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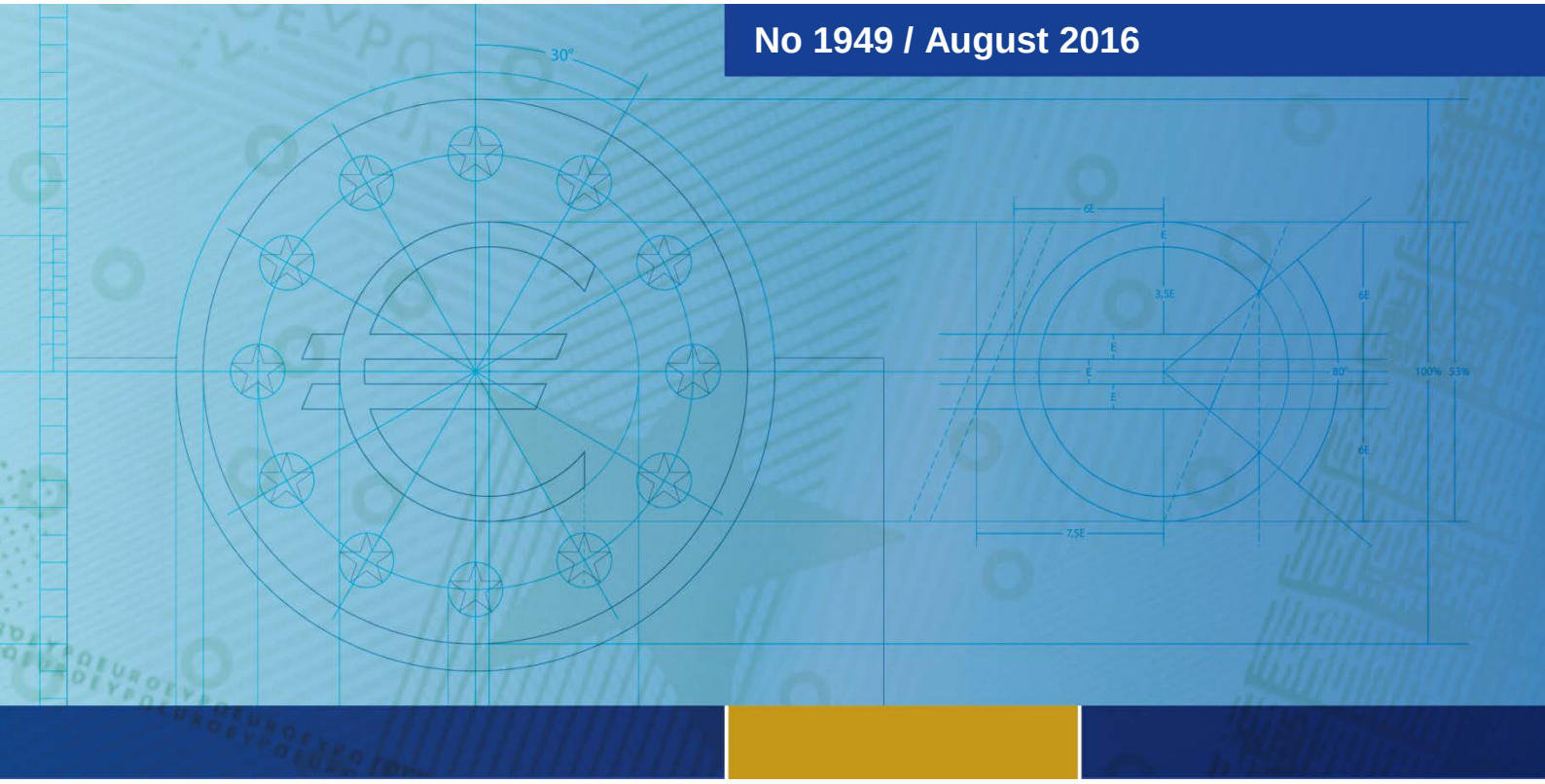
The stock market effects of a securities transaction tax: quasi-experimental evidence from Italy



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Abstract

We study the effects on the stock market of a securities transaction tax (STT). In particular, we focus on the recent introduction of a STT in Italy. Indeed, a peculiarity of the Italian STT is that it only concerns stocks of corporations with a market capitalization above 500 million euros. We exploit this feature via a differences-in-differences approach (comparing taxed and non-taxed stocks both before and after the introduction of the new tax). We find that the new tax widened the bid-ask spread and increased volatility, while it left transaction volumes and returns substantially unaffected. Results are broadly similar using a regression discontinuity design, in which we confront the performance of stocks just above the threshold with those just below.

JEL Classification: G14; G18; H24.

Keywords: Securities transaction Tax; Market liquidity; Market volatility.

NON-TECHNICAL SUMMARY

In the aftermath of the recent financial crisis, taxation of the financial sector has become a hotly debated topic among economists and the general public. Indeed, the crisis highlighted serious externalities and asymmetric information problems in the financial system - therefore suggesting a possible role for properly-designed corrective taxes.

As a consequence, new taxes on the financial sector have been recently proposed and in some cases implemented. In particular, the crisis revamped discussions about the desirability of a Security Transaction Tax (STT). For example, significant steps towards an harmonized STT have been done in the European Union; in the meanwhile, France introduced in August 2012 an STT on French listed shares, and Italy did the same starting from March 2013.

The effects of the STT are highly controversial. As economic theory does not provide a clear- cut guidance, the question has ultimately to be answered on empirical grounds. In the present paper, we provide an empirical investigation of the impact of the Italian STT on the domestic stock market. Our empirical strategy relies on a peculiarity of the Italian STT, namely the fact that it only concerns stocks of corporations with a market capitalization above 500 million euros. We can exploit this feature of the rules by adopting a differences-in-differences strategy, confronting the change in the performance of stocks above the threshold with that of stocks below the threshold. Moreover, given the discontinuity embedded in the rules, we can compare the performance of stocks just above the taxable threshold with those just below, providing in this way a robustness check for our differences-in-differences results.

We find that overall the introduction of the STT induced a reduction in liquidity for the stocks hit by the reform. We also find some evidence that volatility of the “treated” stocks increased. The first result is in line with the theory, as the tax can be considered an increase in the transaction cost. The second result suggests that the impact of the STT on the instability-increasing activity of the noise traders has not been sufficient to compensate the disincentive effect on the stabilizing activity of informed agents.

We do not find any effect of the reform of pre-tax returns or on exchanged volumes. However, we only observe the effects of the STT on the regulated stock market, and there are reasons to believe that the impact on over-the-counter transactions could have been stronger. Indeed, the tax rate on OTC transactions was higher than the tax rate on transactions taking place on regulated markets, and the likely shift of transactions from the OTC to the regulated market is probably the main reason why we did not find evidence of a decrease in volumes on the regulated market.

Our findings are not sufficient to give a full-fledged welfare evaluation of the new tax. For this, we would need a detailed model of the economy, a fully-specified social welfare function and - even if this is often neglected - a full list of the alternative tax instruments available for the government. However, our assessment of the behavioral responses resulting from the tax can provide the basis for such a normative exercise.

1 Introduction[1]

In the aftermath of the recent financial crisis, taxation of the financial sector has become a hotly debated topic among economists and the general public. For example, pre-existing tax distortions may have contributed to the build-up of the excesses that ultimately triggered the crisis, such as excessive risk-taking and leverage, inflated asset prices, the spread of structured finance (e.g. Keen et al., 2010, Keen and de Moji 2012, Keen et al. 2013, Ceriani et al., 2012). Furthermore, the crisis highlighted serious externalities and asymmetric information problems in the financial system, therefore suggesting a possible role for properly-designed corrective taxes (Keen, 2010, Shakleford et al., 2010, Perotti and Suarez, 2011). Finally, as the crisis deteriorated public finances - partly due to the public support granted to distressed financial institutions - many governments need to rise additional revenues, and the financial sector appears to many an obvious contributor.

It is therefore not surprising that new taxes on the financial sector have been recently proposed and in some cases implemented (Keen, 2011a, Shakleford et al., 2010, Hemmelgarn and Nicodeme, 2012, IMF, 2010, Devereux et al., 2013).[2] In particular, the crisis also revamped discussion about the desirability of a Security Transaction Tax (STT).

In September 2011, the European Commission proposed a coordinated STT to be implemented in all member states.[3] In February 2013, the Commission proposed a directive on a harmonized SST to be implemented in 11 member states as a form of enhanced co-operation. While the initial proposals included all types of instruments, subsequently the eleven countries opted for a gradual implementation approach, possibly starting in 2016 with a tax on shares and related derivatives. The negotiation process is still under way.

[1] The views expressed in the paper are those of the authors and do not necessarily reflect those of the Bank of Italy or of the European Central Bank (ECB). We would like to thank Erich Battistin, Nicola Branzoli, Domenico Depalo, Antonio di Cesare, Giorgio Gobbi, Enrico Rettore, Giacomo Ricotti, Martino Tasso, seminar participants at the 108th conference of the National Tax Association and at the 27th conference of the Italian Public Economics Association (SIEP) for their useful comments and suggestions. All remaining errors are our own responsibility.

[2] For example, the IMF(2010), in a report commissioned by the summit of the G-20 leaders, endorsed two tax instruments: a financial stability charge on bank leverage (net of deposits) and a financial activity tax on excess profits and excess wages of financial institutions.

[3] This proposal failed to get support by all member states but 11 countries asked the Commission to submit the proposal to the council for authorizing enhanced cooperation.

In the meanwhile, France unilaterally introduced in August 2012 an STT on french listed shares, and Italy did the same starting from March 2013.[4]

The effects of the STT are highly controversial. As economic theory does not provide a clear-cut guidance, the question has ultimately to be answered on empirical grounds.

In the present paper, we provide an empirical investigation of the impact of the Italian STT on the domestic stock market.

Our empirical strategy relies on a peculiarity of the Italian STT, namely the fact that it only concerns stocks of corporations with a market capitalization above 500 million euros. We can exploit this feature of the rules by adopting a differences-in-differences strategy, confronting the change in the performance of stocks above the threshold with that of stocks below the threshold. Moreover, given the discontinuity embedded in the rules, we can compare the performance of stocks just above the taxable threshold with those just below, providing in this way a robustness check for our differences-in-differences results.

The main contribution of the paper, with respect to previous similar studies, is given by the reliable identification strategy provided by the quasi-experimental circumstances with which the STT has been introduced in Italy.

To give a preview of our results, we find that overall the introduction of the STT induced a reduction in liquidity for the stocks hit by the reform, while equity returns and exchanged volumes were unaffected. We also find evidence that volatility of treated stocks has increased. Notice that we only observe the effects of the STT on the regulated stock market. As we will discuss below, there are reasons to believe that the impact on over-the-counter transactions (for which the tax rate is twice as much) could have been stronger.

The rest of the paper is structured as follows. In Section 2 we review the empirical and theoretical literature concerned with the economic effects of STTs. In Section 3, we summarize the features of the Italian STT, and in Section 4 we describe the dataset used in the empirical analysis. In Section 5 we spell out our empirical strategy and present our results. Section 6 concludes.

[4] The UK has an STT since 1694. The UK stamp duty is levied at a rate of 0,5% on a relatively narrow base (market-makers and derivative transactions are exempt). Analogous taxes are levied in Swizerland and Japan.

2 Literature review

The theoretical literature on the STT is relatively small and does not reach a consensus on its effects. *Prima facie*, following the introduction of the tax, gross-of-tax returns should rise, and trading volume should decrease. Indeed, both reactions make sense as investors try to mitigate the effect of the tax on net-of-tax returns (Matheson, 2012, Kupiec, 1996, Lendvai et al., 2013). However, the impact on returns also depends on capital mobility: if capital supply is imperfectly elastic, after-tax returns can be lower in equilibrium; the effects on volumes can be instead exacerbated by migration of activities toward other markets and/or instruments.

On the contrary, the effects on liquidity are ambiguous, depending among other things on the market micro-structure and on the exact measure of liquidity adopted (Subrahmanyam, 1998, Dupont and Lee, 2007).

When it comes to the effect of the tax on volatility, the disagreement is even stronger. Proponents of the tax argue that it reduces volatility because it discourages the trading activity of destabilizing noise traders (Stiglitz, 1989, Summers and Summers 1989).[5] The STT should therefore be considered as a "pigouvian" form of taxation. On the other side, it is also true that it discourages rational and stabilizing traders, so the overall effect turns out to be theoretically ambiguous (Kupiec, 1996, Song and Zhang, 2005).

To sum up, the question concerning the effects of the SST has to be answered on the empirical ground.

In the seminal empirical paper on the subject, Umlauf (1993) studies the effect of the introduction (in 1984) and of the subsequent increase (in 1986) of an STT in Sweden.

[5] The erliest proposal can be probably found in a famous passage of Chapter 12 of Keynes' *General Theory*: "It is usually agreed that casinos should, in the public interest, be inaccessible and expensive. And perhaps the same is true of Stock Exchanges. That the sins of the London Stock Exchange are less than those of Wall Street may be due, not so much to differences in national character, as to the fact that to the average Englishman Throgmorton Street is, compared with Wall Street to the average American, inaccessible and very expensive. The jobber's "turn", the high brokerage charges and the heavy transfer tax payable to the Exchequer, which attend dealings on the London Stock Exchange, sufficiently diminish the liquidity of the market (...) to rule out a large proportion of the transactions characteristic of Wall Street. The introduction of a substantial government transfer tax on all transactions might prove the most serviceable reform available, with a view to mitigating the predominance of speculation over enterprises in the United States".

He finds a negative effect on the aggregate stock market price, a large decrease in traded volumes, but no clear effect on price volatility.[6] Subsequenty, Hu (1998) examines 14 tax rate changes in four Asian markets (Hong Kong, Taiwan, Japan and Korea) from 1974 to 1994: he finds that prices go down when the tax increases, and turnover (albeit less clearly) seems to decrease; no clear pattern emerges concerning volatility (it goes down in 6 out of 13 episodes). Baltagi et al. (2006) looks at the 1997 increase in the Chinese stamp duty tax and observe a significant increase in volatility, but their results are otherwise in line with those of Umlauf (1993) and Hu (1998).[7]

One important methodological limitation of the aforementioned studies is that they use simple difference-of-means tests comparing market outcomes before and after the tax change.[8] This approach neglects the possibility that other market wide changes might confound the actual effects of the tax change.[9] A step forward in this respect is made by Liu (2007), which uses American Depository Receipts (ADR) of Japanese stocks - which are not subject to the home country's tax legislation - as a control group to assess a reduction in the Japanese STT which took place in 1989. However, the author himself admits that his strategy is problematic because, due to arbitrage, the prices of ADRs and of their "parent" stocks tend to closely move together. In the same vein, Bond et al. (2005) argue that the price effects of an STT change are stronger for shares with higher turnover (because for them the present discounted value of future tax payment is higher). Therefore, they use high-turnover (resp. low-turnover) shares as the treatment (resp. control) group in a differences-in-differences analysis of the 1990 announcement of a reduction in the UK stamp duty tax. They find, in line with their argument, that high-turnover shares are more affected by the reform. Their research design, however, has two limitations: first, the control group

[6] Similar results are obtained if the stock market volatility is normalized dividing it by the corresponding volatility of the NYSE.

[7] Phylatkis and Aristidou (2007) study the effects of changes in the Greek STT. They estimate a GARCH model analogous to the one used by Baltagi et al. (2006) and find no effect altogether of tax changes, neither on the mean nor on the volatility of daily returns. However, they find that the effect on volatility becomes significantly positive (resp. negative) if one looks at bullish (reasp. bearish) market periods.

[8] Baltagi et al. (2006) complement their analysis estimating a GARCH model, but also in this case the effect of the tax change is identified via a time dummy equal to one in the post-reform period.

[9] Similar limitations are to be found in the very small literature which studies the effects of transaction tax rates on derivatives markets (see Chou and Wang, 2006)

was also exposed to the treatment; second, the threshold value used to separate the control from the treatment group is arbitrary.[10]

A full-fledged differences-in-differences approach has been adopted only very recently, to study the introduction in France of an STT in August 2012. In particular, Meyer et al. (2014) and Colliard and Hoffmann (2013) compare the performance of French stocks with those of their British (Meyer et al., 2014) and Dutch (Colliard and Hoffman, 2013) counterparts. Both studies find a negative effect on volumes, but not on the bid-ask spread. Colliard and Hoffmann (2013) also find that volatility was not affected by the tax.[11]

Before concluding, it is also worth mentioning a related literature which does not look at STT changes but at the effects of other portions of the overall transaction costs. An example is the size of the brokerage commission fees. In an early and influential paper, Jones and Seguin (1997) study the effects of the shift from a fixed to a negotiated commission on transactions on the American stock exchange which took place in 1975. This change was followed by a sizable reduction in commissions. To assess the effects of the change, Jones and Seguin (1997) use as a control group the Nasdaq index (Nasdaq transactions were indeed not subject to the change). They find that changes in volatility between the pre-reform and the-post reform period were proportionally more pronounced in the treated portfolio (for instance, NYSE/AMEX volatility moved roughly one-to-one with Nasdaq volatility before the reform, but fell to 75 % of Nasdaq volatility afterwards). However, Liu and Zhu (2009) find opposite results. They apply to the commission deregulation which took place in Japan in 1999 the same methodology of Jones and Seguin (1997), albeit with a different control group (the portfolio of Japan ADR stocks).

3 The Italian STT

Starting from March 1st, 2013, the Italian government levies: (i) a tax on all transactions made - either by residents or non residents, and wherever the transaction occurs -

[10] Furthermore, they only look at prices, nor at volatility and volumes.

[11] A similar exercise on the French STT - with similar results - has been performed by Hafenkorn Zimmerman (2013). They used as a control group the German shares included in the DAX 30 index.

in shares issued by Italian companies[12], and (ii) a tax on high-frequency trading involving Italian shares.[13]

The introduction of the new taxes was decided in December 2012[14], but the details were spelled-out only in February 2013[15].

For 2013, tax rates are set at 0,12% of the value of the transaction if it took place in regulated markets (or multilateral trading facilities) and 0,22% if it took place "over the counter". Concerning the tax on high frequency trading, buy or sell orders modified or deleted are taxed with a tax rate of 0,02%, provided they happen within half a second and exceed a certain ceiling.

Some operations are exempted from the transaction tax: primary market transactions, temporary purchases of securities (such as repurchase agreements), intraday operations, intra-group transactions. Also, some subjects are exempted: the European Union, States, central banks, central clearing counterparts, market makers, mutual funds and pension funds. The exemption for market makers does not extend to OTC brokers operating as liquidity providers.

Finally - and crucially for our analysis - shares of corporations with a market capitalization below 500 million euros are exempted. This feature of the Italian reform is helpful for our purposes because - contrary to most previous empirical studies - it provides us with a natural split of our sample in a control (corporations with less than 500 million market capitalization) and a treatment group (corporations with more than 500 million market capitalization). Another crucial aspect of the reform is that the relevant capitalization is computed as of November 2012, i.e. at a date at which the tax was not yet discussed in the parliament. As we will discuss more at length below, this justifies the assumption - which in turn underlies our identification strategy - that corporations were not able to manipulate

[12] The tax applies to both quoted and non quoted shares. It is formally paid by the buyer and it is calculated on the end-of-the-day net market value of the transactions.

[13] The law passed in December 2012 envisaged two further taxes, on derivatives and on high frequency trading on equity derivatives; these two taxes are levied starting from September 2013. According to the February decree, these taxes shoud have started in July 2013, but in June a second decree (Decree law n. 69, 21 June 2013) postponed their introduction.

[14] Law n. 228, 24 dec. 2012, art. 1, cc. 491-500.

[15] Decree of the Ministry of the Economy, 21 feb. 2013.

the treatment variable (market capitalization) in order to be sure to avoid the tax.

4 Dataset and descriptive statistics

Our analysis relies on daily prices of Italian stocks between March 2012 and September 2013 (i.e.12 months before and 6 months after the introduction of the financial transaction tax).[16] The number of listed Italian shares is around 320, but we exclude from the analysis preference and saving shares. Furthermore, we do not consider foreign shares which are not subject to the taxation. We also excluded 10 shares which were subject in the period covered by our analysis to abnormally large fluctuations (daily returns of above 50% in absolute terms; for all of them, we checked that these fluctuations could be clearly explained by reasons totally unrelated to the tax). The number of remaining shares ranges is around 240, among which around 70 are subject to the tax.[17]

In Table 1 we show how the outcomes of interest changed after the introduction of the STT, with respect to the pre-reform period, distinguishing between the treated and the control group.

Following the introduction of the tax, traded volumes decreased for the treated shares, whereas they increased for the control group. Furthermore, gross returns increased for the treated shares, while they remained constant for the control shares. Bid-ask spreads decreased more in the control group than in the treated group (-0.8 against -0.1 percentage points). Finally, volatility, which was roughly the same in the two groups before the reform, decreased by almost twice as much among the control shares than among the treated ones.

In the next section we will see whether these results are robust to a more formal econometric analysis.

A complementary way to look at the data, often used in RD analysis, is proposed in Figure 1. The figure refers to the month immediately after the reform. It includes a smooth polynomial interpolation of the conditional mean of the outcome variables, for control and

[16] Data taken from Thomson Reuters Datastream.

[17] We use an unbalanced panel. The number of stocks included in the sample ranges between 233 and 239 over the sample period.

Table 1: Summary Statistics.

	March 2012 - February 2013		March 2013 - September 2013	
	Not taxed	Taxed	Not taxed	Taxed
	Daily bid-ask spread (% of the bid price)			
Median	2.10	0.27	1.30	0.17
P10	0.61	0.07	0.63	0.09
P90	7.18	2.62	3.97	0.59
	Daily transaction volume (number of shares)			
Median	31	1,030	34	868
P10	3	115	3	98
P90	457	20,311	335	15,603
	Daily returns (%) (1)			
Median	-0.01	0.08	0.005	0.14
P10	-0.63	-0.61	-0.41	-0.45
P90	0.55	0.60	0.47	0.60
	Standard deviation of daily returns (1)			
Median	2.04	2.09	1.66	1.86
P10	1.02	1.19	0.87	1.18
P90	4.00	3.56	3.08	2.90

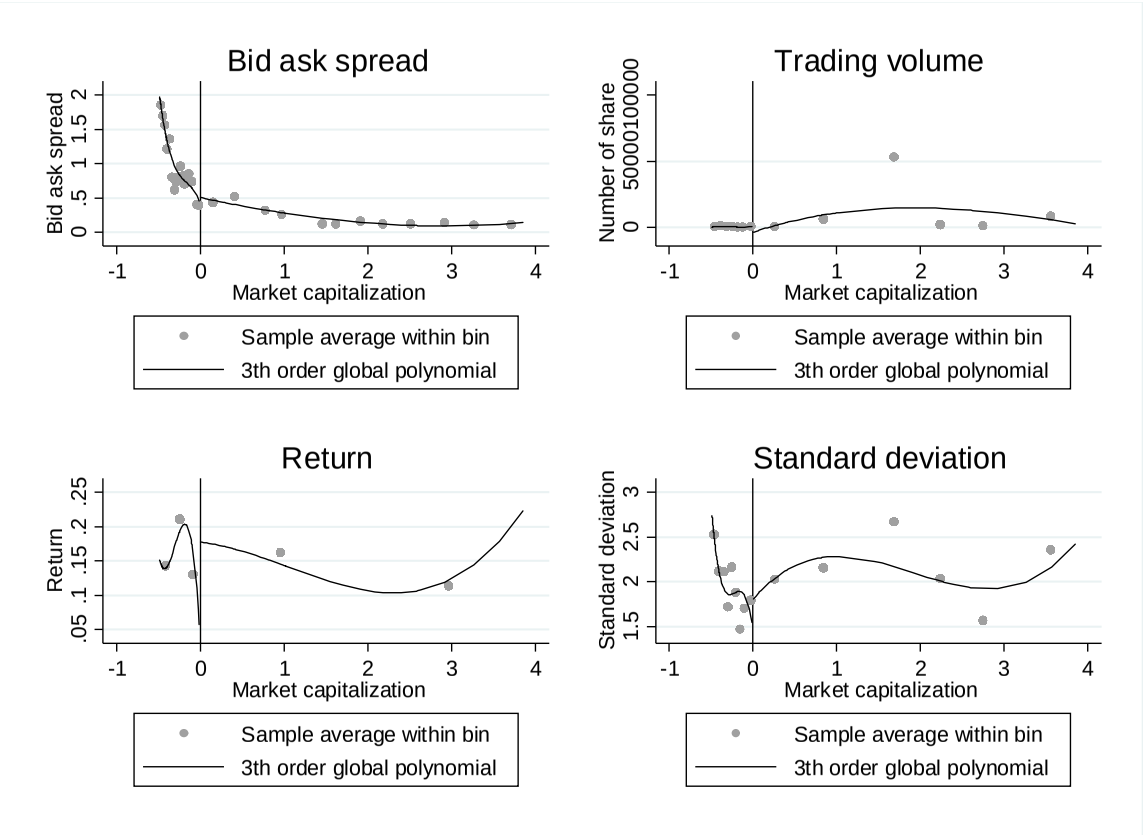
Source: Datastream.
Note: (1) Montly average of daily returns.

tratment units separately, and a collection of local sample means of the same variables (the local intervals, or "bins", over which the sample means are computed, are chosen endogenously, with the approach recently suggested by Calonico et al. (2014a)).

The visual evidence provided in Figure 1 is in line with the descriptive statistics reported in Table 1: at the threshold it is possible to observe a discontinuous increase in the bid-ask spread and in the gross-of-tax returns, as well as a decrease in trading volume, in line with what one could expect in theory. We also observe a jump in volatility.

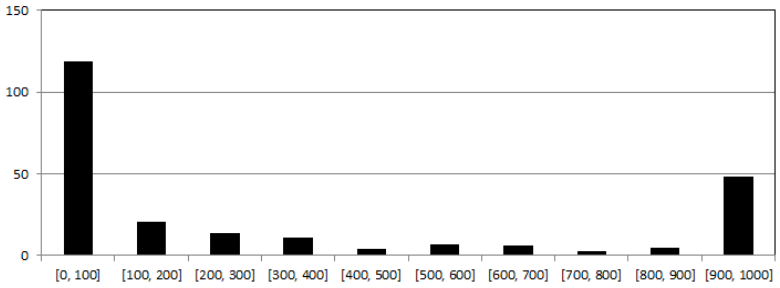
However, as it can be seen from the figure, the number of observations near the threshold is very small (actually, for most corporations the relevant market values were either below

Figure 1: Regression Discontinuity plots (Calonico, Cattaneo, and Titiunik, 2014).



100 or above 900 millions of euro; Figure 2).

Figure 2: Size distribution of observations.



As we will further discuss below, having few observations near the threshold makes it difficult to capture precisely the change in the mean value of the dependent variables at the threshold.

5 Empirical strategy and results

As in the previous section, we will exploit two different sources of identification. First, focusing on the longitudinal dimension of our data, we adopt a differences-in-differences approach. Second, to complement this analysis, we exploit the sharp discontinuity in the treatment at a pre-set market capitalization value, resorting to a regression discontinuity (RD) design.

We take the differences-in-differences as our baseline because, while the RD approach assures a high degree of internal validity of our results (in particular, as we will argue below, it provides unbiased estimates under very weak conditions), RD uses only a small part of the information included in our data set. First, it ignores the period before the reform. Second, it ignores what happens to the corporations whose market value is far from the treatment threshold. As a consequence, from a practical perspective RD leaves us with very few observations and imprecise estimates. Moreover, concerning the interpretation of our results, with RD we cannot say much about the effects of the reforms away from the threshold - which is of course a very relevant issue. This notwithstanding, RD provides a

useful check on the robustness of the differences-in-differences estimates.[18]

5.1 Differences-in-Differences estimation

The differences-in-differences method can be implemented by running the following OLS regression:

$$y_{i,t} = \alpha_i + \lambda_t + \beta \cdot 1_{\{x_i \geq 500\}}(x_i) \cdot Post_t + f(x_{it}) + u_{i,t} \quad (1)$$

where the $y_{i,t}$ is the monthly average of the outcome variable; x_i is the market value in November 2012; $Post_t$ is a variable taking value of one in the period after the reform is implemented (i.e. from March 2013 onwards) and zero before; $f(.)$ is a (possibly non linear) function. Notice that we control for individual (α_i) and time (λ_t) fixed effects. The time span includes the year before and the **six** months after the reform.

We look at four outcome variables (y_i), namely: the monthly average of daily returns[19], of the daily trading volume (proxied by the number of transactions) and of the bid-ask spread (expressed as a fraction of the bid price, in percentages), and the monthly standard deviation of daily returns.

Our estimates - shown in Table 2 - suggest that there is a significant effect of the reform on the bid-ask spread, which widens significantly in the post-treatment period for the stocks subject to taxation, relative to those that avoided the treatment. From an economic viewpoint the effect is substantial, being in most estimates of the order of 0.5%. To put the estimated values in perspective, consider that the median bid-ask spread among the exempted stocks included in our exercises, for the month of March 2013, was about 0.7%.

Moreover, the differences-in-differences estimates also suggest that the reform has increased volatility. Quantitatively, this latter effect amounts to an increase of about 10% (with respect to the median value for the exempted stocks in March 2013).

Differences-in-differences estimates are potentially exposed to the econometric problem

[18] The same approach can be found, for example, in Blundell et al. (2004) and in Lemieux and Milligan (2008). Indeed, both papers look both at RD and at differences-in-differences estimates, in order to assess the robustness of their results.

[19] Dividends are capitalized in the price index use to compute returns.

Table 2: Differences-in-differences estimates

	Bid-ask spread	Trading volume
	(1)	(1)
Financial Transaction Tax	0.618*	-438
f(share's capitalisation)	N	N
Time dummies	Y	Y
Individual fixed effect	Y	Y
Number of obs	4291	4291
Number of groups	239	239

	Return	Standard deviation
	(1)	(1)
Financial Transaction Tax	0.049	0.14*
f(share's capitalisation)	N	N
Time dummies	Y	Y
Individual fixed effect	Y	Y
Number of obs	4291	4291
Number of groups	239	239

Note: (1) Monthly observations between March 2012 and August 2013.

highlighted by Bertrand et al. (2004), namely a tendency (due to correlated residuals) to find a significant treatment effect even when there is none, because the standard deviation of the estimators is underestimated. However, this problem should be partly avoided as we use standard errors clustered at the stock level. Furthermore, results are unchanged even if we follow the Bertrand et al. (2004) recipe of collapsing the dataset into a T = 2 panel, with variables averaged over the pre- and post-treatment period respectively.

The differences-in-differences approach relies on the assumption that, without the treatment, the change in the outcome variable for the treated population would have been the same as the change observed for the control group, conditional on the control variables (common trend assumption). While the common trend assumption is not directly testable - as it relies on a counter-factual scenario - a simple eye-ball test does not seem inconsistent with the assumption (Figure 3).

There are some more formal ways to indirectly assess the plausibility of the common trend assumption. As a first check, we conduct a battery of "placebo" experiments, testing whether a significant difference in the dynamics of bid-ask spread and volatility between taxed and non-taxed stocks appears even in periods in which the treatment does not take place (in other words, we assessed the effects of several "mock reforms"). To do this, we rerun several equation (1), changing each time the definition of the estimated β is significantly different form zero (which would signal a possible problem). It turns out that, in the six months before the reform, the placebo treatment seemingly had an effect on bid-ask spreads only in two out of six cases, and it had never effects in the case of volatility.

As a second check, another set of placebo experiments is performed using only non-taxed stocks. In particular, we pick at random a subset of them and pretend that they are treated; we then run our baseline regression using this fake treatment group. The empirical density distribution of the estimate of the interaction term over the 200 replication of the described exercise, shown in Figure 4 and Figure 5, correctly suggests that the placebo treatment has no effect.[20] All in all, the evidence seems to support the reasonableness of the common trend assumption.

[20] Both densities are clearly single-peaked, with zero mode.

Figure 3: Evolution over time of the outcome variables (bid-ask spread, transaction volumes, returns, volatility)

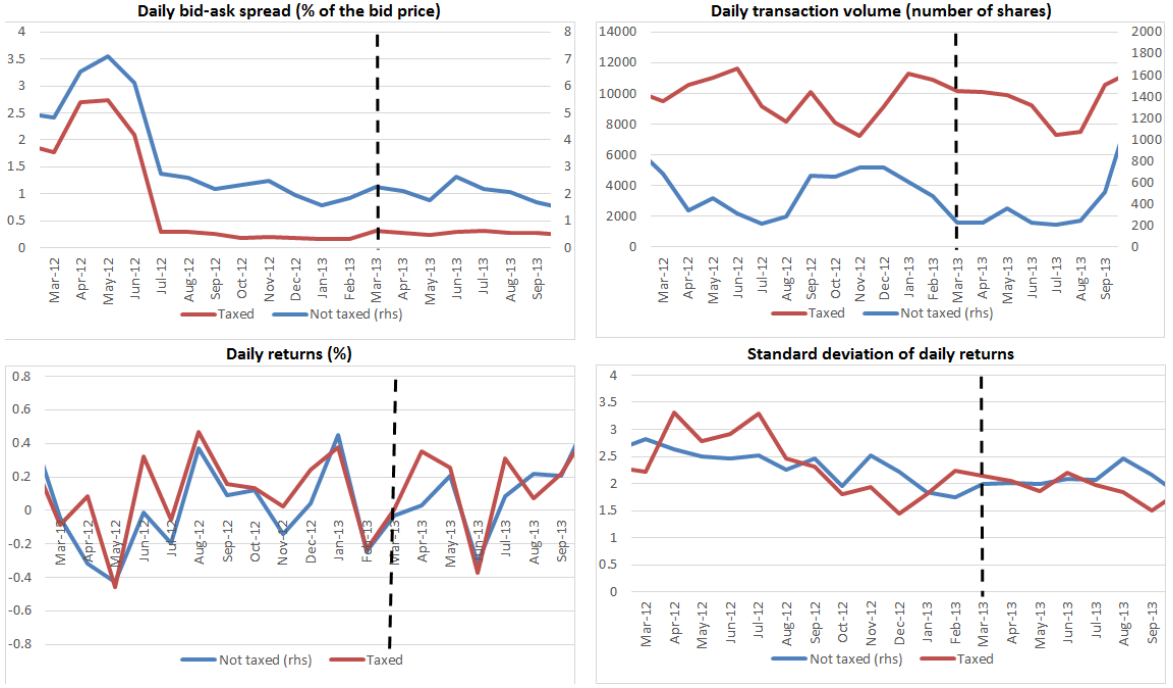
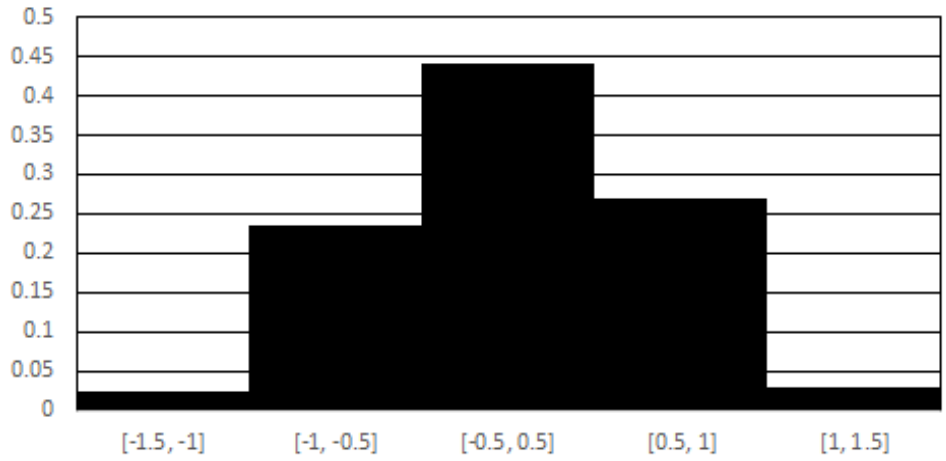


Figure 4: Distribution of the point estimate of the treatment effect ("Beta") in placebo experiments (Bid-ask spread).



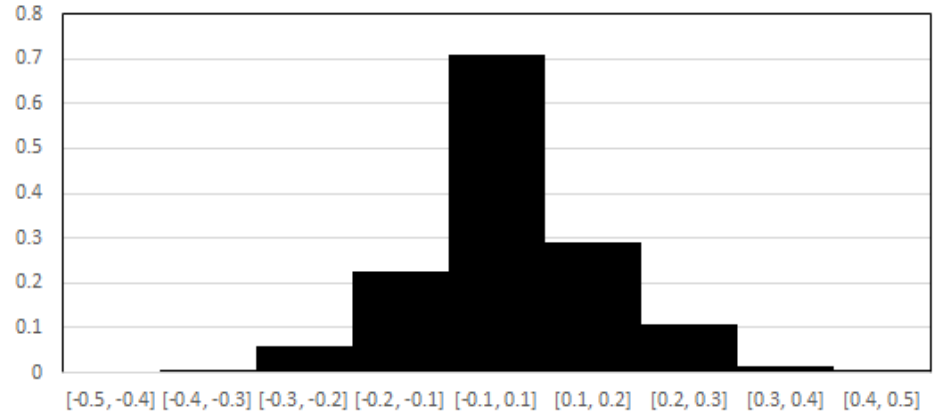
5.2 Regression discontinuity analysis

To test the robustness of our conclusion, we also pursue a different methodology, which rests on weaker identification assumptions with respect to the differences-in-differences method. Indeed, as we already mentioned, the way in which the Italian STT has been designed allows us to capture the average treatment effect of the reform by using a RD design. Intuitively, RD is based on the comparison of stocks just above and just below the threshold in the period immediately after the reform, and rests on the assumption that - in the absence of the reform - the outcome variables would be a smooth function of capitalization.

More technically (see e.g. Lee and Lemieux, 2010, Imbens and Lemieux, 2008), a RD strategy delivers unbiased results if for any unit i the expected values of the outcome variable y_i if the unit is treated ($y_i(1)$) and if it is not treated ($y_i(0)$) are both continuos at the threshold value which separates treated from non treated units (which in our case is equal to 500 billion euros), conditional on the value of the variable which determines the treatment status (x_i, which in our case is the market value in November 2012). If this is so, then the average treatment effect at the threshold is given by:

$$\beta = \lim_{\varepsilon \downarrow 0} E\left[y_i(1) | x_i = 500 + \varepsilon\right] - \lim_{\varepsilon \uparrow 0} E\left[y_i(0) | x_i = 500 + \varepsilon\right].$$

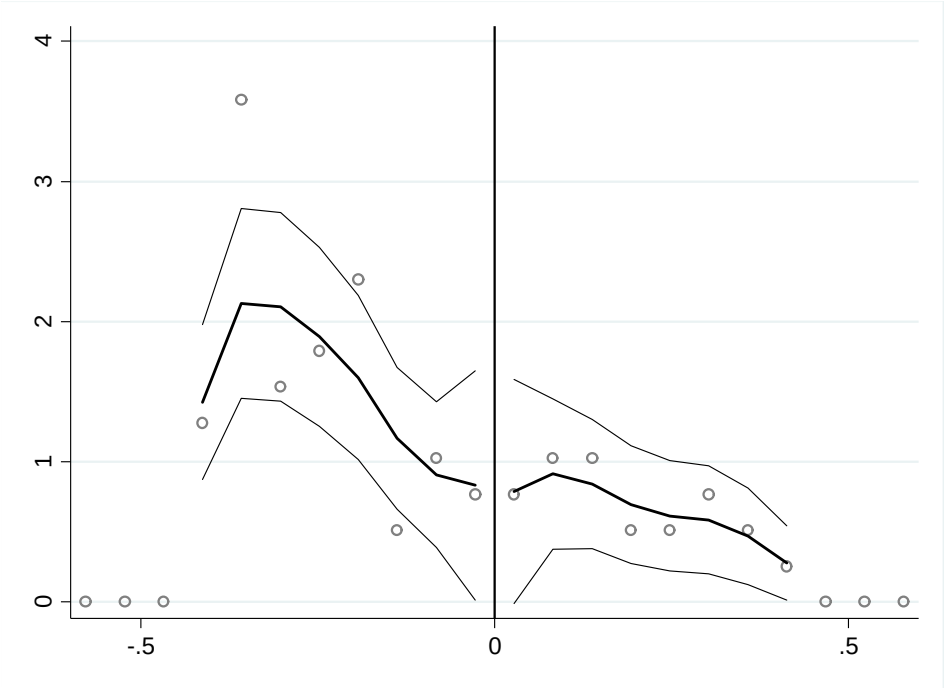
Figure 5: Distribution of the point estimate of the treatment effect ("Beta") in placebo experiments (Return volatility).



These continuity assumptions may not be plausible if agents are able to manipulate the running variable. McCrary (2008) develops a test of manipulation related to continuity of the density function with respect to the running variable (which in our case is market capitalization). Figure 6 reports the results of the McCrary test, and shows that the manipulation hypothesis can be rejected, as the density function does not show any discontinuity at the treatment threshold. Therefore, in our case the identification conditions for applying the RD approach appear satisfied.

Moreover, it can be shown (Lee, 2008) that the *continuity of conditional regression functions* assumption is satisfied even if the units which receive the treatment are able to manipulate the treatment variable, to the extent that their are not able to do it perfectly. This requirement is quite plausible in our set up, because the market capitalization which is relevant for assigning the treatment status is computed at a time (November 2012) before the tax was legislated. Corporations could not know, in November 2012, that the capitalization in that month would have been used as a threshold by a future law and, even if this were the case, no corporation would have been able to perfectly manipulate its market value in November 2012, in order to be sure to avoid taxation (notice also that the 500 billion threshold did not appear in any other law or regulation).

Figure 6: Density estimate of the running variable (stock market capitalization) in the neighborhood of the threshold.



As it is customary in RD settings, we estimate β by running the following regression:

$$y_i = \alpha + \beta \cdot 1_{\{x_i \geq 500\}}(x_i) + f(x_i - 500) + u_i$$

including only observations in an interval of the threshold capitalization.

As in any RD exercise, choosing the bandwidth around the threshold value involves a trade-off between accuracy - the more data points, the lower the variance of the estimator - and the bias which is induced if $f(.)$ is ill-specified. Calonico et al. (2014b) suggest a method for choosing the bandwidth which is fully data-driven and - under certain conditions - asymptotically optimal. Table 3 shows that the Calonico et al. (2014) procedure suggests an interval of around 100/150 millions euros around the threshold, depending on the outcome of interest. Therefore, in the rest of the section, we provide results for two possible bandwidths: in a first set of regression, we consider corporations with a market value between 400 and 600 million euros; in a second set of regressions we include all those between 350 and 650 billion

euros. Concerning the function of the treatment variable (i.e. the average capitalization in November 2012), we consider both a linear and quadratic specification.

We also used a specification in which f(.) is linear but in which its slope at the right of the cut-off point is allowed to be different from that at the left:

$$y_i = \alpha + \beta \cdot 1_{\{x_i \geq 500\}}(x_i) + \delta \cdot (x_i - 500) + \gamma \cdot 1_{\{x_i \geq 500\}}(x_i) \cdot (x_i - 500) + u_i,$$

which is often used in applications.

Across all these exercises, results are quite robust.

Table 3: Optimal bandwidth (Calonico et al., 2014b)

	Bandwidth (mln Euro)	Number of observations
Bid-ask spread	96	10
Trading volume	108	13
Return	171	21
Standard deviation	145	16

Note: Bandwidth in milions of Euro.

In the first row of Table 4 we study the effects of the reforms in the month after its introduction (March 2013). Results are broadly in line with those obtained with the differences-in-differences approach. The coefficient in the case of return volatility looses its statistical significance but remains positive. This is easily explained by the large standard deviation of the RD estimates, due in turn to the very low number of observations.

The β coefficient is statistically significant only in the case of the bid-ask spread, with a positive sign. It is also remarkable that the magnitude of the effect is very similar to the that found in the differences-in-differences exercise.

One could doubt that one month is not enough to judge the effects of the new tax. However, when we consider the first quarter or the first semester after the reform, results do not change: the only variable affected is the bid-ask spread, which widens by about the same amount irrespective of the time window considered (second and third rows of Table

4).

As suggested by Lee (2008), we performed several "placebo" experiments running our baseline RD specification on the months preceding the treatment (Table 7). These tests broadly confirm the lack of any discontinuity at the cut-off.

As a final exercise, we use the estimator suggested by Lemieux and Milligan (2008) which, in a sense, exploits at the same time both the margins that we separately used in the above sections. Indeed, it is an otherwise standard RD which exploits the pre-treatment/post-treatment dimension by using as a dependent variable the *change* of the relevant outcome (in our case, the first difference of the outcome between march 2013 and February 2013; of course we still consider only observations near the cut-off capitalization value). This strengthens further the credibility of the results, because we are comparing the same stock before and after the treatment:

$$\Delta y_i = \alpha + \beta \cdot 1_{\{x_i \geq 500\}}(x_i) + f(x_i - 500) + u_i.$$

The results, shown in Figure 5, are in line with our baseline estimates (standard errors are clustered at the individual stock level).

6 Concluding remarks

We provide an assessment of the stock-market effects of the introduction of a transaction tax, by looking at recent quasi-experimental data from Italy. We found a significant increase in the bid-ask spread. There is also evidence of an increase in the volatility of the taxed stocks.

The first result is in line with the theory, as the tax can be considered an increase in transaction costs. The second result suggests that the impact of the STT on the instability-increasing activity of the noise traders has not been sufficient to compensate the disincentive effect on the stabilizing activity of informed agents.

As we remarked in the introduction, we only observe the effects of the STT on the

Table 4: RD estimates

	Bid-ask spread						Trading volume					
	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
March 2013	0.5**	0.54***	0.59***	0.4***	0.43***	0.43***	1047	931	758	667	528	462
March - May 2013	0.33	0.39***	-	0.22*	0.25***	-	1,450	1,327	-	985	843	-
March - August 2013	0.27	0.33***	-	0.16	0.2***	-	1,084	991	-	734	628	-
f(market capitalisation)	lin	quad	lin. spline	lin	quad	lin. spline	lin	quad	lin. spline	lin	quad	lin. spline
Number of obs	11	11	11	17	17	17	11	11		17	17	
(Bandwidth)	*400/600*	*400/600*	*400/600*	*350/650*	*350/650*	*350/650*	*400/600*	*400/600*	*400/600*	*350/650*	*350/650*	*350/650*

	Return						Standard deviation					
	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
March 2013	0.77	0.83	0.87	0.68	0.72	0.74	0.08	0.04	0.01	0.15	0.12	0.11
March - May 2013	0.38	0.4	-	0.23	0.24	-	0.16	0.16	-	0.02	0.05	-
March - August 2013	0.24	0.25	-	0.19	0.19	-	0.23	0.24	-	0.04	0.06	-
f(market capitalisation)	lin	quad	lin. Spline	lin	quad	lin. Spline	lin	quad	lin. spline	lin	quad	lin. spline
Number of obs	11	11	11	17	17	17	11	11		17	17	
(Bandwidth)	*400/600*	*400/600*	*400/600*	*350/650*	*350/650*	*350/650*	*400/600*	*400/600*	*400/600*	*350/650*	*350/650*	*350/650*

Figure 7: RD estimates of "Beta" at different dates.

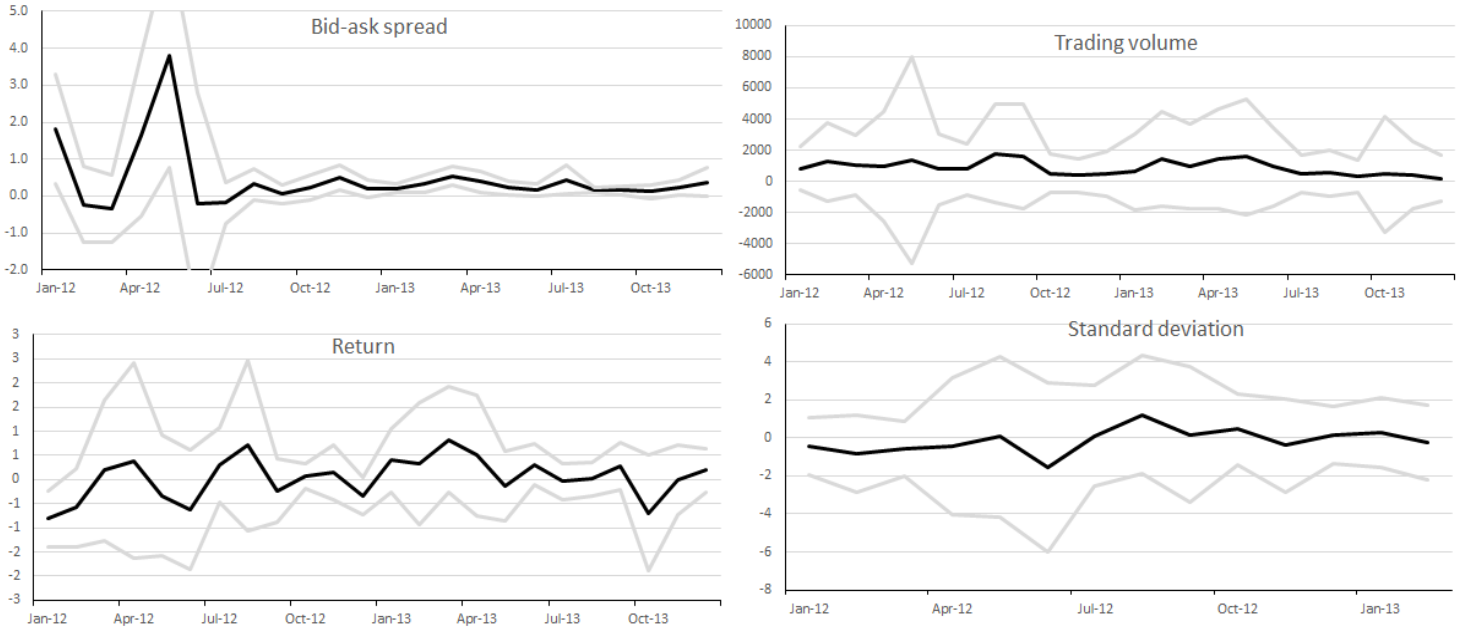
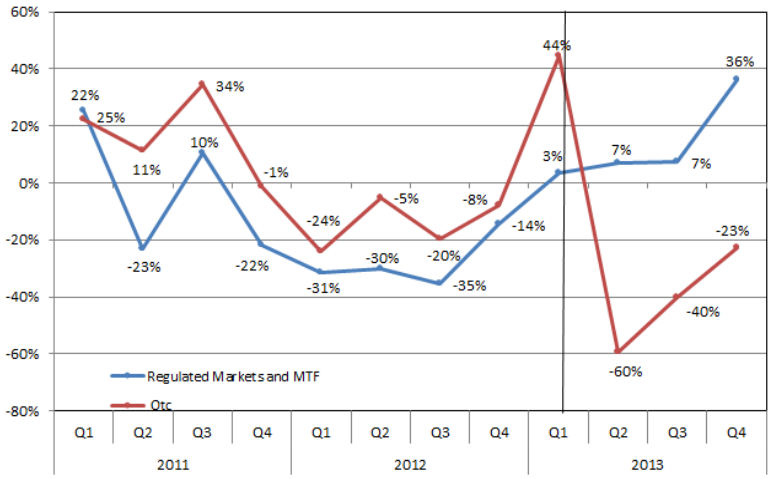


Table 5: RD regressions: dependent variable in first differences (March 2013- February 2013).

	Bid-ask spread				Trading volume			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Financial Transaction Tax	0.18*	0.19*	0.2**	0.21**	-643	-665	-534	-579
f(market capitalisation)	lin	quad	lin	quad	lin	quad	lin	quad
Number of obs	11	11	17	17	11	11	17	17
(Bandwidth)	*400/600*	*400/600*	*350/650*	*350/650*	*400/600*	*400/600*	*350/650*	*350/650*

	Return				Standard deviation			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Financial Transaction Tax	0.44	0.5	0.61	0.63	0.37	0.32	0.51	0.42
f(market capitalisation)	lin	quad	lin	quad	lin	quad	lin	quad
Number of obs	11	11	17	17	11	11	17	17
(Bandwidth)	*400/600*	*400/600*	*350/650*	*350/650*	*400/600*	*400/600*	*350/650*	*350/650*

Figure 8: Change in Trading Volumes (w.r. to the same quarter of the previous year)



regulated stock market, while the bulk of the impact could have been on over-the-counter transactions, for which the tax rate was twice as much.[21] While we do not have data on these transactions on a stock-by-stock basis, the aggregate statistics provided by the Italian Financial Markets Authority (Consob) signal that value of OTC transactions fell by 31% between March 2013 and August 2013, with respect to the same period of the previous year, against an increase of 9% of transactions on regulated platforms (Figure 8). Part of the OTC activity may have migrated to the regulated market, were the STT rate is lower.[22] The likely shift of transactions from the OTC to the regulated market is probably the main reason why we did not find evidence of a decrease in volumes on the regulated market.

Before concluding, let us remark some limits of our analysis. First,we cannot claim that our results can be extended to a tax design that differs significanlty from the one of the Italian STT.[23]

[21] OTC trading, which takes place bilaterally and off-exchange, represents about 30% of the overall trading volume of the Italian equity markets. The OTC market is both an interdealer market and a client-driven market for large investors. Dealers and investors choose to operate on the OTC market mainly because it allows customization and limits the price impact of large trades. It also allows higher intermediation margins for brokers.

[22] A less liquid OTC market translates into higher costs for market markers in regulated markets (which are not directly subject to the tax), because they rely on OTC transactions to manage inventories and reduce inventory-holding risks. This may be an additional channel through which the tax has widened bid-ask spreads.

[23] As recognized by Campbell and Froot (1994), the different national STTs present "*a bewildering variety of details in their nature,size and implementation. Transaction tax rates may vary with the type of financial instruments (equity being taxed typically at a higher rate than debt instruments or derivatives), with the*

Second, our findings are not sufficient to give a full-fledged welfare evaluation of the new tax. For this, we would need a detailed model of the economy, a fully-specified social welfare function and - even if this is often neglected - a full list of the alternative tax instruments available for the government. On the other hand, our assessment of the behavioral responses resulting from the tax is the necessary - even if not sufficient - base for any serious normative exercise, which is an important topic for further research.

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location of trade (on and off an exchange, at home or abroad) and with the identity of the buyer or seller (domestic or foreign resident, market maker or general trader)". These details, alongside differences in market structure, may have an impact on the outcomes.

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