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# Individual Investors and Volatility

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**ABSTRACT**

We show that retail trading activity has a positive effect on the volatility of stock returns, which suggests that retail investors behave as noise traders. To identify this effect, we use a reform of the French stock market that raises the relative cost of speculative trading for retail investors. The daily return volatility of the stocks affected by the reform falls by 20 basis points (a quarter of the sample standard deviation of the return volatility) relative to other stocks. For affected stocks, we also find a significant decrease in the magnitude of return reversals and the price impact of trades.

*Anything that changes the amount or character of noise trading will change the volatility of price.*

—Fischer Black (1986, p. 533).

IN THIS PAPER, we test whether retail trading is a determinant of the idiosyncratic volatility of stock returns. This test has broad implications because the volatility of stock returns is a key variable in various areas of finance (e.g., asset pricing or risk management) yet its determinants are not well understood. For instance, Shiller (1981), Leroy and Porter (1981), and Roll (1988) show that volatility cannot be explained solely by changes in fundamentals. Moreover, the determinants of the time-series behavior of idiosyncratic volatility are still open to debate (e.g., Campbell et al. (2001), Wei and Zhang (2006), Brandt et al. (2010), Fink et al. (2010), or Bekaert, Hodrick, and Zhang (2010)).

Models of noise trading such as DeLong et al. (1990), Campbell and Kyle (1993), Campbell, Grossman, and Wang (1993), or Llorente et al. (2002) predict that noise trading contributes to idiosyncratic volatility above and

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beyond cash flow news. Hence, retail trading may positively affect volatility if individual investors behave as "noise traders" or "liquidity traders." Evidence supports this hypothesis. On average, stocks purchased by retail investors underperform stocks sold by retail investors, which suggests that retail investors trade for non-informational reasons (e.g., misperception of future returns, shifts in risk aversion, or hedging needs).[1] Further, individual investors' trades contain a systematic component (see, for instance, Kumar and Lee (2006), Dorn, Huberman, and Sengmueller (2008), and Barber, Odean, and Zhu (2009)). Hence, individual investors' trades can move stock prices (see, for instance, Kumar and Lee (2006), Dorn, Huberman, and Sengmueller (2008), Kaniel, Saar, and Titman (2008), Hvidkjaer (2008)). However, the question of whether retail trading has a positive effect on volatility has yet to be answered.

Identifying the effect of retail investors on volatility is challenging because retail trading activity in a stock is endogenous and could itself be determined by idiosyncratic volatility. For instance, stocks with high idiosyncratic volatility may grab retail investors' attention, cater to the preferences of this clientele, or feature more frequent execution of stale limit orders placed by retail investors.[2]

To overcome this problem, we consider a policy change in the French stock exchange that triggers variation in retail trading activity for a subset of stocks without plausibly affecting other possible determinants of volatility. Until 2000, each stock listed on Euronext Paris was traded either on a market with end-of-month settlement (the "Règlement Mensuel," henceforth the RM) or on a market with a fixed settlement lag of 5 business days (the "Marché au Comptant," that is, the spot market). Stocks traded on the RM were not simultaneously traded on the spot market.[3] The monthly settlement procedure was suppressed and replaced by the fixed settlement lag procedure on September 25, 2000 to align the organization of Euronext Paris with other equity markets. Thus, since this date, all stocks listed on Euronext trade only on the spot market. We refer to this event as the "reform."

The RM was similar to a futures market (see Solnik (1990), Biais, Bisière, and Descamps (1999), and Section I). Retail investors could short stocks listed on the RM or could buy these stocks on margin, at virtually no cost. In contrast, for stocks listed on the spot market, leveraged positions were (and still are) costly for retail investors. Therefore, after the reform, speculation in stocks previously listed on the market with end-of-month settlement became more expensive for retail investors. Institutional investors were not really affected by this reform as they had other ways to leverage their positions.

[1] See, for instance, Odean (1999), Barber and Odean (2000, 2002), Hvidkjaer (2008), Barber, Odean, and Zhu (2009), and Barber, Lee, Liu, and Odean (2009).

[2] For instance, Kumar (2009) shows that individual investors prefer lottery-type stocks, defined as low-priced stocks with high idiosyncratic skewness and high idiosyncratic volatility. More generally, see Han and Kumar (2010) for reasons for which retail investors can be attracted by high idiosyncratic volatility.

[3] Buyers (sellers) of stocks listed on the RM could obtain immediate delivery (payment) at a cost equal to 1% of the value of the transaction.

Consequently, the reform triggered a drop in retail trading activity for the stocks affected by the reform. To establish this point, we use data on trades by clients at a large online broker. Over our sample period (1999 to 2002), this online broker accounts for about 40% of online brokers' trades on Euronext Paris, which collectively represent 18% of all trades on this market (with a peak of 22% in 2000).[4] We use various measures for the trading activity of the retail investors in our sample. As expected, we find that the reform coincides with a significant drop in retail trading activity for the stocks affected by the reform relative to other stocks (see Section IV.C).

If retail trading has a positive effect on volatility, we should therefore observe a drop in volatility for the stocks affected by the reform. In addition, if this effect arises because individual investors are noise traders, we should observe (i) a reduction in the absolute value of the autocovariance of stock returns and (ii) a reduction in the price impact of trades for the stocks affected by the reform. The reason is as follows. In models of noise trading, noise traders' aggregate demand shocks are transient, and are a source of price reversal. For instance, the sale of a stock by noise traders triggers a drop in its price to attract liquidity from sophisticated investors (arbitrageurs, market makers, etc.). The price then reverts as noise traders' aggregate holding reverts to its long-run level. The greater the contribution of noise trading to volatility, the higher the risk borne by sophisticated investors in providing liquidity to noise traders. Hence, other things equal, noise traders' aggregate trades have a greater impact on prices when noise trading risk is higher. Intuitively, these footprints of noise trading should be less apparent when trading becomes relatively more expensive for noise traders. Thus, after the reform, we should observe a drop in both the size of price reversals and the price impact of trades for the stocks affected by the reform. We check the validity of this logic by considering a simple extension of DeLong et al.'s (1990) model (see Section II).

We test our predictions by comparing the level of volatility, the autocovariance of stock returns, and the impact of trades before and after the reform for the stocks affected by the reform. Of course, other factors than the reform might affect the evolution of these variables over time. We control for these confounding factors by using stocks that are not affected by the reform (i.e., the stocks listed on the spot market throughout our sample period) as a control group. We also control for differences in the characteristics of these stocks and the stocks affected by the reform by using a matched sample estimation approach.

We find that the suppression of the monthly settlement procedure is associated with a significant reduction in the idiosyncratic volatility of daily returns for stocks listed on the RM relative to other stocks. This reduction is about 20 basis points and is economically significant as it represents about a quarter of

[4] We obtain these figures from two different sources: (i) "Acsel," an association of online brokers (see http://www.associationeconomienumerique.fr/) and (ii) a report published in 2000 by the French regulatory agency for financial markets (the COB). See "Les courtiers en Ligne," Bulletin COB n° 348, July–August 2000.

the standard deviation of the volatility of daily returns. We also directly estimate the impact of retail investors on idiosyncratic volatility using the reform as an instrument for retail trading activity. This approach suggests that retail trading contributes to about 23% of the volatility of stock returns in our sample. The auxiliary predictions are also supported by the data: after the reform, the size of stock return reversals and the price impact of trades are smaller for firms originally listed on the RM, relative to other stocks. Thus, overall, our findings are consistent with the view that some retail investors behave as noise traders.

Several empirical papers show that individual investors follow contrarian trading strategies on average (e.g., Grinblatt and Keloharju (2000) or Kaniel, Saar, and Titman (2008)). At first glance, this finding seems inconsistent with our main result because one expects contrarian trades to stabilize prices. To shed light on this issue, we use our data on trades by retail investors to measure the effect of the reform on contrarian and momentum trades by these investors. The reform has a negative effect on both types of trades but the drop in contrarian trades is twice as large. As volatility drops after the reform, we deduce that the destabilizing effect of retail investors' momentum trades must be bigger than the stabilizing effect of retail investors' contrarian trades. Another more puzzling possibility, suggested by recent experimental findings in Bloomfield, O'Hara, and Saar (2009), is that retail contrarian trades also destabilize prices, perhaps by slowing down price discovery.

Andrade, Chang, and Seasholes (2008) show empirically that non-informational trading imbalances in a stock affect the price of that stock and other related stocks. As a proxy for non-informational imbalances, they use the weekly change in shares held in margin account holdings for a large sample of Taiwanese retail investors. Interestingly, they find a positive and high cross-sectional correlation between the variance of non-informational trading imbalances and the variance of stock returns, after controlling for the volatility of firm cash flows.[5] Brandt et al. (2010) show that episodes of high and low idiosyncratic volatility are more pronounced in stocks held relatively more by retail investors, (e.g., stocks with low price, or stocks that catch the attention of retail investors, such as stocks with extreme returns). These findings suggest that there is a positive association between volatility and retail trading activity and are consistent with our findings because we show that retail trading has a positive causal effect on volatility.

The paper is organized as follows. In Section I, we provide more details on the French monthly settlement market and its reform. In Section II, we formulate our testable implications and explain how they can be obtained in a simple extension of DeLong et al. (1990). Formal derivations for this extension are given in Appendix A. We describe the data and variables used for our tests in Section III (the definition of these variables for our tests is given in Appendix B). We present the results of our tests in Section IV. Section V concludes.

[5] Hendershott et al. (2010) measure the contribution of transient price movements to the volatility of NYSE stocks using a state-space model. They also find a positive relationship between trades by individuals and the volatility of stock returns due to transient price movements.

## I. The French Monthly Settlement Market and Retail Trading

As explained in the introduction, a major difference between the RM and the spot market was the ease with which individual investors could leverage their positions on the RM. As this feature is key for our empirical tests, we describe the working of the RM in more detail in this section.

Consider the following example. An investor buys one share of a stock listed on the RM at price $P_{June,5}$ on June 5, 2000 (the treatment of a sale is symmetric). For this month, the settlement date is June 30 and trading for this settlement ends on June 23 (5 business days before the settlement date). That is, from June 24 onward, trades take place for settlement in July. If the investor unwinds her position before the close of June 23, then her cash flow on the settlement date is simply the difference between the resale price and the purchase price. If instead the investor does not close her position by June 23, then she has two options.

First, she can buy the stock on June 30. In this case, the amount of the purchase (i.e., $P_{June,5}$) is withdrawn from her margin account.[6] Alternatively, the investor can ask her broker to roll over her position to the next settlement period. In this second case, the broker closes the investor's position at the June 23 closing price and simultaneously purchases the stock at this price for delivery at the July settlement. For this service, the investor has to pay to her broker on June 30 a fee (called "le taux du report," that is, the roll over rate) proportional to the value of the new position opened at this date. From this point onward her long position is treated as a position opened at price $P_{June,23}$ for the July settlement. On June 30, the net cash flow for the investor is ($P_{June,23}$ $- P_{June,5}) - r * P_{June,23}$, where $r$ is the roll over rate. If the investor then closes her position in July, say on July 7, she receives at the July settlement date (July 28) the difference between the price at which she closed her position, say $P_{July,7}$, and the closing price on June 23. In all cases, cash flows are received or paid by the investor only at the settlement dates.

Table I summarizes this example by describing the cash flows associated with each option for the buyer using a numerical example.

Individuals could easily leverage long positions in stocks listed on the RM by buying a stock and reselling it before the settlement date (or rolling over their position to the next settlement period). Similarly, individuals could easily sell stocks that they did not own by covering their short position before the settlement date (or by rolling it over to the next settlement date). Thus, the RM was a way for retail investors to take short positions.

Retail investors could take long and short positions in stocks listed on the RM with limited capital relative to the size of their positions. Indeed, initial margins for retail investors were set by their brokers, usually at 20% of the value of

[6] To simplify, we assume in our example that the stock does not pay a dividend. The transfer of ownership from sellers to buyers only takes place at the settlement date. Thus, dividends are transferred from sellers to buyers at the settlement date.

**Table I**
**Buying a Stock on Réglement Mensuel**

This table describes the cash flows associated with the purchase of a stock on the Réglement Mensuel (RM) on June 5, 2000 at price $P_{June,5} = €102$. In this year, trading for the June settlement ends on June 23. As explained in the text, after this purchase, the buyer has three possible options: (i) close her position before June 23, (ii) roll over her position to the July settlement period (with settlement date July 28), or (iii) take delivery of the stock on the June settlement date (June 30) and sell it subsequently on, say, July 7. In the first case, we assume that the investor closes her position on June 10 at price $P_{June,10} = €105$ and in the second case we assume that the investor closes her position on July 7 at price $P_{July,7} = €104$ . The closing price on June 23 is set at $P_{June,23} = €100$ and the roll over rate is assumed to be 0.25%. Cash flows are received (or paid) by the investor only at the settlement dates.

| | Cash Flows on | | |
|---|---|---|---|
| Actions | June 5 | June 30 | July 28 |
| (i) Close position on June 10 at $P_{June,10} = €105$ | €0.00 | €3.00 | 0.00 |
| (ii) Roll over and close position on July 7 at $P_{July,7} = €104$ | €0.00 | −€2.25 | €4.00 |
| (iii) Take delivery of the stock on June 30 at $P_{June,5} = €102$ and sell the stock on July 7 at $P_{July,7} = €104$ | €0.00 | −€102.00 | €104 |

their transaction on the RM (both for sales and for purchases).[7] In contrast, it was costly for retail investors to short sell stocks on the spot market or to obtain a loan to buy these stocks.[8] For instance, Biais, Bisière, and Descamps (1999) note that (p. 397): "The monthly settlement system enables traders who do not own the stock to [...] avoid short-sales constraints. In contrast, for stocks traded spot, [...] this is costly and cumbersome in practice [to short sell]. Only large and sophisticated professional investors can undertake such strategies."

Brokerage firms voiced concerns that the suppression of the RM would reduce the trading activity of retail investors.[9] Online brokers were particularly vocal as a large fraction of their clients were actively leveraging their positions on the RM. To alleviate these concerns, Euronext launched a new service, called the "Service de Règlement Différé" (henceforth SRD), especially designed for retail investors.[10] For stocks eligible to the SRD, investors can submit buy or sell orders with settlement at the end of the month. Consider, for instance, a retail investor wishing to short sell 100 shares of Alcatel, a French stock

[7] To minimize counterparty risk, Euronext was acting as a clearing house and each broker had to maintain a margin account with Euronext. A broker clearing margin was calculated on a net basis.

[8] For instance, to short sell a stock listed on the stock market, an investor had to borrow it first while this was not a requirement on the RM (because the short position could be unwound before delivery).

[9] See, for instance, "Paris Bourse Will Require Web Brokers to Advance Clients Funds for Trading," *Wall Street Journal*, Eastern Edition; 07/24/2000.

[10] On October 9, 2000, *La Tribune* (a French financial newspaper) writes that "SRD is just a tool to accustom [domestic] retail investors to the spot market. Institutions, on the other hand, already have margin accounts, which are more suited to their needs." See "Le SRD, un outil transitoire pour faire accepter le comptant," *La Tribune*, October 9, 2000.

eligible to the SRD. This investor must contact a broker who accepts orders with *deferred* execution. In this case, the broker sells 100 shares on the spot market on behalf of the investor and effectively acts as a lender of the stock to the investor. At the end of the month, the investor must deliver the stock to the broker. In a similar way, an investor can purchase 100 shares of Alcatel with deferred payment. In this case, the investor's broker lends the amount required for the purchase. Stocks eligible for the SRD are chosen by Euronext and nearly all stocks listed on the RM in September 2000 became eligible to the SRD.

The SRD is a service provided by brokers to their clients. Not all brokers provide this service and those who do charge an additional fee for handling orders with deferred execution. The extra cost for an order with deferred execution is therefore significant. For instance, for the average trade size of retail investors in our sample, the online broker considered in our study was charging (as of June 2001) a brokerage fee equal to 0.36% of the value of the order for regular orders and an additional fee of 0.20% of the value of the order (with a minimum amount of 6.2 euros) for orders with deferred execution. Overall, the reform increased the cost of short selling or buying on margin stocks previously listed on the RM for retail investors, as was pointed out by many analysts. For instance, on October 6, 2000, the French newspaper *Les Echos* wrote that "If its operations are close to the RM, the SRD is far from presenting the same advantages for the investor, especially in terms of costs." [11]

In summary, the suppression of the RM made trading in the stocks listed on this market relatively more expensive for retail investors than for institutions. Thus, it triggered a drop in the activity of retail investors for these stocks (see Section IV.C). As explained in the introduction, this reform was driven by the need to harmonize settlement procedures for the stocks listed on Euronext Paris. The reform should therefore be unrelated to expectations about the evolution of volatility for the stocks listed on the RM. For this reason, it offers a good way to identify the effect of retail trading activity on volatility.

## II. Testable Hypotheses

The reform gives us a way to identify the effect of retail trading on idiosyncratic volatility. But why should retail trading affect volatility? As explained previously, empirical evidence shows that retail investors have the traits of noise traders. Hence, a natural hypothesis is that retail trading has a positive effect on volatility because a fraction of retail investors behave as noise traders. If this hypothesis is correct, models of noise trading have three testable implications with respect to the reform.

IMPLICATION 1: The volatility of returns for stocks listed on the RM declines after the reform.

[11] See "SRD contre RM-quels avantages," *Les Echos*, October 6, 2000.

IMPLICATION 2: The autocovariance of stock returns for stocks listed on the RM is smaller (in absolute value) after the reform.[12]

IMPLICATION 3: The price impact of retail trades for stocks listed on the RM declines after the reform.

It is intuitive that noise traders' footprints on returns should be less apparent when trading becomes relatively more expensive for these investors. However, to our knowledge, this intuition has not been formalized by models of noise trading. For completeness, we check the validity of implications 1, 2, and 3 using DeLong et al.'s (1990) model (DSSW (1990)).

We now briefly describe how we adapt DSSW (1990) to analyze the policy change considered in our paper. More details on the derivations are given in Appendix A. The model features overlapping generations of investors who trade two securities, a risk-free asset and a stock, at dates $t = 0, 1, 2 \ldots$. The net supply (per capita) of the stock is normalized to one share. At date $t$, the stock pays a dividend $d_t = \bar{d} + \xi_t$, where the $\xi$s are i.i.d., normally distributed with mean zero and variance $\sigma_\xi^2$.[13] A new generation of investors arrives at each date. At the next date, this generation consumes the payoff of its portfolio and leaves the market. Investors have a mean-variance expected utility with a risk aversion parameter $\gamma$. There are two groups of investors, noise traders ($N$) and sophisticated investors ($S$), with relative population weights $\mu$ and $(1-\mu)$. Sophisticated investors have rational expectations on the distribution of the resale price of the stock. In contrast, noise traders arriving at date $t$ expect the mean resale price to be $E_t(p_{t+1}) + \rho_t$, where $\rho_t$ is normally distributed with mean zero and variance $\sigma_\rho^2$. Moreover, $\rho_t$ and $d_\tau$ are independent at all dates $t$ and $\tau$. Parameter $\rho_t$ is an index of noise traders' sentiment.

We compare the same stock in two market structures: (i) the RM and (ii) the spot market. The RM is the case considered in DSSW (1990): noise traders and sophisticated investors bear no trading costs. In contrast, trading in the spot market is relatively more expensive for noise traders than for sophisticated investors. Specifically, in the spot market, noise traders bear a trading cost that is quadratic in the size of their position. For simplicity, we assume that there are zero trading costs for sophisticated investors.[14] In this way, we can analyze the effect of making both purchases and sales more expensive for noise traders relative to sophisticated investors.

Let

$$R_{t+1} = d_{t+1} + p_{t+1} - (1+r)p_t \tag{1}$$

[12] Models of noise trading imply that both the autocovariance and the variance of stock returns should decline after the reform. Thus, their prediction for the autocorrelation of returns is ambiguous. We focus on the autocovariance of returns for this reason.

[13] Results can be extended to the case in which the dividend follows an AR(1) process. In this case, the dividend paid at a given date provides information on future dividends. This information is a source of fundamental volatility.

[14] Results are identical if sophisticated investors and noise traders bear a trading cost in the RM as well. The important point is that this cost is lower than in the spot market for noise traders.

be the excess return of the stock over the period $[t, t+1]$, where $p_t$ is the ex-dividend stock price of the stock at date $t$. In the Appendix, we show that, in equilibrium, the unconditional variance of excess returns is higher in the RM, in line with implication 1. Moreover, in equilibrium, the autocovariance of stock returns ($Cov(R_{t+1}, R_t)$) is higher in absolute value in the RM, consistent with Implication 2. Last, the change in equilibrium prices from date $t$ to date $t+1$ can be expressed as

$$p_{t+1} - p_t = \lambda_k \times \Delta X_{Nt}, \text{ with } k \in \{RM, Spot\}, \tag{2}$$

where $\Delta X_{N_t}$ is the change in noise traders' holdings from date $t$ to date $t+1$ and $\lambda_k$ is a scalar that depends on the parameters of the model. We refer to $\lambda_k$ as *the price impact coefficient* because it measures the sensitivity of the equilibrium price to noise traders' net trade in each period ($\Delta X_{N_t}$). The price impact coefficient is proportional to the conditional variance of returns in market structure $k$ (see the Appendix). As this variance is higher in the RM, trades have a higher impact in the RM, as predicted by implication 3.

Importantly, Implications 1, 2, and 3 are reversed if those who bear higher trading costs in the spot market are sophisticated investors. For instance, in this case, the volatility of excess returns should be higher in the spot market, not smaller. Thus, if in reality all retail investors are sophisticated investors, then the reform should have effects opposite to those predicted by Implications 1, 2, and 3 (or no effect). As shown below, our empirical findings are not consistent with this scenario.

## III. Data

In this section, we describe the data and define the variables that are used to test Implications 1, 2, and 3 derived in the previous section. Appendix B recaps the definition of these variables.

*Treated and Control Stocks.* We use various databases in this study. Our first data set provides us with the daily returns (adjusted for splits and/or dividends) and daily trading volumes for each stock listed on the French stock market from September 1998 to September 2002.[15] This data set also indicates whether a stock is listed on the RM or on the spot market before the reform, and the number of outstanding shares for each stock. We refer to stocks listed on the RM as of September 1, 2000 as the *treated stocks* and to the remaining stocks as the *control stocks*. A few stocks switch from the RM to the spot market or vice versa before the reform (10 stocks switch from the RM to the spot market and 9 from the spot market to the RM). We include these stocks in the control group. The results remain unchanged if we do not include these stocks in the sample (see the Internet Appendix).[16]

[15] These data are made available to us by EUROFIDAI. For information, see http://www.eurofidai.org/.

[16] An Internet Appendix for this article is available online in the "Supplements and Datasets" section at http://www.afajof.org/supplements.asp.

A few stocks in our sample serve as underlying securities for options and, since January 2001, single-stock futures. They are all listed on the RM. Arguably, speculators can use derivatives to avoid trading restrictions in the underlying securities. It should therefore be *more* difficult to identify the effect of the reform on the stocks that serve as the underlying of derivatives contracts. We do not exclude them from our sample. Our findings are robust to this decision (the robustness check is provided in the Internet Appendix).

For some tests, we also use tick-by-tick transaction data. We obtain these data from the *BDM* database made available by Euronext. For each transaction in a given stock, it provides the price of the transaction, the size of the transaction, and the bid–ask spread at the time of the transaction. This data set is described and used in other empirical studies (e.g., Bessembinder and Venkataraman (2003)).

Our unit of observation is a stock-month. On average, our sample in each month features 678 stocks in the control group (standard deviation 55) and 155 stocks in the treated group (standard deviation 5). Consistent with other related papers (e.g., Campbell, Grossman, and Wang (1993), Llorente et al. (2002)), we use daily observations to construct the variables of interest.

To test Implication 1, we use three different measures of volatility for a given stock-month: (i) the standard deviation of its raw daily return (*Volatility1*), (ii) the standard deviation of the daily difference between its return and the market return (*Volatility2*), and (iii) the standard deviation of the residual of the market model, that is, the time-series regression of the daily excess return for stock $i$ on the daily excess market return (*Volatility3*).[17]

To test Implication 2, we use the monthly autocovariance of the daily returns for this stock, denoted by *Autocov*.

To test Implication 3, we need a proxy for the price impact of trades. Toward this end, we use the monthly average of the daily ratio of the absolute return of a stock divided by its trading volume in euros. This ratio is sometimes called the Amihud measure or the Illiquidity ratio. Hasbrouck (2009) or Goyenko, Holden, and Trzcinka (2009) show that the Amihud measure is highly correlated with high frequency measures of price impacts (e.g., Kyle's lambda). Accordingly, we use this measure, which we denote by *PImpact*, as a proxy for the price impact of trades.

Table II, panel A reports summary statistics for the dependent variables in our study: our three measures of volatility, the autocovariance of daily stock returns, and the price impact measure. In each case, we report the means and standard deviations of these variables across months and stocks, separately for treated and control stocks. The table shows that the volatility of treated stocks is lower than for control stocks (241 basis points vs. 300 basis points). For both groups, the average autocovariance of daily returns is negative. However, returns of treated stocks tend to reverse themselves less. Our measure of price impact, *Pimpact*, is lower for treated stocks than for control stocks. These observations suggest that treated stocks are more liquid than control stocks.

[17] This regression is estimated each month.

**Table II**

**Summary Statistics**

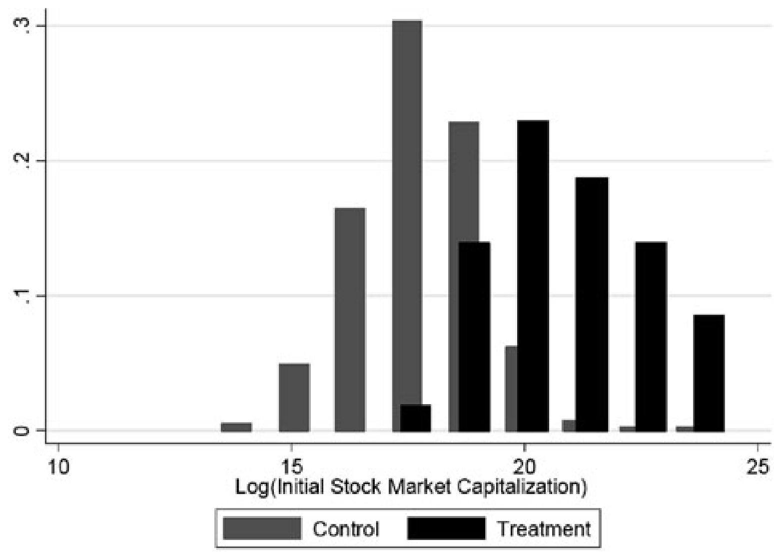
This table reports summary statistics for the main variables used in our study (*Turnover, Volatility1, Volatility2, Volatility3, Pimpact* and *Autocov)*. The definition of these variables is given in Appendix B. Sample means and standard deviations are separately computed for the samples of treated and control stocks. In panel A, each observation corresponds to a stock-month, for all months from 24 months prior to the reform until 24 months after the reform. In panel B, we report firm-level statistics on the characteristics of the firms in our sample as of September 2000.

| | Control | | | Treated | | |
|---|---|---|---|---|---|---|
| | Mean | *SD* | Obs. | Mean | *SD* | Obs. |
| **Panel A: Full Sample Statistics** | | | | | | |
| Market capitalization (bn € ) | 0.2 | 1.9 | 32,301 | 6.5 | 1.5 | 7,596 |
| Turnover (%) | 1.6 | 2.8 | 33,228 | 4.7 | 4.4 | 7,612 |
| Volatility1 (%) | 3.0 | 1.2 | 22,783 | 2.4 | 0.9 | 7,398 |
| Volatility2 (%) | 3.0 | 1.2 | 22,783 | 2.4 | 0.9 | 7,398 |
| Volatility3 (%) | 2.6 | 1.3 | 22,783 | 2.1 | 0.9 | 7,398 |
| Pimpact ($\times 10^6$) | 10.2 | 26.5 | 24,232 | 0.1 | 1.5 | 7,484 |
| Autocov ($\times 10^4$) | −0.5 | 2.8 | 21,947 | −0.2 | 1.9 | 7,377 |
| Bid–ask spread/midquote (%) | 7.0 | 7.8 | 7,793 | 1.8 | 1.0 | 2,731 |
| **Panel B: Firm-Level Statistics** | | | | | | |
| Total number of different stocks | 786 | n.a. | n.a. | 163 | n.a. | n.a. |
| Number of stocks per month | 678 | 55 | 33,222 | 155 | 5 | 7,595 |
| Fraction of previously state-owned firms (%) | 4.4 | 20.6 | 786 | 15.3 | 36.1 | 163 |
| # of years publicly listed (left censored on Jan 1992) | 5.0 | 3.3 | 786 | 7.4 | 2.5 | 163 |
| Fraction of firms listed since January 1992 (%) | 37.8 | 48.5 | 786 | 75.5 | 43.1 | 163 |

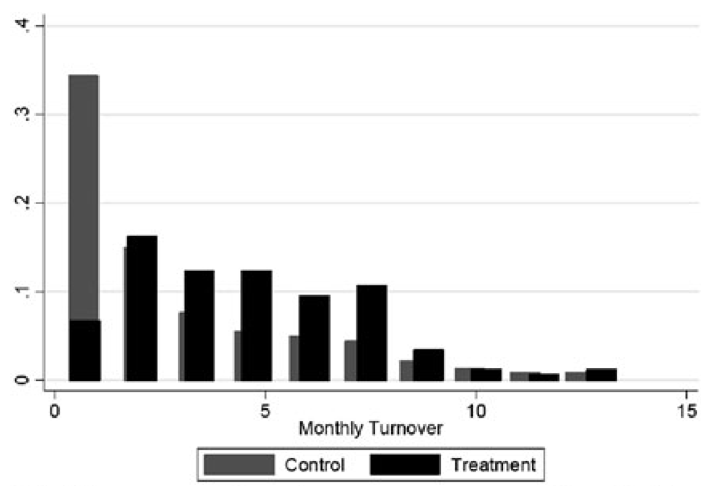
In fact, as shown in Table II, the relative bid–ask spread of treated stocks is smaller than for control stocks (1.8% against 7%).[18]

As shown in Table II, treated stocks have, on average, higher turnover (daily number of shares traded/outstanding number of shares) and larger market capitalization than control stocks. Indeed, Euronext allocated stocks to the RM only if they had a sufficiently large market capitalization or turnover. However, Euronext did not apply mechanical rules to allocate stocks to the RM and shifts from the RM to the spot market were rare. In fact, the transition rates from one group to the other are very low: in our sample, each month a treated (control) stock has a 0.26% (0.06%) likelihood of moving to the spot market (RM). As a result, in August 2000, our sample of treated stocks features only 9 stocks (out of 163) that began trading on the RM after September 1998. Similarly, as of

[18] For each stock, we compute the bid–ask spread by using the bid and ask price observed for the last transaction of each month. Table II reports the average of this bid–ask spread across stocks and months.




Panel A: Mean stock market capitalization over Jan 98–Sep 98




Panel B: Mean monthly share turnover over Jan 98–Sep 98

**Figure 1. Distribution of market capitalization and turnover.** This figure gives the empirical distribution of the log of market capitalization (panel A) and turnover (panel B) for treated stocks (black bars) and control stocks (gray bars) in September 2000. Turnover is defined as the average number of shares traded divided by the number of outstanding shares.

August 2000, only 10 control stocks (out of 758) had been listed on the RM at some point after September 1998.

For this reason, there are many control stocks with characteristics similar to treated stocks. Figure 1 illustrates this point by showing the distributions of stock market capitalization (panel A) and turnover (panel B) for each group at the beginning of our sample period.

The empirical distributions of turnover and market capitalization for the two groups are different.[19] Yet, as can be seen from the figure, the supports for the distributions of market capitalization and turnover largely overlap for control and treated stocks. For instance, 91 treated stocks (56% of the total number of treated stocks) have a capitalization lower than that of the control stocks in the top percentile of market capitalization. Conversely, 395 control stocks (52% of the total number of control stocks) have a capitalization higher than that of the treated stocks in the bottom percentile of market capitalization. This feature of our sample is useful as it enables us to control for differences in the capitalization and turnover of control and treated stocks by using a matched sample approach. We explain the matching procedure in detail in Section IV.B.

In panel B of Table II, we provide firm-level statistics for the firms in our sample. On average, treated stocks have been listed on the French stock market for a longer period than control stocks. For instance, as of September 2000, 75.5% of treated firms and 38% of control stocks were public since at least 1992. In addition, a higher fraction of treated firms used to be state-owned (15.3% against 4.4%).

*Online Retail Investors.* We also use data on trades by retail investors at a major French online broker to build various proxies for retail trading activity. Specifically, we have complete daily transaction records for all clients of this online broker from January 1999 to September 2002. There are 111,264 households in this database and about 5 million trades in the stocks in our sample over the entire period.

Table III presents summary statistics for the trading activity of the retail investors in our sample, for control stocks and treated stocks.[20] Panel A shows that the total number of shares traded per year by investors in our sample is higher for stocks listed on the RM than for stocks listed on the spot market. For instance, in 2000 the total number of buy and sell trades in treated stocks is about five times higher than in control stocks (1.4 million vs. 255,000). The average size of these trades is also greater for treated stocks (€4,037 vs. €2,164 in 2000). These differences may be due to the fact that treated and control stocks have different characteristics, as previously noted.

Panel B shows that in each month 5% to 9% (1% to 2%) of the investors in our sample execute five or more trades in treated stocks (control stocks). For treated stocks, the level of trading activity for the retail investors in our sample is strikingly similar to that found by Kumar and Lee (2006) for retail investors at a major U.S. discount brokerage house (see their Table I on page 2457). For instance, they find that in 1995, 36% of the investors in their sample trade at least 1 share in a month and 0.56% trade at least 25 shares. In our case,

[19] A Kolmogorov–Smirnov test rejects the equality of distributions of turnover and capitalization at the 0.1% level

[20] We just report these statistics for 1999, 2000, and 2001 because our sample stops in September 2002. Thus, figures in 2002 are not directly comparable to the annual figures reported for 1999 to 2001.

**Table III**

**Broker Data: Retail Investor Trading Behavior**

This table reports aggregate, investor-level, and stock-level trading statistics. Our sample consists of 111,264 retail investors with accounts at a large French online broker from 1999 to 2001. In panel A, we compute the total number of buy trades, sell trades, and average trade size per year. The last line of panel A reports the number of ”active investors” in each year, that is, the number of different investors buying or selling treated or control stocks in each year. Some investors may be trading stocks of both groups. In panel B, we compute, for each month, the fraction of investors trading treated or control stocks (some investors may trade stocks in both categories), and report the annual averages. In computing these fractions, we restrict ourselves to investors making at least one transaction during the year. In panel C, we compute, for each year, the fraction of stock-months with at least 1, 5, 10, 25, or 50 trades. In panel D, we report summary statistics for the proxies for retail trading activity used in our empirical tests (*#Buys, #Sells, # Trades, #SpecTrades,* $|NIT_{it}|, CONT_{it}, MOM_{it})$ . The definition of these proxies is given in Appendix B. Each variable is computed monthly for each stock and we report the mean annual value across stocks and across months per year. Variables $|NIT_{it}|, CONT_{it}$, and $MOM_{it}$ are used in Section IV.H. The last line in panel D reports the correlation between $NIT_{id(t)}$ and the daily return on stock $i$.

| | Control Stocks | | | Treated Stocks | | |
|---|---|---|---|---|---|---|
| | 1999 | 2000 | 2001 | 1999 | 2000 | 2001 |
| Panel A: Aggregate Trading Statistics | | | | | | |
| # Purchases | 70,664 | 141,998 | 66,942 | 451,079 | 780,180 | 362,656 |
| # Sales | 54,651 | 113,453 | 55,245 | 424,927 | 651,636 | 324,512 |
| Average trade size (€ ) | 2,122 | 2,164 | 1,626 | 3,634 | 4,037 | 3,151 |
| # Active investors | 21,191 | 30,636 | 22,449 | 43,033 | 65,022 | 55,021 |
| Panel B: Monthly Investor-Level Trading Statistics: Proportion of Investors | | | | | | |
| At least 1 trade | 0.14 | 0.14 | 0.09 | 0.35 | 0.33 | 0.25 |
| 5 or more | 0.01 | 0.02 | 0.01 | 0.09 | 0.09 | 0.05 |
| 10 or more | 0.003 | 0.006 | 0.002 | 0.04 | 0.04 | 0.02 |
| 25 or more | 0.0003 | 0.0010 | 0.0004 | 0.009 | 0.010 | 0.004 |
| 50 or more | 0.0000 | 0.0002 | 0.0001 | 0.003 | 0.003 | 0.001 |
| Panel C: Monthly Stock-Level Trading Statistics: Proportion of Stocks | | | | | | |
| At least 1 trade | 0.83 | 0.79 | 0.75 | 1 | 1 | 0.98 |
| 5 or more | 0.54 | 0.53 | 0.45 | 0.98 | 0.98 | 0.93 |
| 10 or more | 0.38 | 0.40 | 0.31 | 0.97 | 0.96 | 0.89 |
| 25 or more | 0.18 | 0.23 | 0.15 | 0.91 | 0.89 | 0.76 |
| 50 or more | 0.08 | 0.14 | 0.07 | 0.81 | 0.79 | 0.65 |
| Panel D: Monthly Stock-Level Trading Statistics: Trades ($\times$100) | | | | | | |
| # Buys | 0.04 | 0.05 | 0.04 | 0.05 | 0.04 | 0.02 |
| # Sells | 0.03 | 0.04 | 0.03 | 0.04 | 0.04 | 0.02 |
| #Trades | 0.07 | 0.09 | 0.07 | 0.09 | 0.09 | 0.04 |
| #SpecTrades | 0.01 | 0.01 | 0.01 | 0.02 | 0.02 | 0.01 |
| $\lvert NIT_{it}\rvert$ | 0.002 | 0.002 | 0.002 | 0.002 | 0.002 | 0.001 |
| *MOM* | 0.001 | 0.001 | 0.001 | 0.001 | 0.001 | 0.001 |
| *CON* | 0.001 | 0.001 | 0.001 | 0.001 | 0.001 | 0.001 |
| Correlation daily returns/daily NIT | −7 | −7 | −9 | −6 | −8 | −10 |

the corresponding statistics for treated stocks are 33% and 1% in 2000. Last, retail trading takes place across a large number of treated stocks and is not concentrated in a few stocks (see panel C). Indeed, in a given month, more than 60% of all treated stocks have 50 or more trades from investors in our sample.

It is also worth stressing that trades by retail investors represent a significant fraction of the daily trading volume for the stocks in our sample. In a given month, the ratio of their trades (buys and sells) to the number of shares traded in the month is on average 1.9% for treated stocks and 5.5% for control stocks for the entire sample period.

*Proxies for Retail Trading Activity.* In our empirical analysis, we use various proxies for retail trading activity in a stock. All these proxies are normalized by the total number of shares outstanding, as this number does not vary over time (in contrast to monthly turnover). First, for stock $i$ in month $t$, we use the following ratios to measure trading activity:

$$\#Buys_{it} = \frac{\#\text{ shares of stock } i \text{ purchased by the investors in our sample in month } t}{\text{Total Shares Outstanding}_{it}}, \tag{3}$$

and

$$\#Sells_{it} = \frac{\#\text{ of shares of stock } i \text{ sold by the investors in our sample in month } t}{\text{Total Shares Outstanding}_{it}}. \tag{4}$$

The average values of these ratios for treated and control stocks are reported in Table III, panel D. As of 2000, the buy and sell trades of the retail investors in our sample represent 0.05% and 0.04% of the number of shares outstanding in control stocks and treated stocks, respectively.[21] In some tests, we also use the sum of these two ratios, denoted $\#Trades_{it}$, as another measure of retail investors' trading activity.

As explained in Section II, the main advantage of the RM for retail speculators was the possibility of unwinding a position before actually selling or buying the stock. To assess whether investors were using this facility, for each month we count the number of trades (buys and sells) unwound before the end of the month for at least 98% of their size. We call them speculative trades. For each stock-month, we then compute the following ratio:

$$\#SpecTrades_{it} = \frac{\text{Number of speculative trades for stock } i \text{ in month } t}{\text{Total Shares Outstanding}_{it}}. \tag{5}$$

[21] These figures are small simply because the monthly turnover for a stock tends to be small. For instance, for the control stocks, monthly turnover is 1.6% on average (see Table II). Trades by investors in our sample account for about 5.7% of the monthly turnover for control stocks. Thus, they represent about $1.6\% \times 5.7\% \approx 0.09\%$ of the total number of shares outstanding for these stocks.

On average, this ratio is equal to 0.02% of all trades for the stocks listed on the RM against 0.01% for the stocks listed on the spot market (see panel D, Table III).

## IV. Empirical Results

### A. Correlation of Retail Trading and Idiosyncratic Risk

A natural starting point is to examine whether there is a relationship between idiosyncratic volatility and retail trading on the French stock market. To study this question, we estimate the following regression:

$$Volatility2_{it} = \alpha_i + \lambda_t + \beta_1 TA_{it} + \varepsilon_{it}, \tag{6}$$

where $Volatility2_{it}$ is the standard deviation of the daily excess return for stock $i$ in month $t$, $TA_{it}$ is a proxy for retail trading activity for stock $i$ in month $t$, and $\alpha_i$ and $\lambda_t$ are stock and time fixed effects. We estimate this regression with two different proxies for retail trading activity: $\#Trades_{it}$ (the number of purchases and sales for retail investors in our sample normalized by the number of shares outstanding for stock $i$ in month $t$) and $\#SpecTrades_{it}$ (the number of speculative trades normalized by the number of shares outstanding for stock $i$ in month $t$).

Our empirical results (in this section and in subsequent sections) do not depend on the way we measure volatility because our three measures of volatility are highly correlated.[22] Hence, we just report our results for *Volatility2*. Robustness checks with the two other volatility measures are provided in the Internet Appendix.

Table IV reports the findings.[23] There is a significant and positive dependence between retail trading activity and idiosyncratic volatility. Specifically, a one-standard-deviation increase in the number of trades by retail investors is associated with an increase in volatility that is about one-third of the standard deviation of the volatility of stock returns in our sample.[24] Findings are similar for speculative trades.

Thus, there is a positive correlation between retail trading and idiosyncratic volatility for the stocks considered in our study. However, as mentioned in the introduction, retail trading activity could well depend on idiosyncratic volatility, which precludes a causal interpretation of the previous result.

[22] For instance, the correlation between $Volatility2_{it}$ and $Volatility1_{it}$ is 0.93 while the correlation between $Volatility2_{it}$ and $Volatility3_{it}$ is 0.95.

[23] We do not have data on retail investors before January 1999. Hence, all our tests using measures of retail trading activity are for a sample period that runs from January 1999 to September 2002.

[24] The mean value and the standard deviation of *Volatility2* are 2.78% and 1.17%, respectively, while the mean value and standard deviation of *#Trades* are 0.07% and 0.14%, respectively.

**Table IV**

**Return Volatility and Retail Trading**

This table reports the estimate of the following regression:

$$Volatility2_{it} = \alpha_i + \lambda_t + \beta_1 TA_{it} + \varepsilon_{it},$$

where $Volatility2_{it}$ is the standard deviation of the daily difference between the raw return of stock $i$ and the market return in month $t$, $TA_{it}$ is a measure of retail trading activity for stock $i$ in month $t$, and $\alpha_i$ and $\lambda_t$ are stocks and time fixed effects, respectively. In columns 1 to 3, we measure retail trading activity for stock $i$ in month $t$ as the sum of the number of shares of stock $i$ purchased and sold in month $t$ by retail investors in our sample ($TA_{it} = \#Trades_{it}$). In columns 4 to 6, we measure retail trading activity for stock $i$ in month $t$ as the number of "speculative" trades in stock $i$ in month $t$ ($TA_{it} = \#SpecTrades_{it}$). In each month, speculative trades for a given stock are buy and sell trades (in number of shares normalized by the number of shares outstanding) in this stock by retail investors in our sample that are unwound within month $t$ for at least 98% of their size before the end of the month. The sample period for this test starts in January 1999 and ends in September 2002. In brackets, we report $t$-statistics based on doubled-clustered errors that allow for correlation in residuals over time and across firms. Superscripts $^{***}$ indicates that estimates are significantly different from zero at the 1% level of significance.

| | *Volatility2* | | | | | |
|---|---|---|---|---|---|---|
| (×100) | (1) | (2) | (3) | (4) | (5) | (6) |
| #Trades | 3.0$^{***}$ | 2.0$^{***}$ | 1.8$^{***}$ | | | |
| | [16.1] | [22.4] | [11.6] | | | |
| #SpecTrades | | | | 10.5$^{***}$ | 7.2$^{***}$ | 6.0$^{***}$ |
| | | | | [17.2] | [11.4] | [16.2] |
| Stock FE | No | Yes | Yes | No | Yes | Yes |
| Time FE | No | No | Yes | No | No | Yes |
| Observations | 24,625 | 24,625 | 24,625 | 24,625 | 24,625 | 24,625 |

### *B. Identification Strategy and Methodology*

The reform of the French monthly settlement offers a good quasi-natural experiment to identify the effect of retail trading on volatility for several reasons. First, it is unlikely that this policy change had a *direct* effect on volatility. Moreover, it was not triggered by factors that also determine volatility. Last, switches from the RM to the spot market and vice versa were rare, which alleviates self-selection issues.

The main problem is that control and treated stocks differ systematically in terms of capitalization and turnover. In each of our tests of implications 1, 2, and 3, we control for these differences in two ways. First, we run the following differences-in-differences regression:

$$Y_{it} = \alpha + \beta_0 Treated_i + \beta_1 Post_t + \beta_2 Treated_i \times Post_t + \varepsilon_{it}, \tag{7}$$

where $Y_{it}$ is the variable of interest (e.g., *Volatility2*) for stock $i$ in month $t$, $Post_t$ is a dummy variable equal to one after September 2000, and $Treated_i$ is equal to one if stock $i$ is listed on the RM. The OLS standard deviations of differences-in-differences estimates are biased if there is serial correlation in the error terms for a given stock. To account for this problem, we compute standard deviations for our estimates by computing "double-clustered" standard errors, that is, by allowing for correlation in residuals over time and across firms (see Thompson (2009)). In equation (7), $Treated_i$ controls for differences in the characteristics of the two groups that are *fixed over time* and $Post_t$ controls for factors, common to all stocks (e.g., changes in the volatility of market returns), that affect the evolution of the dependent variable around the reform. The identifying assumption is that, on average, these factors have the same effect for control and treated stocks. Under this assumption, $\beta_2$ measures the causal effect of the reform on the dependent variable $Y_{it}$.

The limitation of this approach is that the evolution of, say, volatility may be different for stocks with relatively high capitalization (or high turnover) relative to stocks with low capitalization (or low turnover). In this case, our estimate of the impact of the reform on volatility ($\beta_2$) will in part reflect this difference because, on average, treated stocks have a higher capitalization and turnover than control stocks.

Fortunately, as explained in Section III, there are control and treated stocks with similar characteristics. We therefore address the previous concern by matching each stock in the treated group with a "twin" stock in the control group. We then test our implications by running the following regression:

$$Y_{it} - Y_{it}^{match} = \alpha + \delta_1 Post_t + \zeta_{it}, \tag{8}$$

where $Y_{it}$ is the variable of interest (e.g., *Volatility2*) for stock $i$ listed on the RM in month $t$ and $Y_{it}^{match}$ is the value of this proxy for the twin of stock $i$ in the set of control stocks. In this specification, the effect of the reform on retail trading activity is measured by the coefficient $\delta_1$. We again compute standard deviations for our estimates by computing "double-clustered" standard errors. We refer to this regression as the matched sample regression.

To make sure that our conclusions are robust to the matching method, we use three different matching methods: (i) quartile matching, (ii) percentage difference matching, and (iii) propensity score matching. With quartile matching, we compute the average market capitalization and turnover of each stock over the period 1998 to 1999 and we group stocks in quartiles of market capitalization and turnover. Thus, we obtain 16 groups of stocks. The variable $Y_{it}^{match}$ is then defined as the average value of $Y_{it}$ over all control stocks that are in the same group as treated stock $i$.

For the percentage difference matching, we use the following method suggested by the corporate finance literature (see Guo, Hotchkiss, and Song (2010), for instance). In September 2000, we compute for each treated stock $i$ the percentage differences between (i) its market capitalization and the market capitalization of each control stock and (ii) its turnover and the turnover of each control stock. We then define the "distance" between treated stock $i$ and each control stock as the maximum of these two differences. Finally, we pick as a match for treated stock $i$ the control stock for which this distance is minimum.

If the distance between stock $i$ and its nearest neighbor is greater than 10%, we exclude the treated stock from our sample.

Our last matching procedure uses the standard propensity score matching technique, where the score is computed by estimating the following logistic regression:

$$Treated_i = \alpha + \beta \ln(S_i) + \gamma V_i + \eta_i, \tag{9}$$

where $Treated_i = 1$ if stock $i$ is listed on the RM and $S_i(V_i)$ is the average market capitalization (turnover) of stock $i$ over the period 1998 to 1999. We then use the estimates of this logistic regression to compute the probability (the "score") that a stock is listed on the RM given its capitalization and turnover. Finally, we match each treated stock with the stock in the control group that has the closest score.

In summary, we test implications 1, 2, and 3 in two different ways. As a first pass, we run the differences-in-differences regression (7) and we test whether $\beta_2$ has the expected sign. In addition, to further control for the differences in the characteristics of control and treated stocks, we match each treated stock with a control stock using three different procedures and estimate regression (8) for each matched sample of stocks. We then test whether $\delta_1$ has the expected sign.

### C. Retail Trading and the Reform

Our first step is to show that the reform of the settlement procedure on the RM triggered a drop in retail trading activity for the stocks listed on this segment of the French stock market. Toward this end, we estimate regressions (7) and (8) by using various proxies for retail trading activity as a dependent variable, namely, the (normalized) number of shares of stock $i$ purchased by the retail investors in our sample in month $t$ ($Y_{it} = \#Buys_{it}$), the (normalized) number of shares of stock $i$ sold by the retail investors in our sample in month $t$ ($Y_{it} = \#Sells_{it}$), and the number of speculative trades in stock $i$ in month $t$ ($Y_{it} = \#SpecTrades_{it}$). We expect our estimates for the impact of the reform ($\beta_2$ for the differences-in-differences regression and $\delta_1$ for the matched sample regression) to be negative because the reform should trigger a drop in retail trading activity for the stocks listed on the RM relative to similar stocks listed on the spot market.

Table V reports the results. In column 1, we report estimates of the differences-in-differences regression (7). As expected, we find a drop in retail investors' activity in the stocks listed on the RM relative to the control stocks. The drop is statistically significant at the 1% level for all measures of retail investors' trading activity. The reform should have a negative impact on both buy and sell trades of retail investors because it makes both short sales *and* leveraged purchases of stocks listed on the RM more expensive for retail investors. This is what we find, as shown in panels A and B in Table V. Moreover, the magnitude of the impact of the reform ($\beta_2$) is identical for buy

**Table V**

**The Impact of the Reform on Retail Trading Activity**

In this table, we estimate the impact of the reform on various measures of retail trading activity. In column 1, we estimate the following regression:

$$Y_{it} = \alpha + \beta_0 Treated_i + \beta_1 Post_t + \beta_2 Treated_i \times Post_t + \varepsilon_{it},$$

where $Y_{it}$ is one of the measures of retail activity for stock $i$ in month $t$, $Post_t$ is a dummy variable equal to one after September 2000, and $Treated_i$ is equal to one if stock $i$ is listed on the RM. Coefficient $\beta_2$ is the differences-in-differences estimate of the effect of the reform on the dependent variable. In columns 2, 3, and 4, we estimate the following regression:

$$Y_{it} - Y_{it}^{match} = \alpha_i + \delta_1 Post_t + \varepsilon_{it},$$

where $Y_{it}$ is one of the measures of retail activity for stock $i$ in month $t$ and $Y_{it}^{match}$ is the value of this measure for the match of stock $i$ in month $t$ in the group of control stocks. We use three different procedures to choose a match for stock $i$ in month $t$: quartile matching, percentage difference matching, and propensity score matching (see Section IV.B). Estimates of the effect of the reform ($\delta_1$) with each matching procedure are reported in columns 2, 3, and 4, respectively. In panel A, $Y_{it} = \#Buy_{it}$, that is, retail trading activity is measured by the number of buys by retail investors in our sample in month $t$ for stock $i$. In panel B, $Y_{it} = \#Sell_{it}$, that is, retail trading activity is measured by the number of sells by retail investors in our sample in month $t$ for stock $i$. In panel C, $Y_{it} = \#SpecTrades_{it}$, that is, retail trading activity is measured by the number of speculative trades by the clients of our retail broker in month $t$ for stock $i$. All measures of retail trading activity are normalized by the number of shares outstanding for stock $i$ in month $t$. The sample period starts in January 1999 and ends in September 2002. In brackets, we report $t$-statistics based on doubled-clustered errors allowing for correlation in residuals over time and across firms. Superscripts * and *** indicate that estimates are significantly different from zero at, respectively, the 10% and 1% levels of significance.

| | DD (1) | Quartile Matching (2) | Percentage Difference Matching (3) | Propensity Score Matching (4) |
|---|---|---|---|---|
| Panel A: Dependent Variable: #Buys | | | | |
| Treated×Post ($\beta_2$) | −0.020*** | – | – | – |
| | [−4.15] | | | |
| Treated | 0.002 | – | – | – |
| | [0.37] | | | |
| Post ($\delta_1$) | −0.009* | −0.026*** | −0.026*** | −0.019*** |
| | [−1.81] | [−6.35] | [−5.77] | [−4.14] |
| Constant | 0.046*** | 0.031*** | 0.030*** | 0.024*** |
| | [9.35] | [6.64] | [4.77] | [2.92] |
| Observations | 29,214 | 6,790 | 4,208 | 5,007 |
| $R^2$ | 0.01 | 0.04 | 0.02 | 0.01 |

(*continued*)

and sell trades. Overall, the reform is associated with a drop of about 0.02 in the (normalized) number of buy trades or sell trades by retail investors in our sample. The economic size of this drop is large because it is half the average value of the proxies for retail trading activity in 1999 (see Table III, panel D).

**Table V**—*Continued*

| | DD (1) | Quartile Matching (2) | Percentage Difference Matching (3) | Propensity Score Matching (4) |
|---|---|---|---|---|
| Panel B: Dependent Variable: #Sells | | | | |
| Treated×Post ($\beta_2$) | −0.022*** | – | – | – |
| | [−4.77] | | | |
| Treated | 0.009 | – | – | – |
| | [1.61] | | | |
| Post ($\delta_1$) | −0.005 | −0.024*** | −0.029*** | −0.023*** |
| | [−1.14] | [−6.26] | [−6.48] | [−6.16] |
| Constant | 0.038*** | 0.032*** | 0.033*** | 0.029*** |
| | [8.19] | [7.13] | [5.97] | [4.47] |
| Observations | 29,214 | 6,790 | 4,208 | 5,007 |
| $R^2$ | 0.01 | 0.05 | 0.04 | 0.02 |
| Panel C: Dependent Variable: #SpecTrades | | | | |
| Treated × Post ($\beta_2$) | −0.014*** | – | – | – |
| | [−6.20] | | | |
| Treated | 0.011*** | – | – | – |
| | [4.56] | | | |
| Post ($\delta_1$) | −0.000 | −0.013*** | −0.015*** | −0.014*** |
| | [−0.06] | [−5.90] | [−5.60] | [−5.78] |
| Constant | 0.011*** | 0.017*** | 0.019*** | 0.015*** |
| | [5.86] | [7.00] | [6.54] | [5.41] |
| Observations | 29,214 | 6,790 | 4,208 | 5,007 |
| $R^2$ | 0.01 | 0.04 | 0.03 | 0.02 |

We report estimates of the matched sample regression (8) in columns 2, 3, and 4 of Table V (each column considers a different matching procedure). In each case and for each measure of retail trading activity, we find a significant decline in retail trading activity after the reform ($\delta_1 < 0$). Moreover, the magnitude of the decline is similar across specifications and similar to the magnitude of the decline measured using the differences-in-differences regression. For instance, with propensity score matching, we find a drop of about 0.019 (0.023) in the normalized number of buy (sell) trades for retail investors in our sample.

Figure 2 provides a visual representation of these findings by showing the evolution of the monthly average difference between the (normalized) number of speculative trades (*#SpecTrades*) for each treated stock and its match using the propensity score matching procedure. The reform has a clear and large negative impact on speculative trades.

### *D. Idiosyncratic Volatility and the Reform (Implication 1)*

As expected, the reform has a significant negative effect on retail trading activity for stocks listed on the RM relative to stocks listed on the spot market.

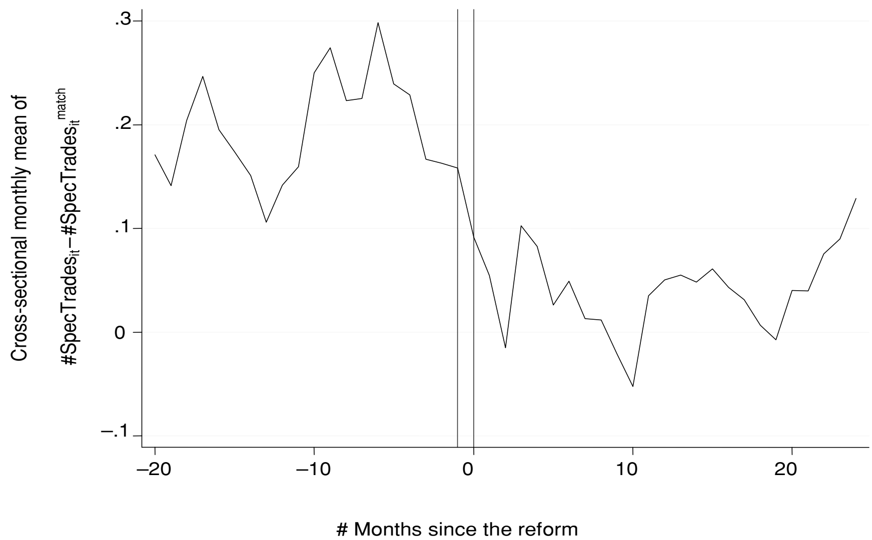



**Figure 2. Speculative trades around the reform.** This figure describes the evolution of the monthly average difference between the number of speculative trades (*#SpecTrades*) for each of the treated stocks and its match in the sample of control stocks when the match is chosen with the propensity score matching procedure. Month 0 is the month of the reform. *#SpecTrades* is the number of buy and sell trades in each month by retail investors in our sample that are unwound for at least 98 of their size before the settlement date.

Thus, if retail trading has a positive effect on idiosyncratic volatility, the reform of the RM should also result in a decrease in the idiosyncratic volatility of these stocks, as predicted by our implication 1.

To test this prediction, we proceed as explained in Section IV.B: we estimate the differences-in-differences regression (7) and the matched sample regression (8) with our proxy for volatility as a dependent variable (i.e., we set $Y_{it} = \mathit{Volatility2}_{it}$).

Column 1 of Table VI (panel A) reports the estimate for the impact of the reform on the idiosyncratic volatility of treated stocks using the differences-in-differences regression. Consistent with our implication 1, this impact is negative and significant. The drop in volatility of stocks listed on the RM relative to control stocks is about 30 basis points (that is, about a quarter of the standard deviation of the volatility of treated stocks).

In columns 2, 3, and 4 of Table VI, we report the estimate of the impact of the reform along with the matched sample estimation ($\delta_1$) for each matching procedure. The conclusions remain unchanged. In each case, we find that the reform is associated with a significant decline in the volatility of stocks listed on the RM (at the 1% level), as predicted by our implication 1. The estimate for this decline varies from 17 basis points (with the percentage difference matching approach) to 27 basis points (with the propensity score approach).

**Table VI**

## Tests of Implications 1, 2, and 3

In this table, we estimate the impact of the reform on our three main dependent variables: *Volatility2* (panel A), *Autocov* (panel B), and *Pimpact* (panel C). Appendix B provides a definition of these variables. In column 1, we estimate the following regression:

$$Y_{it} = \alpha + \beta_0 Treated_i + \beta_1 Post_t + \beta_2 Treated_i \times Post_t + \varepsilon_{it},$$

where $Y_{it}$ is the dependent variable of interest for stock $i$ in month $t$, $Post_t$ is a dummy variable equal to one after September 2000, and $Treated_i$ is equal to one if stock $i$ is listed on the RM. Coefficient $\beta_2$ is the differences-in-differences estimate of the effect of the reform on the dependent variable. In columns 2, 3, and 4, we estimate the following regression:

$$Y_{it} - Y_{it}^{match} = \alpha_i + \delta_1 Post_t + \varepsilon_{it},$$

where $Y_{it}$ is the dependent variable of interest for stock $i$ in month $t$ and $Y_{it}^{match}$ is the value of this variable for the match of stock $i$ in month $t$ in the group of control stocks. We use three different procedures to choose a match for stock $i$ in month $t$: quartile matching, percentage difference matching, and propensity score matching (see Section IV.B). Estimates of the effect of the reform ($\delta_1$) with each matching procedure are reported in columns 2, 3, and 4, respectively. The sample period starts in September 1998 and ends in September 2002. In brackets, we report $t$-statistics based on doubled-clustered errors allowing for correlation in residuals over time and across firms. Superscripts $^{*}$, $^{**}$, and $^{***}$ indicate that estimates are significantly different from zero at, respectively, the 10%, 5%, and 1% levels of significance.

| | DD (1) | Quartile Matching (2) | Percentage Difference Matching (3) | Propensity Score Matching (4) |
|---|---|---|---|---|
| Panel A: Dependent Variable: Volatility2 (Implication 1) | | | | |
| Treated×Post ($\beta_2$) | $-0.297^{***}$ | – | – | – |
| | [−5.47] | | | |
| Treated | $-0.472^{***}$ | – | – | – |
| | [−8.52] | | | |
| Post ($\delta_1$) | 0.200 | $-0.194^{***}$ | $-0.172^{***}$ | $-0.274^{***}$ |
| | [1.60] | [−2.97] | [−2.71] | [−3.25] |
| Constant | $2.877^{***}$ | $-0.227^{***}$ | $-0.192^{***}$ | $-0.238^{***}$ |
| | [30.80] | [−4.41] | [−3.31] | [−3.52] |
| Observations | 30,181 | 7,398 | 4,552 | 5,652 |
| $R^2$ | 0.06 | 0.01 | 0.01 | 0.01 |
| Panel B: Dependent Variable: Autocovariance of Returns (Implication 2) | | | | |
| Treated×Post ($\beta_2$) | $0.293^{***}$ | – | – | – |
| | [3.24] | | | |
| Treated | $0.109^{*}$ | – | – | – |
| | [1.74] | | | |
| Post ($\delta_1$) | $-0.484^{***}$ | $0.611^{***}$ | $0.329^{**}$ | $0.437^{**}$ |
| | [−5.19] | [4.06] | [2.18] | [2.26] |
| Constant | $-0.231^{***}$ | −0.137 | $-0.172^{*}$ | −0.118 |
| | [−2.81] | [−1.27] | [−1.85] | [−1.06] |
| Observations | 29,325 | 7,378 | 4,512 | 5,578 |
| $R^2$ | 0.01 | 0.02 | 0.00 | 0.01 |

(*continued*)

**Table VI**—*Continued*

| | DD (1) | Quartile Matching (2) | Percentage Difference Matching (3) | Propensity Score Matching (4) |
|---|---|---|---|---|
| Panel C. Dependent variable: Pimpact (Implication 3) | | | | |
| Treated×Post ($\beta_2$) | −4.029*** | – | – | – |
| | [−4.36] | | | |
| Treated | −8.120*** | – | – | – |
| | [−11.73] | | | |
| Post ($\delta_1$) | 4.119*** | −1.455*** | −2.087*** | −0.776* |
| | [4.47] | [−4.78] | [−3.05] | [−1.62] |
| Constant | 8.173*** | −0.630*** | −0.776*** | −0.308*** |
| | [11.81] | [−4.83] | [−4.01] | [−3.62] |
| Observations | 31,716 | 7,484 | 4,680 | 5,818 |
| $R^2$ | 0.04 | 0.04 | 0.01 | 0.00 |

Duffee (1995) shows that there is a positive relationship, at the stock level, between the volatility of stock returns and contemporaneous returns, which largely explains the so-called leverage effect (the fact that changes in volatility and lagged returns are inversely related). Duffee's (1995) finding suggests another possible explanation for our results, namely, that treated stocks experience a severe decline in prices around the suppression of the RM relative to control stocks. This is indeed a possibility because the policy change considered in this article coincides with a downturn of the French stock market, which may have been more pronounced for control stocks.

To control for this possibility, we use the return of stock $i$ in month $t$ as an additional control in our regressions. For brevity, the estimates of this regression are reported in the Internet Appendix. The estimate of the impact of the reform on volatility in this case is largely unchanged, both in terms of magnitude and statistical significance. For instance, consider the results when stocks are matched with propensity score matching. Without controlling for contemporaneous returns, we find that the reform is associated with a 27 basis point drop in volatility, with a $t$-statistic of −3.25 (column 4 of Table VI). When we control for contemporaneous returns, the drop in volatility is about 25 basis points with a $t$-statistic of −3.10. Hence, our findings are not explained by differences in the evolution of prices for control and treated stocks around the reform.

### *E. Returns Reversals and the Reform (Implication 2)*

The previous finding confirms our central hypothesis that retail trading is a source of volatility. This finding is consistent with the view that some retail investors play the role of noise traders in securities markets. If this view is correct, the reform should also have a negative impact on the absolute value of the autocovariance of stock returns for treated stocks after the reform (Implication 2).

To test this hypothesis, we use the autocovariance of daily returns for stock $i$ in month $t$ as the dependent variable in regressions (7) and (8), that is, we set $Y_{it} = Autocov_{it}$. From Table II, we know that the average autocovariance of daily returns is negative. Thus, our testable hypothesis implies that $\beta_2$ (in regression (7)) and $\delta_1$ (in regression (9)) should be significantly positive. That is, return reversals for treated stocks are smaller in absolute value after the reform.

The results are reported in panel B of Table VI. In all specifications, we find that the reform significantly reduces the size of reversals for treated stocks relative to control stocks. The differences-in-differences regression yields a point estimate of 0.29 for $\beta_2$, which means that the drop in the absolute value of $Autocov_{it}$ is about 15% of the standard deviation of this variable. The impact of the reform appears even stronger when we use the matched sample regression to measure this impact. Indeed, the point estimate for $\delta_1$ varies between 0.3 to 0.6 (15% to 30% of the standard deviation of $Autocov_{it}$). Thus, the decrease in reversals for treated stocks, following the suppression of the RM, is sizable and consistent with implication 2.

### *F. Price Impacts and the Reform (Implication 3)*

Our final prediction is that the price impact of retail order imbalances should decline for treated stocks after the suppression of the RM (implication 3). The compensation required by sophisticated investors for absorbing noise traders' net order imbalances increases with volatility. Thus, as noise trading risk is reduced, this compensation declines and prices should be less sensitive to order imbalances. Consequently, it should take more volume to move prices after the reform. To test this hypothesis, we use our proxy for price impact ($PImpact_{it}$) as the dependent variable in regressions (7) and (8).

The results are reported in panel C of Table VI. In all specifications, we find that the proxy for price impact falls after the reform. For instance, using the differences-in-differences regression, the point estimate for the impact of the reform on the price impact of trades is −4.029, a drop of about 15% of the standard deviation of $PImpact_{it}$. In general, the drop in price impact due to the reform is smaller when we use the matched sample regression but remains statistically significant, except for when we match treated and control stocks with the propensity score matching approach (the drop is then statistically significant only at the 10% level).

### *G. Robustness Checks*

*The Time Window around the Event.* We first check whether the conclusions of the analysis are affected by the length of the sample period around the reform (48 months equally distributed around the reform). Specifically, we repeat our tests of implications 1, 2, and 3 with (i) a 36-month sample period and (ii) a 24-month sample period, equally distributed around the reform. Results are available in the Internet Appendix.

In general, our findings are robust to a reduction of the estimation window. For instance, when we match treated and control stocks with propensity score matching, the point estimate for the impact of the reform on idiosyncratic volatility indicates a drop in volatility equal to 26 basis points with the 36-month sample period and 23 basis points with the 24-month sample period. These figures are very similar to the estimate obtained for the impact of the reform on volatility with the 48-month window (see column 4, panel A of Table VI).

Moreover, the effect of the reform on the autocovariance of stock returns (in absolute value) is positive (i.e., reversals are smaller after the reform) and statistically significant at the 5% level for all specifications and all time windows, with two exceptions. For the shorter time windows, the effect is significant only at the 10% level when stocks are matched using propensity score matching and insignificant when they are matched using the percentage difference matching method.

Finally, as expected, we find that the reform has a negative effect on our proxy for the price impact of trades for all time windows. In general, the effect is statistically significant, except when stocks are matched using propensity score matching.

*Liquidity Effects.* Our findings may stem from a reduction in quoted bid–ask spreads of treated stocks that is unrelated to the effect of the reform on retail trading. Indeed, a smaller bid–ask spread reduces the bid–ask bounce and therefore lowers return volatility and the absolute value of the autocovariance of stock returns (see Roll (1984)). It may also reduce the Amihud measure (our proxy for price impact) as empirically the bid–ask spread and the Amihud measure are related, albeit weakly (see Goyenko, Holden, and Trzcinka (2009)).

To check whether the reform is associated with a decline in the bid–ask spread, we use the monthly bid–ask spread for each stock as a dependent variable in our regressions. We find no effect from the reform on the quoted bid–ask spreads of treated stocks relative to control stocks (results are reported in the Internet Appendix). Hence, a reduction in the bid–ask spread of treated stocks around the reform cannot be the source of our empirical findings.

*Attrition.* The number of month-stock observations in our two groups of stocks differs before and after the reform because of delistings after September 2000 and, more importantly, because of missing observations for infrequently traded stocks in some months. This attrition is larger for stocks in the control group because they are less liquid, which could bias our inferences.[25] Hence, as a robustness check, we repeat our tests of implications 1, 2, and 3 with the differences-in-differences regression, restricting our attention to the sample of stocks with non-missing observations so that our panel is balanced. Results for this robustness check are reported in the Internet Appendix. The conclusions are similar to those obtained using the original sample.

[25] There are 5,292 (392) missing observations for stocks in the control (treated) group.

*Instrumental Variable Approach.* To directly estimate the effect of retail trading on volatility, we estimate the following IV regression:

$$Volatility2_{it} = \alpha_i + \lambda_t + \beta \times \#Trades_{it} + \varepsilon_{it}, \tag{10}$$

where we use the reform as an instrument for retail trading activity, measured as the sum of all buys and sells (normalized by the number of shares outstanding) for each stock in each month by retail investors in our sample (i.e., $\#Trades_{it} = \#Buys_{it} + \#Sells_{it}$). The results are reported in the Internet Appendix. In this regression, coefficient $\beta$ measures the effect of retail trading activity on volatility.

As expected, we obtain a significant and positive estimate for the effect of retail trading on volatility. The point estimate for beta in the "naive" OLS regression is 2.96 ($t$-statistic: 23.56). This estimate becomes 6.31 ($t$-statistic: 3.76) once retail trading activity is instrumented by the reform. Thus, ignoring the endogeneity of retail trading leads us to underestimate the true impact of retail trading activity in our sample. Of course, the IV estimate is consistent with our findings in Sections IV.C and IV.D. Indeed, the sum of (normalized) buys and sells by retail investors in our sample declines by about 0.04% after the reform (see Table V). Thus, the decline in volatility due to the effect of the reform on retail trading must be about $6.31 * 0.04\% = 0.25\%$, that is, 25 basis points. This estimate for the impact of the reform on volatility is very similar to the estimate reported with various approaches in Table VI, panel A.

One way to assess the impact of retail trading on volatility is to ask by how much volatility would drop in the absence of retail trading. In 2000, the average value of the number of buys and sells per month (normalized by the number of shares outstanding) for retail investors in our sample is about 0.09%. Thus, the IV estimate for $\beta$ implies that volatility would drop by $6.315 * 0.09\% = 0.56\%$, that is, 56 basis points, in the absence of any activity by retail traders. As treated stocks in our sample have an average daily volatility of about 240 basis points, the IV estimate indicates that retail traders contribute to about 23% of the volatility of stock returns in our sample (assuming that the effect of retail investors on volatility is linear).

The impact of retail investors on volatility may seem small. However, the theory, combined with prior empirical evidence, suggests that the impact of noise trading on volatility should not be too large to be plausible. In models of noise trading, the variance of stock returns is the sum of two components, the fundamental volatility component and the excess volatility component due to noise trading. For instance, in our extension of DSSW's (1990) model, the variance of stock returns per period can be written (see equation (A13) in the Appendix)

$$Var^k(R_{t+1}) = V_{fundamental} + V^k_{noise} \text{ for } k \in \{RM, Spot\}, \tag{11}$$

where $V_{fundamental}$ is the volatility of stock returns due to the uncertainty about dividends and information arrival while $V^k_{noise}$ is the contribution of noise trading to return volatility (it is nil when there are no noise traders). The excess volatility component is smaller in the spot market, as implied by implication 1 ($V^{Spot}_{noise} < V^{RM}_{noise}$). As $V^{Spot}_{noise} \geq 0$, it is easily seen that the largest possible difference in the volatility of stock returns between the RM and the spot market is

$$Upper\ Bound = 1 - \left(1 + \frac{V^{RM}_{noise}}{V_{fundamental}}\right)^{-\frac{1}{2}}. \tag{12}$$

Roll's (1988) empirical findings suggest that the component of idiosyncratic volatility that can be attributed to noise trading is about one-third the size of the component of volatility due to fundamental information (see Table IV in Roll (1988), two first lines).[26] Thus, the component of volatility attributed to noise trading is small compared to the component of volatility attributed to information arrival (public and private) on future cash flows. Empirical findings in French and Roll (1986), Vuolteenaho (2000), Durnev et al. (2003), and Shen (2007) point in the same direction.[27] In this case, using equation (12), one expects a moderate decline in the volatility of the stocks affected by the reform in our empirical analysis. For instance, if $V^{RM}_{noise}/V_{fundamental} = \frac{1}{3}$ (as suggested by Roll (1988)), the percentage difference in volatility between the RM and the spot market cannot be greater than 13%, in theory.

### *H. Contrarian Retail Trading and Volatility*

It is well established that, on average, retail investors are contrarian, that is, they tend to buy when prices decrease and sell when prices increase (see, for instance, Choe, Kho, and Stulz (1999), Grinblatt and Keloharju (2000), or Kaniel, Saar, and Titman (2008)). In particular, Kaniel, Saar, and Titman (2008) argue that individual investors act as liquidity providers to institutions. If this is the case, one would expect retail trading to dampen volatility. Instead, our findings suggest that retail trading has a positive effect on volatility.

In this section, we attempt to reconcile these findings, which, at first glance, appear paradoxical. One explanation is that, although momentum trades by retail investors are less prevalent, they have a stronger effect on volatility than contrarian trades. To study this hypothesis, we first need to measure the respective contributions of momentum and contrarian trades to retail trading activity. Toward this end, we introduce a new proxy for retail trading activity. Let

$$NIT_{id(t)} = \#Buys_{id(t)} - \#Sells_{id(t)} \tag{13}$$

[26] Roll (1988) decomposes a stock idiosyncratic volatility in two components: $V_x + pV_y$, where $V_x$ is the component due to noise trading and $pV_y$ is the component due to information arrival. Roll (1988) provides estimates of $V_x$, $p$, and $V_y$ using daily returns. When he includes all daily observations in his sample and adjusts returns using the CAPM, Roll (1988) obtains $\frac{V_y}{V_x} = 20.457$ and $p = 0.14393$. It follows that in this case $\frac{V_x}{pV_y} \approx \frac{1}{3}$.

[27] For instance, French and Roll (1986) estimate that, on average, between 4% and 12% of the daily return variance is due to noise trading.

be the net aggregate trade (normalized by the number of shares outstanding) by retail investors in our sample for stock $i$ on day $d(t)$ in month $t$. The correlation between $NIT_{id(t)}$ and the daily return on stock $i$ is given in panel D of Table III (last line). This correlation is negative. Thus, *on average*, individual investors in our sample tend to trade in a direction opposite to returns, that is, behave as contrarian traders as found in other empirical studies. We denote the mean of the absolute value of $NIT_{id(t)}$ in a given month by $|NIT_{it}|$ and we call it the Net Individual Trading for stock $i$ in month $t$. Thus,

$$|NIT_{it}| = \frac{1}{D_t}\sum_{d(t)}|NIT_{id(t)}|. \tag{14}$$

This variable is another way to measure the monthly retail trading activity in a stock.

To assess the respective contribution of contrarian and momentum trades to this measure of retail trading activity, we proceed as follows. Let $1_{\{R_{d(t)i}\times NIT_{id(t)}<0\}}$ be an indicator variable equal to one if the net trade for stock $i$ on day $d(t)$ by individual investors in our sample has a sign opposite to the sign of the return on this stock for this day. Similarly, let $1_{\{R_{d(t)i}\times NIT_{id(t)}>0\}}$ be an indicator variable equal to one if the net trade for stock $i$ on day $d(t)$ by individual investors in our sample has the same sign as the return on this stock for this day. We measure the contribution of contrarian trades to retail trading activity in month $t$ in stock $i$ by

$$CONT_{it} = \frac{1}{D_t}\sum_{d(t)}|NIT_{id(t)}| \times 1_{\{R_{d(t)i}\times NIT_{id(t)}<0\}}, \tag{15}$$

where $D_t$ is the number of days in month $t$. Similarly, we measure the contribution of momentum trades to retail trading activity in month $t$ in stock $i$ by

$$MOM_{it} = \frac{1}{D_t}\sum_{d(t)}|NIT_{id(T)}| \times 1_{\{R_{d(t)i}\times NIT_{id(t)}>0\}}. \tag{16}$$

We use $CONT_{it}$ as a proxy for contrarian retail trading activity and $MOM_{it}$ as a proxy for momentum retail trading activity. Summary statistics for these variables are provided in panel D of Table III. [28]

Ideally, we would like to estimate the following equation

$$Volatility2_{it} = \theta_1 MOM_{it} + \theta_2 CONT_{it} + \varepsilon_{it}, \tag{17}$$

where $Volatility2_{it}$ is the standard deviation of the daily excess return for stock $i$ in month $t$. Unfortunately, in the absence of one instrumental variable for $MOM_{it}$ on the one hand and $CONT_{it}$ on the other hand, we cannot separately

[28] In the absence of days with zero returns, we would have $|NIT_{it}| = MOM_{it} + CON_{it}$. In general, however, this equality does not hold perfectly, as there are days with zero returns, for which trades are classified neither as contrarian nor as momentum trades.

**Table VII**

**The Impact of the Reform on Contrarian Retail Trading Activity**

In this table, we estimate the impact of the reform on the contrarian component and the momentum component of retail trading activity. We measure retail trading activity by $|NIT_{it}|$, the contrarian component of retail trading activity by $CONT_{it}$, and the momentum component of retail trading activity by $MOM_{it}$ (see Appendix B for the definition of these vaariables). In column 1, we estimate the following regression:

$$Y_{it} = \alpha + \beta_0 Treated_i + \beta_1 Post_t + \beta_2 Treated_i \times Post_t + \varepsilon_{it},$$

where $Y_{it}$ is one of the measures of retail activity for stock $i$ in month $t$, $Post_t$ is a dummy variable equal to one after September 2000, and $Treated_i$ is equal to one if stock $i$ is listed on the RM. In columns 2, 3, and 4, we estimate the following regression:

$$Y_{it} - Y_{it}^{match} = \alpha_i + \delta_1 Post_t + \varepsilon_{it},$$

where $Y_{it}$ is one of the measures of retail activity for stock $i$ in month $t$ and $Y_{it}^{match}$ is the value of this measure for the match of stock $i$ in month $t$ in the group of control stocks. We use three different procedures (see text) to choose a match for stock $i$ in month $t$: quartile matching, percentage difference matching, and propensity score matching. Estimates of the effect of the reform ($\delta_1$ ) with each matching procedure are reported in columns 2, 3, and 4, respectively. In panel A, $Y_{it} = |NIT_{it}|$; in panel B, $Y_{it} = MOM_{it}$; and in panel C, $Y_{it} = CON_{it}$. The sample period starts in January 1999 and ends in September 2002. In brackets, we report $t$-statistics based on doubled-clustered errors allowing for correlation in residuals over time and across firms. Superscripts *** indicates that estimates are significantly different from zero at the 1% level of significance.

| | DD | Quartile Matching | Percentage Difference Matching | Propensity Score Matching |
|---|---|---|---|---|
| Panel A: Dependent Variable: $\|NIT_{it}\|(\times 100)$ | | | | |
| Treated×Post ($\beta_2$) | −0.070*** | – | – | – |
| | [−4.59] | | | |
| Treated | 0.003 | – | – | – |
| | [0.15] | | | |
| Post | −0.044*** | −0.099*** | −0.097*** | −0.087*** |
| | [−2.89] | [−8.01] | [−6.88] | [−6.83] |
| Constant | 0.200 | 0.122 | 0.101 | 0.121 |
| | [12.92] | [8.14] | [5.50] | [5.18] |
| Observations | 32,808 | 6,931 | 4,364 | 5,441 |
| $R^2$ | 0.02 | 0.07 | 0.04 | 0.02 |
| Panel B: Dependent Variable: $MOM_{it}(\times 100)$ | | | | |
| Treated× Post ($\beta_2$) | −0.021*** | – | – | – |
| | [−2.51] | | | |
| Treated | −0.054*** | – | – | – |
| | [−5.44] | | | |
| Post ($\delta_1$) | −0.025*** | −0.041*** | −0.044*** | −0.029*** |
| | [−2.57] | [−7.54] | [−7.07] | [−4.95] |
| Constant | 0.138*** | 0.044*** | 0.041*** | 0.041*** |
| | [14.33] | [6.68] | [5.03] | [3.84] |
| Observations | 22,923 | 6,573 | 3,799 | 4,342 |
| $R^2$ | 0.05 | 0.05 | 0.03 | 0.01 |

(*continued*)

**Table VII**—*Continued*

| | DD | Quartile Matching | Percentage Difference Matching | Propensity Score Matching |
|---|---|---|---|---|
| | Panel C: Dependent Variable: $CON_{it}(\times 100)$ | | | |
| Treated× Post ($\beta_2$) | −0.045*** | – | – | – |
| | [−4.67] | | | |
| Treated | −0.029*** | – | – | – |
| | [−2.54] | | | |
| Post ($\delta_1$) | −0.023*** | −0.061*** | −0.056*** | −0.054*** |
| | [−2.63] | [−7.60] | [−6.10] | [−6.21] |
| Constant | 0.152*** | 0.071*** | 0.058*** | 0.073*** |
| | [16.98] | [7.98] | [5.45] | [5.47] |
| Observations | 23,257 | 6,592 | 3,879 | 4,452 |
| $R^2$ | 0.04 | 0.07 | 0.04 | 0.02 |

identify the causal effect of contrarian retail trading activity ($\theta_1$) and momentum retail trading activity ($\theta_2$) on volatility. A simple OLS estimation (with stock and time fixed effects) of equation (12) is reported in the Internet Appendix. For all specifications, we find a strong positive relationship between volatility and both components of retail trading activity. In particular, the contrarian component is positively related to volatility. Although this finding does not support the view that retail investors following contrarian strategies dampen volatility, reverse causality remains a possibility as sophisticated retail investors may enter the market and act as liquidity providers only in periods of high volatility (perhaps because high volatility signals that the market lacks liquidity).[29]

Thus, we take an indirect approach by estimating the impact of the reform on $MOM_{it}$ and $CONT_{it}$ with the methodology described in Section IV.C. Table VII reports the results. We find that the reform has a negative and significant impact on both components of retail trading activity for the stocks listed on the RM relative to other stocks. But the drop in contrarian retail trading activity is *significantly higher* than the drop in momentum retail trading activity. For instance, when stocks are matched with the propensity score matching method, the impact of the reform on $CON_{it}$ is twice as large as the impact on $MOM_{it}$.[30] Because the reform is associated with a drop in volatility, this leaves us with two possibilities: either $\theta_2$ is positive, that is, contrarian retail investors also enhance volatility, or $\theta_2$ is negative but much smaller in absolute value than $\theta_1$. In this scenario, retail investors following contrarian strategies dampen

[29] Another possibility is that sophisticated investors pick off stale limit orders placed by retail investors when new information arrives (see Linnainmaa (2010) for evidence consistent with this scenario). If this happens more frequently for stocks with high volatility, retail contrarian trades will tend to be positively correlated with volatility.

[30] The difference in the point estimates for the impact of the reform on $CON_{it}$ and $MOM_{it}$ are significant at the 1% level in all specifications, except when stocks are matched using percentage difference matching.

volatility, but their negative effect on volatility is more than outweighed by the effect of retail investors following momentum strategies. This scenario is more intuitive, but the first scenario, in which contrarian trades also work to increase volatility, is also plausible, as discussed in the conclusion.

## V. Conclusion

In this paper, we use a reform of the French stock market to assess the effect of retail investors on volatility. This reform makes trading relatively more costly for retail investors in a subset of listed stocks and triggers a drop in retail trading for these stocks relative to stocks unaffected by the reform. This gives us a way to identify the effect of retail trading on volatility.

We find that the volatility of the stocks affected by the reform declines after the implementation of the reform, relative to other stocks, which means that the effect of retail trading on volatility is positive. We argue that this positive effect is consistent with the view that some retail investors behave as noise traders. In support of this claim, we show that the reform also triggers a drop in the size of price reversals and the price impact of trades for the stocks affected by the reform. All these observations are predicted by models of noise trading.

One must be careful in interpreting these findings: they are consistent with the view that some retail investors play the role of noise traders but they do not imply that *all* retail investors are noise traders or that only retail investors are noise traders. Moreover, we do not identify the drivers of retail trades (misperception of future payoffs, risk aversion, or hedging needs). Thus, our findings should not be construed as evidence that retail investors are irrational traders.

Our findings also raise new questions. The literature on retail investors predominantly finds that these investors follow contrarian strategies, on average. We use our data on retail investors to measure the contribution of contrarian and momentum trades to retail trading activity. The reform has a more negative impact on contrarian trades. This observation can be reconciled with our finding regarding volatility in one of two ways: either retail contrarian trades dampen volatility but their stabilizing effect is smaller than the destabilizing effect of retail momentum trades, or retail contrarian trades also have a positive effect on volatility. Both stories are plausible. The first story is consistent with Kaniel, Saar, and Titman (2008), who argue that retail investors act as liquidity providers. The second story is consistent with Bloomfield, O'Hara, and Saar (2009). They consider an experiment in which some participants have no specific reason to trade and have no information. Instead of staying put, these agents trade and realize losses. Interestingly, they use contrarian trading strategies and contribute to mispricing by slowing down price adjustments to true values.[31] Thus, trades by these agents add noise to prices and may

[31] There might be several reasons why noise traders may appear to act as contrarian investors. For instance, they may be prone to behavioral biases such as the disposition effect or they may not realize that their limit orders are more likely to execute in the case of adverse price movements.

therefore amplify volatility. Our quasi-experiment cannot tell which story is correct. To do so, in keeping with the spirit of our study, one would need to find a separate instrument for contrarian retail trades and momentum retail trades. We leave this question to future research.

## Appendix A: Derivations of the Testable Implications in DSSW (1990)

In this appendix, we derive implications 1, 2, and 3 in the extension of DSSW (1990) described in Section II. Given the assumptions of the model, each investor $j \in \{N, S\}$ at date $t$ chooses his or her portfolio to maximize

$$E_t U_j \equiv E_t(W_{jt+1}) - \frac{\gamma}{2} Var_t(W_{jt+1}), \tag{A1}$$

where $W_{jt+1}$ is the wealth of investor $j$ at date $t+1$. That is,

$$W_{jt+1} = (1+r)n_{jt} + (p_{t+1} + d_{t+1} - (1+r)p_t)X_{jt} - G_j(X_{jt}), \tag{A2}$$

where (i) $n_{jt}$ and $X_{jt}$ are, respectively, the endowment in the riskless security and the position in the stock for investor $j$ at date $t$, (ii) $r > 0$ is the rate of return on the riskless security (in unlimited supply), and (iii) $G_j(X_{jt}) = \frac{c_j^k X_{jt}^2}{2}$ is the cost of taking position $X_{jt}$ for investor $j$ in market structure $k \in \{RM, Spot\}$. In the RM, $c_N^{RM} = c_S^{RM} = 0$ as in DSSW (1990). In the spot market, noise traders bear a higher trading cost because $c_N^{Spot} > 0$ while sophisticated investors' trading cost is unchanged ($c_S^{Spot} = 0$). In market structure $k$, we denote the expectation and the variance of the stock price at date $t+1$, conditional on the information available at date $t$, by $E_t^k\,(p_{t+1})$ and $\sigma_k^2$, respectively.

*Stationary Equilibrium.* Investors' demand functions at date $t$ are

$$X_{St}(p_t) = \frac{E_t(R_{t+1})}{\gamma\left(\sigma_k^2 + \sigma_\xi^2\right)}, \tag{A3}$$

$$X_{Nt}(p_t) = \frac{E_t(R_{t+1}) + \rho_t}{c_N^k + \gamma\left(\sigma_k^2 + \sigma_\xi^2\right)}. \tag{A4}$$

The clearing condition at date $t$ imposes $X_{St}(p_t) + X_{Nt}(p_t) = 1$. Thus,

$$p_t = \frac{1}{1+r}\{E_{kt}(d_{t+1} + p_{t+1}) + \varphi_k(1+r)\rho_t - \theta_k\}, \tag{A5}$$

with

$$\varphi_k = \left(\frac{\mu(1+r)^{-1}}{c_N^k + \gamma\left(\sigma_k^2 + \sigma_\xi^2\right)}\right)\theta_k, \tag{A6}$$

$$\theta_k = \left(\frac{1-\mu}{\gamma\left(\sigma_k^2 + \sigma_\xi^2\right)} + \frac{\mu}{c_N^k + \gamma\left(\sigma_k^2 + \sigma_\xi^2\right)}\right)^{-1}. \tag{A7}$$

The stationary solution for equation (A5) is

$$p_t = \frac{\bar{d}}{r} - \frac{\theta_k}{r} + \varphi_k \rho_t. \tag{A8}$$

On average, the stock price, $\frac{\bar{d}}{r} - \frac{\theta_k}{r}$, is the discounted value of the average dividend ($\frac{\bar{d}}{r}$) adjusted for risk ($\frac{\theta_k}{r}$). The stock price fluctuates randomly around its average level because noise traders' sentiment is a source of price pressures (last term in equation (A8)). For instance, when noise traders are pessimistic ($\rho_t < 0$), they decrease their holdings of the stock. The stock price then decreases to induce sophisticated investors to increase their holdings of the stock.

*Existence of a Stationary Equilibrium.* Using equation (A8), we deduce that the variance of the stock price conditional on information at date $t$ ($Var_t^k\,(p_{t+1})$) is

$$\sigma_k^2 \equiv Var_t^k(p_{t+1}) = \varphi_k^2 \times \sigma_\rho^2 \text{ for } k \in \{RM, Spot\}. \tag{A9}$$

The volatility of the stock price in equilibrium and $\varphi_k$ are solutions of the system of equations (A6) and (A9). Thus, the number of stationary equilibria is determined by the number of solutions to this system, which always has at least one equation. To show this, let us define

$$g(x) = \frac{\dfrac{\mu}{c_N^k + \gamma\left(x + \sigma_\xi^2\right)}}{\dfrac{1-\mu}{\gamma\left(x + \sigma_\xi^2\right)} + \dfrac{\mu}{c_N^k + \gamma\left(x + \sigma_\xi^2\right)}}(1+r)^{-1}, \tag{A10}$$

$$f(x) = \sqrt{\frac{x}{\sigma_\rho^2}}, \tag{A11}$$

$$F(x) = f(x) - g(x). \tag{A12}$$

It is immediately obvious that the equilibrium level of volatility, $\sigma_k^2$, is the (positive) solution of $F(\sigma_k^2) = 0$. Now we observe that $F(0) \leq 0$ and $F((1 + r)^{-2}\,\sigma_\rho^2) > 0$. As $F(.)$ is continuous, we deduce that there is at least one value of $x \in [0, (1+r)^{-2}\sigma_\rho^2)$ such that $F(x) = 0$. Thus, there exists at least one stationary equilibrium. The equilibrium is unique in the RM, whereas there might be two stationary equilibria with differing levels of volatility in the spot market. Our predictions, however, are independent of the equilibrium considered in this case (see below).

*Testable Implications.* Recall that the excess return from date $t$ to date $t+1$ is defined as $R_{t+1} = d_{t+1} + p_{t+1} - (1+r)p_t$. Using equation (A5), we deduce that in equilibrium the unconditional variance of excess returns is

$$Var^k(R_{t+1}) = V_{fundamental} + V^k_{noise} \text{ for } k \in \{RM, Spot\}, \tag{A13}$$

with $V_{fundamental} = \sigma^2_\xi$ and $V^k_{noise} = \varphi^2_k \left((1+r)^2 + 1\right)\sigma^2_\rho$. The variable $V^k_{noise}$ is the contribution of noise traders to volatility because it vanishes when $\mu = 0$ ($\varphi_k = 0$ iff $\mu = 0$). For $\mu > 0$, using the expression for $\varphi_k$ given in equation (A6), we deduce

$$\frac{\varphi_{RM}}{\varphi_{Spot}} = \left(\frac{c_N^{Spot} + \gamma\left(\sigma^2_{Spot} + \sigma^2_\xi\right)}{(\gamma\left(\sigma^2_{Spot} + \sigma^2_\xi\right)}\right) > 1. \tag{A14}$$

Thus, using equation (A5), the unconditional variance of excess returns is higher in the RM, which is our implication 1. Using equation (A13) and the definition of stock returns, we deduce that the autocovariance of stock returns is

$$Cov(R_{t+1}, R_t) = -\varphi^2_k(1+r)\sigma^2_\rho \text{ for } k \in \{RM, Spot\}. \tag{A15}$$

Thus, the autocovariance of stock returns is higher in *absolute value* in the RM because $\varphi_{RM} > \varphi_{Spot}$. This is our second testable hypothesis (implication 2).

Finally, let $\Delta X_{N_t} \stackrel{def}{=} \mu(X_{Nt+1}(p_{t+1}) - X_{Nt}(p_t))$ be the net change in noise traders' holdings from date $t$ to date $t+1$. Using equations (A4) and (A8), we obtain that

$$\Delta X_{N_t} = \left(\frac{\mu(1 - \varphi_k(1+r))}{c^k_N + \gamma(Var^k_t(p_{t+1}) + \sigma^2_\xi)}\right)(\rho_{t+1} - \rho_t). \tag{A16}$$

Using equations (A6) and (A8), after some manipulations we can rewrite the change in price between dates $t$ and date $t+1$ as

$$p_{t+1} - p_t = \lambda_k \times \Delta X_{N_t} \tag{A17}$$

with

$$\lambda_k = \gamma((1-\mu)(1+r))^{-1} Var^k_t(R_{t+1}). \tag{A18}$$

As $Var^{RM}_t(R_{t+1}) > Var^{Spot}_t(R_{t+1})$, we have that $\lambda_{RM} > \lambda_{Spot}$ (implication 3).

## Appendix B: Definitions of the Variables

In this appendix, we define the variables that we use in our empirical tests (Sections III and IV).

*Turnover* is the ratio of the number of shares traded per month to the number of shares outstanding.

*Volatility1* is the monthly standard deviation of daily raw returns.

*Volatility2* is the monthly standard deviation of the daily difference between the raw return and the market return.

*Volatility3* is the monthly standard deviation of the residual of the time-series regression of the daily excess return for a stock on the daily excess market return.

*Autocov* is the monthly aucovariance of the daily returns of a stock.

*Pimpact* is the monthly average of the ratio of the absolute value of the daily return for a stock to its daily trading volume.

*#Buys* is the number of shares purchased sold normalized by the number of shares outstanding for the retail investors in our sample.

*#Sells* is the number of shares purchased sold normalized by the number of shares outstanding for the retail investors in our sample.

*#Trades* is the total number of shares purchased and sold normalized by the number of shares outstanding for the retail investors in our sample.

*#SpecTrades* is the number of buy and sell trades in each month by retail investors in our sample that are unwound for at least 98 of their size before the end of the month.

$|NIT_{it}| = \frac{1}{D_t}\sum_{d(t)}\left|NIT_{id(t)}\right|$, where $NIT_{id(t)} = \#\text{Buys}_{id(t)} - \#\text{Sells}_{id(t)}$ is the net aggregate trade by retail investors in our sample for stock $i$ on day $d(t)$ in month $t$ (normalized by the number of shares outstanding) and $D_t$ is the number of trading days in month $t$.

$CONT_{it} = \frac{1}{D_t}\sum_{d(t)}\left|NIT_{id(t)}\right| \times 1_{\{R_{d(t)i} \times NIT_{id(t)} < 0\}}$ where $1_{\{R_{d(t)i} \times NIT_{id(t)} < 0\}}$ is an indicator variable equal to one if the net trade for stock $i$ on day $d(t)$ by individual investors in our sample has a sign opposite to the sign of the return on this stock for this day.

$MOM_{it} = \frac{1}{D_t}\sum_{d(t)}\left|NIT_{id(t)}\right| \times 1_{\{R_{d(t)i} \times NIT_{id(t)} > 0\}}$, where $1_{\{R_{d(t)i} \times NIT_{id(t)} > 0\}}$ is an indicator variable equal to one if the net trade for stock $i$ on day $d(t)$ by individual investors in our sample has the same sign as the return on this stock for this day.

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