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Do retail traders destabilize financial markets? An investigation surrounding the COVID-19 pandemic

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Existing research suggests that retail trading is associated with volatility in financial markets. To extend the literature, we study the dynamic effects of retail trading on volatility during the COVID-19 pandemic. Using marketable retail trades identified from the Boehmer et al. (2021) algorithm and novel empirical methods discussed in Jordà (2005), we document a negative, persistent impact of retail trading on the stability of stock prices that is particularly stronger during the pandemic than during the pre-pandemic period. These results highlight how periods of crises – like the pandemic – affect the destabilizing influence of retail trading. To provide additional evidence, we replicate our empirical exercise during the 2008-09 financial crisis. Consistent with the COVID-19 period, we again find that retail trading leads to more volatility during the financial crisis vis-à-vis the pre-crisis period. These results again support the idea that periods of crises strengthen the link between retail trading and volatility.

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1. Introduction

“The noise that noise traders put into stock prices will be cumulative, in the same sense that a drunk tends to wander farther and farther from his starting point.” Black (1986)

The stability of financial markets plays an important role in the development of economic activity. When markets are more stable, capital can be allocated more efficiently, which can result in positive welfare effects for the entire economy (see Pagano, 1993, for example). A growing body of research examines how retail trading affects the stability of financial markets. For example, Foucault et al. (2011) utilize a negative exogenous shock to retail trading activity in France and observe a significant decrease in the volatility of affected stocks. Likewise, Brandt et al. (2010) show that much of the increase in idiosyncratic volatility during the 1990s can be partly attributed to an increase in retail trading.

The link between retail trading and volatility might best be explained by theory in Abreu and Brunnermeier (2002). This theory suggests that during periods when asset prices begin to deviate away from fundamental values, the ability of an informed trader to correct market prices is conditional on the willingness of other informed traders to step in and help correct the potential mispricing. Abreu and Brunnermeier (2002) show that when informed traders are not synchronized about when to step in and correct market prices, which they denote as periods of synchronization risk, volatility in financial markets can increase. Brunnermeier and Nagel (2004) present evidence that indicates that hedge funds, which are typically thought of as informed traders, did not attempt to correct the severe mispricing during the dot-com bubble. Additionally, theory in Stein (1987) points out that trading by uninformed investors may exacerbate how this type of synchronization risk can lead to destabilized stock prices. To the extent that retail traders act as uninformed investors (Barber and Odean, 2000; 2008; Barber et al., 2009), an increase in the intensity of retail trading could lead to higher levels of market volatility.

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In this study, we seek to contribute to the existing literature on retail trading and volatility by examining how periods of crises, like the COVID-19 pandemic, contribute to this type of trading-induced volatility. In the framework of Abreu and Brunnermeier (2002), periods of crises might exacerbate synchronization risk as informed traders may become more sensitive to volatility and may be more likely to step away from the market. Additionally, our tests are motivated by two empirical observations. First, Albulescu (2020) and Mazur et al. (2020) document unusually high volatility during the pandemic. Our analysis, which examines whether the link between retail trading and volatility is stronger than usual during the COVID-19 pandemic, can help identify a potential mechanism that explains the unusually high volatility during the pandemic. Second, Chia and Zhong (2020) show that in a number of different countries, trading activity markedly increased during the pandemic. This is particularly true in countries with more favorable attitudes towards gambling, suggesting that individuals may have substituted gambling activity for stock trading when many of the existing gaming institutions were locked down during the pandemic. The results in Chia and Zhong (2020) indicate a surge in retail trading during this time period. In the context of our study, if pandemic lockdowns indeed influenced new retail trading activity, then examining the effect of this new activity on the stability of markets becomes important.

To conduct our tests, we identify marketable retail trades for each stock in our sample using the Boehmer et al. (2021) algorithm. We measure volatility in two ways. First, we follow Alizadeh et al. (2002) and calculate range-based volatility, which approximates stochastic volatility. Second, we model volatility using a GARCH(1,1) process. It is important to note that retail traders may be attracted to volatile stocks (Kumar, 2009), such that a causal relation between retail trading and volatility may be bidirectional. Therefore, we utilize empirical methods that allow us to isolate the causal effect of retail trading on volatility while remaining agnostic to the possibility of reverse causality. In particular, we follow the novel Local Projection (LP) technique proposed in Jordà (2005) and estimate impulse response functions (IRFs) that examine the response of volatility to exogenous shocks in the level of retail trading activity. The use of LPs in our setting is important given that a more traditional vector autoregressive (VAR) approach (i) places greater restrictions on the data-generating process, (ii) does not allow us to add a number of control variables without encountering the ‘curse of dimensionality’, and (iii), perhaps most importantly, a traditional VAR does not enable us to test for any potential non-linearities or interactions in the data. Given that we would like to compare the IRFs from two different time periods (pandemic vs. pre-pandemic), the LP technique allows to isolate the two time periods within one unified modelling framework.

Results from the LP setup show that, in response to a one-standard deviation shock to retail trading, both range and GARCH(1,1) volatility increase in the 15 days following the shock. When comparing the effect of the pandemic to more normal time periods, we find that the shock to retail trading increases volatility significantly more during the COVID period than an identical shock during the pre-COVID period. Stated differently, the effect of retail trading on market volatility is significantly amplified during periods of market stress, like that of COVID-19. In economic terms, the causal effect of retail trading on range-based volatility is around 30% greater during the pandemic than during the pre-pandemic period. Similar results are found when examining the effect of retail trading on GARCH(1,1) volatility. In unreported tests, we replicate our analysis using a different proxy for retail trading. In particular, we examine shocks to the growth in users of the popular trading application Robinhood and find qualitatively similar results to those presented below. Overall, our work supports the notion that retail trading activity causes greater volatility generally and that the effect is exacerbated during the pandemic. In the later parts of the paper, we take our analysis away from COVID-19 and attempt to more generally test whether the response of volatility is still greater during a different type of market stress. To do so, we study the time period surrounding the 2008-09 global financial crisis (GFC) and repeat our empirical exercise. Consistent with our comparison of the pandemic and pre-pandemic periods, we find that retail trading shocks had a stronger effect on market volatility during the financial crisis period vis-à-vis the pre-crisis period. These results again highlight the role that market stress plays when identifying the effect of retail trading on market volatility.

Our results contribute to the existing literature in several ways. First, the increase in volatility during the pandemic, which has been documented in Albulescu (2020) and Mazur et al. (2020), can be partially attributed to retail trading. Second, the increase in retail trading during the pandemic – as shown in Chia and Zhong (2020) – seems to have an unusually strong destabilizing effect on prices. More generally, our findings also add to the recent and growing body of literature on retail trading. For instance, Pagano et al. (2021) analyzes the negative association between retail trading and financial market quality. Eaton et al. (2022) use outages in Robinhood as a negative exogenous shock to retail participation and show that outages lead to a reduction in volatility and an improvement in liquidity of affected stocks. Barber et al. (2021) find that retail traders engage in attention-induced trading, which can lead to buy-side herding events. Consistent with these studies, our findings show that the effect of retail trading on market volatility is stronger during periods of crises, like the COVID-19 pandemic and the financial crisis.

In a different stream of research, Friedman and Zeng (2021) show that retail trading is associated with stronger reactions to earnings announcements, which consistent with the idea that retail investors act as uninformed noise traders. In the context of Abreu and Brunnermeier (2002), noise trading – particularly in times of crises – may crowd out informed trading that usually acts to correct any mispricing and stabilize prices more generally. The findings in our study support this notion and thus contribute to a much broader literature that examine the process of destabilization in asset prices (Flood and Hodrick, 1990; Ofek and Richardson, 2003; Brunnermeier and Nagel, 2004; Malpezzii and Wachter, 2005; Schnabl and Hoffmann, 2008; Wang and Wen, 2012; Joyeux and Milunovich, 2015, among many others).

The rest of this paper follows. Section 2 describes the data used throughout the analysis. In this section, we also discuss the empirical methods used in our tests. Sections 3 and 4 present the bulk of our empirical results. Section 5 presents some robustness tests. Section 6 offers concluding remarks.

2. Data and estimation methodology

2.1. Explaining the data

Our data come from the following sources: the NYSE Trade and Quote (TAQ) database, the Center of Research in Security
Prices (CRSP), the Oxford COVID-19 Government Response Tracker (OxCGRRT), and the St. Louis Federal Reserve’s FRED database. Specifically, we use the TAQ database to identify marketable buy and sell retail trades during normal market hours following the Boehmer et al. (2021) (BJJZ henceforth). BJJZ posit that retail trades typically take place off-exchange and TAQ reports these off-exchange retail trades marked with an exchange code “D”. Furthermore, these retail orders receive small price improvements (fraction of a penny) relative to the National Best Bid or Offer (NBBO). Accordingly, BJJZ identify a trade to be retail buys (sells) if the fractional component of trade prices range between 0.6 and 1 (0 and 0.4) cents. Using the BJJZ algorithm, we construct two measures of retail trading. We define our (first) absolute retail trading measure as \( \ln(1 + mbtrd + mrstrd) \) where \( mbtrd \) and \( mrstrd \) are the marketable retail buy and sell trades, respectively, identified using the BJJZ algorithm. The sum of retail buys and sells allows us to capture complete activity regardless of whether it was a buy or sell trade. To help normalize the distribution retail trades, we take log of this sum and add one to avoid undefined measures when there is no retail activity on a given day. Our (second) relative measure is constructed as \( \ln(\frac{mbtrd + mrstrd}{vol}) \) where \( vol \) is the trading volume for the stock for a particular day.

From CRSP, we gather daily closing prices, trading volumes, closing bid prices, closing ask prices, the highest and lowest prices during the day, shares outstanding, and the exchange listing for each stock. A stream of recent literature suggests that COVID-19 adversely impacted equity markets (e.g., Baker et al., 2020; John and Li, 2021). In particular, the pandemic and its associated government responses generated uncertainty and accordingly, some of the COVID-related variables contributed to market volatility (Osall, 2020; Baig et al., 2020; Baker et al., 2020; John and Li, 2021). In fact, Baker et al. (2020) state that, before the COVID-19 pandemic, no infectious disease outbreak made a sizable contribution to U.S. stock market volatility — pp 748. Therefore, to proxy and control for the effect of uncertainty surrounding the pandemic, we obtain data on confirmed COVID-19 cases, deaths, and the stringency index (SI), which measures the magnitude of non-pharmaceutical government policy interventions from OxCGRRT. We also use Baker et al. (2016) news-based, equity-related measure of economic policy uncertainty (EPU). Moreover, we also obtain the data on daily market-wide returns from Professor Ken French’s website.

We use the following two measures of market volatility: range and GARCH(1,1) volatility. Range-based volatility is defined as \( \ln(\text{high}_t) - \ln(\text{low}_t) \) where \( \text{high} \) and \( \text{low} \) are the highest and the lowest intraday quoted prices during each day (Alizadeh et al., 2002). Following a broad research, our second measure is the average conditional variance obtained from a GARCH(1,1) model. Although both the range-based and GARCH(1,1) measures are intended to proxy for volatility, the two measures are calculated differently and have some unique characteristics (see, for example, Alizadeh et al., 2002). For instance, the GARCH(1,1) model relies on closing prices and ignores the movement of prices inside these reference periods while the range-based measure utilizes the intraday highest and lowest prices in order to capture stochastic volatility resulting in a more dynamic daily measure. The difference between these two measures is especially magnified during more turbulent trading days. Moreover, the range volatility measure is robust to potential contamination by microstructure noise. Nevertheless, in our sample, these two measures have a correlation of about 48% providing us some reassurance that both of these measures reasonably proxy for the same underlying volatility despite their differences. Additionally, we include a number of other stock-level characteristics as control variables, including closing bid-ask spreads, turnover (daily trading volume scaled by shares outstanding), and market capitalization (closing price times the shares outstanding). As discussed above, the primary objective of this study is to explore the role of retail trading on market volatility during periods of crises, therefore, our main time period extends from 05/02/2018 to 08/13/2020 which spans both pre- and COVID-crisis periods, while our secondary analysis spans from 01/01/2007 to 05/31/2009 and covers the period surrounding the global financial crisis (GFC).

Table 1 presents statistics that summarize some of the variables explained above. The daily value of absolute and relative retail trading measures for an average stock is 4.48 and -8.02 respectively. The correlation between our two measures is 78.95%. The mean value for range and GARCH(1,1) volatility are 0.05 and 0.04 respectively, with a correlation of 0.48, which is statistically significant (at a 1% level). The EPU and SI during the period have an average of 157.06 and 13.92, respectively. The average stock in our sample has a mean closing price of $89.89, a market capitalization is $8.29 billion, and a bid-ask spread is 0.01. In total, there are 4024 unique stocks used in the analysis.

2.2. Motivating the question

Figure 1 plots the value-weighted series for the daily absolute and relative retail trading measures as well as the range and GARCH(1,1) volatility measures. Some results are noteworthy. First, movements in both our retail trading measures are generally correlated (correlation coefficient of 0.73), implying that both measures capture similar underlying dynamics of the data generating process. Consistent with our expectations, we also observe similar co-movement between range and GARCH(1,1) volatility. More importantly, both the volatility and retail trading measures seem to increase as soon as the pandemic begins. This observation is consistent with some of the motivation for our research question, which seeks to answer whether retail trading activity contributes more to volatility during the crisis period - relative to the non-crisis period. Figure 1 therefore verifies the variation that is required to identify the state-dependent (COVID vs. pre-COVID) effects of retail activity.

Since we study the dynamic impact of retail activity on volatility, this section attempts to motivate our empirical methodology by using a relatively conventional method in Finance: Vector Auto-regressions (see, for instance, Kumar and Lee, 2006; NÉs et al., 2011; Rapach et al., 2016). To set the stage, we estimate a bivariate Vector Autoregressive (VAR) model consisting of our value-weighted retail trading and volatility measures. Since this is merely a motivation for our main empirical method, we use our absolute measure and estimate two VARs, one for range and one for GARCH(1,1) volatility. To capture the effects of the pandemic and in order to accommodate for any time-related changes in our variables, we also add the first principal component of the growth in confirmed COVID-19 cases and deaths as well as a linear time trend as exogenous variables. In the literature, this formulation is sometimes referred to as a VAR-X model. We estimate the model, which is described in Eq. (1), using OLS.

\[
Y_t = \eta + \Gamma Z_t + t + A_1Y_{t-1} + \ldots + A_tY_{t-T} + u_t; \quad t = 1, 2, \ldots, T
\]

(1)

Here, \( \eta \) is the matrix of constants, \( Z_t \) is the first principal component of confirmed COVID-19 cases and deaths, and \( t \) is the time

3 We obtain the conditional variance by estimating the GARCH (1,1) model for every stock and take the square root of this conditional variance series and use it as our conditional volatility measure.

4 We remove all stocks that are not defined as common shares as well as stocks not listed on NYSE, NASDAQ or NYSE American (formerly AMEX).
Table 1
Summary Statistics.

|                          | Observations | Mean   | Standard Dev | 25th  | 75th  |
|--------------------------|--------------|--------|--------------|-------|-------|
| Absolute Measure         | 1,911,985    | 4.48   | 1.86         | 3.26  | 5.75  |
| Relative Measure         | 1,911,985    | -8.02  | 0.92         | -8.57 | -7.50 |
| MarkCap                  | 1,911,985    | 8.29   | 41.73        | 0.16  | 3.42  |
| Spread                   | 1,911,985    | 0.01   | 0.01         | 0.00  | 0.01  |
| Turnover                 | 1,911,985    | 0.02   | 1.49         | 0.00  | 0.01  |
| Price                    | 1,911,985    | 89.89  | 3,717.91     | 6.40  | 49.25 |
| Range Volatility         | 1,911,985    | 0.05   | 0.05         | 0.02  | 0.06  |
| GARCH(1,1) Volatility    | 1,911,985    | 0.04   | 0.05         | 0.02  | 0.05  |
| EPU                      | 1,911,985    | 157.06 | 139.47       | 72.49 | 166.69|
| SI                       | 1,911,985    | 13.92  | 27.51        | 0.00  | 0.00  |
| Stock Mkt Return         | 1,911,985    | 0.07   | 1.35         | -0.40 | 0.69  |
| VIX                      | 1,911,985    | 20.13  | 11.07        | 13.23 | 22.48 |
| Cases                    | 1,911,985    | 381,401.73 | 1,020,146.05 | 0.00  | 5.00  |
| Deaths                   | 1,911,985    | 16,875.22 | 41,657.07    | 0.00  | 0.00  |

Absolute retail trading measure is defined as \(\ln(1 + \text{mrtrd} + \text{mrsrd})\) where \(\text{mrtrd}\) and \(\text{mrsrd}\) are the marketable retail buy and sell trades (respectively) identified using BJZZ algorithm. Relative retail trading measure is similar to its absolute counterpart except that it is normalized by total trading activity (see text for further details). Market Capitalization (MarkCap) is defined as price times share outstanding in billions. Spread is the ask – bid divided by the midpoint of ask and bid. Turnover is the trading volume divided by shares outstanding. Price is the closing price for the day. Range Vol is defined as the log-difference between the highest and lowest price registered in a day (i.e. \(\ln(\text{high}) - \ln(\text{low})\)). GARCH(1,1) volatility is the square root of the stock-by-stock conditional variance obtained from a GARCH(1,1) model. EPU is the news-based Equity-related economic uncertainty index from Baker et al. (2016). SI is the Stringency Index from OXCGRT database, Mkt Return is the daily stock market return obtained from Professor Ken French’s website, VIX is the CBOE VIX index obtained from the FRED database, and Cases and Deaths are the confirmed COVID-19 cases and deaths. The number of unique firms is 4024.

To suppress the impact of outliers on our results, we winsorize all variables at 1% and 95% levels. The table reports means, standard deviations, and values at the 25th and 75th percentiles.

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Fig. 1. Time-Series plots for Volatility and Retail Trading Measures. Note: The figure plots the value-weighted series for absolute and relative retail measures (constructed from TAQ data), range volatility, and GARCH(1,1) volatility. To ease interpretation, all variables are standardized. Shaded area represents the continuing pandemic, which we take to start from 21st January, 2020. i.e. the date when the first COVID-19 case was registered.

trend. We take \(T = 4\) following different lag selection criteria, though a higher or a lower lag order has no bearing on our results.\(^5\) When estimated, the reduced-form shocks from the model would be correlated. Therefore, we express \(u_t = BE_t\), where \(E_t\) represent the structural shocks in the economy such that \(E_t[E_{t+1}] = I\) and \(BB = \Sigma\). To obtain \(B\), we take the popular Cholesky Decomposition of \(\Sigma\) such that the retail trading measure is ordered first. Imposing this structure on the contemporaneous relation between the two structural shocks implies that the retail trading shock can affect volatility within the same day. However, retail traders respond to market volatility shocks only after a day (i.e., retail trading has inertia). We choose this particular ordering of variables because we are primarily interested in the effect of retail trading on volatility. Therefore, from a modelling perspective, it makes less sense to restrict (though only for a period) the relation that we wish to explore.\(^6\) Furthermore, it is only a restriction on the contemporaneous and not the subsequent response, suggesting that if volatility does impact retail trading in the data-generating process, the impulse response can register that effect after a lag. All in all, our results are robust to modifications like reversed ordering, increasing or decreasing lag length, or adding other financial controls.

\(^5\) With a maximum lags length of 10, Akaike information criteria (AIC) favored 7 lags while Schwartz information criterion selected 4 to be the optimal lag length. Since ours is a time-series with small number of observations, we side with a more parsimonious lag order of 4.

\(^6\) However, at the same time, one might argue that retail traders may be drawn towards more volatile stocks and, therefore, a zero contemporaneous restriction on the directional relation from volatility to retail trading might be implausible. Results from Granger-causality tests, however, reveal that volatility (proxied by GARCH(1,1) model) does not Granger-cause retail trading on 5% level, implying that our identification strategy holds some merit, despite any potential limitations of Granger-type tests.
like the logged S&P500 index in the model. Since we are interested in a crisis vs. pre-crisis comparison, we follow much of the VAR literature (see, for instance, Ilzetzki and Vegh, 2008; Ilzetzki et al., 2013) and split our sample by COVID-19 and pre-COVID periods and run two separate VARs to test whether the response is different across crisis vs. non-crisis periods. We take the COVID period to be days after January 21st, 2020, which is the day when the first COVID case was registered in the US. This simple exercise will not only allow us to see the merits of our hypothesis but it will also, as the reader will note, let us introduce our empirical strategy that improves upon VAR-type specifications.

2.2.1. Results

Figure 2 plots the impulse response functions for both range and GARCH(1,1) volatility to a one-standard-deviation shock in retail trading activity. The response clearly shows that both measures of volatility increase on impact. Range volatility increases by more than 6% relative to its sample mean while GARCH(1,1) volatility increases by almost 9% relative to its sample mean. The effect remains persistent and statistically significant at a 5% percent level for three business weeks. Figure 3 plots the results from the same exercise separately for the COVID- and pre-COVID periods. One can clearly note that the response of both range and GARCH(1,1) volatility is enhanced during the pandemic. The contemporaneous response for range volatility is 2.5 times higher during the COVID-period than during the pre-COVID period. For GARCH(1,1) volatility, this factor is close to five. This preliminary evidence again supports our initial argument that not only does retail trading have deteriorating effects on the stability of stock prices, but that effect might also be particularly enhanced during periods of crisis (in this case, the COVID-19 pandemic).

Even though these initial results are encouraging, they are far from conclusive. First, the VAR includes the two variables of interest but also excludes a wide range of other variables that may traditionally explain volatility. For instance, market capitalization is an obvious candidate that would explain both volatility and retail trading (since a bigger firm would likely attract greater attention). Accommodating all potential controls in a VAR is practically infeasible since VARs suffer from the well-known ‘curse of dimensionality’. According to our VAR setup in Eq. (1), including an additional endogenous variable requires \( q(2n + 1) + 3 \) additional coefficients to be estimated, where \( q \) is the lag length and \( n \) is the number of variables in the baseline VAR.\(^7\) The number of additional coefficients is therefore linearly related to \( n \), which would make our estimation less precise. Second, the current VAR setup uses time series observations of volatility and retail activity without utilizing the rich panel structure of the available data. Most implementations of panel VARs (one of the natural solutions to this issue) use Generalized Method of Moment (GMM) techniques that makes handling more than 4000 panel IDs and almost 2 million observations computationally infeasible (see, for instance, Abrigo and Love, 2016). Furthermore, determining the appropriate instrument set would introduce a layer of specification uncertainty in the process. Lastly, studying state-dependent effects via a VAR (by splitting the sample into crisis and pre-crisis periods) does not constitute robust statistical inference since the impulse responses and their associated error bands come from different models. Hence, comparing those impulse responses directly is technically incorrect. Given these issues above, we employ a popular methodology from the empirical macroeconomics literature that conveniently allows us to study our relationship of interest without compromising the structure or disregarding the rich data set available.

2.3. Empirical estimation

As discussed previously, we study the dynamic response of range and GARCH(1,1) volatility to retail trading fluctuations using Jordá (2005) Local Projection (LP) framework. LP techniques have become a popular methodology to study the effect of one-time changes in variables of interest (see for instance Haug and Smith, 2012; Jordá et al., 2013; 2015a; Teulings and Zubanov, 2014; Jordá et al., 2015b; Ramey and Zubairy, 2018; Miyamoto et al., 2018; Boehm, 2020). The general framework involves a two-step procedure wherein we first estimate exogenous shocks to retail trading through a reduced-form process and, then second, we use those shocks to generate impulse response functions for both range and GARCH(1,1) volatility. Specifically, in the first step, we estimate the following dynamic process for all the stocks in a panel structure:

\[
\text{retail}_{i,t} = \beta + \alpha + \sum_{j=1}^3 \rho_j \text{retail}_{i,t-j} + \sum_{j=0}^1 \gamma_j \text{SI}_{i,t-j} + \gamma_{11} \text{ret} \text{turn}_{i,t-1} + \gamma_{21} \text{VIX}_{i,t-1} + \delta_i + \eta_{it} + \varepsilon_{it}
\]

where \( \text{retail}_{i,t} \) represents the TAQ-based retail trading measures explained in Section 2.1, \( \text{return} \) is the daily stock market return, \( \text{SI} \) is the logged stringency index, \( \text{price} \) is the logged closing price of the stock, \( \text{turnover} \) is the logged turnover, and \( \text{VIX} \) is the logged CBOE VIX index. We have also added a linear time trend to capture any secular changes in retail trading. The specification is estimated with firm and day-of-week fixed effects which controls for firm-specific factors and the so-called weekend effect (see, for example, French, 1980; Keim and Stambaugh, 1984; Berument and Kiyumaz, 2001; Zhang et al., 2017). We include SI since it has been argued that the extended lockdowns have decreased the opportunities for gambling (through casinos shutting down) and therefore, people may have resorted to retail trading to satisfy their risk appetite for gambling (Chiah and Zhong, 2020). From a microeconomic perspective, the pandemic may have triggered an income and substitution effect, both of which may translate into higher retail activity. The provision of supplemental cash through the CARES Act may

\(^7\) From Eq. (1), the total number of coefficients to be estimated is given by \( p(n) = 3n + qn^2 \), where \( q \) is the lag order and \( n \) is the number of variables in the system. The difference \( p(n+1) - p(n) \) is then given by \( q(2n + 1) + 3 \).
have also encouraged higher stock market participation from individuals. Combined, the pandemic presents an important opportunity for retail trading to increase.

One of the distinctive features of the model is the inclusion of autoregressive and lagged terms of other x-variables which implicitly introduces persistence and backward-lookingness in retail trading. Said differently, through autoregressive terms, we account for the idea that an average retail trader may continue investing in a stock which holds strong retail attention in the previous days. Both of these features are expected to be observed in high frequency or daily data, but are often omitted from single-equation estimations. This modelling assumption is also later corroborated from our results when we conduct an F-test for the null $\rho_j = 0 \forall j$, which is rejected at the 1% level. Although retail investors are generally considered to be uninformed (see, for example, Barber and Odean, 2000; 2008; Eaton et al., 2022), some recent studies suggest otherwise (see Kelley and Tetlock, 2013; Fong et al., 2014). Therefore, it is worthy to note that Eq. (2) (through the lagged controls) also allows for the fact that an individual investor may incorporate financial information available from the previous day. The addition of lagged dependent variables also helps minimize any potential serial correlation in the residuals. Five lags of the dependent variable are included to capture movements from the previous business week. In Section 5, we further relax Eq. (2) and increase the number of lags on financial controls to three (guided by the Akaiake Information Criteria) and find qualitatively similar results.

In Eq. (2), $\hat{\delta}$ captures the exogenous movement in retail trading orthogonal to government intervention during the pandemic, realized investment trends in retail trading, and financial market controls including return, price, and turnover. Since the overall magnitude of the shock might vary considerably, we first standardize the predicted $\hat{\delta}_t$ (denoted as $\tilde{\delta}_t$) and then estimate the following series of equations to recover the effect of a one-standard-deviation shock to retail trading on market volatility for each horizon $j$:

$$y_{t+\delta} = \alpha_j + \gamma_j + \sum_{j=1}^{5} \beta_j y_{t-j} + \rho_j \hat{\delta}_t + \Delta \Theta + \epsilon_{t+\delta}$$

(3)

where $j \in \{0, 1, 2, \ldots, 15\}$ and $\gamma$ is the set of variables for which we construct the impulse responses, namely range, GARCH(1,1), and retail trading. Following Blau et al. (2014), Blau (2018), $\Theta$ is a vector of controls that includes turnover, bid-ask spreads, market capitalization, stock market return, and a Nasdaq indicator variable that takes the value of one if the stock is listed on the NASDAQ exchange and zero otherwise. We note that for the shock in absolute retail trading, we add total trading activity as an additional control. Since large cap stocks are more likely to engage retail traders, adding this variable as a control ensures that our coefficients do not wrongly attribute that effect to retail activity. All variables are logged except for daily return and the indicator for Nasdaq.

To cater to COVID-induced changes, we also add the first two lags of daily economic uncertainty (EPU) from Baker et al. (2016). Baker et al. (2016) have shown that EPU, in general, is associated with higher stock-market volatility and therefore serves as a relevant control in our model. We estimate the equation using OLS with 2-digit North American Industry Classification System (NAICS) industry and day-of-week fixed effects. Since the Jorda method is susceptible to serial correlation owing to the successive leading y-variable terms, we are careful to estimate Eq. (3) using Driscoll and Kraay (1998) standard errors which are robust to heteroscedasticity, autocorrelation, and cross-sectional dependence. Equation (3) estimates the relevant coefficients in a linear fashion. The sequence of estimated $\rho_j$ are used to construct the impulse response of our variable of interest to a one-standard-deviation shock to retail trading. More prominently, we add five lags of volatility to the equation to ensure that our coefficients are not biased due to some reverse causal effect from volatility to re-
tail trading.\textsuperscript{9} Adding stock-specific lags ensures that the results are not spurious. Hence, the impact of retail trading on market volatility is uncontaminated from the more traditionally detected channels in the literature so far.

It is instructive to compare and contrast the two retail trading specifications. Our first specification takes into account total retail activity (i.e., logged sum of buy and sell trades) while including total trading activity as an additional control in Eqs. (3) and (4). The relative measure, on the other hand, does not control for total trading activity but instead retail activity is scaled by total trade activity. While both measures capture retail trading, an exogenous increase in both carry subtle differences. A shock to our absolute measure, while keeping overall trading activity fixed, implies that the share of retail trading increases solely through an increase in retail buy or sell trades. A shock to the relative measure could occur if either retail activity increases (i.e., the numerator increases) or total activity decreases (i.e., the denominator decreases). Both of these possibilities can bring about an increase in the overall ratio. Implicitly, this measure equates the presence of retail trades with the absence of non-retail activity. The absolute measure does not make that equivalence and, hence, is equivalent to an increase in the proportion via an increase in retail activity. Notwithstanding the differences above, the two measures are highly correlated with a correlation coefficient of 0.73 (see Fig. 1b).

Until now, the LP model setup can only analyze the causal effect of retail trading on market volatility in a linear fashion. However, as is common during times of crises, one can reasonably expect certain interaction effects to arise from the pandemic. As argued by Abreu and Brunnermeier (2002) and Stein (1987), such interaction effects can result due to higher synchronization risk during times of market crises. In the framework of our research question, one might therefore expect that the state of the COVID-19 pandemic to not only increase retail trading as a whole, but the pandemic might also increase the effect of this type of trading on volatility. Therefore, we modify our baseline Eq. (3) in a way to empirically test for any potential dependence between various states of the pandemic (COVID vs. non-COVID) and retail activity:

\begin{equation}
\gamma_{i,t+j} = D_t \times \left[ \alpha_j^T + \gamma_j^T t + \sum_{j=1}^{5} \beta_j \gamma_{i,j-1}^T + \rho_j^N \xi_{i,t} + \xi_j^T \Theta \right] + (1 - D_t) \\
\times \left[ \alpha_j^N + \gamma_j^N t + \sum_{j=1}^{5} \beta_j \gamma_{i,j-1}^N + \rho_j^N \xi_{i,t} + \xi_j^N \Theta \right] + \epsilon_{i,t+j}
\end{equation}

where $D_t$ defined as an indicator variable which takes the value of unity or zero depending on the state of the pandemic. $C$ or $N$ in the superscript refer to the COVID and non-COVID periods, respectively. In the baseline specification, we define $D_t$ using the date when the first COVID-19 infection was registered in the US:

$D_t = \begin{cases} 1 & \text{if } t \geq 21 \text{st January, 2020} \\ 0 & \text{otherwise} \end{cases}$

In Section 5, we show that our results are robust to changes in $D_t$. Eq. (4), as opposed to (3), allows our coefficients to vary across the states. Therefore, we allow our $y$-variables to react based on the state of the pandemic when the retail trading shock is introduced into the market. Therefore, the effect of retail trading on either range and GARCH(1,1) volatility now becomes a function of the presence of the pandemic. This effect in normal times is given by $\rho_N^j$ whereas the effect during the pandemic is given by $\rho^j$. The difference between the two time periods can formally be tested through a null of $H_0: \rho^N_j = \rho^j$. The magnitude and statistical significance on the difference of the two would thus compare the pre-COVID-19 and COVID-19 period effect on market features of an exogenous shock to retail trading. If $\alpha_j^T = \alpha_j^N$, $\gamma_j^T = \gamma_j^N$, $\rho_j^T = \rho_j^N$, $\xi_j^T = \xi_j^N$ $\forall j$, our state-dependent model collapses to that in Eq. (2). One of the advantages of studying the effects of COVID-19 is that it was truly an unanticipated event. This, therefore, allows us to view $D_t$ and $(1 - D_t)$ as exogenous changes in states.

Although mentioned previously, it might be necessary to emphasize the importance of these empirical methods. Not only does LP allows us to exploit the panel structure of the data but it also allows us to control for a number of variables that would have otherwise been impractical in a VAR. Perhaps most importantly, it allows us to directly model and statistically verify the presence of interaction effects by nesting the two (COVID vs. non-COVID) states within the same model. To use VARs in this context would involve splitting the total sample into the two periods of interest and, then, comparing the impulse responses from those two models (as discussed in Section 2.2.1). Comparisons made under that arrangement lack robust statistical inference since the impulse responses (and their standard errors) would come from two different models. The LP technique, on the other hand, can handle such non-linearities within the umbrella of a single unified model. To the best of our knowledge, our work is one of the first attempts to adopt the LP method in a market microstructure setting, which broadens the contribution of our study.

3. Results

Figure 4 plots the value-weighted average of the extracted shocks from Eq. (2) for our absolute and relative retail trading measures. Retail trading shocks are highly volatile with a few major swings distributed during the sample. Particularly after the first COVID-19 infection was registered in the US, we observe stronger fluctuations with more frequent episodes where $\xi_t > 0$ for both of our measures, which highlights the observed increase in retail trading activity during the pandemic. In fact, the mean of the absolute shock is -0.009 (0.012) during pre-COVID (COVID) period. The increase is also similar for the relative retail trading measure. It also appears that retail activity became less stable during the pandemic. The standard deviation of the absolute retail trading measure during the pre-COVID (COVID) period is 1.80 (1.95). This variation provides additional motivation to examine the impact of retail trading on market volatility both linearly and in a state-dependent framework.

3.1. Linear impulse response analysis

We first consider results from the linear model which assume that the response of our variables is invariant to the pandemic. Figure 5 presents the impulse response functions (IRFs) for range and GARCH(1,1) volatility to a one standard deviation shock in either absolute or relative retail trading asures from the linear model in Eq. (3).

For both the measures, we observe similar responses for range and GARCH(1,1) volatility (see Fig. 5). Range volatility registers an immediate response followed by a slow decay over time. GARCH(1,1) volatility behaves a little differently as it takes a day to attain its peak, then, similar to range volatility, the response slightly decreases across the time horizon. While the volatility response to the shock dissipates, both range and GARCH(1,1) volatility consistently stay at a higher level when compared to the pre-shock level. In economic terms, range (GARCH) volatility contemporaneously increases by almost 12% (1.3%) relative to its sample mean following an absolute retail shock. Similarly, the contempor-
Fig. 4. Value-weighted absolute and relative retail trading shocks. Note: Shaded area indicates the COVID-19 pandemic, which we take to start from 21st January, 2020 i.e., the date when the first COVID-19 case was registered in the US. Relative and absolute shocks are the estimated value-weighted average of $\hat{\varepsilon}_{it}$.

Fig. 5. IRFs from Eq. (3) from relative and absolute retail trading shocks. Left column is for Relative Measures and right column is for Absolute measures. The purple (light grey) shaded bands are the 90% (95%) CI computed from Driscoll and Kraay (1998) standard errors that account for heteroskedasticity, serial correlation, and cross-sectional dependence. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)
neous range (GARCH) volatility response to the relative retail trading shock is 3.4% (1.6%) of its sample mean. It is interesting to note that even though we simulate a one-time shock to retail trading, the volatility response is persistent over the 15-day time horizon. These results highlight the importance of viewing volatility in a dynamic setting, which single-equation regressions may fail to capture.

Interestingly, while the overall gist of our results is the same, the response of our two volatility measures differs in the first 1–2 days, where the reaction of GARCH(1,1) volatility is delayed. That is, GARCH(1,1) volatility takes almost a day before it attains a peak compared to the instantaneous response of range volatility.

Afterwards, both responses decay in a similar way (which is a simple consequence of the fact that volatility series are mean-reverting). This initial delayed response is potentially a direct consequence of how volatility is modeled in a GARCH(1,1) framework where the current period's variance is a function of its lag (and the previous period's returns) i.e., \( \sigma^2_{t+1} = \alpha + \beta \sigma^2_t + \gamma R^2_t \) where \( \sigma^2 \) is the variance and \( R^2 \) is the squared return. Depending on the size of \( \beta \), this autoregressive nature explicitly induces inertia in the series that can potentially explain the delayed response in GARCH(1,1) volatility. In the case of range volatility, the dynamics from the previous day do not (at least not by construction) feed into the movements in period \( t + 1 \). Hence, the series would directly register the effects of any shock. Furthermore, the same argument would also imply that the rate at which the impulse responses decay (after the peak is attained) would be slower for GARCH(1,1) than range volatility as series persistence would also amplify the effects of one-time shocks. Indeed that is also true in our results. For the IRFs in Fig. 5, the half-life (i.e., the average number of days for the effect of impulse response to halve under an exponential decay) is 11.54 for GARCH(1,1) volatility and 6.4 for range volatility (for our absolute trading measure). In other words, it takes almost 1.8 times more time for the effect of GARCH(1,1) volatility to halve, on average, compared to range volatility. Results are similar for the relative measure. Our results also suggest that the difference in persistence has no bearing on the inferences that we draw as the persistence is similar across both of the volatility measures.

3.2. State-Dependent impulse response analysis

The primary objective of our analysis is to compare the causal impact of retail trading on volatility during the COVID period to the pre-COVID period. To do so, we estimate state-dependent IRFs from Eq. (4). The results for shocks in the absolute (relative) retail trading measure are in Fig. 6 (Fig. 8). To illustrate, we will first focus on the results from shocks in the absolute retail trading measure. At first glance, we note similarities in the behavior of the volatility responses across the COVID period and the pre-COVID period. However, we also observe stark, state-dependence in the responses as the COVID period produces volatility responses that are consistently higher than their counterparts from the pre-COVID period. In response to a retail trading shock, range and GARCH(1,1) volatility register a robust increase after which they persistently converge to a higher level vis-à-vis pre-shock levels. The response under the pandemic scenario is both economically and statistically significant. The contemporaneous reaction of range volatility is 1.3 times stronger during the pandemic as compared to normal times. Similarly, the one-period-ahead response in GARCH(1,1) volatility during the pandemic is stronger (almost 1.5 times stronger) than during the non-pandemic period. Over time, the effects in both pre-COVID and COVID periods decay over the 15-day time horizon but remain consistently elevated throughout the horizon. The difference between the state-dependent responses is shown in the right of Fig. 6. Here, we find that the differences are positive throughout the impulse horizon and are also statistically significant. This again signifies that shocks to retail trading during the COVID period increases market volatility by a greater extent than an identical shock during the pre-COVID period.

The results are qualitatively similar when we examine shocks to relative retail trading (see Fig. 8). In fact, volatility appears to respond more persistently under the pandemic as there is no visual decay in either range or GARCH volatility across the 15-day horizon.

Again, the panels on the right show the differences in responses between the COVID and pre-COVID periods. Here, we see that the differences are generally positive and reliably greater than zero (with a small exception in the top right panel during the first day of the time horizon).

Thus far, our results indicate that retail trading during the pandemic generated a greater response in volatility than retail trading during the pre-pandemic period. It might be interesting to examine how retail trading responds to a shock in retail trading. In the bottom two panels of both Figs. 6 and 8, we note that the retail trading response to a shock in retail trading was greater during the pandemic than during the pre-pandemic period (Fig. 6e and f).

Consequently, this implies that the more robust response of volatility during the pandemic may not necessarily be due to a structural change in the relation between volatility and retail trading but, rather, our results may simply be an artifact of higher retail trading activity during the pandemic, which is also reflected at various parts of our earlier evidence (Figs. 1 and 4). This means that our interpretations of the pandemic-dependent impulse responses on range and GARCH(1,1) volatility need to control for different retail trading responses in the two periods as well. Therefore, in the spirit of Shambaugh (2008), Forbes et al. (2018), Benchimol and Qureshi (2020), we construct implied difference measures from our state-dependent impulse responses, which explicitly control for the different responses of retail trading. We define \( \Gamma^y \) as the implied difference of our relevant y-variable to retail trading at horizon \( j \) as:

\[
\Gamma^y_j = \frac{\sum_{i=1}^j (\hat{y}_i | D_t = 1) - \sum_{i=1}^j (\hat{r}_i | D_t = 0)}{2}
\]

where \( \hat{y} \) is the impulse response of our y-variable (i.e., range or GARCH(1,1) volatility) at horizon \( j \) and \( \hat{r} \) is the response of retail trading at horizon \( j \) where \( j \in \{0, 1, 2, 3, \ldots, 15\} \). As before, \( D_t \) is the COVID-19 dummy described in Section 2.3; therefore, \( (\hat{y}_i | D_t = 1) - (\hat{y}_i | D_t = 0) \) is the difference in the impulse response of \( \hat{y} \) during COVID-19 pandemic from pre-pandemic environment (more simply, it is the difference in Fig. 6b and d). The denominator is the mid-point of the retail trading response from both states. \( \Gamma^y \), hence, captures the differential effect of retail trading on market volatility during the pandemic while controlling for different

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10 As GARCH reacts gradually, it attains a peak of 5% (2.7%) after a day when retail activity is given by absolute (relative) measure.

11 This difference might be explained by the argument in Abreu and Brunnermeier (2002) discussed previously. Given the construction of our relative measure, one of the consequences of synchronization risk is that an increase in retail activity (i.e., an increase in the numerator) causes a decrease in the total trade activity (i.e., reduces the denominator), hence further amplifying the effects of the shock over time. Stated differently, synchronization risk assumes that informed investors might step away from the market when uninformed investors begin to trade heavily. A shock to the relative measure of retail trading might increase the numerator, which will drive any informed traders away from the market, which might be reflected in a decrease in the denominator of our retail trading ratio.

12 Our model in Eq. (4) is state-dependent. Since one-standard-deviation in absolute retail trading is 198 (185) in the pre-COVID (COVID) period, a one-standard-deviation shock in the model would induce different responses in COVID and pre-COVID periods depending on the estimated \( \rho^y_j \) and \( \rho^y \) respectively. The reasoning is similar for the relative measure as well.
Fig. 6. IRFs around COVID-19 pandemic for absolute retail trading measure. The figure presents the state-dependent impulse responses from Eq. (4) for range volatility, GARCH(1,1) volatility, and retail trading. All figures on the right-hand side are the difference between COVID-19 and pre-COVID-19 responses from the corresponding left-hand side figures. The purple (light grey) shaded bands are the 90% (95%) CI computed from Driscoll and Kraay (1998) standard errors that account for heteroskedasticity, serial correlation, and cross-sectional dependence. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)
...and retail traders continued to actively participate without significantly de-risking their portfolios during the financial crisis (Hoffmann et al., 2013).

Table 2 shows the summary statistics for some of the key variables from 01/01/2007 to 05/31/2009, which is the period of our analysis. We define $D_t = 1$ (the crisis period) for dates after March 16th, 2008 that corresponds to the collapse and takeover of Bear Stearns. At the same time, it may be argued that any indicator-type variable to proxy for GFC might not be a good choice since several other financial institutions (including Fannie Mae and Freddie Mac) faced a tumultuous spring and summer. Hence, as additional robustness checks, we run two exercises where we omit periods that cannot be classified as either crisis or pre-crisis periods. This removes data that might be contaminated with anticipation effects. The details of these modifications and their results are discussed in Section 5.

From Table 2, we note that the average market capitalization has increased by about 3.5 times between 2007–2009 and 2018–2020 period. The average stock has a turnover of 0.01, bid-ask spread of 0.01, and a closing price of 36.65. Range and GARCH(1,1) volatility have an average value of 0.06 and 0.05, respectively. In total, there are 4253 unique tickers in the GFC sample. Although, we utilize the same BJ2Z-algorithm-based retail measures in the tests that follow, we note that identifying marketable retail trades did not completely stabilize until 2010 because during the 2005 to 2009 period, brokerage firms were gradually in the process of adopting price improvement provisions for retail trades. (see Boehmer et al., 2021 Section I.A. and Figure A1 in their Internet Appendix). Accordingly, we raise caution in the inferences we draw from the results in this section.

We now repeat the same exercise as in Section 3 for the GFC period in an attempt to generalize our results. Figure 10 plots the state-dependent impulse response functions for the GFC period. In line with the previous results, we again find robust evidence that a shock to retail trading influences market volatility by a greater extent during market stress periods than it does during periods of normalcy. Responses in range and GARCH(1,1) volatility exhibit a contemporaneous jump in response to a one standard deviation shock to absolute retail trading measure. Figures 10b and d further show that the response of range and GARCH(1,1) volat-

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13 For a complete timeline, see https://www.reuters.com/article/us-bearstearns-chronology/timeline-a-dozen-key-dates-in-the-demise-of-bear-stearns-idUSN1724031920080317.
Fig. 8. IRFs around COVID-19 pandemic for relative retail trading measure. The figure presents the state-dependent impulse responses from Eq. (4) for range volatility, GARCH(1,1) volatility, and retail trading. All figures on the right-hand side are the difference between COVID-19 and pre-COVID-19 responses from the corresponding left-hand side figures. The purple (light grey) shaded bands are the 90% (95%) CI computed from Driscoll and Kraay (1998) standard errors that account for heteroskedasticity, serial correlation, and cross-sectional dependence. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)
ity is higher for GFC period compared to pre-GFC period. In economic terms, the contemporaneous response of range volatility is 1.5 times stronger during the GFC as compared to the pre-GFC period. Similarly, for GARCH(1,1) volatility, the one-period-ahead response during GFC is also almost 1.3 times stronger in the GFC period than the pre-GFC period.

Similar to the behavior in the COVID period, the results for our relative retail trading measure are both qualitatively similar (in that the GFC response is higher) and quantitatively stronger (since the GFC response for range and GARCH volatility tend not to decay during the 15-period horizon). The corresponding implied differences simply reinforce and summarize these results. For our absolute measure, the implied differences for range (Fig. 12a) and GARCH (Fig. 12b) volatility are both positive and statistically significant for much of the response horizon. The on-impact implied difference for range (GARCH) volatility is around 0.02 (0.0004), which accumulates over three business weeks to a value close to 0.03 (0.018).14

More generally, range and GARCH(1,1) volatility responses to retail trading shocks are stronger in the recent COVID-19 pandemic than that in the GFC. For instance, the contemporaneous response of range (GARCH) volatility to absolute retail trading shock for COVID-19 was 1.8 (1.7) times higher than that in GFC (see Figs. 6a, c, 10a, and c). The results from the relative retail trading measure are also broadly consistent. Implied difference responses sim-

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14 We do not report the implied differences for our absolute measure since the difference between retail trading responses in GFC vs. pre-GFC period is insignificant (see Fig. 10f). This is a simple consequence of the fact that absolute shock has similar standard deviation across both periods. Hence, there isn’t any statistically significant difference in retail trading IRFs. Recall that implied difference, as defined in Eq. (5), only merits discussion if retail trading responds differently in crisis vs. pre-crisis period.

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**Table 2**  
Summary Statistics for the GFC.

|                      | Observations | Mean   | Standard Dev | 25th  | 75th  |
|----------------------|--------------|--------|--------------|-------|-------|
| Absolute Measure     | 1,965,144    | 3.00   | 1.68         | 1.79  | 4.11  |
| Relative Measure     | 1,965,144    | -9.03  | 1.04         | -9.72 | -8.42 |
| Spread               | 1,965,144    | 2.38   | 7.12         | 0.09  | 1.26  |
| Turnover             | 1,965,144    | 0.01   | 0.02         | 0.00  | 0.01  |
| Price                | 1,965,144    | 36.65  | 1,394.33     | 4.37  | 26.50 |
| Range Volatility     | 1,965,144    | 0.06   | 0.06         | 0.03  | 0.07  |
| GARCH(1,1) Volatility| 1,965,144    | 0.05   | 0.03         | 0.03  | 0.05  |
| Stock Mkt Return     | 1,965,144    | -0.03  | 1.85         | -0.92 | 0.74  |
| VIX                  | 1,965,144    | 27.78  | 14.52        | 18.10 | 33.94 |

Absolute retail trading measure is defined as $\ln(1 + mrbrd + mrstd)$ where mrbrd and mrstd are the marketable retail buy and sell trades (respectively) identified using BJZZ algorithm. Relative retail trading measure is similar to its absolute counterpart except that it is normalized by total trading activity (see text for further details). Market Capitalization (MarkCap) is defined as price times share outstanding in billions. Spread is the ask – bid divided by the midpoint of ask and bid. Turnover is the trading volume divided by shares outstanding. Price is the closing price for the day. Range Vol is defined as the log-difference between the highest and lowest price registered in a day (i.e. $\ln(\text{high}) - \ln(\text{low})$). GARCH(1,1) volatility is the square root of the stock-by-stock conditional variance obtained from a GARCH(1,1) model. EPU is the news-based Equity-related economic uncertainty index from Baker et al. (2016), SI is the Stringency Index from OXCGRT database, Mkt Return is the daily stock market return obtained from Professor Ken French’s website, VIX is the CBOE VIX index obtained from the FRED database, and Cases and Deaths are the confirmed COVID-19 cases and deaths. To suppress the impact of outliers on our results, we winsorize all variables at 1% and 99% levels. The table reports means, standard deviations, and values at the 25th and 75th percentile. There are 4253 unique stocks in the sample.
Fig. 10. IRFs around GFC using absolute retail trading measure. The figure presents the state-dependent impulse responses from Eq. (4) for range volatility, GARCH(1,1) volatility, and retail trading. All figures on the right-hand side are the difference between GFC and pre-GFC responses from the corresponding left-hand side figures. The purple (light grey) shaded bands are the 90% (95%) CI computed from Driscoll and Kraay (1998) standard errors that account for heteroskedasticity, serial correlation, and cross-sectional dependence. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)
Fig. 11. IRFs around GFC using relative retail trading measure. The figure presents the state-dependent impulse responses from Eq. (4) for range volatility, GARCH(1,1) volatility, and retail trading. All figures on the right-hand side are the difference between GFC and pre-GFC responses from the corresponding left-hand side figures. The purple (light grey) shaded bands are the 90% (95%) CI computed from Driscoll and Kraay (1998) standard errors that account for heteroskedasticity, serial correlation, and cross-sectional dependence. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)
ply mirror these observations, where the on‐impact implied difference for range volatility during COVID‐19 (GFC) is about 0.08 (0.02), which accumulates over three business weeks to a value close to 0.12 (0.03) (see Figs. 9 and 12). One plausible channel that might explain this result could be the recent technological advances, opportunities for zero‐commission trading, and coordination through social media. In fact, recent empirical research documents increased retail investor participation on social media platforms, which has led to more coordinated trading during the pandemic (see, for example, Pyun, 2021; Tengulov et al., 2021). With a number of platforms to choose from (e.g., WallStreetBets, Discord, Reddit, 4chan) as well as more time to spend at home, the nature of retail activity may have changed during the COVID‐19 episode. Perhaps the pandemic generated more market stress than the GFC, which indirectly influenced retail trading activity. Comparing the last two recessions might provide a fruitful avenue for future research going forward. The results found here and those in Section 3, at a very minimum, highlight the role that crises play in identifying the effect of retail trading on market volatility.

5. Robustness checks

In this section, we conduct several exercises related to the LP technique to confirm whether our baseline results stand up to potential modifications in our specification. To conserve space, we present the resulting implied differences for LP‐related checks in Appendix A.13

First, we slightly modify Eq. (2) to check the sensitivity of our results to a differently estimated shock. In the baseline setup, we assume that retail activity shows persistence and responds to contemporaneous government intervention. Additionally, retail traders may incorporate financial information from the previous day as well. We make two distinct changes in this setup. First, we drop the contemporaneous stringency index variable and increase the number of lags of all variables (except the autoregressive terms which already have five lags) to three. While our baseline specification is fairly nonrestrictive, one might argue that an increase in the SI (i.e., COVID‐related lockdowns) cannot translate into higher retail activity within the same day. Imposing a zero short‐run restriction on the impact of government lockdown, therefore, might better reflect the reality. Consequently, we test the responses of range and GARCH(1,1) volatility when the contemporaneous effect of government lockdown interventions is restricted to zero. In other words, we introduce frictions in the way policy is implemented and is translated into higher retail trading activity. Furthermore, guided by the Akaike Information Criteria (AIC), we increase the number of lags on our x‐variables to three and introduce the contemporaneous terms of the financial controls, therefore, allowing retail trading to depend on information from both today and the relatively distant past, which is a more generous way to model retail trading. Figure 13 and 14 (Appendix A) show the implied differences for both of our volatility and retail trading measures. It is comforting to note that the estimated differences for the COVID vis‐à‐vis the pre‐COVID period is distinct, with the response of retail trading more robust during the former than the latter.

Second, one might argue that the impulse responses from the LP technique are generated without taking into consideration the uncertainty of 𝜀l,t. Said differently, the predicted residuals are included in Eq. (4) without factoring in their associated standard error. Therefore, to cater to this generated regressor problem, we modify our baseline LP model and estimate impulse response functions in a one‐step method where identification is achieved through controls (see Barnichon and Brownlees, 2019, and references therein). To do so, we estimate Eq. (4) by replacing 𝜀l,t with our retail trading measure and using the union of the independent variables in Eqs. (2) and (4) as the controls. The interpretation is equivalent to that in our baseline strategy in Section 2. Figures 15 and 16 similarly display the implied differences for this particular exercise, where the general crux of our baseline results hold.

Third, we have defined the COVID‐19 period as the date when the first COVID‐19 case was registered. At the same time, the date when the COVID‐19 shock occurs need not map onto the date when the first case was confirmed (refer to Fig. 1a where volatility spikes upwards after around 24th February, 2020—almost a month after the first COVID‐19 case is registered). Therefore, we change our definition of COVID‐19 pandemic and define 𝐃l = 1 for days after 24th February, 2020 to better reflect the date when the market reacted to the pandemic. The results from this exercise are in Figure 17 and 18, where they are again consistent with our baseline specification. Moreover, exogenous 𝜀l,t shocks from Eq. (2) are identified only if the shocks are serially uncorrelated. To address any potential serial correlation, we lastly add five lags of 𝜀l,t in Eq. (4) as additional controls. Implied differences from this exer-

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13 The online appendix is available under Supplementary Files.
cise are in Figure 19 and 20, which once again provide additional support to our main results.

As argued in Section 4, using an indicator-type variable around GFC might not be a good choice since several other financial institutions (including Fannie Mae and Freddie Mac) faced turbulence in the spring and summer of 2008. Hence, to cater to that concern, we conduct the following two exercises. First, we drop the 1st March-September 2015th 2008 period, so that the GFC dummy takes one after September 15th and zero before 1st March. This allows us to remove much of the period that is not exactly crisis or pre-crisis. We expect the results to be cleaner than the baseline dummy definition. Second, based on observing the VIX series, we remove the August 1st 2007 - September 15th 2008 period. From Figure 25 in Appendix A, one can note that market volatility had risen well before September 15th, 2008 making August 1st 2007 - September 15th 2008 not a good measure of either stable or unstable period. Hence, to validate our results, we re-run our analysis without it. Implied differences for both our retail measures and for both these modifications are in Figures 21, 22, 23, and 24. It is comforting to note that all responses are positive and statistically significant, implying an amplified role of retail trading in increasing market volatility.

Lastly, one possible concern to our setup in Eqs. (2) and (4) is that of international shocks that were prevalent especially during the time pandemic hit the US. Particularly, there might be concerns of “contagion” effects, as described in Forbes and Rigobon (2002), spilling from Chinese stock market to U.S. markets (as China was the first country affected by COVID shocks and, therefore, a valid transmitter). Within our empirical strategy, this might not be that strong of a concern since any contagion effect that confounds our results also needs to simultaneously increase retail trading. However, to empirically address these concerns, we also implement the correction suggested in Forbes and Rigobon (2002) and test for changes in cross-correlation between the US and Chinese asset return series during the COVID-19 period. Similar to Forbes and Rigobon (2002), we use each country’s aggregate stock market indexes: returns in the S&P500 index for US and the returns in the Shanghai Stock Market Composites Index for China obtained from Bloomberg. For GFC, we can reasonably rule out any possible contagion effect since the shock originated in the US itself. Using Forbes and Rigobon (2002) correction, we observe a cross-correlation of 0.07 in the pre-COVID period and 0.11 after COVID-19 kicked in, where the correlations are not statistically different from each other at a 5% level. This seems to alleviate any concerns of potential bias arising from contagion effects.

6. Conclusion

The outbreak of coronavirus (COVID-19) has caused an unprecedented health crisis and the corresponding government policy has created a framework to better understand financial markets. The economic shutdown generated the fastest 30% correction in the history of US stock markets. During the height of this correction, the VIX soared from $13.68$ on February 14th to an all-time high of $82.69$ on March 16th reflecting a nearly $500\%$ increase in volatility. The extreme stock price movements triggered the circuit breaker mechanism multiple times during March 2020. In fact, Baker et al. (2020) find that no other disease outbreak in the US has contributed to stock market volatility as much as the COVID-19 pandemic. Understanding the evolution of such volatility becomes important as stability in financial markets might create a better framework for the efficient allocation of capital among firms.

Existing research suggests that retail trading is directly associated with volatility in financial markets. In this paper, we attempt to isolate the causal effect of retail trading on volatility during the COVID-19 pandemic. In particular, we hypothesize that during periods of market stress, synchronization risk — as described by Abreu and Brunnermeier (2002) — significantly increases, thus leading to coordination failure among informed market participants and destabilized prices. Accordingly, during such periods, the effect of retail trading on volatility can be amplified. To test our hypothesis, we rely on the Boehmer et al. (2021) algorithm to identify marketable retail trades. Using a series of novel econometric methods, we find a causal, negative impact of retail trading on the stability of financial markets that was particularly stronger during the pandemic than during the pre-pandemic period. These results suggest that periods of crises affect the destabilizing influence of retail trading on stock prices. To provide additional evidence, we replicate our empirical exercise during the 2008-09 financial crisis in an attempt to examine another crisis period. Results from these latter tests lend additional support for our hypothesis, as the effect of retail trading on the volatility of stock prices is stronger during the financial crisis vis-à-vis the pre-crisis period.

Data availability

The authors do not have permission to share data.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jbankfin.2022.106627.

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