High-frequency trading and stock price crash risk

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We investigate the effect of High Frequency Trading (HFT) on stock price crash risk by exploiting the staggered introduction of HFT across international exchanges. Using a difference-in-differences analysis, we find that stock price crash risk increases by more than 80% following the HFT presence. Notably, the positive HFT effect on crash risk is more pronounced on larger firms. Our results survive a battery of robustness tests, including propensity score matching and placebo tests. Channel analysis indicates that HFT increases liquidity and makes stock prices more noisy and less informative, which intensifies investor disagreement and encourages managerial bad news hoarding. Finally, the HFT effect is moderated in countries with well-developed derivatives markets, for firms with higher analyst coverage and in collectivist or religious societies.

Keywords: High-Frequency Trading; stock price crash risk; liquidity; information

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1. Introduction

Financial market and technological innovations such as exchange hosting and microwave network connectivity have significantly reduced trade execution latency. For example, round-trip latency between the Chicago Mercantile Exchange (CME) and the Intercontinental Exchange (ICE) currently stands at 355 microseconds.[1] These developments have given rise to the development of a new type of traders, High Frequency Traders (HFTs). Whilst it remains difficult to measure how much of trading that takes place in organised exchanges originates by HFTs, recent estimates show that High Frequency Trading (HFT) accounts for approximately 50% of the trading volume in US equity markets.[2,3] Despite the possible implications of HFT on asset pricing and trading behavior, its impact on stock price crash risk is largely unexplored.

To date, most of the relevant literature on HFTs focuses on two key aspects of asset prices: liquidity, and price informativeness. With regards to liquidity, both theoretical (Baldauf and Mollner, 2020; Yang and Zhu, 2020) and empirical (Hendershott et al., 2011; Brogaard et al., 2018) studies reach to the same conclusion; HFTs increase liquidity by narrowing bid-ask spreads. By contrast, the HFT effect on price informativeness is a highly debatble issue, and any inferences depend on how price informativess is measured. By focusing on short-horizon

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[1] See NASDAQ (2023). To provide more context, microwave networks cover the distance from Chicago to New York in 4.5 milliseconds (4,500 microseconds). At the speed of light this distance is covered in 4 milliseconds (see Shkilko and Sokolov, 2020).

[2] See detail in Breckenfelder (2019) and SEC (2020).

[3] HFT is defined as "*an algorithmic trading technique characterised by: (a) infrastructure intended to minimise network and other types of latencies, including at least one of the following facilities for algorithmic order entry: co-location, proximity hosting or high-speed direct electronic access; (b) system-determination of order initiation, generation, routing or execution without human intervention for individual trades or orders; and (c) high message intraday rates which constitute orders, quotes or cancellations.*" (ESMA 2020, p. 14).

effiency, Hendershott et al. (2011), and Boehmer et al. (2021) show that HFTs facitate the incorporation of existing information into stock prices. However, as demonstrated by Weller (2018), whilst HFTs may increase price efficiency by incorporating existing information into prices, they decrease price acquisitiveness with respect to new information.[4] To complement the latter findings, recent theory finds that HFTs decrease the incentives of informed traders to obtain new information (Baldauf and Mollner, 2020; Yang and Zhu, 2020). This happens because HFTs can anticipate informed order flow, and as such, they reduce the amount of rent that informed traders can extract from trading on private information. Therefore, it appears that HFTs induce a trade-off between liquidity and information production.

How does the above discussion align with existing theories of stock price crash risk? In the literature, there are two distinct, albeit not mutually exclusive, mechanisms that explain stock price crashes: investor-based and agency-based explanations. The investor viewpoint originates from Hong and Stein (2003), who theorize that investors' disagreement over a firms' fundamental value leads to stock price crashes. Due to short sales constraints, bearish investors' private information is not initially revealed in stock prices, as bearish investors will be sidelined by more bullish investors. However, when prices are falling, these accumulated negative signals are revealed, making stock prices more vulnerable to crashes. The agency theory, as outlined by Jin and Myers (2006), suggests that in the presence of information asymmetries, opportunistic managers hide bad news regarding their poor performance from investors. When it is no longer possible to conceal the bad news, the release of adverse information leads to a stock price crash. In both theories, the implicit assumption that the amount of hidden information, either in the form of bearish investors' private information or managerial bad news

[4] Relatedly, a line of research argues that HFT changes the composition of institutional ownership. In particular, HFT may encourage the entry and exit of short-term institutional investors (Fang et al., 2014). HFT may also "crowd out" institutional investors from traditional stock exchanges and towards dark pools (Goldstein et al., 2023). Indeed, Goldstein et al. (2023) show that fast traders (i.e. HFTs) crowd out slower traders (i.e. retail and institutional traders). In turn, Chang et al. (2017) show that the increase in transient investors and decrease in blockholder investors increases stock price crash risk.

hoarding, is the underlying cause of stock price crashes. Thus, to understand how HFT impacts crash risk, we should examine how liquidity and price informativeness impacts investors' disagreement and managerial opportunistic behaviours.

We expect HFT presence to increase stock price crash risk. From the investor perspective, HFTs could intensify investor disagreement in two possible ways. First, liquidity attracts short-term transient investors (Porter, 1992), discourages information acquisition from institutional investors (Fang et al., 2014), and coincides with more irrational or "noise" trading (Bloomfield et al., 2009). Second, HFTs presence is directly associated with less informative stock prices, as it reduces the investors' incentives to acquire fundamental information (Weller, 2018; Gider et al., 2021). From the agency perspective, HFTs may also incentivize managers to conceal bad news in two possible ways. First, managers may withhold bad news to avoid selling pressure by transient investors (Chang et al., 2017). Second, managers would have more leeway to conceal negative information when stock prices are more noisy and less informative (Andreou et al., 2023).

While our hypothesis for a positive HFT-crash risk relationship seems intuitive, it is not known a priori. In fact, HFT may reduce crash risk in two opposing ways. First, governance theory suggest that higher liquidity could reduce stock price crash risk, as it facilitates managerial monitoring by blockholders (Edmans, 2009). Second, there is a strand of literature which suggests that HFT improves price efficiency, at least in the short run. For instance, Brogaard et al. (2014) show that HFTs trade on the opposite direction of transitory pricing errors, and therefore, they could reduce investors' disagreement, thereby leading to lower crash risk. To the best of our knowledge, this is the first study that examines whether and to which direction HFT impacts stock price crash risk.

We use a sample of 307,358 firm-year observations (31,175 unique firms), spanning 28 years, 15 countries and 18 exchanges. We use order-cancellation ratios and mean trade sizes for each exchange to infer the HFT start dates (see Aitken et al., 2015) and split the sample to HFT and non-HFT exchanges. We first confirm that prior to the HFT start date, crash risk decreases for all exchanges. We then observe that after the HFT start date, stock price crash risk increases dramatically for the HFT exchanges but continues to decrease for non-HFT exchanges.

While HFTs seem to increase stock price crash risk, it is certainly likely that this relationship is not causal. To help establish causality, we use a generalized difference-in-differences (DiD) model with HFT start dates as an exogenous variable. Given that the timing of HFT start dates decisions vary across exchanges, we avoid a potential identification bias in the DiD regressions when the timing of the single shock coincides with exogenous unidentified variables that directly affect the dependent variable (see Roberts and Whited, 2013).

Our main finding is that the start of HFT is associated with an increase in stock price crash risk. This increase is economically significant. On average, firms in HFT exchanges experience an increase of 86.21% in crash risk after the HFT start. To alleviate any potential concerns regarding the DiD design, we employ Propensity Score Matching (PSM) to match HFT firms with similar non-HFT firms. The results are statistically and economically similar to the baseline findings. Further, HFTs tend to be more active on large-cap stocks (Brogaard et al., 2014), we therefore expect that the effect of HFT on stock price crash risk will be more pronounced for large market capitalization stocks. Our analysis confirms this hypothesis. Economically, stock price crash risk for large market capitalization stocks more than triples after the HFT start date, while for smaller stock, the corresponding increase is less than 50%. Our analysis survives a placebo test using the same set of stocks, setting our pseudo-HFT period to start 3 years before the actual HFT year. Further, we confirm that there is no evidence of a pre-treatment trend before the HFT start date. Finally, in line with our baseline hypothesis, we also show that HFT start increases stock price synchronicity.

We then investigate the mechanisms via which HFT increases stock price crash risk. First, Andreou et al. (2023) postulate that whilst agency-based mechanisms have dominated this literature, investor-based explanations are better in explaining the surge of stock price crashes in recent decades. Besides, HFT is expected to primarily affect stock price crash risk via an effect on stock liquidity and information acquisition. We use the firms' effective closing bid-ask spread to proxy for liquidity. We rely on the trading measure introduced by Llorente et al. (2002) to proxy for informed trading and we follow Bai et al. (2016) to estimate a measure of stock price informativeness. We expect that firms listed on HFT exchanges will experience an increase in stock liquidity and noise trading and a decrease in stock price informativeness, all of which increase stock price crash risk. Using a two-stage regression methodology, we confirm that investor-based mechanisms drive the effect of HFT on stock price crashes.

Second, we investigate whether agency-based mechanisms may also explain our results. Conceptually, there are two prominent agency explanations of stock price crashes: financial reporting opacity and overinvestment. Both mechanisms suggest that in the presence of information asymmetries, managers will disguise bad news through earnings management practices or hide bad news through overinvestments. At some point managers are not anymore able to hide bad news which, when released, lead to stock price crashes (Jin and Myers, 2006). There are two ways how HFT may give rise to such agency problems. First, HFT crowds out long-term institutional investors (Goldstein et al., 2023) who according to Edmans (2009) cause prices to reflect fundamental information and they have strong incentives to monitor firms. Second, HFT discourages information acquisition (see Gider et al., 2021 and Weller, 2018) which wides the information asymmetry between managers and investors. Both outcomes encourage financial reporting opacity and overinvestment, therefore increasing stock price crash risk. We use the performance-controlled accruals model of Tucker and Zarowin (2006) to measure financial reporting opacity and we follow the Schrand and Zechman (2012) and Ahmed and Duellman (2013) methods to measure overinvestment. We show that the HFT effect on crash risk is mediated through managerial bad news hoarding, and particularly through reporting opacity and overinvestment.

Next, we look for possible moderating mechanisms. First, derivatives make prices more informative and improve firm valuation (Lin et al., 2009). We hypothesize that in exchanges with a well-developed derivatives market, the HFT effect will be less pronounced. Second, financial analyst coverage improves the firms' information environment and mitigates information asymmetry (Brennan and Subrahmanyam, 1995). Empirically, higher analyst coverage is associated with a decrease in stock price crash risk (Kim et al., 2019). We therefore hypothesize that analyst coverage will further moderate the effect of HFT on stock price crash risk. Third, in line with previous studies (see An et al., 2018, Dang et al., 2019 and Callen and Fang, 2015), cultural norms – such as individualism and collectivism - and religiosity are expected to moderate the impact of HFT start on stock price crash risk. Overall, our results confirm that the positive effect of HFT start on stock price crash risk is less pronounced in markets with a well-developed derivatives market, when firms are followed by a greater number of analysts, in less individualistic and more collectivist or religious societies.

Next, we confirm that the effect of HFT start on stock price crash risk is not symmetric to both tails of the idiosyncratic returns distribution. Intuitively, we expect that HFT does not affect the number of jumps as managers have no incentive to hide good information from investors. Our results confirm this hypothesis. In further tests, we examine the effect of exchange-hosting ie colocation, on stock price crash risk. We expect that the effect of colocation on stock price crash risk to be relatively weak as colocation is not a required condition for the development of HFT but instead reflects the exchanges' response to demand by HFTs (see Aitken et al., 2017). Indeed, our results show that the magnitude of the effect of HFT on stock price crash risk is weaker when we consider colocation as the HFT start date.

We contribute to two strands of the literature. First, we contribute to the burgeoning literature which studies the determinants of stock price crash risk by documenting a positive link between HFT and stock price crash risk (Jin and Myers, 2006; Hutton et al., 2009; Kim et al., 2011; Kim et al., 2019; among others). While most of this literature focuses solely on agency explanations (Andreou et al., 2023), we show that under specific circumstances, investor-based and agency-based explanations are complementary in explaining stock price crash risk. Furthermore, we add to the growing literature which links technological advancements to stock price crash risk (Hu et al., 2021; Li et al., 2023). Second, we contribute to the market microstructure literature that investigates the effect of HFT activities on financial markets. Despite the initial assessment that HFT increases stock liquidity and enhances the price discovery process (Boehmer et al., 2021; Brogaard et al., 2018; Hendershott et al., 2011; Jones, 2013), a nascent literature has emerged investigating the long-term effects of HFT on market efficiency (see Bhattacharya et al., 2020; Gider et al., 2019; Weller, 2018). We extend this literature by documenting the impact of HFT on the ability of prices to reflect all firm-specific information.

The study has important implications for investors, managers and policy makers. We highlight that the crowding-out effect of HFT on institutional investors possibly fuels the increase in stock price crash risk that we have witnessed over the recent decades (see Andreou et al., 2022). We further highlight that HFT weakens the role of stock prices in managerial decision-making. As more exchanges are keen to attract HFTs, we show that this may lead to more future stock price crashes.

The rest of the paper is organised as follows. In Section 2 we present our data and key variables and describe our research design. In Section 3 we present the empirical results and in Section 4 we conclude.

2. Data and methodology

2.1. Sample selection

In our study, we focus on a sample of firms traded in 18 international exchanges (13 HFT and 5 non-HFT) in 15 countries. Our sample selection is based on Aitken et al. (2015) and Gider et al. (2021). To identify HFT exchanges, Aitken et al. (2015) use data on order cancellations and individual trades' size, because HFTs are typically associated with more order cancelations and smaller trades. More precisely, they find dates followed by a substantial and persistent increase in order cancelations (not available for five HFT exchanges). Then, they set. the earlier of the two dates (if they are both available for a specific exchange) as the HFT start date. If order cancellation dates are not available, they simply use the trade size dates. Furthermore, we follow Gider et al. (2021), and we use fixe exchanges as a control group or non-HFT exchanges (Hong Kong, Korea, Shanghai, Shenzhen, and Singapore). These exchanges are not exposed to HFT due to either strict trading regulations, intraday transactions tax, or high trading fees.

As a next step, we collect a sample of firms listed in these 18 exchanges. To be included in our sample, the firms should meet the following criteria. First, the firm should have available stock return data in Datastream and accounting data in Worldscope. Second, we restrict our selection to primary quotes of major securities. Third, stocks with less than 30 weeks of stock return data in a year are excluded. Third, we exclude firms from our sample in cases where the exchange has less than 25 stocks that meet the third criterion in that year. Finally, all stock return data are winsorized at the 1% and 99% to account for the presence of outliers. Our final sample consists of 31,2198 firms in all 18 exchanges.

2.2. Crash risk measures

In line with the extant literature, we calculate our crash risk measures using firm-specific weekly returns. Specifically, we follow Morck et al. (2000), Jin and Myers (2006), Kim et al. (2011), among others, and we estimate we estimate the following expanded market model:

$$\begin{aligned} r_{i,j,t} = a_{i,j} + b_{1,i} r_{m,j,t} + b_{2,i}\left[r_{U.S.,t} + ER_{j,t} \right] + b_{3,i} r_{m,j,t-1} + b_{4,i}\left[r_{U.S.,t-1} + ER_{j,t-1} \right] \\ + b_{5,i} r_{m,j,t-2} + b_{6,i}\left[r_{U.S.,t-2} + ER_{j,t-2} \right] + b_{7,i} r_{m,j,t+1} + b_{8,i}\left[r_{U.S.,t+1} + ER_{j,t+1} \right] + \\ b_{9,i} r_{m,j,t+2} + + b_{10,i}\left[r_{U.S.,t+2} + ER_{j,t+2} \right] + \varepsilon_{i.j,t} \end{aligned} \quad (1)$$

where i is a firm index, j is a country 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 $r_{m,j,\mathrm{t}}$ denotes the domestic market index return in in week t of the same year.[5] In addition, $r_{U.S.,t} + ER_{j,t}$ represents the return of the U.S. market, adjusted for the local currency changes compared to the U.S. dollar. Following Francis et al. (2015), the returns are on a weekly basis (Wednesday-to-Wednesday), to account for the Monday effect. To overcome thin trading issues, we include lags and leads as in Dimson (1979). Then, the firm-specific return w of firm i in country j in week t is defined as follows:

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

To proxy for crash risk, we use the two continuous measures of Chen et al. (2001), which are the most frequently used measures in the relevant literature (Hutton et al., 2009; Kim et al., 2011; DeFond et al., 2015; Balachandran et al., 2020). More precisely, we use the negative skewness (*NSkew*), and the “down-to-up volatility” (*Duvol*). *NSkew* is defined as follows:

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

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

where n is the number of weekly firm-specific returns in a year T. Higher value of *NSkew* represents higher stock price crash risk.

Duvol is calculated as follows:

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

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 with *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.

Finally, we also use a discrete measure (*Count*) as in Callen and Fang (2015). Following Hutton et al. (2009), we calculate the frequency of firm-specific weekly returns exceeding 3.09 standard deviations below and above the mean-specific return in a year. The cut-off value of 3.09 standard deviations is selected to generate o frequency of 0.1% extreme left and right tails in the normal distribution. Then, *Count* is defined as the difference between downside frequencies and upside frequencies.

2.3. Research design

We investigate the impact of HFT on stock price crash risk using an international panel of 18 stock exchanges. Similar to Hu et al. (2020) and Balachandran et al. (2020), we estimate the following generalized DiD model:

$$Crash\ risk_{i,t} = a + b_1 HFT + b_2 X_{t-1} + Industry\ FE + Year\ FE + Exchange\ FE + e_{i,t} \tag{5}$$

where HFT is a dummy variable which takes the value of 1 for all post-HFT years, and 0 otherwise.[6] X_{t-1} represents a vector of firm- and country-level control variables at year t-1. In all regressions, we include industry, year, and exchange fixed effects. Following Petersen (2008), standard errors are clustered at the firm level. To control for the presence of outliers, all continuous variables are winsorized at the 1% and 99% levels.

To alleviate potential firm-level omitted variable bias, we include several controls that are frequently used in the crash risk literature (Chen et al., 2001; Hutton et al., 2009; Xu et al., 2014; Callen and Fang, 2015; DeFond et al., 2015; Andreou et al., 2021; Yousefi et al., 2023, among others). Specially, we use the following firm-level controls: (1) the one-year lagged value of *NSkew* ($NSkew_{t-1}$), (2) the stocks' detrended turnover (*DTurnover*), (3) the natural logarithm of firms' market value of equity (*Size*), (4) the book-to market ratio (*BTM*), (5) the ratio of total debt to total assets (*Leverage*), (6) the return on assets (*ROA*), the average firm-specific weekly returns (*Return*s), (8) the absolute discretionary accruals (|*DACC*|), and (9) firm's age (*Ln(Age)*). All these variables have been found to significantly affect stock price crash risk.

Furthermore, we include four country-level controls. To account for macroeconomic conditions, we use the annual GDP growth rate (*GDP growth*) as in An et al. (2015; 2018). In line with Jin and Myers (2006) and Dang et al. (2020), we use the good government index (*Good government*) to proxy for the level of investor protection. Finally, use the firm Herfindahl index (*Firm HHI*) and the industry Herfindahl index (*Industry HHI*) to proxy for firm-and-industry competition as in Jin and Myers (2006), Kim and Zhang (2016) and Jia (2018).

[6] To alleviate any transition effects, we exclude the actual HFT year from the analysis because HFT start dates may be in the middle of that year. For instance, in the case of New York Stock exchange (NYSE), the HFT start date is in May 2003.

2.4. Summary statistics

Panel A of Table 1 provides an exchange-wise breakdown of the HFT years and the frequency of stock price crashes. More precisely, for the 13 HFT exchanges, we report the year of the initial HFT presence. Additionally, we report the colocation years for 14 exchanges. Collocation refers to the housing of trading firms' computer servers within the exchanges. Gider et al. (2021) argue that exchanges may offer colocation services as an endogenous response to high HFT demand, a fact which may justify why all HFT years precede the colocation years. Furthermore, % crashes represents the percentage of firms (by exchange) that experienced at least one crash during the examination period. Finally, Panel B presents the frequency of crashes (for the whole sample) by year.

[Insert Table 1 Around Here]

Table 2 reports the summary statistics of the HFT sample (Panel A) and the non-HFT sample (Panel B). Statistical tests of mean differences (untabulated for brevity) reveal some interesting patterns. On average, firms traded on HFT exchanges are more prone to stock price crashes relative to firms traded on non-HFT exchanges. Furthermore, the former group of firms are smaller, less profitable, and less leveraged. Furthermore, they have higher turnover ratios and are more prone to earnings manipulation (higher values of discretionary accruals). Finally, investor protection is better in countries with HFT exchanges.

[Insert Table 2 Around Here]

Table 3 shows the Pearson correlation coefficients between our crash risk measures and the firm-and country-level controls used in our analysis. As expected, all three crash risk measures are highly correlated. In terms of our control variables, we observe a modest degree of collinearity.[7] Nonetheless, average inflation factors (VIFs) are below 10 in all models, suggesting that multicollinearity should not be a concern in our empirical setting (Wooldridge, 2016).

[Insert Table 3 Around Here]

3. Main results

3.1. DiD regressions

Table 4 reports the DiD regression results. Columns 1 to 3 report results for the full sample, where the dependent variable is either *NSkew*, *Duvol*, or *Count*. In all 3 columns, the coefficient of *HFT* is positive and statistically significant at the 1% level, indicating that HFT presence is associated with a significant increase in stock price crash risk. Importantly, the coefficient of HFT is also highly economically significant. For instance, in the case of *NSkew*, a coefficient of 0.125 suggests that on average, following the HFT presence, firms in HFT exchanges experience an increase of 86.21% (=0.125/0.145) in crash risk relative to their mean value. For *Duvol* and *Count* the corresponding figures are 84.15% (=0.069/0.082) and 114.86% (=0.85/0.074).

In terms of our control variables, our results are similar to what reported in previous studies. Initially, the one-year-lagged value of *NSkew* is positive and statistically significant at the 1% level in all three models (Hutton et al., 2009; Callen and Fang, 2015; Chang et al., 2017). In line with Chen et al. (2001), Callen and Fang (2015), Ni and Zhu (2016), among others, *Dturnover* and *Size* are positively associated to stock price crash risk. Furthermore, more profitable firms with higher leverage and higher past returns are more likely to crash in the future (Kim et al., 2021; Andreou et al., 2021), while older firms and firms with lower market

[7] The highest positive correlation is observed between *Firm HHI* and *Industry HHI* (0.842). However, their inclusion in our regression models does not alter our findings.

valuation are less prone to crashes (Balachandran et al., 2020; Zhang et al., 2022). Moving to our country-level controls, firms in countries with higher GDP growth are more likely to crash (An et al., 2018), while investor protection rights relate to lower stock price crash risk (Jin and Myers, 2006). Finally, *Firm HHI* is positively related to crash risk, as firms in concentrated environments are more likely to withhold bad news (Kim and Zhang, 2016).

Columns 4 to 6 repeat the baseline regressions for a 6-year sample centered around the HFT year (hereafter [–3, +3] sample) as in Hu et al. (2020). We do so, to alleviate the impact of any confounding events.[8] Furthermore, inspired by Balachandran et al. (2020), we only include firms with available data for the entire [–3, +3] sample. This approach ensures that we do not include firms that disappear before or list after the HFT presence. Notably, this approach yields similar results with our baseline regressions.

[Insert Table 4 Around Here]

Figure 1 provides a graphical illustration of our DiD estimator for the [–3, +3] sample. In the graph, we also include data for the HFT year, which constitutes the event year or year 0. For non-HFT exchanges, there is no event year, as there is no HFT presence in these exchanges during our sample period. Hence, in the spirit of Glendening et al. (2016), we assign 2004, the median year of our examination period, as a pseudo-HFT year. Despite being ad hoc, 2004 serves as a reasonable choice since the event year ranges from 2003 to 2006 in 11 out of the 13 HFT exchanges. By looking at the figure, we observe a parallel and declining trend of crash risk in both treated (HFT) and control (non-HFT) groups. However, after the event year, crash risk in HFT exchanges increases dramatically, while it remains stable and subsequently decreases in non-HFT exchanges. Overall, this figure provides preliminary support for the validity of our DiD estimator.

[8] Our results are similar if we use a 10-year sample [–5, +5]

[Insert Figure 1 Around Here]

One potential concern with our baseline DiD regression is the systematic differences in characteristics between treated and control groups observed in Table 2. On the one hand, the inclusion of controls in our baseline regression might help alleviating this issue. On the other hand, if there is poor distributional overlap in controls between treated and control groups, our regression estimates may be biased (Heckman et al., 1998). Hence, to account for this issue, we follow common practice, and we employ the PSM approach. Specifically, we match firms traded on HFT exchanges with very similar firms traded on non-HFT firms.[9] This approach ensures that we compare the crash risk measures of similar firms across exchanges (Bartram et al., 2012). Then, we rerun our baseline regressions of Table 4 on PSM-matched samples as in Leledakis and Pyrgiotakis (2022). The results for the entire sample and for the [–3, +3] sample confirm our baseline findings (see Table A2 in the Appendices).

Another concern could be that the HFT effect will be solely concentrated on large firms, since high frequency traders are more active on large-cap stocks (Brogaard et al., 2014). To investigate this issue, we breakdown our sample on above-and-below median sized firms, and we re-run our baseline regressions. Overall, we find that HFT is positive and highly statistically significant in the regressions of both subsamples, with is magnitude being substantially larger for above-median stocks (see Table A3 in the Appendices). In fact, the difference in the economic significance between above-and-below median firms is noteworthy. For insurance, in terms of *NSkew*, above-median firms experience an increase of 275.00% (0.132/0.048) following the HFT presence, while the corresponding figure of below-median firms is only 48.40% (0.106/0.219).[10]

[9] Matching is done using the nearest neighbor approach (one-to-one matching) with replacement).
[10] The mean *NSkew* value for above (below) median firms is –0.048 (–0.219).

following the HFT presence, firms in HFT exchanges experience an increase of 86.21% (=0.125/0.145)

To illustrate the difference in economic significance between a

3.2. Testing for parallel trends

The main underlying assumption behind a DiD model is that the parallel trends hypothesis holds (Roberts and Whited, 2013). In other words, this hypothesis suggests that in the pre-treatment period, both treated and control groups were following parallel trends. Figure 1 provides some preliminary evidence to support this hypothesis. However, we further investigate the validity of our DiD estimator in two ways: (1) we conduct a placebo test, and (2) we examine the dynamic effects of HFT presence on crash risk.

To conduct the placebo test, we follow the approach of similar crash risk studies (Hu et al., 2020; Balachandran et al., 2020). More precisely, we focus only on the pre-treatment (pre-HFT period),[11] and we construct a pseudo-HFT period. In line with Hu et al. (2020), we set our pseudo-HFT period to start 3 years before the actual HFT year. Then, we re-run our baseline regressions for the pre-HFT period. The purpose of this analysis is to examine whether in the absence of HFT, the average change in our dependent variable would be similar for the treated and control groups. Panel A of Table 5 presents the results of this analysis. Interestingly, the coefficient of our pseudo-HFT indicator in statistically insignificant in all three regressions, suggesting that our placebo test confirms the validity of the parallel trends hypothesis.

We estimate the dynamic HFT effects following Bertrand and Mullainathan (2003). More precisely, we replace the HFT indicator of Table 4 with the following five indicators: $Year^{-2}$, $Year^{-1}$, $Year^{-1}$, $Year^{0}$, $Year^{1}$, and $Year^{2+}$, which equal one for 2 years prior to the HFT, the pre-

[11] For the non-HFT exchanges, we follow the approach of Figure 1, and we use 2004 as the cut-off year to define the pre-HFT period.

HFT year, the actual HFT year, one year after the HFT, and 2 or more years after the HFT. In this setting, we include the HFT year to allow for transition effects. Panel B of Table 5 presents the results for this analysis. For the pre-HFT periods, both time indicators are statistically insignificant. In the HFT year, the coefficient of the time indicator is negative and statistically significant at the 10% level, or better. Then, in the post-HFT years, we find that both time indicators are positive and highly statistically significant. Importantly, the magnitude of the coefficient of $Year^{2+}$ is substantially larger than the one of $Year^{1}$ in all three models. Collectively, these results indicate that there is no evidence of a pre-treatment trend before the HFT presence. In fact, the HFT impact on crash risk is evident in the year after the event, and it increases in the subsequent years. Therefore, both the placebo test and the dynamic effects analysis support the validity of our DiD estimator.

[Insert Table 5 Around Here]

4. Channel analysis

In our analysis so far, we have established a robust positive link between HFT and stock price crash risk. However, it is still unclear which are the underlying mechanisms that explain this relationship. In the relevant literature, the mechanisms behind stock price crashes fall to two main categories, namely investor-based and agency-based channels. In what follows, we identify all plausible mechanisms by examining how HFT relates to both theories of stock price crash risk.

4.1. Investor mechanism

Hong and Stein (2003) argue that investors' differences of opinion over a firm's fundamental value is associated with an increase in stock price crash risk. Although insightful, investor-based explanations have received limited attention in the literature, possibly due to difficulties in measuring heterogeneity in investors' beliefs. To investigate how HFTs may intensify investor disagreement, we examine its impact on: (1) stock's liquidity, (2) noise trading, and (3) equity mispricing.

The rationale behind using liquidity in this context stems from the short-termism theory, which predicts that stocks with higher liquidity attract more transient investors with short-term horizons (Porter, 1992). In support of this theory, later studies find that liquidity is associated with more noise trading by uninformed investors (Bloomfield et al., 2009), and less institutional monitoring (Fang et al., 2014). At the same time, HFTs are associated with a significance increase in stock liquidity (see Brogaard et al., 2018; Hendershott et al., 2011; Jones, 2013). Hence, to proxy for liquidity, we use the firm's effective bid-ask spread calculated as twice the absolute difference between the closing price and the midpoint of the bid-ask quote, divided by the midpoint of the bid-ask quote (Bessembinder, 2003). Then, considering that a higher spread means lower liquidity, we follow Chang et al. (2017) and define *Liquidity* as the annual effective bid-ask spread multiplied by –1.

The use of noise trading and equity mispricing is more straightforward for two reasons. First, noise trading, accompanied by excessive trading, reflects investors heterogenous expectations (Mahoney, 1995), while less informative prices allow for higher investor disagreements (Sokolinskiy, 2020). Second, previous literature indicates that HFT leads to an increase in noise trading (McGroarty et al., 2019), an increase in pricing errors (Weller, 2018) and a decrease in price informativeness (Gider et al., 2021).

To proxy for noise trading, we rely on the informed trading measure introduced by Llorente et al. (2002). The authors develop an informed trading measure based on stock return autocorrelation conditional on trading volume. They show that their measure captures firm–specific private information impounded into stock prices, a finding which is verified by later empirical studies (Fernandes and Ferreira, 2009; De Cesari and Huang-Meier, 2015). Specifically, we estimate the following time–series regression for each firm–year:

$$r_{i,j,t} = a_{i,j} + \gamma_{i,j,}r_{i,j,t-1} + \theta_{i,j}r_{i,j,t-1}V_{i,j,t-1} + e_{i,j,t} \tag{6}$$

where $r_{i,j,t}$ is the weekly return of firm i of country j in week t of a year and $V_{i,j,t-1}$ is the log turnover detrended by subtracting a 26–week moving average. The coefficient of the interaction term (θ) is the measure of informed trading. According to Llorente et al. (2002), higher (lower) values of θ indicate informed (noise) trading. Hence, similar to *Liquidity*, we define *Noise trading* as θ multiplied by –1.

We proxy for equity mispricing by estimating the level of stock price informativeness. To do so, we follow Bai et al. (2016) which propose a measure of stock price informativeness. Their measure reflects the ability of current market prices to forecast future earnings. Therefore, to compute their measure we run cross-sectional regressions of future earnings on current market prices. More precisely, for every year t and for every horizon h (3 years ahead from year t), we estimate the following regression:

$$\frac{EBIT_{i,t+h}}{Assets_{i,t}} = a_{t,h} + b_{t,h}\log(\frac{MV_{i,t}}{Assets_{i,t}}) + c_{t,h}(\frac{EBIT_{i,t}}{Assets_{i,t}}) + c^{s}{}_{t,h}I^{s}{}_{i,t,} + e_{i,t,h} \tag{7}$$

where *EBIT* stands for the ratio of current earnings before interest and taxes and *MV* represents the firm's market capitalization. In addition, $I^{s}{}_{t,h}$ denotes an industry indicator for firm i (based on 2–digit SIC codes). Then, price informativeness is computed as follows:

$$Price\ informativeness = \hat{b}_{t,h}\sigma_t(\log(\frac{MV_{i,t}}{Assets_{i,t}})) \tag{8}$$

Finally, we define *Equity mispricing* as *price informativeness* multiplied by –1.

To test our conjectures, we follow a a two-step regression approach as in Duan et al. (2021). In the first step, we use either *Liquidity*, *Noise trading,* or *Equity mispricing* as the dependent variable, and our main variable of interest is HFT. In the second step, we use our crash risk measures as dependent variables in separate regressions, and the main variable of interest is the predicted values of step one.

Table 6 presents the results of this analysis. Columns 1, 5, and 9 report the results of the first-stage regressions, and the remaining columns report results of our two-stage regressions. Collectively, our results support our predictions. In detail, in all first-stage regressions, *HFT* bears a positive and statistically significant coefficient at the 1% level. Furthermore, in the second-stage regressions, the coefficients of all our predicter mediator variables are positive and statistically significant at the 1% level.[12] In sum, after the introduction of HFT, stock prices are more liquid, more noisy, and less informative, all of which increase crash risk.

[Insert Table 6 Around Here]

4.2. Agency mechanisms

There are two prominent agency explanations of stock price crashes: (1) financial reporting opacity, and (2) overinvestment. In their seminal study, Jin and Myers (2006) suggest that in the presence of information asymmetries, managers may conceal firm-specific bad news through opaque financial statements. At some point, the stockpiled bad news reaches the public, leading to a crash. This theory has received ample of empirical support from later studies (Hutton et al., 2009; Kim et al., 2011; Callen and Fang, 2015; An et al., 2018; among others). The rationale for the overinvestment story is somewhat similar. Benmelech et al. (2010) argue that when information asymmetry is high, opportunistic managers camouflage bad news through overinvesting in unprofitable projects. At some point, the bad news regarding the poor performance of the overinvested funds will be revealed, leading to a crash.

[12] apart from the coefficient of Equity $\widehat{\text{mispricing}}$in column (12) which is statistically significant at the 10% level.

How does HFT gives rise to such agency problems? Conceptually, HFT presence may intensify information asymmetries, considering that high frequency traders hold securities for short horizons and as such, they are less likely to efficiently monitor managerial behaviour (Fang et al., 2014; Gider et al., 2021). In the absence of effective monitoring, incentivized to conceal managers are more likely to engage in opportunistic practises such as financial reporting opacity, or overinvestment. Furthermore, HFTs are known to increase stocks' liquidity (Jarnecic and Snape, 2014; Malceniece et al., 2019). In turn, Chang et al. (2017) show that more liquid stocks are more prone to crashes. They argue that in a liquid market, managers may be less willing to disclose bad news for fear that transient investors will respond by selling their stocks. Hence, to avoid this selling pressure, managers may withhold bad news either though reporting opacity or overinvestment.

To measure financial reporting opacity, we use the performance-controlled accruals model of Tucker and Zarowin (2006), which adds firms' return on assets as an additional control in the frequency-used accrual models of Jones (1991). We do so, as previous studies indicate that the model of Jones might be misspecified for well-or poor-performing firms (Dechow et al., 1995; Kothari et al., 2005). In detail, for each country *j* at year *t*, we estimate the following cross-sectional regression:

$$\frac{TACC_{i,t}}{TA_{i,t-1}} = \alpha_0\left(\frac{1}{TA_{i,t-1}}\right) + \alpha_1\left(\frac{\Delta REV_{i,t}}{TA_{i,t-1}}\right) + \alpha_2\left(\frac{GPPE_{i,t}}{TA_{i,t-1}}\right) + \alpha_3 ROA_{i,t} + \varepsilon_t \qquad (9)$$

where $TACC_{i,t}$ is the total accruals of firm *i* at year *t*, $\Delta REV_{i,t}$ is the change in revenue of firm *i* from year *t-1* to year *t*, $GPPE_{i,t}$ is the level of gross property, plant, and equipment of firm *i* at year *t*, $ROA_{i,t}$ is the return on assets of firm *i* at year *t*, and $TA_{i,t}$ is the total assets of firm *i* at year *t-1*. We use the residuals from these regressions as proxies for discretionary accruals (DACC). Then, in line with Hutton et al. (2009) we use the three-year moving sum of absolute discretionary accruals |DACC| as proxy for *Opacity*.

We measure overinvestment following Schrand and Zechman (2012) and Ahmed and Duellman (2013). Specifically, for each industry group (using 2-digit SIC codes) and year, we regress firms' asset growth on sales growth, controlling for country-level GDP growth and country fixed effects. The residuals from these regressions measure the unexpected investment. Then, in line with Balachandran et al. (2020), *Overinvestment* equals the unexpected investment for firm-year observations with positive residuals, and 0 otherwise.[13]

To test our conjectures, we follow the same two-step approach as in Table 6. Hence, columns 1 and 5 of Table 7 report the results of the first stage regression, where the dependent variable is either *Opacity* (column 1) or *Overinvestment* (column 5). In line with our conjectures, HFT presence intensifies managerial opportunistic behaviours, as *HFT* enters both regressions with positive and statistically significant coefficients at the 1% level. Moving to the second stage regressions (columns, 2, 3, 4, 6, 7, and 8), we see that both predicted mediator variables are positive and statistically significant at the 1% level. Overall, our results indicate that the positive HFT effect on crash risk is mediated through managerial bad news hoarding, and particularly through reporting opacity and overinvestment.

[Insert Table 7 Around Here]

5. Additional analysis & robustness tests

5.1. Moderating effects

Thus far, our results suggest that HFTs increase stock price crash risk in two main ways. First, HFTs are typically transient investors with short-term horizons that do not engage in price

[13] Our results remain stable if we define *Overinvestment* as a dummy variable similar to Hu et al. (2020) or if we simply use all the residuals (both positive and negative).

discovery, leading to more noisy and less informative stock prices. Second, investor short-termism and weak monitoring allow managers to engage in opportunistic bad news hoarding. Nonetheless, the HFT effect should presumably vary with cross-country or cross-firm differences in information dissemination and external governance.

Theoretically, the existence of a derivatives market improves price informativeness and reduce information asymmetry, as it allows managers to reduce noise associated with their firms' quality (DeMarzo and Duffie, 1995). In support of these arguments, Lin et al. (2009) find that the use of derivatives improves firm valuation by reducing agency-related information asymmetry between managers and investors. On these grounds, we expect the HFT effect to be moderated in markets with a well-developed derivatives market. To proxy for derivatives use, we follow Bartram (2019) and we collect firm-level data on forex exchange, interest rate and commodity price derivatives. Then, we rank the size of the derivatives market relative to the market of the other countries in the sample. Following Lievenbrück and Schmid (2014), the size is calculated by summing the FX and IR derivative markets daily turnover and standardizing by nominal GDP. Since the availability of derivative market turnover data is not continuous[14], the derivative market turnover is linearly interpolated for the years in between.

Financial analyst coverage can also improve the firms' information environment and mitigate information asymmetry (Brennan and Subrahmanyam, 1995). In fact, Kim et al. (2019) show that higher analyst coverage decreases stock price crash risk since financial analysts act as firms' external monitors. Further empirical studies support the negative relationship between crash risk and analyst coverage because analyst monitoring reduces managerial bad news hoarding (Balachandran et al., 2020; An et al., 2020). Hence, we collect

[14] The derivative market size is available for the year 1992, 1995, 1998, 2001, 2004, 2007, 2010, 2013, 2016, and 2019.

financial analysts' data from the Institutional Broker's Estimate System (I/B/E/S) database. Specifically, we define *Analyst coverage* as the number of analysts following a firm.

We examine the moderating impact of derivatives use of analysts' coverage om HFT, we interact either *Derivatives market rank* or *Analyst coverage* with *HFT* and we include the interaction terms (along with the main effects) in our baseline regressions. We present the results of this analysis in Table 8. Consistent with our expectations, both interaction terms *Derivatives market rank* × *HFT* and *Analyst coverage* × *HFT* are negative and statistically significant at the 1% level in all models (besides model 3 where the interaction term is statistically significant at the 10% level). Collectively, these findings suggest that the positive HFT effect is less pronounced in markets with a well-developed derivatives market, or when there is strong external monitoring by financial analysts.

[Insert Table 8 Around Here]

To provide a better insight on the moderating impact of *Derivatives market rank* or *Analyst coverage* on *HFT*, we compute the average marginal effects (see Table A4 in the Appendices). More precisely, we compute the marginal effects for eight different values of *Derivatives market rank* or *Analyst coverage.* In both cases, our values range form the 5th to the 95th percentile. We observe that HFT is positive and statistically significant in all cases, suggesting HFTs drive stock price crashes even in markets/firms with extensive derivatives use and intense analyst monitoring. However, it is noteworthy the magnitude of the effect drops almost by half when we move from the 5th to the 95th percentile.

5.2. Culture & religion

In our cross-country setting, there are several deep-rooted societal characteristics that might affect stock price crash risk. In fact, previous literature has linked societal culture and religiosity with earnings management and crash risk. For instance, An et al. (2018) and Dang et al. (2019) show how a national culture dimension, namely individualism, impacts stock price crashes. They show that crash risk is higher in individualistic societies, since individualistic cultural traits (personal autonomy, self-fulfilment, and personal achievement) encourage managerial bad news hoarding. Furthermore, Callen and Fang (2015) find that firms in religious countries are less likely to crash, because religion is a set of social norms which discourages managerial opportunistic behaviours.

We examine whether our results may be influenced by cross-country differences in societal culture or religiosity. To investigate the impact of culture, we rely on the Hofstede's individualism index for the 15 sampled countries. To measure moderating effects, we define *Collectivism* as the individualism index multiplied by –1. Conceptually, collectivism is the opposite of individualism, and refers to societies where the groups' interests are more important than those of the individuals. Considering that earnings manipulation is less likely in these societies (An et al., 2018; Dang et al., 2019), we expect *Collectivism* to be negatively associated with crash risk. What's more important however, is its ability to moderate the positive HFT effect on crash risk. Thus, we also include an interaction term between *Collectivism* and *HFT.* To measure religiosity, we follow Barro and McCleary (2003), and we use the total share of religion adherence per country (*Religion*). Similar to *Collectivism,* we also include interactions between *Religion* and *HFT.*

Overall, the results reported in Table 9 are supportive of previous studies, as it appears that crash risk is lower in collectivist or religious societies. Despite their inclusion however, the coefficient of *HFT* remains positive and statistically significant at the 1% level in all cases. Importantly, the two interaction terms *HFT* × *Collectivism* and *HFT* × *Religion* are negative and statistically significant at the 5% level, or better, in all models. On the one hand, collectivist culture and religion can effectively moderate the positive HFT effect on crash risk. On the other hand, the magnitude of the moderating effect suggests that HFT increases stock price crash risk even in collectivist or religious societies.[15]

[Insert Table 9 Around Here]

5.3. HFTs and stock price synchronicity

If our arguments regarding the HFT effect on stock price crash risk is valid, then we should expect HFT to also increase stock price synchronicity. Stock price synchronicity refers to the extent to which stock prices move in a parallel fashion. The large literature on stock price synchronicity suggests that in less transparent environments, stock prices are less informative and more synchronous (Morck et al. 2000; Jin and Myers, 2006; Hutton et al., 2009; Eun et al., 2015; among others). In our channel analysis, we have established that HFT deteriorates firms' information environment as it encourages managerial opportunistic behaviour. Furthermore, we also show that HFT makes prices more noisy and less informative due to the short-term horizon of transient investors. Hence, it follows that HFT should increase synchronicity, as stock prices would reflect less firm-specific information.

To measure stock price synchronicity, we use the R^2 obtained from the expanded market model of eq. (1). Then, considering that R^2 is highly skewed and possess the econometrically undesirable characteristic to be bounded between 0 and 1, we apply the following logistic transformation:

$$Synchronicity_{i,j,t} = Ln\left(\frac{R^2_{i,j,t}}{1-R^2_{i,j,t}}\right) \tag{10}$$

Bartram et al. (2012) and Li et al. (2014) indicate that inferences based on synchronicity should be interpreted with caution, as R^2 incorporates elements of both systematic and

[15] In unablated analysis, we estimate the average marginal effects. We find that HFT is positive and highly statistically significant at all different values of collectivism or religiosity (values range from the 5th to the 95th percentile).

unsystematic (idiosyncratic) risk. Hence, to obtain more robust inferences, we follow Bartram et al. (2012), and we decompose synchronicity into systematic risk and idiosyncratic volatility. More precisely, *Idiosyncratic volatility* is measured as the annualized standard deviation of the residuals obtained from eq. (1). *Systematic risk* is measured as the annualized square root of the difference between the variance of weekly firm-specific returns and the variance of the residuals from eq. (1). As a next step, we re-run our baseline regressions by replacing the crash risk measures with either *Synchronicity*, *Systematic risk*, or *Idiosyncratic volatility*.

Table 10 presents the results of this analysis. Our findings are line with our expectations. In the regression of *Synchronicity*, *HFT* enters with a positive and statistically significant coefficient at the 1% level. Focusing on the components, we observe that HFTs increase systematic risk and decrease idiosyncratic volatility (both coefficients are statistically significant at the 1% level). Interestingly, several studies assume idiosyncratic volatility to be a proxy for price informativeness as it measures the amount of firm-specific information impounded into stock prices (Durnev et al., 2003; 2004; Brockman and Yan, 2009). Altogether, we could argue that HFTs increase stock price synchronicity because they decrease the amount of firm-specific information impounded into stock prices.

[Insert Table 10 Around Here]

5.4. Collocation and crash risk

As an additional test, we examine whether collocation (the housing of computer serves in the exchanges) had a similar effect on stock price crash risk. To do so, we replicate our baseline findings by replacing our HFT dummy with a Collocation dummy (1 for every year after the collocation year, and 0 otherwise).[16]

[16] Similar to HFT, we also exclude the collocation year from our analysis.

As we show on Table 1, in all our exchanges, collocation takes place after the introduction of HFT. On the one hand, we could expect collocation to further increase stock price crashes as the introduction of servers may facilitate more high-frequency trading. On the other hand, we should also expect the parallel trends hypothesis of our DiD model to be violated, since HFTs precede collocation.

Table 11 reports the results of this analysis. Columns 1 to 3 report the baseline regressions with the use of *Collocation* as the main explanatory variable. We observe that *Collocation* is positive and statistically significant at the 1% level in all three models, suggesting that crash risk further increases after the housing of trading firms' serves in the exchanges. It is noteworthy however that the magnitude of its coefficient is substantially smaller than the ones of Table 4. Furthermore, in columns 4 to 6, we re-run the dynamic effects analysis of Bertrand and Mullainathan (2003). Unsurprisingly, we find the coefficient of the pre-collocation years to be highly statistically significant in all cases.[17] These results indicate that the introduction of HFT is the main driver of the sudden increase in stock price crashes in our exchanges. However, the housing of serves further strengthens this increasing trend.

[Insert Table 11 Around Here]

5.5. Crash versus jumps

Another potential concern is whether the HFT effect is symmetric in both tails of the firm-specific returns distribution. In other words, does HFT presence leads to more stock price jumps? According to our results, we would not expect so, as we have already documented that HFT increases the skewness and decreases the volatility of firm-specific returns. According to our intuition, we should not expect so, as managers would not hide good information from investors.

[17] The parallel trends assumption is also violated when we re-run our regressions for a pseudo-Collocation period.

To address this issue, we decompose *Count* into the number of crashes (*Crash count*) and number of jumps (*Jump count*). Furthermore, we also define two dummies: (1) *Crash dummy*, which 1 equals 1 if the firm had experienced at least one crash week during a year, and 0 otherwise, and (2) *Jump dummy*, which 1 equals 1 if the firm had experienced at least one jump week during a year, and 0 otherwise. Then, we re-run our baseline analysis by replacing our three crash risk measures with either *Crash dummy*, *Crash count*, *Jump dummy*, or *Jump count*.[18]

Table 12 presents the results of this analysis. The findings for *Crash dummy* and *Crash count* are in line with what we have reported so far. In fact, *HFT* enters both regressions with a positive and a statistically significant coefficient at the 1% level, suggesting that HFTs increase the number of future stock price crashes. On the contrary, *HFT* is indistinguishable from zero in both regressions of *Jump dummy* and *Jump count*, indicating that the HFTs do not create any positive outliers in the distribution of the firm-specific returns.

[Insert Table 12 Around Here]

5.6. Further robustness check

In this section, we employ a battery of robustness checks to ensure the stability of our results. More precisely, we re-estimate our baseline regression using different model specifications. First, we include firm fixed effects and we estimate two-way clustered standard errors (firm and year). Second, we use standard errors clustered at the exchange level. Third, we exclude the U.S. from our analysis, because it is the country with the largest proportion of observations in our sample (27.91%). Fourth, we exclude the period 2007-2009 as the years of the global financial crisis (GCF) from our sample, to examine whether our results are driven by these

[18] When we use our dummies as the dependent variable, we resort to a logistic regression approach. However, we obtain similar results using a linear probability model.

special years. Table 13 shows that our results remain robust across all different model specifications, as *HFT* remains positive and highly statistically significant in all cases.

[Insert Table 13 Around Here]

6. Conclusion

The introduction of HFT created considerable anxiety about the welfare implications of speed in financial markets. For example, Aquilina et al. (2022) show that the total global costs of the high frequency arms race is approximately $5 billion per year. Another area of concern is that HFT induces a trade-off between liquidity and information production, that is whilst liquidity in general improves, there is less incentive to obtain new information, making prices less informative (Baldauf and Mollner, 2020; Yang and Zhu, 2020). The latter also implies that HFTs crowd-out slow traders, retail and institutional investors (Goldstein et al., 2023).

In this paper, we conjecture that the above will lead to an increase in stock price crash risk. Using the staggered introduction of HFT start across 15 exchanges around the world, we find consistent evidence in favour of this hypothesis. Our main finding is that, on average, firms in HFT exchanges experience an increase of 86.21% in crash risk after the HFT start. Consistent with the empirical evidence that HFTs are more active in larger market capitalization stocks (Brogaard et al., 2014), we show that the effect of HFT start on stock price crash risk is more pronounced for larger stocks.

HFT trading increases stock liquidity and noise trading and decreases stock price informativeness, all of which increasing stock price crash risk. Further, HFT start increases reporting opacity and overinvestment. Our results support both financial-based and agency-based explanations of stock price crash risk. The presence of a well-developed derivatives market moderates this relationship. In line with the previous literature (see Kim et al., 2019, An et al., 2018, Dang et al., 2019 and Callen and Fang, 2015), number of analysts following a stock, individualism, collectivism and religion further moderate the effect of HFT start on stock price crash risk. As colocation is not a required condition for the development of HFT but instead reflects the exchanges' response to demand by HFTs (see Aitken et al., 2017), the effect of HFT start on stock price crash risk is smaller in magnitude when we consider colocation as an alternative definition for HFT start.

Stock price crash risk has increased steadily from 5.5% in 1950 to 27% in 2019 (Andreou et al., 2022). This paper shows that this increase in stock price crash risk is, at least partially, attributed to the introduction of HFT. Many exchanges around the world have offered exchange hosting services i.e. colocation, in an attempt to attract HFTs and thereby increase liquidity and boost profits (Brogaard et al., 2015; Frino et al., 2014). Our results show that exchange hosting contributes further, albeit in smaller magnitude, to this effect.

The debate on the effect of HFT activities on financial markets is still ongoing and recent evidence has emerged providing supporting evidence on both sides of this argument (Baldauf and Mollner, 2020; Bhattacharya et al., 2020). Our paper contributes to this debate.

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Figure1

Parallel trends around HFT year [-3, +3]

This graph depicts the average crash risk (NSkew or Duvol) for firms in both HFT and non-HFT exchanges over an event window of [-3, +3 years]. Year 0 represents the HFT year (pseudo HFT year) for firms in HFT (non-HFT) exchanges. Only firms with data over the whole 7-year period are included.

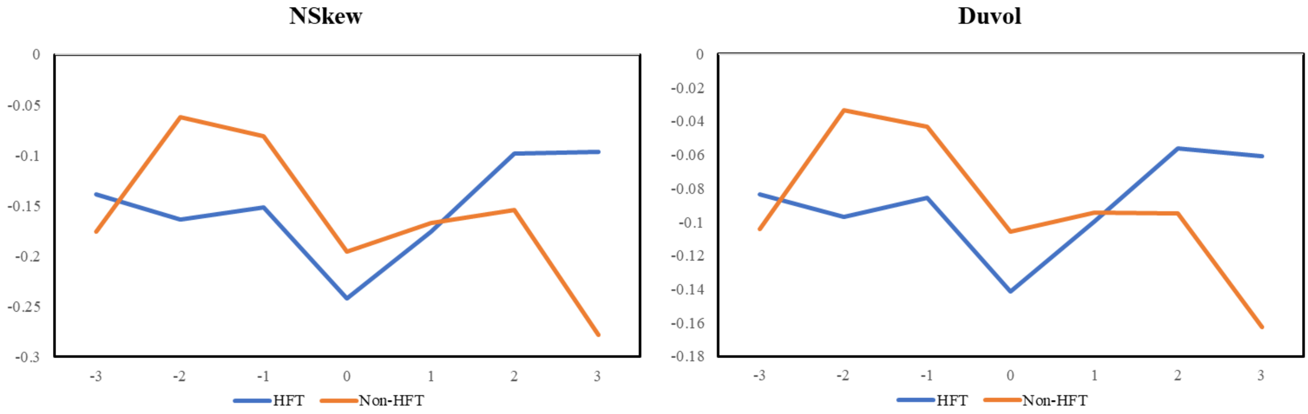

Table 1

Crashes distribution by exchange and year

This table reports the crashes distribution by stock exchange and year. Our sample consists of 18 exchanges during the period 1992 to 2019. Panel A reports years when HFT and collocation were introduced in the exchanges, the mean stock price crash occurrence by stock exchange, the total number of observations and the number of unique firms per exchange. Panel B reports the mean stock price crash occurrence by year.

Panel A: Stock price crashes by exchange

Exchange	Country	HFT	Collocation	% crashes	# obs	# firms
Australia Stock Exchange	Australia	2006	2008	0.129	23,485	2,608
Bombay Stock Exchange	India	2009	2010	0.090	9,757	1,412
Hongkong Stock Exchange	Hong Kong	N/A	2012	0.107	20,609	1,910
Korea Stock Exchange	South Korea	N/A	N/A	0.074	13,205	999
London Stock Exchange	U.K.	2006	2009	0.160	26,479	3,335
NASDAQ	U.S.	2003	2007	0.153	52,726	5,290
NSE India	India	2009	2010	0.059	573	69
NYSE	U.S.	2003	2008	0.157	36,230	3,180
New Zealand Stock Exchange	New Zealand	2004	N/A	0.143	656	91
OLSO Norway	Norway	2005	2010	0.127	2,603	340
Shanghai Stock Exchange	China	N/A	N/A	0.119	15,578	1,397
Shenzhen Stock Exchange	China	N/A	N/A	0.130	18,454	2,081
Singapore Stock Exchange	Singapore	N/A	2011	0.121	7,680	680
Stockholm Stock Exchange	Sweden	2005	2011	0.120	5,249	717
Swiss Stock Exchange	Switzerland	2004	2012	0.113	3,318	265
Tokyo Stock Exchange	Japan	2005	2010	0.114	59,518	4,312
Toronto Stock Exchange	Canada	2005	2008	0.093	14,095	1,645
XETRA Germany	Germany	2003	2006	0.111	8,498	888
Total				0.128	318,713	31,219

Panel B: Stock price crashes by year

1992	*1993*	*1994*	*1995*	*1996*	*1997*	*1998*
0.078	0.125	0.090	0.113	0.105	0.099	0.089
1999	*2000*	*2001*	*2002*	*2003*	*2004*	*2005*
0.064	0.099	0.136	0.120	0.139	0.144	0.155
2006	*2007*	*2008*	*2009*	*2010*	*2011*	*2012*
0.155	0.133	0.115	0.093	0.118	0.134	0.147
2013	*2014*	*2015*	*2016*	*2017*	*2018*	*2019*
0.144	0.146	0.142	0.142	0.173	0.171	0.154

Table 2

Summary statistics

This table presents the summary statistics for the variables of our sample. The sample consists of firm-year observations from 18 exchanges over the period 1990 to 2019. All variables are defined at Table A1 in the Appendices.

Variables	# obs	Mean	Std. dev	P25	median	P75
Panel A: HFT exchanges						
NSkew	250,211	–0.145	0.621	–0.496	–0.139	0.206
Duvol	250,211	–0.082	0.319	–0.291	–0.084	0.122
Count	250,211	–0.074	0.584	0.000	0.000	0.000
DTurnover	250,211	0.003	5.355	–0.013	0.000	0.000
Size	250,211	5.142	2.253	3.573	5.052	6.699
ROA	250,211	–1.721	21.833	–0.560	3.020	7.170
BTM	250,211	0.880	0.899	0.347	0.649	1.136
Leverage	250,211	20.767	20.881	1.930	15.690	33.050
Returns	250,211	–0.002	0.002	–0.002	–0.001	0.000
|DACC|	250,211	0.120	0.281	0.014	0.039	0.097
Ln(Age)	250,211	2.527	0.779	1.978	2.623	3.138
GDP growth	250,211	2.317	2.158	1.484	2.281	3.483
Good government	250,211	1.803	0.185	1.805	1.837	1.878
Firm HHI	250,211	0.014	0.017	0.005	0.005	0.023
Industry HHI	250,211	0.063	0.021	0.050	0.059	0.065
Panel B: Non-HFT exchanges						
NSkew	68,502	–0.204	0.595	–0.552	–0.197	0.144
Duvol	68,502	–0.113	0.309	–0.318	–0.117	0.087
Count	68,502	–0.118	0.573	0.000	0.000	0.000
DTurnover	68,502	–0.007	1.045	–0.064	–0.003	0.040
Size	68,502	5.698	1.706	4.574	5.803	6.829
ROA	68,502	2.374	13.254	1.400	3.940	7.070
BTM	68,502	0.917	1.084	0.270	0.532	1.136
Leverage	68,502	23.632	19.319	7.070	21.300	35.980
Returns	68,502	–0.002	0.002	–0.002	–0.001	–0.001
|DACC|	68,502	0.077	0.148	0.017	0.042	0.085
Ln(Age)	68,502	2.349	0.712	1.858	2.455	2.916
GDP growth	68,502	6.106	3.212	3.102	6.849	8.336
Good government	68,502	1.386	0.383	1.000	1.328	1.848
Firm HHI	68,502	0.024	0.014	0.014	0.020	0.028
Industry HHI	68,502	0.078	0.016	0.066	0.076	0.087

Table 3

Correlation matrix

This table presents pairwise correlation between the variables of our sample. The sample consists of firm-year observations from 18 exchanges over the period 1990 to 2019. 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.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
NSkew (1)	1													
Duvol (2)	0.962[a]													
Count (3)	0.743[a]	0.644[a]												
DTurnover (4)	–0.000	0.001	–0.002											
Size (5)	0.126[a]	0.127[a]	0.088[a]	0.000										
ROA (6)	0.036[a]	0.037[a]	0.022[a]	0.001	0.327[a]									
BTM (7)	–0.076[a]	–0.079[a]	–0.050[a]	0.007[a]	–0.327[a]	0.051[a]								
Leverage (8)	0.016[a]	0.017[a]	0.012[a]	0.006[a]	0.048[a]	–0.005[b]	–0.057[a]							
Returns (9)	0.051[a]	0.056[a]	0.027[a]	–0.015[a]	0.440[a]	0.470[a]	0.000	0.020[a]						
$\lvert$DACC$\rvert$ (10)	–0.009[a]	–0.010[a]	–0.004[c]	0.004[c]	–0.122[a]	–0.263[a]	–0.083[a]	–0.051[a]	–0.224[a]					
Ln(Age) (11)	–0.005[b]	–0.001	–0.012[a]	–0.000	0.225[a]	0.122[a]	0.096[a]	0.056[a]	0.216[a]	–0.088[a]				
GDP growth (12)	0.012[a]	0.011[a]	0.009[a]	–0.002	0.023[a]	0.073[a]	–0.138[a]	0.058[a]	0.026[a]	–0.006[a]	–0.155[a]			
Good government (13)	–0.008[a]	–0.006[a]	–0.007[a]	0.001	–0.016[a]	–0.110[a]	0.065[a]	–0.106[a]	–0.082[a]	0.058[a]	0.085[a]	–0.662[a]		
Firm HHI (14)	–0.032[a]	–0.032[a]	–0.021[a]	–0.001	–0.130[a]	–0.057[a]	0.034[a]	0.005[b]	–0.069[a]	0.013[a]	–0.130[a]	0.162[a]	0.012[a]	
Industry HHI (15)	–0.008[a]	–0.011[a]	–0.000	0.000	–0.048[a]	–0.012[a]	–0.021[a]	0.041[a]	–0.058[a]	–0.002	–0.133[a]	0.286[a]	–0.172[a]	0.842[a]

Table 4
DiD regressions

This table presents panel regression results for a sample of 18 exchanges over the period 1990 to 2019. The sample consists of firm-year observations. The dependent variable is NSkew in models 1, and 4, Duvol in models 2 and 5, and Count in models 3, and 6, respectively. All variables are defined in the Appendix. All continuous variables are winsorized at 1% and 99% level. Robust standard errors, clustered at the firm level, are reported in the parentheses. The symbols *, **, and *** denote statistical significance at the 10%, 5% and 1% levels, respectively, using a 2-tail test.

	Full sample			[–3, +3]		
	NSkew	Duvol	Count	NSkew	Duvol	Count
Variables	(1)	(2)	(3)	(4)	(5)	(6)
HFT	0.125***	0.069***	0.085***	0.096***	0.058***	0.051***
	(0.006)	(0.003)	(0.006)	(0.010)	(0.005)	(0.010)
$NSkew_{t-1}$	0.082***	0.041***	0.056***	0.066***	0.035***	0.041***
	(0.002)	(0.001)	(0.002)	(0.005)	(0.002)	(0.005)
DTurnover	0.019*	0.001	0.026***	0.047**	0.024*	0.004
	(0.010)	(0.005)	(0.010)	(0.023)	(0.012)	(0.023)
Size	0.034***	0.016***	0.025***	0.032***	0.016***	0.023***
	(0.001)	(0.000)	(0.001)	(0.002)	(0.001)	(0.001)
ROA	0.000**	0.000***	0.000	0.000***	0.000***	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
BTM	–0.017***	–0.010***	–0.006***	–0.010***	–0.007***	0.000
	(0.002)	(0.001)	(0.001)	(0.003)	(0.002)	(0.003)
Leverage	0.000**	0.000**	0.000***	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Returns	0.018**	0.014***	–0.434	0.040***	0.027***	-0.169
	(0.731)	(0.368)	(0.669)	(1.418)	(0.731)	(1.340)
|DACC|	–0.006	–0.004	–0.002	–0.013	–0.007	-0.004
	(0.005)	(0.003)	(0.005)	(0.011)	(0.006)	(0.011)
Ln(Age)	–0.028***	–0.013***	–0.023***	–0.031***	–0.015***	-0.027***
	(0.002)	(0.001)	(0.002)	(0.004)	(0.002)	(0.004)
GDP growth	0.002***	0.001**	0.002***	0.004*	0.002*	0.004
	(0.001)	(0.000)	(0.001)	(0.002)	(0.001)	(0.002)
Good government	–0.157***	–0.071***	–0.103***	–0.287***	–0.144***	-0.223***
	(0.023)	(0.012)	(0.022)	(0.076)	(0.040)	(0.076)
Firm HHI	0.944***	0.347**	1.228***	–4.556***	–3.146***	-1.787
	(0.322)	(0.164)	(0.299)	(1.285)	(0.676)	(1.228)
Industry HHI	–0.216	–0.046	–0.355*	–0.315	–0.070	-0.208
	(0.229)	(0.118)	(0.213)	(0.815)	(0.437)	(0.778)
Constant	–0.044	–0.047**	–0.019	0.337**	0.166**	0.273*
	(0.044)	(0.023)	(0.042)	(0.141)	(0.074)	(0.140)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Exchange FE	Yes	Yes	Yes	Yes	Yes	Yes
N	307,358	307,358	307,358	65,431	65,431	65,431
Adjusted R^2	0.043	0.044	0.022	0.061	0.065	0.029

Table 5
Parallel trends assumptions

This table presents falsification tests for a sample of 18 exchanges over the period 1990 to 2019. The sample consists of firm–year observations. Panel A presents results for a pseudo–HFT period (3 years before the actual HFT period). Panel B. presents the results of a dynamic effects analysis as in Bertrand and Mullainathan (2003). The dependent variable is NSkew in model 1, Duvol in model 2, and Count in model 3, respectively. All variables are defined in the Appendix. All continuous variables are winsorized at 1% and 99% level. Robust standard errors, clustered at the firm level, are reported in the parentheses. The symbols *, **, and *** denote statistical significance at the 10%, 5% and 1% levels, respectively, using a 2–tail test.

	NSkew	Duvol	Crash dummy
Panel A: Pseudo HFT	(1)	(2)	(3)
HFT	0.001	–0.003	0.008
	(0.008)	(0.004)	(0.009)
Controls	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Exchange FE	Yes	Yes	Yes
N	83,914	83,914	83,914
Adjusted R^2	0.057	0.058	0.029
Panel B: Dynamic effects of HFT			
$Year^{-2}$	0.012	0.006	0.003
	(0.008)	(0.004)	(0.009)
$Year^{-1}$	0.006	0.007	–0.000
	(0.008)	(0.004)	(0.009)
$Year^{0}$	–0.015*	–0.009**	–0.016*
	(0.009)	(0.005)	(0.010)
$Year^{1}$	0.075***	0.045***	0.042***
	(0.009)	(0.005)	(0.010)
$Year^{2+}$	0.148***	0.082***	0.098***
	(0.008)	(0.004)	(0.009)
Controls	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Exchange FE	Yes	Yes	Yes
N	318,713	318,713	318,713
Adjusted R^2	0.044	0.046	0.024

Table 6

HFT, liquidity and price informativeness

This table presents the results of a two–stage regression analysis tests for a sample of 18 exchanges over the period 1990 to 2019. The sample consists of firm–year observations. In the first stage regressions (models 1, 5, amd 9), the dependent variable is either Liquidity, Noise trading or Equity mispricing. In the second stage regressions (models 2, 3, 4, 6, 7, 8, 10, 11, and 12) the dependent variable is either NSkew, Duvol or Count. All variables are defined in the Appendix. All continuous variables are winsorized at 1% and 99% level. Robust standard errors, clustered at the firm level, are reported in the parentheses. The symbols *, **, and *** denote statistical significance at the 10%, 5% and 1% levels, respectively, using a 2–tail test.

	Liquidity	NSkew	Duvol	Count	Noise trading	NSkew	Duvol	Count	Equity mispricing	NSkew	Duvol	Count
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
HFT	0.006***				0.013**				0.002**			
	(0.001)				(0.005)				(0.001)			
$\widehat{\text{Liquidity}}$		18.996***	10.662***	12.469***								
		(1.023)	(0.530)	(0.977)								
$\widehat{\text{Noise trading}}$						10.710***	5.609***	8.244***				
						(0.666)	(0.351)	(0.645)				
$\widehat{\text{Equity mispricing}}$										7.013**	4.158***	5.083*
										(2.798)	(1.500)	(2.852)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Exchange FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	283,713	283,713	283,691	283,713	138,314	138,314	138,314	138,314	244,805	244,805	244,805	244,805
Adjusted R^2	0.468	0.043	0.044	0.022	0.001	0.050	0.052	0.026	0.486	0.050	0.051	0.025

Table 7

Agency channels

This table presents the results of a two–stage regression analysis tests for a sample of 18 exchanges over the period 1990 to 2019. The sample consists of firm–year observations. In the first stage regressions (models 1 and 5), the dependent variable is either Opacity or Overinvestment. In the second stage regressions (models 2, 3, 4, 6, 7, and 8) the dependent variable is either NSkew, Duvol or Count. All variables are defined in the Appendix. All continuous variables are winsorized at 1% and 99% level. Robust standard errors, clustered at the firm level, are reported in the parentheses. The symbols *, **, and *** denote statistical significance at the 10%, 5% and 1% levels, respectively, using a 2–tail test.

	Opacity	NSkew	Duvol	Count	Overinvestment	NSkew	Duvol	Count
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
HFT	0.020***				0.005***			
	(0.006)				(0.001)			
$\widehat{\text{Opacity}}$		6.376***	3.534***	4.343***				
		(0.304)	(0.157)	(0.289)				
$\widehat{\text{Overinvestment}}$						13.959***	7.846***	8.934***
						(1.295)	(0.724)	(0.945)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Exchange FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	307,358	307,358	307,358	307,358	336,888	336,888	336,888	336,888
Adjusted R^2	0.247	0.042	0.044	0.022	0.057	0.040	0.042	0.021

Table 8

Moderation effects

This table presents our baseline regression results conditioning upon the countries' rank in derivatives market use or the analysts coverage. The sample consists of firm–year observations. The dependent variable is Nskew in models 1, and 4, Duvol in models 2, and 5, and Count in models 3, and 6, respectively. All variables are defined in the Appendix. All continuous variables are winsorized at 1% and 99% level. Robust standard errors, clustered at the firm level, are reported in the parentheses. The symbols *, **, and *** denote statistical significance at the 10%, 5% and 1% levels, respectively, using a 2–tail test.

	NSkew	Duvol	Count	NSkew	Duvol	Count
Variables	(1)	(2)	(3)	(4)	(5)	(6)
HFT	0.144***	0.082***	0.087***	0.092***	0.052***	0.062***
	(0.014)	(0.007)	(0.013)	(0.009)	(0.005)	(0.009)
Derivatives market rank	–0.000	–0.000	–0.001			
	(0.001)	(0.001)	(0.001)			
HFT × Derivatives market rank	–0.004***	–0.003***	–0.002*			
	(0.001)	(0.001)	(0.001)			
Analyst coverage				0.000	0.000*	0.000
				(0.000)	(0.000)	(0.000)
HFT × Analyst coverage				–0.002***	–0.001***	–0.001***
				(0.000)	(0.000)	(0.000)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Exchange FE	Yes	Yes	Yes	Yes	Yes	Yes
N	273,593	273,593	273,593	167,730	167,730	167,730
Adjusted R^2	0.042	0.045	0.021	0.042	0.046	0.046

Table 9

HFT, culture and religion

This table presents the impact of the countries' collectivist culture or religiosity on stock price crash risk. Models 1 to 6 examine the impact of collectivism. Models 7 to 12 examine the impact of religion. Nskew is the dependent variable in models 1, 2, 7, and 8. Duvol is the dependent variable is models 3, 4, 9, and 10. Count is the dependent variable in models 5, 6, 11, and 12. All variables are defined in the Appendix. All continuous variables are winsorized at 1% and 99% level. Robust standard errors, clustered at the firm level, are reported in the parentheses. The symbols *, **, and *** denote statistical significance at the 10%, 5% and 1% levels, respectively, using a 2–tail test.

	Collectivism						Religion					
	NSkew	NSkew	Duvol	Duvol	Count	Count	NSkew	NSkew	Duvol	Duvol	Count	Count
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
HFT	0.131***	0.100***	0.068***	0.050***	0.093***	0.079***	0.145***	2.892***	0.075***	1.566***	0.105***	1.375***
	(0.005)	(0.008)	(0.002)	(0.004)	(0.004)	(0.008)	(0.004)	(0.434)	(0.002)	(0.221)	(0.004)	(0.397)
Collectivism	–0.040***	–0.009	–0.020***	–0.001	–0.041***	–0.026***						
	(0.008)	(0.010)	(0.004)	(0.005)	(0.007)	(0.009)						
HFT × Collectivism		–0.059***		–0.035***		–0.027**						
		(0.013)		(0.006)		(0.012)						
Religion							–0.370	1.591***	–0.273**	0.791***	–0.625***	0.281
							(0.249)	(0.388)	(0.125)	(0.198)	(0.223)	(0.349)
HFT × Religion								–2.756***		–1.496***		–1.274***
								(0.436)		(0.222)		(0.398)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Exchange FE	No	No	No	No	No	No	No	No	No	No	No	No
N	307,358	307,358	307,358	307,358	307,358	307,358	307,358	307,358	307,358	307,358	307,358	307,358
Adjusted R^2	0.040	0.040	0.041	0.042	0.020	0.020	0.040	0.040	0.041	0.041	0.020	0.020

Table 10

HFT, stock price synchronicity and its components

This table presents regression results of stock price synchronicity and its components. The dependent variable is stock price synchronicity in model 1, systematic risk in model 2, and idiosyncratic volatility in model 3. All variables are defined in the Appendix. All continuous variables are winsorized at 1% and 99% level. Robust standard errors, clustered at the firm level, are reported in the parentheses. The symbols *, **, and *** denote statistical significance at the 10%, 5% and 1% levels, respectively, using a 2–tail test.

	Synchronicity	Systematic risk	Idiosyncratic volatility
Variables	(1)	(2)	(3)
HFT	0.256***	0.009***	–0.016***
	(0.008)	(0.001)	(0.001)
Controls	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Exchange FE	Yes	Yes	Yes
N	307,336	307,336	307,336
Adjusted R^2	0.407	0.456	0.646

Table 11

Effect of collocation

This table presents panel regression results for a sample of 18 exchanges over the period 1990 to 2019. The sample consists of firm-year observations. The dependent variable is NSkew in models 1, and 4, Duvol in models 2 and 5, and Count in models 3, and 6, respectively. All variables are defined in the Appendix. All continuous variables are winsorized at 1% and 99% level. Robust standard errors, clustered at the firm level, are reported in the parentheses. The symbols *, **, and *** denote statistical significance at the 10%, 5% and 1% levels, respectively, using a 2-tail test

	NSkew	Duvol	Count	Nskew	Duvol	Count
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Collocation	0.074***	0.038***	0.059***			
	(0.005)	(0.003)	(0.005)			
$Year^{-2}$				0.040***	0.023***	0.022***
				(0.007)	(0.003)	(0.006)
$Year^{-1}$				0.076***	0.045***	0.041***
				(0.007)	(0.003)	(0.007)
$Year^{0}$				0.063***	0.034***	0.048***
				(0.007)	(0.004)	(0.007)
$Year^{1}$				0.100***	0.053***	0.075***
				(0.007)	(0.004)	(0.007)
$Year^{2+}$				0.116***	0.063***	0.081***
				(0.006)	(0.003)	(0.006)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Exchange FE	Yes	Yes	Yes	Yes	Yes	Yes
N	305,188	305,188	305,188	318,713	318,713	318,713
Adjusted R^2	0.041	0.043	0.021	0.043	0.044	0.022

Table 12

Asymmetric effect: Crashes versus jumps

This table presents panel regression results for a sample of 18 exchanges over the period 1990 to 2019. The sample consists of firm-year observations. The dependent variable is Crash dummy in model 1, Crash count in model 2, Jump dummy in model 3, and Jump count in model 4. All variables are defined in the Appendix. All continuous variables are winsorized at 1% and 99% level. Robust standard errors, clustered at the firm level, are reported in the parentheses. The symbols *, **, and *** denote statistical significance at the 10%, 5% and 1% levels, respectively, using a 2-tail test.

	Crash dummy	Crash counts	Jump dummy	Jump counts
Variables	(1)	(2)	(3)	(4)
HFT	0.702***	0.079***	–0.008	–0.006
	(0.034)	(0.004)	(0.027)	(0.004)
Controls	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Exchange FE	Yes	Yes	Yes	Yes
N	307,358	307,358	307,358	307,358
Pseudo R^2 / Adjusted R^2	0.029	0.022	0.034	0.035

Table 13
Further robustness tests

This table presents our robustness tests. Models 1-3 repeat our baseline regression with the inclusion of fixed effects and 2-way clustered standard errors (firm and year clustering). Model 4-6 repeat our baseline models with exchange clustered standard errors. Models 7-9 repeat our baseline models by excluding the U.S. from the sample. Models 10-12 repeat our baseline models without the financial crisis years (2007-2009). All variables are defined in the Appendix. All continuous variables are winsorized at 1% and 99% level. Robust standard errors, clustered at the firm level, are reported in the parentheses. The symbols *, **, and *** denote statistical significance at the 10%, 5% and 1% levels, respectively, using a 2-tail test.

	Firm FE & 2-way clustering			Exchange clustering			Excluding U.S.			Excluding GFC		
	NSkew	Duvol	Count	NSkew	Duvol	Count	NSkew	Duvol	Count	NSkew	Duvol	Count
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
HFT	0.134***	0.074***	0.090***	0.125***	0.069***	0.085***	0.137***	0.076***	0.095***	0.120***	0.066***	0.085***
	(0.024)	(0.014)	(0.014)	(0.019)	(0.010)	(0.013)	(0.007)	(0.003)	(0.006)	(0.006)	(0.003)	(0.006)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	No	No	No	No	No	No	No	No	No
Industry FE	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Exchange FE	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	304,688	304,688	304,688	307,358	307,358	307,358	222,127	222,127	222,127	262,976	262,976	262,976
Adjusted R^2	0.101	0.097	0.058	0.042	0.044	0.022	0.039	0.040	0.020	0.042	0.043	0.022

Appendix

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).
Count	The difference between the number of crash and jump weeks in a year. Crash (jump) week is a week where its firm-specific return is less (more) than 3.09 standard deviation below (above) the mean firm-specific return.
Synchronicity	The logarithmic transformation of the R^2 obtained from eq. (1) for every firm.
Systematic risk	The annualized square root of difference between weekly return variance and variance from residuals of eq. (1).
Idiosyncratic volatility	The annualized squared root of the residuals obtained from eq. (1) for every firm.
HFT	A dummy variable which equals 1 for exchanges with HFT presence, and 0 otherwise.
Collocation	A dummy variable which equals 1 for collocated exchanges, and 0 otherwise.
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.
|DACC|	The absolute value of discretionary accruals measured as the residuals of the performance-controlled accruals model of Tucker and Zarowin (2006).
Ln(Age)	The natural logarithm of the firm's age plus one. Age is defined as the number of years since the IPO year.
GDP growth	The annual average rate of change of GDP.
Good government	The sum of the percentile ranks of two indices from the World Bank's Worldwide Governance Indicators (WGI) Project: (i) government effectiveness, and (ii) control of corruption.
Firm HHI	Firm concentration ratio, as measured by the firm Herfindahl index on an annual basis.
Industry HHI	Industry concentration ratio, as measured by the industry Herfindahl index on an annual basis.
Opacity	The 3-year moving sum of absolute discretionary accruals as in Hutton et al. (2009)
Overinvestment	The amount of unexpected investment in assets that have positive values of unexpected investment, and 0 otherwise, as in Balachandran et al. (2020). Unexpected investment is measured using the residuals of the Schrand and Zechman (2012) model.
Liquidity	The firm's effective bid-ask spread calculated as twice the absolute difference between the closing price and the midpoint of the bid-ask quote, divided by the midpoint of the bid-ask quote.
Noise trading	The informed trading measure of Llorente et al. (2002), multiplied by -1. The informed trading measure is calculated as the coefficient of the interaction term (θ) in eq. (4)
Equity mispricing	The price informativeness measure of Bai et al. (2016), multiplied by -1. This measure examines the ability of current market prices to forecast future earnings. To compute this 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).
Derivatives market rank	Ranking of the size of the derivatives market relative to the other countries in the sample as in Bartram (2019).
Analyst coverage	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

PSM-Matched DiD regressions

This table presents our baseline DiD regressions for the PSM-matched sample. Matching is done using the nearest neighbor approach (one-to-one matching) with replacement). The sample consists of firm-year observations. The dependent variable is NSkew in models 1, and 4, Duvol in models 2 and 5, and Count in models 3, and 6, respectively. All variables are defined in the Appendix. All continuous variables are winsorized at 1% and 99% level. Robust standard errors, clustered at the firm level, are reported in the parentheses. The symbols *, **, and *** denote statistical significance at the 10%, 5% and 1% levels, respectively, using a 2-tail test.

	Full sample			[–3, +3]		
	NSkew	Duvol	Count	NSkew	Duvol	Count
Variables	(1)	(2)	(3)	(4)	(5)	(6)
HFT	0.101***	0.056***	0.071***	0.105***	0.055***	0.077***
	(0.007)	(0.004)	(0.007)	(0.015)	(0.008)	(0.015)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Exchange FE	Yes	Yes	Yes	Yes	Yes	Yes
N	258,162	258,162	258,162	40,160	40,160	40,160
Adjusted R^2	0.044	0.047	0.023	0.054	0.067	0.025

Table A3

Breakdown by size

This table presents the baseline regressions of Table 4 when we breakdown the sample according to the firms' median size value. The sample consists of firm-year observations. The dependent variable is NSkew in models 1, and 2, Duvol in models 3 and 4, and Count in models 5, and 6, respectively. All variables are defined in the Appendix. All continuous variables are winsorized at 1% and 99% level. Robust standard errors, clustered at the firm level, are reported in the parentheses. The symbols *, **, and *** denote statistical significance at the 10%, 5% and 1% levels, respectively, using a 2-tail test.

	NSkew		Duvol		Count	
	High	Low	High	Low	High	Low
Variables	(1)	(2)	(3)	(4)	(5)	(6)
HFT	0.132***	0.106***	0.068***	0.062***	0.099***	0.067***
	(0.010)	(0.008)	(0.005)	(0.004)	(0.009)	(0.007)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Exchange FE	Yes	Yes	Yes	Yes	Yes	Yes
N	153,676	153,682	153,667	153,669	153,676	153,682
Adjusted R^2	0.035	0.036	0.037	0.039	0.016	0.018

Table A4
Marginal effects

This table presents the marginal effects of the regressions presented in Table 8. The first column presents the 8 values of either *Derivatives market rank* or *Analyst coverage* variables. Columns 1, 2, and 3 report the marginal effects when the moderating variable is *Derivatives market rank*. Columns 4, 5, and 6 report the marginal effects when the moderating variable is *Analyst coverage*. 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.

Derivatives marker rank				*Analyst coverage*			
c	NSkew	Duvol	Count	c	NSkew	Duvol	Count
1	0.139***	0.079***	0.085***	1.00	0.090***	0.051***	0.061***
3	0.130***	0.074***	0.081***	3.71	0.086***	0.049***	0.058***
5	0.121***	0.069***	0.077***	6.43	0.0812***	0.046***	0.054***
7	0.113***	0.064***	0.073***	9.14	0.078***	0.044***	0.051***
9	0.104***	0.058***	0.069***	11.86	0.074***	0.042***	0.048***
11	0.095***	0.053***	0.065***	14.57	0.069***	0.040***	0.044***
13	0.086***	0.048***	0.061***	17.29	0.065***	0.038***	0.041***
15	0.077***	0.044***	0.057***	20.00	0.061***	0.036***	0.038***