Are Short-Selling Bans Effective?
Evidence from the Summer 2011 European Bans on Net Short Sales

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During the Summer of 2011, securities regulators in France, Italy and Spain reintroduced a ban on short sales. This ban differed from previous restrictions: it was the first time regulators prohibited net short sales on selected financial stocks, and extended the ban widely to include synthetic short positions. The nature of the ban allows me to employ a unique identification strategy that overcomes endogeneity problems discussed in past literature. Results indicate that the ban is associated with a significant deterioration in market quality – particularly for high free-float stocks – in contrast, I do not find any effect on stock price performance.

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«They wanted to test French resistance. This is our response – as always very determined – and it will be so for all those who want to put us to the test».
(Jean-Pierre Jouyet, head of the AMF, 11 August 2011, quoted from The Financial Times)

1. - Introduction

At the end of the summer 2011, following a drop in financial stocks’ prices, a number of euro-zone securities regulators called for a reintroduction of a ban on short sales. Financial authorities claimed that the ban aimed at restricting the benefits that can be achieved by spreading false rumours. The European Securities and Markets Authority (ESMA) backed national regulators defining short selling as an abusive market practice when used in combination with the dissemination of false or misleading information.

This is not the first time regulators have reacted to a decline in share prices by imposing bans on short sales. The previous experience relates to the 2008-2009 financial crisis, when regulators around the world hurried to impose constraints on this trading strategy, on the basis that short sellers contributed to the deviation of stock prices from their true valuation. According to the regulators, prices went down because short sellers abusively spread false information to make a profit; therefore banning this trading strategy would lead to an increase in share prices. Theoretical models however cast doubts on the effectiveness of short selling bans in supporting share prices, and, most importantly, suggest that short selling bans may have a negative impact on market liquidity. A regulation that impairs market quality is even more detrimental when it comes at a time of hardship and liquidity restraints for market participants, as it has recently been the case.

In this paper I henceforth investigate whether short-selling bans are the right regulatory answer; in particular, I study the impact of the European short selling ban of the summer 2011, to shed light on its impact on market liquidity as well as on its effectiveness on stock price performance.

Short-selling restrictions were first imposed only on selected financial stocks; afterwards, starting from the beginning of December of the same year, some countries extended a naked ban to all stocks from any industry; moreover the European Parliament voted into law a regulation to ban certain CDS trades starting from November 2012.1 This paper departs from the previous literature along several

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1 A net short position can also be achieved via Credit Default Swaps (CDS). For example, buying a naked CDS is equivalent to a bet on rising credit risk, since the investor is not exposed to the credit risk of the underlying bond issuer. From a regulatory perspective, this is hence analogous to a short sale of the underlying bond.
aspects: first of all, the characteristics of the 2011 European ban are remarkably different from short-selling restrictions previously imposed; it is indeed the first time regulators introduce a ban on net short sales on selected financial stocks only and extend the scope widely to take into account synthetic positions altogether. Studying the impact on the market of this different regulatory intervention is therefore crucial from a policymaking point of view to understand which type of regulation is the most effective. The second important contribution of this paper lies in the ability to address the identification problem. Indeed the existence of an endogeneity bias has been pointed out in the past literature, and has resulted in a main limitation in the interpretation of the estimated effect of the ban. In order to control for unobservable characteristics, some authors have matched each banned financial stock with one non-banned stock belonging to a non-financial sector (Boehmer, Jones, Zhang, 2008), while others have introduced stock-level fixed effects (Beber and Pagano, 2009). Although these specifications both try to capture some variability in the market that may indeed affect the estimated results, they still have several shortcomings. In fact, to have a causal interpretation of the estimated effect of the ban, these models make the rather stringent assumption that trends in market quality for stocks in the financial sector are comparable to trends in market quality for non-financial stocks, thus causing results to be driven by possible liquidity differentials between the two groups as well as differential time effects. Additionally, most studies on short-sale bans were conducted in the US amid of the 2008 crisis, concomitantly with TARP announcements; therefore any estimated effect of the ban on prices might have been clouded by rescue plan approvals. To address all these shortcomings I focus on the period from August 11 up to November 11, 2011 when the ban applied only to selected financial stocks. This is a particularly interesting time span because it allows me to specify the model under milder assumptions: I am able to construct counterfactuals from the financial sector and investigate whether there are significant differences in the time trends for the two groups, i.e. the banned financial stocks and the non-banned financial stocks, before the ban. The only assumption behind the causal interpretation of the ban effect in my model is the presence of common time effects across European financial stocks. The tests I conduct for the pre-ban period all give strong supportive evidence in favour of the goodness of the selected controls. Moreover, the single assumption I make is also mild, not only from an econometric point of view, but also from a financial perspective given the high degree of interconnectedness of stocks in the financial sector (see Acharya and Yorulmazer, 2007).
My results indicate that the ban on net short sales is associated with a statistically significant deterioration in market quality, i.e. an increase in percentage quoted bid-ask spreads. I also investigate whether the ban had heterogeneous impacts across different classes of stocks, specifically stocks with and without traded options, and stocks with low and high free float. In contrast with previous literature, I find that the ban did not affect disproportionately stocks without traded options; this is not surprising since the 2011 ban was extended also to synthetic net short positions. Stocks with high free float instead appear to have been affected more harshly by the ban compared to low free float stocks. This might be due to the fact that low free float stocks are mainly in the hands of controlling interest shareholders who are known for being reluctant (or sometimes even forbidden) to lend their shares; it follows that low free float stocks are already difficult for traders to locate and short, even in the absence of a ban. Instead high free float stocks are the ones for which short positions are easier to enter into, and are therefore the group that is more likely to have been hit by the ban. Finally, I inspect the impact of the ban on prices and find that the ban is not associated with a statistically significant improvement in price performance.

2. - Literature Review

Theory is ambiguous over the effects of short sale bans on market liquidity. Diamond and Verrecchia (1987) show that the impossibility to trade on negative news reduces the speed of price discovery, which in turn creates uncertainty about the fundamental value of the stocks and hence tends to increase the bid-ask spread. However substantial empirical evidence (Desai, Krishnamurthy, and Venkataraman, 2006; Cohen, Diether and Malloy, 2007; Boehmer, Jones and Zhang, 2008) shows that short sellers tend to trade on information about fundamentals; theory would hence predict that, by removing a consistent share of informed traders, the ban would reduce the bid-ask spread.

Another channel of transmission would then be the competition among liquidity providers. Khandani and Lo (2007) provide evidence that many quant funds do provide liquidity even if they are not registered market makers. Hence, even if an exemption from the short-sale ban is in place for market makers during the summer 2011, the ban would reduce competition by limiting de facto liquidity suppliers. As a consequence, exempted market makers would have the incentive to widen their spreads.
Theory is ambiguous as well over the impact of short sale restrictions on stock prices. Miller (1977) predicts that short sale constraints would lead to overpricing by taking pessimists out of the market. Diamond and Verrecchia (1987) however incorporate rational expectations and show that when investors are aware of the impact of the ban on the composition of the pool of investors, stocks are not overvalued on average. Yet, Marin and Olivier (2008) develop an extension of the Grossman and Stiglitz (1980) model, and show that when insiders face some realistic trading constraints (such as not being able to short-sell shares of their companies), asset prices exhibit crashes. The authors label this the “dog that did not bark” effect: uncertainty about the stock fundamentals is larger when insiders (who possess valuable information) are out of the market than when they are trading—indeed, uninformed traders see that insiders cannot short sell stocks but they also see that insiders are not buying back the stock either, therefore uninformed investors can infer that there is bad news but not how bad the news really is; it follows that such uncertainty along with a downward revision of beliefs about the stock fundamentals causes prices to go down even more rapidly than if there had been moderate selling by insiders.

Recent literature has focused on price manipulation; in this respect, Goldstein and Guembel (2007) introduce feedback effects from financial markets onto the real economy and show that speculators have the incentive to short sell and hence manipulate prices, even when uninformed. Shkilko, Van Ness and Van Ness (2011) document that short sales may increase downward pressure on prices even in the absence of negative information: they examine large negative intraday price reversals on no-news days and find that short sellers during these reversals are abnormally active and cause substantial price declines.

Empirical evidence was mainly gathered during the 2008-2009 short-selling restrictions. Boehmer, Jones and Zhang (2009) use a matched pairs panel regression to analyse the US ban and find that stocks subject to the ban experienced a severe degradation in market quality (larger spreads, higher price impacts, and increased intraday volatility). Beber and Pagano (2009) exploit cross-country variation in short-selling restrictions and study the effects of the bans on liquidity, price discovery and stock prices; they find that the ban imposed a serious disruption in the liquidity of the banned stocks—particularly the small-cap ones. They also document that information was incorporated into prices more slowly for banned stocks. Finally they provide evidence that the ban failed to support prices, with the exception of the US. It is important to note, however, that both Boehmer et al. (2009) and Beber et al. (2009) acknowledge in their papers that confounding
effects may be driving the estimated results: in fact, the 2008 short-selling ban in
the US was concomitant with TARP announcements, therefore the positive effect
estimated on stock prices in the US might in fact be due to rescue plan approvals,
and not to short-selling bans. It is also worth noting that the 2008 short-selling
ban covered virtually all stocks in all major sectors; therefore, in order to identify
the effect of the ban researchers had to either look for counterfactuals within non-
financial industries or to resort to fixed-effect model estimation. The former spec-
ification implicitly assumes that market-wide variability in the financial sector is
similar to, and can be captured by, the variability appreciated in other sectors.
However, it may be quite stringent to assume that trends in financial stocks can
be controlled for using counterfactuals belonging to very different industries, such
as retail and manufacturing. Therefore, all of this left researchers with a possibly
spurious effect estimated on the ban.

Additional empirical evidence was gathered by Battalio and Schultz (2009),
who study the impact of the 2008-2009 short selling restrictions on the equity
options markets and show that the ban is associated with dramatic increases in
bid-ask spreads for options of banned stocks. Kolasinksi, Reed and Thornock
(2010a,b) report that the negative relation between the percentage volume of
short sales and stock returns became more marked during the 2008 US ban, sug-
gest that banned stocks became more responsive to allowed or synthetic short sales.
Marsh and Payne (2011) restrict their attention to the UK and find similar ad-
verse effects of the ban on market efficiency. Diether, Werner and Lee (2009)
instead study the temporary suspension of the 2004 US Regulation SHO and
show that the suspension of price tests in the US pilot securities worsened spreads
and intraday volatility. Saffi and Sigurdsson (2011) show that short selling con-
straints (i.e. low lending supply and high borrowing fees) are negatively related
to stock price efficiency, and that less constrained firms experience shorter price
discovery delays.

In sum, the consequences of short-selling bans are ambiguous from a theoret-
cal perspective; moreover existing empirical studies on the impact of the bans
on stock prices performance reach conflicting conclusions and suffer from con-
 founding factors. It is therefore important to gather more evidence on short-sell-
ing restrictions in order to identify the causal effect of the ban on market quality,
and I do it in the following sections.
3. - The Setting

3.1 Notes on the Ban

As shown in Table 1, the ban was introduced on the same day – 12\textsuperscript{th} August 2011 – in France, Italy and Spain, with the French markets authority leading the calls and urging a coordinated pan-European response\textsuperscript{2}. The ban was initially intended to last 15 days only, but it was then rolled over to September 30, and once again, up to November 11. It affected only selected stocks in the financial sector, regardless of the trading venue where transactions are executed (MTFs and OTC were henceforth included in the calculation of net short positions). Differently from previous bans, the 2011 short selling restrictions covered also ETFs, covered warrants and certificates, along with synthetic short positions entered through, for instance, the purchase of put options or reverse-ETFs strategies.

In all three countries, reporting obligations were in place since before the introduction of the ban for any player taking short positions in all stocks traded in regulated markets. More specifically, the CNMV, the Spanish regulator, adopted disclosure requirements starting from June 2010 in accordance with ESMA-proposed guidelines; the AMF, the French markets authority, introduced new requirements on reporting starting from February 2011; in Italy reporting obligations have been in place since July 2011 only.\textsuperscript{3}

\textsuperscript{2} To isolate the effect of this ban, I do not include in my analysis countries where short-sale restrictions have been in place since 2008 and never lifted, such as Germany.

\textsuperscript{3} Moreover, while the CNMV and the AMF explicitly provide for the publication of aggregate short sales positions filed by market participants, under the Italian regulation the Consob did not disclose information regarding bearish positions taken on Italian shares. The different periods of adoption of disclosure requirements in the three countries, along with the impossibility to retrieve evidence for short sales for the Italian shares, make a possible visual inspection of the evolution of short sale positions before and during the ban, not much accurate to derive meaningful insights. Moreover, even in the presence of complete data, information would be subject to an overestimation bias: indeed, under the disclosure requirements, it is mandatory to report short positions taken irrespective of the trading venue; short positions taken in OTC markets are hence included in aggregate short volumes; however understanding the evolution of short positions as a percentage of total volume, would require detailed information also on the volume of OTC transactions, for which there is not a regulatory framework. Data Explorers is a data provider that allows the monitoring of short positions alongside the market data; it recently published a note, reported in the Financial Times, showing the level of shorting in the three European countries. The level of short sales reported, appeared already extremely low before the ban was introduced. It would be in fact useful (and I plan to do for future research) to request access to the tick-by-tick database to compute and check on the levels of shorting pre and during the ban, in line with MARIN J. and OLIVIER J. (2008). At the moment, the database is not available free of charge for students at my University.
It is worth noting also, that these European bans did not prohibit short sales *tout court*; what is prohibited under the ban is taking net short positions. For the sake of clarity, I report an example of permitted strategy as quoted by the Consob: an investor who has a long position on a banned stock through a call with cash settlement, is allowed to short a delta-equivalent number of shares, so that the short position is perfectly counterbalanced by the long position on the traded option. It is also worth noting that there is a difference between this ban and the 2008 naked ban; for instance, taking the above example, a cash settlement would not be enough under a naked ban because it does not entitle to, in legal terms, “the right to receive”, *i.e.* to the underlying.

The introduction of the ban was followed by a harsh debate, with the UK regulator, the FSA, taking a firm stance against the ban.

### 3.2 Short Selling- How it Works

Short selling involves the sale of an asset that is not owned. Typically, a short-seller would need to turn to a broker to *locate* the stock. The broker might already have the stock in its own inventory or in the accounts of those clients who allow the broker to access their accounts for lending. If the broker is not able to locate the stock within his accounts, he will need to refer to a custodian bank or institutional investors with buy-and-hold strategies, such as insurance companies and pension funds. It may take considerable time for the broker to locate the stock due to several features of the stock in question, among which the float, the ownership concentration, the presence of certain events such as IPOs and mergers. Once the broker has located the stock, the short seller can short the stock and at the same time must place collateral with the original lender in excess of the market value of the borrowed stock; this collateral is usually placed in cash and is higher in case the short seller located the stock through a broker-dealer. The borrower pays the lender a fee that is *rebated* by the interest that the lender pays to the borrower for the use of the cash posted as collateral. This rate is negotiable and is adjusted on a daily basis; as long as this rate is below the market rate for cash funds, the lender is gaining a cheaper access to sources of funding. In addition, most lenders retain the right to interrupt the loan of the stock at any moment: the lender gives a notice of recall to the short seller, who then has three days to *cover* his position by purchasing the

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stock or by borrowing it from another lender. Failing to do so, a short squeeze occurs and the lender has the right to buy in the stock with the cash collateral. The short seller gains if the price of the stock has declined at the moment he has to close his position. Before disclosure requirements came into place, from a buyer perspective a short sale would be equivalent to a sale, since it was undistinguishable whether the counterparty to the trade already had the stock in its inventory. It is worth mentioning that the term going short refers also to the broad set of activities that come into existence through the derivatives market: for instance a put is a synthetic short position in so far as it entitles the buyer to sell the stock at an agreed strike price, which is equivalent to a bet on declining stock’s price; analogously, being short in futures implies that the trader has the obligation to sell the stock at a later date at a given price, therefore the trader will gain if the price falls below the agreed sale price, since he will be able to buy the stock at a lower price and gain from the difference. As previously mentioned, the 2011 short selling ban covered also these synthetic short positions.

3.3 Short Selling: The Rationale Behind Looking at Impact on Liquidity

Liquid markets are desirable because they are perceived as providing efficient allocation of resources and increased information. There is no one single definition of liquidity, although market participants tend to view as liquid those stocks where a position can easily be unwound. In particular, from market microstructure literature, we can think of liquidity as the cost of reversing an asset trade almost instantaneously after having executed it. In this light, liquidity is naturally defined via bid-ask spreads. The bid-ask spread is the difference between the price at which liquidity suppliers (call them the dealers) are willing to sell (ask price) and the price at which they are willing to buy (bid price). The main determinants of the spread are adverse selection costs arising with asymmetric information and inventory costs. The highest the spread, the costlier it is to reverse the trade, hence the lowest the liquidity.

Liquidity in the markets is tied in a mutual reinforcing behaviour with another type of liquidity, namely funding liquidity defined as the ability of an investor to raise funds either through collateralised loans or thanks to his own capital. Trading when a short position is taken does not free up capital, instead a margin is usually needed to take positions, as mentioned in the previous paragraph. It follows that when funding liquidity is low, trading activity is reduced; decreased trading in turn lowers market liquidity and broadens spreads, increasing losses on existing positions; market liquidity restraints in turn put pressure on risk man-
agement as firms try to minimize their exposure, and consequently reduce the ability of firms to raise capital, hence creating a downwards liquidity spiral\(^6\).

Given these premises, it is important for a financial market regulator not to impose policy interventions that, by broadening bid-ask spreads, amplify the liquidity spiral and that hence make the firms’ quest for capital harsher.

4. - Data and Methodology

My data consist of daily bid and ask prices, market capitalization, volumes, float, leverage, volatility and ban-specific characteristics (starting and lifting date, stocks under shorting constraints, restrictiveness of the ban), for 256 financial stocks for a window of 165 days from May 31\(^{st}\) up to November 11\(^{th}\). Daily data on bid and ask prices are retrieved from Bloomberg; details on the nature of the ban as well as on dates of inception and lift are taken from the website of Spain, France and Italy regulatory authorities, as well as from the European Securities and Market Authority (former CESR). For a stock to be included in my sample, I require it to have positive bid-ask spread over the sample period (223 stocks). Finally, to mitigate the effect of extreme observations, I winsorize the bid-ask spread at the top 1% level for the whole sample. Affected stocks represent 21.5% of the total number of financial stocks traded in the three European countries.

As mentioned before, during this time window the ban affected only selected financial stocks. Therefore, I can draw counterfactuals from the same industry of the stocks subject to short-selling restrictions. The possibility to have this type of controls allows me to rule out any bias due to liquidity differential between financial and non-financial stocks, which may have otherwise driven the results if I used non-financial stocks as controls (as it was done in previous studies, such as Boehmer, Jones and Zhang, 2009; Beber and Pagano, 2009; Marsh and Payne, 2011).

All regressions include a ban dummy that takes value 1 when the stock is banned and 0 otherwise. The choice of the sample period is justified by the need to balance the importance to have enough variability with the trade-off of increased heterogeneity. Hence, in line with existing literature, I consider a pre- and a during-ban time windows that are similar in size, so that my estimated effects are the less affected by confounding factors, at the clear cost of foregoing some information.

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\(^6\) See GARLEANU N. and PEDERSEN L.H. (2007) for an in-depth analysis of the interaction between risk management and market liquidity.
5. - Market Liquidity

5.1 Descriptive Evidence

I consider the percentage quoted bid-ask spreads as a measure of (il)liquidity. Graph 1 shows the evolution of equally weighted average quoted bid-ask spreads for all 223 financial stocks in the three countries considered; the dashed lines correspond to the introduction of the bans and their extensions. This Graph confirms the period of decreasing liquidity experienced in Europe. The evolution of spreads following the ban seems to suggest that restrictions have had a very limited impact on the liquidity of the stocks, whose spread rises back shortly after the (re-)introduction of the ban. On this note, Graph 2 represents quoted bid-ask spreads separately for the banned and the unbanned financial stocks, so to visually identify the effect of the ban. The behaviour of the two groups is initially very similar, showing common trends in liquidity for banned and unbanned financial stocks alike. After August 12 2011, however, banned stocks experience an upward trend in bid-ask spreads and the paths of the two groups diverge, suggesting the ban contributed to the deterioration in market quality for the stocks subjects to restrictions. More precisely, post-ban, the spread of the banned stocks is trending upward (they become less liquid) while the spread of the unbanned assets is stable, as Graph 2 shows. Since the spread of the banned stock is lower in level, this implies a squeezing gap, or in other words a small differential: the spread of the banned stocks is “catching up” with the spread of the unaffected stocks, so that the gap between the two is smaller than it used to be. But this (a no-larger gap) exactly means that the liquidity of the banned stocks has been disrupted compared to the unbanned ones.

5.2 Regression Analysis

5.2.1 Panel Regressions DD

To identify the effect of the ban, it is important to answer to the question of how would the banned stocks have moved had they not been banned. In order to do so, one must identify a group of stocks that would mimic the behaviour of the banned stocks in the absence of the treatment.

In fact a stock-level fixed-effect regression that does not take into account unobserved market variability, particularly at times of turmoil, risks of producing biased results, in so far as estimates may be clouded by omitted correlated variables that vary at the market level. To cloud out the source of omitted variable bias, the key is to find a group of stocks that appear to have reacted to market variability
in the same way as banned stocks have in the past; in other words, identification relies on having two groups of stocks (banned and unbanned) whose trends in liquidity have appeared to be consistently similar, up until the start of the treatment. The introduction of the ban indeed creates a deviation from this common trend. The impact of the ban is then given by the behaviour of banned stocks compared to how they would have behaved had the ban not been introduced – which is captured by the behaviour of counterfactuals. It follows that, differentiating across the two cohorts of stocks, it is possible to disentangle the impact of the ban from unobserved market variability: the latter is in fact captured by the change in liquidity of the control stocks, which is then subtracted from the change in liquidity of the banned stocks.

As explained in Section 5.1, Graph 2 shows that the financial stocks from the three countries of interest that have not been subject to the ban, appear to have behaved in the same way banned stocks have for the period before the ban, their trends in liquidity being consistently similar. It follows that these stocks are good candidates for the group of counterfactuals in the model.

As mentioned, what matters for identification are common trends, not common levels of liquidity. In fact banned and unbanned stocks can differ in levels of bid-ask spreads, and the difference is captured by the presence of stock-level fixed effects, i.e. individual determinants of liquidity. In formulas:

\[
E [ S_{ict} | c, t ] = \gamma_{ic} + \lambda_t
\]

This equation says that in the absence of the ban, the bid-ask spread of stock \( i \) is determined by the sum of constant stock-specific effects, \( \gamma_{ic} \), with “\( c \)” indicating the cohort (banned vs. unbanned)- which allows the levels for the two cohorts to differ, and a time effect, \( \lambda_t \), that is common across the two cohorts of stocks, capturing the market variability. Calling \( D_{ct} \), the dummy variable indicating the presence of the short-selling ban, I can therefore write in regression form:

\[
S_{ict} = \gamma_{ic} + \lambda_t + \delta D_{ct} + \varepsilon_{ict}
\]

More in detail, the variable \( \gamma_{ic} \) captures determinants of stocks liquidity which can be considered time-invariant in the event window under analysis, namely analyst coverage, number of market makers, but also the hard-to-measure regulation and enforcement of insider trading. The time effect, \( \lambda_t \), captures the unobserved
market variability that would bias the estimate on the ban if omitted. To identify \( \delta \), i.e. the impact of the ban, I carry out a difference in difference analysis, where the first difference is a time difference and the second one is carried across the two cohorts of stocks. To illustrate: calling \( t=0 \) the time before the ban and \( t=1 \) the time after the ban, I write

\[
(a) \quad E \left[ S_{oict} \mid c = \text{banned}, t = 1 \right] - E \left[ S_{oict} \mid c = \text{banned}, t = 0 \right] = \\
= (\gamma_{iban} + \lambda_1 + \delta) - (\gamma_{iban} + \lambda_0) = (\lambda_1 - \lambda_0) + \delta
\]

\[
(b) \quad E \left[ S_{oict} \mid c = \text{not banned}, t = 1 \right] - E \left[ S_{oict} \mid c = \text{not banned}, t = 0 \right] = \\
= (\gamma_{inoban} + \lambda_1) - (\gamma_{inoban} + \lambda_0) = \lambda_1 - \lambda_0
\]

which shows the changes in liquidity for the two cohorts of stocks separately. As the equations show, \( \gamma_c \) has disappeared in time difference: hence time-invariant unobserved confounders do not bias estimates\(^7\).

However, as \((a)\) shows, the impact of the ban is still clouded by the presence of the unobserved time trend \( \lambda_t \). Differencing across cohorts (treated and controls) allows me to do just that, namely to filter out the day effect, \( \lambda_t \) from the estimated effect on the ban, \( \delta \), on the mild assumption that this time effect is common across the two groups. I write:

\[
(c) \quad \left( E \left[ S_{oict} \mid c = \text{banned}, t = 1 \right] - E \left[ S_{oict} \mid c = \text{banned}, t = 0 \right] \right) - \\
\left( E \left[ S_{oict} \mid c = \text{not banned}, t = 1 \right] - E \left[ S_{oict} \mid c = \text{not banned}, t = 0 \right] \right) = \\
= (\lambda_1 - \lambda_0 + \delta) - (\lambda_1 - \lambda_0) = \delta
\]

where \( \delta \) is the causal effect of interest. To obtain consistent standard errors on the estimated \( \delta \) I follow the methodology proposed by Bertrand, Duflo and

\(^7\) Note that constant observable variables – as for instant constant free float – do not cloud the estimated impact of the ban, \( \delta \) – I would just explicitly include them in the model. A bias exists if there are hard-to-measure or unobservable variables that may impact stocks liquidity (think for instance of the hard-to-measure “enforcement of insider trading regulation”). Although not accurately measurable, these variables cannot be omitted (see ANGRIST J.D. and PISCHKE J.S., 2009 for an extensive treatment of the issue). However, if in the time span under inspection, such variables can be confidently considered as constant, then the fixed-effects specification allows to get rid of these unobserved confounders.
Mullainathan (2004): the authors have indeed shown that conventional difference-in-difference standard errors obtained with an OLS on a panel dataset largely underestimate the standard deviation of the estimated treatment effect, thus over-estimating $t$-statistics and significance levels. AR(1) corrections -which have been used in past short selling ban studies- have been shown to fare poorly too. In my study I therefore follow the solution proposed by the authors: namely I apply a correction that collapses the time series information into a pre and post period, which has been shown to produce the correct level of significance for studies with numbers of treated individuals (stocks, here) $N > 50$ and with individuals receiving treatment at exactly the same time, as it is the case here.

Specification (2) could be improved adding additional controls, namely additional time-varying variables that may affect stocks’ bid-ask spreads. To do so, I build on theoretical models of the determinants of bid-ask spread and I include in the model specification the matrix of cohort-and-time-varying covariates $X_{ict}$ that captures the stocks market capitalization and price. Therefore:

$$S_{ict} = \gamma_{it} + \lambda_t + \delta D_{ct} + X'_{ict} \beta + \varepsilon_{ict}$$

It follows that the only assumption on which this model rests, is common $\lambda_t$ across the two cohorts of banned and unbanned financial stocks. Differently from previous literature, the assumption in my model is not stringent: indeed, while it is debatable whether financial and non-financial stocks do react similarly to shocks in the market, it is instead highly likely that very interconnected financial stocks comove.

In fact, the interconnectedness among financial stocks and its implications have been discussed at length in the literature. Starting from Chordia, Roll and Subrahmanyam (1999); Forbes and Rigobon (2002); Croux, Forni and Reichlin (2001); Campbell, Koedijk and Kofman (2002); up to Kritzman, Li, Page, Rigobon (2010) and Billio, Getmansky, Lo, Pellizzon (2010), this literature agrees

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9 Theory identifies trading activity as a determinant of stocks liquidity. Yet, although it might be argued that trading activity, captured by volumes of shares traded- should be included as a regressors, if there is reason to think that there exists a causal relationship of the ban on volume, then inserting the latter as controls would be econometrically incorrect and it would bias the estimates on the ban dummy (See ANGRIST J.D. and PISCHKE J.S., 2009) for an extensive treatment of the bad control problem). Results in section 7 show that the impact of the ban on prices is not significant so I include it in my matrix of cohort and time varying covariates.
on findings of high level of financial markets co-movement during both crises and stable periods. In particular Bekaert, Hodrick, Zhang (2009) finds that, in examining time trends, there has been a significant increase in stock return correlations within Europe, giving strong support to my assumption.

Besides relying on past literature, to check the robustness of my assumption I also carry out a parametric and a non-parametric test. First I run a falsification test: for the period before the introduction of the ban, I estimate my regression DD adding cohort specific time trends to the regressors of the model. I write:

\[ S_{ict} = \gamma_{0c} + \gamma_{1ct} \sigma + \lambda_t + \epsilon_{ict} \]

where \( \gamma_{0c} \) is the cohort-specific intercept and \( \gamma_{1ct} \) is the cohort-specific trend coefficient that multiplies the time-trend variable \( t \). In other words, \( \gamma_{1ct} \) is an interaction variable: it interacts the day dummy with the cohort dummy.\(^{10}\) Adding this variable allows banned and unbanned control stocks to follow different trends in the time span considered. Checking the significance of the coefficient \( \sigma \) estimated on the interaction variable therefore means to check whether, in the pre-ban period, banned and unbanned stocks have experienced statistical significantly different trends in liquidity. As reported in Table 2, I find that no estimated coefficient is significant; this means that the spreads of the unbanned stocks that I use as controls appear to have reacted to events in the market in a similar way the spreads of banned stocks have, for the period before the ban was introduced. This result provides strong suggesting evidence in favour of the goodness of the control groups and their ability to mimic the behaviour of the banned stocks in absence of treatment.

I also run a second test: I build on the work of Kritzman, Li, Page, Rigobon (2010) and Billio, Getmansky, Lo, Pellizzon (2010), and use the non-parametric method of Principal Components Analysis to estimate the extent of co-movements due to common factors. For the pre-ban period, I derive the matrix of eigenvectors of the matrix of the stocks returns, and the vector of eigenvalues in descending order. In particular, I extract the principal components from the matrix of banned stocks’ returns; and I do the same for the matrix of unbanned stocks returns. The first principal component (PC) captures the direction of maximum variance, or in other words it is the common factor on which all the variables load. If the first principal component of the banned stocks and the first PC of the unbanned stocks are strongly correlated, then this is an indicator that the

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\(^{10}\) The cohort dummy takes value 1 if the stock will be banned and 0 otherwise.
two groups of stocks co-move, as the common factor that affects all stocks is the same – in a statistical sense – for both groups. I find that the first principal component of the banned stocks and the first PC of the unbanned stocks almost entirely overlap with their correlation being as high as over 0.74. This test gives additional supportive evidence to the assumption in my paper.

Table IIIA report the results of my estimations. Column 1 reports results estimated under specification (2), while column 2 reports estimates under specification (3). Column 1 shows that the ban is associated with an increase of 0.59 percentage points in the quoted bid-ask spread. This result is significant at 1% significance level. This effect is quite large if one considers that the mean spread in the sample is 2.27 and the median spread is 1.76. Column (2) reports a smaller but still positive coefficient estimated at the 10% significance level, confirming a significant increase in bid-ask spreads.

This result is particularly important for two reasons: first it is estimated under very mild identification assumptions when compared to previous literature, therefore it is less likely to be driven by confounding factors. Secondly, this result is economically reasonable if one compares it to the much harsher impact on liquidity estimated during the 2008-2009 experience, when the ban was associated with an increase in the bid-ask spread of financial firms of up to 2.75 percentage points: the 2011 European bans did not prohibit short sales *tout court*, what was prohibited under the ban was taking net short positions, as explained in section 3. This means that trades in this direction were not banned altogether; instead a long balancing position was required, even when in cash-settlement. Hence, the 2008 stricter ban disrupted liquidity more than the 2011 looser ban.

5.2.2 Differential Liquidity Effects

After establishing the average treatment effect on banned stocks’ liquidity, I now assess whether the ban had differential liquidity effects by considering: *i)* the presence of options; *ii)* free float.

*i. Traded Options*

First of all, I inquire whether the presence of an option market has allowed traders to synthetically express bearish positions on the underlying stock. If this is the case, stocks with traded options might have suffered less from the imposed restrictions than stocks without options. This question is particularly interesting from a policy perspective, since this ban differed from the previous ones in so far as restrictions were imposed also on any shorting position expressed through the
derivatives market; in other words, a short position synthetically reproduced by, say, buying a put on the banned stock, would be included in the counting of the trader’s net short positions. Analysing differential liquidity effects for the stocks with traded options would henceforth provide a check on the effectiveness of the ban and its enforceability.

In order to cast light on this issue, I classify stocks into two groups: those that have traded options and those that don’t. To do so, I cross information from Bloomberg and the national exchanges, so to be able to identify the two groups. I expect that no significant differences should arise between the two groups, in the scenario of the correct enforcement of the ban. To assess differential liquidity effects, I run two separate regressions (namely: banned and controls with options; banned and controls without options) and then test whether the difference in the estimated coefficient is significantly different from zero. To obtain a correct estimate, since I cannot compare two separate regressions estimated on independent distributions, I run a seemingly-unrelated estimation that corrects for variance and covariance, producing reliable significance of the test. Results are reported in Table 4. As expected, the difference in liquidity effects for stocks with traded options and stocks without is not significant, so we cannot reject the null hypothesis that the ban was able to prevent traders from entering into net short positions through the use of the derivatives market, as instead had been the case for the 2008 bans.

ii. Free Float

As explained in Section 3.2, restrictions to shorting activity exist also in the absence of legal restrictions or bans due mainly to the nature of the market, to the possibility for the lender to recall the stock and to the presence of strategic investors. While the decentralized nature of the market and the right to recall do affect all stocks alike, the presence of strategic investors in certain groups of assets creates differential conditions for those stocks. Indeed, there may be stocks held largely by retail investors or investors with a direct stake in the firm, and hence harder to borrow and trade even in the absence of a ban. In this section, I focus on this class of stocks to inspect whether the short-selling ban has disrupted their liquidity less than it did for easy-to-trade stocks. To shed light on this possible effect, I inspect the presence of a differential liquidity effect for stocks with above vs. below mean free float\(^\text{11}\). The free float indicates the percentage of shares that

\(^{11}\) Free float is calculated by subtracting the shares held by insiders and those deemed to be stagnant shareholders from the shares outstanding. Stagnant holders include ESOP’s, ESOT’s QUEST’s, employee benefit trusts, corporations not actively managing money, venture capital companies and shares held by Governments.
investors can freely negotiate in secondary market, as they don’t belong to controlling interest investors: the higher the float the easiest it is to locate and trade the stock.\textsuperscript{12} It follows that, since the ban significantly widened banned stocks’ spreads, I expect the liquidity of high free float banned stocks to have been disrupted even more than the liquidity of banned stocks that are mainly in the hands of strategic investors.

Analogously to the analysis conducted for traded options, I run a seemingly-unrelated estimation that corrects for variance and covariance, producing reliable significance of the test. Results are reported in Table 5 and show that the difference between the two coefficients is significantly different from zero. In particular, the ban appears to have dramatically disrupted the liquidity of banned stocks with high free float, while its impact appears non significant on low-free float stocks. This might be due to the fact that low free float stocks are mainly in the hands of controlling interest shareholders who are known for being reluctant (or sometimes even forbidden) to lend their shares; it follows that low free float stocks are already difficult for traders to locate and short, even in the absence of a ban. This is instead not the case for high free float stocks for which short positions are easy and frequent to enter into, and are therefore the group that is more likely to have been hit by the ban.

5.3 Matching Estimation

In Table 3A Column (3) I further check the robustness of my estimation by performing a more refined diff-in-diff where I show that for each banned stock I can find an unaffected stock that is similar along all important dimensions- sensitivity to manipulation, size, leverage, volatility, price- to the banned one. The rationale is as follows.

\textsuperscript{12} To allow for comparability across stocks and in time, it is common to use proxy of lending supply in percentage terms (see SAFFI P. and SIGURDSON H., 2011). In a time series analysis in fact using absolute free float would be a biased measure- over time the free float might increase in absolute terms; however the effective lending base might be larger or smaller according to whether the free float is growing respectively at higher or lower rate than the total capitalization. Also, consider that a purely speculative trader needs to be able to push the price in his favour (down) to avoid losses. This is very hard to achieve when, in percent terms, a big slice of the stock equity is in the hands of strategic investors. Therefore, even when stock A has higher absolute float than stock B, if stock A has lower percentage float, then stock A is harder to manipulate: the pool of optimistic non-lending investors is in relative terms much bigger, and can counteract any speculative attempt. The speculative short seller will therefore not target a low-percentage free float stock.
Unless the regulator wanted to discriminate between stocks, thus introducing a layer of unfair competition by protecting only *selected* assets, it could be that the regulator deemed the banned stocks to be an easy target for abusive short sellers, while it must have deemed the stocks not under ban to be more difficult to manipulate, and thus not in need of a regulatory protection. The first thing to check is therefore whether banned and unbanned stocks show different degree of shorting sensitivity.

I therefore rely on the literature (D’Avolio, 2002 and Asquith, Pathak, Ritter, 2005 among others) and use free float as a measure of sensitivity. For a stock to have a low free float, it means that the majority of the shares are in the hands of non-lending optimistic investors or investors with vested interests; it is therefore very hard for an abusive short seller to start a downward spiral in the price of those stocks. High free float is thereby capturing ease of uninformed shorting.

Beside sensitivity to manipulation, I also rely on the asset pricing literature and consider stock specific characteristics that could have led to a differential treatment with respect to the ban. In particular I focus on size, leverage, volatility and price.

I then rely on the matching methodology proposed in Wurgler and Zhuravskaya (2002): for each banned stock I create a portfolio of substitutes that constitute my new group of controls; the portfolio is made of three unbanned stocks from the financial sector that are closest to the banned stock in terms of the above specified characteristics. More specifically, for each banned stock I select the three unbanned stocks within the financial sector that minimize the sum of the squared percentage difference of size, stock price, leverage, volatility, and float. The matching procedure is done with replacement so that a control stock can be used as match for more than one treated stock. Being the countries in my sample all European and subject to harmonized trading regulation, I choose not to condition the matching on the country of the firm incorporation, on the basis that an investor can easily substitute, say, a banned Italian stock for a French non-banned one. Data used for the matching procedure are computed before the event window under scrutiny, so as at May 2011.

Conditional on all these information, there is no reason why a stock should receive a short sale ban or not. In other words, after controlling for size, leverage, volatility, price and sensitivity to manipulation, it is really hard to argue that there is a source of omitted heterogeneity driving selection; the two groups, banned and controls, are perfectly comparable. I run my regression on this matched subsample and report results in Table 3A Column (3). The regression results are vir-
tually unchanged, and they show that the ban has disrupted liquidity of banned stocks at the 5% significance level.

6. - Notes on Endogeneity

As mentioned earlier, looking back at the previous literature, the possible existence of an endogeneity bias has resulted in a main limitation in the interpretation of results. Indeed, previous papers have tried to capture market-wide variability using two-way fixed effects or selecting control groups from non-financial industries, implicitly assuming that market-wide variability in the financial sector is similar to, and can be captured by, the variability appreciated in other sectors. However, it is hard to think that trends in the financial stocks can be controlled for using counterfactuals belonging to very different industries, such as retail and manufacturing; this left researchers with a possibly spurious effect estimated on the ban. Given these premises, this paper makes a substantial contribution to the identification strategy. Indeed, the only assumption made here is common time trends, $\lambda_t$, across the two cohorts of financial stocks.

It is worth noting that this loose identification assumption is supported by both theoretical and empirical arguments: Chordia, Roll and Subrahmanyam (2000) first inspected common trends in liquidity across assets, and found that, quoted spreads tend to co-move with their respective industry-wide liquidity, even after accounting for individual determinants of liquidity. More recently Brunnermeier and Pedersen (2009) have shown that commonality in liquidity across securities can be explained in the light of funding restraints for speculators: more precisely, they start by considering the fact that trading requires existing capital in the form of a margin, and then show that when an exogenous shock imposes funding constraints on speculators, such traders become reluctant to take position in capital-intensive stocks thus reducing liquidity at the market-wide level. The authors also show that this market-wide liquidity trap effect is harsher during a crisis, when capital available is already diminished and the risk of hitting margin constraints is higher. Moreover, recent research on contagion and systemic risk has cast light on the high level of interconnectedness of players in the financial sector: Acharya and Yorulmazer (2008) have shown that banks have an incentive to herd and undertake correlated investments because this increases the portion of risk that is systemic and hence maximize the chances of them being rescued at times of turbulence; on this note, Brown and Dinc (2011) show that government
running large budget deficit are reluctant to let a failing bank close when the financial sector is weak, thus giving even more credit to the idea that *ex-post* regulation might be time-inconsistent and might indeed foster herding behaviour in the financial arena.

Considering the results by these authors together, it follows that previous research does give reason to believe that very interconnected financial stocks do behave similarly in terms of trends in market liquidity, so that it can be confidently assumed that the $\lambda_t$ is common across the two cohorts of banned and unbanned stocks.

This argument is even more sound if one considers the increased degree of connectedness among different players in the financial sector coming from the diversification of core services: in the past 10 years, financial innovation has blurred the distinctions between business types, with financial players moving aggressively into previously non-core activities, and therefore increasing their linkages\textsuperscript{13}.

Finally, as explained in detail in Section 5.2.1, I have also conducted two tests, a parametric and a non-parametric one, and both tests have given strong suggestive evidence in favour of the goodness of my assumption.

7. - Stock Prices

Short sellers have frequently been pointed out as being the cause to the stocks’ sharp decline in prices. As it is possible to read from the coordinated statement release\textsuperscript{14}, the expectations of the regulators were to stop, though the ban, any abusive short selling from driving down the price of financial instruments to a distorted level. The European 2011 ban was in fact accompanied by claims from the regulatory authorities that such restrictions would prevent any speculator to gain from spreading false or misleading information.

The theory on short selling has been ambiguous on this matter: on the one side, this trading strategy has been seen as contributing to efficient pricing, by uncovering distressed companies; yet on the other side, it has recently been questioned for its ability to price manipulate and negatively impact firms even when trading in the absence of information.\textsuperscript{15}

\textsuperscript{13} See for instance the GENEVA ASSOCIATION SYSTEMIC RISK WORKING GROUP REPORT (2010).
\textsuperscript{14} CESR/10-298
The predicted impact on prices of a short selling ban is hence not clear. European regulators seem to have embraced the view expressed by Brunnermeier and Oehmke (2008), according to which the ban would prevent predatory shorting and hence the liquidation of financial institutions that consequently end up falling short of their capital requirements. If stocks are under-priced because of a group of bearish traders acting on false information, the regulators’ reasoning goes that by preventing that part of traders from spreading misleading information, stocks prices would then be able to go back up to their true value.\footnote{However, if the market believes that the short sellers are indeed informed and it has rational expectations as in DIAMOND D.V. and VERRECCHIA R.E. (1987), this prediction may not be correct: investors are aware of the impact of the ban on the composition of the pool of investors, so stocks are not overvalued on average.}

Evidence gathered during the 2008-2009 worldwide bans seems to suggest that restrictions on short sales have done little to prevent the decline of stock prices. With the exception of the US, where results may have been affected by a multitude of concurring confounding events such as the TARP, short selling bans in Europe have appeared to have had a non-significant impact on shares prices. I therefore investigate whether this time regulators have been successful in achieving their goal and bringing back the values of the stocks higher to their fundamentals.

I begin by visually inspecting the behaviour of the cross-sectional average cumulative excess returns of banned versus unbanned financial stocks. Excess returns are defined as the difference between the stock returns and the respective country index returns. Graph 3 displays the behaviour of the two groups and shows that the two move very closely together, with banned stocks never experiencing an upsurge in terms of cumulative excess returns; this suggests the ban did not do much to support stock values.

I further move to panel regressions to analyse the effect of the ban in more detail. The model specification is equivalent to the specification (2), namely I estimate the impact of the ban using difference-in-difference analysis and a standard error correction, with the dependent variable being here weekly excess returns. The choice of the weekly frequency is justified by the fact that previous literature (Bris, Goetzmann and Zhu, 2007) has found that this frequency strikes the right balance between noise and information. Estimates are reported in Table 3B Column (1). Column (2) instead reports results estimated on the matched subsample. Both results confirm the visual evidence in Graph 3: although the estimated coefficient on the ban is positive, it is not significantly different from zero.
8. - Conclusions

In this paper I study the impact of the European short selling ban of the summer of 2011, with the aim to shed light on the impact of the ban on market liquidity as well as on the effectiveness of the ban on stock prices performance. Studying the ban is particularly interesting because it differs in many aspects from previously imposed short-selling restrictions; moreover, the very nature of this ban gives the opportunity to identify the effect of the regulation on the market with more precision and under a milder assumption.

The 2011 regulatory measure against short selling appears to have significantly increased bid-ask spreads, thus amplifying the strains in traders’ quests for liquidity at a time of already great hardship. Moreover, stock prices did not appear to have benefited from short selling restrictions; the impact of the ban is indeed not significant. Gathering results from previous literature on short-selling bans and the most recent experience analysed in my thesis, it is evident that imposing short selling constraints may not be the right regulatory response: very stringent bans like the ones imposed worldwide in 2008 proved to be extremely detrimental for the market; loosely defined bans, like the recent European ban, hurt liquidity and fail to reach the target of the regulator.

A final note is worth pointing out. It is a good exercise asking whether the results of this paper are consistent when considered together, and whether these results are coherent with the view of short sellers held by the regulators. Can a short-selling ban dramatically disrupt market liquidity, but at the same time significantly increase stock prices? In fact the results of this paper, along with previous empirical evidence, seem to point to a negative answer: decreased market liquidity caused by the ban is paired with no effect on stock prices. The theoretical explanation behind these paired results may then lie in the argument brought forward by Brunnermeier and Pedersen (2009): when market liquidity goes down, market risk increases, thus making investors require a premium to trade in those stocks that are experiencing higher market risk; this premium is then reflected into prices that hence decline. It follows that any possible positive impact on stock prices may therefore be completely offset by the indirect effect that the regulation has on prices through market liquidity. Policy makers should therefore consider this before imposing bans that harshly impair market liquidity, especially at times of already peaking spreads.
STRUCTURE OF THE BAN

The ban was introduced on the same day – 12th August 2011 – in France, Italy and Spain. The ban was initially intended to last 15 days only, but it was then rolled over to September 30, and once again, up to November 11. It affected only selected stocks in the financial sector, regardless of the trading venue where transactions are executed (MTFs and OTC were henceforth included in the calculation of net short positions).

<table>
<thead>
<tr>
<th>Country</th>
<th>Ban start date</th>
<th>Scope</th>
<th>Financial stocks</th>
<th>Banned stocks</th>
</tr>
</thead>
<tbody>
<tr>
<td>France</td>
<td>12-Aug-11</td>
<td>Selected financial</td>
<td>139</td>
<td>10</td>
</tr>
<tr>
<td>Italy</td>
<td>12-Aug-11</td>
<td>Selected financial</td>
<td>65</td>
<td>29</td>
</tr>
<tr>
<td>Spain</td>
<td>12-Aug-11</td>
<td>Selected financial</td>
<td>52</td>
<td>16</td>
</tr>
</tbody>
</table>

DIFFERENCE-IN-DIFFERENCE: COMMON TIME EFFECTS ACROSS COHORTS BEFORE BAN

I inspect whether the differences in trends over time in the liquidity of the banned stocks compared to their controls, for the pre-ban period, are significantly different from zero.

<table>
<thead>
<tr>
<th>Specification</th>
<th>$S_{it} = y_{0i} + y_{1i} t + \lambda + \varepsilon_{it}$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Interaction Variable $y_{1i}$</td>
<td>Average coefficient 0.472</td>
</tr>
</tbody>
</table>

DIFFERENCE-IN-DIFFERENCE REGRESSION ESTIMATES - BID ASK SPREAD

All regressions include a ban dummy that takes value 1 when the stock is banned and 0 otherwise. Coefficient estimates marked with three (two) asterisks are significantly different from zero at the 1 (5-10) percentage level. The number in parenthesis indicates the $t$-statistics.

<table>
<thead>
<tr>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Countries</td>
<td>All</td>
<td>All</td>
</tr>
<tr>
<td>Dependent variable</td>
<td>Bid-Ask Spread</td>
<td>Bid-Ask Spread</td>
</tr>
<tr>
<td>Ban</td>
<td>0.59***</td>
<td>0.17*</td>
</tr>
<tr>
<td>(3.74)</td>
<td>(1.67)</td>
<td>(2.26)</td>
</tr>
<tr>
<td>Stock Fixed Effects</td>
<td>yes</td>
<td>yes</td>
</tr>
<tr>
<td>Time Effects</td>
<td>yes</td>
<td>yes</td>
</tr>
<tr>
<td>Methodology</td>
<td>Diff-in-Diff (specification (2))</td>
<td>Diff-in-Diff (specification (3))</td>
</tr>
<tr>
<td>Number of stocks</td>
<td>223</td>
<td>223</td>
</tr>
</tbody>
</table>
**Table 3B**

**DIFFERENCE-IN-DIFFERENCE REGRESSION ESTIMATES - EXCESS RETURNS**

All regressions include a ban dummy that takes value 1 when the stock is banned and 0 otherwise. Coefficient estimates marked with three (two) asterisks are significantly different from zero at the 1 (5-10) percentage level. The number in parenthesis indicates the \( t \)-statistics.

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Countries</td>
<td>All</td>
<td>All</td>
</tr>
<tr>
<td>Dependent variable</td>
<td>Excess Returns</td>
<td>Excess Returns</td>
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<tr>
<td>Ban</td>
<td>0.005</td>
<td>0.004</td>
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<tr>
<td></td>
<td>(1.34)</td>
<td>(1.14)</td>
</tr>
<tr>
<td>Stock Fixed Effects</td>
<td>yes</td>
<td>yes</td>
</tr>
<tr>
<td>Time Effects</td>
<td>yes</td>
<td>yes</td>
</tr>
<tr>
<td>Methodology</td>
<td>Diff-in-Diff (specification (2))</td>
<td>Diff-in-Diff (Matching Estimation)</td>
</tr>
<tr>
<td>Number of stocks</td>
<td>223</td>
<td>80</td>
</tr>
</tbody>
</table>

**Table 4**

**BID-ASK SPREADS AND SHORT-SELLING BANS: DIFFERENTIAL LIQUIDITY EFFECTS - OPTIONS**

All regressions include a ban dummy that takes value 1 when the stock is banned and 0 otherwise. Below, I run a seemingly-unrelated estimation to test the presence of any differential liquidity effect. The coefficient estimates marked with three (two) asterisks are significantly different from zero at the 1 (5) percentage level. The number in parenthesis indicates the \( t \)-statistics.

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Countries</td>
<td>All</td>
<td>All</td>
</tr>
<tr>
<td>Ban</td>
<td>0.70***</td>
<td>0.56***</td>
</tr>
<tr>
<td></td>
<td>(11.35)</td>
<td>(2.36)</td>
</tr>
<tr>
<td>Sample</td>
<td>Stocks with traded options</td>
<td>Stocks without traded options</td>
</tr>
</tbody>
</table>

Test of Difference in Coefficients: Not significant: \( Pr>\text{Chi2} = 0.254 \)
Table 5

**BID-ASK SPREADS AND SHORT-SELLING BANS: DIFFERENTIAL LIQUIDITY EFFECTS - FREE FLOAT**

All regressions include a ban dummy that takes value 1 when the stock is banned and 0 otherwise. Below, I run a seemingly-unrelated estimation to test the presence of any differential liquidity effect. The coefficient estimates marked with three (two) asterisks are significantly different from zero at the 1 (5) percentage level. The number in parenthesis indicates the *t*-statistics.

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Countries</td>
<td>All</td>
<td>All</td>
</tr>
<tr>
<td>Ban</td>
<td>1.56***</td>
<td>-0.19</td>
</tr>
<tr>
<td></td>
<td>(8.42)</td>
<td>(-1.48)</td>
</tr>
<tr>
<td>Sample</td>
<td>Stocks with high free float</td>
<td>Stocks with low free float</td>
</tr>
<tr>
<td>Test of Difference in Coefficients</td>
<td>Significant: Pr&gt;Chi2 = 0.000</td>
<td></td>
</tr>
</tbody>
</table>

Graph 1

**EVOLUTION OF THE EUROPEAN AVERAGE PERCENTAGE QUOTED BID-ASK SPREADS**

Evolution of equally weighted cross-sectional average quoted bid-ask spreads for all financial stocks, both banned and non-banned, for the matched sample, in the three countries considered; the dashed lines correspond to the introduction of the bans and their extensions.
AVERAGE PERCENTAGE QUOTED BID-ASK SPREADS FOR THE BANNED STOCKS AND THEIR CONTROLS
CUMULATIVE EXCESS RETURNS

Cumulative excess returns of banned and unbanned stocks for the post-ban period. Cross-sectional average cumulative excess returns of banned stocks compared to unbanned stocks for the post-ban period. Excess returns are defined as the difference between the stock returns and the respective country index returns.
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