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Firm-level political risk and stock price crashes☆

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A B S T R A C T

In this study, we examine the relationship between firm-level political risk and stock price crash risk. Using a broad dataset of 4230 U.S. firms, 38,097 firm-year observations from 2002 to 2019, we reveal a positive association between political risk and stock price crash risk. These findings are robust to several model specifications and endogeneity checks. By using the Brexit referendum as a quasi-natural experiment, we provide evidence of a causal relationship between political risk and crash risk. Through channel analysis, we identify that this relationship is mediated via higher idiosyncratic volatility, lower price informativeness, and higher distress risk. We also find that our results are more pronounced in intangible-intensive firms. Interestingly, we show that managers of these firms respond to political risk by engaging in bad news hoarding. Finally, strong (external or internal) corporate governance mechanisms can moderate the positive relationship between political risk and stock price crash risk.

1. Introduction

Why do stock prices crash? Numerous studies have answered this question by relying on two sets of explanations. First, the agency-driven explanations, which attribute stock price crashes to opportunistic managers who withhold bad news through opaque financial reporting or overinvestment (Jin and Myers, 2006; Hutton et al., 2009; Callen and Fang, 2015; Hu et al., 2020; Balachandran et al., 2020). Second, financial market explanations, which build on the idea that investor disagreement is the primary driver of stock price crash risk (Hong and Stein, 2003). It is noteworthy that agency explanations have received most of the attention in the literature, while financial market explanations are largely unexplored (Habib et al., 2018). Nonetheless, in their recent study, Andreou et al. (2023) find that agency channels have played a limited role in explaining stock price crashes after the enactment of the Sarbanes–Oxley Act (SOX). Instead, they propose that in our modern economy, stock price crashes can be explained by financial market explanations, such as the increased participation of unsophisticated "noise" traders, or the rapid growth of "fragile" intangible-intensive firms.

In this study, we ask whether and how firm-level political risk impacts stock price crash risk. Our primary motivation is to examine this relationship from the perspective of financial market explanations. Previous literature suggests that political risk can fuel investor disagreement by introducing uncertainty in firm valuations (Pastor and Veronesi, 2012; Pastor and Veronesi, 2013), and increasing investor information asymmetry (Nagar et al., 2019). Furthermore, while economy-wide risks stemming from major political shocks are usually easy to evaluate, firm-level exposure to such shocks is difficult to quantify (Ho et al., 2024). Hence, difficulties in pricing firms' political exposures may discourage arbitrage by informed investors (Addoum and Kumar, 2016). As informed investors withdraw from trading, uniformed trading by excessively optimistic investors may push stock prices to artificially high levels, leading to speculative bubbles and crashes (Shiller, 2020). This scenario illustrates how heterogeneity in investors' opinions, as predicted by Hong and Stein (2003), can lead to severe stock price crashes. Thus, according to the investors' heterogeneity channel, we expect a positive relationship between political risk and crash risk.

Political risk can also influence stock price crash risk through the agency channel; however, the direction of the effect is more ambiguous. On the one hand, managers of politically risky firms could be more

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inclined to hoard bad news, since political risk intensifies investor information asymmetry. As a matter of fact, Jin and Myers (2006) theorize that information asymmetry is a necessary condition behind such managerial opportunistic behaviors. On the other hand, previous studies find that political uncertainty induces precautionary managerial behaviors. For instance, managers respond to uncertainty by reducing dividends (Huang et al., 2015) or by increasing their voluntary disclosures (Nagar et al., 2019). Consequently, it is reasonable to assume that managers will also refrain from bad news hoarding, as part of their precautious strategy to address political uncertainty. Therefore, under the agency view, the net effect of firm-level political risk on crash risk is not known a priori.

Our research question is timely for two main reasons. First, political risk is of growing concern for corporations, as American politics have become increasingly polarized in recent years (Azzimonti, 2018). Congressional gridlocks and government shutdowns are just a few examples, that characterize the uncertain political environment under which firms operate. Second, as we move from an industrial "tangibles-based" economy to a new economy determined by intangible assets, investor disagreement regarding firms' valuation increases (Barth et al., 2023). In fact, Wu and Lai (2020) show that intangible-intensive firms are more prone to crashes, because they are subject to high valuation uncertainty and intense information asymmetry. At the same time, managers of intangible-intensive firms may have more leeway in hiding bad news, given the degree of subjectivity associated with the valuation of intangible assets. Hence, the increase in political risks combined with the fragility of intangible assets create an ideal environment to test our conjectures.

To address our research question, we examine a panel of 4230 U.S. firms over the period 2002–2019 (38,097 firm-year observations) and utilize the firm-level political risk measure developed by Hassan et al. (2019). By using a firm-level measure, we can obtain a much more granular picture of the relationship, if any, between political risk and stock price crash risk. Moreover, since crash risk represents a negative outlier in the distribution of firms' idiosyncratic returns, it is more likely to be influenced by a firm-specific risk measure than a country-level measure of political uncertainty.

Our baseline findings align with our predictions, indicating a positive association between firm-level political risk and stock price crash risk. Our results remain robust to the inclusion of several frequently used controls and fixed effects (industry and year). Then, we address endogeneity concerns in four ways. First, to mitigate selection bias between firms with high-versus-low political risk, we employ a propensity score matching approach (PSM) in the spirit of Islam et al. (2022), and we replicate our baseline regressions in the PSM-matched sample. Second, to provide an exogenous source of variation for firm-level political risk, we employ a two-stage least squares instrumental variable regression (2SLS IV) analysis. We follow Gulen and Ion (2016) and use the first dimension of the U.S. Senate DW-NOMINATE scores as our instrument. Third, to account for any dynamic relationship between political risk and crash risk, we employ a dynamic GMM approach. The results of all three approaches confirm our baseline findings. Finally, to provide a casual interpretation of our findings, we use the Brexit referendum as a quasi-natural experiment. Brexit offers a unique setting to test our conjectures, as it was an exogenous political shock that affected only a subset of U.S. firms. We find that in the post-referendum period, stock price crash risk was significantly higher for affected firms relative to unaffected firms, a finding which implies a causal link between political risk and crash risk.

As a next step, we follow Liang and Renneboog (2017) and conduct a two-stage channel analysis to investigate the potential underlying mechanisms that explains our results. We focus on three mediator variables that relate to investor disagreement. First, we use idiosyncratic volatility. Idiosyncratic volatility refers to the volatility of a specific stock's returns that cannot be explained by broader market movements. Higher levels of idiosyncratic volatility could indicate higher investor disagreement. Prior research uses idiosyncratic volatility as a proxy for information asymmetry (Lee and Mauck, 2016), since it can capture stock pricing errors (Bartram et al., 2012; Li et al., 2014). Second, we employ a direct measure of price informativeness, as outlined by Bai et al. (2016). This measure captures the ability of current market prices to predict future earnings. Therefore, lower price informativeness is associated with higher valuation uncertainty, and thus, higher investor disagreement. Finally, we use probability of default as a third mediating variable, since previous studies indicate that investor information asymmetry is more intense in firms with high distress risk (Goyal and Wang, 2013). We find that positive impact of firm-level political risk on stock crash risk is transmitted through these three mediating variables.

As mentioned previously, a motivation for this study is the rapid increase of intangible assets, which makes stock prices more "fragile," i.e., more prone to crashes. Hence, if this argument is valid, we should expect our findings to be more pronounced in intangible-intensive firms. To explore this further, we re-run our baseline regressions by interacting firm-level political risk with firms' intangibles ratio. The results confirm our predictions, as the interaction term enters the regressions with positive and statistically significant coefficients. Further analysis on the average marginal effects indicates that the positive effect of political risk on crash risk is concentrated to intangible-intensive firms.

Thus far, one question remains unanswered. How do managers respond to political risk? Do they engage in opportunistic behaviours by hiding unfavourable news, or do they adopt a more cautious approach and improve disclosure? To answer this question, we examine the relationship between firm-level political risk and financial reporting opacity (earnings management) or overinvestment. At a first glance, we do not report any statistically significant relationship between political risk and earnings management. However, when we breakdown the sample based on the firms' intangibles ratio, we obtain more insightful results. Specifically, we find that managers of intangible-intensive firms resort to earnings manipulation when faced with political risk. These findings highlight the importance of valuation uncertainty in a crash risk context and illustrate that earnings management can explain at least part of the positive relationship between political risk and crash risk. Turning to the overinvestment channel, we do not report any significant results. This is not surprising, as it is well-established that firms delay investments in periods of political turbulence (Gulen and Ion, 2016; Bonaime et al., 2018). Finally, subsample analysis shows that the positive relationship between political risk and crash risk is only observed in firms with weak corporate governance mechanisms, further underscoring the relevance of agency channels in explaining our results.

We conduct a series of robustness tests to verify the validity of our results. First, we re-run our baseline regressions by including several aggregate measures of risk and uncertainty, such as the economic policy uncertainty (EPU) index of Baker et al. (2016) or the geopolitical risk (GPR) index of Caldara and Iacoviello (2022). In fact, previous studies document a positive association between both indices and crash risk (Luo and Zhang, 2020; Han et al., 2023; Fiorillo et al., 2024). Second, we re-run our regressions by replacing industry fixed effects with firm fixed effects, or by adding state fixed effects to account for political geography. Third, we exclude the years of the global financial crisis (GFC) from our sample, since crash risk, political risk, idiosyncratic volatility, and probability of default peak during these years. Our results remain qualitatively similar in all cases.

We contribute to three strands of literature. At first, we contribute to the large body of literature on stock price crash risk (Jin and Myers, 2006; Hutton et al., 2009; Boubaker et al., 2014; Callen and Fang, 2015; Hu et al., 2020; Balachandran et al., 2020; Kuang, 2022; among others) in two ways: (1) we highlight the importance of firm-level political risk in explaining such crashes, (2) we do not solely rely on agency mechanisms to explain our results. On these grounds, we contribute to the relatively thin literature which utilizes the theoretical predictions of Hong and Stein (2003) to explain stock price crashes. We therefore contribute to the work of Lobo et al. (2020) and Chang et al. (2022), who show that higher investor disagreement regarding firms' valuation is associated with higher stock price crash risk. Finally, we add to the growing literature, which investigates how firm-level political risk impacts various corporate outcomes (Hassan et al., 2019; Gad et al., 2023; Huang et al., 2023).

The remainder of this study is organized as follows. Section 2 reviews the relevant literature. Section 3 outlines our data collection process and methodology. Section 4 discusses our baseline results. Section 5 includes additional analysis and robustness tests, and Section 6 concludes the paper.

2. Literature review

2.1. Determinants of stock price crashes

In the past two decades, there has been a growing literature on the determinants of stock price crashes. Much of this literature focuses on two agency-based explanations: the "opacity channel" and the "overinvestment channel." The "opacity channel," initially introduced by Jin and Myers (2006), suggests that in the presence of information asymmetry, opportunistic managers may withhold unfavourable news through opaque financial reports. At some point, this accumulated bad news reaches the public, leading to a stock price crash. Later empirical studies confirm this prediction by utilizing the financial opacity proxy developed by Hutton et al. (2009) and documenting a positive relationship between financial opacity and stock price crash risk (Callen and Fang, 2015; An et al., 2013; Hu et al., 2020; Ai et al., 2023). The "overinvestment channel," introduced by Benmelech et al. (2010), suggests that when the growth rate of investment opportunities declines, managers may invest in unprofitable projects to hide negative information from investors. Similar to the opacity channel, bad news accumulates up to a point, beyond which is revealed to the public, leading to a stock price crash. Recent studies that employ the overinvestment channel alongside the opacity channel to explain stock price crashes, are those of Balachandran et al. (2020), Deng et al. (2020), Kim et al. (2021), Eugster and Wang (2023), among others.

An alternative explanation behind stock price crashes revolves around the theory of Hong and Stein (2003). In their seminar study, they posit that heterogeneity in investors' beliefs is one of the key drivers of stock price crashes. Specifically, they argue that in the presence of short-sale constraints, bearish investors' private information would not be initially revealed to the market. When bullish investors exit the market, bearish investors will become the margin buyers, revealing their pessimistic signals regarding the firms' fundamental value. Thus, at some point, all these accumulated negative signals surface, leading to a crash.

The theory of Hong and Stein (2003) has received very limited attention in the literature, nonetheless, recent evidence suggests its growing significance. Andreou et al. (2023) argue that while corporate governance has improved after the enactment of the Sarbanes–Oxley (SOX) Act, stock price crashes have become even more frequent. Consequently, they criticize the efficacy of agency-based channels in explaining stock price crash risk, and they propose alternative mechanisms based on the market explanation of Hong and Stein (2003). They further advocate that market-based explanations are more relevant in our modern economy, which is determined by "fragile" and difficult-to-value intangible assets (Wu and Lai, 2020; Barth et al., 2023).

But how can investor disagreement lead to stock price crashes? DeLong et al. (1990) theorize that a surge in noise trading caused by unsophisticated investors may discourage rational arbitrageurs from entering the market. Hence, in the absence of efficient price discovery by informed arbitrageurs, speculative behaviour by noise traders can push stock prices to artificially high levels. Therefore, higher disagreement between informed and uninformed investors may cause mispricing and amplify information asymmetries (Barber et al., 2008). Finally, Shiller (2020) argues that high investors' disagreement for the firms' true fundamental value would inevitably lead to speculative bubbles and more noisy stock prices.

One challenge in testing the financial market explanation of Hong and Stein (2003) is to find a reliable proxy for heterogeneity in investors' beliefs. Idiosyncratic volatility is the undiversifiable component of the firm's risk and reflects uncertainty regarding the firm's value (Vo and Phan, 2019). Several studies demonstrate a positive relationship between idiosyncratic volatility and mispricing, suggesting that higher idiosyncratic volatility primarily resembles noise trading (Bartram et al., 2012; Li et al., 2014; Aabo et al., 2017; Gu et al., 2018), and captures investor information asymmetry (Lee and Mauck, 2016; Yang et al., 2020). However, some studies suggest that high idiosyncratic volatility may capture a higher degree of firm-specific information impounded into stock prices (Morck et al., 2000; Jin and Myers, 2006). Therefore, employing direct measures of stock price informativeness alongside idiosyncratic volatility is essential for robust inferences. On these grounds, Bai et al. (2016) propose a proxy for price informativeness that assesses the ability of current stock prices to forecast future earnings. Additionally, Goyal and Wang (2013) demonstrate that high information asymmetry leads to high distress risk, as measured by Merton's (1974) market-based model. The idea is that in light of distress risk, investors may struggle to accurately assess the firms' fair value.

2.2. Political risk and valuation uncertainty

Numerous studies have examined how political risk affects corporate outcomes and performance. The consensus view is that political risk negatively impacts investment growth and firms' long-term operating performance (Bernanke, 1983; Bloom et al., 2007; Bloom, 2009; Julio and Yook, 2012). Furthermore, in the face of high uncertainty regarding economic policy, firms tend to delay R&D expenses and hiring (Stein and Stone, 2013), restrict capital expenditures (Gulen and Ion, 2016), and postpone mergers and acquisitions (Nguyen and Phan, 2017; Bonaime et al., 2018).

Besides investments, political uncertainty can also significantly distort firms' market values, as it makes stock prices more volatile and more correlated (Pastor and Veronesi, 2012; Pastor and Veronesi, 2013). Political uncertainty can also complicate the prediction of expected cash flows (Bekaert et al., 2016), and increase the discount rate (Pham, 2019), making firm valuation a more challenging task. In addition, Addoum and Kumar (2016) suggest that changes in political environment discourage informed arbitrageurs from taking positions in the market. The withdrawal of such informed investors is expected to decrease information production and market efficiency. Finally, Baloria and Mamo (2017) suggest that policy uncertainty intensifies information asymmetries among investors, as it is associated with higher analysts' forecast errors.

Most of the aforementioned studies utilize aggregate measures of political uncertainty, such as the economic policy uncertainty (EPU) index of Baker et al. (2016) to assess the impact of political risk on firms' outcomes and valuation. However, aggregate measures of political uncertainty are not expected to have a homogenous effect on firms' outcomes and valuation. To capture this heterogeneity, Hassan et al. (2019) employ textual analysis tools and compute a firm-level measure of political risk represented by the percentages of companies' quarterly earnings conference calls dedicated to discussing political risks. They show that 91.69 % of the total political risk of firms is attributable to firm-level political risk. Their findings highlight the importance of examining the impact of political risk on firm outcomes and valuation using a firm-level measure, rather than relying solely on an aggregate measure of economy-wide policy uncertainty. Several recent empirical studies utilize their measure and find that firms exposed to political risk reduce irreversible investments (Banerjee and Dutta, 2022), engage in tax evasion (Hossain et al., 2023), have higher cost of borrowing (Gad et al., 2023), and are more likely to default (Islam et al., 2022). We extend this literature by examining another potential negative externality of political risk; stock price crashes.

3. Data and methodology

3.1. Sample selection

Our sample consists of U.S. firms listed in the NYSE, AMEX, or NASDAQ exchanges over the period 2002–2019.[1] The choice of the examination period is based on data availability of firm-level political risk, which starts from 2002. We use the firm-level political risk measure developed by Hassan et al. (2019)[2] and collect stock return data from the Centre for Research for Security Prices (CRSP) database and accounting data from Compustat. Following Eun et al. (2015), we exclude stocks with less than 30 weeks of available stock return data in a year. To account for the presence of outliers, all stock return data are winsorized at the 1 % and 99 % levels. Finally, we match data from the three databases using the firms' ticker symbols. After matching, our sample consists of 37,809 firm-year observations (4302 unique firms).

3.2. Crash risk measures

Crash risk represents the occurrence of a low probability event that results in a significant negative outlier in the distribution of firm-specific (idiosyncratic) returns. Hence, to compute our crash risk measures, we should first calculate firm-specific returns. To do so, we follow Jin and Myers (2006), Kim et al. (2011a), (2011b), and Eun et al. (2015), among others, and estimate the following expanded market model for every firm-year observation of our sample:

$$r_{i,t} = a_i + b_{1,i}r_{m,t} + b_{2,i}r_{m,t-1} + b_{3,i}r_{m,t-2} + b_{4,i}r_{m,t+1} + b_{5,i}r_{m,t+2} + \varepsilon_{i,t} \quad (1)$$

Following Francis et al. (2015), the returns are on a weekly basis (Wednesday-to-Wednesday), to account for the Monday effect. Furthermore, in Eq. (1), i is a firm index and t is the time indicator (week). Therefore, $r_{i,j,t}$ denotes the weekly return of firm i of country j in week t of a year, and $r_{m,j,t}$ denotes the CRSP value-weighted (market index) return in in week t of the same year.[3] Finally, to overcome thin trading issues, we include lags and leads as in Dimson (1979). Then, the firm-specific return w of firm i in week t is defined as follows:

$$w_{i,t} = \ln(1+\varepsilon_{i,t}) \quad (2)$$

We compute the two most widely-used measures of crash risk, using the firm-specific returns, introduced by Chen et al. (2001). More precisely, we calculate the negative skewness (*NSkew*), and the "down-to-up volatility" (*Duvol*). *NSkew* is defined as follows:

$$\mathrm{NSkew}_{i,T} = -\frac{n(n-1)^{3/2}\sum_{t=1}^{n} w_{i,t}^3}{(n-1)(n-2)\left(\sum_{t=1}^{n} w_{i,t}^2\right)^{3/2}} \quad (3)$$

where n is the number of weekly firm-specific returns in a year T. In this formula, the denominator is a normalization factor (Greene, 1993). Higher values of *NSkew* represent higher stock price crash risk.

Furthermore, *Duvol* is calculated as follows:

$$\mathrm{Duvol}_{i,j,T} = \log\left(\frac{\sum_{\mathrm{Down}} w_{i,t}^2 \Big/ (n_{\mathrm{Down}}-1)}{\sum_{\mathrm{Up}} w_{i,t}^2 \Big/ (n_{\mathrm{Up}}-1)}\right) \quad (4)$$

where n_{down} and n_{up} stand for the number of up and down weeks in a year T. A down (up) week is the week where the firm-specific return is lower (higher) than the mean firm-specific return in a year T. Similar to *NSkew*, higher values of *Duvol* translate to higher stock price risk. In addition, *Duvol* does not include the third moment, and as a result it is less affected by a small number of extreme returns (Callen and Fang, 2015).

3.3. Channel effects

We focus on three channels through which political risk affects stock price crashes. More precisely, we use the following three variables: (1) Idiosyncratic volatility, (2) Price informativeness, and (3) Probability of default.

Following Bartram et al. (2012), we measure *Idiosyncratic volatility* as the annualized standard deviation of the residuals of Eq. (1). Furthermore, we employ a direct measure of stock price informativeness, outlined by Bai et al. (2016). This measure is based on cross-sectional regressions of future earnings of current market prices. Therefore, for each year t and for every horizon h (1–5 years ahead from year t), we estimate the following regression:[4]

$$\frac{EBIT_{i,t+h}}{Assets_{i,t}} = a_{t,h} + b_{t,h}\log\left(\frac{MV_{i,t}}{Assets_{i,t}}\right) + c_{t,h}\left(\frac{EBIT_{i,t}}{Assets_{i,t}}\right) + c^{s}{}_{t,h}I^{s}{}_{i,t,} + e_{i,t,h} \quad (5)$$

We use the ratio of current earnings before interest and taxes (EBIT) to total assets, as an additional independent variable to control for current publicly-available information. Furthermore, $I^{s}{}_{t,h}$ denotes an industry indicator for firm i (based on 2-digit SIC codes). Then, our measure of price informativeness is computed as follows:

$$Priceinformativeness = \hat{b}_{t,h}\sigma_t\left(\log\left(\frac{MV_{i,t}}{Assets_{i,t}}\right)\right) \quad (6)$$

For ease of interpretation, we also reverse the directionality of *Price informativeness*. We do so because we hypothesize that *Political risk* is associated with higher mispricing errors. Thus, we construct *Equity mispricing* by multiplying *Price informativeness* with —1.

Finally, we use the probability of default. To calculate probability of default, we should first estimate the distance to default (DTD), which is a volatility adjusted measure of the leverage of a firm. In line with previous studies (Nadarajah et al., 2021; Islam et al., 2022), we collect DTD data from the National University of Singapore's Credit Research Initiative (CRI) database. For each firm, DTD is estimated according to the structural model of Merton (1974). Moreover, the model assumes the default point to equal the firm's current liabilities plus 50 % of the long-term liabilities, plus other liabilities times a faction δ. Therefore, the DTD for firm i in year t is computed as follows:

$$DTD_{i,t} = \frac{\log\left(\frac{V_t}{L}\right) + \left(\mu - \frac{\sigma^2}{2}\right)\left(T-t\right)}{\sigma\sqrt{T-t}} \quad (7)$$

where Vt is the asset value following a geometric Brownian motion with drift μ and volatility σ, L is the default point and $\sqrt{T-t}$ is set to one year. Finally, the probability of default is calculated as the cumulative

[1] The sample period including one-lagged control variables spans from 2001 to 2019.

[2] Source: www. https://www.firmlevelrisk.com/.

[3] We use the Datastream Global Equity Indices to find the domestic market return for each country j.

[4] In our baseline regression, we use the price informativeness measure based on a horizon of 3 years. We do so, to maximize our data availability (since t ends in year 2019). Results with alternative horizons remain qualitatively the same.

standard normal distribution of the negative distance to default:

$$Probability of default = N(-DD) \quad (8)$$

3.4. Control variables and model specification

We use a vector of firm-specific characteristics as our baseline control variables. Following Chen et al. (2001), Hutton et al. (2009), and Andreou et al. (2017), among others, we use the following controls: the natural logarithm of firms' market value of equity as a proxy for firm size (*Size*), the book-to market ratio (*BTM*), the ratio of total debt to total assets (*Leverage*), and the return on assets (*ROA*) as a measure of profitability. Hong and Stein (2003) indicate that stock price crash risk is more pronounced after periods of high trading volume. For this reason, we follow Chen et al. (2001), and use the detrended turnover (*DTurnover*) as a control. Furthermore, we include the past average (on annual basis) firm-specific weekly returns (*Returns*) to account for any past momentum effect (Chen, 2001; Campbell et al., 2008). Balachandran et al. (2020) find that older firms are less susceptible to experiencing stock price crashes. Therefore, we control also for firm age, using the natural logarithm of company's firm age (Ln *(*Age)). We define age as the number of years (plus one) since the year of the company's Initial Public Offering (IPO). We account for financial reporting opacity (*Opacity*), using the three-year sum of absolute discretionary accruals as in Hutton et al. (2009). Finally, we use the one-year-lagged value of *NSkew* as a control (Hutton et al., 2009; Callen and Fang, 2015; Chang et al., 2017).

In addition to our baseline controls, we incorporate additional variables into our analysis to address omitted variable bias. Specifically, we use the ratio of goodwill to total assets (*Goodwill*) and the industry-adjusted ratio of firm operating profits to sales, to proxy for competitiveness (*Competitiveness*), following the approach of Andreou et al. (2021). Furthermore, we control for firms' liquidity as in Chang et al. (2017) using the effective bid-ask spread (*Bid-ask spread*). We also use firm's beta (*Beta*) as in Kim and Zhang (2014). Finally, we include the natural logarithm of one plus the number of analysts following the firm (*Number of Analysts*) as a firm-level proxy for external governance (Habib et al., 2018).

To address our main research question, we estimate the following regression:

$$\text{Crash risk}_{i,t}=a+b_1\text{Political risk}_{i,t}+b_2X_{t-1}+\text{Industry FE}+\text{Year FE}+e_{i,t} \quad (9)$$

where *Crash risk*$_{i,t}$ is either *NSkew* or *Duvol* of firm *i* at year *t*, *Political risk*$_{i,t}$ is the average of the four quarters firm's *i* political risk at year *t*, and X_{t-1} denotes a vector of our control variables at year *t*-1.[5] In all regressions, we include industry fixed effects (using Fama-French 48 industry classification) and year fixed effects.[6] Standard errors are clustered at the firm and at the year levels. Finally, all variables are winsorized at the 1 % and 99 % levels.

3.5. Summary statistics

Fig. 1 depicts the evolution of crash risk, political risk, and our three channel variables through time. Some interesting patterns emerge; for instance, crash risk, idiosyncratic volatility and probability of default seem to exhibit parallel trends. They peak during the global financial crisis of 2007–2008, decline sharply in the subsequent years, and then rise again post-2010. *Political risk* also peaks during the crisis years, but its levels remain high until 2014. Interestingly, *Equity mispricing* demonstrates an increasing trend since the beginning of the examination period, reflecting a steady decline of stock price informativeness over the years. It is also noteworthy that in the post-crisis years, the upward trend in *Equity mispricing* coincides with an increase in stock price crash risk.

Fig. 2 depicts the geographic dispersion of *Political risk* across states,[7] with dark blue indicating states with high political risk, and light blue states with low *Political risk*. Evidently, there is substantial cross-state heterogeneity in firm-level political risk, with West Virginia (Rhode Island) being the state with the highest (lowest) value in *Political risk*. Finally, we also observe substantial cross-industry heterogeneity in *Political risk* (see Table A2 in the Appendices), a fact which justifies our choice to use industry fixed effects in all our regression models.[8]

Table 1 presents the summary statistics of our sample. The mean values of our two crash risk measures, *NSkew* and *Duvol*, are —0.010 and —0.014, respectively. The negative sign is in line with Callen and Fang (2015), but our reported mean values are substantially higher. This finding is in line with Andreou et al. (2023), who report a steady increase in stock prices crashes over the past two decades. Additionally, the summary statistics for our control variables closely resemble those reported in prior U.S. studies (Chang et al., 2017; Andreou et al., 2021). To control for the presence of outliers, we winsorize all continuous variables at the 1 % and 99 % levels.

Table 2 shows the correlation coefficients of our independent variables. In Panel A, we report the correlation coefficients between baseline controls and additional controls. For our baseline controls, we observe a modest degree of collinearity, with the highest correlation coefficient being 0.519 between *Size* and *Returns*. Interestingly, we find some high correlation coefficients between baseline controls and additional controls, like the correlation of *Size* with *Number of Analysts* (0.704) or *ROA* with *Competitiveness* (0.700). However, this does not raise any concerns, as in most of our models we rely on our baseline controls. Additionally, average variance inflation factors (VIFs) are below 10 in all cases, indicating that multicollinearity should not be an issue even when we augment our models with additional controls (Wooldridge, 2016). Finally, Panel B reports the correlation coefficients of our main variable of interest (*Political risk*) with the three mediators (*Idiosyncratic volatility*, *Equity mispricing,* and *Probability of default*). This bivariate analysis reveals some interesting patterns in the data. *Political risk* is positively correlated with *Equity mispricing* and *Probability of default*. Furthermore, *Idiosyncratic volatility* is also positively correlated with *Equity mispricing* and *Probability of default*. Notably, all four correlation coefficients are statistically significant at the 1 % level.

4. Main empirical findings

4.1. Baseline results

We move to our multivariate regression analysis, where we

[5] We follow Gyimah et al. (2022), and measure *Political risk* at time *t* and our baseline controls at time *t*—1. It is typical in the crash risk literature to measure controls at the end of the previous year to avoid any forward looking bias. This issue is even more relevant in the case of accounting measures, as they are usually disclosed to the public many months after the previous year end. For instance, net income for the end of the year *t*—1 becomes known at some point in year *t*. By contrast, *Political risk* is measured throughout the year (as the average of the four quartets).

[6] In untabulated analysis, we also run all our regressions by replacing industry fixed effects with firm fixed effects and obtain qualitatively similar results. We do not use them simultaneously in the models, as the industry effects are collinear with firm effects.

[7] For each state, we use the mean value of *Political risk* for all firms headquartered in this state during our examination period. Wyoming is the only state with a light grey color because we do not have any company headquartered in this state in our sample.

[8] We include finance and utilities firms for two reasons. First, these are the two industries with the highest average values in *Political risk*. Second, they are more informationally opaque and intangible-intensive industries relative to all other industries. Nonetheless, we obtain qualitatively similar results if we exclude them from the sample.

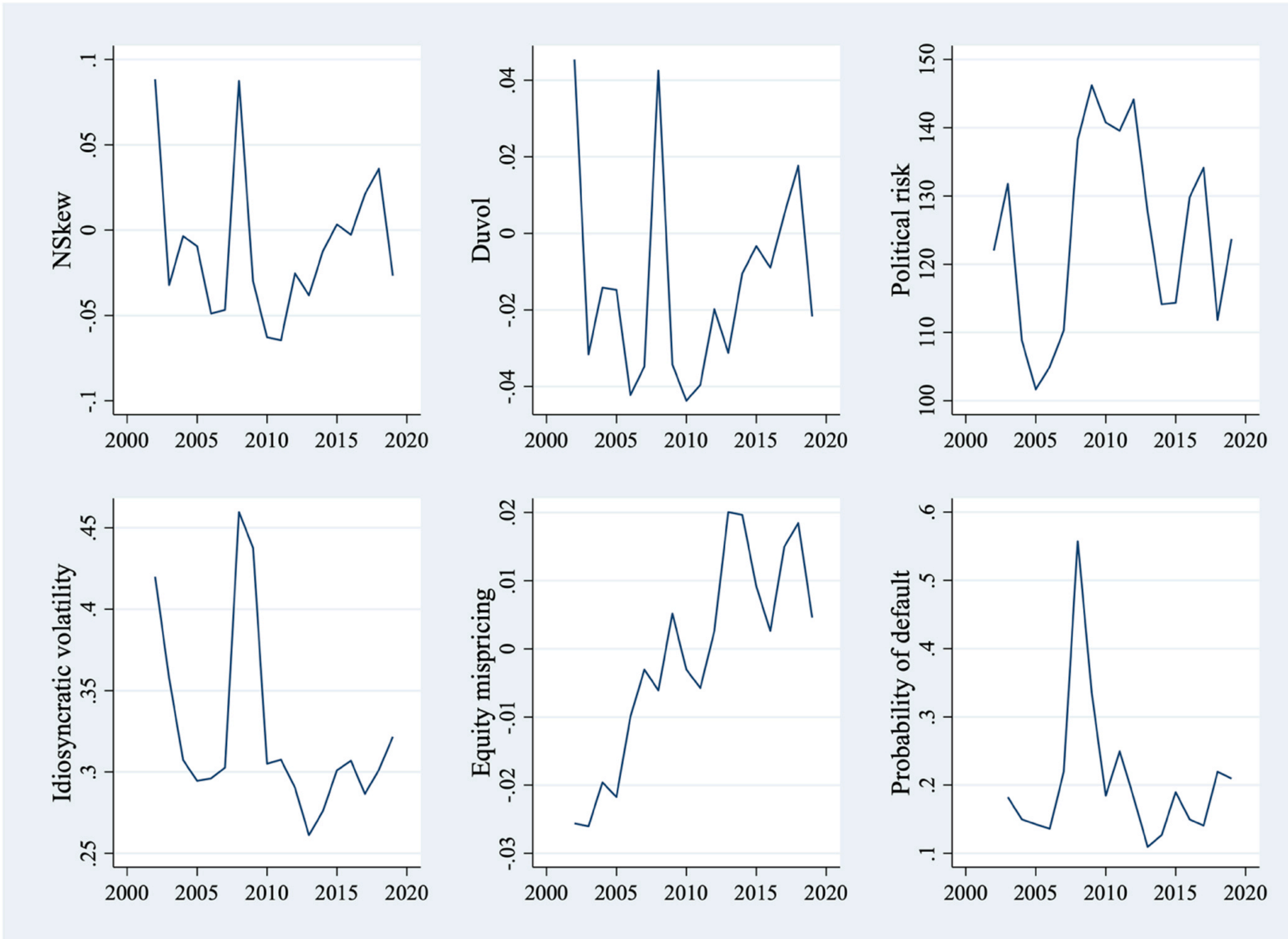

Fig. 1. Crash risk, political risk, probability of default and idiosyncratic volatility overtime. The figure illustrates the evolution of Crash risk (*Nskew* and *Duvol*), our main independent variable (*Political risk*), and our three mediators (*Idiosyncratic volatility*, *Equity mispricing*, and *Probability of default*) overtime. Each year represents the mean value for each one of the four variables.

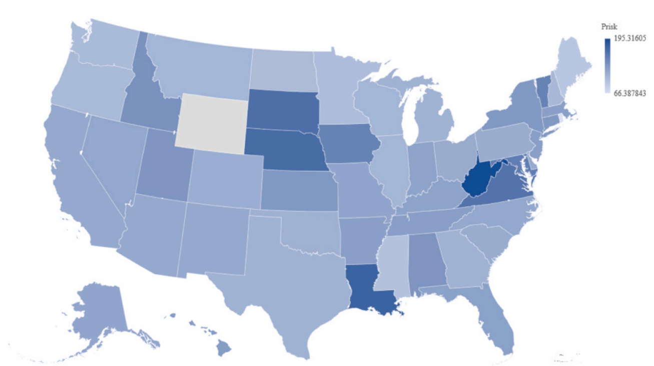

Fig. 2. Geographical dispersion of political risk across states. The figure shows the mean value of firm-level political risk per U.S. state.

investigate the relationship, if any, between firm-level political risk and stock price crash risk. Table 3 presents the results of this analysis. In models 1 and 2, we conduct our analysis without the inclusion of the baseline controls. In line with our expectations, the coefficient of *Political risk* is positive and statistically significant at the 10 % level in model 1, and positive and statistically significant at the 5 % level in model 2. In models 3 and 4, we add our baseline controls. *Political risk* is now positive and statistically significant at the 5 % level in both models. In terms of economic significance, one standard deviation increase in *Political* risk increases *NSkew* by 1.09 % (0.007 × (1.028/0.659)) standard deviation, and *Duvol* by 1.24 % (0.004 × (1.028/0.331)) standard deviation.

Regarding our baseline controls, the findings align with previous studies. Both *DTurnover* and *Size* exhibit positive and statistically significant coefficients at the 1 % level, indicating that investors' heterogeneity and firm size contribute to increased stock price crashes (Chen et al., 2001; Callen and Fang, 2015; Zhu, 2016). Additionally, *ROA* is positive and statistically significant at the 1 % level, a finding which is consistent with Kim et al. (2021) and Andreou et al. (2021). In line with Balachandran et al. (2020), Ln *(*Age) demonstrates a negative and statistically significant coefficient at the 1 % level, suggesting that older firms are less susceptible to crashes. Consistent with Wu and Lai (2020), *BTM* bears a negative and statistically significant coefficient at the 5 % level, or better. Importantly, we observe that *Opacity* enters both regressions with positive and statistically significant coefficient at the 5 % level, a finding which supports the well-established agency explanation behind stock price crashes (Jin and Myers, 2006; Hutton et al., 2009). Finally, in line with most relevant studies, we report a positive and and highly statistically significant relationship between the one-year-lagged value of *NSkew* and crash risk (Hutton et al., 2009; Callen and Fang, 2015; Chang et al., 2017). Throughout the remainder of this study, we

Table 1
Summary statistics. This table presents the summary statistics for the variables of our sample. Our sample consists of U.S. firms from 2002 to 2019. All variables are defined at Table A1 in the Appendices.

Variables	Mean	St.dev	P25	Median	P75
Crash risk measures					
Nskew	-0.010	0.659	-0.397	-0.018	0.370
Duvol	-0.014	0.331	-0.232	-0.015	0.204
Political risk and channel variables					
Political risk	4.332	1.028	3.726	4.383	5.004
Idiosyncratic volatility	0.322	0.169	0.197	0.283	0.405
Equity mispricing	0.000	0.039	-0.021	-0.003	0.018
Probability of default (%)	0.207	0.392	0.004	0.032	0.182
Baseline controls					
DTurnover	0.002	0.637	-0.030	0.001	0.033
Size	6.990	1.694	5.836	6.968	8.163
ROA (%)	1.214	17.134	0.600	4.400	8.480
BTM	0.539	0.533	0.253	0.448	0.709
Leverage	0.236	0.221	0.033	0.195	0.371
Returns	-0.001	0.001	-0.002	-0.001	0.000
Opacity	1.346	1.512	0.375	0.849	1.739
Ln(Age)	2.516	0.954	1.958	2.688	3.268
Additional controls					
Goodwill	0.119	0.146	0.000	0.056	0.193
Competitiveness	-0.024	0.180	-0.067	0.000	0.071
Bid-ask spread	0.004	0.017	0.001	0.002	0.003
Beta	1.246	0.768	0.717	1.145	1.657
Ln (Number of analysts)	1.817	0.880	1.099	1.946	2.485

4.2. Addressing endogeneity

Before we examine the underlying mechanism behind the effect of political risk on stock price crashes, we should address endogeneity concerns regarding our baseline results. So far, to account for omitted variable bias and unobserved heterogeneity, we have included a vector of control variables, along with industry and year fixed effects in our baseline regressions. To further tackle endogeneity concerns, we now employ: (1) the propensity score matching approach (PSM), (2) two-stage least squares instrumental variable regressions (2SLS IV) and a dynamic Generalized Method of Moments (GMM) approach, and (3) a difference-in-differences (DiD) analysis by exploiting a quasi-natural experiment.

4.2.1. Propensity score matching

A potential concern in our baseline regressions is the presence of systematic differences in characteristics between firms with high-versus-low *Political risk*. While the inclusion of controls in our regressions can help alleviate this concern, poor distributional overlap in characteristics across these firms could bias our regression estimates (Heckman et al., 1998). Therefore, to address this issue, we employ the propensity score matching approach. In detail, we match firms with above median (high) *Political risk* to similar firms with below median (low) *Political risk*. Following Islam et al. (2022), the former set of firms is our treated group, and the latter set is our control croup. To conduct the matching, we estimate a logistic regression where the dependent variable equals 1 if the firm belongs in the treated group, and 0 otherwise. Furthermore, we use all our baseline controls along with industry and year fixed effects. Then, we calculate the propensity of each firm to belong in the treated group, and we match firms using the nearest-neighbour matching approach (with replacement).[10]

Column 1 of Table 4, reports the mean differences between high and low Political risk firms for the unmatched sample. With the exceptions of $Nskew_{t-1}$ and *DTurnover*, mean differences of all other control variables are statistically significant at the 1 % level. These results confirm that there are significant differences in characteristics between high-versus-low *Political risk* firms, and highlight the necessity of the PSM approach.

Table 2
Correlation matrix. This table presents pairwise correlations between the independent variables of our sample. Our sample consists of U.S. firms from 2002 to 2019. Panel A presents correlations between our control variables (baseline and additional controls). Panel B presents correlations between the firm political risk and the channel variables. All variables are defined at Table A1 in the Appendices. The symbols c, b, and a denote statistical significance at the 10 %, 5 % and 1 % levels, respectively, using a 2-tail test.

Panel A: Correlation between control variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
DTurnover (1)	1.000												
Size (2)	0.001	1.000											
ROA (3)	0.011^{c}	0.333^{a}	1.000										
BTM (4)	-0.042^{a}	-0.258^{a}	-0.005	1.000									
Leverage (5)	0.010	0.144^{a}	0.057^{a}	-0.104^{a}	1.000								
Returns (6)	-0.119^{a}	0.519^{a}	0.484^{a}	-0.036^{a}	0.093^{a}	1.000							
Opacity (7)	-0.019^{a}	-0.050^{a}	-0.133^{a}	0.013^{c}	0.062^{a}	-0.026^{a}	1.000						
Ln(Age) (8)	-0.026^{a}	0.268^{a}	0.201^{a}	0.027^{a}	-0.010	0.272^{a}	-0.103^{a}	1.000					
Goodwill (9)	-0.004	0.166^{a}	0.134^{a}	-0.091^{a}	0.088^{a}	0.136^{a}	-0.168^{a}	0.052^{a}	1.000				
Competitiveness (10)	0.005	0.384^{a}	0.700^{a}	-0.041^{a}	0.146^{a}	0.453^{a}	-0.074^{a}	0.158^{a}	0.185^{a}	1.000			
Bid-ask spread (11)	-0.011	-0.240^{a}	-0.123^{a}	0.066^{a}	-0.019^{a}	-0.187^{a}	0.026^{a}	-0.029^{a}	-0.049^{a}	-0.112^{a}	1.000		
Beta (12)	-0.045^{a}	-0.113^{a}	-0.182^{a}	0.032^{a}	-0.026^{a}	-0.362^{a}	0.070^{a}	-0.103^{a}	-0.042^{a}	-0.158^{a}	-0.025^{a}	1.000	
Number of analysts (13)	-0.013^{c}	0.704^{a}	0.166^{a}	-0.196^{a}	0.017^{b}	0.260^{a}	-0.062^{a}	0.141^{a}	0.153^{a}	0.227^{a}	-0.145^{a}	0.000	1.000

Panel B: Correlation between political risk and channel variables

	(1)	(2)	(3)	(4)
Political risk (1)	1.000			
Idiosyncratic volatility (2)	0.000	1.000		
Equity mispricing (3)	0.066^{a}	0.094^{a}	1.000	
Probability of default (4)	0.017^{a}	0.513^{a}	-0.052^{a}	1.000

report regression results using this set of baseline controls.[9]

In models 5 and 6, we expand our baseline regressions with additional controls to address any potential omitted variable bias. Among these controls, *Goodwill* stands out with a positive and statistically significant coefficient at the 10 % level or better, while all other controls exhibit statistically insignificant coefficients. Notably, *Political risk* remains positive and statistically significant in both models at the 1 % level, with its coefficient magnitude substantially higher when compared to the one reported in models 1–4. Therefore, our baseline results support our main hypothesis that firm-level political risk is positively associated with stock price crash risk.

[9] In all cases, results remain qualitatively similar if we augment our models with additional controls.

[10] We repeat this exercise by matching without allowing for replacement. Our findings remain similar.

Table 3
Baseline regressions. This table presents panel regression results for a sample of U.S. firm from 2002 to 2019. The sample consists of firm-year observations. The dependent variable is the negative skewness in models 1, 3, and 5, and the Duvol in models 2, 4, and 6, respectively. All continuous variables are winsorized at 1 % and 99 % level. *T*-statistics (in parentheses) are based on standard errors clustered at the firm level and year levels. The symbols *, **, and *** denote statistical significance at the 10 %, 5 % and 1 % levels, respectively, using a 2-tail test.

	NSkew	Duvol	NSkew	Duvol	NSkew	Duvol
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Political risk	0.007*	0.004**	0.007**	0.004**	0.011***	0.006***
	(1.94)	(2.16)	(1.97)	(2.37)	(3.36)	(3.24)
$NSkew_{t-1}$			0.034***	0.016***	0.025***	0.011***
			(5.15)	(4.83)	(4.86)	(4.73)
DTurnover			0.162***	0.086***	0.116**	0.060**
			(4.41)	(4.18)	(2.83)	(2.81)
Size			0.033***	0.018***	0.027***	0.014***
			(7.30)	(7.19)	(3.50)	(3.58)
ROA			0.002***	0.001***	0.002***	0.001***
			(6.81)	(7.42)	(5.58)	(6.40)
BTM			-0.023**	-0.014***	-0.032***	-0.019***
			(-2.69)	(-3.50)	(-3.17)	(-4.03)
Leverage			-0.000	-0.000	-0.000	-0.000
			(-0.66)	(-1.00)	(-0.99)	(-1.34)
Returns			2.635	2.199	-13.415*	-5.401
			(0.59)	(0.89)	(-2.04)	(-1.51)
Opacity			0.008**	0.004**	0.010***	0.005**
			(2.59)	(2.50)	(2.92)	(2.50)
Ln(Age)			-0.028***	-0.014***	-0.025***	-0.013***
			(-4.32)	(-4.28)	(-4.03)	(-3.98)
Goodwill					0.074**	0.031*
					(2.54)	(2.00)
Competitiveness					-0.018	-0.008
					(-0.44)	(-0.40)
Bid-ask spread					-0.454	-0.208
					(-1.19)	(-1.17)
Beta					-0.014	-0.008
					(-0.93)	(-0.95)
Ln (Number of analysts)					0.012	0.008*
					(1.36)	(1.76)
Constant	-0.042**	-0.032***	-0.180***	-0.107***	-0.193***	-0.110***
	(-2.56)	(-3.81)	(-3.42)	(-3.91)	(-3.18)	(-3.38)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	43,625	43,625	38,097	38,097	29,559	29,559
Adjusted R^2	0.008	0.011	0.022	0.029	0.018	0.023

Column 2 presents the results of the logistic regression, where the dependent variable equals 1 for firms with high (above-median) political risk, and 0 otherwise. The signs of the coefficients are consistent with the sign of mean differences, and in line with Islam et al. (2022). Specifically, larger, and less leveraged firms are more likely to belong into the treated group. Furthermore, older firms with higher book-to-market values are also more likely to belong into the treated group. As a next step, we compute the propensity scores on the basis of these estimates, and we match the two groups.

In Column 3 of Table 4, we assess the quality of our matching procedure. To do so, we test the statistical significance of mean differences between firms in the treated group and their matches. The lack of statistical significance indicates that our two comparison groups are adequately matched.[11] Columns 4 and 5 replicate the baseline regressions (columns 3 and 4) of Table 3, for the PSM-matched sample. Our results hold, since *Political risk* is positive and statistically significant at the 5 % level in both regressions of *Nskew* and *Duvol*. Furthermore, the economic significance of our results increases slightly after we account for the systematic differences across the treated and control groups. More precisely, a one standard deviation increase in *Political* risk increases *NSkew* by 1.46 % (0.010 × (0.952/0.653)) standard deviation, and *Duvol* by 1.46 % (0.005 × (0.952/0.327)) standard deviation.[12]

4.2.2. 2SLS IV and GMM

In our baseline regressions, we assume that the variation of the firm-level political risk is exogenously determined. However, there might be some economic factors that jointly determine firm-level political risk and crash risk, thereby raising endogeneity concerns. In the presence of such concerns, the coefficient of *Political risk* might be inconsistent and biased. To address this issue, we implement both a 2SLS IV analysis and a dynamic GMM approach.

The major challenge with the 2SLS IV approach is to find a valid instrument which satisfies the relevance and the exclusion criteria. In other words, we should find an instrument which is correlated with *Political risk* but uncorrelated with the error term. To do so, we rely on a relevant strand of literature which uses the first dimension of DW-NOMINATE scores for the U.S. Senate, as an instrumental variable (Gulen and Ion, 2016; Jin and Wu, 2021). The DW-NOMINATE scores, developed by McCarty et al. (1997), are designed to monitor the ideological stances of legislators across time. The first dimension reflects where legislators stand regarding government involvement in the economy (Poole and Rosenthal, 2000). Thus, our instrumental variable is determined as the difference in average scores between Democratic

[11] The only exception is in the case of *Size*, where mean differences are marginally statistically significant.

[12] In the PSM-matched sample, the standard deviation of *Political risk* is 0.952. For *NSkew* and *Duvol*, the corresponding figures are 0.653, and 0.327, respectively.

Table 4
Propensity score matching. This table presents the results of our PSM approach. Column 1 reports the mean differences between high and low *Political risk* firms for the unmatched sample. Column 2 presents the results of the logistic regression, where the dependent variable equals 1 for firms with high (above-median) political risk, and 0 otherwise. The matching is employed with the propensity score matching (PSM) approach. PSM is conducted according to the 1:1 nearest neighbour approach (with replacement). Column 3 reports the mean differences between high and low *Political risk* firms for the matched sample. Columns 4 and 5 replicate the baseline regressions (columns 3 and 4) of Table 3, for the PSM-matched sample. All continuous variables are winsorized at 1 % and 99 % level. *T*-statistics (in parentheses) are based on standard errors clustered at the firm level and year levels. The symbols *, **, and *** denote statistical significance at the 10 %, 5 % and 1 % levels, respectively, using a 2-tail test.

	Unmatched sample		*Matched sample*		
	Mean differences	Political risk dummy	Mean differences	NSkew	Duvol
Variables	(1)	(2)	(3)	(4)	(5)
Political risk				0.010**	0.005**
				(2.47)	(2.53)
$NSkew_{t-1}$	0.008	0.021	0.00	0.028***	0.014***
	(1.10)	(1.20)	(0.90)	(4.22)	(4.14)
DTurnover	0.000	−0.046	0.001	0.130**	0.070**
	(0.34)	(-0.38)	(0.53)	(2.53)	(2.67)
Size	0.057***	0.056***	0.033*	0.032***	0.017***
	(2.91)	(6.19)	(1.66)	(5.91)	(5.82)
ROA	-2.083***	-0.001	0.308	0.002***	0.001***
	(-11.81)	(-1.18)	(1.59)	(6.90)	(8.03)
BTM	0.045***	0.103***	0.002	-0.034***	-0.019***
	(7.88)	(4.03)	(0.33)	(-2.95)	(-3.44)
Leverage	-1.291***	-0.001**	-0.302	0.000	-0.000
	(-5.35)	(-2.24)	(-1.26)	(0.42)	(-0.05)
Returns	-0.001***	−19.049	0.000	-3.155	-0.360
	(-3.52)	(-1.55)	(0.01)	(-0.55)	(-0.11)
Opacity	0.069***	0.018	-0.020	0.010**	0.006**
	(4.42)	(1.57)	(-1.19)	(2.55)	(2.42)
Ln(Age)	0.026***	0.044***	0.010	-0.029***	-0.015***
	(2.91)	(2.71)	(1.13)	(-3.90)	(-3.85)
Constant		-1.070***		-0.203***	-0.117***
		(-2.69)		(-3.43)	(-3.78)
Industry FE		Yes		Yes	Yes
Year FE		Yes		Yes	Yes
N		33,717		22,256	22,256
Pseudo R^2/ Adjusted R^2		0.064		0.021	0.026

and Republican party members. Higher values of this difference indicate greater partisan polarization. We expect *DW-NOMINATE* to satisfy the relevance criterion, as greater political polarization should increase firms' political exposure. At the same time, we do not believe that an aggregate measure of political disagreement might affect a firm-specific event through any other pathways than its impact on firm-level political risk. Therefore, we believe that *DW-NOMINATE* also satisfies the exclusion criterion.

Panel A in Table 5 presents the results of the 2SLS IV analysis. Column 1 reports the results of the first-stage regressions, where the dependent variable is *Political risk*. Consistent with previous studies, the coefficient of *DW-NOMINATE* is positive and statistically significant at the 1 % level, satisfying the relevance criterion (Gulen and Ion, 2016; Jin and Wu, 2021). In addition, our instrument passes both the weak identification and the under-identification tests. Models 2 and 3 report the results of the second-stage regressions. Our baseline results hold, as the instrumented *Political risk* is positive and statistically significant at the 5 % level in the regression of *NSkew*, and at the 1 % level in the regression of *Duvol*.

As a next step, we conduct the dynamic GMM approach. The benefit of this approach is that it captures the dynamic relationship between crash risk and *Political risk* while controlling for other potential sources of endogeneity that we have not yet addressed. In our analysis so far, we

Table 5
2SLS IV and GMM. This table reports our baseline regressions using either a 2SLS instrumental variable approach or a dynamic GMM approach. Panel A presents the results of the 2SLS IV regressions. In column 1, the dependent variable is the firm's political risk (first stage), and the proxy for political polarization (*DW-Nominate*) is the instrument. Columns 2 and 3 replicate the baseline regressions (columns 3 and 4) of Table 3 using the instrumented *Political risk* as the main explanatory variable (second stage). Panel B re-runs replicate the baseline regressions (columns 3 and 4) using the dynamic GMM approach. All continuous variables are winsorized at 1 % and 99 % level. *T*-statistics (in parentheses) are based on standard errors clustered at the firm and year levels. The symbols *, **, and *** denote statistical significance at the 10 %, 5 % and 1 % levels, respectively, using a 2-tail test.

	Political risk	NSkew	Duvol
Panel A: 2SLS IV	(1)	(2)	(3)
DW-Nominate	3.504***		
	(9.99)		
Political risk (Instrumented)		0.184**	0.115***
		(2.47)	(3.10)
Baseline controls	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
N	34,695	34,695	34,695
Adjusted R^2	0.099	0.018	0.023
Underidentification test (Kleibergen-Paap rk LM statistic)	89.324		
Weak identification test (Kleibergen-Paap Wald F statistic)	89.324		
	NSkew	Duvol	
Panel B: GMM	(1)	(2)	
Political risk	0.580***	0.235***	
	(2.91)	(2.67)	
Baseline controls	Yes	Yes	
Industry FE	Yes	Yes	
Year FE	Yes	Yes	
N	34,695	34,695	
AR (1) test (*p*-value)	0.270	0.371	
AR (2) test (*p*-value)	0.144	0.212	
Hansen test of overidentifying restrictions (*p*-value)	0.569	0.283	

have addressed omitted variable bias with the 2SLS IV analysis (and the use of additional controls). Furthermore, we have accounted for time-invariant unobserved heterogeneity using fixed effects in our regressions. However, our estimates could still be biased if past values of the dependent variable are related to current values of the explanatory variables (Wintoki et al., 2012). GMM helps accounting for this (potential) dynamic relationship using past values of both the dependent and the independent variables as a set of "internal" instruments.

Panel B of Table 5 presents the results of the dynamic GMM approach.[13] Notably, *Political risk* is positive and statistically significant at the 1 % level in both regressions of *NSkew* and *Duvol*. These results suggest that the positive *Political risk* – *Crash risk* relationship holds even after controlling for simultaneity and the dynamic relationship between past values *Crash risk* and current values of *Political risk*. Finally, we use the Hansen J test and the AR (2) test of Arellano and Bond (1991) to test the validity of overidentifying restrictions and the presence of second-order serial correlation in the errors, respectively. In both models, the *p*-values of both tests are higher than 0.10, indicating that our GMM estimator is valid.

4.2.3. A quasi-natural experiment

Although we have addressed endogeneity in many ways, we have yet

[13] We implement the dynamic GMM approach as in Wintoki et al. (2012). Specifically, we use the xtabond2 command in Stata 17 using the options "lag (3 4)" to indicate the most recent and the most distance lag, respectively, and "collapse" to avoid instrument proliferation. Furthermore, we assume only firm age, detrended turnover, and year and industry dummies to be strictly exogenous using the option "iv".

to establish causality between *Political risk* and *Crash risk*. To achieve this goal, we follow Ho et al. (2024), and use the 2016 Brexit referendum as a quasi-natural experiment to measure the effect of political risk on stock price crashes. We posit that following the Brexit vote, U.S. firms that were exposed to Brexit uncertainty will experience higher stock price crash risk relative to unexposed firms.

In our setting, Brexit referendum offers a unique quasi-natural experiment for three main reasons. First, the "Leave" vote came as a surprise as it was largely unexpected. It is noteworthy that even the main betting companies in the United Kingdom (Ladbrokes, Betfair, and William Hill) had not anticipated such an outcome. Second, the Brexit uncertainty persisted well beyond the referendum, marking a period of several rounds of exit negotiations. Third, several studies have shown that the Brexit uncertainty was transmitted across international borders (Bloom et al., 2019; Hill et al., 2019; Campello et al., 2022; Hassan et al., 2024).[14] As a matter of fact, a subset of U.S. firms were exposed to Brexit uncertainty by having headquarters, subsidiaries, and customers in the United Kingdom (Ho et al., 2024). Hence, this allows us to identify both treated (exposed to Brexit) and control (unexposed to Brexit) firms and conduct a difference-in-differences (DiD) analysis.

To identify treated and control firms, we utilize the text-based measure of Hassan et al. (2024) to proxy for U.S. firms' Brexit exposure. In their *BrexitExposure* measure, Hassan et al. (2024) analyze quarterly earnings call transcripts and count the frequency with which Brexit is discussed in these calls. Accordingly, in the spirit of Ho et al. (2024), we identify as treated firms those exhibiting positive *BrexitExposure* during any quarter from July 2016 to June 2017, and as control firms those having zero *BrexitExposure* over the same period.[15]

Fig. 3 illustrates the evolution of crash risk (*NSkew* and *Duvol*) for our treated and control firms from 2014 to 2017. We observe that treated firms realize a surge in both stock price crash risk measures from 2016 and onwards. On the contrary, both measures are relatively flat for control firms over the whole period. More importantly, before 2016, crash risk for both treated and control firms appears to move in a parallel fashion, providing some preliminary validity for our identification strategy. Hence, we proceed with our DiD analysis, by estimating the following regression model:

$$\text{Crash risk}_{i,t} = a + b_1\text{Treated} + b_2\text{Post} + b_3\text{Treated}\times\text{Post} + b_4X_{t-1} + \text{IndystryFE} + \text{YearFE} + \varepsilon_{i,t} \tag{10}$$

where *Treated* is a dummy variable that equals 1 for firms with positive *BrexitExposure* during any quarter from July 2016 to June 2017, and 0 for firms with zero *BrexitExposure* over the same period, *Post* is a dummy variable that equals 1 for periods after the Brexit vote, and 0 otherwise.[16] For consistency, we include the same control variables and fixed effects as in our baseline models. Therefore, *Treated* × *Post* is the DiD estimator. To be consistent with our conjectures, we expect the DiD estimator to be positive and statistically significant.

Table 6 reports the results of this analysis. Columns 1 and 2 refer to the period before and after the Brexit referendum. In both cases, the DiD estimator is positive and statistically significant at the 5 % level, supporting our argument for a causal relationship between political risk and stock price crashes. Columns 3 and 4 repeat the DiD exercise in a hypothetical (pseudo) treatment period, which spans from 2002 to 2005 (with 2004 being the treatment year). The statistically insignificant coefficients of the pseudo DiD estimator provide support for the validity of the parallel trends hypothesis in our setting.

5. Additional analysis

So far, we have established a positive relationship between firm-level political risk and stock price crash risk. In what follows, we provide further insights on this relationship. First, we conduct a channel analysis to explore the underlying mechanisms behind our findings. Second, we examine the impact of firms' intangibles intensity on the *Political risk* - *Crash risk* relationship. Third, we test whether and how agency mechanisms can explain our results. Fourth, we conduct subsample analysis to investigate whether internal or external corporate governance mechanisms can moderate the positive impact of political risk on stock price crashes. Finally, we conduct a battery of robustness tests to verify the stability of our results.

5.1. Channel analysis

According to our proposed mechanisms, we assume that stocks with higher idiosyncratic volatility, higher pricing errors, and higher distress risk will be more susceptible to crashes. We also expect political risk to impact firms towards this direction, since political risk creates uncertainty regarding the firms' valuation (Pastor and Veronesi, 2012). This increased valuation uncertainty suggests greater information asymmetry among investors, leading to more idiosyncratically volatile, less informative, and more distressed stock prices.

To test our transmission channels, we follow a two-step regression approach as in Liang and Renneboog (2017), Griffin et al. (2021), and Duan et al. (2021). In the first step, we use each one of the three mediators as the dependent variable in separate regressions, and we regress them on *Political risk*, baseline controls, and industry and year fixed effects. In the second step, we use the predicted values for our mediators as the main explanatory variables in regressions of *NSkew* and *Duvol.* Essentially, the predicted values measure the firms' *Idiosyncratic volatility*, *Equity mispricing*, or *Probability of default* conditional on their values of *Political risk*. This two-step regression analysis is akin to a 2SLS IV approach, with the difference that *Political risk* is not an instrument for our mediators, since it can operate on stock price crash risk through pathways other than the ones we proposed in this study.

Table 7 presents the results of this analysis. Columns 1, 4, and 7, report the results of the first step. Consistent with our expectations, *Political risk* is positive and statistically significant at the 5 % level in the regressions of *Idiosyncratic volatility* and *Equity mispricing*, and positive and statistically significant at the 10 % level in the regression of *Probability of default*. Importantly, the results of the second stage also support our conjectures. In the regressions of *NSkew* (columns 2, 5, and 8), all predicted mediators are positive and statistically significant at the 10 % level, or better. Similarly, in the regression of *Duvol* (columns 3, 6, and 9) all predicted mediators are positive and statistically significant at the 5 % level. Overall, our results indicate that the positive relationship between Political risk and stock price crash risk is mediated through more idiosyncratically volatile, less informative, and more distressed stock prices.[17]

[14] Other studies show the macroeconomic effects of Brexit uncertainty on the UK, but also on other economies internationally (Makrychoriti and Spyrou, 2023).

[15] To ensure that we do not include in the treated group firms with positive sentiment towards Brexit, we exclude the few cases where *BrexitNetSentiment* is positive.

[16] In our reported specification, we drop 2016 to avoid transition effects. In untabulated analysis we also include 2016, or expand the post-Brexit period to 2018, and we obtain similar findings. We refrain from adding many more years in the DiD analysis to mitigate any confounding effects.

[17] To provide additional support for our empirical findings, we resort to a SEM analysis. The benefit of this approach is that it allows us to quantify both direct and indirect effects of causal relationships. Table A3 in the Appendices presents the results of this analysis. The results are in line with our conjectures, since both direct and indirect effects are statistically significant for all three mediators. The magnitude of the indirect effect (relative to the total effect) ranges from 7.69 % to 17.65 %.

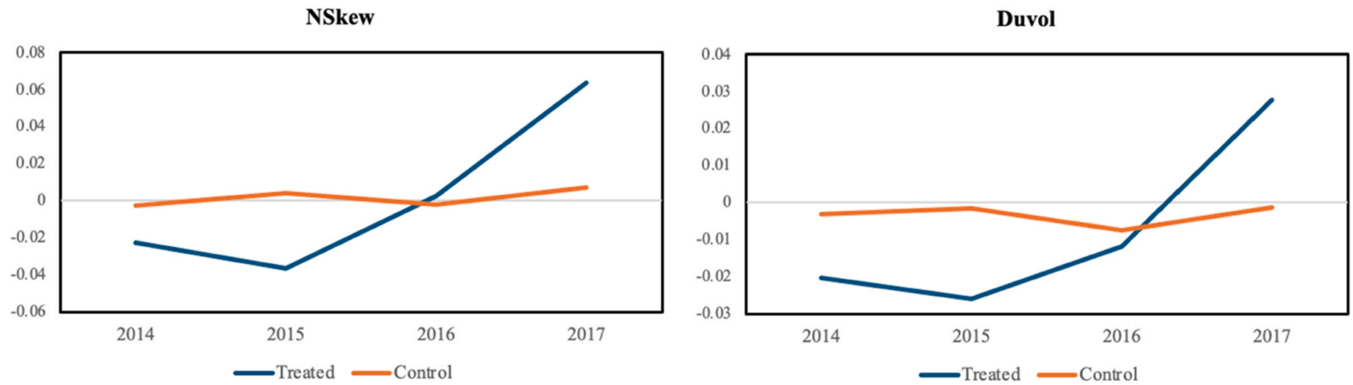

Fig. 3. Crash risk for treated and control groups around Brexit. The figure illustrates the evolution of crash risk (*Nskew* and *Duvol*) for our treated (positive Brexit exposure in any quarter from July 2016 to June 2017) and control (zero Brexit exposure in any quarter from July 2016 to June 2017) firms from 2014 to 2017. NSkew and Duvol are yearly averages for treated and control firms.

Table 6
A quasi-natural experiment. This table reports the difference-in-differences (DiD) estimates for stock price crash risk before and after the Brexit vote (2014–2017). *Treated* is a dummy variable that equals 1 if a firm has positive Brexit exposure in any quarter from July 2016 to June 2017, and 0 otherwise. *Post* is a dummy variable which equals 1 in 2017, and 0 otherwise (2016 is omitted to account for any transition effects). The interaction term *Treated* × *Post* is the DiD estimator. Columns 1 and 2 report the results of our DiD regressions. Columns 3 and 4 report the results of pseudo-DiD regressions, where the pseudo-treatment year is 2005, and 2003 and 2004 are the pre-treatment years. All continuous variables are winsorized at 1 % and 99 % level. *T*-statistics (in parentheses) are based on standard errors clustered at the firm and year levels. The symbols *, **, and *** denote statistical significance at the 10 %, 5 % and 1 % levels, respectively, using a 2-tail test.

	Treatment		*Pseudo-Treatment*	
	NSkew	Duvol	NSkew	Duvol
Variables	(1)	(2)	(3)	(4)
Treated	-0.033	-0.022	-0.019	-0.010
	(-1.19)	(-1.63)	(-0.83)	(-0.83)
Post	0.024	0.014	-0.083**	-0.054***
	(0.71)	(0.86)	(-2.45)	(-3.09)
Treated × Post	0.091**	0.052**	0.021	0.011
	(2.06)	(2.43)	(0.43)	(0.47)
Baseline controls	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	5735	5735	4189	4189
Adjusted R^2	0.010	0.013	0.021	0.030

5.2. The impact of intangibles assets

As mentioned before, one motivation for our study is the growth of intangible assets in the recent decades. Wu and Lai (2020) show that intangible-intensive firms are more susceptible to crashes, because they are subject to high valuation uncertainty and intense information asymmetry. Along these lines, Barth et al. (2023) suggest that the growth of intangibles assets is associated with higher heterogeneity in investors' beliefs. Therefore, we should expect our findings to be more pronounced in intangible-intensive firms, since the positive *Political risk* - *Crash risk* relationship is at least partially mediated through more noisy and less informative stock prices.

To investigate this issue, we replicate the baseline regressions of Table 3 (columns 3 and 4) by interacting *Political risk* with *Intangibles intensity*. To be in line with our conjectures, we expect their joint effect to be positive and statistically significant. We present the results of this analysis in Panel A of Table 8. We find the interaction term *Political risk* × *Intangibles intensity* to be positive and statistically significant at the 5 % level in the regression of *NSkew* and at the 10 % level in the regression of *Duvol*. Therefore, these results highlight the importance of the joint effect in explaining stock price crash risk.

To provide a more comprehensive understanding of our findings, we estimate the average marginal effect of *Political risk* on stock price crash risk for different values of *Intangibles intensity*. We present the average marginal effects in Panel B of Table 8. We observe that *Political risk* is not statistically significant for very low values of *Intangibles intensity*. These results suggest that firms with a greater emphasis on physical assets, such as manufacturing or construction firms, are less vulnerable to price crashes even when they are exposed to political risk. By contrast, the impact of *Political risk* on crash risk becomes stronger as we move from low values to above median values of *Intangibles intensity*.[18]

5.3. Political risk and bad news hoarding

In this section, we examine whether the agency-driven bad news hoarding mechanism can explain our results. We do so for two main reasons. First, information asymmetry constitutes one necessary condition of managerial bad news hoarding. Jin and Myers (2006) theorize that opportunistic managers exploit information asymmetries to conceal bad news from their investors. Considering that politically risky firms are subject to higher valuation uncertainty, corporate insiders may have a greater capacity to withhold unfavorable information and not disclose it to outsiders. Second, the valuation of intangible assets is subject to considerable uncertainties, lacking universal standard for evaluation (Barth et al., 2001; Wu and Lai, 2020). This ambiguity grants managers greater discretion in evaluating intangible assets, such as goodwill (Li et al., 2011). In turn, this discretion could provide managers with more leeway to hide negative information from outsiders. Collectively, we posit that managers of politically risky firms, especially those managing intangible-intensive firms, are more likely to hoard bad news.

In the crash risk literature, bad news hoarding is manifested through two main channels: (1) financial reporting opacity, and (2) overinvestment (Habib et al., 2018). To proxy for financial reporting opacity, we use *Opacity*, which is measured as the 3-year moving sum of discretionary accruals (Hutton et al., 2009). To measure *Overinvestment*, we follow the approach of Balachandran et al. (2020). Then, we re-run our baseline regressions by using either *Opacity* or *Overinvestment* as the dependent variable, while we also breakdown our sample based on *Intangibles intensity* (above-and-below median values).[19]

Table 9 presents the results of this analysis. In the case of *Opacity*, *Political risk* bears a positive but not statistically significant coefficient at

[18] The median value of *Intangibles intensity* in our sample is 0.176.

[19] In our baseline regressions, *Opacity* serves as a control. Apparently in the regression of *Opacity,* we exclude it from our baseline control list.

Table 7

Channel analysis. This table presents the channel analysis using two-stage regressions as in Liang and Renneboog (2017). Columns 1, 4, and 7, present the first-stage regressions, where the dependent variable is *Idiosyncratic volatility*, *Equity mispricing*, and *Probability of default*, respectively, and the main variable of interest is *Political risk*. Columns 2, 3, 5, 6, 8, and 9 present the second-stage regressions, where the dependent variable is either *NSkew* or *Duvol*, and the main variable of interest if the predicted mediator from step one. All continuous variables are winsorized at 1 % and 99 % level. *T*-statistics (in parentheses) are based on standard errors clustered at the firm level. The symbols *, **, and *** denote statistical significance at the 10 %, 5 % and 1 % levels, respectively, using a 2-tail test.

	Idiosyncratic volatility	NSkew	Duvol	Equity misprcing	NSkew	Duvol	Probability of default	NSkew	Duvol
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Political risk	0.002**			0.001**			0.004*		
	(2.40)			(2.82)			(1.76)		
$\widehat{\text{Idiosyncratic volatility}}$		4.016*	2.249**						
		(1.96)	(2.37)						
$\widehat{\text{Equity mispricing}}$					8.594**	4.428**			
					(2.53)	(2.85)			
$\widehat{\text{Probability of default}}$								1.607*	0.904**
								(1.97)	(2.33)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	38,097	38,097	38,097	33,309	33,309	33,302	32,412	32,412	32,405
Adjusted R^2	0.716	0.022	0.029	0.523	0.022	0.028	0.289	0.020	0.026

Table 8

The impact of intangibles assets. Panel A replicates the baseline regressions of Table 3 (columns 3 and 4) by interacting *Political risk* with *Intangibles intensity*. All continuous variables are winsorized at 1 % and 99 % level. *T*-statistics (in parentheses) are based on standard errors clustered at the firm level. Panel B presents the marginal effects of the regressions presented in Panel A. The first column presents the 8 values of *Intangibles intensity*. *dy/dx* reports the marginal effects and Z-score reports *Z*-statistics based on standard errors obtained with the Delta-method. The symbols *, **, and *** denote statistical significance at the 10 %, 5 % and 1 % levels, respectively, using a 2-tail test.

Panel A: Effect of Intangibles	NSkew	Duvol
Variables	(1)	(2)
Political risk	0.000	0.001
	(0.00)	(0.40)
Intangibles intensity	-0.072	-0.026
	(-0.64)	(-0.47)
Political risk × Intangibles intensity	0.050**	0.021*
	(2.04)	(1.76)
Baseline controls	Yes	Yes
Industry FE	Yes	Yes
Year FE	Yes	Yes
N	31,483	31,483
Adjusted R^2	0.062	0.064

Panel B: Marginal Effects	NSkew		Duvol	
Intangibles intensity	dy/dx	Z-score	dy/dx	Z-score
0.00	0.000	0.00	0.001	0.40
0.08	0.004	0.74	0.003	1.09
0.16	0.008	1.63	0.005*	1.89
0.24	0.012**	2.34	0.006**	2.49
0.32	0.016***	2.65	0.008***	2.71
0.40	0.020***	2.72	0.010***	2.71
0.48	0.024***	2.70	0.011***	2.63
0.56	0.028***	2.64	0.013**	2.54

conventional levels (for the whole sample). However, when we breakdown our sample to high-versus-low *Intangibles intensity* firms, we obtain more insightful results. More precisely, *Political risk* is positive and statistically significant at the 5 % level for intangibles-intensive firms, while it loses any significance in the subsample of firms with below median *Intangibles intensity* values. Therefore, these results suggest that heightened political risk could incentivize managers of intangible-intensive firms to hide bad news through earnings manipulation. On the contrary, this result does not hold for less intangible-intensive firms,

Table 9

Agency theory channels. This table summarizes regression results for a sample of U.S. firms over the period 2002–2019. In models 1–3, the dependent variable is Opacity as in Hutton et al. (2009). In models 4–6, the dependent variable Is Overinvestment as in Balachandran et al. (2020). High (Low) refers to firms with above (below) median *Intangibles intensity* values. All continuous variables are winsorized at 1 % and 99 % level. *T*-statistics (in parentheses) are based on standard errors clustered at the firm and year levels. The symbols *, **, and *** denote statistical significance at the 10 %, 5 % and 1 % levels, respectively, using a 2-tail test.

	Opacity			*Overinvestment*		
	All	High	Low	All	High	Low
Intangibles intensity	(1)	(2)	(3)	(4)	(5)	(6)
Political risk	0.009	0.015**	-0.004	0.000	-0.015	0.016
	(1.48)	(2.15)	(-0.38)	(0.05)	(-1.01)	(1.21)
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	33,055	16,528	16,527	17,011	8504	8506
Adjusted R^2	0.542	0.553	0.560	0.117	0.144	0.096

as there is less room for subjectivity in asset evaluation.[20] Hence, considering that the positive *Political risk* – *Crash risk* relationship is mainly evident in intangible-intensive firms, and that *Opacity* is positively associated with crash risk in our regressions (see Table 3), then, *Opacity* can substantially contribute in explaining our results. Finally, *Political* risk is not significantly associated with *Overinvestment.* This finding is expected to a large extend, as it has been documented that firms exposed to political risk retrench investments (Hassan et al., 2019; Banerjee and Dutta, 2022).

5.4. *The role of corporate governance mechanisms*

We have documented that managerial opportunism, as proxied by earnings manipulation, can play a role in the positive relationship between political risk and crash risk. We now examine whether corporate

[20] The mean difference in coefficient across the two subsamples is statistically significant at the 5 % (t=2.37). Furthermore, in untabulated analysis, we interact *Political risk* with *Intangibles intensity* as in Table 8. We find the coefficient of the interaction term to be positive and statistically significant at the 5 % level. Marginal effects analysis suggests that the positive effect *Political risk* on *Opacity* is concentrated in firms with above-median values of *Intangibles intensity*. Overall, these findings further support our conjectures.

governance mechanism can moderate this relationship. In fact, previous studies have shown that strong corporate governance can effectively mitigate information asymmetry and stock price crash risk (Chen et al., 2022; Jin and Wu, 2023). In our context, the rationale is that strong corporate governance should constrain the ability of managers to hoard bad news, even in intangible-intensive and politically risky firms.

To investigate the potential moderating effects of corporate governance, we conduct a subsample analysis based on both external and internal corporate governance mechanisms. In terms of external governance mechanisms, we explore the role of institutional ownership and analysts' coverage. Chen et al. (2017) find that the effect of earnings smoothing on stock price crash risk is more pronounced in firms with lower institutional holdings or less analyst coverage. The rationale is that strong investor monitoring, discourages managerial opportunistic behavior (An and Zhang, 2013; Callen and Fang, 2013), and higher analyst coverage mitigates information asymmetry between insiders and outsiders (Habib et al., 2018). When it comes to internal governance mechanisms, we examine the moderating impact of board size and managerial ability. Andreou et al. (2016) report a negative relationship between board size and stock price crash risk, while Al Mamun et al. (2020) document that managerial ability is associated with less firm crashes. Finally, we also examine the impact of Environmental, Social, and Governance (ESG) scores. While ESG scores are not a corporate governance mechanism per se, they are closely related to corporate governance practices. As a matter of fact, Kim et al. (2014) show that corporate social responsibility is negatively related to crash risk, as managers of socially responsible firms are less likely to hoard bad news.

In Panels A to E of Table 10, we divide the sample according to the median value of: (1) Institutional ownership, (2) Analysts coverage, (3) Board size, (4) Managerial ability, and (5) ESG scores.[21] Then, we re-run the baseline regressions of Table 3. We find that the positive relationship between political risk and crash risk is observed only in those firms with weaker corporate governance practices. By contrast, *Political risk* is not statistically significant in any subsample with above-median values in corporate governance proxies.[22] Therefore, these findings underscore the relevance of agency issues in our findings.

Table 10
The impact of corporate governance: sub-sample analysis. This table presents the baseline estimates of Table 3 (Columns 3 and 4) for 5 different subsamples. In Panels A to E, we divide the sample into two main samples according to the median value of: (1) *Institutional ownership*, (2) *Analyst coverage* (3) *Board size*, (4) *Managerial ability*, and (5) *ESG scores*, respectively. The dependent variable is *NSkew* in models 1 and 2, and *Duvol* in models 3 and 4, respectively. All continuous variables are winsorized at 1 % and 99 % level. *T*-statistics (in parentheses) are based on standard errors clustered at the firm level and year levels. The symbols *, **, and *** denote statistical significance at the 10 %, 5 % and 1 % levels, respectively, using a 2-tail test.

	High	Low	High	Low
	NSkew		Duvol	
Panel A: Institutional ownership	(1)	(2)	(3)	(4)
Political risk	-0.002	0.017*	-0.003	0.008*
	(-0.17)	(1.91)	(-0.46)	(1.85)
Baseline controls	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	4691	4491	4691	4490
Adjusted R^2	0.059	0.084	0.066	0.091
Panel B: Analyst coverage	(1)	(2)	(3)	(4)
Political risk	0.004	0.016**	0.002	0.008***
	(0.54)	(2.82)	(0.57)	(3.15)
Baseline controls	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	15,944	19,073	15,944	19,073
Adjusted R^2	0.044	0.065	0.047	0.070
Panel C: Board size	(1)	(2)	(3)	(4)
Political risk	0.003	0.015*	0.001	0.008**
	(0.57)	(2.01)	(0.52)	(2.24)
Baseline controls	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	17,813	16,094	17,808	16,091
Adjusted R^2	0.052	0.079	0.054	0.083
Panel D: Managerial ability	(1)	(2)	(3)	(4)
Political risk	0.004	0.010**	0.003	0.005**
	(0.43)	(2.22)	(0.63)	(2.11)
Baseline controls	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	12,355	12,324	12,351	12,323
Adjusted R^2	0.020	0.023	0.024	0.029
Panel E: ESG score	(1)	(2)	(3)	(4)
Political risk	0.006	0.028***	0.003	0.014***
	(1.38)	(2.71)	(0.56)	(2.70)
Baseline controls	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	4909	4928	4909	4928
Adjusted R2	0.017	0.011	0.026	0.017

5.5. *Robustness tests*

5.5.1. *Other sources of risk*

In our analysis so far, we have ignored the potential impact of aggregate risk measures in our results. While Hassan et al. (2019) argue that more than 90 % of the variation of political risk occurs at the firm-level, it could always be the case that our results reflect aggregate trends. For instance, Luo and Zhang (2020) and Han et al. (2023) find a positive relationship between Economic Policy Uncertainty (*EPU*) and stock price crash risk in China. Fiorillo et al. (2024) find that stock crash risk increases during times of intense geopolitical risk. Furthermore, previous relevant studies link tail risk with the Chicago Board Option Exchange (CBOE) Volatility index (VIX) (Park, 2015) or the CBOE SKEW index (Bevilacqua and Tunaru, 2021).

To alleviate this concern, we re-run our baseline regressions by separately including the EPU index of Baker et al. (2016), the Geopolitical Risk (*GPR*) index of Caldara and Iacoviello (2022), the CBOE Volatility (*VIX*) index and the CBOE SKEW (*SKEW*) index. In line with Huang et al. (2023), we do not include year fixed effects in these regressions, because all firms in the same year have the same value of aggregate risk measure. Thus, the inclusion of year fixed effects would raise multicollinearity concerns.[23] The results of this analysis are presented in Table 11. We observe that *Political risk* remains positive and statistically significant in all cases, suggesting that our results remain robust even after accounting for aggregate risk and uncertainty.

[21] Institutional ownership is measured as the percentage of shared help by all instructional investors (source: Refinitiv EIKON). Analyst coverage is measured by the number of analysts following the firm (source: I/B/E/S). Board size is measured as the number of directors on the firms' boards (source: BoardEx). Managerial ability is measured with the MA-score developed by Demerjian et al. (2012), and ESG scores are obtained from ASSET4.

[22] However, the mean difference in coefficients is either marginally statistically significant (in the *Board size* and *ESG* subsamples) or not statistically significant. Therefore, any inferences regarding moderating effects should be interpreted with caution.

[23] To illustrate the issue with this approach, rehdhfe Stata command drops each one of the four aggregate risk measures when year fixed effects are included in the regression. This happens because reghdfe prioritizes controlling for fixed effects efficiently, and therefore, drops collinear variables. By contrast, when we run the regression with the simple reg command (adding i.year to capture year fixed effects), Stata only drops some year dummies to avoid perfect multicollinearity. However, the Variance Inflation Factors (VIFs) of the aggregate risk measures exceed 10 in all cases, highlighting the multicollinearity issues of this approach.

Table 11
Other sources of risk. This table presents the baseline estimates of Table 3 (Columns 3 and 4) with the inclusion of additional aggregate risk measures, namely the Economic Policy Uncertainty (EPU) index, the Geopolitical Risk (GPR) index, the CBOE Volatility (VIX) index, and the CBOE SKEW (SKEW) index. All indices are expressed in terms of natural logarithms. All continuous variables are winsorized at 1 % and 99 % level. *T*-statistics (in parentheses) are based on standard errors clustered at the firm and year levels. The symbols *, **, and *** denote statistical significance at the 10 %, 5 % and 1 % levels, respectively, using a 2-tail test.

	NSkew	Duvol	NSkew	Duvol	NSkew	Duvol	NSkew	Duvol
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Political risk	0.006**	0.004**	0.008**	0.004**	0.006*	0.004**	0.007**	0.004**
	(2.03)	(2.29)	(2.36)	(2.80)	(1.82)	(2.02)	(2.23)	(2.56)
EPU	0.038	0.022						
	(1.41)	(1.34)						
GPR			0.016	0.016				
			(0.24)	(0.40)				
VIX					0.042	0.026		
					(1.25)	(1.31)		
SKEW							-0.127	-0.082
							(-0.31)	(-0.35)
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Linear time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	38,097	38,097	38,097	38,097	38,097	38,097	38,097	38,097
Adjusted R^2	0.020	0.025	0.020	0.025	0.020	0.025	0.020	0.025

Table 12
Robustness checks. This table presents our main robustness check using the baseline estimates of Table 3 (Columns 3 and 4). In Column 1, an alternative measure of crash risk is used (Count). In Columns 2 and 3, we include firm fixed effects. In columns 4 and 5, we include state fixed effects. In columns 6 and 7, we exclude the global financial crisis (GFC) years (2007–2009). All continuous variables are winsorized at 1 % and 99 % level. *T*-statistics (in parentheses) are based on standard errors clustered at the firm and year levels. The symbols *, **, and *** denote statistical significance at the 10 %, 5 % and 1 % levels, respectively, using a 2-tail test.

	Alt. crash risk	Firm FE		State FE		Excluding GFC	
	Count	NSkew	Duvol	NSkew	Duvol	NSkew	Duvol
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Political risk	0.006*	0.009**	0.005***	0.008**	0.004**	0.007*	0.004**
	(1.78)	(2.81)	(3.48)	(2.14)	(2.32)	(1.98)	(2.24)
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	Yes	No	No	No	No
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	No	No	Yes	Yes	Yes	Yes
State FE	No	No	No	Yes	Yes	No	No
N	37,745	37,745	37,745	33,602	33,602	31,412	31,412
Adjusted R^2	0.008	0.062	0.066	0.022	0.029	0.023	0.030

5.5.2. Additional robustness tests

Finally, we conduct several robustness tests to ensure the stability of our baseline results. First, we use an alternative measure of crash risk. More precisely, we follow Callen and Fang (2015), and use the number of crashes minus the number of jumps over the fiscal year (*Count*). Second, we replace industry fixed effects with firm fixed effects. Third, we consider the potential influence of political geography on our findings. Kim et al. (2012) demonstrate that firms headquartered in states closely aligned with the ruling political party experience higher returns compared to those in less politically connected states. Notably, substantial cross-state heterogeneity in *Political risk* is evident, as depicted in Fig. 2. To explore the possibility of such heterogeneity driving our results, we augment our baseline models with state fixed effects. Finally, we acknowledge the potential confounding influence of the global financial crisis (GFC) on our results. As illustrated in Fig. 1, indicators such as crash risk, firm-level political risk, idiosyncratic volatility, and probability of default experienced marked escalation during the GFC years. In response, we re-run our baseline regressions by excluding the years 2007, 2008, and 2009 from our sample. Our results hold.

6. Conclusions

In this study, we examine the link between firm-level political risk and stock price crash risk. Our baseline results reveal a positive association between political risk and crash risk. Our findings are robust across various model specifications and endogeneity concerns. Furthermore, to establish causality in our results, we use the Brexit referendum as an exogenous political shock. We find that after the referendum, firms exposed to Brexit were more likely to crash compared to unexposed firms.

In examining the underlying mechanisms behind our results, we initially draw upon the financial market explanation of Hong and Stein (2003), who theorize that investor disagreement over firms' value explain stock price crashes. To capture the investor perspective, we utilize three market-based variables: idiosyncratic volatility, equity mispricing, and probability of default. Our channel analysis suggests that the positive relationship between political risk and crash risk is at least partially mediated by higher idiosyncratic volatility, more pricing errors, and higher distress risk. Therefore, we argue that the increased valuation uncertainty of politically risky stocks should intensify heterogeneity in investors' beliefs, leading to higher stock price crash risk. Furthermore, we show that our results are more pronounced in infantile-intensive firms, as the valuation of such assets is complex and prone to subjectivity.

From the agency viewpoint, higher valuation uncertainty might also intensify information asymmetry between insiders and outsiders, leading to agency issues. In fact, when firm valuation is hard, managers may be more inclined to hide unfavourable news from their investors. In support of this explanation, we find that in intertangle-intensive firms, managers engage in earnings manipulation when faced with heightened political risk. Finally, subsample analysis indicates that the positive relationship between political risk and stock price crash risk is primarily observed in firms with weak corporate governance mechanisms.

Our findings are of interest to academics and practitioners. With crash risk proving resistant to diversification, asset pricing researchers are increasingly recognizing its pivotal role in shaping stock returns (Andreou et al., 2021). Consequently, identifying the extent to which political risk impacts stock price crashes is of paramount importance, particularly in this era of excalating political uncertainty.

CRediT authorship contribution statement

Emmanouil Pyrgiotakis: Writing – review & editing, Writing – original draft, Methodology, Data curation, Conceptualization. **Panagiota Makrychoriti:** Writing – review & editing, Writing – original draft, Methodology, Data curation, Conceptualization.

Appendix A

Table A1
Description of variables

Variable	Description
Nskew	The firms' negative skewness obtained from Eq. (2).
Duvol	The firm's down-to-up volatility obtained from Eq. (3).
Political risk	The natural logarithm of the firm's average quarterly political risk over the fiscal year. Firm political risk is measured as the PRisk of Hassan et al. (2019).
Idiosyncratic volatility	The annualized squared root of the residuals obtained from Eq. (1) for every firm.
Equity mispricing	Equity mispricing is calculated by multiplying the price informativeness measure of Bai et al. (2016) with —1. To compute the price informativeness measure, we first estimate Eq. (5). Then, for every year, we multiply the estimated coefficient $b_{h,t}$ with the standard deviation of the logarithmic ratio of market value to total assets, as in Eq. (6).
Probability of default	The firm's cumulative standard normal distribution of the negative distance to default. Firm's distance to default is obtained from the National University of Singapore's Credit Research Initiative database.
DTurnover	The firm's average monthly share turnover of the fiscal year minus the average monthly share turnover of the previous year. Monthly share turnover is calculated as the monthly share trading volume divided by shares outstanding.
Size	The natural logarithm of the firm's market value of equity.
ROA	The ratio of the firm's net income to the book value of assets.
BTM	The ratio of the firm's book value of equity to market value of equity.
Leverage	The ratio of the firm's book value of debt to the book value of assets.
Returns	The cumulative firm-specific weekly returns over the fiscal year.
Opacity	The three-year sum of discretionary accruals as in Hutton et al. (2009).
Ln(Age)	The natural logarithm of the firm's age plus one. Age is defined as the number of years since the IPO year.
Goodwill	The ratio of the firm's goodwill to the book value of assets.
Competitiveness	The industry-adjusted ratio of the firm's operating profits to sales.
Bid-ask spread	The firm's effective bid-ask spread calculated as twice the difference between the closing price and the midpoint of the bid-ask quote, divided by the midpoint of the bid-ask quote.
Beta	The firm's beta obtained by Eq. (1).
Number of analysts	The natural logarithm of the number of analysts following the firm (plus one). Data for the number of analysts are obtained from I/B/E/S.

Table A2
Political risk by industry. This table presents the distribution of political risk by industry group. Industry groups are based on the Fama–French 12 industry classifications. The distribution is based on the sample used for our baseline regressions (columns 3 and 4 of Table 3).

Fama-French industry group	Political risk	# observations	% of sample
Consumer Non-Durables	3.959	1669	4.38 %
Consumer Durables	4.111	892	2.34 %
Manufacturing	4.185	3671	9.64 %
Oil, Gas, and Coal Extraction Products	4.460	1216	3.19 %
Chemicals and Allied Products	4.194	985	2.59 %
Computers, Software, and Electronic Equipment	4.178	7704	20.22 %
Telephone and Television Transmission	4.069	987	2.59 %
Utilities	4.703	1486	3.90 %
Wholesale, Retail, and Some Services	3.920	3920	10.29 %
Healthcare, Medical Equipment, and Drugs	4.583	4026	10.57 %
Finance	4.705	5764	15.13 %
Other	4.375	5777	15.16 %
Total	4.320	38,097	100.00 %

Table A3
SEM results. This table presents the results of the structural equation model. We run four models. For each dependent variable (NSkew or Duvol) we run three models, one where the mediator variable is *Idiosyncratic volatility*, one where the mediator is the *Equity mispricing*, and one where the mediator variable is *Probability of default*. Direct effect represents the direct effect that firm political risk has on the stock price crash risk. Indirect effect represents the effect that firm political risk has on the stock price crash risk via the mediator variable. It is computed as the product of two path coefficients: (1) the coefficient that measures the effect of firm political risk on the mediator variable, and (2) the coefficient that measures the effect of the mediator variable on the stock price crash risk. Total effect is the sum of the direct and indirect effects. *Z*-statistics (in parentheses) are based on standard errors clustered at the firm and year levels. The symbols *, **, and *** denote statistical significance at the 10 %, 5 % and 1 % levels, respectively, using a 2-tail test.

Path	Direct effect	Indirect effect	Total effect	% Indirect effect to Total effect
Political risk-> Idiosyncratic volatility ->NSkew	0.0081**	0.0007*	0.0088**	7.95 %
	(2.27)	(1.87)	(2.41)	
Political risk-> Idiosyncratic volatility ->Duvol	0.0048**	0.0004*	0.0052**	7.69 %
	(2.44)	(1.88)	(2.58)	
Political risk-> Equity mispricing ->NSkew	0.0078***	0.0011*	0.0089***	12.36 %
	(2.75)	(1.87)	(2.80)	
Political risk-> Equity mispricing ->Duvol	0.0050**	0.0007*	0.0057***	12.28 %
	(2.88)	(1.92)	(3.03)	
Political risk-> Probability of default ->NSkew	0.0070*	0.0015**	0.0085**	17.65 %
	(1.66)	(2.25)	(1.99)	
Political risk-> Probability of default ->Duvol	0.0039*	0.0008**	0.0047**	17.02 %
	(1.84)	(2.25)	(2.20)	

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