risk. JM clearly point out that information opacity per se does not increase crash risk if there is no conflict of interest between controlling insiders and outside minority investors. Our sample of firms with a dual-class share structure is ideally suited to test the empirical implications of JM because a dual-class ownership structure can create an agency conflict between controlling insiders and outside investors that closely resembles the conflict between these two parties described in JM. In many insider-controlled firms, firm management is either part of the controlling group or appointed by that group and the controlling insiders typically have direct access to corporate resources and control over financial reporting, or have influence over those who do. As a result, they are given both opportunities and the ability to expropriate resources for their private gain and mask their activities through opaque reporting (Lins [2003], Haw et al. [2004], Leuz, Nanda, and Wysocki [2003], Gopalan and

[7] This in turn causes some of firm-specific risk (associated with hidden bad news) to shift to the insiders from the outside investors.

[8] This feature of JM theory well matches our choice of opacity, for example, income smoothing, and is in line with the Fudenberg and Tirole [1995] theory of income smoothing.

Jayaraman [2012]).[9] Because of the adverse consequences of the dual-class structure, the market attaches lower value to dual-class firms, compared with the value attached to single-class firms (Dyck and Zingales [2004]). Controlling insiders, nevertheless, choose the dual-class structure as the ownership-control wedge allows insiders to avoid the pro rata consequences of the consumption of private control benefits (Jensen and Ruback [1983], Zingales [1994], Shleifer and Vishny [1997], Gompers, Ishii, and Metrick [2010], Masulis, Wang, and Xie [2009], Hong [2013]).

While prior research suggests, in general, that dual-class firms are associated with severe agency conflicts and financial reporting opacity, this characterization is not universally the case. There are benign reasons for the use of dual-class shares. For example, the dual-class ownership structure can allow founders of companies with specialized knowledge necessary for the success of the business to retain their control rights and incentives to invest in organization-specific human capital, even though they have relatively low ownership rights (DeAngelo and DeAngelo [1985], Fan and Wong [2002]). In addition, as Fan and Wong [2002] point out, the dual-class structure may promote opacity to avoid proprietary costs. Increased opacity may better enable the firm to protect its competitive position from potential competitors in the product market. In this case, the opacity benefits both the controlling insiders and outside investors (Fan and Wong [2002]). This type of opacity would not lead to a stock price crash in the context of the JM model.

In addition, even controlling insiders who expropriate private benefits of control may not necessarily engage in opaque financial reporting to obfuscate their self-serving activities, as long as such insiders are entrenched and thus insulated from external disciplinary forces (e.g., takeover threats) and penalties, due to their complete control of the firm. Given that there are some costs to opacity,[10] controlling insiders would choose to bear these costs only when the benefits in the form of avoiding penalties for consuming private benefits are greater than the costs.[11] Together, these arguments imply that not all firms with dual-class shares are characterized by *both* agency conflicts and opacity.

Our research design exploits variation in the ownership-control wedge and financial reporting opacity across dual-class firms in our international sample. To do so, we first partition the total sample into two subsamples, high- and low-opacity samples. We then introduce a measure of the severity of the agency conflict between controlling insiders and outside shareholders within these subsamples. If the analysis in JM is correct, then

[9] The expropriation could occur, for example, by diverting corporate cash flows and engaging in value-decreasing investment projects (Jensen and Ruback [1983], Shleifer and Vishny [1997], Masulis, Wang, and Xie [2009], Gompers, Ishii, and Metrick [2010]).

[10] For example, the cost of external financing is higher for opaque firms than for transparent firms (e.g., Francis et al. [2004], Kim, Song, and Zhang [2011]). Also outside investors' perception of managers' expropriation risk leads to the valuation discount of common stock (Masulis, Wang, and Xie [2009], Gompers, Ishii, and Metrick [2010]).

[11] We are grateful to an anonymous referee for pointing out this possibility.

stock price crashes should increase with the severity of the agency conflict in high-opacity firms, but not necessarily so in low-opacity firms. This prediction is based on the presumption that, on average, greater ownership-control wedges are associated with greater agency problems for opaque firms.

2.3 EXTERNAL MONITORING AND COSTS OF CONSUMING PRIVATE CONTROL BENEFITS

The *net* benefits to controlling insiders from consuming private control benefits and withholding private information depend critically on the efficacy of external monitoring. As Leuz, Nanda, and Wysocki [2003] and Gopalan and Jayaraman [2012] discuss, external monitoring increases the expected costs of extracting private control benefits, including detection risk or penalties, which constrains controlling insiders from aggressive rent seeking. We therefore predict that the positive relation between the wedge and crash risk is less pronounced in an environment of more effective external monitoring.

We also expect that the costs of consuming private control benefits increase when the opportunity costs of diverting resources from productive use in the firm are higher. For example, inefficiency or value destruction arising from consuming private benefits of control is likely to result in poor, perhaps unsustainable, firm performance. The opportunity costs of consuming private benefits of control increase when the firm has greater growth opportunities, as the returns forgone from the failure to productively invest resources are greater for higher-growth firms. We test for these effects on the wedge-crash risk relation in high-opacity firms, since these firms are predicted by JM to experience heightened crash risk in the first place.[12] These tests also help us strengthen identification because they can provide evidence on whether varying the cost of consuming private benefits of control predictably produces variation in the wedge-crash risk relation.

We test the effects of variation in monitoring and the opportunity costs of consuming private benefits of control in three ways. First, we examine whether the positive relation between the wedge and crash risk is attenuated in countries with more stringent rules regulating self-dealing transactions. Djankov et al. [2008] introduce the anti-self-dealing index and find that the index predicts a variety of stock market outcomes, including the control premium paid in corporate control transactions. This measure of a country's legal and enforcement environment is well suited to our study because it measures legal impediments to extracting the private benefits of control through related party transactions, a particularly pronounced

[12] While external monitoring and opportunity costs of consuming private benefits of control may moderate opacity that arises to obfuscate asset diversion, as we pointed out earlier, there are other reasons why opacity may arise. Hence, the association between the wedge and crash risk in opaque firms should be moderated when monitoring and the costs of asset diversion are high.

concern for dual-class firms. Second, assuming that external monitoring is more intense for firms with higher analyst coverage, we also examine whether the positive relation between the wedge and crash risk in opaque firms is attenuated for firms followed by more analysts. Third, we capture the opportunity costs of consuming private control benefits, using industry-level growth opportunities (Gopalan and Jayaraman [2012]) that we measure by the change in annual sales revenue in the Fama and French [1997] industry to which the firm belongs.

3. Measurement of Key Research Variables

3.1 MEASURING FIRM-SPECIFIC CRASH RISK

The dependent variable in our regression analysis is firm-specific crash risk, or the likelihood of observing extreme negative outliers in firm-specific return distributions. To isolate firm-specific risk from common (industry- and market-wide) risk, we first estimate the following model using weekly return data for each firm in a country (JM):[13]

$$\begin{aligned} r_{it} = {} & \alpha_i + \beta_{1,i} r_{m,j,t} + \beta_{2,i}\left[r_{us,t} + EX_{jt}\right] + \beta_{3,i} r_{m,j,t-1} \\ & + \beta_{4,i}\left[r_{us,t-1} + EX_{j,t-1}\right] + \beta_{5,i} r_{m,j,t-2} + \beta_{6,i}\left[r_{us,t-2} + EX_{j,t-2}\right] \\ & + \beta_{7,i} r_{m,j,t+1} + \beta_{8,i}\left[r_{us,t+1} + EX_{j,t+1}\right] + \beta_{9,i} r_{m,j,t+2} \\ & + \beta_{10j}\left[r_{us,t+2} + EX_{j,t+2}\right] + \varepsilon_{it}, \end{aligned} \tag{1}$$

where r_{it} is the return on a firm's inferior voting share i in week t in country j; $r_{m,j,t}$ is the Morgan Stanley Capital International (MSCI) country-specific market index return, or the country index return (if a country is not included in MSCI) in country j compiled by Datastream in week t; $r_{us,t}$ is the U.S. market index return (a proxy for the global market); $EX_{j,t}$ is the change in country j's exchange rate for one U.S. dollar; and ε_{it} represents unspecified factors. The expression $r_{us,t} + EX_{j,t}$ translates U.S. market returns into country j's local currency unit. We allow for nonsynchronous trading by including lead and lag terms for the market index returns (Dimson [1979]). From equation (1), we then obtain the firm-specific weekly return for firm i in week t, denoted by W_{it}, which is defined as $W_{it} = \ln(1 + \varepsilon_{it})$. In estimating equation (1), we require that at least 20 weekly return observations be available for each firm in each sample year.

Our first measure of crash likelihood is negative conditional firm-specific weakly return skewness (*NEG_SKEWNESS*) introduced by Chen, Hong, and Stein [2001]. Specifically, we calculate *NEG_SKEWNESS* for a given firm in

[13] Following JM and Hutton, Marcus, and Tehranian (2009), we use *raw* stock returns when estimating equation (1). As noted below, and also consistent with the prior literature, throughout the paper, firm-specific weekly return for firm i in week t, denoted by W_{it}, refers to the natural log of 1 plus the residual from equation (1). We use W_{it} to construct our first two crash measures detailed in equations (2) and (3) as well as our third crash measure, *CRASH*.

a fiscal year by taking the negative of the third moment of firm-specific weekly returns, W_{it}, during the fiscal year and dividing it by the standard deviation of firm-specific weekly returns, raised to the third power. The negative sign creates a variable that increases as the return distribution becomes more negatively skewed. Specifically, for each firm i in year t, we obtain *NEG_SKEWNESS* as follows:

$$NEG\_SKEWNESS_{it} = -\left[n(n-1)^{3/2} \sum W_{i\tau}^3\right] / \left[(n-1)(n-2)\left(\sum W_{i\tau}^2\right)^{3/2}\right]. \tag{2}$$

Our second measure of crash risk is the down-to-up volatility of Chen, Hong, and Stein [2001], denoted by *DOWN/UP_RET_SD*. For each firm in each fiscal year, we separate all weeks with firm-specific weekly returns, that is, W_{it}, below the annual mean ("down" weeks) from those with firm-specific returns above the annual mean ("up" weeks), and calculate the standard deviation for each of these subsamples separately. We then compute the *DOWN/UP_RET_SD* measure using the natural log of the ratio of the standard deviation on down weeks to the standard deviation on up weeks. Specifically, for firm i in year t, we obtain:

$$DOWN/UP\_RET\_SD_{it} = \ln\left[(n_u - 1)\sum_{DOWN} W_{i\tau}^2 / (n_d - 1)\sum_{UP} W_{i\tau}^2\right], \tag{3}$$

where n_d and n_u are the number of down and up weeks, respectively.

Our third measure of crash risk is the probability of observing extreme, negative firm-specific returns, denoted by *CRASH*. To obtain this *CRASH* measure, similar to Hutton, Marcus, and Tehranian [2009] and Kim, Li, and Zhang [2011a,b], we define crash weeks (extreme events) in a given fiscal year for a firm as those weeks during which the firm experiences firm-specific weekly returns that are lower than 3.0 standard deviations below mean firm-specific weekly returns over the entire fiscal year, with 3.0 chosen to generate a frequency of 0.1% in the normal distribution.[14,15] *CRASH* is an indicator variable that equals 1 for a firm-year that experiences one or more crash weeks (as defined above) during the fiscal year, and zero otherwise.

[14] In untabulated robustness tests, we choose n standard deviations to generate frequencies of 0.01% and 1% in the lognormal distribution. We find that the use of these alternative measures does not alter our inferences.

[15] Our definition of crash results in substantially negative weekly returns. Untabulated statistics show that the mean (median) raw return for crash weeks is –23.7% (–21.0%). These statistics are largely consistent with those described by prior studies. For example, Hutton, Marcus, and Tehranian [2009] report the mean weekly return for *CRASH* weeks is –22.74%.

3.2 MEASURING THE OWNERSHIP-CONTROL WEDGE

A dual-class ownership structure exists when a firm has at least two classes of shares with different voting rights, namely superior and inferior voting shares. In this study, the severity of the agency conflicts in dual-class firms is captured by two different measures. Our first measure is the divergence between cash flow rights and voting rights or the ownership-control wedge. In dual-class firms with superior and inferior voting shares, the superior voting shares typically confer multiple votes per share and the inferior voting shares confer fewer (often zero) votes per share. We follow Francis, Schipper, and Vincent [2005] to measure the extent of divergence between cash flow rights and voting rights. Specifically, we define a variable, *WEDGE,* from the perspective of inferior class shareholders, as follows:

$$WEDGE = 1 - \frac{Voting\ Rights}{Cash\ Flow\ Rights}, \tag{4}$$

where:

Voting Rights = The percentage of total votes held by inferior class shareholders, which is equal to the number of votes per inferior share times the number of inferior shares (inferior votes), divided by the sum of inferior votes (as defined above) and superior votes (the number of votes per superior share times the number of superior shares); and

Cash Flow Rights = The percentage of total cash flow rights held by inferior class shareholders, equal to the number of inferior class shares divided by the sum of inferior class and superior class shares (Francis, Schipper, and Vincent [2005, p. 346]).

WEDGE takes a value between zero and one. When inferior class shareholders have no voting rights, *WEDGE* is equal to one. *WEDGE* approaches zero as the voting and cash flow rights of the inferior shares converge. *WEDGE* is measured annually and we use *WEDGE* from period $t - 1$ to predict crash risk during period t.

In this study, *WEDGE*, which is based on two classes of shares with different voting rights for a given cash flow right, only applies to firms with a dual-class structure. We acknowledge that a control-ownership wedge can arise from large insider ownership stakes even in the absence of a dual-class share structure. For example, when insider ownership is greater than 50%, insiders have 100% control over the firm's financial and operating decisions. Subsection 5.3.6 discusses a test for the effects of an ownership-control wedge arising from large insider ownership stakes in a sample including both dual-class firms and matched single-class firms.

Our second measure is the market value of the voting premium that the equity market attaches to voting rights, which represents the size of managerial consumption of private control benefits (e.g., Zingales [1995], Doidge [2004], Nenova [2003]). The voting premium is the difference between the market prices of superior voting shares and inferior voting shares. While *WEDGE* captures the ownership structure from which the agency conflicts between controlling insiders and outside investors arise, the voting premium provides an economic measure of the private control benefits. More detailed discussions about the voting premium attached to superior voting shares in dual-class firms are provided later in subsection 5.3.4.

3.3 MEASURING OPACITY

Controlling insiders can conceal their firm's real underlying performance from outside investors by making financial reporting choices. We measure financial reporting opacity using the degree to which controlling insiders of dual-class firms ''smooth'' the reported earnings series, that is, the extent to which they reduce the variability of reported earnings over time by altering the accrual component of earnings over time. Following Leuz, Nanda, and Wysocki [2003], we capture "earnings smoothness" using a firm's Pearson correlation coefficient between the change in accruals and the change in cash flows from operations over the past five years, both scaled by lagged total assets. We compute cash flow from operations as operating income minus accruals. Accruals are calculated as [(Δtotal current assets – Δcash) – (Δtotal current liabilities – Δshort-term debt – Δtaxes payable) – depreciation expense]. Finally, we multiply this correlation measure by minus one (-1) such that a higher value represents a higher degree of earnings smoothing. All else being equal, the higher the value of this measure, the greater the extent to which controlling insiders exercise accounting discretion to smooth reported earnings and hide adverse information, and thus, the higher the opacity in financial reporting.

Our measure of financial reporting opacity, *OPAQUE*, is computed *before* we observe crash risk in year t in our tests. Most of our empirical analyses are conducted after partitioning our sample into low- and high-opacity subsamples, and focus on the high-opacity subsample. Specifically, the low-opacity subsample contains all observations in which the value of *OPAQUE* in year $t-1$ is less than the sample median for each country-year, and the high-opacity subsample contains all observations in which the value of *OPAQUE* in year $t-1$ is greater than or equal to the sample median for each country-year.[16] We split the sample based on country-year medians to ensure that our sample partition is not affected by across-country or overtime variation in smoothing that is unrelated to the smoothing of firm-specific information predicted in JM.

[16] As reported in subsection 5.3.5, we also present results using alternative measures of opacity.

4. *Data and Descriptive Statistics*

4.1 SAMPLE AND DATA SOURCES

We begin the sample collection procedure by identifying firms that have at least two classes of shares according to the Datastream country lists during the period of 1995–2007. After identifying these firms, we follow prior studies and impose the following sample selection criteria: (1) firms must have at least two classes of shares with distinct voting and cash flow rights, (2) both classes of shares must be publicly traded on a domestic stock exchange, (3) the inferior voting shares cannot be convertible into the superior voting shares (though the opposite direction is allowed), (4) neither share class can receive a fixed dividend, and (5) neither share class can be redeemable or callable at the option of the firm at a prearranged price.

The data collection procedures follow the guidelines established in Nenova [2003] and Doidge [2004]. First, for each share class, we extract Friday-to-Friday weekly data from Datastream for the following variables: closing stock price, market value of all equity outstanding, weekly return, dividends paid during the week, number of shares outstanding, and turnover. If the value of turnover is missing from Datastream, it is obtained from Bloomberg (Doidge [2004]). In addition, we collect *lagged* annual financial statement variables from Worldscope. Second, for each fiscal year, a firm is included in the sample only if it has at least 20 weekly stock return observations. Third, because data on the number of voting rights attached to the superior and inferior voting shares for each firm are required, these data are hand-collected from Datastream Manuals, Moody's International Manuals, filings with the national stock exchanges, firms' annual reports, and the firm lists compiled by Doidge [2004].[17] The final sample consists of 3,350 firm-year observations from 449 firms across 20 countries.

4.2 DESCRIPTIVE STATISTICS

Panels A and B of table 1 provide the distribution of the number of firm-years and the percentage of firms that experienced at least one crash week per year by country and year, respectively, with the sample further partitioned into low- and high-opacity subsamples for consistency with our subsequent empirical tests. All continuous variables are winsorized at the 1st and 99th percentiles to avoid outliers and their influence on coefficient estimates. As shown in table 1, panel A, 23.11% of our low-opacity firm-year observations experience at least one crash, while 24.26% of the high-opacity firm-year observations experience at least one crash, and these numbers vary substantially across countries. This marginal difference in stock price crash risk between the high- and low-opacity samples demonstrates the importance of examining the joint effects of opacity and agency problems as

[17] In conducting this research, we received significant support from Craig Doidge with respect to data collection. In addition, if the data are not clear or not available, they are requested from each firm through faxes, emails, and phone calls.

TABLE 1
Sample Distribution

Panel A: Distribution of firm-years with at least one crash week by country

	Low Opacity			High Opacity		
Country	Number of Firm-Year Observations	Number of Firm-Year Observations with Stock Price Crash	Percentage of Firm-Year Observations with Stock Price Crash	Number of Firm-Year Observations	Number of Firm-Year Observations with Stock Price Crash	Percentage of Firm-Year Observations with Stock Price Crash
Australia	11	2	18.18	21	4	19.05
Austria	11	5	45.45	16	8	50.00
Brazil	262	40	15.27	268	66	24.63
Canada	163	32	19.63	172	32	18.60
Chile	23	2	8.70	19	5	26.32
Colombia	0	0	0.00	1	0	0.00
Denmark	59	21	35.59	64	11	17.19
Finland	64	10	15.63	72	16	22.22
France	21	5	23.81	29	5	17.24
Germany	193	39	20.21	202	36	17.82
Italy	156	39	25.00	163	38	23.31
Korea	334	128	38.32	343	132	38.48
Mexico	13	2	15.38	9	5	55.56
Norway	29	5	17.24	35	11	31.43
Portugal	4	2	50.00	2	2	100.00
South Africa	31	7	22.58	36	9	25.00
Sweden	138	30	21.74	145	25	17.24
Switzerland	23	3	13.04	33	6	18.18
UK	10	2	20.00	13	3	23.08
United States	78	1	1.28	84	5	5.95
Total	1,623	375	23.11	1,727	419	24.26

(*Continued*)

TABLE 1—*Continued*

Panel B: Yearly distribution of firm-years with at least one crash week

Country	Low Opacity			High Opacity		
	Number of Firm-Year Observations	Number of Firm-Year Observations with Stock Price Crash	Percentage of Firm-Year Observations with Stock Price Crash	Number of Firm-Year Observations	Number of Firm-Year Observations with Stock Price Crash	Percentage of Firm-Year Observations with Stock Price Crash
1995	76	22	28.94	85	21	24.70
1996	75	14	16.60	84	12	14.20
1997	108	23	21.29	114	20	17.54
1998	120	34	28.30	130	30	23.07
1999	146	30	20.54	154	40	25.97
2000	147	26	17.68	154	38	24.67
2001	146	55	37.67	153	64	41.83
2002	137	34	24.81	147	30	20.40
2003	142	25	17.60	152	29	19.07
2004	141	28	19.85	145	33	22.75
2005	138	38	27.53	149	54	36.24
2006	128	24	18.75	134	26	19.40
2007	119	22	18.48	126	22	17.46

This table reports the number of firm-year observations, and the number and percentage of firm-year observations for annual stock price crashes by country and year. The sample consists of countries with dual-class stocks, which have sufficient accounting and market data from Datastream and Worldscope. Specifically, as for the non-U.S. firms, we construct the sample by identifying all firms with dual-class shares during the sample period from the country lists in the Datastream database. As for the U.S. firms, we construct the sample by identifying all firms with dual-class shares from the Securities Data Company (SDC), S&P's Compustat, the Center for Research in Security Prices (CRSP), and the Investor Responsibility Research Center (IRRC) databases. We then confirm these firms' share structure by checking the SEC filings and contacting each firm's investment relationship department. Following the prior voting premium literature, we impose the following sample criteria on the identified firms: (1) firms have at least two classes of shares with different voting rights, (2) both share classes are publicly traded and listed on the domestic exchange, (3) the inferior voting share is not convertible into the superior voting share, and (4) neither share class receives a fixed dividend (Nenova [2003], Doidge [2004]). We download accounting variables from Worldscope and Compustat North America. The market data we download from Datastream and CRSP include Friday-to-Friday weekly market data for prices, returns, market returns, market value, dividend yields, number of shares outstanding, and turnover. In instances in which the values of turnover are missing, we collect them from Bloomberg. We include a firm in the sample for a given fiscal year if it has at least 20 weekly observations. Finally, we extensively hand-collect data on the different voting rights associated with the superior- and inferior-voting shares (data source: documentation supplied by Doidge [2004], Moody's International Manuals, and firms' annual reports among others). We exclude firm-year observations with missing values for any variable. Consequently, we obtain 3,350 firm-year observations from 20 countries as the final sample.

in JM, as well as the importance of using multivariate regressions to control for other factors. Table 1, panel B, reveals that the annual proportion of firms with at least one crash week per year varies from a low of 14.20% in 1996 for the high-opacity subsample to a high of 41.83% in 2001 for the high-opacity subsample. Table A1 in the online appendix provides the distribution of the number of firm-years and the percentage of firms with at least one crash week per year by industry. It shows that the percentage of firm-year observations with at least one crash week per year varies significantly across industries for both the low- and the high-opacity subsamples.

Table 2, panel A, presents descriptive statistics for three crash risk variables as well as control variables used in our primary regression analysis. The appendix provides detailed variable definitions. As shown in the first three rows of panel A, the mean and median values of all three crash risk measures are slightly more pronounced for the high-opacity sample than for the low-opacity sample. The descriptive statistics for control variables are largely consistent with those reported by the stock price crash literature (e.g., Chen, Hong, and Stein [2001], Hutton, Marcus, and Tehranian [2009], Kim, Li, and Zhang [2011a, b], DeFond et al. [2015]). The low- and high-opacity subsamples are generally similar along most variables except for *OPAQUE*. The *OPAQUE* variable differs significantly between the high-opacity and low-opacity subsamples by construction with a mean of 0.7500 and 0.9544 for the low- and high-opacity subsamples, respectively, indicating significant cross-sectional variation in financial reporting opacity among dual-class firms in our sample.

Table 2, panel B, provides the descriptive statistics for the cash flow rights and voting rights of the dual-class stocks in our sample. On average, the inferior shares possess around 49.40% (47.98%) of the total cash flow rights, but own only 8.85% (8.15%) of the total voting rights, for our sample companies in the low(high)-opacity sample. As shown in the last two rows of panel B, the mean inferior voting stock has only 0.1244 (0.1178) votes per cash flow right in the low(high)-opacity sample, while the mean superior voting stock has 2.6305 (2.4652) votes per cash flow right. Overall, the descriptive statistics are consistent with the notion that the dual-class ownership structure offers superior (inferior) voting rights to controlling parties (common shareholders) compared to their cash flow rights. The magnitude of the difference between controlling parties' cash flow and voting rights suggests the potential for severe agency conflicts. The high- and low-opacity subsamples are generally similar in how they distribute voting and cash flow rights across share classes.

5. *Regression Results*

This section of the paper provides the results of our primary empirical tests.

TABLE 2

Descriptive Statistics

Panel A: Descriptive statistics for variables

	Low Opacity						High Opacity					
	N	Mean	Median	SD	P25	P75	*N*	Mean	Median	SD	P25	P75
Dependent Variables (Crash Risk):												
NEG_SKEWNESS	1,623	−0.1051	−0.1078	1.0943	−0.5858	0.3739	1,727	−0.0597	−0.0808	1.1189	−0.5695	0.3744
DOWN/UP_RET_SD	1,623	−0.0871	−0.0955	0.4331	−0.3309	0.1393	1,727	−0.0760	−0.0830	0.4450	−0.3163	0.1401
CRASH	1,623	0.2311	0.0000	0.4216	0.0000	0.0000	1,727	0.2426	0.0000	0.4288	0.0000	0.0000
Control Variables:												
TRADE_VOLUME	1,623	0.0049	0.0026	0.0053	0.0000	0.0099	1,727	0.0049	0.0030	0.0054	0.0000	0.0099
RETURN_SD	1,623	0.0673	0.0524	0.0448	0.0797	0.0386	1,727	0.0672	0.0535	0.0443	0.0798	0.0372
RETURN	1,623	0.0347	0.0292	0.0313	0.0117	0.0495	1,727	0.0346	0.0295	0.0306	0.0126	0.0485
MARKET_CAP	1,623	6.6571	6.3354	2.9297	4.2005	8.8739	1,727	6.6037	6.2322	2.7513	4.4991	8.6024
MB	1,623	1.5266	1.2104	1.1525	0.6834	2.0153	1,727	1.4805	1.1667	1.1191	0.7061	1.9294
LEVERAGE	1,623	0.2741	0.2465	0.1965	0.1097	0.4183	1,727	0.2711	0.2545	0.1830	0.1264	0.3929
ROA	1,623	0.0605	0.0460	0.0544	0.0204	0.0802	1,727	0.0536	0.0395	0.0513	0.0168	0.0705
OPER_CYCLE	1,623	4.8582	4.8690	0.4905	4.5525	5.1944	1,727	4.9096	4.9292	0.5101	4.5825	5.2586
CFO/SALES	1,623	0.1141	0.0980	0.0986	0.0535	0.1595	1,727	0.1050	0.0829	0.0997	0.0448	0.1451
STD_CFO	1,623	0.4787	0.1565	1.1179	0.0694	0.3999	1,727	0.4124	0.1398	1.0742	0.0638	0.3321
OPAQUE	1,623	0.7500	0.8479	0.2510	0.6529	0.9333	1,727	0.9544	0.9702	0.06406	0.9551	0.9768
ANTI-SD	1,623	0.4281	0.4200	0.1480	0.2800	0.4700	1,727	0.4311	0.4200	0.1511	0.2800	0.4700

(*Continued*)

TABLE 2—*Continued*

Panel B: Sample description: information on the cash flow rights and voting rights of sample dual-class shares

	Low Opacity						High Opacity					
	N	Mean	Median	SD	P25	P75	*N*	Mean	Median	SD	P25	P75
Voting premium	1,623	0.1935	0.0528	0.5551	−0.0284	0.4162	1,727	0.2093	0.0650	0.5746	−0.0307	0.4167
Inferior shares (in millions)	1,623	70.5664	14.0373	143.0253	1.5420	61.0180	1,727	70.2838	12.9260	461.3583	1.1230	49.5979
Superior shares (in millions)	1,623	83.3249	10.8225	197.7674	2.4410	53.9660	1,727	85.3956	10.8225	614.1240	2.0000	44.0030
Inferior cash flow rights (%)	1,623	0.4940	0.5000	0.2680	0.2977	0.6952	1,727	0.4798	0.4934	0.2635	0.2670	0.6667
Superior cash flow rights (%)	1,623	0.5060	0.5000	0.2680	0.3048	0.7023	1,727	0.5202	0.5066	0.2635	0.3333	0.7330
Inferior voting rights (%)	1,623	0.0885	0.0137	0.1289	0.0000	0.1217	1,727	0.0815	0.0137	0.1242	0.0000	0.0983
Superior voting rights (%)	1,623	0.9115	0.9863	0.1289	0.8783	1.0000	1,727	0.9185	0.9863	0.1242	0.9017	1.0000
Inferior voting rights to cash flow rights	1,623	0.1244	0.0236	0.1520	0.0000	0.1957	1,727	0.1178	0.0243	0.1492	0.0000	0.1821
Superior voting rights to cash flow rights	1,623	2.6305	1.9491	2.0774	1.3671	2.9284	1,727	2.4652	1.8182	1.7582	1.3332	2.8419

This table presents descriptive statistics for variables used in the main regression analysis. Table 2, panel A, reports descriptive statistics for stock price crash variables and controls. Table 2, panel B, reports information for inferior and superior voting shares. All variables are measured at the end of fiscal year. Inferior (Superior) refers to the inferior voting shares (superior voting shares). (1) shares = number of shares outstanding, (2) cash flow rights (%) = the number of shares outstanding for that class dividend by the total number of shares outstanding for both classes of stock, (3) voting rights (%) = the number of each class's total votes (equal to their votes per share times the number of shares outstanding) divided by the sum of the total votes for both classes, and (4) voting rights to cash flow rights = the ratio of voting rights (%) to cash flow rights (%). Other variable definitions are provided in the appendix.

5.1 MAIN RESULTS

Panels A and B of table 3 report the results of ordinary least squares (OLS) regressions, using *NEG_SKEWNESS* and *DOWN/UP_RET_SD*, respectively, as the dependent variable, while panel C of the same table presents the results of logit regressions using *CRASH* as the dependent variable. Throughout the paper, our dependent variable, one of three proxies for crash risk, is measured in year t, while our test variable, *WEDGE*, and a set of firm-specific control variables are all measured in year $t-1$, that is, one-year lagged. We split the total sample into the two subsamples of high- and low-opacity firms based upon the median value of *OPAQUE* in year $t-1$ for each country-year. In each panel, columns 1–4 report the results for the low-opacity subsample, and columns 5–8 report the results for the high-opacity subsample. We include year indicators across all columns.

Columns 1 and 5 show the results incorporating all firm-specific controls except lagged negative skewness (i.e., $NEG\_SKEWNESS_{t-1}$) and industry indicators, columns 2 and 6 include industry indicators, columns 3 and 7 additionally include anti-self-dealing measures (*ANTI-SD*) as a country-level control, and columns 4 and 8 include all firm-level controls including $NEG\_SKEWNESS_{t-1}$. The firm-level control variables are drawn from the past literature that examines firm-specific crash risk in U.S. firms (Chen, Hong, and Stein [2001], Hutton, Marcus, and Tehranian [2009], Kim, Li, and Zhang [2011a,b]), and include one-year lagged measures of: (1) the negative skewness of weekly firm-specific returns ($NEG\_SKEWNESS_{t-1}$), (2) de-trended share turnover ($TRADE\_VOLUME_{t-1}$), (3) return standard deviation ($RETURN\_SD_{t-1}$), (4) return ($RETURN_{t-1}$), (5) market capitalization ($MARKET\_CAP_{t-1}$), (6) market to book ratio (MB_{t-1}), (7) leverage ($LEVERAGE_{t-1}$), (8) return on assets (ROA_{t-1}), (9) operating cycle ($OPER\_CYCLE_{t-1}$), and (10) operating cash flow per dollar in sales ($CFO/SALES_{t-1}$). The appendix provides a detailed description of these control variables. In panels A and B, we report t-statistics in parentheses below each coefficient estimate, while in panel C, we report Wald chi-square statistics in parentheses below each coefficient estimate. We report one-tailed p-values when discussing coefficient estimates for our main test variables with directional predictions. Reported t-statistics in all three panels are based on robust standard errors corrected for heteroskedasticity and firm-level clustering.

For the high-opacity sample shown in columns 5–8 of panels A, B, and C of table 3, the ownership-control wedge, *WEDGE* in year $t-1$, is significantly and positively related to *NEG_SKEWNESS, DOWN/UP_RET_SD*, and *CRASH* in year t, respectively. For both the low- and high-opacity subsamples, we also report the economic impact of each explanatory variable for the full-model specification (in columns 4 and 8, respectively), that is, the marginal impact of a one standard deviation change in each variable on crash risk, with all other explanatory variables being held constant. This marginal impact helps us assess the economic significance of the estimated coefficients.

TABLE 3

The Effect of the Ownership-Control Wedge on Stock Price Crash Risk

Panel A: OLS regression of *NEG_SKEWNESS* on wedge

	Low Opacity				Economic	High Opacity				Economic
Model	(1)	(2)	(3)	(4)	Impact (4)	(5)	(6)	(7)	(8)	Impact (8)
WEDGE $_{t-1}$	**−0.0205**	**0.1244**	**0.1467**	**0.1183**	**0.0180**	**0.3601*****	**0.4705*****	**0.4745*****	**0.6220*****	**0.0928**
	(−0.13)	**(0.64)**	**(0.77)**	**(0.60)**		**(2.38)**	**(2.85)**	**(2.85)**	**(3.43)**	
NEG_SKEWNESS $_{t-1}$				0.4545***	0.5073				0.4082***	0.4600
				(7.12)					(9.56)	
TRADE_VOLUME $_{t-1}$	−0.1206***	8.2201	6.6426	8.6911	0.0461	1.1747	0.0936	−0.2840	1.3202	0.0071
	(−3.28)	(0.52)	(0.43)	(0.51)		(0.13)	(0.01)	(−0.03)	(0.12)	
RETURN_SD $_{t-1}$	0.0360	0.7644	0.7868	1.2844	0.0575	0.3201	−0.3024	−0.3002	0.3513	0.0156
	(0.23)	(0.77)	(0.80)	(1.28)		(0.43)	(−0.39)	(−0.38)	(0.44)	
RETURN $_{t-1}$	0.8775***	2.2226**	2.4007**	2.4573**	0.0769	−1.2111	−1.0967	−1.0273	−1.7265*	−0.0528
	(3.48)	(2.02)	(2.14)	(2.22)		(−1.33)	(−1.14)	(−1.06)	(−1.81)	
MARKET_CAP $_{t-1}$	0.0271***	0.0295***	0.0263**	0.0273**	0.0800	0.0261***	0.0250**	0.0244**	0.0194**	0.0534
	(2.77)	(2.83)	(2.54)	(2.47)		(3.10)	(2.55)	(2.45)	(1.99)	
MB $_{t-1}$	0.0058	0.0557**	0.0657**	0.0552**	0.0636	0.0022	−0.0076	−0.0050	−0.0133	−0.0149
	(1.11)	(1.98)	(2.28)	(1.97)		(0.09)	(−0.30)	(−0.20)	(−0.50)	
LEVERAGE $_{t-1}$	−0.1294	−0.2331	−0.2063	−0.2271	−0.0446	0.1963	0.0668	0.0634	−0.0611	−0.0112
	(−1.51)	(−1.07)	(−0.94)	(−0.98)		(1.23)	(0.39)	(0.37)	(−0.35)	
ROA $_{t-1}$	0.0182	−0.7210	−0.6938	−0.6834	−0.0372	1.0388*	1.0381*	1.0213*	0.6403	0.0328
	(0.04)	(−1.07)	(−1.03)	(−1.02)		(1.94)	(1.90)	(1.88)	(1.23)	
OPER_CYCLE $_{t-1}$	0.0272	−0.0993	−0.0944	−0.0827	−0.0406	0.0141	0.0794*	0.0841*	0.0892*	0.0455
	(0.58)	(−1.19)	(−1.15)	(−0.94)		(0.34)	(1.69)	(1.77)	(1.88)	
CFO/SALES $_{t-1}$	0.0295***	−0.7255**	−0.6794**	−0.7714**	−0.0761	−0.4806**	−0.6733***	−0.6617***	−0.7980***	−0.0796
	(7.38)	(−2.20)	(−2.04)	(−2.24)		(−2.09)	(−2.67)	(−2.63)	(−3.10)	

(*Continued*)

TABLE 3—*Continued*

Panel A: OLS regression of *NEG_SKEWNESS* on wedge

	Low Opacity				Economic	High Opacity				Economic
Model	(1)	(2)	(3)	(4)	Impact (4)	(5)	(6)	(7)	(8)	Impact (8)
STD_CFO_{t-1}	−0.0102	0.0046	0.0100	0.0022	0.0025	−0.0497	−0.0290	−0.0276	−0.0675	−0.0725
	(−0.53)	(0.07)	(0.15)	(0.03)		(−0.86)	(−0.46)	(−0.44)	(−1.08)	
ANTI-SD			0.3756	0.3151	0.0466			0.1040	0.1447	0.0219
			(1.53)	(1.27)				(0.56)	(0.81)	
Test of diff (High – Low)	***[1.53]****	***[1.41]****	***[1.71]*****	***[1.69]*****						
Year dummies	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	
Industry dummies	No	Yes	Yes	Yes		No	Yes	Yes	Yes	
Observations	1,623	1,623	1,623	1,623		1,727	1,727	1,727	1,727	
Adjusted-R^2	0.02	0.02	0.02	0.09		0.02	0.03	0.03	0.07	

Panel B: OLS regression of *DOWN/UP_RET_SD* on wedge

	Low Opacity				Economic	High Opacity				Economic
Model	(1)	(2)	(3)	(4)	Impact (4)	(5)	(6)	(7)	(8)	Impact (8)
$WEDGE_{t-1}$	**−0.0062**	**0.0602**	**0.0672**	**0.0556**	**0.0085**	**0.1761*** **	**0.2079*** **	**0.2080*** **	**0.2686*** **	**0.0401**
	(−0.09)	**(0.76)**	**(0.86)**	**(0.68)**		**(2.79)**	**(3.03)**	**(3.02)**	**(3.55)**	
$NEG\_SKEWNESS_{t-1}$				0.1839***	0.2053				0.1749***	0.1971
				(7.97)					(9.60)	
$TRADE\_VOLUME_{t-1}$	3.4761	2.7790	2.2787	3.1285	0.0166	−1.0423	−1.7686	−1.7709	−1.1627	−0.0063
	(0.53)	(0.41)	(0.34)	(0.43)		(−0.24)	(−0.41)	(−0.41)	(−0.24)	
$RETURN\_SD_{t-1}$	0.0852	0.0779	0.0707	−0.1331	0.0060	0.0285	0.0709	0.0709	0.2253	0.0100
	(0.24)	(0.21)	(0.19)	(0.36)		(0.10)	(0.22)	(0.22)	(0.68)	
$RETURN_{t-1}$	1.2141***	1.1731***	1.2296***	1.2517***	0.0392	−0.6294	−0.6869*	−0.6864*	−0.9620**	−0.0294
	(2.63)	(2.60)	(2.67)	(2.74)		(−1.64)	(−1.68)	(−1.65)	(−2.33)	

(*Continued*)

TABLE 3—*Continued*

Panel B: OLS regression of *DOWN/UP_RET_SD* on wedge

	Low Opacity				Economic	High Opacity				Economic
Model	(1)	(2)	(3)	(4)	Impact (4)	(5)	(6)	(7)	(8)	Impact (8)
MARKET_CAP$_{t-1}$	0.0143***	0.0124***	0.0114***	0.0118***	0.0346	0.0105***	0.0105***	0.0105**	0.0084**	0.0231
	(3.58)	(3.04)	(2.79)	(2.77)		(2.73)	(2.61)	(2.57)	(2.07)	
MB$_{t-1}$	0.0131	0.0168	0.0200*	0.0157	0.0181	0.0068	0.0074	0.0075	0.0049	0.0055
	(1.16)	(1.48)	(1.70)	(1.40)		(0.69)	(0.69)	(0.69)	(0.43)	
LEVERAGE$_{t-1}$	−0.1138	−0.1037	−0.0952	−0.1032	−0.0203	0.0337	−0.0189	−0.0190	−0.0706	−0.0129
	(−1.32)	(−1.11)	(−1.00)	(−1.04)		(0.54)	(−0.29)	(−0.29)	(−1.05)	
ROA$_{t-1}$	−0.2235	−0.1761	−0.1675	−0.1641	−0.0089	0.1228	0.0729	0.0728	−0.0757	−0.0039
	(−0.76)	(−0.63)	(−0.60)	(−0.60)		(0.57)	(0.33)	(0.33)	(−0.36)	
OPER_CYCLE$_{t-1}$	−0.0149	−0.0489	−0.0474	−0.0423	−0.0207	0.0170	0.0345*	0.0345*	0.0360*	0.0184
	(−0.58)	(−1.44)	(−1.41)	(−1.18)		(0.96)	(1.77)	(1.74)	(1.83)	
CFO/SALES$_{t-1}$	−0.2679**	−0.1832	−0.1686	−0.2061	−0.0203	−0.1952**	−0.2776**	−0.2775**	−0.3230***	−0.0322
	(−2.30)	(−1.36)	(−1.23)	(−1.48)		(−2.03)	(−2.56)	(−2.55)	(−2.87)	
STD_CFO$_{t-1}$	−0.0035	−0.0046	−0.0029	−0.0060	−0.0067	−0.0262	−0.0190	−0.0190	−0.0355	−0.0381
	(−0.13)	(−0.15)	(−0.09)	(−0.19)		(−1.06)	(−0.74)	(−0.74)	(−1.41)	
ANTI-SD			0.1192	0.0947	0.0140			0.0006	0.0232	0.0035
			(1.19)	(0.93)				(0.01)	(0.30)	
Test of diff (High – Low)	***[1.84]****	***[1.59]****	***[1.52]****	***[2.05]****						
Year dummies	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	
Industry dummies	No	Yes	Yes	Yes		No	Yes	Yes	Yes	
Observations	1,623	1,623	1,623	1,623		1,727	1,727	1,727	1,727	
Adjusted-R^2	0.02	0.03	0.03	0.09		0.02	0.03	0.03	0.06	

(*Continued*)

TABLE 3—*Continued*

Panel C: Logistic regression of *CRASH* on wedge

Model	Low Opacity (1)	(2)	(3)	(4)	Economic Impact (4)	High Opacity (5)	(6)	(7)	(8)	Economic Impact (8)
WEDGE$_{t-1}$	**0.4700**	**−0.0058**	**0.0293**	**−0.1032**	**−0.0157**	**1.4019*****	**1.1794****	**1.1897****	**1.3561*****	**0.2023**
	(1.16)	**(0.00)**	**(0.00)**	**(0.04)**		**(8.43)**	**(6.04)**	**(6.10)**	**(7.42)**	
NEG_SKEWNESS$_{t-1}$				0.7294***	0.8142				0.5065***	0.5708
				(40.66)					(28.65)	
TRADE_VOLUME$_{t-1}$	19.8447	36.1214	33.3659	31.5420	0.1672	−2.5676	1.4387	0.5310	3.3416	0.0180
	(0.87)	(1.69)	(1.43)	(1.25)		(0.01)	(0.00)	(0.00)	(0.02)	
RETURN_SD$_{t-1}$	3.0735*	4.7856**	4.7991**	5.9374***	0.2660	3.0254**	5.5153***	5.5235***	6.3292***	0.2804
	(3.06)	(5.49)	(5.53)	(7.32)		(4.02)	(10.04)	(10.04)	(12.45)	
RETURN$_{t-1}$	8.4139***	6.6857***	7.0305***	7.6682***	0.2400	−0.4232	−3.3495	−3.2480	−4.0443	−0.1238
	(16.41)	(8.68)	(9.49)	(10.00)		(0.03)	(1.60)	(1.48)	(2.14)	
MARKET_CAP$_{t-1}$	0.0565**	0.0603**	0.0526**	0.0612**	0.1793	0.0336	0.0332	0.0320	0.0283	0.0779
	(6.11)	(5.75)	(4.23)	(4.34)		(2.12)	(1.86)	(1.68)	(1.21)	
MB$_{t-1}$	0.0096	−0.0166	0.0047	−0.0180	−0.0207	−0.1945**	−0.1093	−0.1045	−0.1134	−0.1269
	(0.02)	(0.07)	(0.01)	(0.08)		(6.55)	(1.70)	(1.54)	(1.63)	
LEVERAGE$_{t-1}$	0.3425	0.6226	0.6843	0.6457	0.1269	0.0295	−0.3255	−0.3323	−0.4821	−0.0882
	(0.72)	(1.80)	(2.16)	(1.71)		(0.01)	(0.69)	(0.73)	(1.34)	
ROA$_{t-1}$	−0.0994	0.1201	0.1792	0.2278	0.0124	0.3673	−0.6246	−0.6623	−1.2277	−0.0630
	(0.01)	(0.01)	(0.02)	(0.03)		(0.07)	(0.17)	(0.19)	(0.65)	
OPER_CYCLE$_{t-1}$	0.0602	0.0373	0.0443	0.0698	0.0342	−0.1268	−0.0830	−0.0740	−0.0624	−0.0318
	(0.18)	(0.05)	(0.08)	(0.16)		(1.15)	(0.36)	(0.29)	(0.19)	
CFO/SALES$_{t-1}$	−1.2945*	−0.7639	−0.6699	−0.9052	−0.0893	−1.2074*	−0.2883	−0.2685	−0.3832	−0.0382
	(2.86)	(0.86)	(0.67)	(1.01)		(3.30)	(0.13)	(0.11)	(0.21)	
STD_CFO$_{t-1}$	0.1107	0.0534	0.0617	0.0601	0.0672	0.1015	0.0803	0.0808	0.0563	0.0605
	(1.09)	(0.20)	(0.26)	(0.23)		(0.82)	(0.47)	(0.47)	(0.21)	

(*Continued*)

TABLE 3—*Continued*

Panel C: Logistic regression of *CRASH* on wedge

	Low Opacity					High Opacity				
Model	(1)	(2)	(3)	(4)	Economic Impact (4)	(5)	(6)	(7)	(8)	Economic Impact (8)
ANTI-SD			0.7885	0.7503	0.1110			0.1995	0.2643	0.0399
			(2.03)	(1.63)				(0.22)	(0.37)	
Test of diff (High – Low)	*[2.89]**	*[3.47]**	*[3.50]**	*[3.71]**						
Year dummies	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes	
Industry dummies	No	Yes	Yes	Yes		No	Yes	Yes	Yes	
Observations	1,623	1,623	1,623	1,623		1,727	1,727	1,727	1,727	
Pseudo-adjusted-R^2	0.06	0.09	0.09	0.14		0.04	0.06	0.06	0.16	

This table presents the results of testing the association between the ownership-control wedge and stock price crash risk. In each year and each country, we partition the total sample into the low- and high-opacity subsamples using the sample median of *OPAQUE* per country in the previous year. The regressions are estimated separately for the low- and high-opacity subsamples. We use three proxies for the stock price crash as dependent variables in the analyses, that is, *NEG_SKEWNESS*, *DOWN/UP_RET_SD* and *CRASH*. *t*-statistics (Wald chi-square statistics) are reported in parentheses, based on standard errors corrected for heteroskedasticity and firm-level clustering for the *NEG_SKEWNESS* and *DOWN/UP_RET_SD* models (for the *CRASH* models). Economic impact is computed by the estimated coefficient on each variable in the full model times its standard deviation, and represents a change in a crash risk measure in response to a one standard deviation change in each independent variable with all other independent variables set to their mean values. The sample consists of 3,350 firm-year observations during the period from 1995 to 2007. Variable definitions are provided in the appendix.

***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively (one-tailed when the sign of coefficients are predicted, otherwise, two-tailed). The numbers on the *Test of diff (High – Low)* row represent *t*-statistics for testing the differences in the *WEDGE* coefficients between the high- and low-opacity samples. Bold text indicates variables/tests of interest.

For example, with respect to the high-opacity subsample, a one standard deviation increase in the ownership-control wedge (i.e., 14.92%) implies a 9.28%, 4.01%, and 20.23% (raw, not relative) increase in *NEG_SKEWNESS*, *DOWN/UP_RET_SD*, and *CRASH*, respectively.[18] These changes are economically significant, considering that the unconditional means of these variables are −5.97%, −7.60%, and 24.26%, respectively, in the high-opacity sample, as reported in panel A of table 2. These results are consistent with our first prediction, suggesting that, for the subsample of high-opacity firms, high-wedge firms are more crash-prone than low-wedge firms.

In contrast, for the low-opacity sample shown in columns 1–4 of panels A, B, and C, table 3, the coefficients on *WEDGE* are insignificant across all columns. Moreover, we test whether there are significant differences in the coefficient on *WEDGE* between regressions in the high-opacity sample (in columns 5–8) and those in the low-opacity sample (in columns 1–4, respectively). As indicated in test statistics (provided at the fifth row from the bottom of the table in columns 1–4), we find that the coefficients in columns 5–8 are significantly larger than those in columns 1–4, respectively, across all columns in all three panels of table 3. These results suggest that the wedge-induced agency conflict increases crash risk for high-opacity firms more so than for low-opacity firms.

As for the control variables, all three panels of table 3 show that, in general, lagged negative return skewness ($NEG\_SKEWNESS_{t-1}$) and firm size ($MARKET\_CAP_{t-1}$) are positively associated with crash risk, and the level of cash flow ($CFO/SALES_{t-1}$) is negatively associated with crash risk. Although it is challenging to make direct comparisons with prior studies due to differences in the sample and time period, these results are largely consistent with those reported by Chen, Hong, and Stein [2001], Hutton, Marcus, and Tehranian [2009], Kim, Li, and Zhang [2011a, b], and DeFond et al. [2015].

5.2 CROSS-SECTIONAL TESTS

Table 4 presents the results of our cross-sectional tests. We predict that three factors, namely, (1) the efficacy of external monitoring measured by a country's anti-self-dealing rules (*ANTI-SD*), (2) the strength of *firm-level* external monitoring by outside stakeholders measured by analyst coverage (*ANALYSTS*), and (3) opportunity costs of consuming private control benefits measured by industry-level growth opportunities (*SALES_GROWTH*),

[18] For example, the estimated coefficient on *WEDGE* in the full model regression for the high-opacity sample with *NEG_SKEWNESS* as the dependent variable is 0.6220, as shown in column 8 of panel A, table 3, and the standard deviation for *WEDGE* (which equals one minus the ratio of voting rights to cash flow rights for inferior share) is 0.1492 for the high-opacity sample. Accordingly, the economic impact associated with a one standard deviation increase in *WEDGE* is 9.28% or 0.0928 = 0.6220*0.1492 while holding other covariates constant. Economic impacts using other crash measures as the dependent variable can be computed in a similar way.

TABLE 4

The Ownership-Control Wedge, Opacity and Stock Price Crash Risk, Conditional on the Strength of External Monitoring, and the Opportunity Costs of Private Benefit Consumption

Dependent Variable =	(1) *NEG_SKEWNESS*	(2) *DOWN/UP_RET_SD*	(3) *CRASH*
Panel A: Conditional on the strength of anti-self-dealing index			
WEDGE$_{t-1}$	1.6194***	0.4825**	0.2822***
	(3.97)	(2.63)	(3.17)
ANTI-SD	0.2486	0.0792	−0.0964**
	(1.72)	(1.24)	(−2.13)
WEDGE*$_{t-1}$**ANTI-SD***	**−2.0111*****	**−0.6185*****	**−0.2926****
	(−4.33)	**(−3.00)**	**(−1.96)**
Firm controls	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes
Observations	1,727	1,727	1,727
Adjusted-R^2 for *NEG_SKEWNESS* and *DOWN/UP_RET_SD*; Pseudo-R^2 for *CRASH*	0.09	0.06	0.12

Dependent Variable =	(1) *NEG_SKEWNESS*	(2) *DOWN/UP_RET_SD*	(3) *CRASH*
Panel B: Conditional on analyst following			
WEDGE$_{t-1}$	1.6568**	0.6404**	1.1312***
	(2.49)	(2.54)	(4.98)
ANALYST$_{t-1}$	−0.0736	−0.0446	−0.0718**
	(−0.59)	(−0.86)	(−2.18)
***WEDGE*$_{t-1}$** ***ANALYST*$_{t-1}$**	**−1.6215****	**−0.5814***	**−1.3201*****
	(−1.72)	**(−1.60)**	**(−3.53)**
Firm controls	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes

(Continued)

TABLE 4—*Continued*

Dependent Variable =	(1) *NEG_SKEWNESS*	(2) *DOWN/UP_RET_SD*	(3) *CRASH*
Panel B: Conditional on analyst following			
Industry dummies	Yes	Yes	Yes
Observations	1,727	1,727	1,727
Adjusted-R^2 for *NEG_SKEWNESS* and *DOWN/UP_RET_SD*; Pseudo-R^2 for *CRASH*	0.16	0.17	0.12

Dependent Variable =	(1) *NEG_SKEWNESS*	(2) *DOWN/UP_RET_SD*	(3) *CRASH*
Panel C: Conditional on sales growth			
$WEDGE_{t-1}$	0.9136**	0.3700***	0.0941***
	(2.46)	(2.76)	(3.99)
$SALE\_GROWTH_{t-1}$	0.0252	0.0245	−0.1603***
	(0.15)	(0.37)	(−2.46)
$\boldsymbol{WEDGE_{t-1}{}^{*}SALES\_GROWTH_{t-1}}$	**−1.1010****	**−0.4064****	**−0.0895*****
	(−1.78)	**(−1.90)**	**(−2.21)**
Firm controls	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes
Observations	1,727	1,727	1,727
Adjusted-R^2 for *NEG_SKEWNESS* and *DOWN/UP_RET_SD*; Pseudo-R^2 for *CRASH*	0.14	0.14	0.10

This table presents the results of testing the relation between the ownership-control wedge and stock price crash, conditional on external monitoring and the opportunity costs of consuming private benefits of control. In each year and each country, we partition the total sample into the low- and high-opacity subsamples using the sample median of *OPAQUE* per country in the previous year. The regressions are estimated separately for the low- and the high-opacity subsamples. *t*-statistics are reported in parentheses, based on standard errors corrected for heteroskedasticity for the *NEG_SKEWNESS* and *DOWN/UP_RET_SD* models. Wald chi-square statistics, reported in parentheses for the *CRASH* model, are based on standard errors adjusted by the procedure of Norton, Wang, and Ai (2004). In panel A, standard errors are clustered at the country level, while in panels B and C, standard errors are clustered at the firm level. The sample consists of 1,727 firm-year observations during the period from 1995 to 2007 in the high-opacity subsample. Variable definitions are provided in the appendix.

***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively (one-tailed when the sign of coefficients are predicted; otherwise, two-tailed). Bold text indicates variables/tests of interest.

play a role in shaping the relation between the ownership-control wedge and crash risk when firms are opaque. Panels A, B, and C show the results for *ANTI-SD, ANALYSTS,* and *SALES_GROWTH*, respectively. As we discussed earlier, we present these results for the high-opacity subsample only, because that is the sample where the wedge-crash risk association is predicted by JM, and these tests examine variables expected to moderate that association.[19] The full results, including the estimated coefficients on all control variables, of these tests for the low-opacity sample are provided in the online appendix (table A2). In panels A–C of table 4, for brevity, we report the results only for the variables of interest. The full results including control variable coefficients for the high-opacity sample are also reported in the online appendix (table A3). Ai and Norton [2003] and Norton, Wang, and Ai [2004] show that both the marginal effects and standard errors of interaction terms in logit or probit models are biased, and introduce an adjustment method to correct for these biases. Thus, throughout the paper, including the online appendix, we report our results by applying the adjustment procedure suggested by Ai and Norton [2003] and Norton, Wang, and Ai [2004] to estimate the coefficients and standard errors of the interaction terms in the logit models with *CRASH* as the dependent variable.[20]

In panel A, table 4, we find that the interaction variable, *WEDGE∗ANTI-SD,* is negatively associated with all three measures of crash risk at less than the 1% level in the first two columns and at less than the 5% level in the last column.[21] This result indicates that, when firms are opaque, anti-self-dealing rules constrain managerial extraction of private control benefits associated with the wedge, and therefore, attenuate the strength of the positive association between the ownership-control wedge and crash risk.

As shown in panel B, table 4, we find that the coefficients on the interaction variable, *WEDGE∗ANALYST,* are all negative and significant at less than the 5% and 10% levels in columns 1 and 2, respectively, and at less than the 1% level in column 3. The results suggest that stronger external monitoring, proxied by greater analyst following, increases the costs to controlling insiders of consuming private control benefits, thereby leading to a

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[19] In this and other cross-sectional results presented in this paper and accompanying online appendix, we de-mean *WEDGE* when it is interacted with another variable. We do this because our sample contains no firms with $WEDGE = 0$, but this would be the main effect reported for the variable interacted with *WEDGE* if we did not de-mean *WEDGE*. After de-meaning *WEDGE*, the main effect of the variable that is interacted with *WEDGE* is the effect of that variable at the sample mean of *WEDGE*, not at $WEDGE = 0$.

[20] Ai and Norton [2003] and Norton, Wang, and Ai [2004] show that both the marginal effects and standard errors of interaction terms in logit or probit models are biased if the standard logit procedure is applied, and introduce an adjustment method to correct for these biases. We find, however, that our main inferences remain unaltered when the OLS method is applied. The primary difference is that *WEDGE* loses significance in the high-opacity sample and *WEDGE* becomes significantly positive and *WEDGE∗IFRS_Enforce* becomes significantly negative in the low-opacity sample in table 5.

[21] When a country-level interaction variable is included, *t*-statistics are based on robust standard errors clustered by country. Otherwise, standard errors are clustered by firm.

weaker relation between the wedge and crash risk across all three measures of crash risk.

Panel C of table 4 reports similar results for the coefficients on *WEDGE*SALES_GROWTH*. We find that this coefficient is significant at less than the 5% level in columns 1 and 2 and at less than the 1% level in column 3, suggesting that controlling insiders of higher growth opportunity firms engage less in the consumption of private control benefits, even when firms are opaque. The finding is in line with the view that the opportunity costs associated with the consumption of private control benefits (e.g., forgone investment opportunities) are higher when growth opportunities are higher, which, in turn, weakens the positive relation between *WEDGE* and crash risk.[22]

5.3 SENSITIVITY CHECKS

Our analysis thus far should be interpreted as documenting an *association* between the ownership-control wedge and firm-level crash risk and highlighting the role of opacity in shaping this link. Caution should be exercised in making any causal inferences based on our results, particularly because our research design does not rule out concerns about potential correlated omitted variables and errors in variable measurement. To alleviate these concerns, we conduct a variety of sensitivity tests, including using mandatory IFRS adoption as an exogenous change to financial reporting at the firm level (subsection 5.3.1), changing the econometric methods chosen (subsection 5.3.2), changing how we measure key variables (subsections 5.3.3, 5.3.4, and 5.3.5), the use of a matched sample (subsection 5.3.6), controlling for additional variables (subsection 5.3.7), alternative measures of firm-level monitoring and the opportunity costs of consuming private control benefits (subsection 5.3.8).

5.3.1 IFRS Adoption as an Exogenous Change to Financial Reporting at the Firm Level. To tighten our identification, we take advantage of mandatory IFRS adoption in 2005, along with concurrent regulatory changes, by European Union (EU) member countries and other countries. We view the IFRS mandate and concurrent regulatory changes as exogenous changes to financial reporting at the firm level that possibly lead to a shift in the wedge-crash risk relation from the pre- to the post-IFRS period, especially for firms that were opaque prior to mandatory IFRS adoption.[23] Our sample period of 1995–2007 spans the mandatory IFRS adoption in 2005 by listed firms in EU member countries as well as Australia and South Africa. The IFRS

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[22] As discussed earlier, the opacity that exists in high-growth firms could stem from controlling insiders' effort to avoid proprietary costs rather than to obscure private benefit consumption, and this sort of opacity would not contribute to crash risk in JM.

[23] The adoption of IFRS and related regulatory changes are exogenous changes to firms, but not to countries that choose to make these regulatory modifications.

mandate provides us with a quasi-natural experimental setting in which to apply a difference-in-differences (DiD) regression design.[24]

To implement the DiD regression design, we divide the total sample into two subsamples based on the level of ex ante financial reporting opacity in the pre-IFRS period. We describe the details and report results of two of these tests in the online appendix. First, as shown in the online appendix, table A4, we find that for the high-opacity sample the coefficient on the three-way interaction term, *WEDGE∗MANDTORY∗POST*, is negative and significant at less than the 5% level across all columns. This finding suggests that for firms that were opaque prior to the IFRS mandate, the positive relation between the ownership-control wedge and crash risk becomes weakened significantly for IFRS-adopting firms from the pre-IFRS period to the post-IFRS period, compared with the corresponding effect for non-IFRS adopters for the same period.

Further, Christensen, Hail, and Leuz [2013] report capital market effects of IFRS adoption that appear in countries that change their level of financial reporting enforcement along with IFRS adoption. To examine whether the results of IFRS adoption in our sample are concentrated in these countries, we repeat our earlier analysis of IFRS adoption after including the two indicator variables, *IFRS_Enforce* and *IFRS_NoEnforce*, along with their interactions with our test variable *WEDGE*. These indicator variables distinguish IFRS adopter firms from countries with enforcement changes bundled with IFRS adoption (*IFRS_Enforce* = 1) versus IFRS adopters from countries that did not have enforcement changes bundled with IFRS adoption (*IFRS_NoEnforce* = 1) in the post-IFRS period (Chirstensen, Hail, and Leuz [2013]). The values of *IFRS_Enforce* and *IFRS_NoEnforce* are zero in the pre-IFRS period. We then estimate this augmented regression, separately, for the low- and high-opacity subsamples. As shown in table 5, we find that the coefficients on *WEDGE∗IFRS_Enforce* are negative and significant at less than the 5% level for all three cases only for the high-opacity subsample; it is insignificant for two cases and significant at the 10% level with a negative sign in one case for the low-opacity sample. This suggests that the concurrent regulatory changes bundled with the IFRS mandate have a moderating impact on the positive wedge-crash risk relation when firms are relatively opaque, but not when firms are relatively transparent.

Finally, as shown in the online appendix, table A5, we find that the positive wedge-crash risk relation for the high-opacity sample is moderated after staggered changes in security regulations in countries with such changes. Specifically, these tests show that the positive relation between the wedge

[24] It should be noted that the focus of our analysis here is not to rehash the debate about the consequences of mandatory IFRS adoption, but to examine whether the wedge-crash risk relation for the IFRS adopter sample becomes weakened significantly from the pre- to the post-IFRS period, compared with the same change for the nonadopter sample during the same period.

TABLE 5

The Ownership-Control Wedge, Stock Price Crash Risk: Enforcement Changes Concurrent with IFRS Adoption (Christensen, Hail, and Leuz [2013])

	Low Opacity			High Opacity		
Dependent Variable =	*NEG_SKEWNESS*	*DOWN/UP_RET_SD*	*CRASH*	*NEG_SKEWNESS*	*DOWN/UP_RET_SD*	*CRASH*
$WEDGE_{t-1}$	0.0667	0.0003	−0.0681	0.6004***	0.2763***	0.1163*
	(0.49)	(0.00)	(−0.95)	(5.14)	(5.27)	(1.85)
IFRS_Enforce	0.0502	0.0324	0.0129	−0.0577	−0.0389	−0.0471
	(0.43)	(0.79)	(0.27)	(−0.55)	(−0.97)	(−0.65)
$WEDGE_{t-1}$* IFRS_ Enforce	**−0.3588**	**−0.2383***	**−0.1478**	**−0.6185****	**−0.2997*****	**−0.2584****
	(−1.35)	**(−1.97)**	**(−0.98)**	**(−2.79)**	**(−3.83)**	**(−2.14)**
IFRS_NoEnforce	−0.1015	−0.0503	−0.0738	0.0324	0.0135	0.0013
	(−0.92)	(−1.15)	(−1.15)	(0.27)	(0.26)	(0.02)
$WEDGE_{t-1}$* *IFRS_ NoEnforce*	0.3430	0.0773	0.0774	−0.0874	0.0053	−0.1903*
	(1.26)	(0.67)	(0.49)	(−0.35)	(0.05)	(−1.82)
$NEG\_SKEWNESS_{t-1}$	0.3874***	0.1481***	0.0691***	0.3353	0.1381	0.0935***
	(6.49)	(7.32)	(6.11)	(1.53)	(1.65)	(5.67)
$TRADE\_VOLUME_{t-1}$	−1.2914***	−0.6701***	−1.0114***	4.5821	−0.0659	3.1318
	(−6.80)	(−6.30)	(−2.69)	(0.69)	(−0.02)	(1.11)
$RETURN\_SD_{t-1}$	0.0264	−0.0431	0.5035**	1.1740***	0.3853***	0.8661***
	(0.17)	(−0.80)	(2.04)	(3.02)	(3.49)	(4.52)
$RETURN_{t-1}$	−0.0512	0.0495	0.1152	1.5941	0.4034	0.0168
	(−0.37)	(1.06)	(0.58)	(1.58)	(1.03)	(0.06)
$MARKET\_CAP_{t-1}$	0.0031	0.0065	−0.0144	0.0647**	0.0300***	−0.0003
	(0.32)	(1.54)	(−1.60)	(2.55)	(3.24)	(−0.02)
MB_{t-1}	0.0017	0.0010	0.0045***	0.0039	−0.0007	−0.0112
	(0.39)	(0.53)	(2.74)	(0.13)	(−0.05)	(−1.16)
$LEVERAGE_{t-1}$	−0.1067	−0.0536	0.0186	0.0336	−0.0105	−0.0861
	(−1.70)	(−1.49)	(0.86)	(0.21)	(−0.15)	(−1.45)

(*Continued*)

TABLE 5—*Continued*

	Low Opacity			High Opacity		
Dependent Variable =	*NEG_SKEWNESS*	*DOWN/UP_RET_SD*	*CRASH*	*NEG_SKEWNESS*	*DOWN/UP_RET_SD*	*CRASH*
ROA_{t-1}	0.0474	0.0140	−0.1246	0.2074	−0.0179	−0.1030
	(0.15)	(0.10)	(−0.85)	(0.49)	(−0.09)	(−0.53)
$OPER\_CYCLE_{t-1}$	0.0342	0.0079	0.0115	0.0233	0.0182	−0.0189
	(0.35)	(0.26)	(0.70)	(0.31)	(0.71)	(−0.96)
$CFO/SALES_{t-1}$	0.0432***	0.0165***	0.0338**	−0.6151**	−0.2989**	−0.0935
	(3.17)	(6.21)	(2.09)	(−2.41)	(−2.57)	(−0.96)
STD_CFO_{t-1}	−0.0206*	−0.0101**	−0.0055	0.0414	0.0183	0.0040
	(−1.90)	(−2.89)	(−0.73)	(0.76)	(0.91)	(0.41)
Test of diff (High – Low)	***[0.21]***	***[0.27]***	***[0.00]***			
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes	Yes	Yes
Country dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,628	1,628	1,628	1,678	1,678	1,678
Adjusted-R^2 for *NEG_SKEWNESS* and *DOWN/UP_RET_SD*; Pseudo-R^2 for *CRASH*	0.76	0.77	0.23	0.64	0.65	0.46

This table presents the results of testing whether concurrent securities regulation changes influences the relation between the ownership-control wedge and stock price crash risk. The sample consists of 3,306 firm-year observations. We partition the total sample into the low- and high-opacity subsamples using the sample median of *OPAQUE* for each country in the pre-enforcement period. The regressions are estimated separately for the low- and high-opacity subsamples. *IFRS_ENFORCE* is an indicator, which takes the value of one for mandatory IFRS adopters from EU countries that bundled IFRS adoption with substantive changes in enforcement in the post-IFRS period, zero otherwise. *IFRS_NOENFORCE* is an indicator, which takes the value of one for mandatory IFRS adopters from EU and other countries with no such substantive changes in enforcement in the post-IFRS period, zero otherwise (Christensen, Hail, and Leuz [2013]). *t*-statistics are reported in parentheses, based on standard errors corrected for heteroskedasticity and clustered at the country level for the *NEG_SKEWNESS* and *DOWN/UP_RET_SD* models. Wald chi-square statistics, reported in parentheses for the *CRASH* model, are based on standard errors adjusted by the procedure of Norton, Wang, and Ai [2004]. The numbers on the *Test of diff (High – Low)* row represent *t*-statistics for testing the differences in the $WEDGE_{t-1} * IFRS\_Enforce$ coefficients between the high- and low-opacity samples. Variable definitions are provided in the appendix.

***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively (one-tailed when the sign of coefficients are predicted, otherwise, two-tailed). Bold text indicates variables/tests of interest.

and crash risk for the high-opacity sample becomes weaker after adoption of the Market Abuse Directive (MAD). This securities regulation is implemented to reduce insider dealing and market manipulation, and improve transparency in the EU capital market. The above findings are consistent in spirit with those of Christensen, Hail, and Leuz [2016], who document a significant increase in market liquidity after MAD adoption in the European Union. In summary, the above findings, taken together, lend further support to JM's main prediction that the positive impact of the wedge on crash risk is attenuated in a less opaque environment, characterized by more transparent disclosure, stronger enforcement, and/or better regulation.

5.3.2 Hazard Model Tests. JM suggest that past crash history may be a potentially important factor determining the likelihood of future crash occurrences. Given that crashes cause all (hidden and accumulated) negative information to be released all at once, the likelihood of another crash immediately after a crash is zero. As noted by Kim, Li, and Zhang [2011a,b] and Kim and Zhang [2016], a proportional hazard model approach could be more appropriate in examining the impact of the ownership-control wedge on stock price crashes, because it naturally controls for the past history of crash occurrences.

Following their lead, we test the robustness of our main results using the Cox [1972] proportional hazard model.[25] Specifically, we estimate the following hazard model:

$$\begin{aligned} lnh_{jk}\ (t) = {} & \mu\left(t - t_{j(k-1)}\right) + \beta_1 WEDGE_{jk} \\ & + \sum_{i=2}^{n} \beta_i \left(i\text{th}\, Control\ Variable_{jk}\right) + \varepsilon_{jk}, \end{aligned} \tag{5}$$

where $h_{jk}(t)$ is the hazard, or instantaneous likelihood of crash occurrence, for firm j at time t, conditional on the fact that firm j has had k crashes by time t; $t_{j(k-1)}$ is the time of the $(k{-}1)$th event; and μ is an unspecified function that represents the baseline hazard. We predict $\beta_1 > 0$ in the high-opacity subgroup, indicating that the hazard of crash occurrences increases with the ownership-control wedge in the opaque reporting environment.

The results of the hazard model approach are reported in the online appendix (table A6). We find that the coefficient on *WEDGE*, that is, β_1, is significantly positive for the high-opacity sample, while it is insignificant for the low-opacity sample. Overall, the hazard-model results are in line with the logistic regression results reported in table 3, suggesting that the instantaneous crash likelihood increases significantly with the ownership-control wedge in the high-opacity sample, but not in the low-opacity sample.

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[25] See Kim and Zhang [2016] for a more detailed discussion of the Cox proportional hazard model and its application for prediction of future crash occurrences.

5.3.3 Alternative Measures of Crash Risk. We also examine two other measures of crash risk, *COUNT* introduced by JM and *Extra_SIGMA* introduced by Bradshaw et al. [2010]. *COUNT* is the difference in frequencies between the negative and positive jumps of firm-specific returns. Following JM, we compute *COUNT* as the difference between the frequency that firm-specific weekly returns fall 3.0 standard deviations below the mean firm-specific weekly return and the frequency that firm-specific weekly returns fall 3.0 standard deviations above the mean firm-specific weekly return over a fiscal year.[26]

Additionally, we also compute a fifth measure of crash risk that captures crash magnitude, not just crash incidence. Following Bradshaw et al. [2010], $Extra\_SIGMA_{jt}$ is defined as the number of standard deviations of weekly firm-specific return for fiscal year t (i.e., W_{it}) by which the worst firm-specific weekly return in year t falls below the mean firm-specific weekly return over year t:

$$Extra\_SIGMA_{it} = -\text{Minimum}\left(\frac{W_{j\tau} - \text{Mean of } W_{i\tau} \text{ over year } t}{\text{SD of } W_{i\tau} \text{ over year } t}\right). \quad (6)$$

In equation (6), we take the negative of the minimum so that larger values of $Extra\_SIGMA_{jt}$ indicate more severe price crashes for firm i in year t. We then reestimate the regression reported in table 3, using these two measures of crash risk as the dependent variables.

The results using the above two alternative measures of crash risk, that is, *Extra SIGMA* and *COUNT,* are reported in the online appendix (table A7). We find that for the high-opacity sample *WEDGE* is significantly positively related to these two alternative measures, while for the low-opacity sample it is insignificant. The findings suggest that our main results are robust to the use of alternative crash risk measures.

5.3.4 Alternative Measure of Private Benefits of Control. We use the voting premium attached to superior voting shares as an alternative measure of the private benefits of control in dual-class firms. Due to the additional voting power associated with superior voting shares, the price of these shares at dual-class firms is generally higher than that of inferior voting shares. This price difference is referred to as the voting premium. On a regular trading day, inferior and superior voting shares are traded among generic shareholders who have not had an opportunity to consume control benefits. The voting premium measures the value that market participants place on the additional votes attached to superior shares.[27] Thus, the voting premium

[26] Recall that firm-specific weekly return for firm i in week t (i.e., W_{it}) is defined as $W_{it} = \ln(1 + \varepsilon_{it})$ where ε_{it} refers to residual (firm-specific) returns obtained from estimation of equation (1). The 3.0 standard deviations is chosen to generate the critical value of 0.1% in the W_{it} distribution.

[27] Under some reasonable assumptions on the probability of a control contest, the voting premium reflects a lower bound on the extent of private control benefits, because the market

can be viewed as a market-based proxy for the size of private control benefits for the controlling party. In this study, the voting premium is defined as (Zingales [1995], Doidge, [2004], Masulis, Wang, and Xie [2009]):[28]

$$VOTING\ PREMIUM = \frac{P_S - P_I}{P_S - rv * P_I}, \tag{7}$$

where:

P_S = the market closing price of a firm's superior voting shares;
P_I = the market closing price of a firm's inferior voting shares; and
rv = the ratio of the number of votes of an inferior voting share to that of a superior voting share.

We replace the ownership-control wedge with the voting premium in the regressions that explain our three measures of stock price crash risk. As shown in the online appendix, table A8, we find that the voting premium is positively associated with all three measures of crash risk in the high-opacity sample at less than the 1% level in columns 4 and 5 and at the 10% level in column 6.[29] In the high-opacity sample, a one standard deviation increase in the voting premium leads to a raw increase of 3.15%, 1.41%, and 4.50% in *NEG_SKEWNESS, DOWN/UP_RET_SD*, and *CRASH,* respectively.[30] These magnitudes are economically meaningful, considering that the unconditional means of these three crash measures are −5.97%, −7.60%, and 24.26%, respectively, for the high-opacity sample (as shown in table 2, panel A). For the low-opacity sample, we find that the voting premium is insignificant for all measures of crash risk. We also test whether the coefficients on *VOTING_PREMIUM* differ significantly between the high- and low-opacity samples. As indicated in the test statistics (provided at the fifth row from the bottom of the table), we find that the *VOTING_PREMIUM* coefficient

participants who buy superior voting shares but do not gain control of the company will only realize the value of their superior votes in the event of a future control contest (Zingales [1994, 1995], Doidge [2004]). Zingales [1995] cites three cases in which there were changes in the distribution of voting power. In each case, the premium associated with the superior voting shares surged around the respective event. These cases are: the unexpected death of the largest shareholder (William Crosby) at Resorts International, a conflict among the Wang family at Wang Laboratories, and the largest shareholder's decision to exchange his stock holdings for assets because of differences of view with the board of directors at Moog Inc.

[28] Since the voting premium is normalized by the ratio of the number of votes of an inferior voting share to the number of votes of a superior voting share, the voting premium in equation (7) accounts for these differences in dual-class structure.

[29] Recall that, when the binary variable, *CRASH,* is used as the dependent variable, the numbers in parentheses represent Wald chi-square statistics.

[30] As in panel B of table 2, the standard deviation of voting premium is 0.5746 for the high-opacity sample. The estimated coefficient on *NEG_SKEWNESS* is 0.0549 or 5.49% for the high-opacity sample, as shown in table A8. Therefore economic impact associated with a one standard deviation increase in voting premium is 3.15% or 0.0315 = 0.0549*0.5746 while holding other covariates constant. Economic impact using other crash measures can be computed in a similar way.

is significantly larger for the high-opacity sample than for the low-opacity sample except when *CRASH* is the dependent variable. In short, the results using *VOTING_PREMIUM* as an alternative proxy for the agency conflict are, overall, consistent with our main results using *WEDGE* (in table 3).

5.3.5 Alternative Measures of Opacity. We also separate our total sample into high- and low-opacity samples using two alternative proxies for opacity, that is: (1) raw total accruals and (2) an earnings opacity proxy by Hutton et al. [2007], which is defined as the previous three years' moving sum of the absolute value of discretionary accruals. Specifically, $OPAQUE_t = |DiscAcc|_{t-1} + |DiscAcc|_{t-2} + |DiscAcc|_{t-3}$, where DiscAcc is measured using the modified Jones model as specified in Dechow, Sloan, and Sweeney [1995]. The coefficients in the modified Jones model are estimated by year and Fama and French 48 industries at the global level. We find that our results are robust to the use of these new partitioning variables. Finally, we use, as the partitioning variable, the country-level median of *OPAQUE,* which is based on our earnings smoothing variable, using all firms, not just dual-class firms, to form the low- and high-opacity samples. The results of these tests are presented in the online appendix, table A9, panels A, B, and C. In each panel, our main inferences are not affected by these changes in the measurement of opacity. Crash risk increases significantly with the ownership-control wedge for the high-opacity sample, but not for the low-opacity sample, irrespective of how opacity is measured.

5.3.6 Using a Matched Sample of Firms Without Dual-Class Shares as a Benchmark. Our analysis thus far relies on the sample of firms with dual-class shares, without considering those with single-class shares, to examine the impact on crash risk of the agency conflict associated with the ownership-control wedge. In this subsection, we construct a matched sample of both dual-class and single-class firms to address two distinct, but related, issues.

One concern is with potential omitted variables associated with the dual-class structure. The dual-class structure is potentially chosen by managers who are interested in reducing the costs of transferring corporate resources from outside shareholders to themselves and securing the consumption of private benefits of control. As a result, the existence of dual-class shares may be correlated with other firm characteristics that may be omitted from our analysis. Creating a matched sample of single-class firms that share fundamental firm characteristics with the dual-class firms in our sample helps to mitigate this concern.

In addition, as explained earlier in subsection 3.2, we note that an ownership-control wedge can arise not only from the deviation of voting rights from cash flow rights, but also from large insider ownership stakes. For example, both dual-class and single-class firms could have an insider-outsider agency conflict if controlling insiders hold enough common voting shares to exercise full control over key corporate decisions. In the case of single-class firms, divergence between ownership and control that is economically similar to that found in dual-class firms can be created through insiders' concentrated ownership. For example, when insiders control 51% of the voting and cash flow rights, they then have 100% control with only 51% of the voting rights. To separately test the divergence in ownership and control associated with the dual-class structure and that arising from large insider ownership stakes, our analyses in table A10 include in our regression models both the ownership-control wedge and insider ownership concentration (*INSIDE_OWN*) that is defined as the cash flow rights of insiders, as potential sources of ownership-induced agency conflicts.[31] Including *INSIDE_OWN* as an additional test variable allows for the possibility that large ownership stakes could result in agency conflicts between controlling and minority shareholders even when there is little or no explicit ownership-control wedge as defined in this paper.

We match dual-class firms in our sample with single-class firms, based on fundamental firm characteristics that are generally related to agency conflicts and opacity (e.g., Lang, Raedy, and Yetman [2003]). Specifically, we match dual-class firms in our sample to single-class firms by country, year, and industry group based on Campbell [1996]. We then partition the dual-class firms into quintiles based on size (total assets) and then select single-class firms within the same size quintile in the same country, year, and industry group. Among the multiple single-class firms meeting these matching criteria, we match the single-class firm with a combined rank of book-to-market (*MB*) and *OPAQUE* closest to that of the dual-class firm in the same quintile. The literature shows a negative association between the wedge and firm value (as captured by *MB*, Claessens, Djankov, and Lang [2000], Lemmon and Lins [2003], Lins [2003]). Such a negative correlation indicates that outside shareholders at firms with a higher wedge are subject to expropriation risk. Thus, matching on *MB* and *OPAQUE* should identify single-class firms with similar agency conflicts and information opacity to our dual-class firms.

Table A10, panel A, in the online appendix provides the covariate balance between the treated dual-class firms ($N = 3{,}350$) and the matched single-class firms ($N = 3{,}350$). We conduct formal tests for differences in means between the dual-class and matched single-class samples, using t-tests, and find that the sample of dual-class firms is, overall, similar to the sample of matched single-class firms except for *RETURN*. This suggests that our matching procedures were successful in randomizing various characteristics between the two samples.

[31] There is an additional benefit of creating a matched sample of single-class firms. One of the concerns about our sample consisting of only dual-class firms is that many of these dual-class firms have inferior shares with no voting rights and therefore a *WEDGE* equal to one. This reduces the variation in *WEDGE* for this sample of firms, which may, in turn, decrease the power of our tests. In the preceding subsection, we address this concern, to some extent, by performing a sensitivity test using the voting premium as an alternative measure of the agency conflict arising from dual-class firms. Another way to increase the variation in *WEDGE* in our sample is to include comparable single-class firms that have no dual-class shares, and therefore, have *WEDGE* equal to zero based on our definition.

Panel B of table A10 shows our main regression results using the matched sample of both dual- and single-class firms. We find that the coefficients on *WEDGE* are positive and significant at less than the 1% level across all three columns for the high-opacity sample. For the low-opacity sample, however, the same coefficients are either insignificant (with *NEG_SKEWNESS* and *DOWN/UP_RET_SD* as the dependent variable) or positively significant at the 1% level (with *CRASH* as the dependent variable). As indicated in the test statistics (provided at the fifth row from the bottom of the table), we find that the differences in the *WEDGE* coefficients between the two samples are significant at the 1%, 5%, and 10% levels when *NEG_SKEWNESS, DOWN/UP_RET_SD,* and *CRASH,* respectively, are used as the dependent variables. The use of the matched sample of both dual-class firms and single-class firms does not alter the economic magnitude of the results.[32] The coefficient on inside ownership (*INSIDE_OWN*) is consistently negative and insignificant across five of six cases (it is negative and significant at less than the 10% level when *DOWN/UP_RET_SD* is the measure of crash risk for the high-opacity sample). Inside ownership per se does not appear to be associated with agency conflicts that lead to crash risk in this matched sample.[33]

5.3.7 Controlling for Additional Country-Level and Firm-Level Determinants of Dual-Class Structure and Opacity. As an additional test to alleviate concerns over correlated omitted variables, we collect a wide range of country- and firm-level variables that the past literature finds could be associated with dual-class share structures or financial reporting opacity, but are not included in our baseline specification in table 3. To conserve space, we do not reproduce the details of this extensive list of variables here but the notes accompanying table A11 in the online appendix provide the detailed definitions of these new country-level and firm-level variables. We include three new country-level institutional variables, *LAWENFORCE, CAPITAL MKT DEVELOPMENT,* and *ANTITAKEOVER.* In addition, we include a country-level control for the extent of insider ownership in a sample country (*OWNERSHIP).* We also include seven additional firm-specific variables (*SALES GROWTH, AUDIT FEE, WW_ETR, DIV_YIELD, M&A FREQ, M&A RETURN,* and *IFRS*) that capture operating, auditing, tax, and investment related characteristics that could be correlated to firms' ownership structure or opacity as well as the above four country-level variables. These

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[32] In the high-opacity subsample, a one standard deviation increase in the wedge (14.92%) leads to a 2.61%, 1.13%, and 13.13% raw increase in *NEG_SKEWNESS, DOWN/UP_RET_SD,* and *CRASH,* respectively.

[33] In our sample of matched single-class firms, 58.65% of the observations have an inside owner with greater than a 50% ownership stake. Perhaps not surprisingly, single-class firms that exhibit firm characteristics that are similar to dual-class firms are typically firms with large insider ownership stakes that could give rise to agency conflicts. The result is a sample of matched single-class firms with limited variation in the size of inside ownership stakes that could be poorly suited to test the effects of inside ownership per se on crash risk.

variables are drawn from the past literature (Francis, Schipper, and Vincent [2005], Khalil, Magnan, and Cohen [2008], McGuire, Wang, and Wilson [2014], Jordan, Liu, and Wu [2014], Holmen and Nivorozhkin [2007], Barth, Landsman, and Lang [2008]).

As shown in the online appendix, table A11, the inclusion of these additional controls does not alter statistical inferences on the *WEDGE* coefficients in the high-opacity sample as these coefficients are all positive and highly significant in table A11 for all three crash risk measures. These coefficients are insignificant across all three columns in the low-opacity sample. We also find that the differences in the *WEDGE* coefficients between the two samples are significant at less than the 5% level, for all crash risk measures. The above findings lend further support to our earlier results and help alleviate concerns that may arise from correlated omitted variables.[34]

5.3.8 Alternative Measures of External Monitoring and the Opportunity Costs of Consuming Private Control Benefits. Recall that table 4 shows results using analysts following as a firm-level proxy for external monitoring and sales growth as a proxy for the opportunity costs of consuming private control benefits. As a robustness check, we repeat this analysis using: (1) institutional ownership as a proxy for firm-level external monitoring; and (2) product market competition, measured using the Herfindahl-Hirshman Index, which is multiplied by negative one, as a proxy for the costs of consuming private control benefits. The Herfindahl-Hirshman Index is based on the sales of all firms with data available in Compustat Global for non-U.S. firms and Compustat America for U.S. firms, defined as $H = \sum_{i=1}^{n} (\Pi_i)^2$, where Π_i is the market share of company i, and n is the number of firms in the industry for a given country. Table A12 in the online appendix shows that these variables produce similar inferences to our results in table 4, though the *WEDGE∗MARKET_COMP* interaction is insignificant when *CRASH* is the dependent variable.

6. Conclusion

This study investigates whether and how the deviation of cash flow rights (ownership) from voting rights (control), or simply an ownership-control wedge, along with financial reporting opacity, influences future stock price crash risk. Using a comprehensive panel data set of firms with a dual-class share structure from 20 countries during the period of 1995–2007, we find that the effect of agency conflicts in dual-class firms on crash

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[34] As pointed out by Demsetz and Villalonga [2001], there is evidence suggesting that measures of ownership structure have a nonlinear relation with firm value measured by Tobin's *Q*. This nonlinear relation may reflect the effects of other variables correlated with ownership structures. While their discussion is not specific to the ownership-control wedge, it suggests that our results should be cautiously interpreted, because we cannot ensure that our attempts to address this issue sufficiently resolve these concerns.

risk differs systematically between high- and low-opacity firms. For high-opacity or opaque firms, we find a significantly positive relation between the ownership-control wedge, which is a proxy for the severity of agency conflicts in dual-class firms, and several measures of stock price crash risk. For low-opacity or transparent firms, however, we find that the same relation is insignificant across most cases we analyze. Our finding is consistent with the JM [2006] prediction that stock prices are more prone to crashes when the agency conflicts between corporate insiders and outside stakeholders are combined with information opaqueness. We further find that the positive relation between the wedge and crash risk when firms are opaque is moderated by a country-level investor protection, firm-level external monitoring, and the opportunity costs of consuming private control benefits.

We also perform a variety of sensitivity tests and supplemental analyses, including using mandatory IFRS adoption as an exogenous shock to opacity, an alternative econometric method, alternative definitions of key variables, the use of a matched sample, the inclusion of additional controls, and alternative measures of external monitoring and the opportunity costs of consuming private control benefits. We find that our main results are robust to all these additional analyses.

We admit that our research cannot rule out all concerns about omitted variables correlated with ownership structure and opacity, even with our extensive sensitivity tests. For example, our measure of opacity is necessarily indirect and is based on properties of accounting data that could be associated with unobserved firm characteristics other than controlling insiders' attempts to obscure the consumption of private control benefits. Given that our empirical tests cannot establish causal links unambiguously, our results should be interpreted as an indication that an association exists between the ownership-control wedge and opacity and crash risk. Our empirical tests rely on an assumption that, in some sample firms, the dual-class structure is associated with agency conflicts and that these conflicts produce incentives for opaque reporting. While we believe that this assumption is supported by the past literature, our research cannot verify the accuracy of this assumption. We therefore continue to suggest that our results should be interpreted cautiously.

APPENDIX

Variable Definitions

Key variables:		
Firm-Specific-Weekly Return	=	ln(1+residual), where the residual is from the augmented market model regression: $r_{it} = \alpha_i + \beta_{1,i} r_{m,j,t} + \beta_{2,i}[r_{\mathrm{US},t} + EX_{jt}] + \beta_{3,i} r_{m,j,t-1} + \beta_{4,i}[r_{\mathrm{US},t-1} + EX_{j,t-1}] + \beta_{5,i} r_{m,j,t-2} + \beta_{6,i}[r_{\mathrm{US},t-2} + EX_{j,t-2}] + \beta_{7,i} r_{m,j,t+1} + \beta_{8,i}[r_{\mathrm{US},t+1} + EX_{j,t+1}] + \beta_{9,i} r_{m,j,t+2} + \beta_{10j}[r_{\mathrm{US},t+2} + EX_{j,t+2}] + \varepsilon_{it}$, where r_{it} is the return on a firm's inferior voting shares i in week t in country j, $r_{m,j,t}$ is the return on the MSCI country-specific market index or the country index compiled by Datastream in week t, $r_{us,t}$ is the U.S. market index return (a proxy for the global market), and $EX_{j,t}$ is the change in country j's exchange rate versus the U.S. dollar.
NEG_SKEWNESS	=	The negative skewness of *Firm-Specific Weekly Return* over the fiscal year, which is calculated by taking the negative of the third moment of firm-specific weekly returns, *Firm-Specific Weekly Return*, during the fiscal year and dividing it by the standard deviation of firm-specific weekly returns, raised to the third power in a fiscal year.
DOWN/UP_RET_SD	=	The log of the ratio of the standard deviations of down-week to up-week *Firm-Specific Weekly Return*.
CRASH	=	An indicator variable equal to 1 if a firm experiences one or more *Firm-Specific Weekly Return* falling 3.0 or more standard deviations below the mean of *Firm-Specific Weekly Return* within a year and equal to zero otherwise.
WEDGE	=	The wedge between voting and cash flow rights, defined as one minus the ratio of voting rights to cash flow rights for the inferior voting shares.
ANALYST	=	The log of the number of analysts following the firm according to I/B/E/S.
SALES_GROWTH	=	The percentage change in sales in a given year in the Fama and French industry to which the firm belongs.
OPAQUE	=	The correlation between changes in cash flow from operations divided by total assets and total accruals scaled by total assets during the previous five years' rolling window (Leuz, Nanda, and Wysocki [2003]). Cash flow from operations is equal to operating income minus accruals, where accruals are calculated as: (Δtotal current assets – Δcash) – (Δtotal current liabilities – Δshort-term debt – Δtaxes payable) – depreciation expense. This correlation is multiplied by negative one such that firms with larger *OPAQUE* values correspond to those with more smooth earnings.

(*Continued*)

APPENDIX—*Continued*

Firm-level control variables:		
TRADE_VOLUME	=	The average monthly share turnover over the current year, minus the average monthly share turnover over the previous year, where monthly share turnover is calculated as the monthly trading volume divided by total number of shares outstanding during the month.
RETURN_SD	=	The standard deviation of the *Firm-Specific Weekly Return* over the current year.
RETURN	=	The mean of the *Firm-Specific Weekly Return* over the current year.
MARKET_CAP	=	The natural log of a firm's total market capitalization (where total market capitalization is the sum of the market capitalization of the superior voting and inferior voting share classes (in millions of U.S. dollars).
MB	=	The ratio of market value of equity (where market value of equity is the sum of the market value of the superior voting and inferior voting share classes) to the book value of equity at the end of the year.
LEVERAGE	=	The book value of long-term debt scaled by the sum of market value of equity and book value of long-term debt at the end of the year.
ROA	=	Income before extraordinary items divided by the beginning-of-year total assets.
OPER CYCLE	=	The log of receivables to sales plus inventory to cost of goods sold multiplied by 360.
CFO/SALES	=	The ratio of cash flow from operating activities (CFO) to sales.
STD CFO	=	The firm-specific standard deviation of the ratio of cash flow from operations and average total assets from years $t-5$ to $t-1$
Country-level control variables:		
ANTI-SD	=	The anti-self-dealing index measures the laws in place to regulate a potential related party transaction proposed by Mr. James, who is the controlling shareholder in both the Buyer and Seller in the proposed transaction, but with different ownership stakes in the two companies. The index captures both ex-ante rules and disclosures that apply before the transaction can take place as well as the ex-post remedies and disclosures that apply after the transaction has occurred (Djankov et al. [2008]).

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