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ABSTRACT

Individual Investors and Volatility*

We test the hypothesis that individual investors contribute to the idiosyncratic volatility of stock returns because they act as noise traders. To this end, we consider a reform that makes short selling or buying on margin more expensive for retail investors relative to institutions, for a subset of French stocks. If retail investors are noise traders, theory implies that the volatility of stocks affected by the reform should decrease relative to other stocks. This prediction is borne out by the data. Moreover, around the reform, we observe a significant decrease in (i) the magnitude of returns reversals, and (ii) the Amihud ratio for the stocks affected by the reform relative to other stocks. We show that these findings are also consistent with models in which individual investors, acting as noise traders, are a source of volatility.

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1 Introduction

Individual investors are often viewed as noise traders. That is, investors who trade for reasons other than fundamental information, including mis-perceptions of future returns or liquidity needs. Recent empirical findings support this view. For instance, using small-trade volume as a proxy for retail trading, Barber et al.(2006) and Hvidkjaer (2008) show that stocks heavily purchased by individuals underperform stocks heavily sold by individuals. Grinblatt and Keloharju (2000) or Frazzini and Lamont (2008) find that individual investors systematically lose money to institutional investors. Derrien (2007) show that French IPOs in high demand by individuals are more likely to underperform.¹

In models such as DeLong et al.(1990), Campbell et al.(1993), Campbell and Kyle (1993) or Llorente et al.(2002), limited risk bearing capacity prevents sophisticated investors (e.g., liquidity providers or arbitrageurs) from fully eliminating price pressures due to noise traders. Thus, shifts in noise traders' demand for a stock contributes to its return volatility. According to this logic, retail trading should have a positive effect on volatility if retail investors are noise traders. This hypothesis provides one possible explanation for episodes of high idiosyncratic volatility (see Brandt et al.(2005)). Yet, it has received little attention empirically. One difficulty is to control for factors that simultaneously affect a stock return volatility and retail trading in this stock. In this article, we exploit a change in the organization of the French stock market to overcome this difficulty.

Until 2000, the French stock market was a two-tier market. Stocks with high turnover were traded exclusively on a forward market with monthly settlement, whereas less actively traded stocks were traded spot. The forward market was suppressed in September 2000 to align settlement procedures of the Paris Bourse with those used in other equity markets. For individual investors, speculation in the forward market was much easier than in the spot market, as they could short stocks listed on the forward market or leverage long positions in these stocks, at virtually no costs (see Biais et al. (2000)). Therefore, after the reform, speculation on stocks previously listed on the forward market became more costly to individuals relative to institutions.² As the reform applies only to a subset of stocks, it provides us with an opportunity to identify the impact of retail investors on volatility, while controlling for other unobservable factors that might affect volatility.

A trading restraint on noise traders lowers their ability to weight on prices and, thereby, it should reduce return volatility. We formally derive this implication by introducing differential trading restraints in DeLong et al.(1990)'s model of noise trading. Thus, if

¹See also, for instance, Barber and Odean (2000) and Barber and Odean (2002).

²We give a more detailed account of the reform in Section 2.

retail investors are noise traders, we expect a decline in the volatility of stocks listed on the French forward market after its suppression. This effect could be difficult to detect if only a small fraction of French individuals participate to the stock market. But, as of 1998, 15% (resp. 19%) of French (resp. U.S.) households own stocks directly (see Guiso et al.(2003)). Hence, compared to the U.S., the level of stock market participation by individuals is not low in France.

The model yields two auxiliary predictions. First, noise trading is a source of serial dependence in stock returns. Thus, when noise traders face a more stringent trading restraint, the autocovariance of returns becomes smaller in absolute value. Second, in this case, the price impact of noise traders' net order imbalances (the net change in their holdings) decreases as well. Indeed, a more stringent trading restraint for noise traders reduces noise trading risk, and thereby it induces sophisticated investors to counteract more the price pressures due to noise trading. Accordingly, the impact of trades on prices should be smaller after the suppression of the forward market for stocks previously listed on this market.

We test these predictions using four years of data around the reform of the French forward equity market. Stocks that trade on the spot market throughout our sample period provides a useful counter-factual for the evolution of our dependent variables (volatility, autocovariance of stock returns etc. . .) both before and after the reform. Thus, using these stocks as control, we identify the impact of the reform on volatility (and other variables of interest) with "differences-in-differences" estimates.

We first check that the reform is associated with a drop in retail trading since it is a premise of our analysis. We use the fraction of small trades as a proxy for retail trading as is common in the literature (e.g., Barber et al.(2006) and Hvidkjaer (2008)). As expected, we find that the daily fraction of small trades in stocks affected by the reform declines significantly after the reform whereas it does not change for other stocks. Moreover, the daily fraction of very large trades (which usually are carried out by institutions) enlarges significantly for the stocks affected by the reform. Overall, this shift in the distribution of trade sizes is consistent with a decline in the ratio of retail to institutional trading for the stocks affected by the reform.

We then study the effect of the reform on various measures of stock return volatility. As predicted, we find that the reform of the forward equity market is associated with a statistically significant reduction in the volatility of daily returns for stocks traded on this market relative to other stocks. This reduction varies from eleven to twenty-six basis points (depending on the estimation method), and represents about 11% to 34% of the standard deviation of the pre-reform volatility of daily returns.

The auxiliary predictions are also supported by the data. We find that stock returns in our sample tend to reverse themselves. But, the size of these reversals drops for firms listed on the forward market after the suppression of this market. Moreover, in line with the model, trades in stocks listed on the forward market have less impact on prices after the reform. Indeed, the reform is associated with a significant decline in the Amihud ratio (the ratio of absolute returns to contemporaneous trading volume) for stocks affected by the reform relative to other stocks. Overall, these findings provide additional support for our interpretation of the link between volatility and retail trading.

We perform several robustness checks of these findings. One problem is that stocks traded on the spot market have different characteristics (market capitalization and turnover) than stocks listed on the forward market. However, our findings persist when we use a sub-sample of control stocks that are more comparable to the stocks listed on the forward market. Another concern is that the link between the reform and volatility is spurious. To address this issue, we generate placebo "reforms" before the actual reform (in 1996 and 1997). In this way, we obtain an empirical distribution of the coefficients measuring its effect on the various variables of interest, under the null of no effect. The point estimate for the effect of the actual reform on volatility is much greater (in absolute value) than that obtained in the placebo treatments, which suggests that our findings are not spurious.

Our paper is most related to Brandt et al.(2005). They show that the rise in idiosyncratic volatility of U.S. stocks documented by Campbell et al.(2001) during the 1962-1997 period reverses itself by 2007. They argue that surges in volatility are explained by episodes of speculation by retail investors. In support of this hypothesis, they find empirically that idiosyncratic volatility patterns are more pronounced for stocks with higher participation of retail investors (in particular low price stocks). Our approach offers another way to study the effect of retail trading on volatility. Our analysis is also related to the growing literature about the effect of individual investors on price movements (e.g., Lee et al.(2004), Andrade et al.(2008), Barber et al.(2006), Hvidkjaer (2008), Kaniel et al. (2008)). This literature however focuses on the dynamic relationships between returns and measures of individual investors' signed order imbalances. In contrast, our goal is to establish a causal link between retail trading and stock returns volatility.

Our study finally relates, indirectly, to empirical evaluations of the effect of a securities transaction tax on volatility. Indeed, advocates of a securities transaction tax often argue that it would reduce excess volatility by discouraging noise trading. Empirical evidence supporting this claim are either inconclusive (Roll (1989)) or even suggest that transaction taxes increase volatility (Umlauf (1993) and Jones and Seguin (1997)). A potential explanation for these findings is that a securities transaction tax restrains trades both by

noise traders *and* arbitrageurs, as pointed out in Schwert and Seguin (1993). Thus, its overall impact on volatility is ambiguous, and could even be positive.³ In contrast, the reform considered in this paper primarily affects individual investors. Thus, if individual investors are noise traders, this reform potentially constitutes a better test of the idea that "chaining up" noise traders reduces volatility.

In our model, we assume that noise traders' demands shift over time because their misperception of future returns (investors' sentiment) fluctuates. It is worth stressing that our predictions hold more generally when the demand of one class of investors ("noise traders") shifts for non informational reasons such as changes in risk aversion (as in Campbell et al.(1993)) or in non-tradable endowments (as in Llorente et al.(2002)). In this paper, we are only interested in the effect of these shifts on volatility, not in the source of these shifts *per se*. For policy-making, identifying the origin of noise trading (hedging need vs. erroneous beliefs) is important but this issue is beyond the scope of our paper.

The remainder of the paper is organized as follows. In the next section we describe the French forward equity market and its reform in 2000. In Section 3, we develop testable implications. We present our empirical findings and various robustness checks in Section 4. Section 5 concludes. The appendix contains the proofs of the claims in Section 3, the tables, and the figures.

2 The French forward equity market

The French forward equity market dates from the 19th century. Originally, it was an OTC market, operating in parallel with the official market. It became an official segment of the Paris Bourse in 1885. Stocks traded on this segment were also traded on a spot market. This organization changed in 1983 when the Paris Bourse introduced two distinct segments in its list: (i) "Le Marché au Comptant" with only spot transactions, and (ii) "Le Marché à Règlement Mensuel" (henceforth RM) with only forward transactions.⁴ Transactions on the RM were settled at the end of every month, with a possibility for investors to roll-over their position from one settlement date to the next (see Solnik (1990)). Stocks listed on the RM were selected by the Paris Bourse on the basis of their trading volume. They were typically larger and more active than those listed on the spot market.

³In fact, theoretical analysis of the effect of a uniform securities transaction tax on volatility (Kupiec (1996), Song and Zhang (2005)) predict ambiguous effects. Bloomfield, O'Hara and Saar (2007) experimentally observe that a securities transaction tax reduces, roughly equally, trades by informed traders and noise traders. As a result, they find that pricing errors are not affected by a securities transaction tax.

⁴Buyers (resp. sellers) for stocks listed on the RM could demand immediate delivery (payment) of the stock but they had to pay a fee for this service equal to 1% of the value of the transaction.

The RM was suppressed on September 25, 2000 and, on this occasion, the settlement date for spot transactions was also reduced from five to three days. We refer to this event as the “reform.” The goal of the reform was to harmonize the settlement procedures of the French stock market with international practices.

A major difference between the RM and the spot market was the ease with which individual investors could leverage their positions on the RM (see Biais et al. (2000)). Investors could sell a stock listed on the RM without owning the stock at the time of the transaction or buy stocks on margin. They just had to put a cash deposit equal to 20% of the value of their transaction on the RM (both for sales and purchases).⁵ They could also unwind their positions before the settlement date by entering into a trade in the direction opposite to their initial transaction. Last, at the settlement date, they could postpone delivery or acquisition of the stock by rolling over their position to the next settlement date.⁶ These features of the RM were especially attractive for retail investors as it is difficult for them to short sell stocks listed on the spot market or to buy these stocks on margin. For instance, Biais et al.(2000) note that (p.397):“*The monthly settlement system enables traders who do not own the stock to engage in sales. Consequently, it enables traders 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 (especially on-line brokers) voiced concerns that the suppression of the RM would reduce the trading activity of retail investors.⁷ In response, the Paris Bourse encouraged brokers to offer a new service, called the “Service de Règlement Différé” (henceforth SRD).⁸ For stocks eligible to this service, 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 one hundred shares of Alcatel, a French stock eligible to the SRD. This investor must contact a broker accepting orders with *deferred* execution. In this case, the broker sells one hundred shares on the spot market on behalf of the investor and effectively acts

⁵This deposit could also be made in securities. In this case, the value of the deposit had to be equal to at least 40% of the value the transaction.

⁶Practically, to roll over their position, investors had to close their initial position at the settlement price and then to reopen a new position at this price. Thus, sellers rolling over their position were receiving or paying at the end of the month the difference between the price at which they initially established their position and the settlement price. The treatment of buyers was symmetric.

⁷See for instance “*Paris Bourse Will Require Web Brokers To Advance Clients Funds for Trading,*” Wall Street Journal, Eastern Edition; 07/24/2000.

⁸On 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.

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 one hundred 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 the Paris Bourse. Most stocks listed on the RM in September 2000 became afterward eligible to the SRD.

Even for stocks eligible to the SRD, the cost of short selling or buying on margin is significantly higher after the reform as brokers charge a fee for this service, and can set margin levels higher than those prevalent on the RM. Moreover, few brokers initially decided to offer this service.⁹ Overall, for individual investors, the SRD is at best an imperfect substitute to the RM. For instance, on October 6, 2000, the French newspaper "Les Echos" pointed out 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.*"¹⁰

To sum up, the suppression of the RM makes trading relatively more costly for individuals than for institutions. Thus, this reform provides an interesting opportunity to identify the effect of retail trading on volatility.

3 Testable predictions

Theories of noise trading imply that shifts in noise traders' demands are a source of volatility. Thus, if retail trading has a positive effect on volatility because individuals are noise traders, then the suppression of the RM should reduce volatility. In this section, we derive this prediction in DeLong et al.(1990)'s model. We also derive other effects that we should observe around the suppression of the RM if retail traders affect volatility because they act as noise traders.

In the model, shifts in noise traders' demands are due to fluctuations in their misperception of future payoffs. But the predictions can be obtained with other causes for noise trading. For instance, we have checked that all the results of this section are unchanged if noise traders have correct expectations but trade to hedge non-tradable endowments, as in Llorente et al.(2002). In this case, noise traders' non-tradable endowment (per capita), adjusted for its correlation with the payoff of the stock, plays the role of parameter ρ_t (noise traders' sentiment) in the model below. Proofs of the results in this section are in Appendix.

⁹Retail investors trade in small sizes. Thus, provision of this service to retail investors is cumbersome for brokers since the costs of financing loans or borrowing stocks are in part fixed.

¹⁰See "*SRD contre RM-quels avantages.*" Les Echos, October 6, 2000.

In contrast to DeLong et al.(1990), we apply the model to individual stocks since our empirical analysis focuses on the effect of the reform on individual stocks volatility. Thus, sophisticated investors in our model are exposed to idiosyncratic risk. Several empirical papers show that idiosyncratic risk is indeed a limit to arbitrage (e.g., Scruggs (2007), Baker and Savasoglu (2002), Pontiff (2006)).

3.1 Model

Overlapping generations of investors trade two securities, a riskless asset (a “bond”) and a stock, at dates $t = 0, 1, 2, \dots$. The riskless asset is in unlimited supply and each dollar invested in this asset returns $(1 + r)$, with $r > 0$. The net supply (per capita) of the stock is normalized to one share. At date t , the stock pays a dividend d_t such that

$$d_t = \bar{d} + \beta(d_{t-1} - \bar{d}) + \xi_t, \quad (1)$$

with $0 \leq \beta \leq 1$.¹¹ Innovations in dividends (ξ) are i.i.d, normally distributed with mean 0 and variance σ_ξ^2 . The ex-dividend stock price at date t is denoted p_t . 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 γ . Thus, each investor k at date t chooses his or her portfolio to maximize

$$E_t U_k \equiv E_t(W_{kt+1}) - \frac{\gamma}{2} Var_t(W_{kt+1}), \quad (2)$$

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

$$W_{kt+1} = (1 + r)n_{kt} + (p_{t+1} + d_{t+1} - (1 + r)p_t)X_{kt} - G_k(X_{kt}), \quad (3)$$

where (i) n_{kt} and X_{kt} are respectively the endowment in the bond and the position in the stock for investor k at date t , and (ii) $G_k(X_{kt})$ is the cost of taking a position X_{kt} for investor k (more on this below). We denote the expectation and the variance of the stock price at date $t + 1$, conditional on the information available at date t , by $E_t(p_{t+1})$ and $Var_t(p_{t+1})$, respectively.

There are two groups of investors, noise traders (N) and sophisticated investors (S), with relative population weights μ and $(1 - \mu)$. Moreover, $\mu < 1$. 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 $E_t(p_{t+1}) + \rho_t$. Parameter

¹¹Campbell, Grossman and Wang (1993), for instance, use a similar specification for the dividend process. In DeLong et al.(1990), the dividend is constant over time.

ρ_t is an index of noise traders' sentiment. It varies over time according to the following process

$$\rho_{t+1} = \alpha\rho_t + \varepsilon_{t+1}, \quad (4)$$

with $0 \leq \alpha < 1$. Innovations in sentiment (ε) are i.i.d, normally distributed with mean 0 and variance σ_ξ^2 , and they are independent from innovations in dividends.¹²

The cost of establishing a position can differ between sophisticated traders and noise traders. We assume that it is quadratic as in Subrahmanyam (1998) or Dow and Rahi (2000). Specifically

$$G_k(X_{kt}) = \frac{c_k X_{kt}^2}{2}, \quad (5)$$

with $c_k = c_S$ for sophisticated investors and $c_k = c_N$ for noise traders. This specification enables us to analyze in a tractable way the effect of making purchases and sales more expensive for one group of investors relative to the other. To see this, let

$$R_{t+1} = d_{t+1} + p_{t+1} - (1+r)p_t, \quad (6)$$

be the excess return of the stock over the period $[t, t+1]$. Investors' demand functions at date t are

$$X_t^S(p_t) = \frac{E_t(R_{t+1})}{c_S + \gamma(\text{Var}_t(p_{t+1}) + \sigma_\xi^2)}, \quad (7)$$

$$X_t^N(p_t) = \frac{E_t(R_{t+1}) + \rho_t}{c_N + \gamma(\text{Var}_t(p_{t+1}) + \sigma_\xi^2)}. \quad (8)$$

Investors buy (resp. sell) the security when they expect a positive (resp. negative) return. The elasticity of their demand to this expectation decreases with parameter c_k . Thus, by increasing c_k , we can study the effect of restraining one category of investors while keeping the restraint on the other category constant. We refer to c_k as the *restraint coefficient* for group k .

In the rest of this section, we focus on steady state equilibria in which the conditional volatility of prices, $\text{Var}_t(p_{t+1})$, is constant.

Proposition 1 *For all parameter values, there exists a steady state equilibrium. In a steady state equilibrium, the stock price at date t is*

$$p_t = \frac{\bar{d}}{r} - \frac{\theta(\mu, c_N, c_S)}{r} + \frac{\beta(d_t - \bar{d})}{1+r-\beta} + \lambda(\mu, c_N, c_S)\rho_t, \quad (9)$$

¹²As in DeLong et al.(1990), we assume that all noise traders have the same mis-perception, ρ_t , of the future stock price. Results are unchanged if there is an idiosyncratic component in noise traders' misperception. Dorn, Huberman and Sengmueller (2006) or Barber, Odean and Zhu (2005) document the existence of a systematic component in individual investors' orders.

where $\theta(\mu, c_N, c_S)$ and $\lambda(\mu, c_N, c_S)$ are positive constants defined in the appendix ($\lambda(\mu, c_N, c_S) > 0$ iff $\mu > 0$).

For some parameter values, there are two steady state equilibria with differing values for variables $\theta(\mu, c_N, c_S)$ and $\lambda(\mu, c_N, c_S)$ (see the proof of Proposition 1). But our testable implications do not depend on the equilibrium we pick among steady state equilibria.

The average stock price, $\frac{\bar{d}}{r} - \frac{\theta(\mu, c_N, c_S)}{r}$, is equal to the discounted value of the average dividend ($\frac{\bar{d}}{r}$) adjusted for risk ($\frac{\theta(\mu, c_N, c_S)}{r}$). The stock price fluctuates randomly around this average level because (i) the dividend paid in each period contains information about future dividends when $\beta > 0$ (third term in equation (9)), and (ii) noise traders' sentiment is a source of price pressures (last term in equation (9)). For instance, when noise traders are pessimistic ($\rho_t < 0$), they decrease their holdings of the stock. The stock price must then decrease to induce sophisticated investors to increase their holdings of the stock since the latter are risk averse. In contrast, when noise traders are euphoric ($\rho_t > 0$), the stock price must increase to induce sophisticated investors to decumulate their inventory in the stock.

3.2 Chaining up noise traders

Now, to obtain our testable predictions, we study the effect of making trading relatively more expensive for one group of investors. We assume that the stock is listed either in market F or in market C . In these markets

$$c_N^F = c_S^F \leq \text{Min}\{c_S^C, c_N^C\}, \quad (10)$$

where c_k^j is the restraint coefficient for group k in market $j \in \{C, F\}$. In Market F , trading restraints are identical for both categories of investors. In contrast, in market C , the trading restraint differs across the two categories of investors. Moreover, all investors are more restrained in market C than in market F .

Using equation (9), we obtain

$$R_{t+1} = E_t(R_{t+1}) + (1+r)(1+r-\beta)^{-1}\xi_t + \lambda(\mu, c_N, c_S)\varepsilon_{t+1}, \quad (11)$$

with

$$E_t(R_{t+1}) = \theta(\mu, c_N, c_S) - \lambda(\mu, c_N, c_S)(1+r-\alpha)\rho_t. \quad (12)$$

Conditional expected returns are negatively related to noise traders' sentiment and therefore vary over time. To see this point, suppose that, at date t , noise traders are optimistic ($\rho_t > 0$). Thus, at this date, they have a strong demand for the stock, which increases

its clearing price relative to its long run mean. Moreover, since shifts in noise traders' misperceptions are transient ($\alpha < 1$), investors expect the price pressure exerted by noise traders to be smaller at date $t+1$ on average. Overall these two effects reduce the expected return of the stock relative to its unconditional value.

Using equations (11) and (12), we obtain the following expression for the unconditional variance of excess returns:

$$Var(R_{t+1}) = \underbrace{\left(\frac{1+r}{1+r-\beta}\right)^2 \sigma_\eta^2}_{\text{Fundamental Volatility}} + \underbrace{\lambda(\mu, c_N, c_S)^2 \left(\frac{1+r-\alpha}{1-\alpha^2} + 1\right) \sigma_\varepsilon^2}_{\text{Excess Volatility}}. \quad (13)$$

In this expression, we decompose the stock return volatility in two components. The first component ("fundamental volatility") is the volatility of stock returns due to the uncertainty on the dividend of the security, and the arrival of public information on future dividends in each period (the dividend paid in period t provides information on future dividends). The second component ("excess volatility") is the contribution of noise trading to return volatility, and would disappear if $\mu = 0$. It implies that the variability of stock returns cannot be fully explained by the variability of dividends and public information. For this reason, we refer to this component as the "excess volatility" component. Using this expression for the variance of stock returns, we obtain the following result.

Implication 1 : *If $\mu = 0$ or $c_S^C = c_N^C$, the variance of the stock return is identical in markets C and F . If $\mu > 0$ and $c_S^C < c_N^C$ then the variance of the stock return is smaller in market C than in market F . If $\mu > 0$ and $c_S^C > c_N^C$ then the variance of the stock return is larger in market C than in market F .*

When $c_S^C < c_N^C$, the impact of the increase in the trading restraint is higher for noise traders, other things equal (see equations (7) and (8)). Thus, price pressures due to noise trading are smaller in market C , and excess volatility is smaller. Opposite effects are obtained if sophisticated investors are more restrained, that is $c_S^C > c_N^C$. Moreover, if noise traders and sophisticated investors are equally constrained in market C then return volatility is identical in markets F and C , even though trading restraints are higher in market C . Thus, as noted by other authors (e.g., Schwert and Seguin (1993)), the net effect of a trading restraint (e.g., a securities trading tax) on volatility is ambiguous in presence of noise trading. It reduces volatility if and only if it is tighter for noise traders.

Last, in absence of noise trading ($\mu = 0$), the trading restraint has no effect on volatility. Actually, in this case, the volatility of stock returns is entirely due to uncertainty on dividends and the arrival of public information on future dividends. These sources of volatility are independent of changes in trading restraints.

Implication 1 yields our main testable hypothesis. The suppression of the RM is similar to a switch from market F to market C for a stock initially listed on the RM. Moreover, if individual investors are noise traders, this stock switches to an environment in which $c_S^C < c_N^C$ since trading restraints are more stringent for individuals on the spot market. In this case, the model implies a decline in the return volatility of stocks listed on the RM after the reform.

The model does not yield a clear-cut prediction for the impact of tighter trading restraints on the level of the stock price. Consider the case in which $c_S^C < c_N^C$ and $\mu \geq 0$. The return on the stock is less volatile in market C (Implication 1). Thus, other things equal, investors require a smaller risk premium (θ) to hold the stock. However, noise traders take smaller positions in equilibrium. Thus, sophisticated investors must bear a larger share of risk in market C . In equilibrium, this effect increases the risk premium on the stock. The net effect depends on parameter values. For this reason, we do not empirically investigate the impact of the suppression of the forward market on the level of prices for stocks listed on the RM.

Rather, we focus on two other implications of the model. First, consider the serial covariance of stock returns. Using equation (11), we obtain

$$Cov(R_{t+1}, R_t) = Cov(E_t(R_{t+1}), E_{t-1}(R_t)) + \lambda * Cov(E_t(R_{t+1}), \varepsilon_t) \quad (14)$$

$$= \lambda(\mu, c_N, c_S)^2(1+r-\alpha)\sigma_\varepsilon^2\left(\frac{\alpha(1+r)-1}{1-\alpha^2}\right). \quad (15)$$

To understand equation (15), consider first the case in which $\alpha = 0$. In this case, the autocovariance of stock returns is negative. For instance, suppose that there is a positive shock on investor's sentiment at date t . This shock pushes prices up at this date and implies that the return from date $t-1$ to date t is higher than expected. However, since investors' sentiment is not persistent, the return from date t to date $t+1$ will be smaller than during the previous period. Thus, transient variations in investors' sentiment generate reversals in stock returns. When $\alpha > 0$, noise traders' sentiment decays more slowly over time. Thus, at short horizons, reversals are smaller and for α large enough ($\alpha > 1/(1+r)$), the autocovariance of stock returns is positive. In all cases, however, noise trading is the source of autocovariance in stock returns. Thus, a restraint on noise traders should reduce, in absolute value, this autocovariance. This is our next implication.

Implication 2 *If $\mu = 0$ or $c_S^C = c_N^C$, the autocovariance of the stock return is identical in markets C and F . If $\mu > 0$ and $c_S^C < c_N^C$ then, in absolute value, the autocovariance of stock returns is smaller in market C than in market F . If $\mu > 0$ and $c_S^C > c_N^C$ then in absolute value, the autocovariance of stock returns is larger in market C than in market F .*

This result yields our second testable hypothesis. Namely, if retail investors are noise traders, the autocovariance of returns for stocks listed on the RM should decrease after the reform of this market.¹³

Let $\Delta X_t^N \stackrel{def}{=} \mu(X_t^N(p_t) - X_{t-1}^N(p_{t-1}))$ and $\Delta X_t^S \stackrel{def}{=} (1 - \mu)(X_t^S(p_t) - X_{t-1}^S(p_{t-1}))$ be the net changes in noise traders' holdings and sophisticated investors' holdings from date $t - 1$ to date t , respectively. Using Proposition 1, we obtain that in equilibrium:

$$\Delta X_t^N = \left(\frac{\mu(1 - \lambda(\mu, c_N, c_S)(1 + r - \alpha))}{c_N + 2\gamma(\text{Var}_t(p_{t+1}) + \sigma_\xi^2)} \right) (\rho_t - \rho_{t-1}). \quad (16)$$

The clearing condition imposes

$$\mu X_t^N(p_t) + (1 - \mu)X_t^S(p_t) = 1 = \mu X_{t-1}^N(p_{t-1}) + (1 - \mu)X_{t-1}^S(p_{t-1}), \quad (17)$$

which yields

$$\Delta X_t^N = -\Delta X_t^S. \quad (18)$$

If investors' sentiment increases (resp. decreases) from date $t-1$ to date t then noise traders are net buyers (resp. sellers) of the stock at date t (see equation (16)). In this case, as shown by equation (18), sophisticated investors must be net sellers (resp., net buyers). Thus, shocks on investors' sentiment generate price changes to induce sophisticated investors to increase or decrease their holdings of the stock (depending on whether noise traders are net sellers or net buyers). In fact, using equations (8) and (9), we can write the change in price between date $t - 1$ and date t as

$$p_{t+1} - p_t = \Upsilon \Delta X_t^N + \eta_t, \quad (19)$$

with

$$\Upsilon(\mu, c_N^k, c_S^k) \stackrel{def}{=} \frac{(c_S^k + 2\gamma(\text{Var}_t(p_{t+1}) + \sigma_\xi^2))}{(1 - \mu)(1 + r - \alpha)}. \quad (20)$$

and $\eta_t \stackrel{def}{=} (1 + r - \beta)^{-1}\beta(d_{t+2} - d_{t+1})$. Hence, the model implies a positive relationship between the change in noise traders' holdings and the contemporaneous change in price (equation (19)). Parameter Υ measures the impact of the net trade by noise traders at date $t + 1$ on the stock price. Intuitively, the inverse of Υ is a measure of market liquidity for noise traders. We refer to Υ as *the price impact coefficient*.

Implication 3 *Suppose that $c_S^C = c_S^F$. If $\mu = 0$ or $c_S^C = c_N^C$, the price impact coefficient, Υ , is identical in markets C and F. If $\mu > 0$ and $c_S^C < c_N^C$ then the price impact coefficient is smaller in market C than in market F. If $\mu > 0$ and $c_S^C > c_N^C$ then the price impact coefficient is larger in market C than in market F.*

¹³The model implies that the autocovariance and the variance of stock returns should both decline after the reform. Thus, its prediction for the autocorrelation of return is ambiguous. We focus on the autocovariance of returns for this reason.

The intuition for this result is as follows. If $\mu > 0$ and $c_S^C < c_N^C$, volatility is smaller in market C since the restraint coefficient is larger for noise traders. Accordingly, other things equal, sophisticated investors demand smaller price concessions to absorb noise traders' order imbalances since their inventory risk is smaller. A symmetric reasoning applies when $\mu > 0$ and $c_S^C > c_N^C$. If individuals are noise traders, the last result implies that stocks listed on the RM should experience a decrease in the price impact coefficient after the reform.

Testing this prediction is not straightforward as we do not observe changes in individual investors' holdings. Thus, we cannot directly estimate the price impact coefficient, Υ . As a proxy for Υ , we use the Amihud ratio (see Amihud (2002)), i.e., the ratio of the absolute return over a given period of time to the contemporaneous volume in dollar. Intuitively, a larger sensitivity of returns to volume indicates that trades have larger price impacts. For this reason, the Amihud ratio is often used as a proxy for price impact. Goyenko et al.(2008) show empirically that this proxy is indeed highly correlated with high frequency measures of price impacts.

4 Empirical tests

In this section, we test the predictions of the model. We first describe the data and our methodology (Section 4.1). Then, we test our main predictions (Section 4.2). Finally, we discuss alternative explanations for our empirical findings and perform additional robustness checks (Section 4.3).

4.1 Data and methodology

4.1.1 Data Description

Our main dataset provides daily variables for each stock listed on the French stock market from September 1998 to September 2002.¹⁴ For each stock, we have: the closing price, the daily return (adjusted for split and/or dividends), the number of outstanding shares, and the trading volume. Moreover, before the reform, we know whether the stock is listed on the RM or on the spot market. For some tests, we use another dataset that provides, for each transaction, the price of the transaction, the size of the transaction, and the bid-ask

¹⁴These data are collected by the Paris Bourse. They are made available in a user-friendly format by EUROFIDAI. For information, see <http://www.eurofidai.org/>.

spread at the time of the transaction.¹⁵

We refer to stocks listed on the RM as of September 1, 2000 as the *treated stocks* (173 stocks) and to the remaining stocks as the *control stocks* (1,004 stocks). The control group includes a few stocks that were listed on the RM at the beginning of our sample period but that switched to the spot market before September 2001. Our results are unchanged if we do not include these stocks in the control group.

A few stocks in our sample serve as underlying securities for options and, since January 2001, single stock futures. They all belong to the treated group. Arguably, speculators can use derivatives to avoid trading restraints on the underlying securities. In this case, it should be *more* difficult to identify the effect of the reform on the stocks that serve as underlying of derivatives contracts. For this reason, we do not exclude them from our sample, but check that the findings are robust to this decision.

Table 1 reports summary statistics for the key variables in our study.

[Insert Table 1 about here]

We use three measures of volatility: (i) the monthly standard deviation of daily returns, (ii) the monthly standard deviation of daily stock returns *minus* the daily market return, and (iii) the monthly standard deviation of the residual of a regression of daily stock returns on market returns. In each case, we remove returns that are above (resp. below) the median plus (resp. minus) five interquartile ranges. The findings are robust to the trimming method. Moreover, when a firm has fewer than twenty nonmissing daily returns through a given month, the variable of interest are set to missing values in this month. We use daily observations of returns as in other related papers (e.g., Campbell et al. (1993), Llorente et al.(2002) or Lee et al.(2004)).

Table 1 shows that the mean values of the three measures of volatility are similar, both for treated and control stocks. Overall, the volatility of treated stocks is lower than the volatility of control stocks. For instance, the daily volatility of raw returns is 290 basis points for control stocks and 250 basis points for treated stocks, on average.

For each stock and in each month, we also compute (i) the autocovariance in daily returns, and (ii) the average of the daily ratio of absolute return to trading volume in euros (the Amihud ratio). Table 1 reports the mean values of these variables across month and across stocks, separately for treated and control stocks. For both groups, the average autocovariance of daily returns is negative. However, returns of treated stocks tend to reverse

¹⁵This dataset, called BDM, is provided by the Paris Bourse and is used in other empirical studies (e.g., Bessembinder and Venkataraman (2003)).

themselves less. The Amihud ratio is also lower for treated stocks than for control stocks. These observations indicate that treated stocks are more liquid than control stocks. In fact, Table 1 shows that treated stocks have, on average, a higher turnover (daily number of shares traded/outstanding number of shares) and smaller bid-ask spreads compared to control stocks.¹⁶ Moreover, treated stocks have, on average, a larger market capitalization than control stocks. We explain below how we control for these differences in the characteristics of our two groups of stocks.

4.1.2 Methodology

As explained in Section 2, it is more costly for individual investors, relative to institutions, to trade treated stocks after September 25, 2000. In contrast, trading restraints on individual investors are identical throughout the sample period for control stocks, since these stocks trade on the spot market before and after the reform. Thus, we can isolate the effects of restraining trades by individual investors by considering changes (e.g., in volatility) for stocks in the treated group, while controlling for market wide movements using stocks in the control group. Our empirical strategy consists in comparing treated and control stocks using a “differences-in-differences” estimation. Our baseline regression is:

$$Y_{it} = \alpha + \beta_0 T_i + \beta_1 POST_t + \beta_2 T_i \times POST_t + \varepsilon_{it}, \quad (21)$$

where Y_{it} is the outcome of interest (e.g., volatility) for stock i in month t , $POST_t$ is a dummy variable equal to one after September 2000, and T_i is equal to one if the firm belongs to the treated group.

In this regression, coefficient β_0 measures the difference between the mean values of the dependent variable for the treated group and the control group before the reform. Thus, T_i controls for differences in the characteristics of the two groups that are fixed over time. Coefficient β_1 measures the change in the mean value of the dependent variable before and after the reform. Hence, $POST_t$ controls for factors that affect the evolution of the dependent variable around the reform and which are common to all stocks. The identifying assumption is that, on average, these factors have the same effect for control and treated stocks. Under this assumption, β_2 measures the causal effect of the reform on the dependent variable Y_{it} . Indeed, it is the difference in the mean value of the dependent variable for treated stocks before and after the reform after controlling for (i) fixed differences in the characteristics of the two groups of stocks, and (ii) common factors affecting the evolution

¹⁶For each stock, we compute the bid-ask spread by using the bid and ask price observed for the last transaction of each month. Table 1 reports the average of this bid-ask spread across stocks and across month.

of the dependent variable over time.

In estimating equation (21), we take into account several methodological issues. The OLS standard deviations of differences-in-differences estimates are biased if there is serial correlation in error terms for a given stock. This serial correlation is expected given the nature of the independent variables in equation (21) (see Bertrand, Duflo and Mullainathan (2004)). Thus, for a given stock, we allow for correlations between error terms, ε_{it} , by “clustering” at the firm level. Second, the number of observations in each group 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 since they are less liquid.¹⁷ This could bias our inferences. One way to deal with this problem is simply to restrict our attention to a sample of stocks with non-missing observations. Another way is to estimate equation (21) using a stock fixed effect, that is:

$$Y_{it} = \alpha_i + \beta_1 POST_t + \beta_2 T_i \times POST_t + \varepsilon_{it}, \quad (22)$$

We find that both approaches deliver very similar findings. Thus, for brevity and to retain the largest number of observations, we only report the results with the second approach.

Last, our methodology assumes that factors affecting the evolution of the outcome Y_{it} over time have, on average, the same effect on both groups of stocks. This assumption is more plausible if these two groups have similar characteristics. In Figure 1, we compare the distributions of stock market capitalization (left panel) and turnover (right panel) for each group at the beginning of our sample period. We focus on these characteristics since they were used by the Paris Bourse to allocate stocks to the RM or to the spot market.

[Insert Figure 1 about here]

The distributions of turnover for the two groups largely overlap. The distributions of market capitalization are more heterogeneous but they still overlap. For instance, more than 50% (resp. 36%) of the treated stocks have a market capitalization smaller (resp. higher) than the market capitalizations in the last (first) percentile of the size distribution for control stocks. Overall, Figure 1 shows that there are many stocks in each group that are comparable in terms of both turnover and size.

To control for differences in size and trading activity between the two groups of stocks, we use the methodology proposed by Crump et al. (2006). Namely, using the observations of September 2000, we run a logistic regression to determine the likelihood that a firm

¹⁷There are 10,903 (resp. 234) missing observations for stocks in the control (resp. treated) group.

belongs to the treated group. Specifically, we estimate the following logistic regression (results unreported for brevity):

$$T_i = \alpha + \sum_{q=1}^4 \beta_q S_q + \sum_{q=1}^4 \gamma_q V_q + \eta_i, \quad (23)$$

where $T_i = 1$ if stock i is treated, $S_q = 1$ if the initial market capitalization of stock i belongs to the q^{th} quartile in terms of capitalization, and $V_q = 1$ if the turnover of stock i belongs to the q^{th} quartile in terms of turnover. As expected, the probability of being treated increases in both size and turnover. We then use the estimates of this logistic regression to compute the probability (the “score”) that a stock belongs to the treated group given its characteristics. We then only retain stocks with a score between 0.1 and 0.9. The resulting subsample of stocks contains 71 stocks in the treated group and 124 stocks in the control group. We refer to this subsample as being the *restricted* sample.

[Insert Figure 2 about here]

Figure 2 shows the distributions of market capitalization and turnover for treated and control stocks in the restricted sample. As expected, these distributions are now much more comparable. Thus, as a robustness check, in all our tests, we estimate equation (22) for the restricted sample as well.

In the spirit of the “propensity score matching” literature, we also run regressions (21) and (22) with the full sample, but controlling for $POST \times SCORE_i$, where $SCORE_i$ is the probability that stock i belongs to the treated group as estimated using equation (23). This alternative approach gives similar results, that we do not report here to save space.

Finally, we also test the robustness of our findings by using a foreign sample of control stocks. Namely, we use a sample of thirty-nine Belgian and Dutch blue-chips listed on the Brussels and the Amsterdam stock exchanges. At the time of our study, there is no forward equity market on these exchanges. Thus, it is easier to find stocks listed on these markets that are comparable to our treated stocks. On average, the logarithm of market capitalization for the Belgian and Dutch blue-chips in our sample is 22, with a standard deviation of 1.6. These figures are very similar to the corresponding figures for the group of treated stocks in our study (see Table 1). However, the level of turnover is much higher for Belgian and Dutch stocks (0.8 against 0.22). The findings obtained with this control group are very similar to those we obtain with the French control stocks. Statistical tests have less power, however, since the control group is smaller. For brevity, we do not report the findings obtained with this approach.

4.1.3 The distribution of trade sizes

A premise of our analysis is that the suppression of the RM makes trading more expensive for retail investors relative to institutional investors. Hence, this suppression should be associated with a drop in retail trading relative to institutional trading.

We cannot directly test this conjecture because we do not observe individual investors' trades. Several papers suggest that the fraction of small trades is a good proxy for retail trading (e.g., Derrien (2005), Barber, Odean and Zhu (2006), and Hvidkjaer (2008)). Institutions also place small orders to minimize price impact but, in contrast to retail investors, they also place large orders. Thus, if our conjecture is correct, the suppression of the RM should coincide with a change in the distribution of trade sizes; namely, a decline in the frequency of small trades and an increase in the frequency of large trades.

To study this question, we use the time stamped record of all transactions (prices and quantities) from August 1, 2000 to November 30, 2000. We define small and large trades with respect to cutoffs that depend on firm size as in Hvidkjaer (2006). Specifically, in each month t , we group stocks into quintiles, q , based on their market capitalization. Within each quintile, we compute the stock price p_q that stands at the 95th percentile. For quintile q , we define the cutoff for small trades as $100 \times p_q$, and the cutoff for large trades as $150 \times p_q$. Thus, the cutoffs vary across quintiles and across months. They increase in firm size because larger firms tend to have higher stock prices. With these cutoffs, we classify 90% of all trades as small trades and 6% as large trades (see the second panel of Table 1). The fractions of small and large trades do not add up to one as trades with intermediate sizes are not classified. The conclusions are robust to other definitions for the cutoffs (and thereby other baseline fractions of large and small trades).

[Insert Figure 3 about here]

We first compute the average daily fraction of small and large trades for each group of stocks, as shown in Figure 3. The top-left panel shows that, as expected, for treated stocks, there is a marked drop in the fraction of small trades after the reform. In contrast, there is no apparent change in the fraction of small trades for control stocks (top-right panel). The bottom right panel shows that there is a sharp increase in the fraction of large trades for treated stocks after the reform. Again, there is no clear change for control stocks.

To quantify the effects in Figure 3, we implement the methodology described in Section 4.1.2: we estimate equations (21) and (22) using as dependent variables (i) the fraction of small trades (i.e., $Y_{it} = ST_{it}$ where ST_{it} is the fraction of small trades in day t for stock

i) and (ii) the fraction of large trades (i.e., $Y_{it} = LT_{it}$ where LT_{it} is the fraction of large trades in day t for stock i).

[Insert Table 2 about here]

Table 2 reports the results. There is a significant decline in the fraction of small trades for treated stocks after the reform and no significant change in this fraction for control stocks. The decline in the fraction of small trades for treated stocks is equal to about 1.4%, that is 10% of the standard deviation of the fraction of small trades for these stocks. This finding is robust to the inclusion of fixed effects and it persists when we use the restricted sample.

The findings for the proportion of large trades mirror those obtained for the proportion of small trades (see columns 4-6 of Table 2). There is a significant increase in the fraction of large trades for treated stocks after the reform relative to the fraction of large trades for control stocks. This increase is equal to about 1.6%, that is 10% of the standard deviation of the fraction of large trades for treated stocks.

Overall, for treated stocks, small (resp. large) trades are less (resp. more) frequent after the reform. This evolution in the distribution of trade sizes of treated stocks after the reform indicates that there is less retail trading in these stocks after the suppression of the forward market, as conjectured.

4.2 Empirical findings

4.2.1 Volatility

Our central hypothesis is that the suppression of the RM should decrease the volatility of stocks listed on this market as it restrains retail trading. As explained in Section 4.1.2, to test this hypothesis, we estimate equations (21) and (22) using volatility as a dependent variable (i.e. $Y_{it} = volat_{it}$ where $volat_{it}$ is the volatility measure for stock i in month t). Our testable hypothesis for volatility implies that β_2 should be significantly negative.

Our findings are identical for all measures of volatility defined in Table 1. Hence, for brevity, we just report in Table 3 the results with our second measure of volatility, that is the standard deviation of the daily stock return minus the daily market return.

[Insert Table 3 about here]

Column 1 of Table 3 reports the estimates of our baseline regression. These estimates confirm that treated stocks are significantly less volatile than control stocks, as observed in Table 1. Moreover, as implied by the theory, we find a significant decline in the volatility of treated stocks after the reform, relative to the volatility of control stocks.

The point estimates for β_2 is sensitive to the method that we use to estimate the effect of the reform. When we do not include stock fixed effects, the drop in the volatility of treated stocks after the reform is equal to twenty-six basis points (34% of the standard deviation of the volatility of treated stocks before the reform). When we include stock fixed effects, the drop in volatility of treated stocks is smaller, and equal to eighteen basis points. Last, when we estimate equation (22) for the restricted sample (our preferred specification), the drop in volatility for treated stocks is equal to eleven basis points, that is 11% of the standard deviation of the volatility of treated stocks before the reform. For all specifications, the decline in volatility of treated stocks after the reform is statistically significant (at the 1% level).

Overall, in economic terms, the effect of the reform on volatility is moderate, but not negligible. The model, combined with other empirical evidence, indeed suggests that the drop in the volatility of daily return cannot be too large to be plausible. To see this point, recall that the variance of stock returns in the model is the sum of two components: the fundamental volatility component and the excess volatility component (see equation (13)). The fundamental volatility component is not affected by noise trading. Thus, restraining noise traders can reduce the variance of stock returns by an amount at most equal to the excess volatility component. Using this observation, it is easily shown that the percentage difference in the standard deviation of returns between market F and C cannot be larger than:

$$\text{upper bound} = 1 - \frac{1}{\sqrt{1 + \Omega}}, \quad (24)$$

where Ω is the ratio of the excess volatility component to the fundamental volatility component. Roll (1988) provides an estimate of the inverse of this ratio using daily returns of stocks listed on the NYSE. His findings suggest a value of Ω equal to about 33% (see Table IV in Roll (1988), two first lines).¹⁸ Thus, even though a large fraction of stock volatility is idiosyncratic (see Roll (1988)), the excess volatility component seems small compared to the component of volatility due to information arrival (public and private) on future

¹⁸Roll (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. Thus, V_x corresponds to our "excess volatility" component and pV_y to our "fundamental volatility" component. 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 that $\frac{V_y}{V_x} = 20.457$ and $p = 0.14393$. It follows that in this case $\Omega \stackrel{def}{=} \frac{V_x}{pV_y} \simeq 33\%$.

cash-flows. Empirical findings in Vuolteenaho (2000), Durnev et al.(2003) and Shen (2007) point in the same direction. Moreover, the stocks affected by the reform have relatively large capitalizations. We expect μ to be relatively small in these stocks since retail trading is relatively more prevalent in stocks with small capitalization (see for instance Brandt et al.(2005)). Again, this implies a relatively small value for Ω (see equation (13)). As shown by equation (24), in these conditions, one expects a moderate decline in the volatility of the stocks affected by the reform in our empirical analysis. For instance, if $\Omega = 33\%$, the percentage difference in volatility between markets F and C in the model is at most 13%.

4.2.2 Returns reversals

Our second prediction is that the autocovariance of stock returns should decrease (in absolute value) for the treated stocks after the reform. Swings in noise traders' demands are a source of serial correlation in returns (see Section 3.2). For this reason, a restraint on noise traders reduces the size of the autocovariance in stock returns. To test this hypothesis, we set $Y_{it} = autocov_{it}$ in equations (21) and (22) where $autocov_{it}$ is the autocovariance of daily returns for stock i in month t . From Table 1, we know that the average autocovariance of daily returns is negative. Thus, our testable hypothesis implies that β_2 should be significantly positive. That is, the size of return reversals for treated stocks is closer to zero after the reform.

[Insert Table 4 about here]

The results are reported in Table 4. In all specifications, we find that the reform significantly reduces the size of reversals for treated stocks relative to control stocks. The largest estimate for β_2 is obtained when we use the restricted sample. In this case, the estimate for β_2 indicates a drop in the absolute value of the autocovariance of treated stocks equal to about 0.2 basis points, that is 13% of the standard deviation of this variable before the reform (see Table 1). Overall, the decrease in reversals for treated stocks, following the suppression of the forward equity market, is sizeable and consistent with the model.

4.2.3 Price impact

Our final prediction is that the Amihud ratio (the ratio of absolute return to contemporaneous trading volume) should decline for treated stocks after the suppression of the RM. In the model, 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 noise traders' order

imbalances. As a consequence, it should take more volume to move prices after the reform (see Section 3.2). To test this hypothesis, we now use the Amihud ratio as the dependent variable in equations (21) and (22).

[Insert Table 5 about here]

Table 5 report the findings. When we do not control for stock fixed effects, we find a decline in the Amihud ratio of treated stocks relative to control stocks but the effect is not significant. When we control for stock fixed effects, the decline in the Amihud ratio of treated stocks is much larger and significant. The point estimate for β_2 indicates that the reform is associated with a decline of thirteen basis points for the Amihud ratio of treated stocks, that is 29% of its standard deviation for treated stock before the reform. The finding is even stronger (fifty-seven basis points) and more significant when we use the restricted sample to better control for differences in sizes and trading activity between treated and control stocks (see Column 3).

4.3 Robustness

We find that the suppression of the French forward equity market is associated with a significant reduction in (i) the return volatility, (ii) the return autocovariance, and (iii) the Amihud ratio of the stocks affected by the reform. All these effects are consistent with the view that individual investors acting as noise traders are a source of volatility, as shown by the model. We now discuss alternative explanations for our empirical findings, and we discuss further the robustness of our results.

Alternative explanations. Our findings may stem from a reduction in quoted bid-ask spreads of treated stocks. Indeed, a smaller bid-ask spread reduces the bid-ask bounce. Thus, it lowers return volatility and the absolute value of the autocovariance in stock returns (see Roll (1984)). It may also reduce the Amihud ratio but, empirically, the relationship between the bid-ask spread and the Amihud ratio is weak (see Goyenko et al.(2008)).

[Insert Figure 4 about here]

Figure 4 depicts the evolution of the difference between the average bid-ask spreads of treated and control stocks between January 2000 and June 2001 (along with a four month moving average). There is no clear trend, neither upward nor downward, in this difference. This observation suggests that the reform has no effect on quoted spreads. This conjecture

is confirmed when we estimate equations (21) and (22) with the monthly bid-ask spread for each stock as dependent variable. In this case, we do not find any significant effect of the suppression of the RM on the bid-ask spread of treated stocks. We do not report the results for brevity. As there is no significant change in bid-ask spreads of treated stocks around the reform, a reduction in bid-ask spreads of these stocks over time cannot be the source of our empirical findings.

Duffee (1995) shows that there is a positive relationship, at the firm level, between the volatility of stock returns and contemporaneous returns. Moreover, this relationship largely explains the so called leverage effect (the fact that changes in volatility and lagged returns are inversely related). Duffee (1995)'s finding suggests another possible explanation for our results, namely that treated stocks experience a more severe decline in prices after the suppression of the RM than control stocks. This is indeed a possibility since the reform considered in this article coincides with a downturn of the French stock market.

[Insert Figure 5 about here]

Figure 5 does not support this explanation, however. It shows the evolution of the mean market capitalizations of the control and treated stocks over our sample period (normalized at 100 in September 2000). Stock prices peak in August 2000, but control stocks are more severely hit by the downturn than treated stocks. As an additional check, we run the following regression:

$$Y_{it} = \alpha_i + \beta_1 POST_t + \beta_2 T_i \times POST_t + \beta_3 R_{it} + \varepsilon_{it}, \quad (25)$$

where Y_{it} is one of the three dependent variables analyzed in Section 4.2 and R_{it} is the return for stock i in month t . The other variables are defined as in Section 4.2. Coefficient β_3 controls for variations in stock returns. For all dependent variables, we find that our estimates for β_2 are similar, both in terms of magnitude and statistical significance, to those found in Section 4.2 (we do not report these estimates for brevity). Hence, our findings are not explained by differences in the evolution of prices for control and treated stocks around the reform.

Additional robustness tests. To check whether our findings are not spurious, we estimate equation (22) using as the event date each month between January 1996 and December 1997. In this way, we obtain twenty-four estimates for coefficient β_2 under the null hypothesis that the reform has no effect (since there was no reform for treated stocks during this sample period).

[Insert Figure 6 about here]

Figure 6 presents the corresponding empirical distribution of β_2 when volatility is the dependent variable. The estimates of β_2 in this case varies from -9 basis points to 8 basis points. All these estimates are strictly larger than the estimate of β_2 for September 2000, that is at the time of the actual reform. Thus, it is unlikely that the drop in volatility of treated stocks observed at the time of the suppression of the French forward equity market in 2000 is obtained just by chance. Results for the autocovariance of returns and the Amihud ratio are less strong. Using the empirical distributions of β_2 in these cases (not shown for brevity), we would reject the null of no effect of the reform on these variables at the 10% level only.

Last, we check whether the conclusions of the analysis are affected by the length of the sample period around the reform (fourty-eight months equally distributed around the reform). Specifically, we consider (i) a thirty-six months sample period and (ii) a twenty-four months sample period, equally distributed around the reform. We only use the restricted sample. The results are reported in Table 6.

[Insert Table 6 about here]

Clearly, the results are unchanged when we use a thirty six months sample period. The estimates of the effect of the reform are in fact very similar to those reported in the previous section. With a shorter sample period, the sign of the estimates for the impact of the reform on volatility and the autocovariance of stock returns are unchanged. The estimates are insignificant however as our statistical tests loose power due to the reduction in sample size. Yet, the reduction in the Amihud ratio remains significant and has the same magnitude as that obtained with longer sample periods.

5 Conclusion

In this article, we test the hypothesis that retail trading has a positive impact on the volatility of individual stocks. Testing this hypothesis is important since, as suggested by Brandt et al.(2005), it could explain episodes of high idiosyncratic volatility. Moreover, it has implications for arbitrage activities and various regulatory debates regarding financial markets.

To identify the effect of retail investors on return volatility, we study a reform of the French equity market that makes speculation more expensive for retail investors. Specifically, a subset of French stocks used to trade exclusively in a forward market. This forward market is replaced by a spot market in September 2000. This reform made it more costly

for retail investors to short sell or to buy on margin stocks that were listed on the forward market. In fact, we find that the fraction of small trades (a proxy for retail trading) drops significantly for the stocks affected by the reform while no such change is observed for other stocks.

The hypothesis that retail trading has a positive effect on return volatility follows from the view that individuals are noise traders. Therefore, we use a standard model of noise trading (DeLong et al.(1990)) to develop predictions regarding the effect of making speculation more costly for noise traders. The model has three predictions. Namely, (i) the return volatility, (ii) the autocovariance of stock returns in absolute value, and (iii) the Amihud ratio should decrease for the stocks affected by the reform in 2000. No such change should be observed for other stocks and different predictions would obtain for the stocks affected by the reform if individual investors were not noise traders.

We test these predictions using stocks not affected by the reform as control stocks. Differences-in-differences estimates indicate that the reform has indeed significantly reduced the volatility of the stocks affected by the reform relative to other stocks. Moreover, for these stocks, the Amihud ratio and the autocovariance of stock returns are also significantly smaller after the reform. These findings persist when we perform various robustness checks. Overall, the findings support the hypothesis that individual investors, acting as noise traders, have a positive effect on return volatility.

Our study focuses on one event and in this sense it adds only one datapoint to the debate regarding the effect of retail investors on volatility. Moreover, models of limited participation suggest that volatility should be reduced as stock market participation enlarges (Allen and Gale (1994)). Thus, the long-term effect of changes in the level of market participation by retail investors on volatility may be more complex than that suggested by our empirical analysis. We leave this question for future research.

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A Proofs

In this appendix, we denote $Var_t(p_{t+1})$ by $\sigma^2(\mu, c_N, c_S)$ to stress that parameters $\{\mu, c_N, c_S\}$ determine the volatility of the stock price in the model.

A.1 Proof of Proposition 1

The clearing condition at date t imposes

$$X_t^S(p_t) + X_t^N(p_t) = 1.$$

Using equations (7) and (8), we deduce that

$$p_t = \frac{1}{1+r} \{E_t(d_{t+1} + p_{t+1}) + \lambda(\mu, c_N, c_S)(1+r-\alpha)\rho_t - \theta(\mu, c_N, c_S)\}, \quad (26)$$

with

$$\lambda(\mu, c_N, c_S) = \left(\frac{\frac{\mu}{c_N + \gamma(\sigma^2(\mu, c_N, c_S) + \sigma_\xi^2)}}{\frac{1-\mu}{c_S + \gamma(\sigma^2(\mu, c_N, c_S) + \sigma_\xi^2)} + \frac{\mu}{c_N + \gamma(\sigma^2(\mu, c_N, c_S) + \sigma_\xi^2)}} \right) (1+r-\alpha)^{-1}, \quad (27)$$

$$\theta(\mu, c_N, c_S) = \frac{1}{\frac{1-\mu}{c_S + \gamma(\sigma^2(\mu, c_N, c_S) + \sigma_\xi^2)} + \frac{\mu}{c_N + \gamma(\sigma^2(\mu, c_N, c_S) + \sigma_\xi^2)}}. \quad (28)$$

It is easily checked that the stationary solution for equation (26) is

$$p_t = \frac{\bar{d}}{r} - \frac{\theta(\mu, c_N, c_S)}{r} + \frac{\beta(d_t - \bar{d})}{1+r-\beta} + \lambda(\mu, c_N, c_S)\rho_t. \quad (29)$$

Hence, we deduce that the conditional variance of price ($Var_t(p_{t+1})$) is

$$\sigma^2(\mu, c_N, c_S) = \lambda(\mu, c_N, c_S)^2 \sigma_\varepsilon^2 + \beta^2(1+r-\beta)^{-2} \sigma_\xi^2. \quad (30)$$

The volatility of the stock price in equilibrium (and $\lambda(\mu, c_N, c_S)$) are solutions of the system of equations (27) and (30). If such a solution exists, it is independent from time since the system of equations does not depend on time. We now show that the system of equations (27) and (30) always has at least one solution.

Case 1. For $\mu = 0$, $\lambda(0, c_N, c_S) = 0$. Hence

$$\sigma^2(0, c_N, c_S) = \beta^2(1+r-\beta)^{-2} \sigma_\xi^2. \quad (31)$$

Case 2. For $\mu > 0$ and $c_N = c_S = c$, $\lambda(\mu, c_N, c_S) = \frac{\mu}{(1+r-\alpha)}$. Hence

$$\sigma^2(\mu, c, c) = \mu^2(1+r-\alpha)^{-2} \sigma_\varepsilon^2 + \beta^2(1+r-\beta)^{-2} \sigma_\xi^2. \quad (32)$$

Case 3. In other cases ($c_N \neq c_S$ and $\mu > 0$), there is no closed form solution to the system of equations (27) and (30). However, this system always has at least one solution. To see this define $\bar{\sigma}^2 = (1 + r - \alpha)^{-2}\sigma_\varepsilon^2 + \beta^2((1 + r) - \beta)^{-2}\sigma_\xi^2$, $\hat{\sigma}^2 = \beta^2((1 + r) - \beta)^{-2}\sigma_\xi^2$ and

$$\begin{aligned} g(x) &= \frac{\frac{\mu}{c_N + \gamma(x + \sigma_\xi^2)}}{\frac{1 - \mu}{c_S + \gamma(x + \sigma_\varepsilon^2)} + \frac{\mu}{c_N + \gamma(x + \sigma_\xi^2)}}(1 + r - \alpha)^{-1}, \\ f(x) &= \sqrt{\frac{x - \beta^2((1 + r) - \beta)^{-2}\sigma_\xi^2}{\sigma_\varepsilon^2}} \text{ for } x \geq \hat{\sigma}^2, \\ F(x) &= f(x) - g(x). \end{aligned}$$

It is immediate that the equilibrium level of volatility, $\sigma^2(\mu, c_N, c_S)$, is such that

$$F(\sigma^2(\mu, c_N, c_S)) = 0.$$

Now we observe that $F(\hat{\sigma}^2) < 0$ (if $\mu > 0$) and $F(\bar{\sigma}^2) > 0$. As $F(\cdot)$ is continuous, we deduce that there is at least one value of $x \in (\hat{\sigma}^2, \bar{\sigma}^2)$ such that $F(x) = 0$. Thus, there always exists at least one steady state equilibrium. Moreover, if

$$((1 - \mu)c_S + \mu c_N)^2 > 2\mu^2(1 - \mu)\sigma_\varepsilon^2\gamma(c_N - c_S), \quad (33)$$

then $F'(\cdot) > 0$. Thus, condition (33) is sufficient to guarantee the existence of a unique equilibrium. It is always satisfied if $c_S \geq c_N$. For c_N large enough compared to c_S , multiple equilibria with differing levels of volatility can exist. Our predictions however are identical across all equilibria.

A.2 Proof of Implication 1

As shown by equation (13), the effect of changing a restraint coefficient on the volatility of returns is identical to its effect on $\lambda(\mu, c_N, c_S)$. We now study this effect.

If $c_N = c_S = c$, the value of $\lambda(\mu, c_N, c_S)$ is (see the proof of Proposition 1):

$$\lambda(\mu, c, c) = \frac{\mu}{(1 + r - \alpha)}. \quad (34)$$

Moreover if $\mu = 0$, $\lambda(0, c_N, c_S) = 0$. Thus, if $c_N = c_S = c$ or $\mu = 0$, $Var(R_{t+1})$ does not depend on restraint coefficients (c_j) and the volatility of the stock return is identical in both markets. This observation yields the first part of the proposition.

Now consider the case in which $\mu > 0$ and $c_N^C \neq c_S^C$. In this case, in equilibrium, we have (see the proof of Proposition 1)

$$\lambda(\mu, c_N^C, c_S^C) = \left(\frac{\frac{\mu}{c_N^C + \gamma(\sigma^2(\mu, c_N^C, c_S^C) + \sigma_\xi^2)}}{\frac{1 - \mu}{c_S^C + \gamma(\sigma^2(\mu, c_N^C, c_S^C) + \sigma_\varepsilon^2)} + \frac{\mu}{c_N^C + \gamma(\sigma^2(\mu, c_N^C, c_S^C) + \sigma_\xi^2)}} \right) (1 + r - \alpha)^{-1}. \quad (35)$$

Thus, when $c_N^C > c_S^C$, we have $\lambda(\mu, c_N^C, c_S^C) < \mu(1+r-\alpha)^{-1}$. Now since $c_N^F = c_S^F$, $\lambda(\mu, c_N^F, c_S^F) = \mu(1+r-\alpha)^{-1}$ (see equation (34)). Thus, $\lambda(\mu, c_N^C, c_S^C) < \lambda(\mu, c_N^F, c_S^F)$ if $c_N^C > c_S^C$. A similar argument shows that $\lambda(\mu, c_N^C, c_S^C) > \lambda(\mu, c_N^F, c_S^F)$ if $c_N^C < c_S^C$. Items 1 and 2 in Implication 1 follow.

A.3 Proof of Implication 2

The absolute value of the covariance is linear in $\lambda^2(\mu, c_N, c_S)$. Thus, the proof is identical to the proof of Implication 1.

A.4 Proof of Implication 3

We have shown in the proof of Proposition 1 that

$$\sigma^2(\mu, c_N, c_S) = \lambda(\mu, c_N, c_S)^2 \sigma_\varepsilon^2 + \beta^2(1+r-\beta)^{-2} \sigma_\xi^2. \quad (36)$$

Thus, using the proof of Implication 1 and the condition $c_N^F = c_S^F$, we deduce that

1. $\sigma^2(\mu, c_N^F, c_S^F) = \sigma^2(\mu, c_N^C, c_S^C)$ if $c_N^C = c_S^C$ or $\mu = 0$.
2. $\sigma^2(\mu, c_N^F, c_S^F) > \sigma^2(\mu, c_N^C, c_S^C)$ if $c_N^C > c_S^C$ and $\mu > 0$.
3. $\sigma^2(\mu, c_N^F, c_S^F) < \sigma^2(\mu, c_N^C, c_S^C)$ if $c_N^C < c_S^C$ and $\mu > 0$.

As $\Upsilon(\mu, c_N^k, c_S^k) \stackrel{def}{=} \frac{(c_S^k + 2\gamma(\sigma^2(\mu, c_N^k, c_S^k) + \sigma_\xi^2))}{(1-\mu)(1+r-\alpha)}$, Implication 3 immediately follows if $c_S^C = c_S^F$.

B Tables

Table 1: Summary Statistics

	Control			Treated		
	Mean	St. Dev.	Obs.	Mean	St. Dev.	Obs.
Panel A						
Log(Market capitalization)	17.4	1.5	26,030	21.0	1.5	7,549
Daily nber of shares traded/outstanding (%)	0.10	0.09	26,189	0.22	0.12	7,549
S.d. of daily returns	0.029	0.017	22,434	0.025	0.012	7,823
S.d. of daily (return-mkt)	0.028	0.011	22,468	0.023	0.008	7,826
S.d. of daily abn. returns	0.025	0.011	22,468	0.021	0.008	7,826
Daily Amihud ratio ($\times 10^6$)	2.6	3.7	26,374	0.1	0.7	8,036
Autocovariance of daily returns ($\times 10^4$)	-0.49	2.00	20,472	-0.27	1.5	7,686
Bid Ask Spread / Midquote	0.05	0.04	7,701	0.02	0.01	2,895
Panel B						
Share of small trades	0.93	0.21	30,170	0.87	0.13	15,010
Share of large trades	0.04	0.16	30,170	0.09	0.11	15,010

Notes: For variable definitions, see main text. In Panel A, each observation corresponds to a stock-month, for all months from twenty-four months prior to the reform until twenty-four months after the reform. In Panel B, each observation corresponds to a stock-day, for all trading days from August 1, 2000 until October 30, 2000. Sample means and standard deviations are computed for the sample of stock - months corresponding to treated and control stocks separately.

Table 2: The distribution of trades size before and after the reform

($\times 100$)	% small transactions			% large transactions		
	(1)	(2)	(3)	(4)	(5)	(6)
POST \times Treated	-1.4*** (0.5)	-1.4*** (0.4)	-1.3*** (0.5)	1.6*** (0.4)	1.6*** (0.3)	0.9*** (0.4)
POST	-0.4 (0.5)	-0.4 (0.3)	0.1 (0.4)	0.2 (0.3)	0.2 (0.2)	0.1 (0.3)
Treated	-4.4*** (0.9)	-	-	3.8*** (0.7)	-	-
Score $\in [0.1; 0.9]$	No	No	Yes	No	No	Yes
Stock FE	No	Yes	Yes	No	Yes	Yes
Observations	45,180	45,180	13,935	45,180	45,180	13,935
<i>Adj. R</i> ²	0.02	0.28	0.16	0.02	0.26	0.13

Notes: In columns (1) and (4), this table reports the results for the estimation of

$$Y_{it} = \alpha + \beta_0 T_i + \beta_1 POST_t + \beta_2 T_i \times POST_t + \varepsilon_{it},$$

with (i) the daily fraction of small trades as dependent variable (column 1), and (ii) the daily fraction of large trades as dependent variable (column 4). In columns (2) and (5), we report estimates of the same equation but we include stock fixed effects, as explained in the text. In columns (3) and (6), we report estimates of the specification with stock fixed effects for the restricted sample only. In columns (1) and (4), error terms are clustered at the stock level. In columns (2), (3), (5) and (6) they are clustered at the stock \times POST level, due to the presence of stock fixed effects. Superscripts *, **, and *** means statistically different from zero at 10%, 5% and 1% levels of significance, respectively.

Table 3: The effect of the reform on return volatility

($\times 100$)	Monthly s.d. (ret-mkt)		
	(1)	(2)	(3)
POST \times Treated	-0.26*** (0.04)	-0.18*** (0.03)	-0.11*** (0.04)
POST	0.14*** (0.02)	0.04*** (0.02)	0.06** (0.03)
Treated	-0.37*** (0.04)	-	-
Stock FE	No	Yes	Yes
Score $\in [0.1; 0.9]$	No	No	Yes
Observations	30,294	30,294	8,229
<i>Adj. R</i> ²	0.05	0.38	0.32

Notes: Column (1) of this table reports estimates of

$$Y_{it} = \alpha + \beta_0 T_i + \beta_1 POST_t + \beta_2 T_i \times POST_t + \varepsilon_{it},$$

with volatility as a dependent variable. For a given stock, volatility is measured as the monthly average of the standard deviation of daily returns minus the market return. Column (2) reports estimates of a similar specification with stock fixed effects (see the main text). Column (3) reports estimates with stock fixed effects for the restricted sample only. In column (1), error terms are clustered at the stock level. In columns (2) and (3) they are clustered at the stock \times POST level, due to the presence of stock fixed effects. Superscripts *, **, and *** means statistically different from zero at 10%, 5% and 1% levels of significance.

Table 4: The effect of the reform on the autocovariance of stock returns

($\times 10^4$)	Autocovariance of Returns		
	(1)	(2)	(3)
POST \times Treated	0.11** (0.05)	0.13*** (0.04)	0.20*** (0.07)
POST	-0.25*** (0.03)	-0.30*** (0.03)	-0.38*** (0.05)
Treated	0.16*** (0.05)	-	-
Stock FE	No	Yes	Yes
Score $\in [0.1; 0.9]$	No	No	Yes
Observations	28,158	28,158	7,919
<i>Adj. R</i> ²	0.01	0.08	0.07

Notes: Column (1) of this table reports estimates of

$$Y_{it} = \alpha + \beta_0 T_i + \beta_1 POST_t + \beta_2 T_i \times POST_t + \varepsilon_{it},$$

with the autocovariance of stock returns as a dependent variable. Specifically, for a given stock, the dependent variable is the monthly average of the autocovariance of daily returns. Column (2) reports the estimates of a similar specification but with stock fixed effects. Column (3) reports estimates of the specification with stock fixed effects using the restricted sample only. In column (1), error terms are clustered at the stock level. In column (2) and (3), they are clustered at the stock \times POST level, due to the presence of stock fixed effects. Superscript *, **, and *** means statistically different from zero at 10%, 5% and 1% levels of significance.

Table 5: The effect of the reform on price impact

	Amihud ratio		
	(1)	(2)	(3)
POST×Treated	-0.06 (0.05)	-0.13** (0.06)	-0.57*** (0.10)
POST	0.15** (0.08)	0.23*** (0.05)	0.63*** (0.10)
Treated	-2.46*** (0.09)	-	-
Stock FE	No	Yes	Yes
Score ∈ [0.1; 0.9]	No	No	Yes
Observations	34,410	34,410	8,860
<i>Adj. R</i> ²	0.09	0.41	0.31

Notes: Column (1) of this table reports estimates of

$$Y_{it} = \alpha + \beta_0 T_i + \beta_1 POST_t + \beta_2 T_i \times POST_t + \varepsilon_{it},$$

with the Amihud ratio as a dependent variable. Specifically, for a given stock, the dependent variable is the monthly average of the Amihud ratio of the stock (the daily absolute return divided by the daily trading volume). We multiply this ratio by 10^6 for the estimation. Column (2) reports the estimates of a similar specification with stock fixed effects. Column (3) reports estimates of the specification with stock fixed effects for the restricted sample only. In column (1), error terms are clustered at the stock level. In columns (2) and (3), they are clustered at the stock × POST level, due to the presence of stock fixed effects. Superscripts *, **, and *** means statistically different from zero at 10%, 5% and 1% levels of significance.

Table 6: Changing the sample periods

	-12;+12 months			-18;+18 months		
	Volat	Amihud Ratio	Autocov	Volat	Amihud Ratio	Autocov
POST \times Treated	-0.06 (0.04)	-0.58*** (0.09)	0.04 (0.08)	-0.10*** (0.04)	-0.55*** (0.10)	0.15** (0.07)
POST	0.01 (0.03)	0.62*** (0.09)	-0.16*** (0.06)	0.11*** (0.03)	0.61*** (0.10)	-0.29*** (0.06)
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes
Score \in [0.1; 0.9]	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,261	4,260	4,077	6,285	6,781	6,052
<i>Adj. R</i> ²	0.41	0.36	0.11	0.37	0.34	0.08

Notes: In this table we estimate equation

$$Y_{it} = \alpha_i + \beta_1 POST_t + \beta_2 T_i \times POST_t + \varepsilon_{it},$$

using different sample periods. In all cases we use only the restricted sample of stocks. The first three columns consider the case in which the sample period is equal to twenty-four month (twelve before the reform and twelve after). The last three columns report the estimates when the sample period is equal to thirty six months (eighteen before the reform and eighteen after). In columns (1) and (4), the dependent variable is the monthly average of the standard deviation of the daily stock return minus the market return. In columns (2) and (5), the dependent variable is the monthly average of the daily Amihud ratio (multiplied by 10^6). In columns (3) and (6), the dependent variable is the monthly average of the autocovariance of daily returns. In all regressions, error terms are clustered at the stock \times POST level. Superscripts *, **, and *** means statistically different from zero at 10%, 5% and 1% level of significance.

C Figures

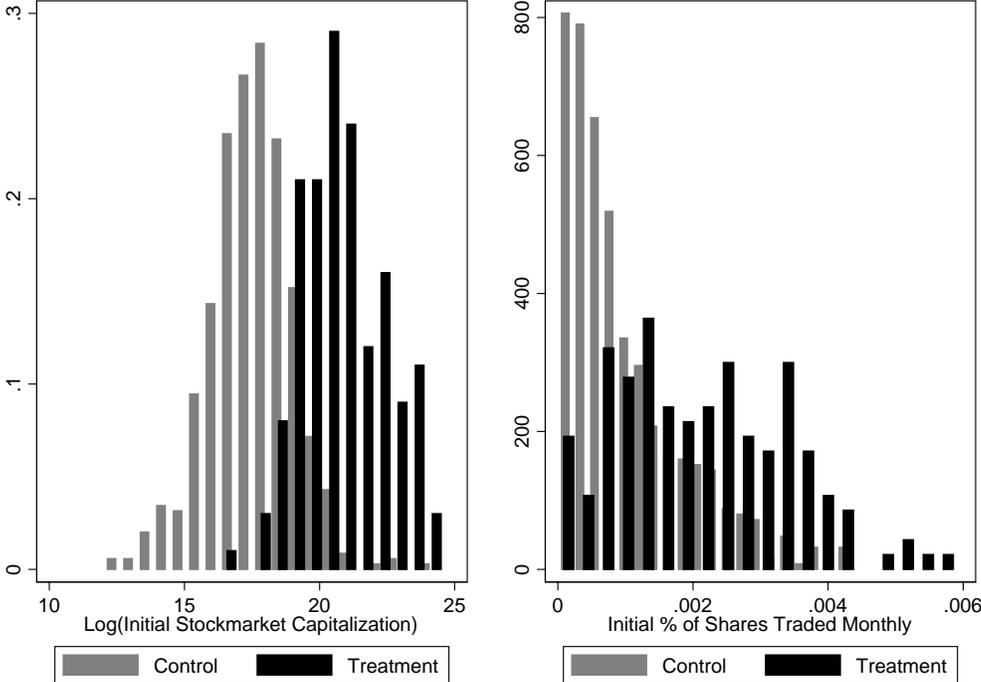


Figure 1: Distribution of Market Capitalization and Turnover

This figure gives the distribution of the log of market capitalization and the turnover (average number of shares traded divided by number of outstanding shares) for treated stocks (black bars) and control stocks (grey bars) in September 2000.

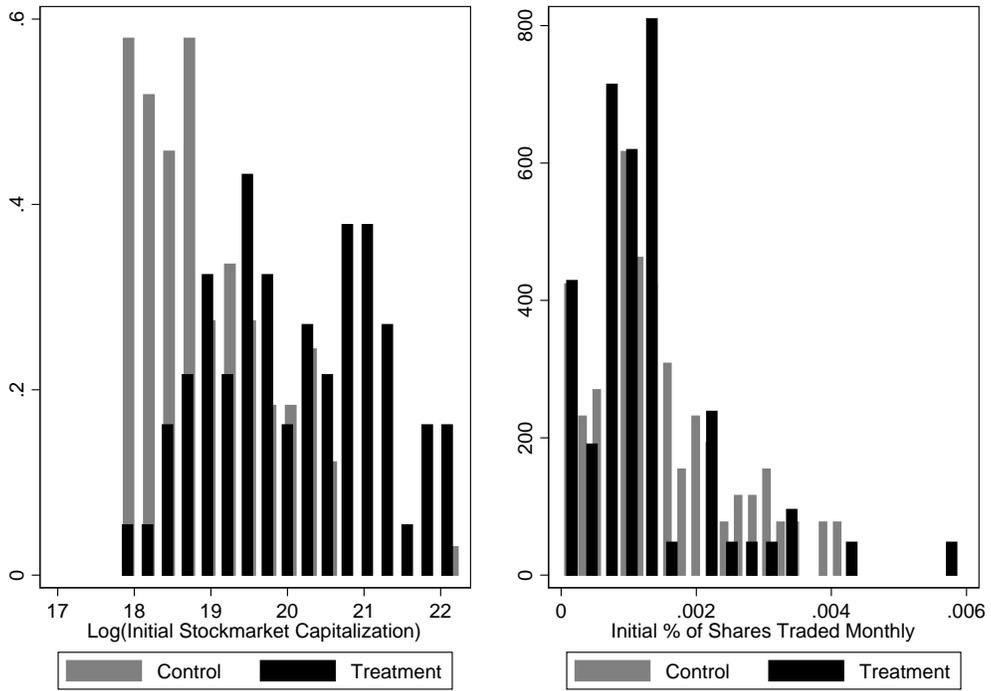


Figure 2: Distribution of Market Capitalization and Turnover: restricted sample

This figure gives the distribution of the log of market capitalization and the turnover (average number of shares traded divided by number of outstanding shares), as of September 2000, for treated stocks (black bars) and control stocks (grey bars) in the restricted sample.

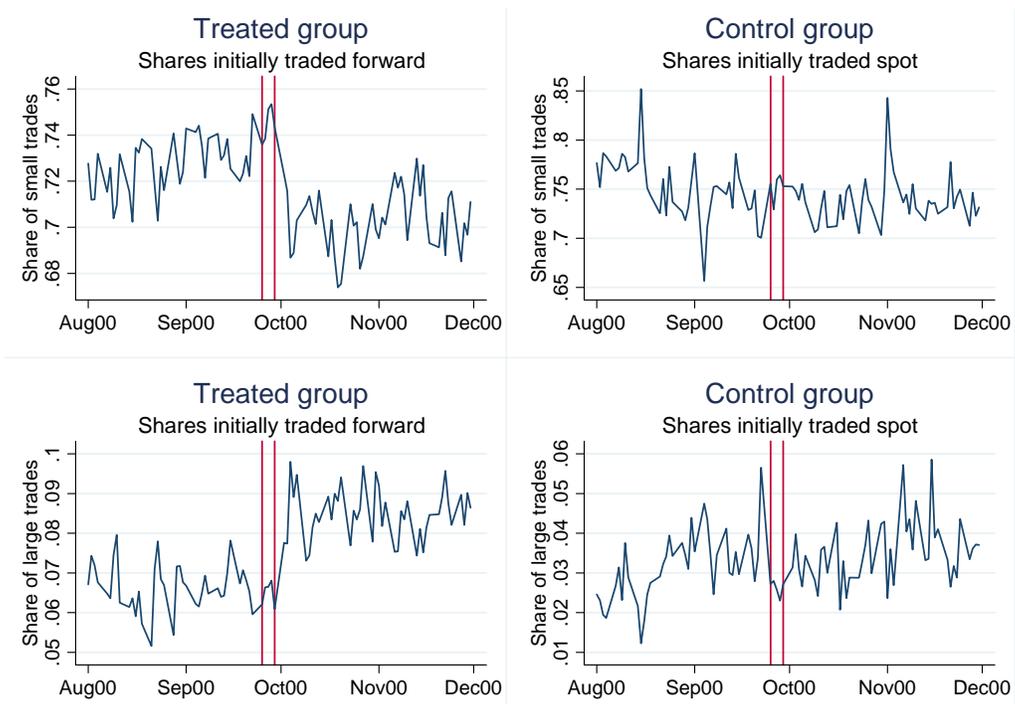


Figure 3: The fraction of small and large trades

This figure gives the evolution of the cross-sectional averages of the fractions of small trades (upper panel) and large trades (lower panel) from August 1, 2000 to December 31, 2000 for the sample of treated stocks and the sample of control stocks.

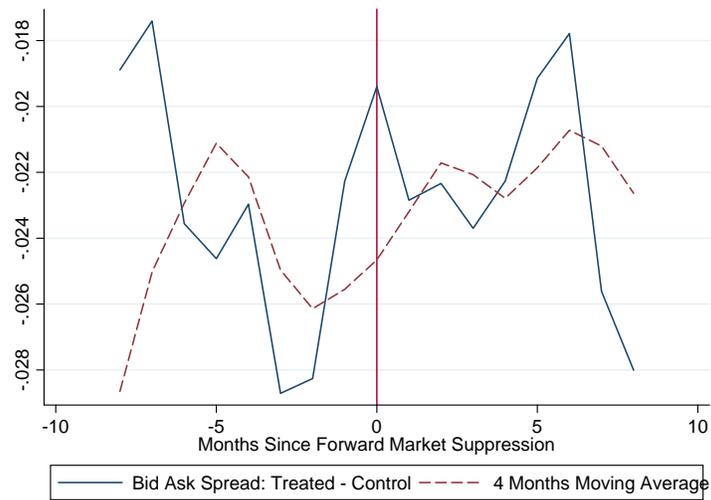


Figure 4: Bid-Ask Spread Around the reform

This figure represents the evolution of (i) the monthly difference between the average bid-ask spread of treated stocks and the average bid-ask spread of control stocks, from ten months before the reform to ten months after the reform (plain line) and (ii) a four months moving average of this difference (dashed line).

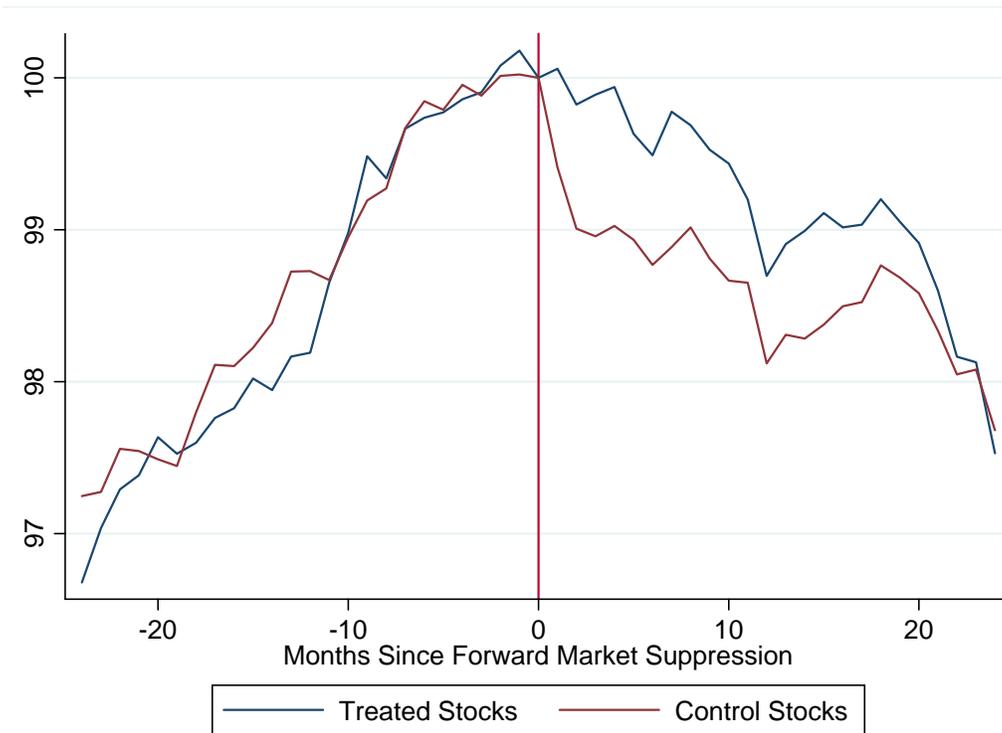


Figure 5: Market capitalizations around the reform

This figure represents the evolution of the average market capitalization of treated stocks (red line) and control stocks (blue line) from twenty months before the reform to twenty months after the reform. Prices of all stocks have been normalized to 100 September 2000.

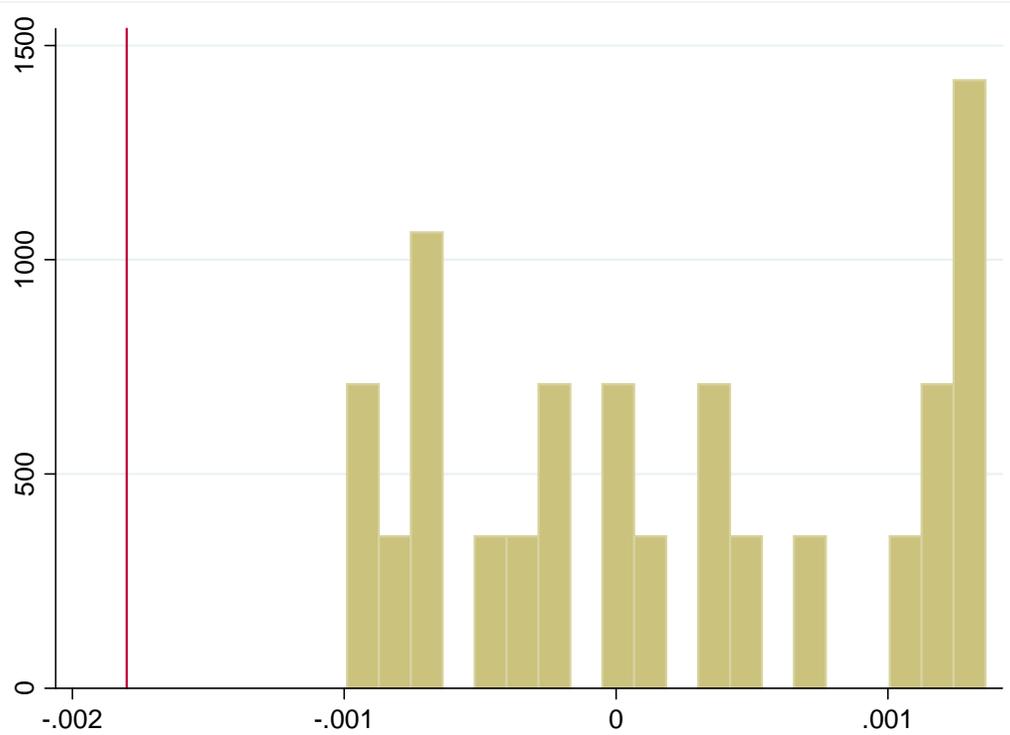


Figure 6: Placebos for volatility

This figure gives the distribution of β_2 when equation (22) is estimated using each month from January 1996 to December 1997 as a placebo experiment and using volatility as the dependent variable.