Fixed Income ETFs, Bond Liquidity, and Stressed Markets

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Abstract

This paper examines the impact of Fixed Income Exchange-Traded Funds (ETFs) on corporate bond liquidity. I find that corporate bonds ETFs decrease the transaction costs of their constituent securities. The use of two distinct quasi-natural experiments, that control for the identification issues of self-selection and of an index effect, establishes a causal relation between ETF ownership and bond liquidity. Moreover, ETFs do not deteriorate the liquidity of their bonds during ETF arbitrage and market stress events. The ETF trading volume appears to be the transmission mechanism driving the results.

JEL codes: G12, G14, G15.

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Internet appendix

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1 Introduction

Since their introduction in 2002, Fixed Income Exchange-Traded Funds (ETFs) have experienced sustained growth in assets under management (AuM) and trading volume, particularly in the U.S. Owing to their benefits as inexpensive diversified investment vehicles with intraday liquidity, ETFs are now directly used by both institutional and individual investors. As a result of this success, ETFs have become a close substitute to the securities they track.

The growth of ETFs is a rising concern for regulators such as the Financial Stability Board and the U.S. Securities and Exchange Commission (SEC, see Strench (2016)), which recalls the classic question of the impact of an investment vehicle on its underlying securities. For instance, the *informational efficiency* hypothesis argues that a new investment vehicle improves price informativeness and liquidity. The market becomes more complete, which facilitates risk reallocation and increases both arbitrage opportunities and the incorporation of information into prices (Danthine (1978), Turnovsky (1983), Grossman (1988), Fremault (1991), and Kumar and Seppi (1994)). In contrast, the alternative hypothesis enunciates that a new investment vehicle deteriorates liquidity on the underlying securities due to the *adverse selection* induced by arbitrage activities (Gammill and Perold (1989), Subrahmanyam (1991), Gorton and Pennacchi (1993), Dow (1998), and Foucault, Kozhan, and Tham (2017)).

There is a suspicion that the impact can be more problematic in the case of ETFs and especially corporate bond ETFs. This is because the correct valuation of ETFs is based on the arbitrage of ETFs with their underlying securities. The arbitrage can take place daily as through a physical exchange via the primary market. The existence of a primary market is a distinct feature of ETFs compared with other investment vehicles. Furthermore, in the case of corporate bond ETFs, the underlying securities are often illiquid bonds. The size and the growth of corporate bond ETFs, combined with the active trading of their underlying securities, raise the question of the potential effects of liquid and highly exchanged corporate bond ETFs on an arguably fragile and illiquid bond market. These concerns have triggered the SEC to create a Subcommittee focusing on the effects of Fixed Income ETFs especially during stressed markets.[1] Corporate bond ETFs thus form an attractive candidate to test the conflicting theories on the effect of an investment vehicle on the liquidity of its underlying securities, particularly on days of market stress.

The main contribution of this paper is to show that Fixed Income ETFs increase the liquidity of their corporate bonds even during stressed market conditions and periods of ETF arbitrage. The result that Fixed Income ETFs increase the liquidity of their corporate bonds is also shared in two contemporaneous papers by Holden and Nam (2019) and Ye (2019). I differ in four important ways. First, I use recent novel quasi-natural experiments, occurring more than a decade after the introduction of corporate bond ETFs.[2] Hence, I study the corporate bond ETFs in a mature state. The assets of U.S. corporate bond ETFs increased from \$10 billion in 2009 to \$317 billion in 2020. This growth was accompanied by a similar increase in their trading volume and turnover. I hereby proceed to test the effects of corporate bond ETFs after they have effectively formed a viable substitute for the assets they track. Second, the novel identification strategies presented in this paper extend previous studies by addressing the important issues of (i) the self-selection of liquid securities by ETFs (see Section 5) and (ii) the distinction between the ETF effect and the index effect (see Section 6). Third, following regulators' concerns, I pay special attention to the study of market stress events. Contrary to Saglam, Tuzun, and Wermers (2019) findings for equity ETFs, I find that corporate bond ETFs have positive impact on the liquidity of their underlying securities even during sell-offs and existing market stress events. Due to the cost of trading bonds, arbitrage for corporate bond ETFs is limited both in frequency and magnitude. From April 2010 to April 2020, on average only 15% of the daily trading volume

[1] The SEC committee is "charged with assessing the consequences of the increased presence of Fixed Income mutual funds and ETFs in these markets, including their current and possible future impacts on the liquidity and pricing of the underlying bonds under a variety of scenarios [...] topics that may be considered include the interaction of Fixed Income prices and fund prices, including through the ETF arbitrage process, an assessment of the impact of redemptions from funds in stressed conditions, an assessment of the potential impact of the rebalancing process on underlying markets" Report on the Design of Exchange-Traded Funds and Bond Funds – Implications for Fund Investors and Underlying Security Markets Under Stressful Conditions (2019), report dated April 10, 2019, p.1.

[2] Holden and Nam (2019) quasi-natural experiment takes place in 2009 for the High Yield ETF HYG, two years after its introduction.

in corporate bond ETFs leads to arbitrage using the primary market. Fourth, I highlight a new transmission mechanism. The improvement in bond liquidity is linked to the trading volume of the ETFs holding them.

The empirical analyses presented in this paper start with panel regressions of bond-level liquidity on corporate bond ETF ownership for the period April 2010 to April 2020. For all liquidity measures, I find a positive relation between ETF ownership and bond liquidity for both investment grade and high yield bonds. To account for the various dimensions of liquidity, I compute multiple standard monthly bond illiquidity measures such as price impact, bid-ask spread, price dispersion, and trading frequency. All measures of bond illiquidity exhibit a negative and significant relation with ETF ownership. Thus, my results indicate a positive link between ETF ownership and liquidity.

When it matters, liquidity tends to elude the ones who seek it (see Brunnermeier and Pedersen (2009) and Pastor and Stambaugh (2019)). To investigate the impact of ETFs on bond liquidity during stressed markets, I rerun previous regressions on a subsample restricted to periods of market stress: the July 2011 debt ceiling crisis, the 2013 taper tantrum, the 2016 election, and the 2020 pandemic. Even during these periods, I confirm the positive association between ETF ownership and bond liquidity.

Additionally, I find that contrary to the predictions of *adverse selection*, the relation between ETF ownership and bond liquidity remains positive even when ETF market makers actively trade the underlying bonds. In particular, the main result is supported on periods with large ETF redemptions.

To identify the causal impact of ETFs on the liquidity of their constituent securities, I next use two quasi-natural experiments. First, I exploit the quasi-natural experiment identified by Dathan and Davydenko (2018). By using exogenous variation in ETF eligibility for bonds *already* included in ETFs, the empirical strategy addresses the potential bias of ETFs self-selecting liquid bonds. On April 1, 2017, Bloomberg, the leading provider of corporate bond indexes, increased the investment grade index size minimum threshold. Compared with similar bonds staying in ETFs, bonds that no longer meet the Bloomberg index requirements and exit ETFs tracking Bloomberg indexes are found to experience a liquidity deterioration. Their exclusion from ETFs significantly increases their round trip cost and Amihud price impact by 28% and 21% of a standard deviation, respectively. This result confirms that ETFs increase the liquidity of their underlying corporate bonds. In addition, even during market stress days with large ETF redemptions (sell-offs) and arbitrage, I find that the control bonds remaining in the ETFs still exhibit higher liquidity.

Having established a causal relation between the exclusion of a bond from an ETF and an increase in its transaction costs, I use a second quasi-natural experiment to study the impact for a bond of joining an ETF. I exploit the index switch of a large investment grade ETF. Crucially, since indexes do not change in this second experiment, this identification method disentangles the ETF effect from the index effect. On August 1, 2018, after the ETF switched index, bonds with a time to maturity between 3 and 5 years became eligible to the ETF. I study the entry of newly eligible bonds into the ETF. I show that bonds joining the ETF experience a decrease of their transaction costs compared with similar bonds whose ETF ownership remains stable. Besides, confirming that neither ETF arbitrage nor stressed markets impact the findings, the results are unchanged in subsamples of days when the ETF experiences (i) redemptions, (ii) creations, and (iii) no ETF primary market activity altogether.

Turning to the transmission mechanism behind the results, I regress bond liquidity on the ETF trading volume at the bond level, given the trading volume of the ETFs holding them. I find that bond liquidity increases with ETF trading volume. This result supports three possible explanations. First, ETFs can produce tradable information that is valuable to bond market participants. Second, with widely traded bond ETFs, market participants can trade bond ETFs instead of bonds which puts competitive pressure on bond dealers. Third, bond dealers can also benefit from bond ETFs by using them as a hedge. My data does not allow me to distinguish between these three possible explanations, but anecdotal evidence supports all three (see Section 4 of the internet appendix).

Overall, I show that corporate bond ETFs increase liquidity of their underlying bonds. Contrary to the predictions of the adverse selection hypothesis, neither ETF arbitrage nor stressed markets are found to deteriorate the liquidity of the bonds held by ETFs. In particular, I show that bond liquidity is positively linked to their ETF trading volume.

Related literature. My study relates to several strands of the literature. The literature on the effects of ETFs on their underlying securities is expanding. The focus has mostly been on equity ETFs and the effect on volatility (Bhattacharya and O'Hara (2016), Ben-David, Franzoni, and Moussawi (2018), Malamud (2015), Sushko and Turner (2018), and Wermers and Xue (2015)). In contrast with the view that ETFs increase volatility, Box, Davis, Evans, and Lynch (2021) show that for equity ETFs, the underlying securities actually lead the ETF and that mispricing is not due to ETF arbitrage. Equity ETFs are found to improve informational efficiency by incorporating systematic fundamental information (Glosten, Nallareddy, and Zou (2016)) and facilitating long-short strategies (Huang, O'Hara, and Zhong (2018)). There is no consensus in the literature on the impact of ETFs on the liquidity of their underlying securities. Using quasi-natural experiments, Hegde and McDermott (2004), Sushko and Turner (2018), and Saglam, Tuzun, and Wermers (2019) find that transaction costs decrease for stocks entering ETFs, while Hamm (2010) and Israeli, Lee, and Sridharan (2017) find an opposite relation. My findings support the results of Hegde and McDermott (2004), Sushko and Turner (2018), and Saglam, Tuzun, and Wermers (2019) for the corporate bond market.

My study is closely related to the literature focusing on Fixed Income ETFs. Dannhauser (2017) shows that Fixed Income ETFs permanently increase their bond prices. Pan and Zeng (2016) study the determinants of ETF market makers arbitrage, underlying the role of market makers inventory and volatility in the liquidity mismatch between the underlying assets and the ETFs. Dannhauser (2017), Rhodes and Mason (2019), and Sultan (2015) also study the effects of ETFs on bond liquidity. I extend their studies by addressing endogeneity concerns via quasi-natural experiments. I share Dathan and Davydenko (2018) identification procedure in their study of the corporate bond issuance reaction to Fixed Income ETFs.

Finally, this paper also relates to the vast corporate bond market liquidity literature following the creation of the TRACE transaction reporting database (Anderson and Stulz (2017), Bao, O'Hara, and Zhou (2018), Bessembinder, Maxwell, and Venkataraman (2006), Bessembinder, Jacobsen, Maxwell, and Venkataraman (2018), Edwards, Harris, and Piwowar (2007), Dick-Nielsen, Feldhütter, and Lando (2012), Hendershott and Madhavan (2015), O'Hara and Zhou (2021)).

The remainder of the paper is organized as follows: Section 2 describes the tested hypotheses. Section 3 presents the data. Section 4 examines the relation between ETF ownership and corporate bond market liquidity in a panel setting. Section 5 explores a quasi-natural experiment addressing the identification issue of self-selection, while Section 6 provides a different quasi-natural experiment that disentangles the ETF effect from the index effect. Section 7 studies the transmission mechanism. Section 8 presents several robustness tests. Section 9 concludes.

2 Hypotheses development

In this section, I present the conflicting hypotheses regarding the effects of ETFs on their bond liquidity. The internet appendix provides a conceptual framework for the hypothesis development.

2.1 ETFs could decrease liquidity

2.1.1 Theoretical predictions of *adverse selection*

One strand of the literature argues that arbitrage leads to *adverse selection*, which in turn deteriorates the liquidity of underlying securities. For example, Subrahmanyam (1991) information-based model predicts a liquidity trader exodus from the underlying securities due to the introduction of a new investment vehicle that allows to trade a diversified basket. The exodus is the result of an increase in the probability of trading with an informed investor on the individual securities compared with the basket security. The liquidity of individual securities is therefore expected to decrease.

Similar in spirit, Dow (1998) information-based model predicts that the new investment vehicle facilitates arbitrage and fosters the negative impact of informed traders on the liquidity of the underlying securities. More recently, Foucault, Kozhan, and Tham (2017) show that arbitrageurs can decrease the liquidity of the underlying securities by increasing the risk of *adverse selection* for dealers. In this context, the prediction is that the more corporate bond ETF arbitrageurs benefit from information asymmetries, the wider the bid-ask spreads set by bond dealers.

2.1.2 ETF primary market activity and liquidity consumption of the underlying securities

Although ETFs are passive products aimed at reproducing indexes, they involve an active management of their underlying assets. First, given that 98% of ETFs use physical replication,[3] ETFs are funds composed of the securities they track. Second, ETF primary market requires the trading of ETF underlying securities. The daily ETF outflows and inflows directly impact the underlying securities. This is a major difference with closed-end funds which contributes to the scrutiny that ETFs face compared to mutual funds. Specifically, when ETF market makers buy an ETF and proceed to an ETF redemption on the primary market, they sell the underlying bonds as part of the arbitrage. As ETF market makers hit the bids of bond dealers, ETF market markers arguably consume liquidity from the underlying securities market (see Malamud (2015)). Moreover, Cespa and Foucault (2014) show that due to the arbitrage activity, a drop in liquidity in one market can create an illiquidity

[3] "Synthetic-ETF net assets remained steady around $75 billion, which represents about 2% of all global ETF. "Aramonte, Sirio, and Cecilia Caglio, Tugkan Tuzun (2017). "Synthetic ETF," FEDS Notes. Washington: Board of Governors of the Federal Reserve System, August 10, 2017.

contagion to another market. I thus formulate the following hypothesis :

Hypothesis 1A. *Fixed Income ETFs decrease the liquidity of their bonds.*

2.2 ETFs could increase liquidity

2.2.1 The liquidity mismatch could limit the frequency and impact of ETF arbitrage

An ETF trade is generally not accompanied by another trade on the underlying securities.[4] Indeed, the difference in liquidity between the ETF and the underlying securities limits the arbitrage frequency. Because bonds are generally more costly to trade than ETFs, trading the bonds for every ETF trade would be a loss-maker. The arbitrage only takes place when the ETF and the bonds become sufficiently misaligned so that trading the illiquid bonds is profitable (Pan and Zeng (2016)). Besides, in Malamud (2015), the existence of multiple ETFs allows ETF market makers to trade ETFs against one another, without trading the underlying securities. Thus, the liquidity mismatch could limit both the frequency and magnitude of ETF arbitrage, which in turns could limit the potential detrimental impact of *adverse selection* on the liquidity of corporate bonds.

2.2.2 The theoretical predictions of a beneficial effect

In contrast with the adverse selection hypothesis, part of the theoretical literature considers that investment vehicles can increase informational efficiency and in turn increase the liquidity of the underlying securities. The information produced by ETFs (Kumar and Seppi (1994), Madhavan and Sobczyk (2016)) could reduce the risk of adverse selection of

[4] "Year to date the entirety of all the high yield ETFs redemptions is 6 billion while there was over a trillion of trading in US HY cash bonds. So the maths do not work on how High Yield ETFs are causing any degree of price distortion in High Yield... 85 to 90% of the orders in HYG and nothing happens in the fund ... It is easier and cheaper to trade HYG for a penny than to trade all the bonds of HYG that would cost 50 basis points in transaction costs." Martin Small. Head of iShares. Us May 31, 2018 "Bloomberg Trillions". In the internet appendix, I document that for 85% of the trading volume on corporate bond ETFs there is no primary market activity.

bond dealers (Fremault (1991)), reduce the search costs of the bond market participants (Duffie, Gârleanu, and Pedersen (2005)), and facilitate risk reallocation (Danthine (1978) and Turnovsky (1983)). This beneficial effect here relies on the corporate bond ETFs trading in sufficient volume on the transparent stock market.

To illustrate the risk reallocation and search costs benefits, we can hypothesize that with Fixed Income ETFs, market participants have access to the illiquid and opaque corporate bond market at low cost through the liquid ETF market. Accordingly, Fixed Income ETFs can be used to trade in and out of a position, without trading the underlying bonds.[5] For example, Guo (2019) shows that mutual funds with illiquid holdings tend to use ETFs as a liquidity buffer to avoid liquidation costs (Edelen (1999), Coval and Stafford (2007)) and delayed investment costs (Wermers (2000)). Another example of risk reallocation is that bond dealers use bond ETFs as an hedge. Moreover, since bond dealers use RFQs and have a last look before trading, they can observe the ETF price before trading. This corporate bond market structure differs from the set up of the theoretical models such as Subrahmanyam (1991) and Foucault, Kozhan, and Tham (2017), which mitigates the risk of adverse selection.

Thus, considering that Fixed Income ETFs are now matured, liquid, and with limited arbitrage magnitude, I formulate the alternative hypothesis:

Hypothesis 1B. *Fixed Income ETFs can increase the liquidity of their bonds.*

3 Data

3.1 ETF holdings of corporate bonds

My analysis focuses on corporate bond Fixed Income ETFs that are listed on U.S. exchanges. I include every investment grade and high yield ETF present in CRSP Mutual Fund. Both the investment grade and the high yield ETF markets are heavily concentrated. For instance,

[5]I provide multiple supporting anecdotal evidence in the internet appendix.

the 5 largest investment grade ETFs hold 75% of the AuM of the 50 largest investment grade ETFs.[6]

Following Dannhauser (2017), I retrieve ETFs and corporate bond mutual funds quarterly holdings since April 2010 from CRSP Mutual Fund holdings. The reporting for ETFs is inconsistent before that date. To avoid missing information, if one fund fails to report its holdings for one quarter in between two reporting quarters, I use the last holding data available for the missing quarter. I apply the last reported end of quarter holding weights to all the months within the quarter. To obtain the ETF holdings per bond per month while avoiding the bias of issuers reporting holdings for all the fund classes (ETFs and non-ETFs) under one portfolio number,[7] I multiply the portfolio quarterly holding weights by the ETF market value at the end of the month. I thereby obtain the bond holding per ETF per month. I use Bloomberg to collect the ETF market value at the end of the month. Finally, to obtain the total ETF ownership per bond per month, I then sum the ETF holdings across all ETFs.

I hand-collect the index provider from each individual ETF prospectus and consult the ETF historical announcement to find potential index provider changes. I thereby obtain the ownership per bond per month for ETFs replicating Bloomberg indexes, and the ownership per bond per month for ETFs replicating other indexes. The average proportion of AuM of investment grade ETFs tracking Bloomberg index is 80.5%.[8]

To compute the traditional control for the effect of ownership by other institutional investors, I use corporate bond mutual funds holdings. To select corporate bond mutual funds, I only include funds from CRSP mutual fund summary database with at least 50% of corporate bonds in their AuM. As an additional check to separate ETFs from mutual funds, I require that the names of Mutual Funds do not include the terms "ETF" or "Exchange-Traded". As with ETFs, I apply the reported end of quarter holdings to all months of the

[6]The internet appendix details the list of the main ETFs in the sample Table IA1 and IA2.

[7]e.g. Vanguard as identified in Dannhauser (2017).

[8]I report the evolution of the proportion of ETF ownership of investment grade ETFs tracking Bloomberg index in the internet appendix.

quarter. The procedure yields 1,899 mutual funds.

3.2 Corporate bond transactions and characteristics

TRACE reports bond transaction data. To create the bond month liquidity variables, I extract all the trades from January 2010 to April 2020. My data set contains over one hundred million trades. To clean that data set, I use Dick-Nielsen (2014)'s methodology.

To collect U.S. corporate bond characteristics, I use Mergent FISD. The data set includes the issue amount, issue date, coupon, and maturity. I create a time series of the rating and the amount outstanding per month per bond, using the entire data set of rating and amount outstanding updates from FISD Mergent bonds historical rating, and fisd.fisd_amt_out_hist on WRDS servers. I form a numerical rating from the grades provided by the big three credit rating agencies (Standard & Poor's, Moody's and Fitch Ratings), and I use the mean of the ratings when the bond is rated by multiple agencies. Table IA5 in the internet appendix presents my numerical rating correspondence, while Table IA3 details the sample construction. My main data set contains 1,341,160 bond month observations for the 18,968 bonds. For some specifications, I use CRSP daily to obtain the parent company stock price based on the permno code of the bonds. For robustness tests, I use Refinitiv and Bloomberg to obtain ETF daily trading data such as net asset value, price, market value, and shares outstanding.

Finally, I use Compustat to obtain the financial information of the parent company and its historical Standard Industrial Classification (SIC) for an alternative specification. If the SIC is unavailable from Compustat, I use Bloomberg to obtain the missing information. I convert the historical SIC to Fama-French 12 industry classification.

3.3 Ownership variables

I construct the ownership variables following Dannhauser (2017) and Ben-David, Franzoni, and Moussawi (2018). The ETF ownership of bond i, for month t, is defined as the sum of the dollar value of holdings by all ETFs investing in that bond, divided by the bond's amount outstanding at the end of the month:

$$\text{ETFownership}_{i,t} = \frac{\sum_{ETF} \text{ETFholdings}_{\text{ETF,i,t}}}{\text{AmountOutstanding}_{i,t}}$$

The control variable, Mutual Fund ownership, is similarly constructed.

The descriptive statistics of the variables presented in Table 1b are globally in line with Dannhauser (2017), with an ETF ownership average of 1% and a mutual fund ownership average of 13%. Compared to Ben-David, Franzoni, and Moussawi (2018) who report a mean ETF ownership of 2.8% for Russell 3000 stocks, ETF ownership tends to be lower for U.S. corporate bonds than for U.S. stocks.

3.4 Bond illiquidity variables

I construct bond-month liquidity measures from TRACE transaction data. To be exhaustive, I include the main liquidity measures developed in the literature using TRACE. TRACE discloses transaction prices, that are affected by both the underlying Treasury's yield and the credit spread. To avoid the noise created by Treasuries' price moves in between corporate bond transactions (sometimes multiple days or hours apart), when possible, I construct my measures of bond liquidity for short intraday intervals.

I compute Amihud (2002) measure of the price impact of trade as the volume-weighted absolute mean return of all the trades within the trading day, where the return is the price variation from one trade to the other. To avoid the noise arising from Treasuries price changes, I only include trades less than 3 hours apart. Following Dannhauser (2017), the Amihud measure is the basis point price impact of a \$1 million trade. The distribution of the obtained Amihud measure is consistent with the current literature using similar data sets, with a median Amihud measure of 21 basis points compared with 20 basis points for Dannhauser (2017).

The Imputed Roundtrip Cost (IRC) from Feldhütter (2012) proxies the effective bid-ask spread. I identify a bond round trip trade as two or three trades of the same size happening on the same day within a 5-minute time interval. The measure is computed as the difference between the highest and the lowest price in a round trip over the highest price in the time interval. My IRC estimate is also in line with current literature using similar data sets, with a mean IRC of 21 basis points in comparison with 23 basis points for Pan and Zeng (2016).

I compute multiple measures of the bid-ask spread. The bid-ask spread in the style of Hong and Warga (2000) and Chakravarty and Sarkar (1999) uses trading side indicators to compute the difference between the dollar-weighted average price of trades made on the ask side and on the bid side. I include Pu (2009) dispersion estimate based on the inter-quartile range of trade prices during a day. Also, I consider Corwin and Schultz (2012)'s High Low and Roll (1984)'s estimators of the bid-ask spread.

To account for the bias of measuring transaction costs based exclusively on trades, I create a Zeros measure (in the spirit of Lesmond, Ogden, and Trzcinka (1999)) that relies on the absence of trades to estimate transaction costs. Here the measure is a simple count of the days without trades or with a zero return. Following Dannhauser (2017), Zeros is constructed per month per bond, as the sum of zero trade days and zero returns days over the total number of trading days in a month.

Following Bessembinder, Maxwell, and Venkataraman (2006), I use the deltaQ measure to capture trade execution costs. Here, I compute the measure at the month-bond frequency for bonds having more than 16 trades per month. I exclude negative estimates of deltaQ. I further describe the construction of the measure in the internet appendix.

Analog to Dannhauser (2017), all the liquidity measures that can be computed at the daily frequency are converted to monthly measures using their median value. The liquidity measure construction and summary statistics are detailed in Table 1b. I further describe these variables in later sections and provide definitions in Table 1a.

4 Fixed income ETFs and bond liquidity: panel regressions

This section performs the analysis of the link between bond liquidity and ETF ownership over the whole data set. I first present the empirical specifications before turning to the results. Next, I investigate periods of stress and the role of arbitrage in the findings.

4.1 Panel regression methodology

For every bond i, and month t, in the sample period from April 2010 to April 2020, I run the following OLS regression:

$$\text{IlliquidityMeasure}_{i,t} = \alpha_i + \lambda_t + \beta_1 . \text{ETFownership}_{i,t-1} + \beta' . X_{i,t} + \varepsilon_{i,t} \tag{1}$$

To account for time-invariant bond heterogeneity and common shocks and trends, the panel regression includes bond, α_i, and month-year, λ_t, fixed effects. To avoid reverse causality, ETF ownership is lagged by one month. Covariates, $X_{i,t}$, control for time-varying bond characteristics, which include the time to maturity, the age, the yield, the numerical rating, the amount outstanding, and the spread over the maturity-matched treasury. To further disentangle the effect of ETF inclusion, from the effect of index inclusion, I also control for bond mutual fund ownership. To avoid autocorrelation and heteroskedasticity biases, I compute robust standard errors. To address intra-group correlations between observations from the same bond and between observations in the same month, standard errors are clustered at both the bond and month levels.

4.2 Panel regression results

Table 2 reports the results for investment grade bonds. There is a negative and significant association between lagged ETF ownership and all bond illiquidity measures. A one standard deviation increase in ETF ownership is associated with a decrease of illiquidity by 1 to 3% of a standard deviation depending on the illiquidity variable. The effect is significant at the 1% level. The magnitude is economically important, as a one standard deviation increase in ETF ownership (ie. 1.86 percentage point) is associated with a decrease of the imputed roundtrip cost of 4%. The negative and significant association between ETF ownership and bond illiquidity measures indicates a positive effect of ETFs on bond liquidity. The result is the first piece of evidence in support of the *informational efficiency* hypothesis.

4.3 Exploring stress events and ETF arbitrage

First, to investigate the relation between ETFs and the liquidity of corporate bonds during periods of stress, I rerun the panel regressions on a subsample of previously identified periods of market stress. Following Dannhauser and Hoseinzade (2018), I include the Taper Tantrum from May 2013 to September 2013. I also include the debt ceiling crisis of July 2011 and the November 2016 election. Finally, I include the Covid epidemic from February to April 2020. The estimates presented in Table 3 show that the main findings are corroborated on these periods of stress. All transaction cost measures exhibit a negative association with ETF ownership. The result is significant for the Amihud, IRC, Roll, and DeltaQ measures. The economic magnitude is similar to the main panel regressions, with one standard deviation in lagged ETF ownership associated with a decrease of 1 to 5% of a standard deviation of the transaction cost measures.

Second, I investigate the role of ETF arbitrage. To test whether ETF market makers' trades on underlying bonds potentially consume liquidity on the underlying securities market or pick off bond dealers, I rerun the panel regressions in subsamples of months (i) with the largest ETF redemptions, (ii) with the largest ETF creations, and (iii) without significant ETF primary market activity. I first turn to the months with large ETF redemptions which tend to coincide with market stress events. I select the months for which the sum of ETF primary market activity is below the 5th percentile and obtain 6 months for the sample

2010 to April 2020.[9] I proceed similarly for the months with the largest ETF creations. Results in Table 4 confirm the negative and significant association between ETF ownership and bond transaction cost measures for both months with the largest ETF redemptions and months with the largest ETF creations. The results are also confirmed for the sub-sample including the rest of the months. Overall, my results support the view that the positive link between ETF ownership and bond liquidity cannot be driven by ETF arbitrage. Contrary to the *adverse selection* hypothesis and to Ye (2019)'s hypothesis, ETF arbitrage is neither impacting negatively nor positively the liquidity of the bonds.

Taken together my results differ from Saglam, Tuzun, and Wermers (2019). Saglam, Tuzun, and Wermers (2019) show that on average equity ETFs tend to increase the liquidity of their underlying stocks, except when ETFs tend to experience large redemptions such as during the 2011 U.S debt ceiling crisis. In contrast, the results presented in this section are consistent with hypothesis of beneficial effect of ETFs. Indeed, I find that despite stress events and ETF market makers actively trading the underlying bonds, the relation between a bond's ETF ownership and its liquidity is still positive. Furthermore, the findings contradict the hypothesis that ETFs deteriorate the liquidity of the underlying securities through the arbitrage channel. A possible explanation of the divergence between Saglam, Tuzun, and Wermers (2019) results and mine may reside in the differences between the large-cap equity ETFs they study and the corporate bond ETFs here studied. Since transaction costs are higher for corporate bonds than for large cap equities, the arbitrage of corporate bond ETFs is constrained. It follows that the cost of arbitrage reduces its magnitude and also its potential adverse effects (see Section 2.2.1).

5 Quasi-natural experiment #1: exclusion from ETFs

Overall, the panel regressions reveal a positive relation between ETF ownership and corporate bond liquidity. Nevertheless, ETF ownership might be an endogenous variable. Indeed,

[9] The internet appendix provides additional details on the sub-samples.

ETFs tend to track liquid bonds indexes.[10] To corroborate the analysis, I exploit a quasi-natural experiment provided by a recent increase in size threshold of Bloomberg investment grade indexes identified by Dathan and Davydenko (2018). I adopt a difference-in-differences approach to identify the ETF causal impact on the liquidity of their underlying bonds. The specification allows me to compare treated bonds that exit the ETFs due to an arguably exogenous shock, with a group of non-treated bonds that are otherwise as similar as possible. Since the exogenous variation in ETF eligibility takes place for bonds already included in ETFs, this identification addresses the potential bias of ETFs self-selecting liquid bonds.

5.1 Quasi-natural experiment #1: identification strategy

From 2017 onwards, Bloomberg investment grade indexes implemented a new index inclusion rule, specifically an increase in the minimum size (outstanding amount) of the bonds for inclusion. In particular, on April 1, 2017, the minimum amount outstanding for Bloomberg indexes increased to $300 million (announced January 24, 2017), up from $250 million. The increase in the threshold for index inclusion provides a plausibly exogenous shock to the ETF inclusion. Compared with previous studies where ETF portfolio managers could self select liquid bonds to add to ETFs, at the beginning of this experiment all the bonds are already included in ETFs.

The identification strategy relies on comparing bonds that were removed from ETFs due to the minimum size increase, to similar bonds just above the threshold that remained in ETFs. Thus, my treatment group is composed of bonds with an amount outstanding between $250 to $299 million and whose Bloomberg index ETF ownership was higher than zero before the announcement and equal to zero after the rule was implemented.[11] For

[10]See for example the full names of the largest high yield and investment grade ETFs : "Markit iBoxx USD Liquid High Yield Index" and "Markit iBoxx USD Liquid Investment Grade Index".

[11]In the main specification, I only include post-treatment bond observations if the bond's Bloomberg indexes ETF ownership equals 0 after April 1, 2017. In the robustness tests, I relax this condition to also include the cases in which some ETFs portfolio managers did not follow the new index threshold and partially kept bonds that should have been excluded. Table IA15 presents the results for this alternative specification. The main results are corroborated.

better identification, the treatment bonds are solely in ETFs tracking Bloomberg indexes and impacted by the index inclusion rule, therefore I exclude from the treatment group bonds in ETFs replicating other indexes.

My control group is composed of bonds with an amount outstanding above the threshold of $300 million and whose ETF ownership remains positive for the whole sample period. To avoid selection bias, following Dannhauser (2017) I use propensity score matching to select control bonds similar to treatment bonds. Using December 2016 data, I match control bonds based on their maturity, rating, volume weighted spread over the matched treasury, issue amount, and Fama French 12 sector.[12] Specifically, the following logit regression is conducted using nearest neighbor matching to increase precision:

$$P(\mathbb{1}_{Treatment_i} = 1|\alpha, \text{Maturity}_i, \text{Rating}_i, \text{Spread}_i, \text{IssueAmount}_i, \text{Sector}_i) \tag{2}$$

where $\mathbb{1}_{\text{Treatment}=i}$ is equal to one for the bonds excluded from Bloomberg indexes.

To avoid attrition bias, I require that the treatment bonds and control bonds are exchanged for at least 5 months for both the pre-event and the post-event 6 months windows. Bonds maturing during the sample period are hereby excluded. This produces 144 treatment bonds and 149 control bonds. Table IA7 of the internet appendix presents the summary statistics per group.

For every investment grade bond i in my treatment and control group, for the month m in the sample period from April 2016 to October 2017 (excluding the months between the announcement and the effective date), I run my main difference-in-differences regression:

$$\text{IlliquidityMeasure}_{i,m} = \alpha_i + \lambda_m + \beta_1.\mathbb{1}_{Post_m * Treatment_i} + \beta'.X_{i,m} + \varepsilon_{i,m} \tag{3}$$

where *Post* is an information variable equal to one from April 2017, *Treatment* is equal to one

[12] Because the numerical value of one sector can be numerically close to the numerical value of another sector without the two sectors being particularly linked, I require an exact match of the Fama French 12 sector.

for bonds in the treatment group and zero for bonds of the control group, and β_1 measures the average effect of exiting an ETF. The regression includes bond α_i and month-year λ_m fixed effects. I control for time-varying bond characteristics with the time to maturity, the credit spread, the parent firm equity volatility following Campbell and Taksler (2003), and the numerical FISD rating. Standard errors are clustered at both the bond and month year levels.

A possible identification concern is that the experiment can capture the index exclusion effect instead of the ETF exclusion effect. To disentangle the ETF effect from the the index effect, I control for bond mutual fund ownership. Moreover, contrary to corporate bond ETFs, it is unlikely that bond mutual funds would actively follow the new index rules because (i) bond mutual fund managers tend to have more leeway regarding their tracking error and (ii) excluding bonds requires selling the bonds which results in transaction costs. Therefore, bond mutual fund managers would prefer to avoid the transaction costs by keeping the bonds until maturity.[13]

5.2 Quasi-natural experiment #1: results

Table 5 presents the estimates for the main regression. I find that the treatment bonds exiting ETFs (due to the exclusion from Bloomberg indexes) become more illiquid than the matched control bonds staying in ETFs. In particular, following the bond exclusion from the ETFs, the round trip cost, the price impact ratio, the bid-ask spread and the interquartile price range, all increase compared with similar bonds staying in ETFs. The exit from ETFs is associated with an increase by 20% and 25% of a standard deviation in the Amihud price impact and the round trip cost. This effect is significant at the 1% level. The magnitude is economically important especially in relation to the 0.64% ETF ownership of the treatment bonds before the event.

To investigate the role of ETF arbitrage during periods of market stress, I re-estimate the

[13] I find that some ETF portfolio managers also kept the bonds until maturity. I discuss their impact in the robustness section. After taking them into account, the results are qualitatively unchanged.

treatment effect for days with large selling activity of ETF market makers on the underlying bonds. Specifically, at the daily frequency, I can interact the variable of interest, *Post* $*$ *Treatment*, with a binary variable that identifies the days with the largest ETFs redemptions. I compute the ETF flow per day and focus on the days with negative flows (ie. days with the largest redemption of Fixed Income ETFs). To measure the impact of large redemptions, the binary variable takes the value one on the days where the ETF flow is below the 5th percentile. The results presented in Table IA21 confirm the positive ETF effect on bonds' liquidity for the days with intense bond selling activity by ETF market makers. Even on the days when bonds in ETFs are sold by ETF market makers, bonds remaining in ETFs exhibit higher liquidity than similar bonds no longer in ETFs.

These findings support the hypothesis of a positive effect of ETFs on bond liquidity and corroborate Holden and Nam (2019) and Ye (2019) (i) using recent data, (ii) without the identification concern of the self selection of liquid bonds by the ETF portfolio managers, and (iii) with multiple measures of bond liquidity. Importantly, I show that the results are robust to ETF arbitrage and market stress events. More generally, the results are consistent with the hypothesis of a beneficial effect of ETFs.

5.3 Placebo test

To alleviate concerns about model misspecification and endogeneity, I conduct a placebo test. I create a "phantom" treatment event, 6 months before the actual announcement of the index threshold change, and rerun the regression with the same treatment and control bonds. Table 6 presents the results of the Placebo test for the period from January 2016 to December 2016, where *Post* is a binary variable equal to one after July 2016, and the treatment and control bonds remain unchanged compared to the main specification. Supporting both the identification and the parallel trend assumption, no results are statistically significant for our variable of interest measuring the average treatment effect: the interaction term *Post* $*$ *Treatment*.

6 Quasi-natural experiment #2: joining an ETF

To address remaining concerns about the index effect, I exploit a second recent quasi-natural experiment in which I investigate the effect of joining an ETF. The experiment relies on the switch of index by a large investment grade ETF. To identify the effect of ETFs on the liquidity of their bonds, I use the positive shock to the ETF ownership of bonds joining the ETF following the switch of index. Since only the composition of the ETF changes, while the composition of the indexes remains unchanged, the experiment provides a unique identification of the ETF effect without an index effect.

6.1 Quasi-natural experiment #2: identification strategy

On August 1, 2018, iShares Short-Term Corporate Bond ETF (IGSB), changed the index it replicates, from the Bloomberg Barclays U.S. 1-3 year Credit Index to the ICE BofAML 1-5 Year US Corporate Index. As such, after the index switched, bonds with a time to maturity between 3 and 5 years became eligible to the ETF. On the date of the switch, the ETF held $10.9 billion worth of bonds, making it the fourth-largest investment grade ETF.

Bonds included in the treatment group have a time-to-maturity over 3 years and joined the ETF (IGSB) after the index switch. These treatment bonds become eligible for inclusion in ETF after August 1, 2018. To select treatment bonds that joined the ETF, I only select bonds absent from the ETF prior to August 1, 2018. The identifying assumption is that the treatment bonds only differ from the control bonds in the positive shock to their ETF ownership they experience following their inclusion to the ETF. For better identification, I require that the shock (from IGSB inclusion) to the treatment bond's global ETF ownership is superior to 0.7%.[14]

My control group is composed of bonds as similar as possible to the treatment group but whose ETF ownership remains stable over the whole sample period. To avoid selection

[14] I test the implications of this discretionary threshold in the internet appendix. In Table IA23, I show that results are confirmed for Treatment bonds experiencing an ETF ownership shock superior to 0.5%.

bias, I reproduce the propensity score matching describe previously. To make sure the ETF ownership of the control bonds remains stable, I require that the maximum absolute shock to the ETF ownership is inferior to 0.05% and that the ETF ownership does not change by more than $1 million. I test an alternative threshold to select the control bonds in the internet appendix.[15] To avoid attrition bias, I require that treatment and control bonds are present every month of both the pre-event and the post-event windows (hereby excluding expiring bonds and bonds issued during the sample period).

This procedures yields 70 treatment bonds and 70 control bonds. Table IA8 of the internet appendix presents the summary statistics per group in the period preceding the switch. Validating the selection process of counterfactual bonds that could have been added to the ETF, the control bonds are found to be included in other ETFs. Before the event, the control (treatment) bonds have an average ETF ownership of 2.14% (2.64%) with a standard deviation of 1%.

The shock to the ETF ownership of bonds that joined IGSB is plausibly exogenous for two reasons. First, since Barclays (the original index provider of iShares bond ETFs) sold its index division in December 2015, iShares switched multiple ETFs to tracking ICE indexes when the previous contracts expired. Second, iShares decision to switch to a 1-5 index can be interpreted as a competitive move against a similar ETF from its competitor Vanguard.[16]

For every investment grade bond i in my treatment and control group, in the sample period from December 2017 to February 2019 (excluding the months between the announcement and its application, ie. June and July 2018), I run my baseline difference-in-differences (Equation 3).

[15]In Table IA23, I show that results are confirmed for control bonds whose ETF ownership variation stays inferior to 0.25%. Since in this alternative specification the control bonds tend to have a larger ETF ownership, the results also alleviate concerns about the IGSB portfolio manager self selecting liquid securities compared with the control bonds.

[16]VCSH from Vanguard is the second investment grade ETF by AuM with a time to maturity of 1 to 5 years). Another element indicating this competitive strategy is the expense ratio of IGSB: 0.06% against 0.07% for VCSH.

6.2 Quasi-natural experiment #2: results

Table 7 and Figure 1 present the results from the second difference-in-differences. The average treatment effect is negative and significant. The results indicate that the treatment bonds joining the ETF due to the change of replicating index tend to become more liquid than the matched control bonds whose ETF ownership remains stable. In particular, following the bond inclusion to the ETF, the round trip cost, the Amihud price impact ratio, the bid-ask spread, the high low, and the interquartile price range, all decrease compared with the control bonds. Joining the ETF is associated with a decrease of 2% to 11% of a standard deviation of these illiquidity measures. The effect is significant at the 1% level. The economic magnitude is important relative to the magnitude of the ETF ownership shock -with a threshold of 0.7%.

I now examine the effect of ETF arbitrage on the findings. I first rerun the difference-in-differences at the daily frequency on a subsample of days with ETF arbitrage (Table 9). Even on days with ETF arbitrage, the bonds that entered the ETF exhibit higher liquidity than the control bonds. Panel B of Table 9 also confirms the findings for the days without ETF arbitrage. These results provide two important implications. First, contrary to the theoretical predictions of *adverse selection* and liquidity consumption, ETF arbitrage is not impacting negatively bond's liquidity. Second, since the positive relation between ETF ownership and bond liquidity is persistent in the absence of ETF arbitrage, ETF arbitrage is not the driver of the increase in bonds' liquidity. Henceforth, contrary to Ye's findings, I conclude that ETF arbitrage cannot be the transmission mechanism behind the findings.

7 Transmission Mechanism

In this section, I show that the increase in the bonds' liquidity is linked to ETF trading volume.

To compute the ETF trading volume at the bond-month level, I use the monthly trading volume for the 50 largest corporate bond ETFs. For simplicity, the sample is restricted to the largest ETFs because ETF trading volume is extremely concentrated around the largest ETFs, as shown in Table IA1. I obtain from Refinitiv the daily volume-weighted average price (VWAP) and the number of shares traded per ETF. The dollar volume is the VWAP multiplied by the number of shares traded. I sum the dollar volume per month per ETF. Finally, I weight the ETF dollar volume by the ETF holdings of the bond. $ETFTradingVolume_{i,m}$ captures the impact of ETFs trading volume during month m on bond i.

$$\mathrm{ETFTradingVolume_{i,m}} = \frac{\sum_{ETF} ETFDollarVolume_{ETF,m} * ETFholdings_{ETF,i,m}}{\sum_{ETF} ETFholdings_{ETF,i,m}}$$

To test how the liquidity of the bonds is impacted by ETF trading volume, I rerun my panel regressions 1 replacing the ETF ownership by the ETF trading volume. Table 10 presents the results of this estimation. The results are similar to those with ETF ownership. I find a strong positive relationship between bond liquidity and the ETF trading volume. I find across most proxies of transaction costs that the higher ETF trading volume is linked to lower bond transaction costs. The results are consistent except for $HighLow_{i,m}$ which is a proxy designed for equities and $IRC_{i,m}$ which measures only the dealers' round trip costs.

The results of this section suggest that the ETF trading volume is the mechanism driving the increase in bond liquidity. This result is consistent with the predictions of the information efficiency models. As noted earlier, when an ETF trades in size then market participants can substitute the bonds for the ETF, especially if the bonds are costly to trade. For instance, when LQD, the largest investment-grade ETF, trades over $2 billion per day on average in 2020, its price becomes tradable information. The bond market opacity is then reduced[17] and it becomes harder for bond dealers to justify their wide bid-ask spreads. Moreover, given sufficient ETF trading volume, bond dealers can use corporate bond ETFs to hedge their books which can help them tighten the bid-ask spread on their quotes.

[17] For instance, Laipply and Madhavan (2020) show that despite apparent discounts, corporate bond ETFs actually provided price discovery during the 2020 Covid-19 crisis

8 Robustness tests

This section presents several robustness tests. I first focus on the panel regressions using the entire data set, then I turn to the two quasi-natural experiments.

8.1 Panel regression robustness

To assess the robustness of the results, I conduct a variety of additional tests.[18] First, to test the sensitivity of the findings to the level of liquidity of the bonds, I separate bonds into a liquid sub-sample and an illiquid sub-sample. I use Principal Component Analysis (PCA) to construct a factor capturing the information most relevant to liquidity out of all the liquidity variables. I then split the sample into a liquid and an illiquid subsample based on their median first principal component. I confirm the negative and significant association between ETF ownership and bond transaction costs for both subsamples (Table IA12 and Table IA13). The positive association between ETF and bond liquidity is therefore confirmed independently of the bonds' liquidity. Both liquid and illiquid bond present similar results.

Second, to address concerns regarding rating non-linearity, especially around the high yield threshold, I split the sample of bonds into an investment grade sub-sample and a high yield sub-sample. The estimates are consistent across both sub-samples and confirm the findings of the main specification (Table IA10 presents the estimates for the panel of investment grade bonds while Table IA11 provides the estimates for the panel of high yield bonds).

Finally, following Anderson and Stulz (2017) I recompute the bond transaction costs using only large trades. The main results are corroborated (see Table IA14).

[18]The additional robustness tables are found in the internet appendix.

8.2 Quasi-natural experiment #1 robustness

Following the increase in the index threshold, there was heterogeneity in its application across ETFs. Notwithstanding the bonds were no longer meeting their index requirements, some ETF portfolio managers partially kept bonds. In the baseline specification of the difference in differences, bonds that should have been excluded but partially remained in Bloomberg ETFs were excluded from the treatment group. However, the selection process of the ETF portfolio managers could create a potential bias. To investigate this potential selection bias, I rerun the difference-in-differences considering all the bonds that should have been considered as treated, without consideration for the actual application of the new threshold by the ETF portfolio managers. The treatment group is then composed of bonds in ETFs tracking a Bloomberg index before the index threshold increase, and with an amount outstanding between $250 and $299 (i.e. impacted). Therefore, I consider the effect as exogenously determined by the index provider with no possible selection by the ETF portfolio managers. Table IA15 presents the regression estimates. The results of the main difference-in-differences are corroborated for this alternative specification, confirming that this potential selection bias has no impact on my findings.

To limit the concerns about researcher discretion, I test the selection of control bonds and verify the results using alternative selection criteria. In particular, I include financial data on the parent company to select the control bonds. I add leverage, book-to-market, and profitability to the propensity score matching (equation 2). In Table IA16, I show that the results are qualitatively similar.

I next investigate the robustness of the findings to an alternative data frequency. I confirm the results at the daily frequency instead of the monthly frequency for the illiquidity variables that can be computed daily: Amihud and IRC. Robustness test results and their placebo tests are included in the internet appendix.

8.3 Quasi-natural experiment #2 robustness

To alleviate concerns about model misspecification and endogeneity, I reproduce the placebo test previously described. Table 8 presents the estimates of the Placebo test for a six-month window period around January 2018, where *Post* is a binary variable equal to one after January 2018, and treatment and control bonds remain unchanged compared to the main regression. Supporting the identification and the parallel trend assumption, the variable of interest, the average treatment effect, is inconsistent before the actual event, and only significant for one of the measures.

In Table IA23, I turn to concerns about researcher discretion. I test alternative thresholds to select both the treatment and control bonds. Finally, I confirm the results at the daily frequency. The supporting test is included in the internet appendix Table IA22.

9 Conclusion

In this study, I investigate the impact of corporate bond ETFs on the liquidity of their underlying bonds. I start with fixed effects models indicating a generally positive relation between a bond's ETF ownership and its liquidity. Next, I explore quasi-natural experiments to identify the causality while addressing self-selection and index effect biases.

Using plausibly exogenous variation in ETF eligibility, my results evidence a positive causal relation between Fixed Income ETF ownership and the liquidity of the underlying bonds. In particular, corporate bonds joining (exiting) ETFs experience a liquidity improvement (deterioration). Contrary to the prediction of *adverse selection* and of liquidity consumption of ETF arbitrageurs, ETF arbitrageurs trading the underlying bonds do not negatively affect the liquidity of the bonds. Furthermore, throughout the study, I find a positive association between ETF ownership and bond's liquidity during market stress events. Specifically, my results suggest that bond liquidity increases with the trading volume of corporate bond ETFs. These results provide implications both in the specific discussion on the benefits and harms of corporate bond ETFs and for the broader topic of the impact of innovative investment vehicles on their underlying securities.

Overall the results are consistent with three possible explanations: corporate bond ETFs (i) increase information efficiency, (ii) increase bond dealers' competition, and (iii) facilitate bond dealers hedging. Additional research with better data could help in distinguishing their relative role.

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10 Appendix

Figure 1: ETF inclusion and the transaction costs
The chart reports estimates from quasi-natural experiment #2 relying on the IGSB ETF index switch, effective August 1, 2018. The ETF changed replication index from the Bloomberg Barclays U.S. 1-3 year Credit Index to the ICE BofAML 1-5 Year US Corporate Index. I measure the effect of joining an ETF on bond liquidity. The 70 treatment bonds have a time to maturity over 3 years and joined the ETF after the index switch. I require that the switch increased the treatment bonds' ETF ownership by more than 0.7%. Control group includes similar bonds selected by PSM whose ETF ownership remains stable for the whole sample period. The sample is composed of observations 6 months before the switch was announced (from December 2017 to May 2018), and 6 months after the switch was effective (from August 2018 to January 2019). The frequency of the observations is monthly. IRC is a measure of transaction costs based on the round trip cost using bond transactions per day. IRC is expressed in %.

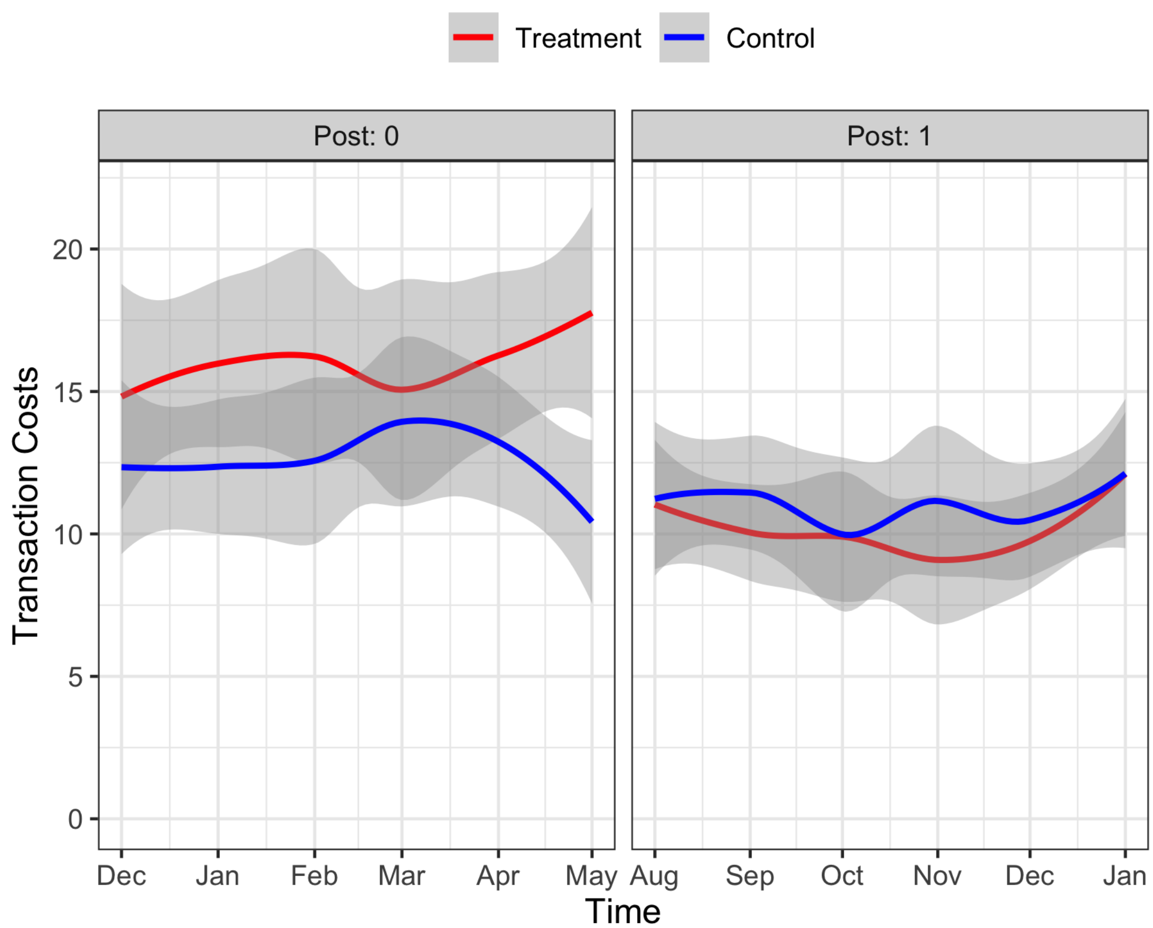

Table 1: Summary statistics

Panel A details the variables of interests including the bonds' illiquidity variables and the ownership variables. Panel B presents the bonds summary statistics. The liquidity variables are in basis points while ETF ownership and MF ownership are in %. The sample ranges between April 2010 and April 2020.

(a) Panel A: Variable description

Parameter	Definition/Interpretation
Amihud	$\text{Amihud}_{i,d} = \frac{1}{\text{NberTrade}_d}\sum_j \frac{r_j}{v_j} * 1,000,000$ Where r_j is the transaction return within a 3 hour interval.
bid-ask spread	difference between the daily average selling price from the daily average buying price
deltaQ	estimates from Bessembinder, Maxwell and Venkataraman (2006) trading cost model
High and Low	daily Corwin Schultz bid-ask spread estimator (with negative spreads set to zero)
IQR	interquartile daily price range
Imputed Roundtrip Cost	$IRC_d = (P_{max} - P_{min})/P_{min} * 100$ average from two or three trades with the same volume within 5 minutes.
Roll	$Roll_m = 2\sqrt{-cov(r_d, r_{d-1})} * 100$ setting positive covariance to zero
Zeros	Number of zero trading days and zero returns days over the number of trading days in the month.
ETF ownership	ETF market value of bond i over the bond's amount outstanding.
MF ownership	Mutual funds market value of bond i over the bond's amount outstanding.

(b) Panel B: Bonds Summary Statistics

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Median	Pctl(75)	Max
Amihud	906,447	27.05	30.36	0.01	8.37	16.50	33.88	205.39
BidAsk	811,447	54.39	70.68	0.01	12.77	27.60	63.82	415.08
deltaQ	648,331	38.54	50.17	0.63	9.74	21.87	47.02	364.25
HighLow	775,665	81.68	95.30	0.43	19.90	47.18	105.32	521.11
IQR	782,137	23.32	27.90	0.01	5.40	13.06	29.33	163.04
IRC	790,147	21.99	31.37	0.01	4.93	9.73	24.59	206.54
Roll	571,137	81.04	87.95	0.08	26.40	52.80	101.80	542.32
Zeros	965,333	0.39	0.31	0.00	0.09	0.35	0.67	0.96
ETFOwnership	947,823	1.31	1.86	0.00	0.00	0.27	2.24	8.36
MFOwnership	947,823	6.59	11.04	0.00	0.00	1.74	7.88	54.91

Table 2: Panel regressions: corporate bond liquidity and ETF ownership

The table reports estimates from OLS regression of bond liquidity measures on lagged ETF ownership and controls for corporate bonds. Measures are computed at the bond month frequency and normalized. Amihud, BidAsk, Bessembinder et al. (2006) deltaQ, HighLow, the round trip cost IRC, the interquartile price range IQR, Roll, and Zeros are all illiquidity measures. The controls include the time to Maturity, the amount outstanding, the FISD numerical rating, the spread over the treasury, and the bond mutual fund ownership. Bond and month fixed effects are included. Standard errors are double clustered at the bond and month levels. Robust standard errors are presented in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively. The sample ranges between April 2010 and April 2020.

	Amihud	BidAsk	deltaQ	HighLow	IQR	IRC	Roll	Zeros
ETFOwnership	-0.01^{***}	-0.01^{***}	-0.02^{***}	0.001	0.004	-0.03^{***}	-0.02^{***}	-0.06^{***}
	(0.003)	(0.003)	(0.003)	(0.004)	(0.01)	(0.003)	(0.003)	(0.005)
MFOwnership	-0.04^{***}	-0.05^{***}	-0.03^{***}	-0.05^{***}	-0.02^{***}	-0.02^{***}	-0.04^{***}	0.005
	(0.005)	(0.01)	(0.005)	(0.01)	(0.01)	(0.005)	(0.01)	(0.004)
TimetoMaturity	-0.003^{**}	-0.005^{*}	0.0003	-0.0001	0.001	-0.005^{***}	0.002	0.0005
	(0.001)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Age	0.003^{***}	0.03^{***}	0.01^{***}	0.001	0.003^{**}	0.002^{***}	0.01^{***}	0.002^{***}
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Rating	-0.004	-0.01^{***}	-0.01^{**}	0.003	-0.02^{***}	-0.01^{***}	0.0002	-0.03^{***}
	(0.003)	(0.004)	(0.002)	(0.004)	(0.005)	(0.003)	(0.003)	(0.003)
Amount	-0.01	-0.14^{***}	-0.33^{***}	0.10^{***}	0.09^{***}	-0.11^{***}	-0.39^{***}	-0.45^{***}
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)
Spread	0.34^{***}	0.09^{***}	0.23^{***}	0.42^{***}	0.27^{***}	0.16^{***}	0.41^{***}	-0.03^{***}
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.005)
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	810,373	728,670	592,127	701,445	704,497	713,453	521,685	856,503
R^2	0.41	0.51	0.41	0.53	0.48	0.29	0.55	0.80

Table 3: Corporate bond liquidity, ETF ownership, and stress events

The table reports estimates from OLS regression of bond liquidity measures on lagged ETF ownership and controls for Investment Grade bonds. Measures are computed at the bond month frequency and normalized. Amihud, BidAsk, Bessembinder et al. (2006) deltaQ, HighLow, the round trip cost IRC, the interquartile price range IQR, Roll, and Zeros are all illiquidity measures. The controls include the time to Maturity, the amount outstanding, the FISD numerical rating, the spread over the treasury, and the bond mutual fund ownership. Bond and month fixed effects are included. Standard errors are double clustered at the bond and month levels. Robust standard errors are presented in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively. The sample only includes stress periods: the debt ceiling crisis of June and July 2011, the Taper Tantrum from May 2013 to September 2013, the two months preceding the 2016 election, and the Covid epidemic from February 2020 to April 2020.

	Amihud	BidAsk	deltaQ	HighLow	IQR	IRC	Roll	Zeros
ETFOwnership	−0.01	−0.01*	−0.04**	0.01	0.03	−0.03***	−0.04**	−0.09***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)
MFOwnership	−0.04**	−0.05***	−0.03**	−0.04***	−0.04**	−0.03**	−0.01	0.01*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)
TimetoMaturity	0.0003	−0.003	−0.01	−0.002	0.0004	−0.004	0.001	−0.001
	(0.001)	(0.004)	(0.01)	(0.003)	(0.004)	(0.002)	(0.01)	(0.002)
Age	0.01***	0.03***	0.001	0.004	0.01**	0.002	−0.001	0.003**
	(0.002)	(0.003)	(0.004)	(0.003)	(0.002)	(0.002)	(0.004)	(0.002)
Rating	0.01	−0.003	−0.02*	0.02	−0.0003	0.002	−0.02*	−0.04***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.005)
Amount	0.05	−0.13**	−0.39***	0.16***	0.15**	−0.15**	−0.49***	−0.52***
	(0.03)	(0.05)	(0.06)	(0.04)	(0.05)	(0.05)	(0.06)	(0.03)
Spread	0.35***	0.07**	0.19***	0.41***	0.31***	0.12***	0.44***	−0.04***
	(0.04)	(0.03)	(0.04)	(0.05)	(0.04)	(0.02)	(0.04)	(0.01)
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	79,659	71,276	57,850	69,162	69,130	69,781	47,217	84,105
R^2	0.50	0.57	0.55	0.62	0.61	0.40	0.71	0.85

Table 4: Bond liquidity, ETF ownership and ETF primary market

The table reports estimates from OLS regression of bond illiquidity measures on lagged ETF ownership and controls for corporate bonds. Measures are computed at the bond month frequency. Amihud, BidAsk, Bessembinder et al. (2006) deltaQ, HighLow, the round trip cost IRC, the interquartile price range IQR, Roll, and Zeros are all illiquidity measures. The controls include the time to maturity, the age the amount outstanding, the FISD numerical rating, and the bond mutual fund ownership. Bond and month fixed effects are included. The sample ranges between April 2010 and April 2020. Panel A includes the months (above the 95th percentile) with the largest ETFs creations, while Panel B includes the months with the largest ETFs redemptions (below the 5th percentile). Panel C includes the rest of the months. Robust standard errors double clustered at the bond and month levels are presented in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

	Amihud	BidAsk	deltaQ	HighLow	IQR	IRC	Roll	Zeros
				(a) Panel A: Redeem Months				
ETFOwnership	−0.03*	−0.04***	−0.05***	−0.004	−0.01	−0.04***	−0.06***	−0.11***
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	32,611	30,431	25,106	29,171	28,675	28,804	21,850	34,258
R^2	0.62	0.68	0.76	0.77	0.72	0.61	0.84	0.85
				(b) Panel B: Create Months				
ETFOwnership	−0.02**	−0.03*	−0.02**	−0.02*	0.001	−0.03***	0.04	−0.07***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.04)	(0.01)
Observations	37,129	32,447	22,254	27,871	28,240	29,337	16,281	44,653
R^2	0.62	0.6	0.73	0.73	0.71	0.59	0.87	0.83
				(c) Panel C: Low Arbitrage Months				
ETFOwnership	−0.01***	−0.01**	−0.01***	−0.004	−0.005**	−0.01***	−0.01**	−0.05***
	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Observations	724,199	649,133	527,738	625,154	628,635	637,271	466,393	765,734
R^2	0.41	0.52	0.41	0.53	0.48	0.29	0.54	0.80

Table 5: Quasi-Natural Experiment #1 -Exclusion from ETFs

The table reports estimates from a quasi-natural experiment relying on the index size threshold change of Bloomberg investment-grade bond indexes announced January 24, 2017, and effective April 1, 2017. The difference-in-differences coefficie, Post Treatment, measures the effect of an ETF exit on bond liquidity. The sample consists of investment grade bonds with an amount outstanding between $250 to 300 million and whose ETF ownership before the announcement was positive. The treatment bonds include bonds whose Bloomberg indexes ETF ownership is positive before the announcement and equal to zero after the Bloomberg index rule is implemented and the bonds exit ETFs. Treatment bonds have zero ownership of non-Bloomberg index ETFs. Control group includes bonds just above the new index threshold and whose ETF ownership (from non-Bloomberg ETF indexes) stays positive for the whole sample period. The sample is composed of observations 6 months before the announcement, and 6 months after the implementation. Measures are computed at the bond month frequency, winsorized and normalized. Amihud, BidAsk, deltaQ, HighLow, IQR, IRC, Roll, and Zeros are all illiquidity measures. The controls include the time to Maturity, the FISD numerical rating, the parent company stock volatility, the spread over the maturity-matched treasury and the bond mutual fund ownership. Bond and month fixed effects are included. Robust standard errors are presented in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

	Amihud	BidAsk	deltaQ	HighLow	IQR	IRC	Roll	Zeros
Post∗Treatment	0.24**	0.13**	0.15*	0.05	0.14*	0.31**	−0.05	−0.09*
	(0.08)	(0.05)	(0.08)	(0.07)	(0.07)	(0.10)	(0.08)	(0.05)
Spread	1.11**	0.23	0.37**	1.12***	1.36**	0.38	0.15	−0.21
	(0.39)	(0.34)	(0.07)	(0.15)	(0.45)	(0.33)	(0.32)	(0.17)
TimetoMaturity	−0.02	−0.05	−0.004	0.07***	−0.06	−0.04	−0.01	0.02**
	(0.05)	(0.03)	(0.06)	(0.02)	(0.05)	(0.08)	(0.04)	(0.01)
Rating	0.05	0.07	−0.01	0.07	0.14*	0.01	0.17**	−0.12***
	(0.10)	(0.05)	(0.09)	(0.06)	(0.08)	(0.11)	(0.06)	(0.03)
MFOwnership	0.09	0.04	−0.32	−0.02	0.05	−0.25***	0.12	0.09
	(0.06)	(0.06)	(0.10)	(0.07)	(0.05)	(0.05)	(0.14)	(0.08)
Volatility	−4.84	−0.82	4.33	5.90	5.23	−14.78***	8.07	−3.89**
	(4.31)	(4.73)	(7.51)	(4.31)	(3.02)	(2.18)	(5.60)	(1.68)
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Observations	2,711	2,245	1,301	1,821	2,020	2,296	966	3,276
R^2	0.39	0.38	0.42	0.54	0.55	0.37	0.55	0.72

Table 6: Placebo test quasi natural experiment #1

The table reports the falsification results of the main regression where Post is a dummy variable equal to one after July 2016 (6 months before the announcement of the new index rules) and the sample period is the full year 2016.

	Amihud	BidAsk	deltaQ	HighLow	IQR	IRC	Roll	Zeros
Post∗Treatment	−0.03	−0.02	−0.06	0.04	0.04	−0.11	0.04	−0.04
	(0.11)	(0.07)	(0.12)	(0.08)	(0.10)	(0.12)	(0.08)	(0.06)
Spread	0.33	−0.15	−0.34	0.25	0.59	−0.08	0.34	−0.33*
	(0.48)	(0.28)	(0.46)	(0.27)	(0.47)	(0.48)	(0.38)	(0.16)
TimetoMaturity	0.15***	0.10	0.11	0.18***	0.10	0.33***	−0.04	−0.07**
	(0.04)	(0.07)	(0.07)	(0.05)	(0.07)	(0.02)	(0.08)	(0.03)
Rating	−0.03	0.02	−0.16	−0.05	0.11	0.11	−0.16	−0.04
	(0.08)	(0.06)	(0.15)	(0.10)	(0.08)	(0.13)	(0.11)	(0.04)
MFOwnership	0.25	0.18	0.03	0.11	0.20	0.35	−0.02	−0.05
	(0.19)	(0.18)	(0.26)	(0.11)	(0.19)	(0.22)	(0.16)	(0.11)
Volatility	−1.00	4.84*	10.22	−1.38	5.13	−4.38	−7.03	−0.55
	(4.66)	(2.22)	(9.85)	(4.20)	(4.48)	(5.19)	(7.70)	(3.27)
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,572	2,154	1,179	1,707	1,909	2,180	851	3,047
R^2	0.35	0.33	0.42	0.48	0.48	0.34	0.52	0.69

Table 7: Quasi-Natural Experiment #2 - Joining an ETF

The treatment bonds have a maturity over 3 years and entered the ETF after the ETF changed replication index from the Bloomberg Barclays U.S. 1-3 year Credit Index to the ICE BofAML 1-5 Year US Corporate Index. Control group includes bond whose ETF ownership stays stable for the whole sample period. The sample is composed of observations in a 6 month-window around the August 1, 2018 event. Measures are computed at the bond month frequency, winsorized and normalized. Amihud, BidAsk, deltaQ, HighLow, IQR, IRC, Roll, and Zeros are all illiquidity measures. The controls include the time to Maturity, the FISD numerical rating, the spread over the maturity-matched treasury and the bond mutual fund ownership. Bond and month fixed effects are included. Robust standard errors are presented in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

	Amihud	BidAsk	deltaQ	HighLow	IQR	IRC	Roll	Zeros
Post∗Treatment	−0.07**	−0.04**	−0.07**	−0.11**	−0.04	−0.13***	0.003	−0.01
	(0.02)	(0.01)	(0.03)	(0.03)	(0.02)	(0.04)	(0.03)	(0.04)
Spread	0.63***	0.30***	0.21	1.14***	0.75***	0.40	−0.04	−0.91**
	(0.11)	(0.10)	(0.13)	(0.25)	(0.14)	(0.26)	(0.23)	(0.33)
Rating	0.03**	−0.01	−0.01	0.02	0.02	0.01	0.01	0.01
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)
MFOwnership	0.04	−0.02	0.003	0.02	−0.03	0.03	−0.02	0.01
	(0.04)	(0.03)	(0.04)	(0.02)	(0.04)	(0.07)	(0.04)	(0.07)
Age	−0.02*	−0.001	0.04**	−0.03***	−0.04***	−0.02	0.03	−0.01
	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,735	1,713	1,590	1,711	1,681	1,637	1,550	1,743
R^2	0.55	0.51	0.40	0.74	0.61	0.40	0.38	0.75

Table 8: Quasi-Natural Experiment #2 - Placebo Test

The table reports the falsification results of the main regression where Post is a dummy variable equal to one, one year before the actual switch, and the six month window period is updated accordingly. Robust standard errors are presented in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

	Amihud	BidAsk	deltaQ	HighLow	IQR	IRC	Roll	Zeros
Post∗Treatment	0.03	−0.01	0.02	−0.002	−0.005	0.12***	−0.02	0.04
	(0.04)	(0.02)	(0.05)	(0.05)	(0.03)	(0.03)	(0.04)	(0.05)
Spread	0.83***	0.30	0.20	1.31***	0.77***	1.36***	−0.08	−0.49*
	(0.18)	(0.29)	(0.31)	(0.36)	(0.23)	(0.24)	(0.36)	(0.24)
Rating	−0.003	−0.005	0.03	0.06*	−0.01	0.01	0.01	0.04
	(0.02)	(0.01)	(0.02)	(0.03)	(0.02)	(0.02)	(0.01)	(0.02)
MFOwnership	−0.02	−0.03	0.01	−0.11**	0.004	−0.05	0.02	0.003
	(0.04)	(0.02)	(0.03)	(0.05)	(0.04)	(0.07)	(0.02)	(0.06)
Age	−0.03	−0.06**	−0.004	−0.04**	−0.04**	0.03***	0.10***	0.01
	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,547	1,517	1,404	1,511	1,457	1,431	1,373	1,556
R^2	0.46	0.44	0.34	0.68	0.61	0.40	0.30	0.78

Table 9: Quasi-Natural Experiment #2 - Joining an ETF Daily frequency- Subsamples of days with and without ETF arbitrage

The chart reports estimates from quasi-natural experiment #2 relying on the IGSB ETF index switch, effective August 1, 2018. The sample is divided in days when the ETF, IGSB, experienced a creation, a redemption or no primary market activity. The sample covers a six-month window around the switch. Measures are computed at the bond-day frequency, winsorized and normalized. Amihud, BidAsk, HighLow, IQR, IRC are all illiquidity measures. The controls include the time to Maturity, the FISD numerical rating, the credit spread and the bond mutual fund ownership. Bond and month fixed effects are included. Robust standard errors clustered at the bond level are presented in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: with ETF arbitrage

	Amihud	BidAsk	HighLow	IQR	IRC
Post∗Treatment	-0.08^{**}	-0.004	-0.09^{**}	-0.04	-0.11^{***}
	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)
Spread	0.003^{***}	0.003^{***}	0.01^{***}	0.004^{***}	0.002^{*}
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Rating	0.003	-0.04	0.02	-0.03	-0.05
	(0.04)	(0.04)	(0.05)	(0.04)	(0.05)
MFownership	0.01	0.0002	0.002	0.003	0.01^{**}
	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)
Fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	7,966	6,368	6,867	8,803	7,141
R^2	0.16	0.26	0.31	0.20	0.17

Panel B: without ETF arbitrage

	Amihud	BidAsk	HighLow	IQR	IRC
Post∗Treatment	-0.06^{**}	-0.07^{*}	-0.10^{***}	-0.04	-0.08^{**}
	(0.03)	(0.04)	(0.03)	(0.03)	(0.04)
Spread	0.001^{**}	0.002^{**}	0.004^{***}	0.003^{***}	0.0004
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Rating	0.03	-0.02	0.01	-0.01	0.02
	(0.03)	(0.03)	(0.04)	(0.03)	(0.04)
MFownership	0.004	-0.01^{***}	0.01^{*}	-0.001	0.01^{***}
	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)
Fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	15,020	12,045	12,928	16,579	13,408
R^2	0.13	0.23	0.28	0.18	0.11

Table 10: Panel regressions: corporate bond liquidity and ETF trading volume

The table reports estimates from OLS regression of bond liquidity measures on ETF trading volume and controls for corporate bonds. Measures are computed at the bond month frequency and normalized. Amihud, BidAsk, Bessembinder et al. (2006) deltaQ, HighLow, the round trip cost IRC, the interquartile price range IQR, Roll, and Zeros are all illiquidity measures. The ETF trading volume is computed per month per bond based on the ETF holdings of the bonds. The controls include the time to Maturity, the amount outstanding, the FISD numerical rating, the spread over the treasury, and the bond mutual fund ownership. Bond and month fixed effects are included. Standard errors are double clustered at the bond and month levels. Robust standard errors are presented in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively. The sample ranges between April 2010 and April 2020.

	Amihud	BidAsk	deltaQ	HighLow	IQR	IRC	Roll	Zeros
ETFTradingVolume	−0.01	−0.04***	−0.03***	0.02***	0.02***	−0.02***	−0.03***	−0.06***
	(0.004)	(0.01)	(0.005)	(0.01)	(0.01)	(0.004)	(0.005)	(0.005)
MFOwnership	−0.04***	−0.05***	−0.03***	−0.05***	−0.02***	−0.02***	−0.04***	0.004
	(0.005)	(0.01)	(0.01)	(0.01)	(0.01)	(0.004)	(0.01)	(0.004)
TimetoMaturity	−0.003**	−0.004	0.001	−0.0003	0.001	−0.005***	0.003	0.001
	(0.001)	(0.003)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)
Age	0.003***	0.03***	0.01***	0.001	0.003**	0.002***	0.01***	0.002**
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Rating	−0.005	−0.01***	−0.01***	0.003	−0.02***	−0.01***	−0.001	−0.03***
	(0.003)	(0.004)	(0.002)	(0.004)	(0.005)	(0.003)	(0.003)	(0.003)
Amount	−0.01	−0.13***	−0.32***	0.10***	0.08***	−0.11***	−0.38***	−0.45***
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)
Spread	0.34***	0.09***	0.23***	0.42***	0.27***	0.17***	0.41***	−0.02***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.005)
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	823,749	740,819	600,923	712,895	716,875	725,126	529,316	870,345
R^2	0.41	0.51	0.40	0.53	0.48	0.29	0.55	0.79