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# Financial transaction taxes, market composition, and liquidity

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**ABSTRACT**

We use the introduction of a financial transaction tax (FTT) in France in 2012 to test competing theories on its impact. We find no support for the idea that an FTT improves market quality by affecting the composition of trading volume. Instead, our results are in line with the hypothesis that a lower trading volume reduces liquidity, and thereby market quality. Consistent with theories of asset pricing under transaction costs, we document a shift in security holdings from short-term to long-term investors. Finally, our findings show that moderate aggregate effects on market quality can mask large adjustments made by individual agents.



Keywords: Financial transaction tax, institutional trading, liquidity, high-frequency trading

# Non-technical summary

This paper empirically examines the 2012 introduction of a financial transaction tax (FTT) on equity trading in France. We motivate our analysis by contrasting two opposing strands of theoretical literature that make opposing predictions on the impact of such as policy. Models based on “composition effects” suggest that FTTs will primarily affect agents that are the source of excessive stock price volatility (so-called noise traders), and thus improve market quality. However, other researchers question the relevance of this channel and instead highlight a “liquidity effect” with opposing consequences. They argue that reduced market participation lowers liquidity, which makes markets more volatile and prices less efficient as arbitrageurs face higher costs for correcting price dislocations.

Our empirical analysis shows that, following the policy change, average trading volume decreased by around 10%, and this development was accompanied by a moderate decline in market quality. Moreover, the FTT’s impact was heterogeneous. While the most liquid stocks were largely unaffected, less liquid securities were subject to considerable adverse effects. Using data that enables the identification of different types of market participants, we further show that high-frequency traders were strongly affected by the decline in market liquidity, even though these agents were effectively tax-exempt because the French FTT does not apply to intraday trading. In sum, these findings support theories highlighting the “liquidity effect”, and reject the idea that FTTs can help improve market quality by altering the composition of the trader population.

Finally, we use data on institutional portfolios to shed light on two different mechanisms through which agents react to changes in transaction costs (such as FTTs). Consistent with the model of Amihud and Mendelson (1986), we find that investors with a high portfolio turnover reduced their holdings of French stocks, whereas investors with a low turnover increased them. Moreover, in line with Constantinides (1986) and Vayanos (1998), investors also significantly decreased their turnover. This second effect was particularly strong for investors with a high trading activity. These findings suggest that the FTT’s muted impact on the most liquid French stocks masks significant changes in the individual portfolio allocations and trading behaviour. Following Constantinides (1986), this is a simple consequence of agents adapting their behaviour to the tax in order to minimize its impact.

Our evidence also informs the policy debate on FTTs, which are often motivated by a mix of fiscal (raising revenue) and Pigovian (correcting externalities) motives. While we fail to find evidence in favour of the Pigovian rationale, the official revenue figures show that the FTT also fell short of expectations in this dimension.[1] At the same time, the negative effects were rather muted for the most liquid securities, which account for most of the revenue. Moreover, micro-level data suggests that the policy led to a re-allocation of shareholdings to investors with a longer investment horizon, an effect that can potentially improve the corporate governance of the affected companies.

[1] The French FTT yielded 198 million EUR in tax revenues for August-December 2012 (see http://www.assemblee-nationale.fr/14/rap-info/i1328.asp), which is considerably less than the initial projection of 1.1 bln EUR per year (see http://www.senat.fr/rap/r12-259/r12-2591.pdf).

The global financial crisis has renewed interest in financial transaction taxes (FTTs), a development that has been fueled by the combination of strapped public finances and public discontent with the financial industry.[1] Since suggested by Keynes (1936), the idea of taxing trading activity has been at the center of a more fundamental debate on whether there is too much trading in financial markets.

Indeed, proponents of FTTs like Stiglitz (1989) argue that markets are populated by too many noise traders whose actions are not based on information and thus generate "excess volatility". According to this view, an FTT improves market quality by reducing the proportion of such "futile" trading. Opponents to FTTs question the relevance of this *composition effect*. Moreover, they argue that it is dominated by a *liquidity effect*: Reduced participation, even by uninformed agents, has a negative effect on liquidity. This prevents the correction of mispricings by arbitrageurs, thereby increasing volatility and reducing price efficiency.

The theoretical literature, surveyed in Section I, has rationalized these different arguments. In line with this tension, empirical work has delivered mixed conclusions.[2] This is largely a consequence of the fact that existing studies have focused on the aggregate impact of FTTs, mainly due to a lack of granular data. Accordingly, they have not been able to shed light on the individual roles of composition and liquidity effects, but just on the sum of both parts.

This paper aims to bridge this gap between the existing theoretical and empirical work by examining the introduction of a 20 bps tax on the purchase of French equities on August 1st, 2012. We assess the FTT's causal impact using a difference-in-differences framework. Importantly, we estimate treatment effects for different types of market participants, which allows us to assess the relative roles of composition and liquidity effects.

Our analysis starts out with an overview of the French FTT's aggregate impact. We document that trading volume decreased on average by around 10%, accompanied by a moderate decline in market quality. However, the overall impact masks some significant heterogeneity across stocks. We find that stocks under Euronext's "Supplemental Liquidity Provider" (SLP) programme, a rebate scheme aimed at incentivizing high-frequency traders (HFTs) to provide liquidity in the most active stocks, were only marginally affected by the FTT. In contrast, the remaining stocks displayed a strong reduction in trading volume (-20%) and a significant decline in market quality. This discrepancy suggests that an FTT's impact depends on the affected stock's level of liquidity.

We proceed to analyzing the FTT's impact on different trader types using a dataset that groups market participants into three distinct categories according to their speed. We find that, despite being effectively tax-exempt, HFTs were the most affected trader type with a decline in trading activity of 35%. This indirect impact constitutes strong evidence in favor of a significant liquidity effect. We further provide evidence suggesting that HFTs were affected due to i) an increase in bid-ask spreads hurting the profitability of market orders, as well as ii) an overall decline in trading activity, which decreased opportunities for arbitrage or intermediation.

Finally, we use theories of asset pricing under transaction costs in order to explore the two different mechanisms through which FTTs can affect trading volume. First, investors can react to higher transaction costs by keeping the same portfolio allocation, but adjusting their turnover (intensive margin). Second, a tax can encourage some investors to sell the affected stocks to other market participants with a longer holding horizon (extensive margin). In order to separate these two effects, we examine the portfolio holdings and turnover of institutional investors and find support for both channels. Consistent with the model of Amihud and Mendelson (1986), we find that investors with a high portfolio turnover reduced their holdings of French stocks, whereas investors with a low turnover increased them. Moreover, in line with Constantinides (1986) and Vayanos (1998), investors also significantly decreased their turnover. We show that this effect was stronger for investors with a high trading activity.

A striking result of our analysis is that the rather muted impact of the tax on the market quality of the most liquid French stocks masks significant changes in the portfolio allocations and trading behavior of the affected market participants. This discrepancy is consistent with the model of Constantinides (1986). Intuitively, agents adjust to the tax in order to minimize its impact. As a result, the effect of the tax on aggregate variables is of second order relative to its effect on individual strategies. Consequently, understanding the full impact of an FTT requires analyzing the reaction of individual market participants, and not only changes in aggregate measures of market quality. To our knowledge, the French FTT is the first policy experiment for which data availability permits steps in this direction.

Besides being important for understanding the impact of transaction costs in securities markets in general, our evidence also informs the policy debate on transaction taxes. FTTs are frequently motivated by a mix of fiscal (raising revenue) and Pigovian (correcting externalities) motives. While we fail to find evidence in favor of the Pigovian rationale, the official revenue figures show that the FTT also fell short of expectations in this dimension.[3] While the tax had an overall negative impact on market quality, this effect was rather muted for the most liquid stocks. Moreover, the tax had an important effect on the clientele of French stocks that requires looking beyond aggregate measures of market quality. It reallocated the ownership of French stocks towards more long-term institutional investors, an effect that can potentially improve the corporate governance of the affected companies.[4] The impact on market quality for less liquid stocks, however, was considerably more negative.

This paper coincides with a number of other empirical studies of the French FTT which, despite using a variety of different control groups, reach remarkably similar conclusions concerning the FTT's aggregate impact.[5] In addition, Meyer, Wagener, and Weinhardt (2015) show that the FTT's impact is roughly comparable across different trading venues. Gomber, Haferkorn, and Zimmermann (2016) focus on price discovery and document a decrease in market integration following the policy experiment. Coelho (2014) provides additional evidence from the introduction of the Italian FTT in 2013. Our paper has a different focus. We aim at shedding light on the FTT's impact on different market participants and the underlying economic mechanisms, which is facilitated by our more granular data.

The remainder of this paper is organized as follows. Section I details the policy experiment, our main testable hypotheses, and our identification strategy. Section II presents our results on aggregate market quality. Section III provides additional evidence on the relevance of the liquidity effect by assessing the FTT's impact on different trader types, while Section IV considers the impact on the portfolios and trading volumes of different institutional investors. Section V concludes.

# I. Hypothesis development and methodology

## A. *The policy experiment*

On August 1st, 2012, France introduced an FTT of 20 bps on stock purchases. This tax applies to shares of all listed companies incorporated in France with a market capitalization above one billion euros on December 1st of the previous year,[6] to trades on any trading platform as well as in the over-the-counter market, and to all investors, irrespective of their country of residence.[7]

Importantly, the tax is payable on daily net position changes (i.e., ownership transfers), which implies that pure intraday trading is de facto exempted (similar to the British stamp duty). In addition, the tax does not apply to newly emitted shares, to transactions by clearing houses, to employee stock ownership plans and, most importantly, to market makers.[8]

Simultaneously, the government introduced a tax of 1 bp on the notional amount of modified or cancelled messages by HFTs exceeding an order-to-trade ratio of 5:1. Unlike the FTT, this tax applies to trading in all French stocks. However, it is only levered on HFTs residing in France, thereby excluding all major HFT firms. Moreover, message traffic due to market-making is exempt. Accordingly, the scope of this policy is extremely limited, and the French securities markets regulator itself described its impact as "minimal" (see Megarbane (2013)).[9] Thus, we henceforth consider the policy experiment to have only consisted of the FTT.

## *B. Hypothesis development*

Many arguments in favor and against FTTs have been made since the idea was floated by Keynes (1936), either through the lens of theoretical models or more informally in a number of essays. Arguments supporting such a tax broadly rely on the idea that it will correct existing market inefficiencies by changing the composition of the trader population. However, opponents of FTTs contend the economic significance as well as the general desirability of this "composition effect". Instead, they warn of a negative "liquidity effect" associated with a widespread decrease in trading activity due to an increase in transaction costs. Most of the debate revolves around the relative importance of these two countervailing mechanisms and their combined impact. In the following, we briefly contrast both perspectives and the competing hypotheses they imply concerning the effects of FTTs on market quality.

### *B.1. Composition vs. liquidity effects*

Loosely speaking, proponents of a tax argue that the trading activity of some market participants constitutes a negative externality. At the same time, these agents are assumed to be particularly sensitive to transaction costs. Accordingly, an FTT will affect them disproportionately and thus help to reduce the existing "pollution" in the market. For example, Stiglitz (1989) postulates that an FTT has a stronger effect on noise traders than on agents with accurate fundamental information. The resulting change in the trader population is expected to decrease (non-fundamental) volatility and improve price efficiency.[10] Similar in spirit, Summers and Summers (1989) argue that frequent trading is the essence of "positive feedback traders" (e.g., trend followers) who tend to amplify price swings. Hence, an FTT will decrease their activity to a larger extent than that of traders who base their decisions on fundamentals.

The opposing view relies on the existence of strong liquidity externalities in financial markets (see, e.g., Pagano (1989)), resulting in indirect effects of FTTs. For example, Schwert and Seguin (1993) suggest that such a policy will lead to wider bid-ask spreads due to decreased turnover, increased hedging costs, and a possible increase in adverse selection. Likewise, Ross (1989) argues that the absence of short-term traders will hurt liquidity by increasing the role of costly inventory positions for the provision of liquidity. As a consequence, arbitrageurs will find it more costly to correct mispricings, whose magnitude and persistence are therefore expected to increase. This implies that prices become more volatile and less efficient.

Song and Zhang (2005) present an equilibrium model in which both effects are present, and the net effect of an FTT depends on their relative strength.[11] If, for example, the liquidity effect is sufficiently strong, informed traders reduce their trading activity in a response to lower liquidity. In this case, volatility increases and price efficiency declines.

Based on these two polar views, we arrive at the following competing hypotheses concerning the FTT's impact on aggregate market quality.

**HYPOTHESIS 1A** (Composition effect dominates)**.** *The French FTT led to a decrease in trading volume and volatility, and an increase in price efficiency.*

**HYPOTHESIS 1B** (Liquidity effect dominates)**.** *The French FTT led to a decrease in trading volume, liquidity, and price efficiency, and an increase in volatility.*

Estimating an FTT's impact on aggregate variables provides useful information only on the relative strength of these two effects. Fortunately, the institutional design of the French FTT allows for a more direct test of the presence and magnitude of the liquidity effect. As mentioned in the previous section, the French FTT only applied to ownership transfers and thus implicitly exempted HFTs. Accordingly, any impact of the FTT on their trading activity must be due to a liquidity effect.

**HYPOTHESIS 2** (Liquidity effect)**.** *The French FTT led to a decrease in tax-exempt HFT trading volume.*

## *B.2. Clientele effects*

While both Hypotheses 1A and 1B predict that an FTT reduces trading volume, the theoretical literature on asset pricing under transaction costs highlights two different mechanisms through which this occurs. First, investors can simply scale back their trading activity and thus reduce their portfolio turnover ("turnover adjustment"). This mechanism is at the center of the equilibrium models of Constantinides (1986) and Vayanos (1998). Second, investors with a high portfolio turnover can reduce some of their holdings of affected securities by selling them to market participants with a lower portfolio turnover ("holdings adjustment"). This "clientele effect" features prominently in the work of Amihud and Mendelson (1986), who show that, in equilibrium, assets with higher transaction costs are held by investors with longer average holding periods. These theoretical mechanisms lead to the following two hypotheses, which will be tested using data on institutional portfolio snapshots.

**HYPOTHESIS 3A** (Holdings adjustment)**.** *Investors reacted to the French FTT by adjusting their portfolio holdings in affected securities. In particular, market participants with a long investment horizon increased their holdings in affected securities relative to short-term investors.*

**HYPOTHESIS 3B** (Turnover adjustment)**.** *Investors reacted to the French FTT by reducing their portfolio turnover in affected securities.*

Notice that these two hypotheses are not to be taken as competing, but rather as complementary. In practice, investors can use both margins of adjustment, and their relative importance is ultimately an empirical question that we aim to shed light on. Importantly, the mentioned "clientele effect" also features prominently in the literature on FTTs. Indeed, Keynes (1936), Stiglitz (1989) and Summers and Summers (1989) suggest that the resulting holdings adjustment will increase the influence of long-term shareholders and lead to better corporate decisions.

However, it is not entirely clear whether the same effects can be expected to arise if investors mainly adjust via their portfolio turnover. While this mechanism also leads to an increase in the average holding period for the affected securities, it is less obvious that it will at the same time induce investors to become more involved in corporate governance.

### *C. Identification strategy and data*

We adopt a simple difference-in-differences approach for identifying the FTT's causal impact on market quality and the trading volume of different market participants. To this end, we compare treated French stocks to a group of non-treated control stocks that are otherwise as similar as possible.

As much of our work relies on high-frequency data, it is important to ensure that the data for both groups stem from the same microstructural environment, including the trading protocol, the tick size regime, and the fee structure. The last point is of particular importance because part of our analysis makes use of a rebate scheme for limit orders offered by Euronext, the primary market for French stocks. Fortunately, Euronext also constitutes the main trading venue for Belgian, Dutch and Portuguese stocks. Moreover, the Luxembourg Stock Exchange also uses Euronext's Universal Trading Platform (UTP) as part of a cross-membership cooperation. Accordingly, non-French stocks listed on Euronext (as of the cutoff date January 1st 2012) form a natural pool of control stocks for our diff-in-diff analysis.

We define our final sample of stocks as follows. We start by collecting all the constituents of the Euronext 100 and Euronext Next 150 Indices, which represent the 250 most liquid stocks listed on Euronext. Because Belgium increased its pre-existing FTT on August 1st 2012 and Portugal was heavily affected by the sovereign debt crisis, we restrict our sample to stocks registered in France, Luxembourg and the Netherlands. We also drop bank stocks because they were strongly affected by the prevailing crisis environment and are concentrated among French stocks. Further, we require stocks to have traded at least 20 times a day in order to ensure a minimum level of liquidity. Finally, we drop two stocks due to takeover activity, so that our final sample comprises of 168 stocks.

Our first specification consists in comparing all French stocks with a market capitalization above 1 bln EUR and thus affected by the FTT (87 stocks) to the non-French stocks above the same threshold (32 stocks). In a second step, we separately estimate the FTT's impact on stocks that were part of Euronext's SLP programme and those that were not. We have 49 French stocks that were subject to the FTT and at the same time part of the SLP programme, for which the 27 non-French SLP stocks form a natural control group. For the 38 French non-SLP stocks affected by the tax, there are only 5 non-French counterparts above the 1 bln threshold. To form a control group of sufficient size, we thus additionally include 47 non-SLP stocks below the 1 bln EUR (30 French and 17 non-French). This identification method is similar in spirit to using a regression discontinuity design around the 1 bln EUR cutoff. Table I summarizes the different groups of stocks we are using.

[Insert Table I here.]

Importantly, practitioners, government officials and regulators advised us in private conversations that the trading activity in August was unlikely to correctly reflect the impact of the policy change because of i) temporary (legal) uncertainty among investors on whether they were subject to the tax or not and ii) a seasonal decline in trading activity for French stocks due to country-wide summer holidays. In order to take such a possibility into account, we use a 5-month sample period from June to October 2012 (109 trading days) and opt for a flexible framework that allows the treatment effect in the first month after the policy change (i.e., August) to be potentially different from the impact in September and October.[12] Formally, the assumption underlying our approach is that for each stock $i$ and date $t$ the variable of interest, $y_{i,t}$, satisfies the following equation.

$$\mathbb{E}(y_{i,t} \mid i, t) = \alpha_i + \gamma_t + \beta^{Aug} D_{i,t}^{Aug} + \beta^{Sep/Oct} D_{i,t}^{Sep/Oct}, \tag{1}$$

where $D_{i,t}^{Aug}$ and $D_{i,t}^{Sep/Oct}$ are dummy variables that take the value of one for treated stocks in August and September/October, respectively, and zero otherwise. The coefficient $\beta^{Sep/Oct}$ captures the permanent impact of the treatment.

This specification relies on the standard common trends assumption that the variables of interest for both groups of stocks should co-move closely absent any treatment. A common issue concerning the difference-in-differences methodology is that this assumption cannot be tested formally. However, the Online Appendix provides some "placebo" tests that have become customary in the literature on policy evaluation. Together with visual inspection of the data series shown in Appendix A and the high correlations between the treated and the control group in the pre-event window (reported in the Online Appendix), these tests confirm the validity of our control group.

Our empirical tests are based on data from several sources. We obtain millisecondstamped intraday data for the market activity (trades and quotes) on Euronext from *Thomson Reuters Tick History*, which we use to compute a wide range of microstructure variables at the stock-day level (see Section II). Further, we were granted access to the *BEDOFIH* database, which assigns each side of every transaction to one out of three trader categories: HFT, non-HFT, and Mixed Traders (MTs). Finally, we obtain data on institutional investment portfolios from *Factset* in order to gauge the reaction of individual funds to the tax. These data are described in more detail in Sections III and IV, respectively.

# II. The effect on market quality

In this section, we examine the FTT's impact on aggregate market quality on the background of the competing Hypotheses 1A and 1B. To this end, we focus on the measures emphasized by the theoretical literature: trading activity, volatility, price efficiency, and liquidity.

## *A. Measures of market quality*

First, we briefly detail the different variables that we construct in order to test the predictions from the literature. All measures are computed at the stock-day level using intraday data from Euronext's limit order book.[13] We discard trades that are executed off-book, during call auctions and the "trading-at-last" period.

Trading activity. We measure trading activity by the natural logarithm of the EUR value traded for stock $i$ on day $t$, denoted $log\ volume_{i,t}$.

Volatility. We use two different variables in order to assess intraday price volatility. We define $realized\ volatility_{i,t}$ as the sum of squared returns based on the final mid-quote of 5-min intervals. This measure is annualized through multiplication with a factor of $\sqrt{252}$. Alternatively, we compute $range_{i,t}$ as the intraday price range across all trades, normalized by the average traded price. Both measures are expressed in percentage points.

Price efficiency. We measure price inefficiency as the absolute value of first-order return autocorrelations, $abs.\ autocorrelation_{i,t}$, based on the final mid-quote of 5-min intervals. Intuitively, efficient prices should be unpredictable, and both positive and negative autocorrelations indicate deviations from a random walk process.

Liquidity. We use a number of variables in order to capture all three dimensions of liquidity as defined by Kyle (1985): Tightness, depth, and resiliency. A standard measure of tightness is the quoted relative half-spread, which we compute as a weighted average across all time intervals on day $t$ and denote by $quoted\ spread_{i,t}$. The relative $effective\ spread_{i,t}$ measures the spread at the time of a transaction and thus reflects the actual trading cost incurred. We compute this variable as an equal-weighted average across all trades in a given stock across the entire trading day. The effective spread for the $\tau$-th trade in a given stock is defined as

$$effective\ spread_\tau = q_\tau \frac{p_\tau - mid_\tau}{mid_\tau}, \tag{2}$$

where $p_\tau$ denote the transaction price, $mid_\tau$ refers to the contemporaneously prevailing mid-quote, and $q_\tau$ is a buy-sell indicator taking the value of 1 (-1) for buys (sells).[14] It is useful to decompose the effective spread into two separate components, $price\ impact_{i,t}$, and

$realized\ spread_{i,t}$. For the $\tau$-th trade, these are defined as:

$$price\ impact_\tau = q_\tau \frac{mid_{\tau+5min} - mid_\tau}{mid_\tau},\ realized\ spread_\tau = q_\tau \frac{p_\tau - mid_{\tau+5min}}{mid_\tau}, \qquad (3)$$

where $mid_{\tau+5min}$ denotes the mid-quote 5 minutes after the transaction. The price impact is frequently interpreted as a measure of adverse selection, while the realized spread is often taken as a proxy for the revenues of liquidity providers.

While spreads are relevant to estimate the trading costs of small transactions, a larger trader will also take into account the quoted depth, denoted $depth_{i,t}$. This measure is computed as the sum of the available liquidity at the inside spread (both bid and ask side), weighted by time and measured in thousands of EUR.

Finally, resiliency measures the speed at which depth is replenished over time following a shock to liquidity. We follow Kempf, Mayston, and Yadav (2009) and estimate the following dynamic model for each stock-day,

$$\Delta depth_\tau = \alpha - \kappa \cdot depth_\tau + \sum_{k=1}^{K} \gamma_k \Delta depth_{\tau-k} + \epsilon_\tau, \qquad (4)$$

where $depth_\tau$ denotes quoted depth at the end of the $\tau$-th time interval, and $\Delta$ is the first-difference operator. Our estimation is based on 1-minute intervals and 5 lags. The resulting estimate of $\kappa$ for stock $i$ on day $t$, denoted $resiliency_{i,t}$, is our measure of resiliency, as a larger coefficient estimate indicates faster mean-reversion. Economically, $(\ln 2)/\kappa$ measures the half-life of a shock to market depth, see Kempf, Mayston, and Yadav (2009).

Table II presents the mean and standard deviation of each variable for French and non-

French stocks during the pre-event period. Overall, the differences in terms of the overall level of market quality are rather small across both groups of stocks. We also include statistics for the log market capitalization and the inverse of the stock price, and the results show that there is no notable difference for these two variables either.

[Insert Table II here.]

## B. *Aggregate impact*

Column (2) of Table III contains the estimated treatment effects from our baseline diff-in-diff analysis based on stocks above the 1 bln EUR threshold. As explained in Section I.C, we restrict our attention to the results for September/October due to the seasonal influences affecting the estimates for August. The $t$-statistics are based on standard errors clustered by stock and time. In addition, we graphically illustrate these estimates in Figure 1 by plotting the cross-sectional averages of the variables for both groups of stocks minus their respective pre-event average over time.

[Insert Table III and Fig. 1 here.]

In line with Hypotheses 1A and 1B, the results show that the French FTT led to a significant reduction in trading activity. Compared to the control group, trading volume in French stocks declined by approximately 10% in September and October 2012. This underlines that the FTT was an important policy event with considerable impact.

We next turn to the results on volatility and price efficiency, for which Hypotheses 1A and 1B have opposite predictions. First, we observe a decline in the informational efficiency of prices following the introduction of the FTT. Relative to the control group, the absolute value of 5-minute midquote return autocorrelations increased by 0.007 for French stocks (significant at the 5% level). While the economic magnitude of this effect was relatively modest, it still is noteworthy because one would assume that the exemption of intraday trading allowed short-term arbitrageurs to continue eliminating price inefficiencies quickly. This result supports Hypothesis 1B. Second, we find that the FTT had no impact on intraday volatility, both for realized volatility and for the intraday price range. This is at odds with both Hypotheses 1A and 1B, and is indicative of an overall moderate impact of the tax.

Finally, we turn to the FTT's impact on liquidity. Our estimates show that both quoted and effective spreads were essentially unaffected by the policy experiment. Similarly, we also observe no meaningful variation in price impacts and realized spreads. In contrast, we find a sizeable and strongly significant decrease in quoted depth of approximately 10,800 EUR. This corresponds to a decline of roughly 20%, compared to the pre-event average. Moreover, we also document a small and statistically significant reduction in market resiliency.[15]

In sum, our estimates suggest that the French FTT had an overall negative impact on market quality. This result is consistent with the views from the theoretical literature emphasizing the importance of the liquidity effect (Hypothesis 1B). In contrast, we reject the predictions associated with a beneficial impact of such a policy through composition effects (Hypothesis 1A). However, the broad picture can potentially mask more significant effects for specific groups of stocks. We next turn to investigating this issue in more detail.

### *C. Heterogeneity across stocks*

One potential source for a heterogeneous impact of FTTs across stocks is the possibility that the strength of the liquidity effect varies with the actual level of liquidity. Clearly, decreased market participation can be expected to have more adverse effects for less liquid securities. In order to investigate this issue, we exploit Euronext's "Supplemental Liquidity Provider" (SLP) programme. This incentive scheme grants rebates on limit orders to a set of market participants in exchange for a commitment to providing additional liquidity, similarly to the NYSE's DMM programme.[16] Its structure is particularly geared towards high-frequency market-makers.

Given that this programme effectively enhanced liquidity in the stocks with the highest ex-ante liquidity, it gives rise to a natural partitioning of our sample. Thus, in order to examine whether the FTT's impact varied with the level of liquidity, we estimate separate treatment effects for SLP and non-SLP stocks.

For the SLP stocks, our difference-in-differences procedure is based, as before, on comparing treated French stocks to non-French control stocks. As explained in Section I.C, the control group for non-SLP stocks additionally includes non-treated French and non-French stocks with a market capitalization below 1 billion EUR. Table IV contains the diff-in-diff estimates.

[Insert Table IV here.]

Our estimates show that the impact of the FTT varied substantially with SLP membership. While trading volume was broadly unchanged for SLP stocks in September/October, non-SLP stocks experienced a decrease in market activity of about 20% due to the FTT.[17] Moreover, non-SLP stocks suffered significant increases in intraday volatility, quoted and effective spreads, price impacts, as well as a decrease in resiliency. All these negative effects on market quality were absent in SLP stocks, which only displayed a reduction in market depth and a slight decrease in price efficiency.

Our results for non-SLP stocks are thus all in line with Hypothesis 1B, and clearly reject Hypothesis 1A. The decrease in volume associated with the FTT hurt liquidity and crowded out "useful" trades, so that volatility increased and price efficiency decreased. Moreover, the relatively modest aggregate impact was actually hiding a large impact on non-SLP stocks and a minor impact on SLP stocks. This is in line with the strength of the liquidity effect depending on the actual level of liquidity. The most liquid stocks were relatively insulated against the adverse effects, while the rest of the market experienced a more pronounced decline in trading volume and market quality.[18]

# III. Liquidity effects

As mentioned previously, the French tax effectively exempts intraday trading activity due to a restriction to ownership transfers. Given that high-frequency traders exclusively engage in tax-exempt intraday trading, they can only have been affected indirectly through the liquidity effect. We can therefore obtain direct evidence on this channel by examining the activity of different trader types, and in particular HFTs.

To investigate this issue, we rely on the *BEDOFIH* database, which assigns both sides of each transaction (limit and market order) to one of three distinct categories: high-frequency traders (HFTs), mixed traders (MTs), and non-high-frequency traders (non-HFTs).[19] Importantly, this database only covers stocks for which the AMF is the competent authority, that is, French securities. We thus cannot compare HFT volume in French stocks to HFT volume in non-French stocks. Accordingly, we restrict our analysis in this section to non-SLP stocks, for which we can use other French stocks below the 1 bln EUR threshold as a control group.[20]

### A. Descriptive statistics

Table V details the market shares for each of the three different trader categories across the set of French securities subject to the FTT. We report separate statistics for SLP and non-SLP stocks given that we expect these two groups to differ significantly with respect to the importance of HFT activity. A glance at the numbers confirms this, as we find HFTs to account for 27.5% of the trading volume in SLP stocks, but only around 16.9% in non-SLP stocks. We further report each trader type's market share for market and limit orders in order to see whether some market participants are more likely to trade with a particular order type. This decomposition reveals a striking difference between both groups of stocks in terms of the type of HFT activity. While HFTs roughly split their trades equally among both order types in SLP stocks, they almost exclusively trade via market orders in non-SLP stocks. Instead, we observe that both MTs and non-HFTs display a higher share of trading via limit orders in the latter group of stocks. Liquidity provision is thus structured very differently in both groups. While HFTs are on the passive side for 27.3% of all trades in SLP stocks, liquidity is almost exclusively provided by MTs (65.2%) and non-HFTs (31.0%) in non-SLP stocks.

Table VI provides trader-type level information on price impacts and realized spreads. For robustness, we report the results for different frequencies. These statistics allow us to gauge differences in informed trading, as well as agents' ability to avoid being picked off when submitting limit orders. Consistent with previous studies (Brogaard, Hendershott, and Riordan (2014), Carrion (2013)), we find that HFTs' market orders have the largest permanent price impact. Moreover, HFTs also earn the largest realized spreads, in line with a superior management of outstanding limit orders. In contrast, non-HFTs suffer the largest adverse selection costs, being reflected in the smallest realized spreads.

[Insert Tables V and VI here.]

## *B. Treatment effects across trader types*

Table VII contains the diff-in-diff estimates for trading volume, as well as the impact on the share of market and limit orders attributable to each of the three different trader groups.[21] We find that, in line with Hypothesis 2, HFTs were strongly impacted by the FTT despite the effective exemption of intraday trading. Their activity decreased by 35% (significant at the 1% level), which is in fact the largest effect across all three trader groups. While the trading volume of MTs declined by around 22%, non-HFTs were only marginally affected, and the estimated treatment effect of -3% is statistically insignificant.

[Insert Table VII here.]

Interestingly, we observe that the FTT did not only affect the composition of the overall trading volume, but also triggered changes in the relative use of market and limit orders across trader types. For example, MTs strongly reduced their use of limit orders in favour of market orders. In contrast, the relative increase in non-HFT activity was particularly driven by an increased use of limit orders, indicating that this trader type became more important for liquidity provision due to the FTT.

The fact that the FTT led to a large decrease in HFT activity despite the exemption of intraday activity indicates the presence of a strong liquidity effect.[22] Our data allows us to further investigate the mechanisms behind this effect. In particular, two possible explanations come to mind. First, HFTs act as intermediaries or arbitrageurs, such that their trading activity varies with that of end-investors such as, e.g., asset managers. Accordingly, a decrease in the overall trading volume should also yield a decrease in HFT activity. Second, as shown previously, HFTs mainly traded via market orders in non-SLP stocks. This implies that their trading activity was sensitive to an increase in transaction costs such as the effective spread. Accordingly, the reduced level of market liquidity can have forced them to scale back their trading because some previously profitable trading strategies became unprofitable. As we will show, both effects contributed to the decrease in HFT volume.

In order to explore the first mechanism, we re-estimate the treatment effect for HFTs when simultaneously controlling for the FTT-induced reduction in the trading activity of other market participants (MTs and non-HFTs). If the entire decrease in HFT volume is due to the overall decline in market activity, the resulting estimate for the FTT's causal impact should be equal to zero. However, Table VIII reveals that we continue to obtain a significant and negative treatment effect of around -24% after controlling for the overall decline in trading volume. This corresponds to roughly two thirds of our initial estimate. Our conclusions are qualitatively unchanged when adding a squared volume term in order to allow for a potentially non-linear relationship.

[Insert Table VIII here.]

We next turn to examining the potential effects of increased effective spreads on the profitability of HFTs' market orders. We proxy HFT's profits earned on market orders (which account for around 90% of their trades) by the *negative* of the realized spread, which is natural given that this measure is widely used to gauge the revenues of liquidity providers. The first column of Table IX contains the estimated profits on HFT market orders in treated non-SLP stocks for June and July, averaged across all stock-days. For robustness, we present the figures for different time horizons. In line with their large price impact, we generally find that these trades were profitable. In order to gauge the effects of a decline in liquidity on HFT profits, we then apply a mechanical increase in effective spreads of 1.088 bps to these figures, in line with the estimated average treatment effect (see Table IV). We observe that this increase in trading costs would have largely rendered HFT in non-SLP stocks a money-losing business. To quantify the possible effects on HFTs' trading behaviour, we compute the fraction of stock-days that would have turned from profitable into unprofitable, based on our measure of HFT profits. The third column reveals that the resulting effect is quantitatively very large. The increase in effective spreads would have turned between 14% and 38% of all stock-days from profit-making enterprises into loss-generating ones. This is consistent with the increase in trading costs through reduced market liquidity being able to generate a large indirect effect on tax-exempt HFTs.

[Insert Table IX here.]

### *C. Discussion*

A number of theoretical contributions associate FTTs with a decline in liquidity, and the evidence presented here supports the importance of the liquidity effect. However, the relevant models consider stylized Walrasian or dealer markets, which differ considerably from the reality of order-driven limit order markets with decentralized liquidity provision. Absent a complete theory, we can only provide a tentative interpretation of the mechanisms that seem to play a role.

Since HFT activity was de facto exempt, the results from Table VII indicate that the FTT had a direct impact only on traders in the MT category. MTs are likely to include large buy-side investors who trade via sophisticated execution algorithms through large brokerage firms, using both limit and market orders. This is in line with the observation that 65% of all executed limit orders in non-SLP stocks were submitted by MTs (see Table V). As these agents scaled back their trading activity, other agents needed to step up their provision of limit orders. Table VII shows that this void was filled by non-HFTs, whose share of executed limit orders increased from 31% to 36.6%. However, given that these agents are less efficient at managing their outstanding limit orders and thus incur higher adverse selection costs (see Table VI), bid-ask spreads had to increase in equilibrium.

Naturally, the increase in bid-ask spreads also affected the submission of market orders. In particular, HFTs mainly relied on market orders in non-SLP stocks so that their profits were extremely sensitive to trading costs. Because they are also the most informed trader type, a decline in their activity reduced the adverse selection risk faced by limit orders. However, this second effect could not fully neutralize the initial increase in spreads in equilibrium.

Otherwise, HFTs would have had no reason to scale back their trading in the first place.

# IV. Two margins of adjustment

We now turn to testing Hypotheses 3A and 3B, that is, we examine the relative importance of portfolio holdings and portfolio turnover for investors' reaction to the policy experiment. We base our analysis on portfolio snapshots of institutional investors, obtained from *Factset*. This database also contains useful information on fund characteristics, in particular their portfolio turnover. An additional advantage of this data is that it pertains to the "buy-side", that is investors with a relatively long investment horizon that usually do not engage in intraday trading. Accordingly, all institutions in this sample can be expected to be fully exposed to the tax.

We start out by screening the database for investment funds holding any of our sample securities throughout the calendar year 2012. Most funds report at the monthly or quarterly frequency, but not always at the quarter end. In order to bring all data to the same frequency, we only consider the last report in a given calendar quarter and assume it is filed at the quarter end.[23] We then restrict our sample to funds reporting at least once per quarter and with non-zero holdings of at least one French and one control stock throughout the entire period 2012:Q1-2012:Q4. We also limit the sample to Closed-end Funds, Hedge Funds, Non-Public Funds, Open-end Funds, Pension Funds, and Offshore Funds. This leaves us with 3,241 funds.[24]

Given our interest in the cross-sectional variation of the FTT's impact, we need to measure the treatment effect at the fund level. Based on the hypotheses developed in Section I.B, we focus on the FTT's impact on portfolio holdings and trading volume. In line with the literature on institutional investment (e.g., Grinblatt, Titman, and Wermers (1995)), we compute fund-specific treatment effects based on changes in portfolio holdings of treated and control stocks. Here, the set of treated and control stocks corresponds to that of Section II.B, that is the baseline specification is based on stocks above the 1 bln EUR threshold (see Table I).

## *A. Fund-specific treatment effects*

We use the following two measures of fund-specific treatment effects. First, $did_f^H$ quantifies the FTT's impact on security holdings. It is computed as the log change in fund $f$'s total holdings of treated stocks between the end of the second quarter (Q2) and the end of the third quarter (Q3) of 2012, minus the contemporaneous log change in holdings of control stocks. In order to avoid picking up effects related to price changes, we value all holdings using the stock prices prevailing at the end of Q2.

The second variable, $did_f^V$, measures how funds' trading activity changes in response to the implementation of the FTT. We define fund-specific trading volume for quarter $t$ as the absolute value of the change in holdings between quarter $t-1$ and quarter $t$, again evaluated at the closing prices at $t-1$. Given that the computation of trading volume uses data from two adjacent quarters, we compute $did_f^V$ as the log change of trading volume in treated stocks between Q4 and Q2, minus the contemporaneous log change for control stocks. Notice that $did_f^V$ is only defined for the 2,436 funds that trade both in treated and control stocks during Q2 and Q4, and we henceforth restrict our analysis to these funds.[25]

## B. *Explanatory variables*

Notice that Hypothesis 3A in particular predicts that the FTT's impact differs across investors with different investment horizons. In order to test this prediction, we draw on a set of fund characteristics provided by *Factset* and construct the following explanatory variables. The main variable of interest is $turnover_f$, which assigns funds into five different categories according to their portfolio turnover (i.e., their trading intensity). It takes values ranging from -2 ("Very Low") to +2 ("Very High"), and is based on a classification provided by *Factset*. The variable $log\ size_f$ is defined as the natural logarithm of a fund's total assets under management (in USD). We use $price-to-book\ ratio_f$ as a proxy for the investment style of fund $f$. A high (low) value implies that a fund predominantly invests into growth (value) stocks. The measure is computed by *Factset* as the average price-to-book ratio across all portfolio constituents. Finally, $Index_f$ is a dummy variable which takes a value of one for index funds, and zero otherwise.

Table X contains some summary statistics for these explanatory variables. The average fund has a price-to-book ratio of slightly above 3 and approximately 1.2 billion USD assets under management. Around 13% of the funds are index funds. The average fund is categorized to have a low portfolio turnover ($-0.89$) but there is considerable cross-sectional variation across funds with a standard deviation of 1.14.

[Insert Tables X and XI here.]

## *C. Results*

Panel A of Table XI reports the coefficients from cross-sectional regressions of the fund-level treatment effects on our explanatory variables, separately for changes in portfolio holdings ($did_f^H$) and changes in trading volume ($did_f^V$). In all regressions, $t$-statistics are based on White standard errors robust to heteroskedasticty.

We first discuss the impact on portfolio holdings. We start out with a simple regression on only a constant in order to gauge the average treatment effect across funds. The resulting coefficient estimate in column (1) is very small and statistically insignificant. This indicates that the FTT did not induce the average institutional investor in our sample to increase or decrease her holdings of French stocks relative to the control group.

We proceed to examining how the cross-sectional variation in funds' reaction to the FTT can be explained by differences in fund characteristics. The resulting estimates are tabulated in column (2). In order to facilitate the interpretation of the intercept, we demean both $log\ size_f$ and $price - to - book\ ratio_f$ before the estimation. Accordingly, the constant represents the treatment effect for a non-index fund of average size, average investment style, and with a medium turnover.

In line with Hypothesis 3A, the coefficient on $turnover_f$ is negative and strongly statistically significant, indicating that investors with shorter (longer) average holding periods reduced (increased) their holdings in French stocks subject to the FTT, relative to control stocks. Notably, the economic magnitude of this effect is large. The coefficient estimate of $-0.038$ implies that investors with a very high turnover (+2) sold 8.5% of their holdings of French stocks, while investors with a very low turnover ($-2$) increased them by 6.7%. This provides strong support for the "clientele effect" in Amihud and Mendelson (1986) and the associated prediction that an exogenous increase in transaction costs will induce agents with a short investment horizon to sell some of their holdings to long-term investors. Concerning the control variables, we find no effect related to fund size, but a reduction in holdings of French stocks by funds with a high price-to-book ratio as well as index funds.[26]

We now turn our attention to trading volume. As before, we start by gauging the average treatment effect across all funds. Notice, however, that Hypothesis 3B formally refers to portfolio turnover, and not trading volume. Given that trading volume is defined as the product of holdings and turnover, the average treatment effect on turnover is equal to the average treatment effect on volume conditional on the treatment effect on holdings.[27] Accordingly, we do not only regress $did_f^V$ on a constant, but we additionally control for $did_f^H$.

The resulting coefficient estimate for the constant term of 0.180 in column (3) implies that the average fund reduced its turnover in French stocks by approximately 16%, relative to the control group. This confirms the significant role for turnover as an alternative margin of adjustment, in line with Hypothesis 3B.

A number of interesting findings emerge when adding the individual fund characteristics as additional regressors (see column (4)). In particular, we find that the coefficient on portfolio turnover is negative and strongly significant, which suggests that the turnover adjustment following the FTT's introduction was stronger (weaker) for the most (least) active investors. While intuitive, we are not aware of any theoretical model that predicts this relationship, as standard theories of transaction costs usually assume either homogeneity across investors (see Constantinides (1986) and Vayanos (1998)) or exogenous investment horizons (as in Amihud and Mendelson (1986)). Notice that the effect is economically large, as the coefficient estimate of $-0.148$ implies that the funds with the highest portfolio turnover reduced their trading in French stocks by around 42%, while those with the least reshuffling activity did not change their behaviour.[28] This is consistent with the most active investors being particularly apt to use both turnover and holdings as alternative margins of adjustment to the exogenous increase in trading costs.

We proceed by repeating the above analysis separately for SLP and non-SLP stocks. We follow our approach from Section II.C and use both French and non-French stocks below the 1 billion EUR threshold as a control group for French non-SLP stocks subject to the tax. For each subsample, we only include funds that have positive holdings of at least one treated and one control stocks throughout the entire year 2012. Moreover, we discard funds for which $did_f^V$ is not defined due to zero trading volume. The results are depicted in Table XI, where Panel B refers to SLP stocks and Panel C to non-SLP stocks.

Interestingly, we find that the negative relationship between holdings adjustment and portfolio turnover predicted by Hypothesis 3A is limited to SLP stocks. Given that SLP stocks experienced a rather muted decrease in aggregate trading volume and liquidity, this observation illustrates the argument of Constantinides (1986) that transaction costs can significantly affect individual strategies and at the same time only have a weak effect on aggregate market variables.

For non-SLP stocks, there is no evidence that long-term investors increased their holdings relative to short-term investors. In turn, we find a more pronounced decrease in turnover, which declined by around 18% for SLP stocks and 23% for non-SLP stocks. This observation is consistent with our results concerning the differences in the FTT's impact on market quality across both groups of stocks (Section II.C) being driven by a liquidity effect. For the less liquid stocks, investors exclusively adjusted via the intensive margin, thereby hurting market liquidity disproportionately. This finding is also in line with the more prominent role of MTs and non-HFTs for liquidity provision in non-SLP stocks documented in Section III.

In sum, our results show that the reaction of institutional investors to the introduction of the French FTT was consistent with existing equilibrium models of trading under transaction costs. We provide evidence for investors using portfolio holdings and portfolio turnover as alternative margins of adjustment.

Beyond shedding light on existing theories of trading under transaction costs, this result is also in line with some of the arguments brought forward by advocates of FTTs. Indeed, Keynes (1936) and others argue that a tax will decrease the influence of short-term investors by changing the ownership structure of the affected companies (the "holdings adjustment"). This argument is supported by recent empirical research linking investor horizons to managerial decisions (e.g., Derrien, Kecskes, and Thesmar (2013)). Our analysis confirms that FTTs indeed have a significant impact on the ownership structure. However, we additionally show that particularly short-term funds do not only sell shares, but also reduce their turnover. While this second mechanism also implies an increase in the average holding period for the affected securities, it is less obvious that it will induce these investors to become more involved in corporate governance.

# V. Conclusion

This paper uses the French FTT launched in 2012 in order to shed light on the main economic mechanisms stressed in the debate on FTTs. We find no evidence for the composition effect through which an FTT is supposed to improve market quality. Instead, our results support the existence of a liquidity effect through which such a tax worsens market quality and indirectly affects even exempted traders.

The idea to implement a Pigovian tax on trading volume thus does not seem to apply well to modern financial markets. The policy debate on FTTs, which has been revived recently with the project of a pan-European FTT, should therefore rather focus on two alternative motivations, for which we provide more mixed evidence.

First, the resulting revenues need to be compared to the associated economic distortions. With its exemptions and safeguards for liquidity provision, the French design attempts to minimize the negative side effects.[29] Nevertheless, our results suggest that the French FTT was to a large extent a disguised tax on savers, whose costs and benefits require a general equilibrium analysis.

Second, we find evidence that the tax led to changes in the shareholder composition of the affected companies. This is in line with the argument of proponents of FTTs, who expect this effect to foster a reduction of managerial myopia or short-termism. However, we find that short-term investors adjusted to the tax by reducing their turnover, and not only by selling their shares. While this second effect also increases the average holding period, it is unclear whether both margins of adjustment have the same consequences for corporate decision making.

This second point illustrates a more general phenomenon. The theoretical literature suggests that market participants adjust their behavior so as to minimize the impact of the tax (e.g., Constantinides (1986)). The impact of a tax on aggregate market outcomes should thus be second order compared to changes in the affected investors' portfolios and trading strategies. To our knowledge, the French FTT is the first experiment of this kind for which the necessary disaggregated data is available, opening new avenues for research on FTTs and transaction costs in general.

**Table I**
Treated and control groups

| Treated group | Control group |
|---|---|
| **FTT for all stocks** | |
| 87 French stocks above 1 bln EUR | 32 non-French stocks above 1 bln EUR |
| **FTT for SLP stocks** | |
| 49 French SLP stocks above 1 bln EUR | 27 non-French SLP stocks above 1 bln EUR |
| **FTT for non-SLP stocks** | |
| 38 French non-SLP stocks above 1 bln EUR | 5 non-French non-SLP stocks above 1 bln EUR<br>47 non-SLP stocks below 1 bln EUR |

## Table II

Summary statistics for treated and control stocks

This table contains the empirical averages and standard deviations of market quality variables for French and non-French stocks above the 1 bln EUR threshold, over the period June-July 2012. All figures are computed at the stock-day level.

| | French | Non-French |
|---|---|---|
| *log volume* | 16.16 | 16.71 |
| | (1.46) | (1.14) |
| *realized volatility* | 26.79 | 24.36 |
| | (10.68) | (10.23) |
| *range* | 2.75 | 2.49 |
| | (1.46) | (1.40) |
| *quoted spread* | 5.77 | 4.19 |
| | (4.22) | (2.14) |
| *effective spread* | 4.41 | 3.32 |
| | (3.08) | (1.56) |
| *realized spread* | 0.38 | 0.15 |
| | (2.07) | (1.03) |
| *price impact* | 4.03 | 3.18 |
| | (2.51) | (1.71) |
| *depth* | 57.39 | 80.61 |
| | (51.31) | (47.17) |
| *resiliency* | 0.49 | 0.53 |
| | (0.15) | (0.13) |
| *abs. autocorrelation* | 0.11 | 0.11 |
| | (0.09) | (0.09) |
| *inverse price* | 0.06 | 0.07 |
| | (0.10) | (0.06) |
| *log market cap.* | 22.39 | 22.50 |
| | (1.09) | (1.09) |
| # Stocks | 87 | 32 |
| # Obs. | 3,741 | 1,376 |

**Table III**

Causal impact of the FTT on all stocks

This table contains the estimates for the coefficients $\beta^{Aug}$ and $\beta^{Sep/Oct}$ from specification (1), which corresponds to the regression equation

$$y_{i,t} = \alpha_i + \gamma_t + \beta^{Aug} D_{i,t}^{Aug} + \beta^{Sep/Oct} D_{i,t}^{Sep/Oct} + \epsilon_{i,t}, \tag{5}$$

where $y_{i,t}$ denotes one of the ten market quality variables defined in Section II. We consider the sample of stocks with a market capitalization of more than 1 billion EUR and $\beta^{Sep/Oct}$ identifies the average impact of the FTT. $T$-statistics based on standard errors clustered at the stock and day level are given in parentheses. $***$, $**$, and $*$ denote statistical significance at the 1%, 5%, and 10% level, respectively.

| Variable | $\beta^{Aug}$ | $\beta^{Sep/Oct}$ |
|---|---|---|
| *log volume* | -0.397*** | -0.106** |
| | (-7.68) | (-2.47) |
| *realized volatility* | -1.014 | 0.317 |
| | (-1.58) | (0.36) |
| *range* | -0.160** | -0.053 |
| | (-2.14) | (-0.50) |
| *quoted spread* | 0.075 | -0.038 |
| | (0.38) | (-0.15) |
| *effective spread* | -0.018 | 0.015 |
| | (-0.12) | (0.08) |
| *price impact* | 0.167 | 0.193 |
| | (1.24) | (1.18) |
| *realized spread* | -0.186 | -0.177 |
| | (-1.28) | (-1.32) |
| *depth* | -10.731** | -10.773*** |
| | (-2.55) | (-2.82) |
| *resiliency* | -0.021** | -0.018* |
| | (-1.99) | (-1.93) |
| *abs. autocorrelation* | -0.004 | 0.007* |
| | (-0.60) | (1.91) |
| # Treated | 87 | |
| # Control | 32 | |
| # Obs. | 12,971 | |

## Table IV

Causal impact of the FTT on SLP and non-SLP stocks

This table contains the estimates for the coefficients $\beta^{Aug}$ and $\beta^{Sep/Oct}$ from the regression equation

$$y_{i,t} = \alpha_i + \gamma_t + \beta^{Aug} D_{i,t}^{Aug} + \beta^{Sep/Oct} D_{i,t}^{Sep/Oct} + \epsilon_{i,t}, \tag{6}$$

where $y_{i,t}$ denotes one of the ten market quality variables defined in Section II. Columns (1) and (2) refer to the stocks pertaining to Euronext's SLP programme and uses non-French SLP stocks as control group. Columns (3) and (4) refer to the remaining stocks, and uses both non-French non-SLP stocks and French non-SLP stocks below the 1 billion EUR threshold as control group. $T$-statistics based on standard errors clustered at the stock and day level are given in parentheses. $***$, $**$, and $*$ denote statistical significance at the 1%, 5%, and 10% level, respectively.

| Variable | SLP | | Non-SLP | |
|---|---|---|---|---|
| | (1) $\beta^{Aug}$ | (2) $\beta^{Sep/Oct}$ | (3) $\beta^{Aug}$ | (4) $\beta^{Sep/Oct}$ |
| *log volume* | -0.326*** | -0.032 | -0.382*** | -0.224*** |
| | (-5.65) | (-0.68) | (-5.68) | (-3.29) |
| *realized volatility* | -0.616 | 0.470 | -0.584 | 2.421** |
| | (-0.84) | (0.51) | (-0.51) | (2.03) |
| *range* | -0.140* | -0.069 | -0.018 | 0.256* |
| | (-1.66) | (-0.61) | (-0.14) | (1.87) |
| *quoted spread* | 0.287*** | 0.176 | 0.457 | 1.254* |
| | (2.70) | (1.43) | (0.99) | (1.85) |
| *effective spread* | 0.194** | 0.175 | -0.008 | 1.088** |
| | (2.07) | (1.56) | (-0.02) | (2.08) |
| *price impact* | 0.327*** | 0.181 | 0.797** | 1.835*** |
| | (3.00) | (1.29) | (2.07) | (5.34) |
| *realized spread* | -0.133 | -0.006 | -0.803** | -0.746** |
| | (-1.30) | (-0.05) | (-2.02) | (-2.14) |
| *depth* | -14.195*** | -12.750*** | 0.187 | -2.112 |
| | (-2.68) | (-2.67) | (0.35) | (-1.13) |
| *resiliency* | -0.017 | -0.008 | -0.034*** | -0.029*** |
| | (-1.42) | (-0.83) | (-3.82) | (-3.24) |
| *abs. autocorrelation* | -0.005 | 0.009** | 0.013** | 0.007 |
| | (-0.89) | (1.98) | (1.99) | (1.20) |
| # Treated | 49 | 38 | 49 | 38 |
| # Control | 27 | 52 | 27 | 52 |
| # Obs. | 8,284 | 9,810 | 8,284 | 9,810 |

## Table V

Breakdown of trading activity by trader type for SLP and non-SLP stocks

This table contains the cross-sectional averages for the proportion of trading volume (Share Volume) as well as the proportion of executed limit orders (Share Limit) and market orders (Share Market) attributable to each of the three trader type categories in the *BEDOFIH* database (HFT, MT, non-HFT). The estimates are tabulated separately for SLP and non-SLP stocks above the 1 billion EUR threshold. Standard errors computed across stock-days and clustered by stock and time are given in parentheses. All figures are based on the months of June and July only.

| Variable/Group | SLP | | | Non-SLP > 1 bln | | |
|---|---|---|---|---|---|---|
| | Share Volume | Share Limit | Share Market | Share Volume | Share Limit | Share Market |
| HFT | 27.53 | 27.28 | 27.78 | 16.91 | 3.73 | 30.09 |
| | (0.86) | (0.85) | (1.17) | (0.90) | (0.21) | (1.70) |
| MT | 56.44 | 55.38 | 57.50 | 55.74 | 65.23 | 46.26 |
| | (0.83) | (0.89) | (1.18) | (1.10) | (1.41) | (1.09) |
| Non HFT | 16.03 | 17.34 | 14.72 | 27.35 | 31.04 | 23.65 |
| | (0.46) | (0.51) | (0.56) | (1.50) | (1.38) | (1.77) |

## Table VI

Price impacts and realized spreads, by trader type, for SLP and non-SLP stocks

This table contains the cross-sectional averages for price impacts and realized spreads at different time horizons. Both measures are tabulated separately for SLP and non-SLP stocks above the 1 billion EUR threshold, and each trader type available in the *BEDOFIH* database (HFT, MT, non-HFT). In the computation, stock-day observations with a missing value for at least one trader category were discarded. Standard errors computed across stock-days and clustered by stock and time are given in parentheses. All figures are based on the months of June and July only.

| Variable/Group | | SLP | | | Non-SLP > 1 bln | | |
|---|---|---|---|---|---|---|---|
| | Horizon | HFT | MT | Non-HFT | HFT | MT | Non-HFT |
| Price impact | 10s | 3.25 | 2.44 | 1.38 | 5.46 | 3.62 | 3.14 |
| | | (0.14) | (0.11) | (0.07) | (0.29) | (0.20) | (0.27) |
| | 5min | 3.25 | 2.98 | 1.74 | 6.12 | 5.16 | 4.26 |
| | | (0.15) | (0.16) | (0.13) | (0.31) | (0.27) | (0.34) |
| | 30min | 3.27 | 2.90 | 1.54 | 6.53 | 5.95 | 4.59 |
| | | (0.16) | (0.20) | (0.21) | (0.37) | (0.40) | (0.46) |
| Realized spread | 10s | 0.29 | -0.10 | -0.20 | 4.25 | 2.15 | 2.06 |
| | | (0.07) | (0.06) | (0.06) | (0.64) | (0.31) | (0.35) |
| | 5min | -0.05 | -0.37 | -0.46 | 3.19 | 1.17 | 0.64 |
| | | (0.09) | (0.08) | (0.12) | (0.52) | (0.27) | (0.35) |
| | 30min | -0.10 | -0.17 | -0.37 | 2.51 | 0.85 | -0.08 |
| | | (0.10) | (0.10) | (0.21) | (0.60) | (0.28) | (0.42) |

**Table VII**

Causal impact of the FTT on trading volume and order flow composition for different trader types, Non-SLP stocks

This table contains the estimates for the coefficient $\beta^{Sep/Oct}$ from the regression equation

$$\Delta y_i = \alpha + \beta^{Sep/Oct} \times treated_i + \epsilon_i, \quad (7)$$

where $treated_i$ is a dummy variable equal to one for treated stocks, $\Delta y_i = y_i^{Sep/Oct} - y_i^{Jun/Jul}$, and $y_i^{Jun/Jul}$ and $y_i^{Sep/Oct}$ denote, for a specific trader type (HFT, MT, non-HFT) either the log of the average daily trading volume, the average share of limit order volume, or the average share of market order volume, across all trading days in June-July and September-October, respectively. This procedure corresponds to the estimation of treatment effects based on a cross-sectional regression with time-series collapsed information suggested in Bertrand, Duflo, and Mullainathan (2004). $T$-statistics based on White standard errors robust to heteroskedasticity are given in parentheses. $***$, $**$, and $*$ denote statistical significance at the 1%, 5%, and 10% level, respectively.

| Group/Variable | Log volume | Share Limit (%) | Share Market (%) |
|---|---|---|---|
| HFT | -0.434*** | -0.654** | -7.109*** |
| | (-3.05) | (-2.06) | (-4.76) |
| MT | -0.253*** | -4.933*** | 3.776*** |
| | (-3.00) | (-3.66) | (2.69) |
| Non-HFT | -0.029 | 5.586*** | 3.332** |
| | (-0.32) | (3.96) | (2.43) |
| # Treated | 38 | 38 | 38 |
| # Control | 29 | 29 | 29 |

## Table VIII

Test for a mechanical decrease in HFT activity, Non-SLP stocks

This table contains the estimates for the coefficient $\beta^{Sep/Oct}$ from the regression equation

$$\Delta log\ volume_i^{HFT} = \alpha + \beta^{Sep/Oct} \times treated_i + \gamma(\Delta X_i) + \epsilon_i, \quad (8)$$

where $\Delta log\ volume_i^{HFT} = log\ volume_i^{HFT,Sep/Oct} - log\ volume_i^{HFT,Jun/Jul}$, and $log\ volume_i^{HFT,Jun/Jul}$ and $log\ volume_i^{HFT,Sep/Oct}$ denote the log of average daily HFT trading volume for stock $i$, and ($\Delta X_i = X_i^{Sep/Oct} - X_i^{Jun/Jul}$) is a vector of control variables based on the difference of sub-period averages. These variables are the log of the daily average non-HFT and MT volume (used in columns (1) and (2)), and its square (used in column (2)). This procedure corresponds to the estimation of treatment effects based on a cross-sectional regression with time-series collapsed information suggested in Bertrand, Duflo, and Mullainathan (2004). $T$-statistics based on White standard errors robust to heteroskedasticity are given in parentheses. $***$, $**$, and $*$ denote statistical significance at the 1%, 5%, and 10% level, respectively.

| Variable | (1) | (2) |
|---|---|---|
| $treated_i$ | -0.272** | -0.266** |
| | (-2.25) | (-2.38) |
| $\Delta log\ volume^{Other}$ | 0.860*** | 0.198 |
| | (4.71) | (0.07) |
| $(\Delta log\ volume^{Other})^2$ | | 0.024 |
| | | (0.23) |
| # Treated | 38 | |
| # Control | 29 | |

## Table IX

HFT gains on market orders, before and after the increase in spreads

This table contains the cross-sectional averages at different time horizons of the following variables: (1) the negative of the realized spread (in bps) on HFTs' market orders, representing HFTs' trading profits; (2) the same figure minus 1.088 bps, the estimated treatment effect for effective spreads in non-SLP stocks, thus representing the HFTs' counterfactual trading gains; (3) the proportion of stock-days that turn from profitable into unprofitable after the 1.088 increase in effective spreads. All figures are based on June-July only.

| Horizon | HFT profits (before) | HFT profits (after) | % stock-days becoming unprofitable |
|---|---|---|---|
| 10s | 0.18 | -0.90 | 37.91 |
| 5min | 0.83 | -0.26 | 25.67 |
| 30min | 1.25 | 0.17 | 13.70 |

## Table X

Summary statistics of fund characteristics

This table contains summary statistics on fund characteristics for the 2,436 investment funds used in our analysis. *Size* denotes total assets under management in billion USD. *turnover* is a discrete variable ranging from $-2$ ("Very Low" portfolio turnover) to $+2$ ("Very High" portfolio turnover). $price - to - book\ ratio$ denotes the fund's average price-to-book ratio based on its portfolio holdings, and $index\ fund$ is a binary variable equal to one for Index Funds, and zero otherwise. Standard deviations are reported in parentheses. All variables are provided by *Factset*.

| Variable | Mean |
|---|---|
| *Size* | 1.20 |
| | (6.27) |
| *turnover* | -0.89 |
| | (1.14) |
| $price - to - book\ ratio$ | 3.20 |
| | (1.17) |
| $index\ fund$ | 0.13 |
| | (0.34) |

## Table XI

Causal impact of the FTT on investment funds' portfolio holdings and trading volume, for all stocks, SLP and non-SLP stocks

This table contains coefficient estimates from a linear regression of fund-specific treatment effects in terms of portfolio holdings (columns (1) and (2)) and trading volume (columns (3) and (4)) on investment fund characteristics. Panel A refers to all stocks, and the control group consists of all non-French stocks. Panel B refers to SLP stocks, and the control group consists of all non-French SLP stocks. Panel C refers to non-SLP stocks, and the control group consists of non-French non-SLP stocks as well French non-SLP stocks below the 1 bln EUR threshold. The dependent variables are defined as

$$did_f^H = [log(p_{Q2}^T.x_{f,Q3}^T) - log(p_{Q2}^T.x_{f,Q2}^T)] - [log(p_{Q2}^C.x_{f,Q3}^C) - log(p_{Q2}^C.x_{f,Q2}^C)] \quad (9)$$

$$\text{and } did_f^V = [log(p_{Q3}^T.|\Delta x_{f,Q4}^T|) - log(p_{Q1}^T.|\Delta x_{f,Q2}^T|)] - [log(p_{Q3}^C.|\Delta x_{f,Q4}^C|) - log(p_{Q1}^C.|\Delta x_{f,Q2}^C|)], \quad (10)$$

where $x_{f,t}^T$ and $x_{f,t}^C$ denote the (column) vectors of holdings in treated and control stocks by fund $f$ at time $t \in \{Q2, Q3\}$ and $p_t^T$ and $p_t^C$ denote the associated (row) price vectors. $\Delta x_{f,t}^T$ denotes $x_{f,t}^T - x_{f,t-1}^T$, with $\Delta x_{f,t}^C$ defined accordingly, and the notation $|.|$ is used for the element-wise absolute value of a vector. $Size$ denotes total assets under management in million USD. $turnover$ is a discrete variable ranging from $-2$ ("Very Low" portfolio turnover) to $+2$ ("Very High" portfolio turnover). $price-to-book\ ratio$ denotes the fund's average price-to-book ratio based on its portfolio holdings, and $index\ fund$ is a binary variable equal to one for Index Funds, and zero otherwise. All variables are provided by *Factset*. $T$-statistics based on standard errors clustered at the stock level are given in parentheses. $***$, $**$, and $*$ denote statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A: All stocks.

| | Holdings | | Trading volume | |
|---|---|---|---|---|
| Expl. variable | (1) | (2) | (3) | (4) |
| $constant$ | 0.001 | -0.009 | -0.181*** | -0.247*** |
| | (0.12) | (-0.55) | (-4.93) | (-4.75) |
| $log\ size$ | | -0.002 | | -0.049** |
| | | (-0.41) | | (-2.45) |
| $turnover$ | | -0.038*** | | -0.131*** |
| | | (-3.40) | | (-3.75) |
| $price-to-book\ ratio$ | | -0.025** | | -0.085** |
| | | (-2.53) | | (-2.35) |
| $index\ fund$ | | -0.183*** | | -0.383*** |
| | | (-5.18) | | (-4.21) |
| $did_f^H$ | | | 0.504*** | 0.442*** |
| | | | (2.95) | (2.78) |
| $R^2$ | 0.000 | 0.023 | 0.020 | 0.035 |
| # Obs. | 2,436 | 2,436 | 2,436 | 2,436 |

Panel B: SLP stocks.

| | Holdings | | Trading volume | |
|---|---|---|---|---|
| Expl. variable | (1) | (2) | (3) | (4) |
| *constant* | -0.001 | -0.013 | -0.194*** | -0.286*** |
| | (-0.12) | (-0.83) | (-4.78) | (-4.93) |
| *log size* | | -0.012** | | -0.063*** |
| | | (-2.41) | | (-2.89) |
| *turnover* | | -0.034*** | | -0.160*** |
| | | (-3.47) | | (-4.21) |
| $price - to - book\ ratio$ | | 0.011 | | -0.081* |
| | | (1.08) | | (-1.95) |
| *index fund* | | -0.142*** | | -0.388*** |
| | | (-4.28) | | (-4.05) |
| $did_f^H$ | | | 0.398** | 0.347** |
| | | | (2.36) | (2.22) |
| $R^2$ | 0.000 | 0.016 | 0.011 | 0.029 |
| # Obs. | 2,189 | 2,189 | 2,189 | 2,189 |

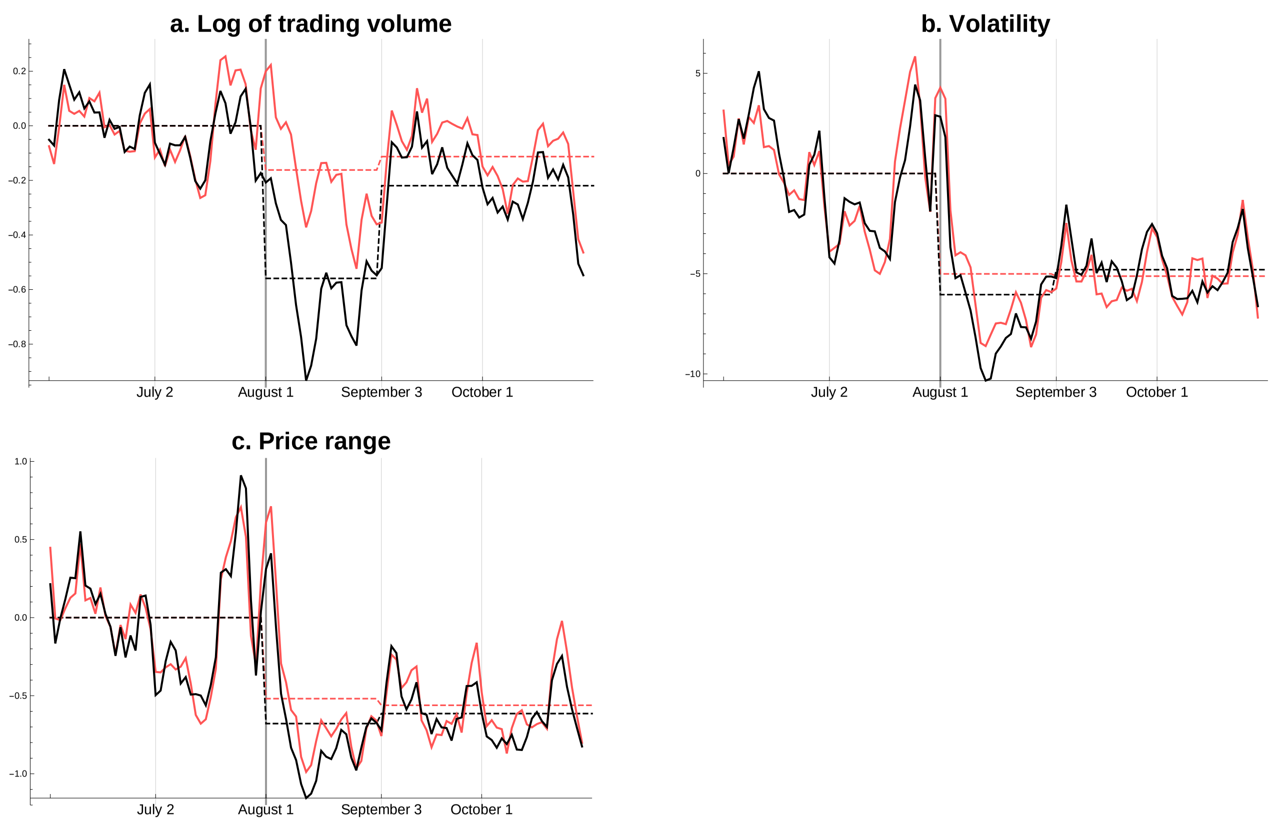
Panel C: Non-SLP stocks.

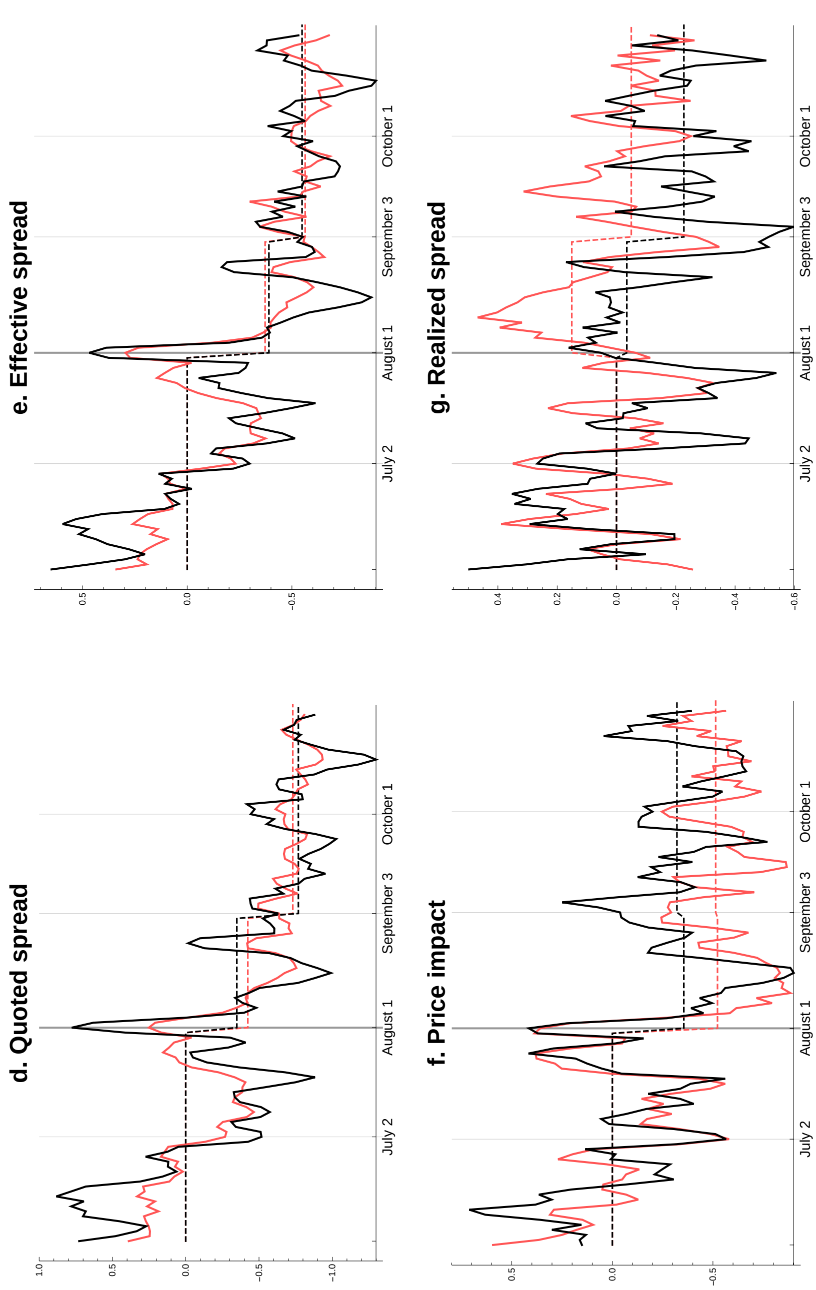
| | Holdings | | Trading volume | |
|---|---|---|---|---|
| Expl. variable | (1) | (2) | (3) | (4) |
| *constant* | -0.007 | -0.011 | -0.261*** | -0.144 |
| | (-0.41) | (-0.39) | (-4.34) | (-1.62) |
| *log size* | | -0.006 | | -0.105*** |
| | | (-0.68) | | (-3.03) |
| *turnover* | | 0.003 | | -0.012 |
| | | (0.13) | | (-0.20) |
| $price - to - book\ ratio$ | | -0.037*** | | -0.013 |
| | | (-2.68) | | (-0.23) |
| *index fund* | | 0.032 | | -0.623*** |
| | | (0.51) | | (-4.46) |
| $did_f^H$ | | | 0.605*** | 0.607*** |
| | | | (2.90) | (2.84) |
| $R^2$ | 0.000 | 0.008 | 0.030 | 0.067 |
| # Obs. | 818 | 818 | 818 | 818 |

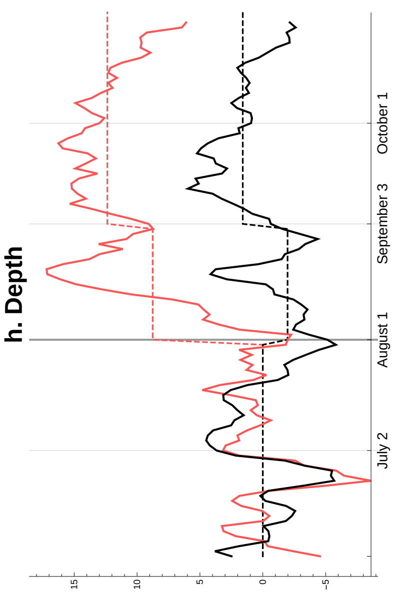
# Appendix A. Figures

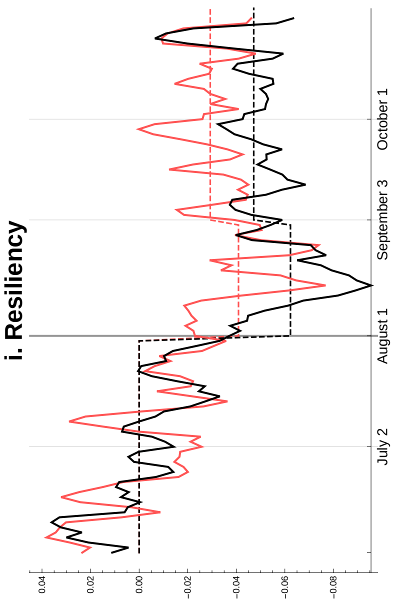
**Figure 1.** Graphical illustration of the causal impact of the FTT

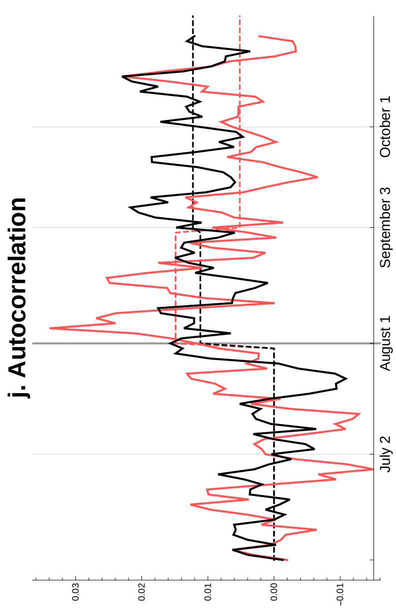
This figure illustrates our difference-in-difference estimates for the causal impact of the FTT on the market quality variables defined in Section II. For each variable, we plot the cross-sectional average for treated (in black) and control (light red) stocks with a market capitalization of more than 1 billion EUR, minus the respective pre-event average. For improved readability, we use 3-day moving averages. The dashed lines indicate the sub-period averages for June/July, August, and September/October. The difference between the two dashed lines in September/October is equal to the diff-in-diff estimate of the causal impact of the tax.




d. Quoted spread
e. Effective spread
f. Price impact
g. Realized spread
July 2
August 1
September 3
October 1


h. Depth
July 2
August 1
September 3
October 1


i. Resiliency
July 2
August 1
September 3
October 1


j. Autocorrelation
July 2
August 1
September 3
October 1

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# Notes

[1]In October 2012, 11 EU countries committed to the introduction of a harmonized tax on financial transactions, initially planned to be launched by 2016. However, the negotiations stalled repeatedly and were still not finalized at the time of writing. See "EU financial transaction tax progress stalls", Financial Times (online), June 5, 2016. The debate on FTTs has also recently resurfaced in the US, in the context of the Democratic party presidential primaries. Transaction taxes and administrative charges on trading activity (e.g., the SEC's Section 31 fee) are internationally widespread. See Matheson (2011) for an overview.

[2]There are numerous empirical studies estimating the impact of FTTs and transaction costs in general. A non-exhaustive list includes Roll (1989), Umlauf (1993), Jones and Seguin (1997), Baltagi, Li, and Li (2006), Hau (2006), Liu and Zhu (2009), and Pomeranets and Weaver (2012). For a more complete overview, see Matheson (2011).

[3]The French FTT yielded 198 million EUR in tax revenues for August-December 2012 (`http://www.assemblee-nationale.fr/14/rap-info/i1328.asp`), which is considerably less than initial projections of 1.1 bln EUR per year (see `http://www.senat.fr/rap/r12-259/r12-2591.pdf`).

[4]See Stein (1989) for a theoretical argument, and Bushee (2001) and Derrien, Kecskes, and Thesmar (2013) for recent evidence.

[5]See Meyer, Wagener, and Weinhardt (2015), Becchetti, Ferrari, and Trenta (2014), Capelle-Blancard and Havrylchyk (2015), Coelho (2014), and Gomber, Haferkorn, and Zimmermann (2016).

[6]With the exception of the first year of implementation, for which the relevant date was January 1st 2012.

[7]The French FTT relies on the so-called "issuance principle", under which taxation is based on a security's country of registration and not on the residence of the counterparties involved in the transaction. American Depository Receipts (ADRs) were not subject to the tax during the sample period. In the Online Appendix, we present some evidence that refutes the idea that these instruments were used actively in an effort to evade the French FTT.

[8]Market-making is defined as either quoting competitive bid and ask prices and/or providing liquidity on a regular and continuous basis, or executing orders on the behalf of clients, or hedging positions due to these activities.

[9]In the Online Appendix, we provide additional evidence that corroborates this view.

[10]Foucault, Sraer, and Thesmar (2011) show that an increase in transaction costs for retail investors causes a decrease in volatility, but this experiment is different from increasing transaction costs for all market participants simultaneously. Indeed, experimental evidence shown in Bloomfield, O'Hara, and Saar (2009) casts doubt on the idea that noise traders are more affected by a general tax than other agents. Deng, Liu, and Wei (2014) suggest that a tax on all transactions can have the desired composition effect when the proportion of noise traders on the market is large enough, but they cannot test the hypothesis that these traders are disproportionately affected by the tax.

[11]Other equilibrium models also conclude that the effect of an FTT is ambiguous. In Dow and Rahi (2000), the impact of an FTT on price informativeness is positive if and only if informed traders are more risk averse than uninformed traders. Similarly, Subrahmanyam (1998) presents a setting in which an FTT decreases liquidity if and only if the number of informed traders is sufficiently large.

[12]In the Online Appendix, we provide empirical support for our choice of specification by confirming the suspicion that trading activity in French stocks is generally subject to a slowdown in August, while both September and October are free from seasonal influences. Moreover, we also show that our results do not change qualitatively if we extend our sample period until the end of December. If anything, the FTT's impact is slightly stronger.

[13]While most stocks are also traded on a number of competing trading platforms, Euronext clearly dominates trading in French and Dutch stocks. Moreover, public data available on the webpage of BATS Chi-X Europe shows that its market share was basically unaffected by the FTT: 68.6% during June-July 2012, 69.2% in August 2012, and 68.4% during September-October 2012.

[14]Trades are signed using the Lee and Ready (1991) method, and we aggregate individual orders that are executed simultaneously into one single transaction. While the *BEDOFIH* data used in Section III only covers French stocks, it contains trade signs based on Euronext data. We use it to check the accuracy of the signing algorithm on a random subsample, and find that it exceeds 95%.

[15]Given the pre-event average speed of mean reversion $\kappa = 0.50$ at a 1-minute frequency, the estimated treatment effect of $-0.018$ implies an increase in the expected half-life of shocks to market depth of approx-

imately 3 seconds (from 85s to 88s).

[16] More specifically, during our sample period liquidity providers were required to post two-sided quotes with a minimum size of 5,000 EUR during 95% of the trading day. In addition, they had to commit to a fixed percentage of time during which they would maintain presence at the inside quote via a competitive bidding procedure during the application process. The details of the rebates were not disclosed, but there was a maximum maker rebate of $-0.2$ bps, and a minimum taker fee of $+0.3$ bps. See `https://www.euronext.com/sites/www.euronext.com/files/launch_of_a_supplemental_liquidity_provider_programme_on_european_blue_chips.pdf` and `https://www.euronext.com/sites/www.euronext.com/files/ifca120326.pdf` for further details.

[17]The interpretation of a coefficient $\beta$ in a semi-log specification as a percentage change is only valid if its magnitude is sufficiently small. In the text we always report the correct percentage change, given by $\exp(\beta) - 1$ (up to a Jensen error), see Halvorsen and Palmquist (1980).

[18]In the Online Appendix, we additionally allow the treatment effects to vary with a stock's market capitalization. The associated results are in line with those obtained only on the basis of the SLP/non-SLP partition. We further show that our results are also not driven by tick size constraints.

[19]The classification was conducted by the AMF, the French securities markets regulator, and is based on the median order lifetimes of individual exchange members as well as additional expert knowledge concerning their trading strategies and business model. The first category covers firms that can unambiguously be identified as pure-play HFT outlets trading on their own account. The MT category is composed of exchange members whose order flow is a blend of HFT and non-HFT. According to the providers of the database, this group mainly comprises of large banks and brokers that either have some proprietary HFT activities or offer direct market access to HFT firms. Finally, the remaining category (non-HFTs) includes smaller banks and retail brokerage firms. Notice that we use the term "trader type" in a loose way, given that the classification is conducted at the member level.

[20]There are only four non-French stocks in our sample which are predominantly traded on Euronext Paris. These are EADS, STMicroelectronics, SES, and Gemalto. Note that our extract from the *BEDOFIH* database does not include Sequana, so that there are 29 control stocks.

[21] In order to avoid estimation errors due to a small number of missing values, we follow Bertrand, Duflo, and Mullainathan (2004) and collapse all the pre-treatment data into a single "pre period" and the post-treatment data into a single "post period".

[22] In particular, note that the HFT tax could not possibly explain this result, since it penalizes limit orders, whereas HFTs in non-SLP stocks mostly use market orders.

[23] We discard all reports filed in July 2012 in order to ensure that all reports allocated to the third quarter are filed after the launch of the FTT. This reduces the number of funds in our sample by around 3%, and does not affect our results.

[24] We verify that our sample of funds represents a significant part of the overall tax base by applying a 20 bps surcharge to all purchases of French securities taking place in Q4:2012 across all funds. This yields a tax revenue estimate of 22.65 million EUR, which can be linearly extrapolated to 37.75 million EUR for the 5 months of 2012 under the FTT regime. This corresponds to roughly 19% of the total 2012 revenue of 198 million EUR.

[25] All our results for $did_f^H$ are qualitatively unchanged when including funds that do not trade in either Q4 or Q2 in at least one group of stocks, i.e., funds for which $did_f^V$ is not defined.

[26] The latter result is surprising at first sight, as index funds are not expected to have a lot of flexibility in their portfolio choice. However, as shown in the Online Appendix, this result is entirely due to index funds using synthetic replication strategies via total return swaps. In contrast, the FTT does not affect the holdings of index funds with physical replication strategies.

[27] To see this, let $H_t$ and $X_t$ denote holdings and turnover at time $t$, so that trading volume is equal to $V_t = H_t \times X_t$. Then, the log change between two adjacent periods is given by $\Delta v_t = \Delta h_t + \Delta x_t$, with lower case letters indicating variables in logs and $\Delta$ being the first difference operator.

[28] Given an intercept of $-0.251$, the treatment effects for investors with very high (very low) turnover are $-0.251 \pm 2 \times (-0.148)$. We then apply the transformation explained in footnote 17.

[29] In particular, the French design compares rather well to the planned pan-European project. Indeed, we estimate that the French design would yield approximately 2.7 billion EUR if implemented at the EU level, based on an extrapolation of the actual 2012 revenues of 198 Million for August-December 2012. This

compares to 4.8-6.5 billion EUR that the European Commission expects to raise with a tax on *all* equity transactions (including intraday activity), and without any safeguards for liquidity provision. Details on the computations can be found in the Online Appendix.

# Online Appendix to "Financial Transaction Taxes, Market Composition, and Liquidity"

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This Appendix presents a number of additional results and robustness checks. Section A provides a list of the stocks used in our analysis. Section B reports the correlations between the treated and the control group for the variables studied in Section II. Section C empirically estimates the impact of the HFT Tax and rejects the hypothesis that this tax had a significant effect. Section D provides evidence for a seasonal slowdown of trading activity in French stocks for the month of August, thus rationalizing our econometric approach. Moreover, this placebo test confirms the validity of the common trends assumption underlying our difference-in-differences framework. Section E verifies that the impact of the tax is not temporary and extends to the rest of 2012. Section F checks that there was no sizeable anticipation of the French FTT's implementation, as we do not observe considerable changes in the overall levels of trading activity and market quality in the months prior to the FTT's implementation. This validates our choice of June and July 2012 as the reference period. Sections G, H, I, J conduct several robustness exercises, and verify that our main results are not affected by a different method for computing standard errors, by controlling for the stocks' market capitalization and price, and are not driven by tick-size constraints. Section K shows that our results on non-SLP stocks are robust to using a regression discontinuity design approach. Section Section L shows that the effect of the tax on the holdings and volume of index funds is entirely driven by synthetic funds. Section M provides evidence that American Depositary Receipts (ADRs) were not used to circumvent the FTT. Section N details our estimates for an extension of French FTT to other European countries. Finally, Section O provides additional diff-in-diff graphs pertaining to the results obtained with the BEDOFIH database.

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## A. *Sample composition*

The following Table provides the complete list of stocks used in our analysis. Stocks marked with an asterisk were part of Euronext's SLP programme during our sample period.

**Table IA.1**
List of treated and control stocks

Panel A: French stocks affected by the FTT

| | | |
|---|---|---|
| Accor SA* | Euler Hermes SA | PPR* |
| Aeroports de Paris | Eurazeo | Publicis Groupe SA* |
| Air France-KLM* | Eutelsat Communications SA* | Rallye SA |
| Air Liquide SA* | Faurecia | Remy Cointreau SA |
| Alcatel-Lucent/France* | Fonciere Des Regions | Renault SA* |
| Alstom SA* | France Telecom SA* | Rexel SA |
| Arkema SA* | GDF Suez* | Rubis SCA |
| AtoS* | Gecina SA | Safran SA* |
| AXA SA* | Groupe Eurotunnel SA | Sanofi* |
| BioMerieux | Havas SA | Schneider Electric SA* |
| Bourbon SA | ICADE | SCOR SE |
| Bouygues SA* | Iliad SA* | SEB SA |
| Bureau Veritas SA* | Imerys SA | Silic |
| Cap Gemini SA* | Ingenico | Societe BIC SA |
| Carrefour SA* | Ipsen SA | Societe Television Francaise 1 |
| Casino Guichard Perrachon SA* | JCDecaux SA | Sodexo* |
| Christian Dior SA | Klepierre | Suez Environnement Co* |
| Cie de St-Gobain* | Lafarge SA* | Technip SA* |
| Cie Generale de Geophysique - Veritas* | Lagardere SCA* | Thales SA |
| Cie Generale des Etablissements Michelin* | Legrand SA* | Total SA* |
| CNP Assurances | L'Oreal SA* | Unibail-Rodamco SE* |
| Danone SA* | LVMH SA* | Valeo SA* |
| Dassault Systemes SA* | Mercialys SA | Vallourec SA* |
| Edenred | Metropole Television SA | Veolia Environnement SA* |
| Eiffage SA | Neopost SA | Vinci SA* |
| Electricite de France SA* | Nexans SA* | Virbac SA |
| Eramet | Orpea | Vivendi SA* |
| Essilor International SA* | Pernod-Ricard SA* | Wendel SA |
| Etablissements Maurel et Prom | Peugeot SA* | Zodiac Aerospace* |

Panel B: French stocks not affected by the FTT, and non-French stocks

| French, market cap. < 1 bln. | Non-French, market cap. > 1 bln. | Non-French, market cap. < 1 bln. |
|---|---|---|
| AB Science SA | Aalberts Industries NV | AMG Advanced Metallurgical Group NV |
| Akka Technologies SA | Aegon NV* | Arcadis NV |
| Alten SA | Akzo Nobel NV* | BinckBank NV |
| Altran Technologies SA | ArcelorMittal* | Brunel International NV |
| Archos | ASM International NV | CSM |
| Artprice.com | ASML Holding NV* | Dockwise Ltd |
| Assystem | Corio NV* | Heijmans NV |
| Beneteau SA | Delta Lloyd NV | Koninklijke BAM Groep NV |
| Bull | EADS* | Koninklijke Ten Cate NV |
| Club Mediterranee | Eurocommercial Properties NV | Koninklijke Wessanen NV |
| Derichebourg SA | Fugro NV* | LBi International NV |
| Eurofins Scientific | Gemalto NV* | Mediq NV |
| Faiveley Transport SA | Heineken NV* | Nieuwe Steen Investments NV |
| GameLoft SA | ING Groep NV* | PostNL NV* |
| Groupe Steria SCA | Koninklijke Ahold NV* | SNS REAAL NV |
| IPSOS | Koninklijke Boskalis Westminster NV* | TomTom NV* |
| Medica SA | Koninklijke DSM NV* | Unit4 NV |
| Mersen | Koninklijke KPN NV* | USG People NV |
| Nexity SA | Koninklijke Philips Electronics NV* | Vastned Retail NV |
| NicOx SA | Koninklijke Vopak NV* | |
| PagesJaunes Groupe | Nutreco NV | |
| Pierre & Vacances SA | Randstad Holding NV* | |
| Plastic Omnium SA | Reed Elsevier NV* | |
| Saft Groupe SA | Royal Dutch Shell PLC* | |
| Sequana SA | Royal Imtech NV* | |
| Societe de la Tour Eiffel | SBM Offshore NV* | |
| SOITEC | SES SA* | |
| Technicolor SA | STMicroelectronics NV* | |
| Teleperformance SA | TNT Express NV* | |
| UBISOFT Entertainment | Unilever NV* | |
| | Wereldhave NV* | |
| | Wolters Kluwer NV* | |

## *B. Correlations*

We report the correlations over the period June-July 2012 between the 87 French stocks affected by the FTT and the 32 non-French stocks forming our control group, for the different variables studied in Section II. Intuitively, a high correlation means that variables strongly co-move before the introduction of the tax, strengthening the assumption that they would have continued to behave similarly without the tax implementation. Note that this is not a formal test however, as even a low correlation coefficient is compatible with the common trends assumption.

**Table IA.3**

Correlations over June-July 2012

For each variable $y$, this table reports $corr(\bar{y}_t^{FR}, \bar{y}_t^{Non-FR})$, where $\bar{y}_t^{FR}$ and $\bar{y}_t^{Non-FR}$ are the cross-sectional averages of $y_{i,t}$ on date $t$ for the treated and the control group, respectively, and $t$ covers the two months before the treatment.

| Variable | corr. |
|---|---|
| *log volume* | 0.88 |
| *realized volatility* | 0.91 |
| *range* | 0.93 |
| *quoted spread* | 0.86 |
| *effective spread* | 0.81 |
| *price impact* | 0.79 |
| *realized spread* | 0.61 |
| *depth* | 0.54 |
| *resiliency* | 0.72 |
| *abs. autocorrelation* | 0.26 |

## *C. The HFT Tax*

As mentioned in Section I.C., there are good reasons to suspect that the HFT Tax did not have any significant impact on affected French stocks. In particular, this tax generated zero revenues.[1] To further corroborate this idea, we can use the fact that the HFT Tax applies to all French stocks, and not only to those with a market capitalization above 1 bln EUR. We use a diff-in-diff procedure on the French and non-French stocks below the 1 bln EUR threshold in our sample, considering the French stocks as treated and the non-French stocks as control (see Table IA.4 for summary statistics on these two groups).[2] Since the French stocks below the threshold are not affected by the FTT, under the null assumption that the HFT Tax had no impact, the diff-in-diff regressions should return non-significant coefficients for the interaction term.

Table IA.5 shows our estimates for this specification. A glance at the results directly reveals that the HFT tax did not have any significant impact on market quality. Out of a total of 10 coefficients, only one is statistically significant at the 10% level. Moreover, the observed increase in the intraday price range is economically small and not corroborated by a similar effect for realized volatility. Our estimates thus verify that the surcharge on order cancellations was essentially a "cosmetic" measure without real impact due to its geographical restriction to French firms. Due to the absence of an impact of the HFT tax, we can interpret the diff-in-diff estimates pertaining to the set of stocks above the 1 billion EUR threshold as the causal effects of the FTT.

[1] See the report of the French parliament's finance committee, available at: `http://www.assemblee-nationale.fr/14/rap-info/i1328.asp`.

[2] There are 49 stocks below the 1 bn EUR threshold in our sample. Among those, two non-French stocks are part of the SLP programme.

**Table IA.4**

Summary statistics for stocks below the 1 bln EUR threshold

This table contains the empirical averages and standard deviations of market quality variables for French and non-French stocks below the 1 bln EUR threshold, over the period June-July 2012. All figures are computed at the stock-day level.

| | French | Non-French |
|---|---|---|
| *log volume* | 13.38 | 13.89 |
| | (0.98) | (1.09) |
| *realized volatility* | 34.97 | 33.66 |
| | (17.18) | (15.79) |
| *range* | 3.64 | 3.42 |
| | (2.10) | (1.98) |
| *quoted spread* | 19.10 | 16.24 |
| | (7.27) | (7.47) |
| *effective spread* | 16.53 | 13.41 |
| | (7.60) | (6.85) |
| *realized spread* | 5.18 | 3.55 |
| | (6.19) | (5.12) |
| *price impact* | 11.35 | 9.86 |
| | (6.40) | (5.22) |
| *depth* | 10.99 | 17.40 |
| | (7.23) | (11.16) |
| *resiliency* | 0.26 | 0.31 |
| | (0.12) | (0.14) |
| *abs. autocorrelation* | 0.12 | 0.12 |
| | (0.09) | (0.09) |
| *inverse price* | 0.18 | 0.22 |
| | (0.19) | (0.22) |
| *log market cap* | 19.83 | 19.98 |
| | (0.64) | (0.63) |
| # Stocks | 30 | 19 |
| # Obs. | 1,290 | 817 |

**Table IA.5**
Causal impact of the HFT tax

This table contains the estimates for the coefficient $\beta^{Sep/Oct}$ from the regression equation

$$y_{i,t} = \alpha_i + \gamma_t + \beta^{Aug} D_{i,t}^{Aug} + \beta^{Sep/Oct} D_{i,t}^{Sep/Oct} + \epsilon_{i,t}, \quad \text{(IA.1)}$$

where $y_{i,t}$ denotes one of the ten market quality variables defined in Section II. We include only stocks below the 1 bln EUR threshold and consider the French stocks as treated. The coefficient thus identifies the impact of the HFT tax only. $T$-statistics based on standard errors clustered at the stock and day level are given in parentheses. $***$, $**$, and $*$ denote statistical significance at the 1%, 5%, and 10% level, respectively.

| Variable | $\beta^{Sep/Oct}$ |
|---|---|
| *log volume* | 0.037 |
| | (0.34) |
| *realized volatility* | 2.860 |
| | (1.42) |
| *range* | 0.406* |
| | (1.84) |
| *quoted spread* | 0.712 |
| | (0.58) |
| *effective spread* | 0.721 |
| | (0.71) |
| *price impact* | 0.393 |
| | (0.65) |
| *realized spread* | 0.326 |
| | (0.52) |
| *depth* | -5.878 |
| | (-1.18) |
| *resiliency* | 0.016 |
| | (1.45) |
| *abs. autocorrelation* | -0.006 |
| | (-0.91) |
| # Treated | 30 |
| # Control | 19 |
| # Obs. | 5,341 |

## *D. Seasonality effects*

As mentioned in Section I.C., practitioners and regulators suggested to us that the trading activity in August 2012 may not properly reflect the permanent impact of the policy change due to short-run (legal) uncertainty and seasonality in trading activity. While it is close to impossible to measure the extent of uncertainty among investors, it is relatively straightforward to verify whether certain variables are subject to seasonal factors based on past data.

To this end, we collect data for the months June - October for the three years prior to our sample period (2009 - 2011) and estimate a placebo-DiD, in which we allow a different treatment effect for each calendar month. We discard five stocks from our initial sample because of incomplete data for this period. Moreover, we drop October 23rd 2009 due to some missing observations for some stocks.

The resulting estimates in Table IA.6 strongly confirm the existence of seasonal factors in trading activity and volatility. In line with Hong and Yu (2009), we hypothesize that this effect is due to different vacation patterns across France and the Netherlands.[3] Compared to the control group, French stocks generally display a drop in traded volume of roughly 15% during the month of August, accompanied by a slight (and statistically significant) decrease in intraday volatility, price range and realized spreads. None of the remaining variables appears to be subject to a seasonal influence during August. The coefficients for September and October are all statistically insignificant, confirming the absence of seasonal influences in these months. This analysis also serves as a placebo diff-in-diff and additionally supports the validity of our control group (see e.g. Autor (2003)).

[3]While it is common in France to take off most or even the entire month of August for summer holidays, this pattern is less prevalent in the Netherlands.

**Table IA.6**

Test for seasonal effects 2009-2011

This table contains the estimates for the coefficients $\beta^{Aug}, \beta^{Sep}$ and $\beta^{Oct}$ from the regression equation

$$y_{i,t} = \alpha_i + \gamma_t + \beta^{Aug} D_{i,t}^{Aug} + \beta^{Sep} D_{i,t}^{Sep} + \beta^{Oct} D_{i,t}^{Oct} + \epsilon_{i,t}, \quad \text{(IA.2)}$$

where $y_{i,t}$ denotes one of the ten market quality variables defined in Section II. $T$-statistics based on standard errors clustered at the stock and day level are given in parentheses. $***$, $**$, and $*$ denote statistical significance at the 1%, 5%, and 10% level, respectively.

| Variable | $\beta^{Aug}$ | $\beta^{Sep}$ | $\beta^{Oct}$ |
|---|---|---|---|
| *log volume* | -0.166*** | -0.005 | -0.036 |
| | (-4.40) | (-0.15) | (-0.93) |
| *realized volatility* | -1.465** | -0.843 | -0.500 |
| | (-2.23) | (-1.08) | (-0.78) |
| *range* | -0.148* | 0.053 | -0.041 |
| | (-1.71) | (1.19) | (-0.65) |
| *quoted spread* | -0.096 | -0.192 | -0.222 |
| | (-0.65) | (-1.05) | (-0.96) |
| *effective spread* | -0.056 | -0.000 | -0.023 |
| | (-0.51) | (-0.00) | (-0.13) |
| *price impact* | 0.165 | 0.084 | 0.087 |
| | (1.27) | (0.88) | (0.71) |
| *realized spread* | -0.221** | -0.084 | -0.111 |
| | (-2.01) | (-0.72) | (-0.87) |
| *depth* | 0.614 | -1.556 | 0.275 |
| | (0.36) | (-0.75) | (0.12) |
| *resiliency* | -0.008 | 0.007 | -0.002 |
| | (-1.58) | (1.11) | (-0.46) |
| *abs. autocorrelation* | -0.000 | -0.002 | 0.004 |
| | (-0.07) | (-0.52) | (1.04) |
| # Treated | | 84 | |
| # Control | | 30 | |
| # Obs. | | 37,278 | |

We now de-seasonalize our original treatment effect estimates for trading volume, volatility and the price range during August 2012 via a diff-in-diff-in-diff procedure. Given that the remaining variables are not subject to seasonality, applying this procedure to them would only (incorrectly) decrease the precision of the estimates. Hence we estimate the following equation for the months June - August and years 2009 - 2012 exclusively for trading volume and our two measures of volatility:

$$y_{i,t} = \alpha_i + \gamma_t + \beta^{Aug} D_{i,t}^{Aug} + \beta^{Aug,2012} D_{i,t}^{Aug,2012} + \epsilon_{i,t}, \qquad \text{(IA.3)}$$

where $D_{i,t}t^{Aug}$ is equal to 1 for treated stocks on all dates in August between 2009 and 2012, while $D_{i,t}^{Aug,2012}$ is equal to 1 for treated stocks in August 2012 only. The seasonally adjusted treatment effect for August is given by $\beta^{Aug,2012}$. The results in Table IA.7 show that the de-seasonalized impact on trading volume is now roughly -24% instead of the -33% obtained in Table III. The remaining discrepancy with the long-term impact of -10% may be due to the mentioned (legal) uncertainty or other short-run factors. In terms of volatility, we conclude that the FTT did not have a statistically significant impact even in the short run.

**Table IA.7**
Diff-in-diff-in-diff estimates for August

This table contains the regression coefficients from estimating the diff-in-diff-in-diff model in equation (IA.3). $T$-statistics based on standard errors clustered at the stock and day level are given in parentheses. $***$, $**$, and $*$ denote statistical significance at the 1%, 5%, and 10% level, respectively.

| Variable/Coefficient | $\beta^{Aug,2012}$ |
|---|---|
| *log volume* | -0.276*** |
| | (-3.03) |
| *realized volatility* | 0.958 |
| | (0.83) |
| *range* | 0.076 |
| | (0.64) |
| # Treated | 84 |
| # Control | 30 |
| # Obs. | 30,096 |

## *E. Extended post-event window*

In order to ensure that our estimated treatment effects are indeed permanent, and not merely transitory, we extend the post-event period until December. This effectively doubles the time window used to identify the FTT's permanent impact from two to four months. Table IA.8 provides the coefficient estimates for the entire sample, while Table IA.9 gives separate results for SLP and non-SLP stocks. The results are qualitatively very similar to those reported in Tables III and IV in the main text. In particular, the results obtained with the extended post-event period confirm the two main conclusions drawn from our analysis of the FTT's aggregate impact: The overall effect on market quality was rather modest, and less liquid non-SLP stocks were considerably more affected than SLP stocks. The only notable difference is that the decrease in trading volume for SLP stocks has become slightly stronger and statistically significant. In sum, we can thus safely reject the idea that our analysis has only identified a temporary impact. This is also consistent with the results reported by Capelle-Blancard and Havrylchyk (2015) and Megarbane (2013).

## Table IA.8

Impact of the tax: from September to December, all stocks

This table contains the estimates for the coefficient $\beta^{Sep-Dec}$ from the following regression equation.

$$y_{i,t} = \alpha_i + \gamma_t + \beta^{Aug} D_{i,t}^{Aug} + \beta^{Sep-Dec} D_{i,t}^{Sep-Dec} + \epsilon_{i,t}, \quad \text{(IA.4)}$$

where $y_{i,t}$ denotes one of the ten market quality variables defined in Section II, and $D_{i,t}^{Sep-Dec}$ equals 1 if $i$ is treated and $t$ is a day between September 1st and December 31st, 2012. The first column reports the coefficients estimating the impact of the tax over the period September-December for all stocks. $T$-statistics based on standard errors clustered at the stock and day level are given in parentheses. $***$, $**$, and $*$ denote statistical significance at the 1%, 5%, and 10% level, respectively.

| | All stocks |
|---|---|
| *log volume* | -0.140*** |
| | (-3.42) |
| *realized volatility* | -0.527 |
| | (-0.49) |
| *range* | -0.136 |
| | (-1.12) |
| *quoted spread* | 0.066 |
| | (0.28) |
| *effective spread* | 0.085 |
| | (0.48) |
| *price impact* | 0.090 |
| | (0.52) |
| *realized spread* | -0.003 |
| | (-0.03) |
| *depth* | -10.146** |
| | (-2.43) |
| *resiliency* | -0.018* |
| | (-1.82) |
| *abs. autocorrelation* | 0.010*** |
| | (2.83) |
| # Treated | 87 |
| # Control | 32 |
| # Obs. | 17,850 |

## Table IA.9

Impact of the tax: from September to December, SLP and non-SLP stocks

This table contains the estimates for the coefficient $\beta^{Sep-Dec}$ from the following regression equation.

$$y_{i,t} = \alpha_i + \gamma_t + \beta^{Aug} D_{i,t}^{Aug} + \beta^{Sep-Dec} D_{i,t}^{Sep-Dec} + \epsilon_{i,t}, \tag{IA.5}$$

where $y_{i,t}$ denotes one of the ten market quality variables defined in Section II, and $D_{i,t}^{Sep-Dec}$ equals 1 if $i$ is treated and $t$ is a day between September 1st and December 31st, 2012. The first column reports the coefficients estimating the impact of the tax over the period September-December for SLP stocks, while the second column reports the results on non-SLP stocks. $T$-statistics based on standard errors clustered at the stock and day level are given in parentheses. $***$, $**$, and $*$ denote statistical significance at the 1%, 5%, and 10% level, respectively.

| | SLP stocks | Non-SLP stocks |
|---|---|---|
| *log volume* | -0.115** | -0.284*** |
| | (-2.51) | (-3.96) |
| *realized volatility* | -1.211 | 2.485** |
| | (-1.03) | (2.01) |
| *range* | -0.211 | 0.225* |
| | (-1.57) | (1.67) |
| *quoted spread* | 0.257* | 1.626*** |
| | (1.87) | (2.63) |
| *effective spread* | 0.205 | 1.253*** |
| | (1.63) | (2.59) |
| *price impact* | -0.002 | 2.171*** |
| | (-0.02) | (6.10) |
| *realized spread* | 0.206** | -0.920*** |
| | (1.96) | (-2.87) |
| *depth* | -11.987** | -4.663 |
| | (-2.24) | (-1.16) |
| *resiliency* | -0.014 | -0.032*** |
| | (-1.36) | (-3.19) |
| *abs. autocorrelation* | 0.011*** | 0.008* |
| | (2.93) | (1.82) |
| # Treated | 49 | 38 |
| # Control | 27 | 52 |
| # Obs. | 11,400 | 13,500 |

## *F. Extended pre-treatment period*

While the French FTT was implemented on August 1, this policy change was known to investors in advance. The tax was voted in Parliament on February 16 and subsequently published in the French tax code on March 14. Our identification relies on the assumption that agents did not modify their trading behavior significantly in the months preceding the implementation of the tax. In order to test for this possibility, we conduct a new difference-in-differences analysis with January-February as the "pre-treatment" period, estimating the impact for March-April, May-June, and July. The regression equation is given by:

$$y_{i,t} = \alpha_i + \gamma_t + \beta^{Mar/Apr} D_{i,t}^{Mar/Apr} + \beta^{May/Jun} D_{i,t}^{May/Jun} + \beta^{Jul} D_{i,t}^{Jul} + \epsilon_{i,t}, \tag{IA.6}$$

In the absence of anticipation effects, the coefficients $\beta$ should be insignificant for all months. Table IA.10 below shows our estimates. We drop January 2nd which has an unusually low trading activity, as well as the stock FDR.PA due to missing observations. Across the 7 variables we consider and the 3 pre-tax periods, we find four coefficients that are statistically significant, the only impact that is economically meaningful being depth.

## Table IA.10

Anticipation effects - Placebo analysis

This table contains the estimates for the coefficients $\beta^{Mar/Apr}, \beta^{May/Jun}, \beta^{Jul}$ from the regression equation (IA.6) for the ten market quality variables defined in Section II. $T$-statistics based on standard errors clustered at the stock level are given in parentheses. $***$, $**$, and $*$ denote statistical significance at the 1%, 5%, and 10% level, respectively.

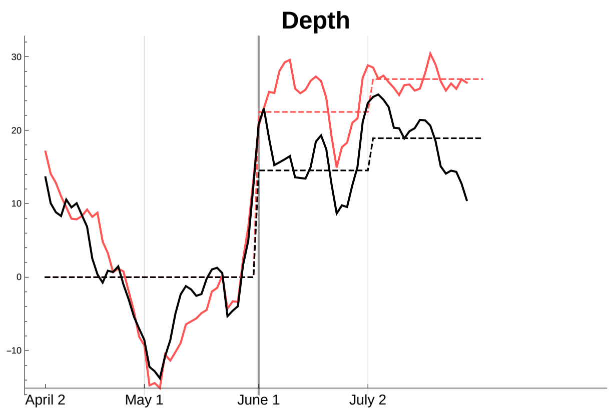
| Variable | $\beta^{Mar/Apr}$ | $\beta^{May/Jun}$ | $\beta^{Jul}$ |
|---|---|---|---|
| *log volume* | 0.028 | -0.012 | -0.108 |
| | (0.56) | (-0.19) | (-1.52) |
| *realized volatility* | 0.005 | 0.709 | 0.508 |
| | (0.01) | (0.74) | (0.51) |
| *range* | 0.030 | 0.068 | 0.123 |
| | (0.29) | (0.59) | (1.08) |
| *quoted spread* | -0.249 | 0.101 | -0.210 |
| | (-1.57) | (0.40) | (-0.82) |
| *effective spread* | -0.122 | -0.030 | -0.194 |
| | (-0.95) | (-0.16) | (-0.95) |
| *price impact* | -0.092 | 0.090 | 0.069 |
| | (-0.76) | (0.58) | (0.43) |
| *realized spread* | -0.030 | -0.119 | -0.263* |
| | (-0.25) | (-0.82) | (-1.76) |
| *depth* | -3.249 | -6.990* | -14.944*** |
| | (-1.41) | (-1.65) | (-2.93) |
| *resiliency* | 0.019* | 0.013 | 0.013 |
| | (1.79) | (0.88) | (0.87) |
| *abs. autocorrelation* | 0.002 | 0.008 | 0.006 |
| | (0.42) | (1.53) | (1.12) |
| # Treated | | 86 | |
| # Control | | 32 | |
| # Obs. | | 17,818 | |

More precisely, the results in Table IA.10 suggest that quoted depth in French stocks had already been decreasing relative to that of control stocks before the implementation of the French FTT in August 2012. In particular, the estimates suggest a decline of 7,000 EUR for May-June, and of 14,000 EUR for July (the latter significant at the 1% level).

It turns out that this discrepancy is caused by the renewal of Euronext's SLP programme on June 1st 2012 (as pre-announced on 26 March, 2016, see `https://www.euronext.com/sites/www.euronext.com/files/ifca120326.pdf`). While Euronext refused to provide details on whether any details were changed or the renewal only led to a change in the programme participants, even the latter was likely to be sufficient to alter the competitive landscape. Figure IA.1 graphically illustrates the effects of this event on quoted depth by plotting the respective averages for French and non-French SLP stocks for April-July. For better readability, we have demeaned both groups by their respective averages for April-May.

**Figure IA.1.** Impact of the renewal of the SLP programme (June 1st, 2012) on quoted depth

This figure illustrates the impact of the renewal of the SLP programme on quoted depth, for French and non-French stocks separately. We plot the cross-sectional average for French SLP stocks (in black) and non-French SLP stocks (in red), minus the respective pre-event average. The data only pertains to stocks part of the SLP programme both before and after the event. For improved readability, we use 3-day moving averages. The dashed lines indicate the sub-period averages for April/May, June, and July.




Three observations can be made. First, quoted depth increased strongly following the renewal of the SLP programme. Second, the adjustment to the new level was very rapid and remained relatively constant throughout June-July, consistent with the market adjusting immediately to an event happening on June 1st. In particular, this observation is at odds with the existence of a trend over these two months. Third, the effect was stronger for non-French stocks (the difference is almost 9,000 EUR and statistically significant at the 1% level).

We now show that this effect of the SLP renewal entirely explains the discrepancy in depth across the treatment and the control groups. We adjust the analysis above for the effects of the renewal of the SLP programme by augmenting equation (IA.6) with two dummy variables that take the value of one for French and non-French SLP stocks after June 1st, respectively. Table IA.11 tabulates the corrected estimates, and the coefficients on quoted depth are no longer statistically significant. While we find a statistically significant (at the 5% level) decrease in realized spreads, the economic magnitude is very small.

Notice also that these regressions provide a so-called "placebo-test" of our methodology: the overall absence of statistically significant coefficients before August not only supports the absence of anticipation effects, but also shows that the common trends assumption is satisfied in the time prior to August 1st and that both groups of stocks do not seem to be differentially impacted by other shocks than the FTT, even over an extended period of 5 months.

## Table IA.11

Anticipation effects - Corrected for the SLP effect

This table contains the estimates for the coefficients $\beta^{Mar/Apr}$, $\beta^{May/Jun}$, $\beta^{Jul}$ from the regression equation (IA.6) for the ten market quality variables defined in Section II. We add two dummy variables that take the value of one for French and non-French SLP stocks after June 1st, respectively. $T$-statistics based on standard errors clustered at the stock level are given in parentheses. $***$, $**$, and $*$ denote statistical significance at the 1%, 5%, and 10% level, respectively.

| Variable | $\beta^{Mar/Apr}$ | $\beta^{May/Jun}$ | $\beta^{Jul}$ |
|---|---|---|---|
| *log volume* | 0.011 | 0.000 | -0.061 |
| | (0.20) | (0.00) | (-0.72) |
| *realized volatility* | 0.018 | 0.502 | 0.162 |
| | (0.02) | (0.51) | (0.15) |
| *range* | 0.006 | 0.032 | 0.094 |
| | (0.05) | (0.25) | (0.74) |
| *quoted spread* | -0.186 | 0.153 | -0.254 |
| | (-1.14) | (0.57) | (-0.79) |
| *effective spread* | -0.093 | 0.007 | -0.195 |
| | (-0.70) | (0.04) | (-0.70) |
| *price impact* | -0.048 | 0.188 | 0.223 |
| | (-0.36) | (1.10) | (1.07) |
| *realized spread* | -0.045 | -0.181 | -0.418** |
| | (-0.33) | (-1.01) | (-2.06) |
| *depth* | -2.450 | 0.569 | -0.241 |
| | (-1.05) | (0.17) | (-0.08) |
| *resiliency* | 0.017 | 0.013 | 0.015 |
| | (1.43) | (0.83) | (0.91) |
| *abs. autocorrelation* | 0.001 | 0.008 | 0.006 |
| | (0.26) | (1.17) | (0.67) |
| # Treated | | 78 | |
| # Control | | 30 | |
| # Obs. | | 16,308 | |

## *G. Collapsing the time-series information*

As an additional robustness check, we follow the recommendation of Bertrand, Duflo, and Mullainathan (2004) and replicate our results after collapsing all the pre-treatment data into a single "pre period" and the post-treatment data into a single "post period" (for reasons already explained, we exclude August from this post period). This procedure entirely eliminates the concern that time series autocorrelation may artificially inflate the statistical significance of the treatment effect. Table IA.12 replicates our Table III using this approach. The results are almost identical to the initial tables.

## Table IA.12

Causal impact of the FTT - Time series data collapsed into two periods

This table contains the estimates for the coefficient $\beta^{Sep/Oct}$ from the regression equation

$$\Delta y_i \quad = \quad \alpha + \beta^{Sep/Oct} \times treated_i + \epsilon_i, \qquad \text{(IA.7)}$$

where $\Delta y_i = y_i^{Sep/Oct} - y_i^{Jun/Jul}$, and $y_i^{Jun/Jul}$ and $y_i^{Sep/Oct}$ denote the average of a market quality variable across all trading days in June-July and September-October, respectively. This procedure corresponds to the estimation of treatment effects based on a cross-sectional regression with time-series collapsed information suggested in Bertrand, Duflo, and Mullainathan (2004). $T$-statistics based on White standard errors robust to heteroskedasticity are given in parentheses. $***$, $**$, and $*$ denote statistical significance at the 1%, 5%, and 10% level, respectively.

| Variable | (1) All stocks | (2) SLP stocks | (3) Non-SLP stocks |
|---|---|---|---|
| *log volume* | -0.106** | -0.032 | -0.224*** |
| | (-2.49) | (-0.68) | (-3.44) |
| *realized volatility* | 0.317 | 0.470 | 2.421** |
| | (0.36) | (0.50) | (2.17) |
| *range* | -0.053 | -0.069 | 0.256* |
| | (-0.50) | (-0.61) | (1.98) |
| *quoted spread* | -0.038 | 0.176 | 1.254* |
| | (-0.16) | (1.43) | (1.88) |
| *effective spread* | 0.015 | 0.175 | 1.088** |
| | (0.08) | (1.56) | (2.16) |
| *price impact* | 0.193 | 0.181 | 1.835*** |
| | (1.22) | (1.36) | (5.69) |
| *realized spread* | -0.177 | -0.006 | -0.746** |
| | (-1.39) | (-0.05) | (-2.17) |
| *depth* | -10.773*** | -12.750*** | -2.112 |
| | (-2.83) | (-2.68) | (-1.13) |
| *resiliency* | -0.018* | -0.008 | -0.029*** |
| | (-1.90) | (-0.81) | (-3.17) |
| *abs. autocorrelation* | 0.007** | 0.009** | 0.007 |
| | (2.05) | (2.21) | (1.36) |
| # Treated | 87 | 49 | 38 |
| # Control | 32 | 27 | 52 |

## H. *Cross-sectional heterogeneity*

We investigate whether, among SLP and non-SLP stocks, the impact of the FTT depends on the company size. To this end, we estimate a triple-diff specification where we allow the treatment effect to vary with $logmv_i$, the average log market capitalization of stock $i$ during June-July 2012. The resulting specification is given by

$$\begin{aligned} y_{i,t} &= \alpha_i + \gamma_t + \beta^{Aug} D_{i,t}^{Aug} + \beta^{Sep/Oct} D_{i,t}^{Sep/Oct} \\ &+ \delta^{Aug} Post_t^{Aug} \times logmv_i + \delta^{Sep/Oct} Post_t^{Sep/Oct} \times logmv_i \\ &+ \beta^{Aug-logmv} D_{i,t}^{Aug} \times logmv_i + \beta^{Sep/Oct-logmv} D_{i,t}^{Sep/Oct} \times logmv_i + \epsilon_{i,t}, \end{aligned} \tag{IA.8}$$

where $Post_t^{Aug}$ and $Post_t^{Sep/Oct}$ are dummy variables that take the value of one during August and September-October, respectively, and zero otherwise. The results in Table IA.13 reveal that the differential effect associated with firm size, denoted $\beta^{Sep/Oct-logmv}$, is only significant in three instances. In these cases, the results are fully in line with those obtained only based on the SLP/non-SLP partition. The FTT's impact on market quality is more muted for larger (and thus more liquid) stocks, with the exception of market depth.

**Table IA.13**

Causal impact of the FTT, interacted with market capitalization

This table contains the coefficient estimates for $\beta^{Sep/Oct}$ and $\beta^{Sep/Oct-logmv}$ of the regression equation (IA.8). The first two columns refer to SLP stocks, while the last two columns refer to non-SLP stocks. $T$-statistics based on standard errors clustered at the stock and day level are given in parentheses. $***$, $**$, and $*$ denote statistical significance at the 1%, 5%, and 10% level, respectively.

| Variable | SLP stocks | | Non-SLP stocks | |
|---|---|---|---|---|
| | $\beta^{Sep/Oct}$ | $\beta^{Sep/Oct-logmv}$ | $\beta^{Sep/Oct}$ | $\beta^{Sep/Oct-logmv}$ |
| *log volume* | -0.048 | -0.062 | -0.332*** | -0.006 |
| | (-1.02) | (-1.22) | (-3.31) | (-0.06) |
| *realized volatility* | 0.421 | -0.832 | 1.070 | 1.573 |
| | (0.47) | (-1.36) | (0.72) | (1.04) |
| *range* | -0.101 | -0.031 | 0.040 | 0.169 |
| | (-0.98) | (-0.39) | (0.23) | (0.91) |
| *quoted spread* | 0.184 | -0.171 | 2.197** | 0.516 |
| | (1.48) | (-1.30) | (2.15) | (0.43) |
| *effective spread* | 0.180 | -0.206 | 1.177* | -0.099 |
| | (1.59) | (-1.64) | (1.69) | (-0.12) |
| *price impact* | 0.190 | -0.205* | 1.087** | 0.263 |
| | (1.43) | (-1.91) | (2.15) | (0.55) |
| *realized spread* | -0.009 | -0.002 | 0.090 | -0.363 |
| | (-0.08) | (-0.02) | (0.18) | (-0.63) |
| *depth* | -13.039*** | -9.623*** | -3.265* | 0.131 |
| | (-2.99) | (-3.37) | (-1.70) | (0.11) |
| *resiliency* | -0.011 | -0.003 | -0.024* | 0.015 |
| | (-1.11) | (-0.31) | (-1.66) | (1.12) |
| *abs. autocorrelation* | 0.009** | -0.003 | 0.020** | -0.016** |
| | (2.26) | (-0.76) | (2.57) | (-2.01) |
| # Treated | 87 | | 38 | |
| # Control | 32 | | 52 | |

# I. *Additional controls*

Following Hendershott, Jones, and Menkveld (2011), we use as additional controls in our regressions the log of market capitalization and the inverse of market price. We replicate Tables III and VII and do not observe any significant change in our estimates.

**Table IA.14**

Causal impact of the FTT, with additional controls

This table contains the estimates for the coefficient $\beta^{Sep/Oct}$ from the regression equation

$$y_{i,t} = \alpha_i + \gamma_t + \beta^{Aug} D_{i,t}^{Aug} + \beta^{Sep/Oct} D_{i,t}^{Sep/Oct} + \eta_1 logmv_{i,t} + \eta_2 inverse\ price_{i,t} + \epsilon_{i,t}, \quad \text{(IA.9)}$$

where $y_{i,t}$ denotes one of the ten market quality variables defined in Section II, $logmv_{i,t}$ is the market capitalization of stock $i$ on date $t$ and $inverse\ price_{i,t}$ the inverse of the share price. Column (1), (2) and (3) report the results for all stocks, SLP stocks, and non-SLP stocks, respectively. $T$-statistics based on standard errors clustered at the stock and day level are given in parentheses. $***$, $**$, and $*$ denote statistical significance at the 1%, 5%, and 10% level, respectively.

| Variable | (1) All stocks | (2) SLP stocks | (3) Non-SLP stocks |
|---|---|---|---|
| *log volume* | -0.115*** | -0.045 | -0.225*** |
| | (-2.79) | (-1.07) | (-3.21) |
| *realized volatility* | 0.252 | 0.467 | 2.375* |
| | (0.30) | (0.54) | (1.95) |
| *range* | -0.054 | -0.059 | 0.250* |
| | (-0.57) | (-0.60) | (1.78) |
| *quoted spread* | -0.063 | 0.170 | 1.213* |
| | (-0.25) | (1.46) | (1.77) |
| *effective spread* | -0.021 | 0.145 | 1.060** |
| | (-0.12) | (1.42) | (1.99) |
| *price impact* | 0.187 | 0.194 | 1.843*** |
| | (1.16) | (1.45) | (5.29) |
| *realized spread* | -0.208 | -0.049 | -0.781** |
| | (-1.53) | (-0.40) | (-2.22) |
| *depth* | -11.308*** | -14.147*** | -2.144 |
| | (-3.07) | (-3.07) | (-1.16) |
| *resiliency* | -0.016* | -0.006 | -0.029*** |
| | (-1.70) | (-0.53) | (-3.17) |
| *abs. autocorrelation* | 0.007* | 0.009* | 0.007 |
| | (1.83) | (1.93) | (1.30) |
| Add. controls | Yes | Yes | Yes |
| # Treated | 87 | 49 | 38 |
| # Control | 32 | 27 | 52 |
| # Obs. | 12,971 | 8,284 | 9,810 |

**Table IA.15**

Causal impact of the FTT on trading volume and order flow composition for different trader types. Non-SLP stocks, with additional controls

This table contains the estimates for the coefficient $\beta^{Sep/Oct}$ from the regression equation

$$\begin{aligned} \Delta y_i &= \alpha + \beta^{Sep/Oct} treated_i \\ &+ \eta_1(\Delta logmv_i) + \eta_2(\Delta inverse\ price_i) + \epsilon_i, \end{aligned} \qquad \text{(IA.10)}$$

where $\Delta y_i = y_i^{Sep/Oct} - y_i^{Jun/Jul}$, and $y_i^{Jun/Jul}$ and $y_i^{Sep/Oct}$ denote, for a specific trader type (HFT, MT, non-HFT) either the log of the average daily trading volume, the average share of limit order volume, or the average share of market order volume, across all trading days in June-July and September-October, respectively. $\Delta logmv_i$ and $\Delta inverse\ price_i$ are defined analogously using the log of the market capitalization and inverse share price of stock $i$, respectively. This procedure corresponds to the estimation of treatment effects based on a cross-sectional regression with time-series collapsed information suggested in Bertrand, Duflo, and Mullainathan (2004), with additional control variables. $T$-statistics based on White standard errors robust to heteroskedasticity are given in parentheses. $***$, $**$, and $*$ denote statistical significance at the 1%, 5%, and 10% level, respectively.

| Group/Variable | Log volume | Share Limit (%) | Share Market (%) |
|---|---|---|---|
| HFT | -0.419*** | -0.727** | -7.152*** |
| | (-2.90) | (-2.28) | (-4.78) |
| MT | -0.236*** | -4.951*** | 3.732** |
| | (-2.73) | (-3.54) | (2.63) |
| Non-HFT | -0.011 | 5.677*** | 3.419** |
| | (-0.12) | (3.87) | (2.41) |
| Add. controls | Yes | Yes | Yes |
| # Treated | 38 | 38 | 38 |
| # Control | 29 | 29 | 29 |

## J. Tick size

One potential concern with the observation that non-SLP stocks are significantly more affected by the FTT than non-SLP pertains to the role of the minimum tick size. If, for example, the bid-ask spread of SLP stocks is frequently constrained by the minimum tick, the observed non-result for these stocks could be the consequence of the pure mechanics of such a microstructural friction. In particular, a deterioration of market liquidity will not necessarily lead to an increase in the bid-ask spread for stocks for which the minimum tick size represents a binding constraint. Instead, depth will tend to be affected significantly. In contrast, the bid-ask spread of unconstrained stocks will be more reactive to changes in liquidity conditions. To investigate this issue, we estimate a triple-diff specification where we allow the treatment effect to vary with the extent to which the pricing grid represents a binding constraint.

To this end, we compute $Cons_i$ as the stock-specific fraction of trading volume in the pre-event window that was executed when the bid-ask spread was equal to the minimum tick size. We then define the dummy variable $Tick_i$ as equal to one for stocks in the upper quartile of the cross-sectional distribution of $Cons_i$. Then, our triple-diff specification reads as follows

$$\begin{aligned} y_{i,t} &= \alpha_i + \gamma_t + \beta^{Aug} D_{i,t}^{Aug} + \beta^{Sep/Oct} D_{i,t}^{Sep/Oct} \\ &+ \delta^{Aug} Post_t^{Aug} \times Tick_i + \delta^{Sep/Oct} Post_t^{Sep/Oct} \times Tick_i \\ &+ \beta^{Aug-Tick} D_{i,t}^{Aug} \times Tick_i + \beta^{Sep/Oct-Tick} D_{i,t}^{Sep/Oct} \times Tick_i + \epsilon_{i,t}, \end{aligned} \tag{IA.11}$$

where $Post_t^{Aug}$ and $Post_t^{Sep/Oct}$ are dummy variables that take the value of one during August and September-October, respectively, and zero otherwise. The results are tabulated in Table IA.16. As can be seen, the coefficient $\beta^{Sep/Oct-Tick}$ on the interaction term is never significant, indicating that differences in tick size constraints do no explain differences in the FTT's impact across stocks. We also obtain qualitatively similar results when using $Cons_i$ directly instead of $Tick_i$.

**Table IA.16**

Causal impact of the FTT, interacted with the share of tick-constrained spreads

This table contains the estimates for the coefficients $\beta^{Sep/Oct}$ and $\beta^{Sep/Oct-Tick}$ from the regression equation (IA.11). $\beta^{Sep/Oct}$ identifies the average impact of the FTT, and $\beta^{Sep/Oct-Tick}$ the FTT's additional impact for a stock in the top quartile. Columns (1) and (2) report results for SLP stocks, and columns (3) and (4) for non-SLP stocks. $T$-statistics based on standard errors clustered at the stock and day level are given in parentheses. $***$, $**$, and $*$ denote statistical significance at the 1%, 5%, and 10% level, respectively.

| Variable | SLP stocks | | Non-SLP stocks | |
|---|---|---|---|---|
| | $\beta^{Sep/Oct}$ | $\beta^{Sep/Oct-Tick}$ | $\beta^{Sep/Oct}$ | $\beta^{Sep/Oct-Tick}$ |
| *log volume* | -0.037 | 0.021 | -0.237*** | 0.089 |
| | (-0.62) | (0.26) | (-2.88) | (0.73) |
| *realized volatility* | 0.781 | -1.140 | 2.449* | -1.466 |
| | (0.76) | (-0.52) | (1.67) | (-0.67) |
| *range* | -0.047 | -0.070 | 0.217 | 0.098 |
| | (-0.39) | (-0.26) | (1.32) | (0.39) |
| *quoted spread* | 0.200 | -0.089 | 1.267 | -1.189 |
| | (1.40) | (-0.32) | (1.44) | (-1.24) |
| *effective spread* | 0.211 | -0.135 | 1.034 | -0.909 |
| | (1.61) | (-0.56) | (1.60) | (-1.26) |
| *price impact* | 0.289* | -0.413 | 1.835*** | -0.838 |
| | (1.91) | (-1.34) | (4.49) | (-1.46) |
| *realized spread* | -0.078 | 0.277 | -0.800* | -0.073 |
| | (-0.52) | (1.22) | (-1.81) | (-0.12) |
| *depth* | -9.861* | -11.425 | -2.797 | 2.429 |
| | (-1.75) | (-1.11) | (-1.30) | (0.86) |
| *resiliency* | -0.006 | -0.007 | -0.029** | 0.020 |
| | (-0.52) | (-0.30) | (-2.34) | (1.00) |
| *abs. autocorrelation* | 0.012** | -0.012 | 0.007 | -0.001 |
| | (2.18) | (-1.27) | (1.05) | (-0.10) |
| # Treated | 49 | | 38 | |
| # Control | 27 | | 52 | |
| # Obs. | 8,284 | | 9,810 | |

# *K. Non-SLP stocks: regression discontinuity design approach*

Our identification strategy for non-SLP stocks is very close to a regression discontinuity design (RDD) approach: we compare the treated non-SLP stocks, that is the 52 smallest

French stocks with a market capitalization above 1 bn EUR, to both 5 non-French stocks above this threshold and the 47 largest French and non-French stocks below.

As a robustness check, we present results from alternative specifications, using only stocks even closer to the 1 bn EUR threshold. As is well known, this implies a trade-off: selecting fewer stocks on both sides of the discontinuity leads to less bias in the estimation as we get closer to a random assignment of the treated and control stocks, but with fewer observations there is more noise in the estimation. Table IA.17 reports our baseline results on non-SLP stocks (Table IV) as well as the results of two alternative RDD specifications, using either 35 stocks above and below the threshold, or 30 stocks above and below the threshold. The results are very similar to those of Table IV, even quantitatively.

## Table IA.17

Causal impact of the FTT non-SLP stocks, regression discontinuity design

This table contains the estimates for the coefficient $\beta^{Sep/Oct}$ from the regression equation

$$y_{i,t} = \alpha_i + \gamma_t + \beta^{Aug} D_{i,t}^{Aug} + \beta^{Sep/Oct} D_{i,t}^{Sep/Oct} + \epsilon_{i,t}, \qquad \text{(IA.12)}$$

where $y_{i,t}$ denotes one of the ten market quality variables defined in Section II. Column (1) replicates column (2) of Table IV. Column (2) uses the 35 smallest French stocks above 1 bn EUR as the treated group and the 35 largest French and non-French stocks below 1 bn EUR as a control group. Column (3) uses only 30 stocks on each side. $T$-statistics based on standard errors clustered at the stock and day level are given in parentheses. $***$, $**$, and $*$ denote statistical significance at the 1%, 5%, and 10% level, respectively.

| Variable | (1) 38/52 | (2) 35/35 | (3) 30/30 |
|---|---|---|---|
| *log volume* | -0.224*** | -0.263*** | -0.259*** |
| | (-3.29) | (-3.66) | (-3.27) |
| *realized volatility* | 2.420** | 2.403* | 2.319* |
| | (2.03) | (1.91) | (1.66) |
| *range* | 0.256* | 0.288** | 0.279* |
| | (1.87) | (2.03) | (1.75) |
| *quoted spread* | 1.254* | 1.764*** | 1.729** |
| | (1.85) | (2.60) | (2.26) |
| *effective spread* | 1.088** | 1.134** | 1.128* |
| | (2.08) | (2.04) | (1.82) |
| *price impact* | 1.835*** | 1.790*** | 1.612*** |
| | (5.34) | (4.65) | (3.77) |
| *realized spread* | -0.746** | -0.655* | -0.483 |
| | (-2.14) | (-1.86) | (-1.23) |
| *depth* | -2.112 | -3.415 | -3.811 |
| | (-1.13) | (-1.42) | (-1.40) |
| *resiliency* | -0.029*** | -0.020** | -0.022** |
| | (-3.24) | (-2.30) | (-2.16) |
| *abs. autocorrelation* | 0.007 | 0.009 | 0.014** |
| | (1.20) | (1.47) | (2.46) |
| # Treated | 38 | 35 | 30 |
| # Control | 52 | 35 | 30 |
| # Obs. | 9,810 | 7,630 | 6,540 |

## L. *Synthetic vs. Physical index funds*

The results in Table XI showed that the French FTT had a particularly significant impact on the holdings and the trading activity of passive investors. This result may appear puzzling based on the conventional view of index funds as buy-and-hold investors that only trade in response to changes in the benchmark composition or in-/outflows from end investors. Indeed, most U.S.-based index funds tend to track the underlying benchmark physically. However, a significant share of the European ETF industry is based on synthetic replication methods which rely on the use of total return swaps.[4] Accordingly, the actual portfolios held by these funds tend to differ substantially from the composition of the benchmark index. Hurlin et al. (2015) provide a more detailed overview and show that the portfolio composition of swap-based ETFs tends to change frequently. are flexible with respect to the stocks they physically hold, and may simply have chosen to stop holding French stocks subject to the tax. In order to test this, we build on the fact that the replication strategy differs systematically across fund providers. According to Morningstar (2012), 99% of the ETFs managed by DB X-Trackers, Amundi, and Lyxor are swap-based. Therefore, we classify these funds as investors using synthetic replication methods. In contrast, more than 95% of Blackrock's iShares ETFs track the underlying benchmark by physically investing into the actual index constituents. In addition, the 1940 Investment Company Act effectively requires U.S. index funds to invest at least 80% of their assets in the underlying benchmark. Hence, we assume that these two groups of investors are engaging in physical replication. This simple scheme allows us to classify 237 out of 320 index funds in our sample. In line with the idea that swap-based funds engage in more frequent trading, we find that the cross-sectional average of $turnover_f$ for these investors is 0.87, compared to $-1.65$ for funds assumed to physically track their benchmark.

In order to investigate whether the FTT's large impact on passive investors is related to their replication methodology, we re-estimate our cross-sectional regression for the subset of classified index funds. We start out by a simple regression of $did_f^H$ on a constant in order to

[4]According to Morningstar's 2012 report "Synthetic ETFs Under the Microscope: A Global Study", around 40% of the total assets under management in the European ETF industry were subject to synthetic replication methods throughout 2011.

confirm that the subset of index funds used for this analysis is comparable to set of all 320 passive investors in our dataset. The resulting coefficient estimate of $-0.208$ is indeed close to the average estimated treatment effect from column (2) of Table XI. Next, we add the variable $synthetic_f$, which takes a value of 1 for swap-based funds, and zero otherwise. The resulting estimates in column (2) suggest that the FTT's strongly negative impact on passive investors is entirely driven by funds with a synthetic replication strategy. While the constant is statistically insignificant, the coefficient estimate for $synthetic_f$ is $-0.797$ with a t-statistic of -6.66. This suggests that the FTT induced swap-based funds to reduce their holdings of French stocks by a staggering 55%. In contrast, "true" index trackers did not change their holdings materially. Column (3) confirms that this conclusion does not change materially when we add the remaining control variables. Note that we do not include $turnover_f$ due to its high correlation with $synthetic_f$ (around 0.76).[5]

Turning to the results on trading volume ($did_f^V$), column (4) again reveals that the average treatment effect for our subset of index funds, $-0.718$, is close to the estimate in column (3) of Table XI ($-0.245 - 0.456$). Adding the *synthetic* variable (column(5)), we see a similar pattern as before: While the coefficient estimate for the intercept is small and borderline significant (p-value -0.166), we observe a large and highly statistically significant decrease in the trading volume of swap-based funds of close to 90%. In other words, funds relying on synthetic replication virtually cease trading French stocks. While the addition of $log\ size_f$ and $price - to - book\ ratio_f$ in column (6) leaves this figure virtually unchanged, controlling for the large observed change in holdings ($did_f^H$) slightly attenuates the estimated treatment effect for synthetic index trackers.

In sum, we conclude that the observed negative impact on passive investors is entirely concentrated in those funds that use synthetic replication strategies. Given that the portfolios of these investors can differ considerably from the underlying benchmark composition, these investors have clear incentives to reduce their holdings and their trading activity in French stocks. In contrast, truly passive investors such as funds engaging in physical index replication were hardly affected by the FTT, in line with the conjecture of Stiglitz (1989).

[5]However, including this variable as additional regressor does not affect our results.

**Table IA.18**

Causal impact of the FTT on index funds' portfolio holdings and trading volume, for all stocks

This table contains coefficient estimates from a linear regression of fund-specific treatment effects in terms of portfolio holdings (columns (1) to (3)) and trading volume (columns (4) - (7)) on investment fund characteristics. The analysis is restricted to index funds. The dependent variables are defined as

$$did_f^H = [log(p_{Q2}^T.x_{f,Q3}^T) - log(p_{Q2}^T.x_{f,Q2}^T)] - [log(p_{Q2}^C.x_{f,Q3}^C) - log(p_{Q2}^C.x_{f,Q2}^C)] \quad \text{(IA.13)}$$

$$\text{and } did_f^V = [log(p_{Q3}^T.|\Delta x_{f,Q4}^T|) - log(p_{Q1}^T.|\Delta x_{f,Q2}^T|)] - [log(p_{Q3}^C.|\Delta x_{f,Q4}^C|) - log(p_{Q1}^C.|\Delta x_{f,Q2}^C|)],$$

where $x_{f,t}^T$ and $x_{f,t}^C$ denote the (column) vectors of holdings in treated and control stocks by fund $f$ at time $t \in \{Q2, Q3\}$ and $p_t^T$ and $p_t^C$ denote the associated (row) price vectors. $\Delta x_{f,t}^T$ is equal to $x_{f,t}^T - x_{f,t-1}^T$, with $\Delta x_{f,t}^C$ defined accordingly, and the notation $|.|$ is used for the element-wise absolute value of a vector. $log\ size$ denotes the log of total assets under management, in million USD. $price - to - book\ ratio$ denotes the fund's average price-to-book ratio based on its portfolio holdings. $synthetic$ is a binary variable equal to one for synthetic index funds, and zero otherwise. $T$-statistics based on standard errors clustered at the stock level are given in parentheses. $***$, $**$, and $*$ denote statistical significance at the 1%, 5%, and 10% level, respectively.

| | Holdings | | | Trading volume | | | |
|---|---|---|---|---|---|---|---|
| Expl. variable | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| $constant$ | -0.208*** | 0.024 | -0.005 | -0.718*** | -0.122* | -0.089 | -0.085 |
| | (-4.83) | (1.62) | (-0.21) | (-7.20) | (-1.66) | (-1.15) | (-1.08) |
| $synthetic$ | | -0.797*** | -0.700*** | | -2.049*** | -2.162*** | -1.663*** |
| | | (-6.66) | (-4.78) | | (-9.36) | (-8.51) | (-6.65) |
| $log\ size$ | | | 0.026 | | | -0.050 | -0.068 |
| | | | (1.55) | | | (-1.01) | (-1.37) |
| $price - to - book\ ratio$ | | | -0.120*** | | | 0.116 | 0.202*** |
| | | | (-2.73) | | | (1.50) | (2.74) |
| $did_f^H$ | | | | | | | 0.713*** |
| | | | | | | | (4.44) |
| $R^2$ | 0.000 | 0.298 | 0.344 | 0.000 | 0.367 | 0.379 | 0.441 |
| # Obs. | 237 | 237 | 237 | 237 | 237 | 237 | 237 |

## *M. American Depositary Receipts*

One feature of the French FTT is that trading in ADRs was exempt throughout our sample period. Accordingly, it is possible that some investors substituted their trading in French stocks by using these instruments. However, there are only six stocks in our sample with sponsored ADRs that were actively traded on U.S. exchanges during the sample period.[6] For most of the remaining stocks subject to the FTT, unsponsored ADRs were occasionally traded in the U.S. OTC market, but the trading volume was typically below 1% of that traded on Euronext. Accordingly, the results emerging from the following analysis are only to be taken suggestive.

For each ADR, we collect data on trades executed on the various U.S. exchanges from Thomson Reuters Tick History and calculate the daily EUR trading volume using the ECB's official EUR/USD exchange rate.[7] Following Section I, we then estimate a simple diff-in-diff for the log of trading volume, where the ADRs serve as a control group for the respective French stocks subject to the FTT. The resulting coefficient estimate for the permanent impact, $\beta^{Sep/Oct}$, is equal to 0.50, with a t-statistic of 3.20 (using standard errors clustered at the stock level). This shows that trading volume in the six French stocks subject to the FTT increased strongly relative to their U.S.-traded ADRs. Despite the small sample size, this finding is clearly at odds with investors using these instruments in order to avoid paying the tax. Consequently, there are very little concerns that the temporary exemption of ADRs from the French FTT affects the validity of our causal impacts throughout the paper.

## *N. Tax revenues*

One important issue surrounding the foreseen implementation of a pan-European FTT is the question of how much revenue such a levy could raise. In its impact assessment of the European FTT, the European Commission uses a simple formula with various parameterizations to estimate potential revenues of a 10 bps tax on all equity transactions in the EU-27

[6] These stocks are: Alcatel-Lucent, CGG Veritas, France Telecom, Sanofi, Total, and Veolia Environnement.

[7] We discard trades occurring outside the official trading hours and also exclude data reported through Alternative Display Facilities.

(payable by both sellers and buyers, no exemption from market-making, no limitation to the largest companies). The estimates range from 4.8 to 6.5 billion EUR.[8] However, previous experiences have suggested that official revenue estimates usually tend to be overly optimistic (see e.g. Umlauf (1993)). A rather realistic estimate can be obtained by extrapolating the French implementation to other European markets.

According to a report of the lower house of the French Parliament, the French authorities levied 198 million EUR between August and December 2012 due to the FTT.[9] Extrapolating this figure to a full year yields an annual revenue of 475 million EUR. Importantly, this figure falls considerably short of the French authorities' estimate of 1.6 billion EUR per year, which once again points at an underestimation of the impact on revenue-generating market activity.

To obtain an estimate of how much an extended French FTT would yield, we assume that a country's tax revenue would be proportional to its share in the total turnover of European stocks with a market capitalization above 1 billion EUR (the tax base). If we call $T_i$ this total turnover in country $i$ and $i = F$ for France, we thus assume that the yearly revenue of the tax is equal to $475(T_i/T_F)$ million EUR. Table IA.19 presents the results for several European countries, where the stock turnover for country $i$ is based on stocks contained in the Stoxx 600 index.[10] An interesting cross-check consists in comparing our estimate for the UK, 573.6 million EUR, with the actual revenue obtained from the UK stamp duty. Over the period August-December 2012[11], HM Treasury received 977 million GBP, which corresponds to 2.9 billion EUR for a full year based on an average exchange rate of 1.2475 EUR/GBP. This is about 5 times our estimate, but given that the stamp duty (50 bps) is 2.5 times higher than the French FTT and is also levied on smaller stocks, our estimates appear rather reasonable. For all EU-countries, we arrive at an estimated revenue of 2.1 billion EUR, and restricting the set to the group of 11 that has committed to the FTT yields 1.7 billion EUR.

The comparison of our figures with the European Commission's projections of 4.8 - 6.5

[8]The documentation is available on the Commission's website: `http://ec.europa.eu/taxation_customs/taxation/other_taxes/financial_sector/`.

[9]Source: `http://www.assemblee-nationale.fr/14/rap-info/i1328.asp`.

[10]We compute country shares based on the turnover in the 6 months prior to the FTT's adoption. Note that several smaller EU countries are not represented in the index, but this is due to their low stock market capitalization. Given the 1 billion EUR threshold, this is unlikely to affect our results (the smallest ES600 component has a market capitalization below this threshold).

[11]See `http://www.hmrc.gov.uk/statistics/receipts/info-analysis.pdf`.

billion EUR suggests that the potentially lost revenue through an exemption of market-making and a restriction to stocks of large companies may be rather small compared to the potential inefficiencies arising from a tougher implementation.

**Table IA.19**
Country share of EuroStoxx600 turnover for companies with market capitalization above 1 billion EUR, and imputed potential tax revenues

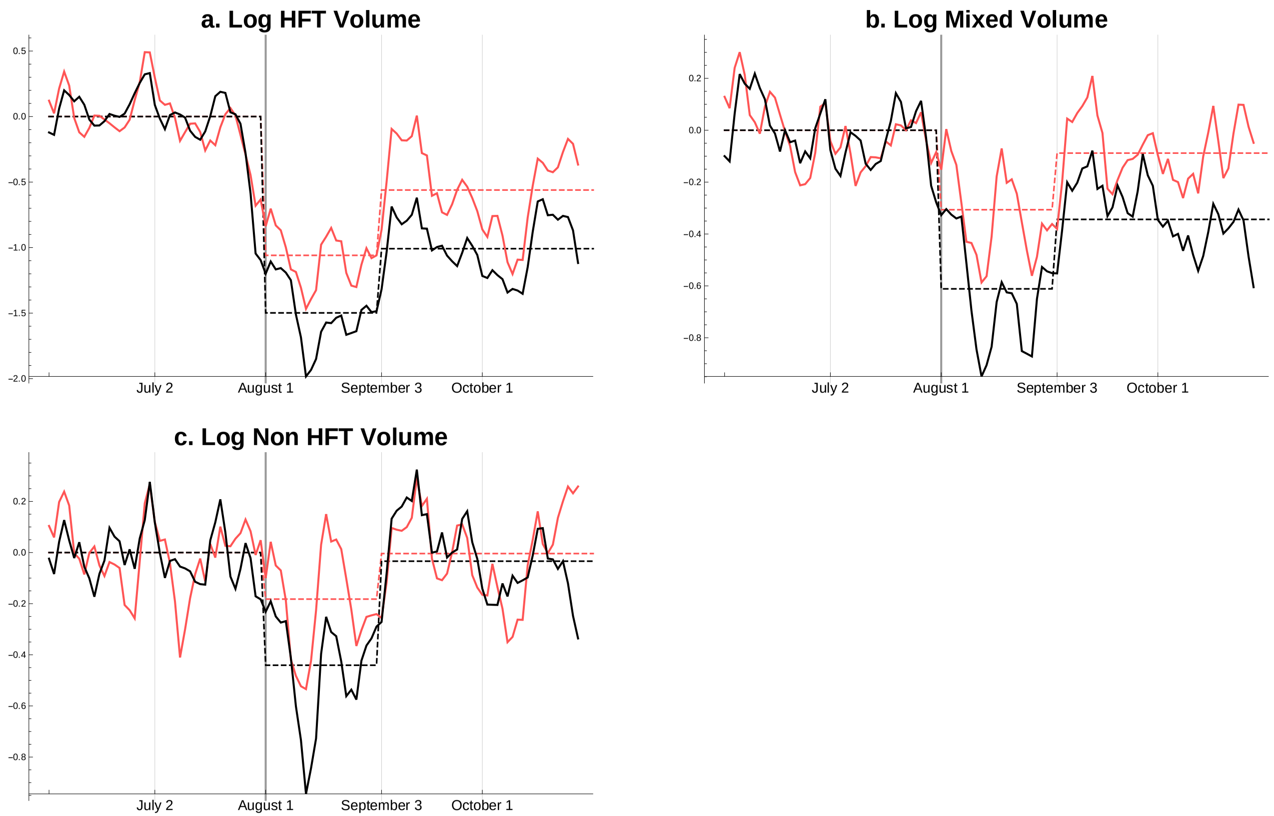
This table contains the country-specific shares in Euro Stoxx 600 turnover (in percentage points) as well as the resulting revenue estimates for an implementation of the French FTT. The underlined countries are among the 11 that have committed to the implementation of an FTT.

| Country | Turnover (share) | Estimated revenues (Million EUR) |
|---|---|---|
| Austria | 0.3 | 9.3 |
| Belgium | 1.3 | 36.5 |
| Denmark | 1.4 | 39.4 |
| Finland | 2.0 | 54.1 |
| France | 17.5 | 475.2 |
| Germany | 19.0 | 516.1 |
| Greece | 0.1 | 3.1 |
| Ireland | 0.7 | 18.5 |
| Italy | 9.6 | 260.3 |
| Luxembourg | 0.9 | 24.7 |
| Netherlands | 6.0 | 162.5 |
| Portugal | 0.3 | 8.4 |
| Spain | 13.8 | 376.3 |
| Sweden | 6.0 | 164.6 |
| United Kingdom | 21.1 | 573.6 |
| Total - EU | 100.0 | 2,148.9 |
| Total - 11 | 61.9 | 1,685.2 |

## *O. Additional graphs*

**Figure IA.2.** Graphical illustration of the causal impact of the FTT on non-SLP stocks, breakdown by type of trader

This figure illustrates our difference-in-difference estimates for the causal impact of the FTT on the (log) trading volume of different trader groups. For each variable, we plot the cross-sectional average for French non-SLP stocks (in black) with a market capitalization of more than 1 billion EUR and French non-SLP stocks below this threshold (in light red), minus the respective pre-event average. For improved readability, we use 3-day moving averages. The dashed lines indicate the sub-period averages for June/July, August, and September/October.



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