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Short-Selling Equity Exchange Traded Funds and Its Effect on Stock Market Liquidity

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Abstract

We examine short selling of equity exchange traded funds (ETFs) using the 2008 short-sale ban. Contrasting the previously documented contractions in bearish strategies during the ban, we find a significant increase in short sales of the largest, most liquid ETF, the S&P 500 Spider. We offer evidence suggesting that this upsurge was driven primarily by investors circumventing the ban. We show that the ban's detrimental effect on stock liquidity was around 30% less severe for the Spider's constituents. Our results suggest that ETF shorts can substitute for short sales of individual stocks, thereby alleviating short-sale constraints' adverse effect on liquidity.

I. Introduction

Existing empirical literature finds that regulatory short-selling constraints are severely detrimental to stock market quality (Beber and Pagano (2013), Boehmer, Jones, and Zhang (2013)). One could expect bearish derivative strategies to alleviate some of these constraints (e.g., Figlewski and Webb (1993)). However, empirically, it is not the case; derivative markets seem to fail to replace stock short sales, particularly during times in which this replacement is needed most. Analyzing the 2008 U.S. temporary short-sale ban on financial-sector stocks ("the ban" or "short-sale ban" hereafter), prior studies show that short-sale order flow did not migrate from stocks to either option markets or single-stock future markets. Instead, they find that those markets experienced a pronounced deterioration in liquidity (Battalio and Schultz (2011), Grundy, Lim, and Verwijmeren (2012)).

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We focus on financial instruments that have not been previously examined in this context: the exchange traded funds (ETFs; portfolios of securities that, similar to stocks, trade continuously on the stock exchange and can be sold short). Over the last decade, the ETF market grew immensely, and its total global size in 2020 is estimated to be over \$6 trillion (ETFGI (2020)). Regulators and practitioners have expressed concern about the potential negative effects of ETFs.¹ The concern is not unfounded; a growing body of work on this subject shows that ETFs can, for example, increase nonfundamental volatility and return comovement of the underlying securities (Ben-David, Franzoni, and Moussawi (2018), Da and Shive (2018)). Moreover, there is some evidence that ETFs can have an adverse effect on liquidity (e.g., Hamm (2014), Israeli, Lee, and Sridharan (2017)); however, the evidence is mixed (Sağlam, Tuzun, and Wermers (2019)). In this article, we bridge the literature on short-sale constraints and ETFs. Using the setting of the 2008 short-sale ban as a laboratory, we examine short selling of equity ETFs, its ability to alleviate short-sale constraints, and its effects on the liquidity of the underlying stocks.

The Sept. 2008 short-sale ban was a surprise, temporary regulatory intervention by the U.S. Securities and Exchange Commission (SEC) banning short sales of essentially all the listed financial-sector stocks. The ban period lasted 14 trading days and led to a significant decrease in short sales of the banned stocks (Boehmer et al. (2013)). It, however, placed no restrictions on the short selling of ETFs. Extant literature documents patterns suggesting that short sellers wanted to short banned stocks during the ban, but were unable to do so (e.g., Boehmer et al. (2013) find that short sales of financial-sector stocks decreased during the ban, but reverted back to pre-ban levels as soon as the ban was lifted). In other words, there may have been substantial pent-up demand for shorting the banned stocks. Hence, we ask whether some of the short-sale order flow migrated to the ETF market.

To answer this question, we examine a sample of 198 U.S. equity, long-only, nonsynthetic (*vanilla*) ETFs that were traded at the time of the ban. We find no increase in short sales in either the full sample of ETFs, or among the financial-sector ETFs. We do, however, find a strong increase in the short sales of the S&P Depository Receipt (SPDR) S&P 500 ETF (ticker symbol SPY), also known as the Spider. The short interest of the Spider increased, on average, by 35% during the ban period. We estimate that, at its maximum, around \$5 billion of new short positions were established using this ETF during the ban. In addition, we document that, during the ban, the number of Spider shares outstanding increased by around 26%, which can be seen as evidence of the “create-to-lend” practice (creating ETF shares for the sole purpose of lending them to short sellers). Our findings are in stark contrast to those of the prior literature showing a contraction in all other bearish trading strategies (e.g., Battalio and Schultz (2011), Grundy et al. (2012)).

We conjecture that the increase in SPY short sales was driven primarily by investors circumventing the ban. We argue that, although the Spider was not a perfect substitute, once one considers its characteristics vis-à-vis other available ETFs, and the relevant institutional details, the SPY emerges as the most appropriate

¹See, for example, “ETF Growth is ‘in Danger of Devouring Capitalism,’” by Robin Wigglesworth, *Financial Times*, Feb. 4, 2018.

instrument to bypass the ban. First, the SPY is well-established, large, liquid, and resilient. In fact, it is the U.S.' oldest, and world's largest and most liquid ETF. The average market capitalization of the Spider (\$75 billion) around the ban period represents 25% of the total market capitalization of all the ETFs in our sample. The Spider's market capitalization was 6 times larger than the total market capitalization of all the financial-sector ETFs, and around 45 times as large as the market capitalization of an average ETF. The Spider's borrowing fees and bid-ask spreads were around 3 and 10 times smaller, respectively, than those of an average ETF during our sample period. In addition, we find that the Spider's liquidity did not deteriorate during the ban, unlike that of the financial-sector ETFs. Second, short selling the Spider as a way of bypassing the ban would have allowed short sellers to mask the true intent of their trades and minimize the risk of their new short positions being banned. These were short sellers' key concerns at the time due to regulatory uncertainty (Battalio and Schultz (2011)), "moral suasion," and regulators' "intimidation tactics" (Sirri (2009)). Finally, and most importantly, shorting the Spider provided effective short exposure to the banned financial-sector stocks; around 70 stocks in its portfolio were banned, and the correlation between the daily returns of the Spider and a financial-sector index was over 0.8 during our sample period.

An alternative explanation is that SPY short sales were driven by investors wishing to short the aggregate market, rather than just the banned stocks, due perhaps to an increase in market pessimism. However, we do not see evidence suggesting that these types of trades were the primary drivers of SPY short sales during the ban. If SPY short sales were driven by a broad increase in short selling, we would expect it to also be reflected in the short sales of its constituents and other broad-index ETFs. We examine the short sales' dynamics of SPY's non-banned constituent stocks and another large, frequently shorted ETF (Russel 2000 ETF), finding that the short sales of these assets actually decreased during the ban. Importantly, we find robust evidence that the buying pressure of large, nonbanned SPY constituent stocks increased significantly during the ban. This evidence is highly suggestive of investors simultaneously taking SPY short positions and offsetting long positions in the key, nonbanned constituents of the SPY, thereby creating net shorts in the banned stocks. This pattern, however, would be difficult to reconcile with a significant spike in aggregate market pessimism during the ban.

Irrespective of the exact motives for the increase in the short sales of the Spider, our results imply that its constituents could be sold short indirectly via Spider short sales despite the ban. Hence, the banned Spider constituents were relatively less short-sale constrained than other similar banned stocks. Given that regulatory short-sale constraints worsen liquidity (e.g., Diamond and Verrecchia (1987)), we examine whether the relaxation of such restrictions via ETF short sales can offset some of this detrimental effect. In particular, we investigate whether the banned constituents of the Spider (i.e., the members of the S&P 500 index) experienced a less severe deterioration in liquidity during the ban than the banned stocks for which short-sale constraints were strictly binding. To this end, we calculate the standard liquidity measures of Holden and Jacobsen (2014) and use a difference-in-difference-in-difference (triple difference) approach to evaluate whether the

average change in the relative liquidity of the banned, S&P 500 member stocks during the ban was significantly different to the nonmember banned stocks.²

Corroborating the results of the existing studies, we show that the average liquidity of the banned stocks severely deteriorated during the ban. For the group of the Spider's banned constituents, however, this detrimental liquidity effect was around 30% less severe than for the other stocks. In other words, the banned S&P 500 member stocks experienced a significantly milder liquidity deterioration relative to similar firms during the ban. This effect holds even after accounting for the aggregate liquidity deterioration. The result is also robust to controlling for stock characteristics like firm size; Brogaard, Hendershott, and Riordan (2017) show that larger firms experienced a relatively milder liquidity deterioration during the short-sale ban. The theoretical model of Diamond and Verrecchia (1987) shows that short-sale constraints negatively affect private information diffusion; thus, we would expect the alleviation of short-sale constraints to reflect primarily in measures relating to information. In line with these predictions, we find that the alleviating effect is most pronounced in the price impact liquidity measure, which relates to the level of information asymmetry, and we find little effect on the realized spread liquidity measure, which relates to inventory and order processing costs (see, e.g., Holden, Jacobsen, and Subrahmanyam (2014) for a discussion on the interpretation of different liquidity measures). In sum, our findings are consistent with the hypothesis that stocks that were less short-sale constrained, due to the ETF short-selling channel, experience a less severe deterioration in their liquidity. However, given the particularity of the S&P 500 firms, we are unable to claim that all of the differential effect stems from the ETF-short-selling channel.³ Nevertheless, our results highlight an additional dimension as to how ETFs can affect the liquidity of their constituents.

We contribute to two strands of literature. First, we directly relate to the research on short-sale restrictions, in particular the work on the 2008 short-sale ban. Due to its surprise imposition and temporary nature, among other factors, this ban remains a useful laboratory for studying the effects of short-sale constraints. The setting has been used to study the impact of short-sale restrictions on the equity markets (Boehmer et al. (2013)), option markets (Battalio and Schultz (2011), Grundy et al. (2012)), American Depositary Receipts (Jain, Jain, McNish, and McKenzie (2013)), and high-frequency trading (Brogaard et al. (2017)). To the best

²We note the difficulty of finding an appropriate comparison group for S&P 500 member stocks, which are, on average, relatively large firms. We mitigate this concern through careful sample selection and by appropriately controlling for firm size in all of our regressions. For example, to reduce the effect of small, illiquid stocks, we only consider stocks with a size greater than that of the smallest S&P 500 member firm, and that had listed options at the time of the ban. In addition, our sample selection procedure excludes the largest financial institutions and S&P 500 index members that were also the greatest benefactors of the TARP rescue package (such as Citigroup, JP Morgan, and Bank of America). Hence, our results are not driven by the liquidity dynamics of the largest TARP recipients.

³The existing literature finds that S&P 500 index addition can have a positive impact on the price of the added firm (Chen, Noronha, and Singal (2004)) and its liquidity (Hegde and McDermott (2003)). Their focus, however, is on the level effects, and the existing studies offer no guidance on the potential reasons why the liquidity of S&P 500 firms would be affected differently by the imposition of short-sale constraints.

of our knowledge, this is the only article analyzing short selling of ETFs during the short-sale ban, and our results indicate that ETFs were the only financial instruments that experienced a meaningful increase in short positions during that period.⁴

Second, we contribute to the growing literature on ETFs, specifically the few studies examining ETF short sales. Evans, Moussawi, Pagano, and Sedunov (2019) examine operational ETF short sales that arise when market makers satisfy excess demand in secondary markets by selling ETF shares that have not yet been created. Huang, O'Hara, and Zhong (2021) focus on the hedging role of industry ETFs, in the context of the "long-the-stock/short-the-ETF" trading strategy, where an investor buys a stock to exploit positive, firm-specific information, while simultaneously short selling an industry ETF to hedge industry risk. We differ from these 2 studies as we examine speculative ETF short sales, particularly the SPY, in the setting of the ban. Our work is closest in spirit to Li and Zhu (2018), who study directional (speculative) ETF short sales and find that high levels of ETF short sales predict future returns of the underlying securities. They further argue that ETFs are used to short stocks that are difficult to short directly (they do not, however, consider the effects on market quality). In addition, by examining the effects of ETF short sales on the liquidity of ETFs' underlying securities, we also add to the current debate on the potential side effects of ETFs. Bhattacharya and O'Hara (2018) develop a model showing that ETFs can increase market fragility. Existing empirical studies show that ETF ownership indeed leads to a number of undesirable outcomes, like increasing nonfundamental volatility (Ben-David et al. (2018)), return comovement (Da and Shive (2018)), and commonality in liquidity (Agarwal, Hanouna, Moussawi, and Stahel (2018)). On the other hand, ETFs have also been shown to improve the informational efficiency of their underlying stocks (Glosten, Nallareddy, and Zou (2020)), and to have long-term positive valuation impact on corporate bonds (Dannhauser (2017)). However, the existing literature finds conflicting results regarding the effect of ETFs on the liquidity levels of their underlying assets. Hamm (2014) and Israeli et al. (2017) find that ETF ownership leads to deterioration of its constituent stocks' liquidity, whereas Sağlam et al. (2019), using higher frequency data, find that liquidity actually improves. Our results are mostly in line with the latter; we find that ETFs alleviate the detrimental effect of short-sale restrictions on their constituents' liquidity. However, we differ significantly from the existing studies on liquidity in that we do not consider the level effect. Moreover, the channel that we investigate is not driven by ETF ownership per se, but rather by the ETF short selling.

Finally, our findings have implications for policymakers. Despite the evidence of their adverse effect on market quality, short-sale bans remain widely used policy tools, as can be seen from the short-sale bans enacted during the COVID-19 crisis in multiple countries.⁵ In recent years, the ETF market grew immensely, and ETFs have become large enough that they can affect financial markets. Thus, regulators

⁴Hendershott, Namvar, and Phillips (2013), who survey the literature, and Grundy et al. (2012) briefly examine the inverse equity ETFs and report that trade in these ETFs was severely disrupted by the ban.

⁵Short-selling bans were introduced in Austria, Belgium, France, Italy, Indonesia, Greece, Malaysia, South Korea, Spain, and Turkey.

wishing to restrict short sales need to pay more considerable attention to the effects of the proposed regulation on the ETFs.

II. Institutional Background

We use the setting of the 2008 temporary short-sale ban on the financial-sector stocks in the U.S. to conduct our study. The setting has been used and explained in detail in a number of previous studies (see, e.g., Battalio and Schultz (2011), Grundy et al. (2012), and Boehmer et al. (2013)). Hence, we provide only a brief description of the ban and focus on the issues that are most relevant to our research question.

Sept. 2008 was a particularly turbulent period for the U.S. financial markets, with growing political pressure for regulators to intervene. In an attempt to stabilize the markets, the SEC uncharacteristically imposed a number of short-selling restrictions. For more than 70 years, regulators had been consistently relaxing short-selling constraints; hence, any short-sale restrictions would have been a surprise to the market (see Sirri (2009) for a discussion).

First, on the evening of Wednesday, Sept. 17, the SEC issued an emergency order banning “naked short selling” of all U.S. stocks, effective from 12:01AM the following day (release no. 34-58572).⁶ On Thursday Sept. 18, after the U.S. market closed, the SEC made a surprise announcement, issuing another emergency order temporarily banning all short sales in 797 financial-sector stocks (release no. 34-58592). A subsequent 134 companies were added to the list, and 10 removed, during the ban. No ETFs were on the initial list, nor were they ever added to the list of banned securities. The ban was effective immediately and was to last 10 business days, terminating at 11:59PM EST on Oct. 2, 2008, with the possibility of an extension to a maximum of 30 calendar days.

On the same day, Sept. 18, the SEC issued an additional order requiring institutional money managers with more than \$100 million in assets under management to file a new form, Form SH, on a weekly basis, detailing their short-selling activity in the previous week (release no. 58591).⁷ The following day, Sept. 19, the SEC issued a press release announcing an expansion of a “sweeping investigation of market manipulation.” The expanded investigation included obtaining statements under oath from hedge fund managers, broker dealers, and other market participants.⁸ Both of these regulatory actions exemplify the use of moral suasion by the U.S. regulators as an additional tool to discourage short selling during that period (see McCaffrey (2009) and Sirri (2009) for detailed discussions).

On Sunday, Sept. 21, the SEC made a few technical amendments to the initial ban that were effective immediately (release no. 34-58611). The key amendments to the ban were the delegation of the decision-making about the ban status of the firms

⁶The SEC defines “naked short selling” as selling short without borrowing the necessary securities in time to make delivery.

⁷The short-selling activities to be disclosed included the number and value of securities sold short and the exact timing of the trades. The order required that the form be filed electronically and made publicly available on EDGAR. A subsequent amendment allowed Form SH to be filed on a nonpublic basis.

⁸See U.S. Securities and Exchange Commission Press Release, “SEC Expands Sweeping Investigation of Market Manipulation” (Sept. 19, 2008).

to the exchanges and the clarification of the fact that market makers were exempt from the ban if they were shorting as part of the bona fide market making and hedging activities. However, in its release, the SEC also stressed that market makers are strongly discouraged from using their exemption to facilitate customers' short sales if the market maker knows that such a trade would result in "establishing or increasing an economic net short position (i.e., through actual positions, derivatives, or otherwise)" in the shares of a firm covered by the ban. The wording seems designed to discourage the exploitation of potential regulatory loopholes that would allow one to bypass the ban and is, arguably, another example of the SEC's use of moral suasion to discourage short selling.

On Thursday, Oct. 2, at the end of the initial period, the SEC chose to extend the ban to Oct. 17 (its statutory limit), or 3 business days following the enactment of Troubled Asset Relief Program (TARP), whichever came first.⁹ On Friday, Oct. 3, President George W. Bush signed the TARP bill, and the ban was lifted on Oct. 8, 2008.

III. Data

Our sample period is from Aug. 1 to Oct. 31, 2008, which we select to ensure homogeneity in the time series, and to better relate to the existing studies on the short-sale ban (particularly Battalio and Schultz (2011), Boehmer et al. (2013)).

We utilize data from several sources. In our analysis of U.S. equity ETF short-sales, we use the securities-lending data from the Markit Securities Finance (MSF, formerly Data Explorers), and the data on prices and basic characteristics are from the Center for Research in Security Prices (CRSP). We source the daily data on ETF net asset value (NAV) and shares outstanding from the State Street Global Advisors website. We construct our sample of 198 long-only, physical (nonsynthetic) U.S. equity ETFs by matching the MSF and CRSP databases and applying a number of filters that identify the relevant ETFs. We provide a description of the ETF sample selection procedure in the Supplementary Material.

MSF collects self-reported data from the lending desks of most of the largest participants in the securities-lending industry, including custodians, lenders, borrowers, and brokers, thus offering an extensive coverage.¹⁰ The securities-lending data are daily. The frequency of the data suits our needs, because we are interested in positions that persist overnight and are unrelated to high-frequency trading or market making. The data comprise security-level information on lending activity. In particular, we use the values and quantities of securities on loan and the lending fees. These variables are measured at settlement day, which is typically 3 days after trade day. Following Jones, Reed, and Waller (2016), we adjust the variables by 3 days to eliminate this settlement lag and reflect the data in trade time.¹¹ Although the securities-lending variables are not a direct measure of short-selling activity,

⁹The TARP (formally, H.R. 1424, the Emergency Economic Stabilization Act of 2008) enabled the U.S. federal government to buy up to \$700 billion of distressed and difficult-to-value assets.

¹⁰According to MSF, their data cover at least 80% of the equity loan transactions in the market. Drechsler and Drechsler (2014) report that for the period from Jan. 2004 to Dec. 2013, MSF database includes over 95% of the U.S. equities in the CRSP database.

¹¹Not adjusting for the timing of the settlement date does not change our results.

they are a good proxy that have been used in the past (e.g., Jones et al. (2016), Geraci, Garbaravicius, and Veredas (2018)) and are well suited for the questions posed in this article. Hence, we treat the securities-lending variables as short-selling variables in our analysis.

We also examine the buying pressure and liquidity of common stocks around the ban. We source the stock prices, returns, and characteristics from CRSP, and we use the Monthly TAQ database to calculate buying pressure and liquidity measures. We obtain S&P 500 index constituents from the Compustat-Capital IQ database. In addition, we use the OptionMetrics database to establish whether a stock had traded options during our sample period. To identify the stocks that were subject to the short-sale ban, we use the list of 797 stocks provided by the SEC in its original release, and the supplementary information (available from the NASDAQ website), on all the subsequent additions to the list of banned stocks and removals from it.¹² We provide a detailed description of the stock sample selection procedure in Section V.A.1 and the Supplementary Material.

IV. ETFs and the Short-Sale Ban

In this section, we examine the effect of the ban on the short sales of U.S. equity ETFs. In particular, given that ETF short sales were never banned, we investigate whether shorting order flow migrated to the ETF market during the ban.

A. ETF Descriptive Statistics

In this subsection, we provide an overview of our sample of ETFs. Table 1 reports the descriptive statistics for the market capitalizations, the short-sale variables, and the bid-ask spreads. We primarily consider the period before the short-sale ban (Aug. 1, 2008 to Sept. 18, 2008) for computing the statistics to give a clearer picture of the ETF market at the onset of the ban.

Our sample consists of 198 U.S. equity, long-only, nonsynthetic ETFs. Their total market capitalization was, on average, \$302 billion just before the ban period. There are 12 pure financial-sector ETFs in our sample, but they constitute only around 4% of the total ETF market capitalization. The most distinctive feature of the ETF market is the severe skewness of the distribution of ETF sizes. Figure 1 plots the average market capitalization of each of the 198 ETFs in our sample, highlighting the lopsidedness of the distribution. The 5 largest ETFs capture close to 42% of the total market capitalization. Most noteworthy is the Spider, the largest ETF, which accounts for around 25% of the total market capitalization of the U.S. equity ETF market during our sample period. The Spider is 4 times larger than the second-largest ETF in our sample, the Powershares NASDAQ 100 (ticker symbol QQQ), making the SPY unique.

Examining the short-sale variables highlights the Spider's prominence. The total market value of ETF short positions during our sample period was around \$31 billion, with short sales of the Spider accounting for a third of that amount (Table 1). Focusing on short interest, the daily quantity of shares on loan scaled by

¹²Information on all of the changes to the banned stocks list that were made during the short-sale ban is available at www.nasdaqtrader.com/Trader.aspx?id=trader_sec_shortsale.

TABLE 1
Exchange Traded Fund (ETF) Descriptive Statistics

Panel A of Table 1 presents the descriptive statistics of the market capitalization, short-sale measures, and the bid-ask spread for the long-only, physical (nonsynthetic) U.S. equity ETFs in our sample. Statistics are presented separately for the full sample of ETFs (All), the purely financial-sector ETFs, the nonfinancial-sector ETFs (nonfinancial), and the S&P Depository Receipt S&P 500 ETF (SPY). The subsample of nonfinancial-sector ETFs excludes the SPY. The statistics for the market capitalization (SIZE) include the total, mean, median, 10th percentile, and 90th percentile, and are expressed in billion dollars. Also reported are the shares, in percent, of the total market capitalization represented by the largest (*top 1 share*) and the 5 largest ETFs in each subsample (*top 5 share*), respectively. The short-sale statistics include the average total market value of short positions (in billion dollars), the average equal-weighted and value-weighted (*v.w.*) short interest (defined as the total number of shares on loan for an ETF on each day over the total number of shares outstanding on Sept. 18, 2008 and expressed in percent), and the average equal-weighted (*fee*) and value-weighted (*v.w. fee*) lending fee (both fee statistics are in percent). The average equal-weighted and value-weighted bid-ask spreads (both in percent) are calculated using the end-of-day bid and ask prices. All the statistics are based on the time-series averages of daily cross-sectional averages. The variable of interest at the ETF level is scaled by its share of the total ETF market capitalization before averaging for value-weighted statistics. The sample period for the summary statistics is from Aug. 1, 2008 to Sept. 18, 2008 (pre-ban period). Panel B presents the average difference between the pre-ban period and the ban period (Sept. 19, 2008 to Oct. 8, 2008) for the value-weighted borrowing fees and the value-weighted bid-ask spreads. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	All ETFs	Financial-Sector ETFs	Nonfinancial ETFs	SPY
<i>Panel A. Summary Statistics</i>				
<i>N</i>	198	12	185	1
Size (\$ billion)				
Total	302.12	12.07	214.35	75.70
Mean	1.66	1.05	1.26	
Median	0.37	0.40	0.36	
10th percentile	0.06	0.07	0.06	
90th percentile	3.55	2.00	3.34	
Top 1 share (%)	25.06	54.79	8.42	
Top 5 share (%)	42.02	88.88	28.14	
Short sales				
Total value (\$ billion)	31.14	1.14	19.33	10.67
Short interest (%)	5.61	10.85	5.17	13.92
<i>v.w.</i> short interest (%)	10.18	9.3	8.91	13.92
Fee (%)	2.27	2.21	2.29	0.35
<i>v.w.</i> fee (%)	1.04	1.04	1.28	0.35
Bid-ask spread (%)	0.21	0.19	0.21	0.01
<i>v.w.</i> bid-ask spread (%)	0.07	0.13	0.09	0.01
<i>Panel B. Ban – Pre-Ban</i>				
Δ <i>v.w.</i> fee (%)	0.75**	0.95***	0.98**	0.24***
Δ <i>v.w.</i> bid-ask spread	0.19***	0.23***	0.25***	0.01

the number of shares outstanding on Sept. 18, 2008, we see that the average short interests for the full sample and the Spider are 5.61% and 13.92%, respectively.¹³

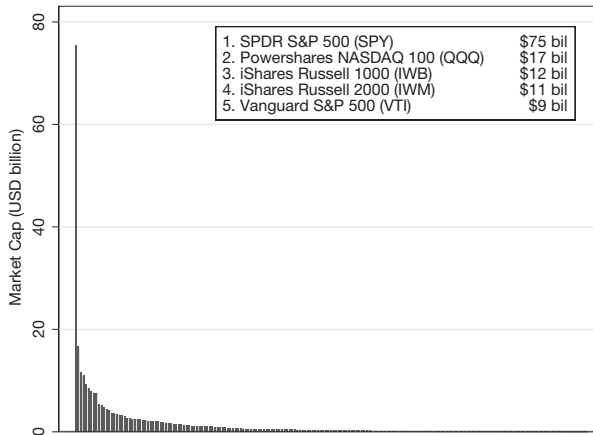
Finally, we look at borrowing fees (a measure of short-sale cost) and bid-ask spreads.¹⁴ For both measures, we calculate the equal-weighted and value-weighted averages. The results highlight the stark difference between the SPY's borrowing fees and spreads, and those of the other ETFs. The Spider's average borrowing fee (0.35%) and bid-ask spread (0.01%) were around 3 and 10 times smaller, respectively, than those of an average ETF during our sample period. Similar to what was documented for other assets, equity ETFs experienced a deterioration in liquidity

¹³Given the distribution of ETF sizes, the simple average may put too much weight on the smaller ETFs, distorting the economic interpretation. Hence, we also calculate the market capitalization weighted average short interest. Regardless of the measure used during our sample period, the Spider appears to have been more actively short sold than the other ETFs.

¹⁴The borrowing fees are expressed in percent per annum and represent a rate that a short seller is required to pay to borrow a security. The bid-ask spread is the difference between the end-of-day ask and bid price from CRSP divided by half their sum (expressed as a percentage).

FIGURE 1
Market Capitalization of U.S. Equity ETFs Around the 2008 Short-Sale Ban

Figure 1 displays the average market capitalization of all the long-only, physical (nonsynthetic) U.S. equity exchange traded funds (ETFs) in our sample. The box reports the average market capitalizations (in billion dollars) of the 5 largest ETFs during our sample period. Our sample contains 198 ETFs, and the sample period is from Aug. 1, 2008 to Oct. 31, 2008.



during the ban period. The bid–ask spread rose, on average, by 0.19 percentage points across all of the ETFs. However, the increase in the bid–ask spread of the Spider was small and statistically insignificant. ETF borrowing fees also increased during the ban period. However, the absolute increase in the borrowing fees of the Spider (0.24 percentage points) was around 4 times smaller than the corresponding increase in the borrowing fees of financial-sector and nonfinancial ETFs. In sum, the Spider is substantially larger, more liquid, and more resilient to aggregate liquidity shocks than any of the other ETFs in our sample.¹⁵

B. ETF Short Sales

In this subsection, we ask whether there was a significant increase in ETF short selling during the short-sale ban. To this end, we estimate the following ordinary least squares (OLS) regression on our ETF panel:

$$(1) \quad \text{SHORT_INTEREST}_{i,t} = \alpha_i + b\text{BAN}_t + \varepsilon_{i,t},$$

where $\text{SHORT_INTEREST}_{i,t}$ denotes the number of shares on loan for ETF i on day t that is scaled by the total number of shares outstanding on Sept. 18, 2008.¹⁶ α_i is a time-invariant ETF fixed effect, and BAN_t is an indicator variable that takes the value of 1 on the 2008 short-sale ban days (Sept. 18 to Oct. 8), and 0 otherwise.

¹⁵Khomyn, Putnins, and Zoican (2020) show that highly liquid ETFs like the Spider are able to charge higher management fees due to their relatively superior liquidity.

¹⁶We scale by the shares outstanding on a specific date, because ETF shares outstanding fluctuate daily. Hence, scaling by each day's shares outstanding would introduce additional noise into the analysis of short sales. We discuss ETF share creation later in the article.

TABLE 2
The Impact of the Short-Sale Ban on the Short Interest of ETFs

Table 2 reports the regression results of daily short interest on the short-sale ban indicator. Short interest is defined as the total number of shares on loan for an exchange traded fund (ETF) each day divided by the total number of outstanding shares on Sept. 18, 2008 and expressed in percent. BAN is an indicator variable that takes the value of 1 on the days of the 2008 short-sale ban, and 0 otherwise. Columns 1 and 2 display the results for the full sample of ETFs and the subsample of financial-sector ETFs, respectively. The specifications include ETF fixed effects. In columns 1 and 2, the reported standard errors are clustered at ETF and time level. Column 3 displays the results for the S&P Depository Receipt S&P 500 (SPY) ETF. Column 4 displays the results for the short interest index of the common stocks, excluding financial-sector stocks, in the S&P 500 index. The short-sale index for the S&P 500 stocks is the value-weighted sum of the daily short interest of the individual S&P 500 member stocks. In columns 3 and 4, (Newey and West (1987)) standard errors with 3 lags are reported. The data are daily, and the sample period is from Aug. 1, 2008 to Oct. 31, 2008. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	All	Financial ETFs	SPY	Ex-Financials S&P 500
	1	2	3	4
BAN	-0.03 (0.25)	2.06 (1.63)	4.83*** (0.70)	-0.09 (0.11)
Constant			14.48*** (0.37)	2.60*** (0.09)
No. of obs.	10,506	717	65	65
Adj. R^2	0.92	0.73	0.63	0.00
ETF fixed effects	Yes	Yes	No	No

We use standard errors that are clustered by individual ETF and time when considering a panel of ETFs, and Newey and West (1987) standard errors when considering just a single series. We present the regression results in Table 2.

First, we examine the full sample of ETFs to establish a benchmark. The estimated coefficient on the ban indicator is around 0 and statistically insignificant, suggesting that there was no widespread increase in short interest among equity ETFs (column 1 of Table 2). This finding is not surprising. Given that the 2008 short-sale ban covered only the financial-sector stocks, one would not expect to see an increase in short sales in a broad spectrum of equity ETFs even in the presence of order flow migration.

Second, we analyze the short selling of the 12 financial-sector ETFs. Our results indicate that there was also no significant increase in short selling of financial-sector ETFs during the ban period (column 2 of Table 2). These results may seem surprising, because, at first glance, financial-sector ETFs appear as a suitable substitute for the banned financial-sector stocks. However, once we consider their small size, their relative illiquidity, and regulatory pressure, it becomes clear that short-selling financial-sector ETFs were not a viable alternative to short selling the banned stocks.

Third, we separately examine the dynamics of the short interest of the Spider. We proceed by estimating regression 1 which, in this case, simplifies to a single time-series regression. We observe a quantitatively substantial and statistically significant increase in the short interest of the SPY during the ban period (column 3 of Table 2). The average level of the short interest increased by 4.83 percentage points during the ban, representing a 35% increase from SPY's average short interest level in the pre-ban period. This increase is economically significant. Based on SPY's price on Sept. 18, 2008 (\$120 per share), this increase in short interest amounted to the creation of at least \$3.5 billion worth of new short positions during

the ban period.¹⁷ One may be concerned that this increase in SPY short sales is driven by arbitrage activity. In particular, if an ETF trades at a premium from its NAV, an arbitrageur can buy the underlying portfolio and short the ETF to profit from the spread. This type of arbitrage, however, occurs at a very high frequency (see, e.g., Petajisto (2017), Staer and Sottile (2018)), while our short interest measure captures only the short positions that are kept open overnight. Thus, NAV arbitrage is unlikely to be driving our results.¹⁸ We elaborate further on the ETF NAV-arbitrage mechanism when discussing ETF creation in Section IV.E.

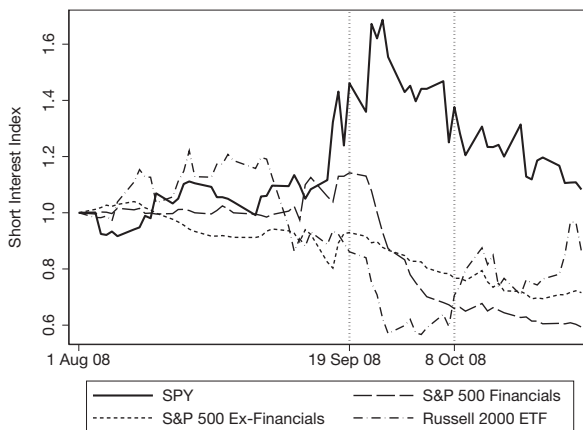
C. The Drivers of Spider Short Sales

In this subsection, we discuss potential explanations for the drivers of the increase in short sales of the SPY during the ban.

Figure 2 shows a time series of the Spider's scaled short interest during our sample period, which visually supports the regression results of the previous subsection. SPY's short interest was significantly elevated during the 14 trading days of the ban period. It reached its sample maximum during the ban and returned

FIGURE 2
Short Selling of the SPY and Its Constituents Around the 2008 Short-Sale Ban

Figure 2 displays the time series of daily short interests of the S&P Depository Receipt S&P 500 exchange traded fund (ETF) (SPY), the short interest indexes of the common stocks in the S&P 500 index, and the Russell 2000 ETF. The short-sale indexes for the underlying stocks are constructed as the value-weighted sums of the daily short interest of the individual S&P 500 member stocks. Each series is scaled by their respective values on Aug. 1, 2008. The 2 vertical dotted lines indicate the short-sale ban period. The data are daily, and the sample period is from Aug. 1, 2008 to Oct. 31, 2008.



¹⁷This quantity can be viewed as a lower bound of the total value of new short positions that were established during the ban, because it represents a net average increase over the 14 trading days of the ban and does not account for turnover in short sales. The median duration of a stock loan is around 3 trading days (Geczy, Musto, and Reed (2002)), that is, many short positions are opened and closed within a couple of days. Hence, if each of the newly established short positions during the ban was held open for only 3 days, it is possible that up to 5 times as many new short positions were established during the ban than our estimated average would suggest.

¹⁸In unreported results, we consider SPY premium as a control in the regression, and we find that its estimated coefficient is near 0 and there is no effect on the other estimates.

to the pre-ban level after the ban was lifted. Visually, it appears that the ban was the principal driver of this increase. However, given that the ban coincided with the epicenter of the financial crisis, it is also possible that SPY short sales were driven by the aggregate increase in short selling due to, for example, “market pessimism.”

To check whether investors short selling the aggregate market is the main driver of the observed increase in SPY short sales, we first examine the short sales of SPY’s underlying stocks. We posit that if SPY short sales are driven by a broad increase in short selling, it would also be reflected in the short sales of its constituents. We construct value-weighted indexes of short interest of S&P 500 member stocks. [Figure 2](#) plots the time series of the short-selling index of the financial-sector S&P 500 member stocks and the index of S&P 500 stocks, which excludes the financial-sector stocks (S&P 500 Ex-Financials). Although we see a slight increase in the short interest of both the SPY and its underlying shares around the Lehman Brothers bankruptcy, we do not observe any pronounced trends before the imposition of the ban. Moreover, short selling of all of the S&P 500 stocks appears to decrease during the ban period. We confirm our visual intuition by estimating regression 1 on the short interest index of S&P 500 Ex-Financials stocks.¹⁹ The estimated coefficient on the ban indicator is negative, albeit insignificant, implying that there was no increase in the short interest of the nonfinancial-sector S&P 500 stocks (column 4 of [Table 2](#)).²⁰

Short selling an ETF like the SPY is easier and more cost-effective than short selling a basket of individual stocks. Hence, it is possible that investors, who wished to short the market, may have had strict preferences for short-selling ETFs. However, the Spider was not the only suitable ETF that could have been used to short the aggregate market. Thus, if SPY short sales were driven by an aggregate increase in short selling, we should also see elevated short sales in other broad-market index ETFs. To see if this is the case, we examine another large ETF with similar characteristics to those of the Spider. We consider the Russell 2000 ETF (ticker symbol IWM), which is a portfolio of 2000 small U.S. stocks and the fourth largest ETF in our sample. It is a broad market index and, arguably, an ideal target for a trader wishing to short the market, because small stocks typically underperform in a recession (see, e.g., Perez-Quiros and Timmermann (2000)). Importantly, similar to the SPY, it is actively sold short.²¹ We plot the short interest of the Russell 2000 ETF in [Figure 2](#). Similar to the SPY constituents, short interest of the Russell 2000 ETF decreases during the ban period. This pattern suggests that there is little evidence of an aggregate increase in short selling during the ban period.

¹⁹We consider only the stocks that were not subject to the short-sale ban, S&P 500 Ex-Financials, because it has already been shown that short selling of the financial-sector stocks decreased significantly during the ban (e.g., Boehmer et al. (2013)).

²⁰We verify that the decrease in the average short selling of the underlying stocks (a decrease in quantity) is not driven by a significant increase in borrowing fees (price). In unreported results, we examine the average borrowing fees of the underlying nonfinancial sector stocks, contrasting it to the Spider borrowing fees during the ban. We do not observe a significant increase in the fees of the underlying shares, which implies that cost-based substitution was not the driver of the rise in Spider short sales.

²¹The average short interest of IWM during our sample period is around 49%, which contrasts the other 3 large ETFs (the QQQ, the IWB, and the VTI) whose average short interests are less than 0.6%.

We cannot entirely rule out that some of the new short positions in the SPY were created by those wishing to short the entire market. A more likely explanation, however, is that the surge in short positions of the SPY was driven primarily by short sellers wishing to short banned financial-sector stocks. During the ban period, a typical short seller needed to establish a temporary alternative position on short notice that would provide short exposure to a portfolio of financial-sector stocks, and that would not attract undue regulatory attention to either the short seller or her broker. Given this problem, we argue that the Spider was an ideal instrument for short-selling financial-sector stocks during the ban. First, short selling the SPY would have provided effective exposure to the financial sector and was simple to execute. At the time of the ban, around 16% of the Spider's underlying portfolio was comprised of financial-sector stocks.²² Of course, by short selling the Spider, the short seller would be effectively shorting all of its underlying stocks. However, in order to create an almost perfect short of the financial-sector S&P 500 stocks, the short seller of the SPY would have needed to simply open some offsetting long positions in the SPY's underlying nonfinancial-sector stocks (we present evidence for this mechanism in the next subsection). Second, given that the Spider is a popular and well-understood financial instrument (e.g., Elton, Gruber, Comer, and Li (2002)) and the fact that practitioners are known to frequently short ETFs, particularly the SPY, for risk management purposes (Gastineau (2010)), it would have been a simple trade to execute, even at short notice. Third, given that the Spider is an equity instrument, investors (those with strict investment mandates limiting their use of derivatives and investors with no established derivative trading technology at the time of the ban) would have preferred shorting the Spider to bearish derivatives strategies. Finally, short-selling financial-sector stocks via the Spider would have allowed short sellers to mask the true intent of their trades and give their brokers "plausible deniability" in facilitating such trades, because it could be argued that SPY short sales were for risk management.²³ Given the concern for secrecy during the ban, shorting banned stocks via the Spider would have been a better alternative to short-selling financial-sector ETFs or using derivatives (e.g., buying put options) in which one's intentions were obvious to one's broker and the regulators.

D. Offsetting Positions

In this subsection, we present empirical support for the conjecture that the observed increase in SPY short sales was driven by traders wishing to circumvent the ban. In particular, we exploit the idea that if investors shorting the SPY aimed to short only the banned financial-sector stocks, they would have needed to establish offsetting long positions in nonbanned (nonfinancial) constituents of the SPY to render their SPY short sales into net short positions of financial-sector stocks.²⁴ We

²²The correlation of daily returns of the Financial Select Sector SPDR ETF (which seeks to replicate the performance of the S&P 500 Index's financial sector and the daily returns of the SPY) was 0.83.

²³As we have discussed in Section II, the SEC explicitly instructed that market makers not facilitate transactions that were attempting to bypass the spirit of the ban.

²⁴We thank the anonymous referee for this suggestion.

posit that, if SPY short sellers were establishing such offsetting long positions, we would see differential effect on buying pressure of nonfinancial SPY constituents during the ban. We note that the S&P 500 is a value-weighted index. Hence, a trader wishing to isolate her short exposure to financial-sector SPY constituents would be mechanically buying more of the larger nonfinancial SPY constituents. In addition, to reduce trading costs and capital commitment, traders who were shorting the Spider to circumvent the ban could have opted for an imperfect hedge by establishing just the key offsetting positions in the large nonfinancials. To test these conjectures formally, we estimate the following OLS panel regression:

$$(2) \quad \text{BUY}_{i,t} = \alpha_i + e_1 \text{BAN}_t + e_2 \text{LARGE}_i + e_3 \text{BAN}_t \times \text{LARGE}_i + \varepsilon_{i,t},$$

where $\text{BUY}_{i,t}$ is a measure of signed buying pressure on day t for stock i , constructed using intraday data from TAQ.²⁵ As our main measure, we define the stock-day fraction of buy initiations using a dollar-volume-weighted, BUY_DOLLAR , daily buying pressure scaled by the total buy and sell volume for each stock on each day (expressed in percent). BAN_t is an indicator variable that takes the value of 1 on the 2008 short-sale ban days, and 0 otherwise. In later specifications, we replace BAN_t with $\text{SI}_{\text{SPY},t}$ (daily short interest of SPY ETF (shares sold short, normalized by shares outstanding on Sept. 18, 2008, in percent)) to exploit the continuous variation in SPY shorting. LARGE_i is an indicator variable which is equal to 1 if the pre-ban market capitalization of stock i is larger than the median, and $\text{BAN}_t \times \text{LARGE}_i$ is the interaction between the 2 variables. α_i is a time-invariant stock fixed effect. We use standard errors clustered at stock and time level. Table 3 presents the results.

We find that, unconditionally, buying pressure did not increase during the ban period (column 1 of Table 3). However, we demonstrate that there was significantly more buying of large, nonbanned S&P 500 stocks (column 2). The coefficient on $\text{BAN}_t \times \text{LARGE}_i$ is positive (0.25) and statistically significant. Importantly, in economic terms, the average increase in buying pressure of large, nonbanned S&P 500 stocks during the ban roughly corresponds to the increase in the SPY short sales during that period.²⁶ The result remains similar after replacing the ban indicator with a continuous variable measuring daily SPY short interest (column 3). This measure better captures the timing of short sales and the required offsetting positions. The results are also robust to using equal-weighted, BUY_EQUAL , and trade-size weighted, BUY_TRADE , measures of buying pressure (columns 4 and 5). Moreover, we find that the average buying pressure returns to essentially pre-ban level in the post-ban period.²⁷

²⁵We consider 391 nonbanned S&P 500 stocks and use the Lee and Ready (1991) algorithm to identify trades initiated by buyers and sellers.

²⁶The total trading volume of large, nonbanned S&P 500 stocks during the ban was over \$1.2 trillion. Our estimated effect of a 0.25% increase in buying pressure translates roughly to additional buying of \$3.04 billion worth of shares, which is in the ballpark of the \$3.5 billion worth of new SPY short positions that were established during the short-sale ban (based on the SPY price on Sept. 18, 2008).

²⁷The Supplementary Material plots the average buying pressure for the nonbanned SPY constituents during the different periods of our sample.

TABLE 3

The Impact of the Ban on the Buying Pressure of Nonbanned S&P 500 Stocks

Table 3 reports panel regression results of daily measures of buying pressure for the sample of S&P 500 index stocks that were not subject to the 2008 short-sale ban of the form:

$$\text{BUY}_{i,t} = \alpha_i + e_1 \text{BAN}_t + e_2 \text{LARGE}_t + e_3 \text{BAN}_t \times \text{LARGE}_t + e_{i,t}.$$

The daily buying pressure measure for each stock is the weighted sum of intraday buy trades where trades are identified as buys using the Lee and Ready (1991) algorithm, scaled by the daily dollar volume on each day. The 3 different weighting schemes are: dollar-volume weighted, BUY_DOLLAR, equal-weighted, BUY_EQUAL, and share-weighted, BUY_TRADE. Buying pressure measures are in percent. BAN is an indicator variable that takes the value of 1 on the 2008 short-sale ban days, and 0 otherwise. Variable LARGE takes a value of 1 if the average stock market capitalization in July 2008 is above the sample median, and 0 otherwise. In column 3, BAN is replaced by SI_{SPY} , the daily short interest of the S&P Depository Receipt S&P 500 (SPY) exchange traded fund, defined as its total number of shares on loan on each day scaled by its total number of shares outstanding on Sept. 18, 2008, expressed in percent. $\text{BAN} \times \text{LARGE}$ and $\text{SI}_{\text{SPY}} \times \text{LARGE}$ are the interactions between the 2 variables. All regressions include stock fixed effects. Standard errors are clustered at stock and time level. The data are daily, and the sample period is from Aug. 1, 2008 to Oct. 31, 2008. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	BUY_DOLLAR 1	BUY_DOLLAR 2	BUY_DOLLAR 3	BUY_TRADE 4	BUY_EQUAL 5
BAN	0.14 (0.26)	0.02 (0.25)		0.19 (0.31)	0.02 (0.24)
BAN \times LARGE		0.25** (0.12)		0.28** (0.12)	0.25** (0.12)
SI_{SPY}			-0.01 (0.04)		
$\text{SI}_{\text{SPY}} \times \text{LARGE}$			0.06** (0.02)		
No. of obs.	25,285	25,285	25,285	25,285	25,285
Adj. R^2	0.03	0.03	0.03	0.03	0.03

In sum, our results show that the buying pressure for large, S&P 500 nonbanned constituents increased during the ban period and decreased to pre-ban levels after the ban was lifted. This evidence is highly suggestive of investors simultaneously taking SPY short positions and offsetting long positions in the key, nonbanned constituents of the SPY. These results support our argument that investors were short selling the SPY to circumvent the ban. In contrast, this evidence would be difficult to reconcile with a significant spike in aggregate market pessimism during the ban.

E. Supply of the Spider and the “Create-to-Lend” Mechanism

A distinctive feature of ETFs is that the number of ETF shares can change daily through the creation–redemption mechanism. Typically, this mechanism ensures that ETFs trade close to their NAV. However, additional shares of an ETF can be created solely to facilitate short selling. This ability, termed “create-to-lend,” is unique to ETFs (Welter (2012)). Essentially, if a broker’s client requests to borrow an ETF, which is not readily available, the broker can borrow or buy the underlying securities and create a new ETF unit to subsequently lend to the short seller (the Appendix presents a diagrammatic depiction of this mechanism). It is important to note that while short selling of financial-sector stocks was banned, borrowing was not.²⁸ There is anecdotal evidence of this practice (see, e.g., Gastineau (2004),

²⁸In the event that the broker purchases the underlying basket of securities to create an ETF for a short seller, the broker can short the underlying securities to offset its exposure (market makers were exempt from the short-sale ban if their short positions were used for hedging).

(2010)); however, to the best of our knowledge, there is no analysis of it in the academic literature.²⁹

In this subsection, we present some empirical support for the “create-to-lead” mechanism by examining the dynamics of SPY shares outstanding around the short-sale ban. Graph A of Figure 3 plots the SPY shares outstanding during our sample period. We observe a dramatic increase in the number of SPY shares outstanding during the ban, and a decrease, but not a complete reversal, because the ban is lifted.³⁰ Regression analysis confirms that the increase in SPY shares during the ban is statistically significant (the Supplementary Material presents these results). Interestingly, despite the general decrease in prices, Spider’s asset under management (AUM) increased during the ban, because the effect of the new share issuance overshadowed the impact of the falling price. Given the documented surge in SPY short sales during the ban, these patterns are in line with the “create-to-lead” mechanism.

One concern is that all of the share creation could have been driven by NAV-arbitrage activity, particularly if SPY was trading at a premium during the short-sale ban. An important role of the ETF creation mechanism is to ensure that an ETF trades close to its NAV. If the total cost of the underlying basket of assets, the ETF’s NAV, is less than the price of the ETF (i.e., the ETF is trading at a premium), an arbitrageur can purchase the underlying assets, deliver the basket of constituent securities to the AP, and sell the newly created ETF (Brown, Davies, and Ringgenberg (2021) provide a detailed description of this mechanism). To evaluate the potential effect of the NAV-arbitrage channel, we begin by visually examining SPY’s premium alongside SPY’s shares outstanding (Graph C of Figure 3). Visually, we do not observe a clear pattern in SPY premium that could explain the large increase in SPY shares during the ban. Moreover, including SPY premium as a control in a regression has no impact on either the size or statistical significance of the increase in SPY shares during the ban (the Supplementary Material reports the results).³¹

Another concern is whether it would have been reasonable for a short seller, or her broker, to choose to borrow multiple securities to create the needed ETF, instead of just borrowing the ETF. We note that using the “create-to-lead” channel instead of directly borrowing an ETF minimizes recall risk, because the borrowing is diversified across hundreds of securities. There is anecdotal evidence suggesting that the willingness of certain institutions to lend during that time may have been

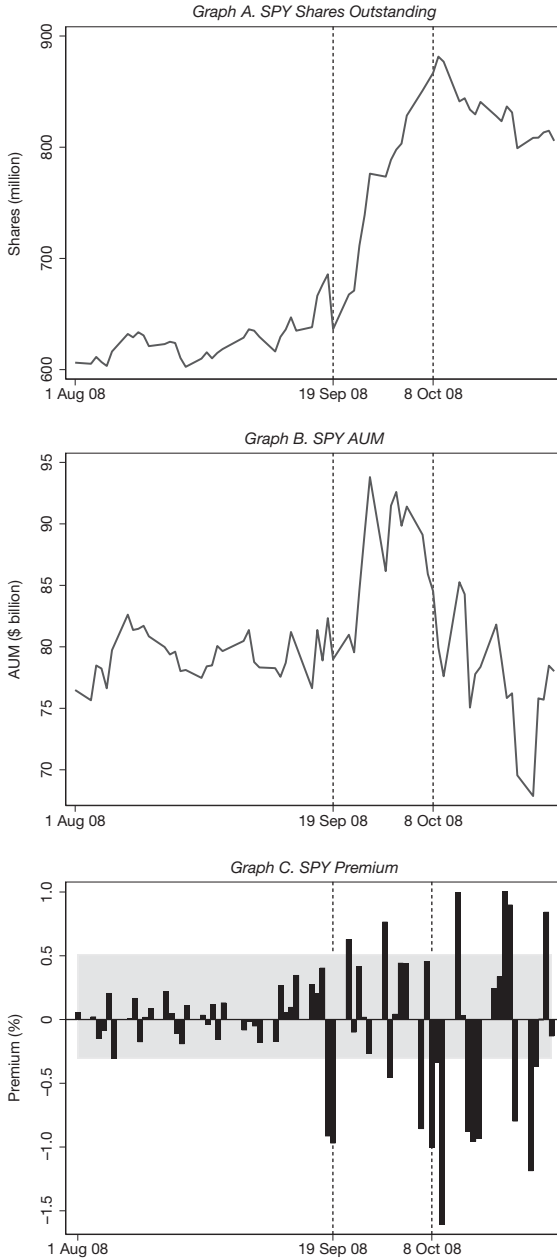
²⁹For a more recent example, see “JP Morgan Sees Shorts Behind \$9 Billion Influx Into S&P ETF” by Katherine Greifeld, *Bloomberg*, Mar. 17, 2020.

³⁰Because both the creation and destruction of ETF shares carry a cost, we do not expect all the newly created ETF shares to be destroyed after the short positions are covered and the borrowed ETFs are returned.

³¹Daily measures of premium may not adequately control for the NAV arbitrage, because the bulk of it takes place intraday (Petajisto (2017)). Investigation of high-frequency NAV arbitrage is beyond the scope of this article. However, the “create-to-lead” channel and the NAV arbitrage are not mutually exclusive. Around 180 million additional SPY shares were created during the ban period, which is substantially more than the total 135 million SPY shares borrowed at the peak of the SPY short interest during the ban. Thus, it is reasonable to believe that even if a large part of the new share creation was explained by NAV arbitrage, there is sufficient scope for a portion of the increase to be driven by the “create-to-lead” practice.

FIGURE 3
 SPY ETF Share Creation and
 Premium Around the 2008 Short-Sale Ban

Graphs A and B of Figure 3 plot the daily time series of the number of S&P Depository Receipt S&P 500 exchange traded funds (ETF (SPY)) shares outstanding (in US\$ millions) and its asset under management (AUM; in billions). The 2 vertical dotted lines indicate the short-sale ban period. Graph C plots SPY's daily premium (in percent). The shaded area represents 2 pre-ban period standard deviations below and above the pre-ban mean. The data are daily, and the sample period is from Aug. 1, 2008 to Oct. 31, 2008.



affected due to political reasons. For example, public pension funds in New York and California withdrew their shares from asset lending programs around the time of the 2008 short-sale ban.³² Hence, minimizing recall risk may have been an important concern at the time. Moreover, institutional ownership of the SPY's constituent stocks is very high, making it fairly easy to borrow.³³

Although we cannot prove that the additional SPY shares were created primarily for the purpose of lending to the short sellers, our analysis supports this motive.

V. Liquidity of the Underlying Stocks

In this section, we examine the effect of ETF short selling on the liquidity of the underlying stocks. Existing literature finds that short-sale bans have a strongly detrimental effect on liquidity (Beber and Pagano (2013), Boehmer et al. (2013)). If regulatory short-sale constraints deteriorate stock liquidity, one could expect the easing of such restrictions via ETF short sales to at least partially counteract their detrimental effect. Thus, using the setting of the ban, we examine stock liquidity and ask whether the adverse impact of the ban on liquidity is different for stocks that were sold short indirectly via ETFs.

Given our finding that only the Spider's short sales increased significantly during the ban, we compare the effect of the ban on the liquidity of the Spider's constituent stocks with the ban's effect on liquidity of the stocks not in the Spider's portfolio. The Spider is a portfolio of S&P 500 stocks; hence, our analysis amounts to the comparison of the liquidity changes around the time of the ban of both the banned S&P 500 index member and nonmember stocks. Naturally, the special features of the S&P 500 member stocks present a challenge for disentangling any effects. To alleviate this concern, we first select our sample accordingly. Second, we use a triple difference approach in our formal analysis. We present the detailed description of our approach and the results in the next subsections.

A. Stock Sample Construction, Variable Definitions, and Descriptive Statistics

1. Sample Construction

We closely follow the existing literature in constructing our sample of stocks. In particular, we restrict our sample to only the common stocks which were listed on the 3 main U.S. exchanges, priced higher than \$5 a share at the start of the ban, and had traded options. After applying these initial filters, we identify the stocks that were subject to the short-sale ban. We exclude from the sample all stocks that were added to the banned list after Sept. 23, 2008, or that were removed from it at any time before the ban's expiration.³⁴ We differ from the prior studies on the short-sale ban in that our research question requires the comparison between S&P 500 index

³²See "A Bid to Curb Profit Gambit as Banks Fall" by Vikas Bajaj and Jonathan D. Glater in *The New York Times*, Sept. 18, 2008.

³³Using the institutional holding database of Thomson Reuters, we find that the average (median) institutional ownership of S&P 500 stocks in Sept. 2008 was 76% (78%).

³⁴We provide additional details regarding our sample construction in the Supplementary Material.

member firms and nonmember firms. This comparison complicates the sample selection process, because S&P 500 member firms are typically larger. However, size is not the only determinant of a firm's inclusion in the S&P 500 index.³⁵ The determination of the S&P 500 membership is not purely rule-based but is decided, with a degree of discretion, by S&P's Index Committee based on the criteria such as domicile, liquidity, size of its public float, sector classification, and other factors. Thus, there is scope for finding a control group of non-S&P 500 member firms relatively comparable in size to those of the index members.

We identify the firms that were S&P 500 members as of the first day of the short-sale ban. Then, guided by the sample trimming rule proposed by Crump, Hotz, Imbens, and Mitnik (2009) to alleviate limited overlap in covariate distributions between treatment groups, we restrict our sample to firms whose average market capitalization in July 2008 was between the minimum (\$0.7 billion) and the 90th percentile (\$46 billion) of the market capitalization of the banned S&P 500 stocks.³⁶ Our final sample consists of 1,397 stocks, 66 of which are banned S&P 500 index members and 110 of which are also banned, but are not members of the S&P 500 index (nonmembers).³⁷ The other 1,121 stocks are the stocks that were not subject to the short-sale ban (nonbanned stocks), and 344 of those were members of the S&P 500 index. We include in our sample the stocks that were never subject to the short-sale ban, because they are necessary for controlling for broad market effects. However, the identification of the differential effect of interest is driven exclusively by the banned stocks in our sample; hence, we focus the discussion on their characteristics.

Panel A of Table 4 presents the descriptive statistics of the firm sizes for the 4 different groups of stocks in our sample: the banned S&P 500 members, the nonbanned S&P 500 members, the banned nonmembers of the S&P 500, and the nonbanned nonmembers of the S&P 500. Among the group of S&P 500 member stocks and the group of nonmember stocks, the average sizes of the banned and nonbanned firms are very similar. However, as expected, an average S&P 500 member firm is larger than an average nonmember firm. The average (median) sizes of a banned S&P 500 member firm and a banned nonmember firm are \$13.79 billion (\$10.27 billion) and \$3.3 billion (\$1.86 billion), respectively. Notwithstanding, the 2 groups are not incomparable. Figure 4 separately displays the average sizes of each firm of the banned S&P 500 members and the banned nonmembers. Visual inspection of Figure 4 suggests that there are several commonalities between the 2 groups. For example, the minimum and maximum sizes of the different types of firms in our sample are, by construction, essentially identical. Moreover, for any

³⁵This is well illustrated by the fact that, among the 500 largest common stocks in our initial sample on Sept. 18, 2008, only 358 were members of the S&P 500 index. In addition, prior to 2013, the S&P 500 index could only include U.S. companies. For example, Goldman Sachs and Deutsche Bank were on the short-sale ban list and were similar in size (approximately \$42 billion) at the onset of the ban, yet only Goldman Sachs was a member of the S&P 500 index.

³⁶Our main results remain quantitatively similar if we instead consider the 1st, 5th, or 10th percentile of the market capitalization of the banned S&P 500 stocks as the lower cutoff for sample selection.

³⁷Among the members of the S&P 500 index that were on the ban list, our trimming procedure eliminates the 6 largest firms such as General Electric (market capitalization around \$280 billion) and JP Morgan (market capitalization around \$130 billion), for which no reasonable comparable firms exist.

TABLE 4
Descriptive Statistics of Market Capitalization and Liquidity for the Sample of Stocks

Table 4 reports descriptive statistics of the market capitalization and 4 liquidity measures for the sample of common stocks listed in the U.S. The sample is split into stocks that were subject to the 2008 short-sale ban (banned) and those that were not (nonbanned). It is further split into stocks that are members of the S&P 500 index and the stocks that are not. Each firm's market capitalization is its average market capitalization in July 2008. The statistics for the market capitalization (SIZE) include the mean, median, minimum, and maximum, and are expressed in billion dollars. The 4 liquidity measures are the quoted spreads, effective spreads, 5-minute price impact, and 5-minute realized spreads (see Section V.A.2 for their definitions). For each stock, the 4 liquidity measures are computed using intraday trade and quote data and aggregated to the daily level (quoted spreads are time-weighted, and effective spreads, price impact, and realized spreads are trade-weighted). Each measure is proportional to the prevailing quote midpoint and is expressed in basis points. Δ denotes differences (either between periods or between subsamples of stocks), and 2Δ denotes the difference in differences. The data are daily. Two sample periods for the liquidity statistics are the *pre-ban* period, which is the period before the imposition of the short-sale ban (Aug. 1, 2008 to Sept. 18, 2008) and the *ban* period (Sept. 19, 2008 to Oct. 8, 2008).

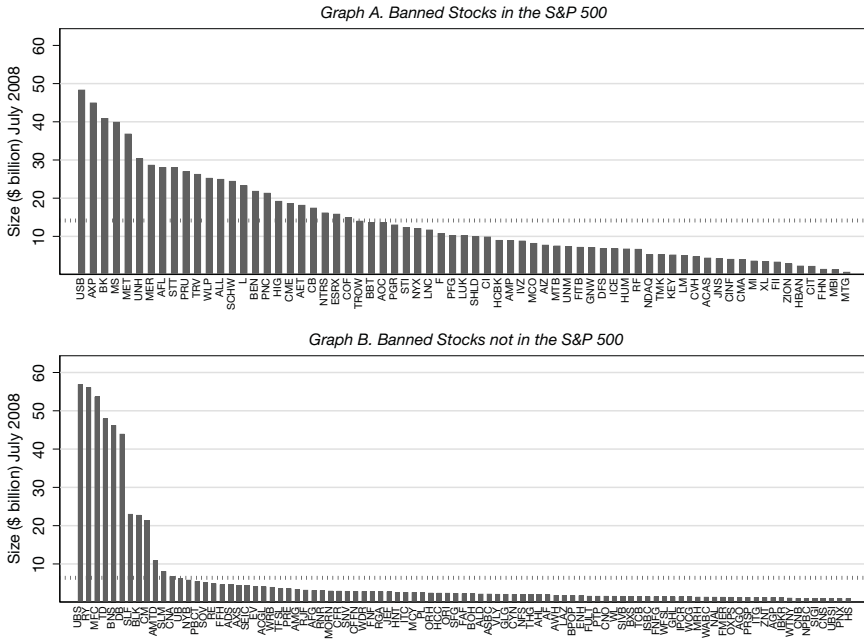
	Period	S&P 500			Non-S&P 500			
		Banned	Nonbanned	Δ	Banned	Nonbanned	Δ	2Δ
<i>Panel A. Firm Size Summary Statistics</i>								
N		66	344		110	877		
Size (\$ billion)								
Mean	July 8	13.79	13.12	0.67	3.30	3.00	0.30	
Min	July 8	0.72	0.71		0.74	0.70		
Median	July 8	10.27	9.09		1.86	1.70		
Max	July 8	45.04	45.38		43.99	44.98		
<i>Panel B. Liquidity Measures Summary Statistics</i>								
Quoted spreads	Pre-ban	7.07	5.96		13.68	12.23		
	Ban	15.33	8.34		27.27	17.33		
	Δ	8.26	2.38	5.88	13.59	5.10	8.49	2.61
Effective spreads	Pre-ban	8.45	6.76		12.16	11.42		
	Ban	20.60	10.41		30.04	17.51		
	Δ	12.15	3.65	8.50	17.88	6.09	11.79	3.29
Price impact	Pre-ban	3.83	2.74		4.24	4.19		
	Ban	10.43	4.93		11.89	6.47		
	Δ	6.60	2.19	4.41	7.65	2.28	5.37	0.96
Realized spreads	Pre-ban	4.55	3.87		7.82	7.16		
	Ban	10.05	5.40		17.64	10.68		
	Δ	5.50	1.53	3.97	9.82	3.52	6.30	2.33

firm of a given size in the group of banned S&P 500 member stocks, it is possible to find at least one firm of comparable size in the group of banned, nonmember stocks. In fact, a few firms in the group of banned nonmembers were subsequently included in the S&P 500 index. For example, NASDAQ (ticker symbol NDAQ) and Black-Rock (ticker symbol BLK) were added to the S&P 500 index on Oct. 22, 2008 and Apr. 1, 2011, respectively. It is worth noting that in Boehmer et al. (2013) analysis of subsamples of the banned stocks of different sizes, most, if not all, of the banned stocks in our sample would have been assigned to their “largest-quartile” group of stocks.³⁸ Nevertheless, there is a higher prevalence of larger firms in the group of banned S&P 500 member stocks. Unfortunately, a limitation of analyzing members

³⁸Boehmer et al. (2013) report that they have 182 stocks in their largest quartile of stock, whereas our sample contains 176 large stocks (we have dropped 6 of the largest stocks). The reported median sizes of their third quartile (second-largest quartile) of stocks are only \$0.481 billion. In contrast, the smallest banned firm in our sample size is \$0.7 billion, which makes it unlikely that many, if any, of the firms in our sample would be assigned to their third quartile. Although we do not know the exact firms in their sample, given these statistics and the fact that we follow a similar sample construction procedure, it is reasonable to assume that most of the banned stocks in our sample would be in their largest-quartile subsample.

FIGURE 4
Market Capitalization of Stocks Banned from Short Selling

Figure 4 displays market capitalization of our sample of firms for stocks banned from short selling during the 2008 short-sale ban. The sample is split into stocks that are members of the S&P 500 index and other stocks. Each stock's ticker symbol is shown on the horizontal axis. Market capitalization is an average market capitalization over July 2008 expressed in billion dollars. The horizontal dotted line in each figure indicates the cross-sectional average of each subsample.



of the S&P 500 index is the inability to find a perfect control group. However, as we discuss in the next subsections, we diminish this concern by using an identification strategy that does not rely on the treatment and control groups having the same ex ante characteristics, and we control for firm size in all of our formal empirical tests.

2. Liquidity Measures

Similar to Boehmer et al. (2013), for each stock and day in our sample, we calculate 4 standard measures of liquidity: the quoted spread, effective spread, realized spread, and price impact. When calculating these measures, we follow the procedure of Holden and Jacobsen (2014), which has been shown to deliver more precise estimates of liquidity by appropriately accounting for withdrawn quotes and economically irrational states during computation. The 4 liquidity measures are defined as follows:

$$(3) \quad \text{QUOTED_SPREAD}_s = (\text{ASK}_s - \text{BID}_s) / \text{MID}_s,$$

$$(4) \quad \text{EFFECTIVE_SPREAD}_k = 2 \times D_k \times (P_k - \text{MID}_k) / \text{MID}_k,$$

$$(5) \quad \text{REALIZED_SPREAD}_k = 2 \times D_k \times (P_k - \text{MID}_{k+5}) / \text{MID}_k,$$

$$(6) \quad \text{PRICE_IMPACT}_k = D_k \times (\text{MID}_{k+5} - \text{MID}_k) / \text{MID}_k,$$

where ASK_s and BID_s are the National Best Ask and Best Bid assigned to interval s and MID_s is the midpoint, which is the average between ASK_s and BID_s . MID_{k+5} is a midpoint price 5 minutes after trade k . P_k denotes the stock price at trade k . D_k takes a value of 1 if trade k is categorized as buyer-initiated and the value of -1 if it is categorized as being seller-initiated. We categorize trades using the Lee and Ready (1991) algorithm. We aggregate each measure to the daily level. For each stock on each day, the averages are time-weighted for quoted spreads and dollar-volume-weighted across all the trades for effective spreads, realized spreads, and price impact. After calculating the measures, we winsorize each one at the 1st and the 99th percentile to limit the effect of outliers.³⁹ For ease of exposition, we express the liquidity measures in basis points. It is worth noting that each of our liquidity measures is a measure of illiquidity; hence, an increase signifies a deterioration of liquidity.

In Panel B of Table 4, we present the averages of the 4 liquidity measures for the period before the imposition of the ban (Aug. 1 to Sept. 18) and the ban period (Sept. 19 to Oct. 8). These statistics show that there was a universal deterioration of stock market liquidity during the ban, which makes sense given the market dislocation around that time. In addition, in line with the results of the existing studies, we clearly see that the banned stocks' liquidity deteriorates more severely. For example, the average quoted spread of a banned S&P 500 stock increased by 8.26 basis points during the ban from its pre-ban average level of 7.07 basis points. In comparison, the average change in the quoted spread during the same period for a nonbanned S&P 500 firm of a similar size and with similar pre-ban liquidity was only 2.38 basis points.

Importantly, we observe another dimension of the effect that has not been discussed in the existing literature. Irrespective of the liquidity measure we consider, it appears that the liquidity of the banned stocks that were not members of the S&P 500 index deteriorated more than the liquidity of the banned S&P 500 member stocks. For example, the average quoted spread of the banned nonmember firms increased by 13.59 basis points during the ban from its average level of 13.68 basis points before the ban. Nonmember firms are, on average, smaller with slightly lower stock liquidity. Hence, it is possible that this differential effect could be driven by greater exposure to the aggregate liquidity shocks. We observe a relatively stronger deterioration in the nonbanned, nonmember stocks' liquidity than that of the nonbanned S&P 500 member stocks. For example, during the ban, the average quoted spread of the nonbanned, nonmember stocks increased by 5.10 basis points, compared with the 2.38 basis point increase for the nonbanned member stocks. However, adjusting for the broad market effects by subtracting

³⁹The bulk of the outliers are found on Sept. 19, 2008, which was a unique day, because it was the first day of the ban and a "triple witching" day (a day occurring once a quarter, when stock options, stock index futures, and stock index option contracts expire simultaneously). Our main results are robust to alternative winsorizing procedures, including not winsorizing the data.

the average change during the ban in the liquidity of the nonbanned shares from the average change in liquidity of the banned stocks appears to preserve the difference in the liquidity deterioration between the banned S&P 500 member stocks and the banned nonmember stocks (column 2Δ of Table 4). These statistics suggest that the banned S&P 500 member stocks (i.e., the Spider's constituent stocks) experienced a less severe deterioration in liquidity during the ban than the banned stocks not in the Spider's portfolio. We examine these patterns formally in the next subsection.

B. Identification Strategy and Empirical Methodology

We employ the triple difference methodology to formally evaluate the average effect of a SPY membership on banned stocks' liquidity during the short-sale ban. In particular, we estimate the following panel regression:

$$(7) \quad y_{i,t} = \beta_1 \text{BAN}_t + \beta_2 \text{BAN}_t \times \text{BANNED}_i + \beta_3 \text{BAN}_t \times \text{BANNED}_i \times \text{SP}_i \\ + \beta_4 \text{BAN}_t \times \text{SP}_i + C' X_{i,t} + \alpha_i + \alpha_t + \varepsilon_{i,t},$$

where the dependent variable, $y_{i,t}$, is one of the 4 liquidity measures defined in the previous section (the 5-minute price impact, quoted spreads, effective spreads, or 5-minute realized spreads) for each stock i on each day t . BAN_t is an indicator variable that takes the value of 1 on the days of the 2008 short-sale ban, and 0 otherwise. BANNED_i and SP_i are indicator variables that take the value of 1 for the banned stocks and S&P 500 index members (as of Sept. 18, 2008) respectively, and 0 otherwise.⁴⁰ $X_{i,t}$ is a vector of controls that includes daily turnover (the ratio of daily trading volume to its shares outstanding) and market capitalization. Given that liquidity levels differ among stocks, regression 7 includes the time-invariant stock fixed effect, α_i , which absorbs the time-invariant differences in liquidity among stocks.⁴¹ Finally, we include the stock-invariant time fixed effect, α_t , which absorbs market-wide liquidity shocks. We conduct all of our statistical inference using the standard errors clustered by individual stock and time.

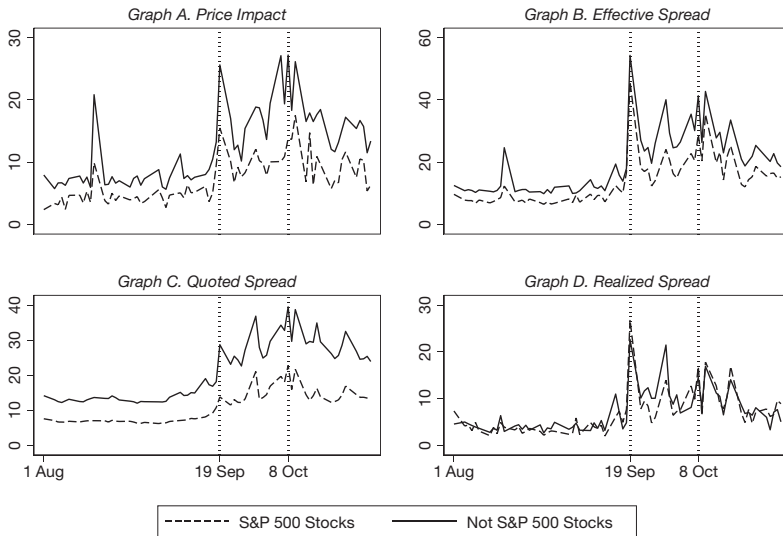
Regression 7 can be interpreted from the difference-in-difference perspective. The first difference compares the average liquidity between the short-sale ban period and the other periods. The coefficient β_1 captures that effect. The second difference compares the average change in liquidity of the banned stocks to the average change in the nonbanned stocks' liquidity during the ban period. This differential effect of the short-sale ban on the banned stocks is the focus of the Boehmer et al. (2013) study and is captured by the coefficient β_2 . Estimated on a sample containing both the banned and nonbanned stocks β_1 captures the effect of market-wide liquidity shocks during the ban on stock liquidity, whereas β_2 estimates the effect of the short-sale constraints. Our specification adds an additional

⁴⁰The BANNED_i indicator does not differentiate between stocks on the initial list and the stocks that were banned on Monday, Sept. 23, 2008. Restricting our analysis to only the stocks on the initial ban list does not materially alter the empirical results.

⁴¹Due to the inclusion of firm fixed effects, we are unable to identify the coefficients on BANNED_i and SP_i indicators; thus, we omitted them from regression 7.

FIGURE 5
Parallel Trends in the Liquidity Measures

Figure 5 displays the time series of the daily averages of 4 liquidity measures for the banned stocks. The 4 liquidity measures are price impact, quoted spread, effective spread, and realized spread. All the measures are expressed in basis points. The sample is split into stocks that are members of the S&P 500 index and other stocks, then the equal-weighted average daily liquidity is calculated for each group of stocks. The 2 vertical lines indicate the short-sale ban period. The data are daily. The sample period is from Aug. 1, 2008 to Oct. 31, 2008.



layer – the difference between S&P 500 member and nonmember stocks. We focus on the effect of the ban on the change in liquidity of the banned S&P 500 member stocks captured by the coefficient β_3 .

To be able to estimate β_3 consistently, the parallel trends assumption needs to hold. In particular, we require that any trends in the liquidity measures for the banned S&P 500 members and the banned nonmembers to be the same before the ban. Figure 5 plots the time series of the daily averages of the 4 liquidity measures for the banned S&P member and nonmember stocks. Visually, the evolution of the liquidity of the 2 groups of stocks appears to be similar before the imposition of the ban. Hence, the parallel trend assumptions seem reasonable in this case.

However, in our case, this may not be sufficient to identify the S&P 500 member effect. As we have discussed in the previous subsection, the effect of a firm's size is an important concern, because S&P 500 member stocks tend to be larger and could respond differently to liquidity shocks. Thus, similar to regression 2, we include as controls an indicator $LARGE_i$ which is equal to 1 if the market capitalization of stock i in July 2008 is larger than the S&P 500 median (and 0 otherwise), and its interactions with the BAN_t and $BANNED_t$ indicators. We also include the logarithmic value of each stock's daily market capitalization and daily turnover.

In sum, given that the parallel trends assumption appears to hold and we adequately control for firm characteristics, we interpret the β_3 as the effect of the S&P 500 membership.

C. Liquidity Regression Results

In this section, we discuss the results of regression 7, focusing our discussion, for the reasons described below, on the price impact measure of illiquidity. Liquidity can be viewed as a cost of a hypothetical round trip trade, in which an agent buys and sells at the current offer and bid price, respectively. This cost, the effective spread, is the sum of two main liquidity components: realized spread and price impact. The realized spread measures inventory and order processing costs, whereas the price impact relates to the level of information asymmetry (see, e.g., Holden et al. (2014)). Short-sale constraints negatively affect private information diffusion in the markets (Diamond and Verrecchia (1987)). Hence, price impact, a measure of information asymmetry, should increase when the short-sale constraints bind, and the alleviation of these constraints should reflect primarily in price impact. In contrast, it is not obvious as to why alleviation of short-selling constraints would affect inventory and trading costs; thus, one should not expect realized spreads to be significantly affected.

Table 5 reports the results. We begin with a benchmark difference-in-difference specification, estimated on the subsample of banned stocks, which includes only the stock fixed effects and the $BAN_i \times BANNED_i$ and $BAN_i \times BANNED_i \times SP_i$ indicators. In line with the finding of the existing literature, the results show that the ban led to a strong deterioration in liquidity of the affected stocks. Importantly, we observe that the deterioration in liquidity was significantly lower for the S&P 500 members, that is, the coefficient β_3 is negative and statistically significant (column 1). The quotient of β_3 divided by β_2 can be interpreted as the economic impact of S&P 500 membership mitigating effect. In the benchmark case, we find that S&P 500 member firms experienced 47% less severe liquidity deterioration than the nonmembers.

However, the benchmark specification does not account for the effect of firm size on liquidity changes. Thus, we also include the interaction of the indicator $LARGE_i$ with the $BAN \times BANNED$ indicator and reestimate the regression. The inclusion of the size controls preserves both the sign and the statistical significance of the β_2 and β_3 coefficients. It also leads to an increase in the point estimate of the β_2 coefficient resulting in more intuitive estimate of the economic effect. The results indicate that the ban led, on average, to a 7.6 basis point deterioration in the price impact among the banned stocks, with the banned S&P 500 stocks experiencing 3.1 basis point lower deterioration in price impact (column 2 of Table 5). This effect amounts to a 41% less severe liquidity deterioration.

Next, we estimate regression 7 on the full sample that includes both the banned and nonbanned stocks. The triple difference specification corrects for the effect of the aggregate deterioration in liquidity during the ban. The absolute value of the point estimate of the β_3 coefficient decreases, but remains negative and statistically significant (column 3 of Table 5). Including additional controls (daily turnover and market capitalization), and time fixed effects, has little impact on the results (column 4). The full specification shows that, on average, the liquidity deterioration, as measured by price impact, of the S&P 500 banned firms during the ban was around 36% relatively less severe—an economically meaningful difference.

TABLE 5
The Effect of S&P 500 Membership on Stock Liquidity Around the Short-Sale Ban

Table 5 reports the estimates of the difference-in-difference-in-difference ordinary least squares regressions of the form:

$$y_{i,t} = \beta_1 \text{BAN}_t + \beta_2 \text{BAN}_t \times \text{BANNED}_t + \beta_3 \text{BAN}_t \times \text{BANNED}_t \times \text{SP}_t + \beta_4 \text{BAN}_t \times \text{SP}_t + C' X_{i,t} + \alpha_i + \alpha_t + \epsilon_{i,t}.$$

The dependent variables are 4 liquidity measures: the 5-minute price impact, quoted spreads, effective spreads, and 5-minute realized spreads. For each stock, the 4 liquidity measures are computed using intraday trade and quote data, and are aggregated to the daily level (quoted spreads are time-weighted, and effective spreads, price impact, and realized spreads are trade-weighted). Each measure is proportional to the prevailing quote midpoint and is expressed in basis points. BAN is an indicator variable that takes the value of 1 on the 2008 short-sale ban days. BANNED and SP are indicator variables that take the value of 1 for the banned stocks and S&P 500 index member (as of Sept. 18, 2008) stocks, respectively. All of the specifications include time-invariant stock fixed effects, α_i . The specifications in columns 4, 6, 8, and 10 include time fixed effects, α_t . The regressions are estimated first on a subsample of banned stocks, and then on a full sample that includes banned and nonbanned stocks. The vector of controls, $X_{i,t}$, includes an interaction of LARGE (indicator variable taking the value of 1 if a firm's July 2008 average market capitalization is above the S&P 500 sample median) with the BAN and BANNED indicators (BAN \times LARGE and BAN \times BANNED \times LARGE), the logarithmic value of a stock's daily market capitalization (SIZE), and the ratio of each stock's daily trading volume to its shares outstanding (TURNOVER, in percent). The *S&P 500 effect* is the coefficient β_3 divided by the coefficient β_2 . The standard errors are clustered at stock and time level. The data are daily, and the sample period is from Aug. 1, 2008 to Oct. 31, 2008. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Price Impact				Effective Spread		Realized Spread		Quoted Spread	
	1	2	3	4	5	6	7	8	9	10
BAN \times BANNED	7.534*** (1.607)	7.579*** (1.618)	6.448*** (1.118)	6.907*** (1.141)	14.230*** (2.695)	12.380*** (1.129)	6.495*** (1.317)	5.504*** (0.873)	10.200*** (1.787)	9.276*** (0.800)
BAN \times BANNED \times SP	-3.563*** (1.170)	-3.089** (1.312)	-2.446** (1.089)	-2.452** (1.143)	-4.271*** (1.080)	-3.238*** (1.147)	-0.678 (1.276)	-0.511 (1.234)	-3.684*** (1.081)	-3.431*** (1.013)
BAN			1.131 (1.452)							
BAN \times SP			-0.643 (0.527)	-0.622 (0.491)		-0.827 (0.556)		0.0229 (0.322)		-0.378 (0.572)
BAN \times LARGE			-0.337 (0.430)	-0.412 (0.437)		-0.235 (0.440)		0.324 (0.264)		-0.351 (0.342)
BAN \times BANNED \times LARGE		-0.964 (1.059)	-0.627 (0.989)	-0.757 (0.988)	-1.710* (0.939)	-1.963* (1.134)	-1.433 (1.135)	-2.033* (1.178)	-1.263 (1.171)	-1.008 (1.178)
SIZE				-7.087*** (0.685)		-10.64*** (0.838)		-3.306*** (0.523)		-8.163*** (0.592)
TURNOVER				-0.136* (0.0691)		0.240*** (0.0861)		0.381*** (0.0500)		-0.505*** (0.0599)
No. of obs.	11,245	11,245	89,959	89,959	11,245	89,959	11,245	89,959	11,245	89,959
Adj. R^2	0.185	0.185	0.156	0.278	0.355	0.590	0.074	0.108	0.556	0.837
Stock fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effect	No	No	No	Yes	No	Yes	No	Yes	No	Yes
Sample	Banned	Banned	All	All	Banned	All	Banned	All	Banned	All
S&P 500 effect	-47.30%	-40.76%	-37.94%	-35.50%	-30.02%	-26.15%	-10.45%	-9.28%	-36.13%	-36.99%

Repeating the analysis on the effective spreads (columns 5 and 6 of Table 5) and realized spreads (columns 7 and 8) liquidity measures, we find results that are in line with the theoretical predictions. We find that although, on average, the ban led to a 12.4 basis point increase in the effective spreads of the banned stocks, this increase was 3.2 basis points less for the S&P 500 members. The difference is statistically significant and amounts to a 26% milder liquidity deterioration for the S&P 500 members. In contrast, we find a negative, but statistically insignificant, effect of S&P 500 membership on the realized spread measure. This result is consistent with the theoretical predictions that the effect of short-selling constraints should be reflected in liquidity measures capturing information asymmetry, such as price impact. In addition, examining quoted spreads, a more generic liquidity measure that does not take into account trade direction corroborates our main results (columns 9 and 10).

Our results are consistent with those of Boehmer et al. (2013) who document a significant adverse effect of the ban on all 4 measures of liquidity. As we mentioned earlier, the coefficient β_2 on the interaction variable $BAN_i \times BANNED_i$ measuring the effect of the ban on the banned stocks is positive (indicating deterioration of liquidity) and strongly statistically significant in all of our specifications. The point estimates are also similar to those found by Boehmer et al. (2013); however, ours are typically slightly more conservative.⁴² Although Boehmer et al. (2013) examine cross-sectional differences between stocks of different size quartiles, we differ from Boehmer et al. (2013) in that we explore additional cross-sectional differences among the largest stocks. In particular, we ask whether the S&P 500 members experience a differential liquidity impact of the ban relative to other similar, large stocks. We find that even among the sample of the largest stocks, there are additional, significant differences between member and nonmember stocks.

In sum, given our finding that the Spider was heavily shorted during the ban period and that Spider's constituents experienced a less severe liquidity deterioration during the ban (particularly in liquidity measures related to information asymmetry), the results suggest that the ETF short sales could have driven some of the liquidity improvement.

D. Do ETF Short Sales Improve Liquidity of Its Underlying Stocks?

In the previous subsection, we show that the Spider's constituents experienced a relatively less severe liquidity deterioration during the ban. We posit that the short sales of the Spider drove at least some of this effect by alleviating the regulatory short-sale constraints. However, we are unable to show this link empirically. In this subsection, we take a step further in connecting the Spider's short sales during the ban with the liquidity of its underlying stocks. We estimate the following regression:

⁴²For example, Boehmer et al. (2013) find that an average differential adverse effect on banned stocks (in the largest quartile) is 15 basis points for the price impact measure, whereas we find the corresponding effect to be around 7 basis points. Our estimates are likely to be more conservative due to i) slight differences in the samples, ii) different empirical specification and controls, and iii) winsorizing.

$$(8) \quad y_{i,t} = \beta_1 \text{SI}_{\text{SPY},t} + \beta_2 \text{SI}_{\text{SPY},t} \times \text{BANNED}_i + \beta_3 \text{SI}_{\text{SPY},t} \times \text{BANNED}_i \times \text{SP}_i \\ + \beta_4 \text{SI}_{\text{SPY},t} \times \text{SP}_i + \text{CX}_{i,t} + \alpha_i + \alpha_t + \varepsilon_{i,t},$$

where the dependent variable, $y_{i,t}$, is one of the 4 liquidity measures for each stock i on each day t . $\text{SI}_{\text{SPY},t}$ denotes the number of SPY shares on loan on day t scaled by its total number of shares outstanding on Sept. 18, 2008 (identical to $\text{SHORT_INTEREST}_{i,t}$ in regression 1 for $i = \text{SPY}$). BANNED_i and SP_i , as before, are indicator variables that take the value of 1 for the banned stocks and S&P 500 index members, respectively. $\text{X}_{i,t}$ is a vector of controls as in regression 7. α_i and α_t are the stock and time fixed effects, respectively.

The sole difference between regressions 7 and 8 is that the BAN_i indicator is replaced by a continuous measure of the Spider's short interest, $\text{SI}_{\text{SPY},t}$. We observe in Figure 2 that the short sales of the Spider were relatively stable before the ban and increased rapidly during the ban; however, the increase was not uniform during the ban period. Regression 8 exploits the timing of the increase in Spider short sales during the ban to identify the link between Spider short sales during the ban and the liquidity of its constituents. As before, β_3 is the coefficient of interest, because it captures the differential effect of the increase in Spider short sales on its constituents.

The results show that, during our sample period, an increase in the Spider's short interest is correlated with a deterioration of liquidity of the banned stocks (Table 6). The coefficient β_2 on $\text{SI}_{\text{SPY},t} \times \text{BANNED}_i$ is positive and significant. This finding is, however, merely an outcome of the relatively high correlation of 0.57 between the Spider's short interest and the Ban indicator during our sample period; hence, we interpret it as such. Our focus is on the coefficient β_3 on $\text{SI}_{\text{SPY},t} \times \text{BANNED}_i \times \text{SP}_i$, which is negative and statistically significant for the price impact, effect spreads, and quoted spread liquidity measures. Moreover, the magnitude of the effect is similar to that obtained from regression 7 (the detrimental effect of the ban on price impact is around 25% less severe for the constituents of the Spider). This finding corroborates the results of the previous subsection and indicates that higher levels of short sales of the Spider are associated with improved relative liquidity for the S&P 500 firms during our sample period.

We note that our analysis cannot establish a causal link between the short sales of the Spider and differential change in the liquidity of its constituents. Nevertheless, the robustness of our results to this alternative specification further supports this explanation.

VI. Conclusion

ETFs have proved to be one of the most successful financial innovations of recent history. Since the introduction of the first ETF in the early '90s, the global ETF market has grown to the formidable size of over \$6 trillion. Moreover, ETF trading currently accounts for over 30% of the dollar volume on U.S. exchanges.⁴³

⁴³See "ETFs are eating the US stock market" by Robin Wigglesworth, *Financial Times*, Jan. 24, 2017.

TABLE 6
Stock Liquidity and SPY ETF Short Sales

Table 6 reports the estimates of the ordinary least squares regressions of the form:

$$Y_{i,t} = \beta_1 SI_{SPY,t} + \beta_2 SISP_{Y,t} \times BANNED_i + \beta_3 SISP_{Y,t} \times BANNED_i \times SP_i + \beta_4 SISP_{Y,t} \times SP_i + CX_{i,t} + \varepsilon_{i,t}$$

The dependent variables are 4 liquidity measures: the 5-minute price impact, quoted spreads, effective spreads, and 5-minute realized spreads (see Section V.A.2 for the definitions). For each stock, the 4 liquidity measures are computed using intraday trade and quote data, and aggregated to the daily level (quoted spreads are time-weighted, and effective spreads, price impact, and realized spreads are trade-weighted). Each measure is proportional to the prevailing quote midpoint and is expressed in basis points. $SI_{SPY,t}$ is the daily short interest of the S&P Depository Receipt S&P 500 (SPY) exchange traded fund (ETF), defined as its total number of shares on loan on each day divided by its total number of shares outstanding on Sept. 18, 2008, and it is expressed in percent. BANNED and SP are indicator variables that take the value of 1 for the banned stocks and S&P 500 index member (as of Sept. 18, 2008) stocks, respectively. All the specifications include time-invariant stock fixed effects. The specifications 2, 4, 6, and 8 include time fixed effects. The regressions are estimated first on a subsample of banned stocks and then on a full sample that includes banned and nonbanned stocks. The vector of controls, $CX_{i,t}$, includes an interaction of the indicator variable taking value of 1 if firm's average July 2008 market capitalization is above the S&P 500 sample median LARGE with the SI and BANNED indicators ($SI \times LARGE$ and $SI \times BANNED \times LARGE$), the logarithmic value of a stock's daily market capitalization (SIZE), and the ratio of each stock's daily trading volume to its shares outstanding (TURNOVER, in percent). The standard errors are clustered at stock and time level. The data are daily, and the sample period is from Aug. 1, 2008 to Oct. 31, 2008. ***, **, * and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Price Impact		Effective Spreads		Realized Spreads		Quoted Spreads	
	1	2	3	4	5	6	7	8
SI × BANNED	1.62*** (0.24)	1.15*** (0.16)	2.81*** (0.37)	2.07*** (0.20)	1.19*** (0.20)	0.93*** (0.15)	2.53*** (0.31)	1.63*** (0.14)
SI × BANNED × SP	-0.52** (0.20)	-0.29* (0.17)	-0.75*** (0.19)	-0.45** (0.18)	-0.13 (0.15)	-0.08 (0.16)	-1.02*** (0.18)	-0.65*** (0.17)
SI × SP		-0.28*** (0.08)		-0.35*** (0.09)		-0.06 (0.04)		-0.43*** (0.12)
SI × LARGE		-0.12** (0.06)		-0.13** (0.06)		0.00 (0.04)		-0.27*** (0.07)
SI × BANNED × LARGE	-0.29* (0.16)	-0.21 (0.15)	-0.34** (0.13)	-0.31** (0.15)	-0.18 (0.13)	-0.24 (0.15)	-0.32* (0.18)	-0.08 (0.18)
SIZE		-7.31*** (0.68)		-11.09*** (0.82)		-3.55*** (0.52)		-8.48*** (0.55)
TURNOVER		-0.13* (0.07)		0.25*** (0.09)		0.38*** (0.05)		-0.49*** (0.06)
No. of obs.	11,245	89,959	11,245	89,959	11,245	89,959	11,245	89,959
Adj. R ²	0.21	0.28	0.42	0.59	0.08	0.11	0.68	0.84
Stock fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effect	No	Yes	No	Yes	No	Yes	No	Yes
Sample	Banned	All	Banned	All	Banned	All	Banned	All
S&P 500 effect	-32.16%	-25.13%	-26.58%	-21.96%	-10.94%	-8.72%	-40.46%	-40.18%

The increasing importance of ETFs has sparked the growth of research on the subject; however, some gaps in our understanding remain. In particular, there are relatively few studies examining ETF short sales, which is surprising given the prevalence of ETF short sales in practice. We contribute to filling this gap in the literature.

Using the setting of the 2008 short-sale ban, we examine the short selling of equity ETFs and find a substantial increase in short sales of the Spider during the ban period. Although we are unable to pinpoint the exact proportion of these short sales as directional shorts targeting the banned stocks, we present evidence suggesting that many were likely speculative short sales aimed at circumventing the ban. We also find a concurrent increase in the supply of the Spider's shares outstanding, which would have allowed for an even greater short-sale order flow migration. Importantly, we show that the detrimental effect of regulatory short-sale constraints on stock liquidity was around 30% less severe for the Spider's constituents. As a whole, our findings imply that ETF short sales can substitute for directional short

sales of individual stocks, thereby alleviating the adverse effects of regulatory short-sale constraints on stock liquidity.

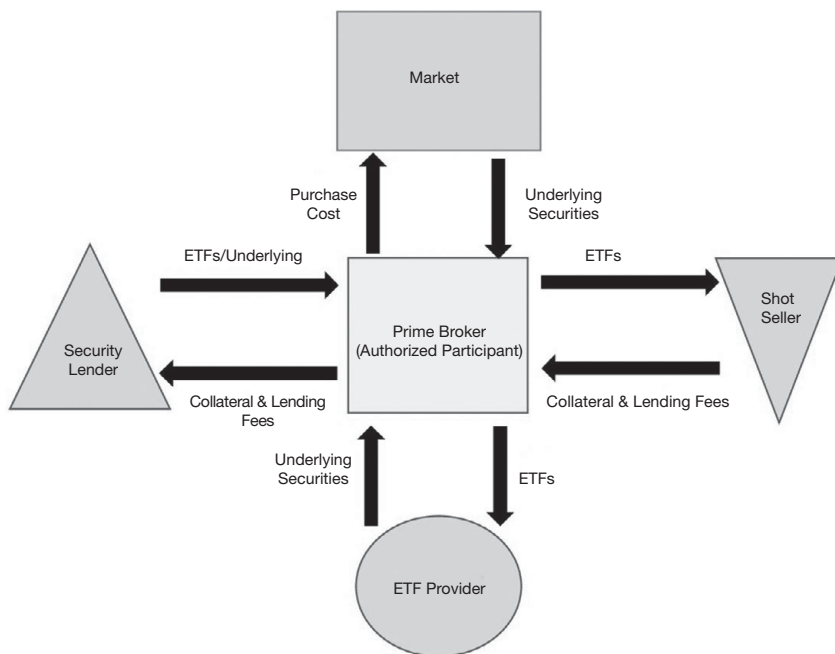
However, our study has a few limitations. First, we focus on a period that is characterized by very high volatility; thus, our results may not easily generalize to other settings. Second, we are unable to identify what proportion of the ETF short sales that we observe were executed with the sole purpose of bypassing the ban. In order to do so, one would require complete trader-level portfolio data, to which only the financial intermediaries are likely to have access. Given the sensitive nature of such data, particularly for the ban period, the data are unlikely to be made available. Future work could examine ETF short selling in different settings to better understand its impact on market quality, and researchers with access to granular positions data could shed more light on the incentives for speculative ETF short sales.

Despite the limitations, our results have direct implications for policymakers wishing to regulate short selling. Given the global proliferation of ETFs and their ability to be used for speculative short sales, any future regulation would require a more elaborate design to be able to both restrict short selling of the underlying assets, and not disrupt the workings of the ETF market.

Appendix. The “Create-to-Lend” Process

In a standard short-sale transaction, a short seller (typically a hedge fund) approaches a prime broker (typically a large investment bank) in order to borrow a security for shorting. The prime broker sources this security from a lender, such as a large institutional investor, in exchange for collateral and a fee, which is then passed onto the short seller. If the security demanded by the short seller is an ETF, the mechanics of the transaction could be the same, with the exception that if the actual ETF is hard to borrow (e.g., due to low institutional ownership), the prime broker can borrow (or purchase) the underlying securities, and pass them onto the ETF provider who, in turn, creates additional ETF shares and issues the prime broker with new ETF shares that can be given to the short seller. [Figure A1](#) presents a diagrammatic representation of this mechanism.

FIGURE A1
Diagrammatic Representation of the “Create-to-Lend” Process



Supplementary Material

To view supplementary material for this article, please visit <http://doi.org/10.1017/S0022109021000181>.

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