

# Three Essays on Corporate Bond Market Liquidity

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Three Essays on Corporate Bond Market Liquidity

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**Jens Dick-Nielsen**

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Jens Dick-Nielsen

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# Three Essays on Corporate Bond Market Liquidity

Jens Dick-Nielsen

Ph.D. Dissertation  
Department of Finance  
Copenhagen Business School

August, 2010



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# Preface

This thesis marks the end of my Ph.D studies in Finance at Copenhagen Business School. The thesis consists of three empirical studies on the liquidity of the corporate bond market. Each of the three essays in the thesis are self-contained with included literature reviews and can be read independently.

## Structure of the thesis

The three essays study the US corporate bond market with special attention to bond liquidity. All essays are empirical studies which rely heavily on the availability of transactions data. Earlier studies had to use quoted bond prices for empirical studies, but with the introduction of the TRACE system and with the following dissemination of transaction prices the data quality on corporate bonds has improved immensely. In the years after 2000 a range of studies assessed the performance of structural credit risk models and found that they were not able to fully explain the size of the average credit spread for corporate bonds. Huang and Huang (2003) suggested (among others) that the remaining non-default-component of the credit spread was an illiquidity premium. Using transaction data this thesis studies the impact of illiquidity and trading frictions on corporate bonds.

The first essay forms the basis of the following two essays and describes in detail how the data from TRACE should be handled and cleaned up before usage. Most other papers using the disseminated data from TRACE lack such a description, are doing an insufficient clean up procedure or have misunderstood how the errors accumulate in TRACE. Both Bloomberg and WRDS which both provide TRACE data fail to remove the majority of the errors. In the essay I present an error filter and show that it is able to clean out almost all errors. I also show how commonly used liquidity measures will be severely biased if nothing is done.

The second essay (co-authored with Peter Feldhütter and David Lando) use the transaction data from TRACE to estimate various corporate bond liquidity measures and asses their performance before and after the onset of the subprime crisis. A linear combination of the Amihud price impact measure, a measure for roundtrip costs and the standard deviation of these

two can explain most of the illiquidity related variation in credit spreads. We further use the measure to shed new light on flight-to-quality, liquidity risk, the impact of trading frequency, the effect of funding shocks to the lead underwriter and the liquidity of bonds issued by financial firms.

The third essay investigates index driven price pressure in corporate bonds. Corporate bond index revisions are completely information free events. Still, the trading activity from index trackers makes the price temporarily change for bonds included or excluded from the index. Both the price pressure return and the following reversal return are significant. In the cases when the reversal returns are also economically significant, bond dealers participate as liquidity providers and trade against their inventory.

## Acknowledgements

I have benefitted greatly from comments and useful suggestions from a number of persons and they are mentioned in each essay, but a few deserve more than a general word of thanks.

First of all, I am indebted to my Ph.D. advisor David Lando for invaluable guidance and help throughout my years as Ph.D. student and even before that. David agreed to become my advisor while I was still a bachelor student, three years before I started my Ph.D. which gave my Master's studies an excellent structure. Furthermore, I thank colleagues and fellow Ph.D. students at the Department of Finance and Center for Statistics for many rewarding discussions and fun hours. In particular, I thank Mads Stenbo Nielsen for always taking the time to listen to my latest findings at our almost weekly five-minute-seminars. Also, I thank Peter Feldhütter for finding at least one weak spot in each and every argument I have ever presented for him.

During my studies, I had the opportunity to spend a great semester at Stern School of Business, New York University, and I thank Yakov Amihud for making the stay possible.

Finally, I thank Søren Hvidkjær and Ilya Strebulaev for participating in my pre-defense and for providing me with a range of critical and useful comments.

Copenhagen, August 2010  
Jens Dick-Nielsen

# Essay 1

## Liquidity Biases in TRACE <sup>1</sup>

Published in the *Journal of Fixed Income* 19(2), 2009.

### Abstract

The transactions database TRACE is rapidly becoming the standard data source for empirical research on US corporate bonds. This paper is the first to thoroughly discuss the assumptions needed to clean the disseminated TRACE data and to suggest that different filters should be used depending upon the application. 7.7% of all reports in TRACE are errors and in some cases up to 18% of the reports should be deleted. Failing to correct for these errors will bias popular liquidity measures towards a more liquid market. The median bias for the daily turnover will be 7.4% and for a quarter of the bonds the Amihud price impact measure will be underestimated by at least 14.6%. Further, calculating these two measures on the same data sample would potentially bias one of them.

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<sup>1</sup>This paper has been published in the *Journal of Fixed Income* 19(2), 2009. I would like to thank Marti Subrahmanyam, Raghu Sundaram, Peter Feldhütter, David Lando, and participants in the "Seminar in Derivatives" at Stern School of Business. All errors are my responsibility.

## 1.1 Introduction

Since July 2002, all corporate bond transactions in the secondary market have been disseminated through the TRACE system (Trade Reporting and Compliance Engine). Before TRACE most empirical corporate bond market studies had to rely on daily quotes and constructed matrix prices for the bonds which could bias the results as discussed in Sarig and Warga (1989) and Dick-Nielsen, Feldhütter, and Lando (2009). With the higher data quality and growing time series dimension of the TRACE database the scope for research applications with corporate bonds has increased immensely. However, 7.7% of the reports in TRACE are errors i.e. reports that are later corrected, cancelled etc. Failing to correct for these errors will bias the two popular liquidity measures, turnover and Amihud price impact, in the direction of a more liquid market. The median error for the average daily turnover is 7.4% and the Amihud price impact measure will be at least 14.6% too low for a quarter of the total sample. These biases can almost be avoided if the TRACE data are cleaned up properly before use, but this cleaning requires certain assumptions about which reports are actually errors. Even after correcting for the reporting errors, liquidity measures depending on the price sequence, e.g. the Amihud price impact measure, might still be biased towards a more liquid market. This happens because of the way so-called agency transactions are registered in TRACE. An agency transaction is a transaction in which a broker facilitates a trade between a customer and another broker. Typically the middle broker passes the bond on to the customer but charges a commission. This commission is not always visible in the TRACE recorded price. When the commission is not visible in an agency transaction two consecutive prices will be the same, because both the interdealer trade between the two brokers and the broker-customer trade are reported with the same price. Since the customer typically pays a commission, whether visible in TRACE or not, this price sequence is misleading. It looks like the broker facilitated a service free of charge or the bond traded without moving the price, neither of which are true. If we eliminate the agency transaction duplicates, the Amihud price impact measure empirically increases. On the other hand, the agency transactions are included in the official FINRA TRACE fact book statistics which means that calculating the turnover measure on the data sample without the agency transactions will bias the turnover downwards when comparing with the fact book statistics. This suggests that the Amihud price impact measure and the turnover measure should not be calculated on the same data sample if the turnover should be comparable to the official statistics. This reasoning is likely to extend more generally to measures depending on the price sequence versus measures depending on the total volume.

Goldstein, Hotchkiss, and Sirri (2007) provides a good description of how they filter the TRACE data for errors. But they have a proprietary version

of the data which enables them to completely eliminate reporting errors (assuming that brokers actually comply with the reporting guidelines). The same complete elimination of errors cannot be done with the disseminated TRACE data, because the disseminated information only allows a partial identification of the reporting errors. The empirical identification then has to rely on a number of assumptions that are usually not stated in research papers. This paper aims at setting a common standard for how to use the disseminated data by clarifying the necessary assumptions and shortcomings of error filters. We set up an algorithm designed to filter out the errors and test the performance against official numbers for the trading activity. Based on this indirect quality test of the filter we are able to filter out almost all errors.

This paper is not the first to point out that bond market data should be used with care. Sarig and Warga (1989) argue that there can be liquidity driven noise errors in daily prices for bonds because prices are given even on days where the particular bond has not been traded for several days. In these cases the broker sets a matrix price based on prices of similar bonds that did trade and issuer characteristics. Differences in what the brokers define as similar bonds lead to a dispersion in the prices reported by different brokers. Dick-Nielsen, Feldhütter, and Lando (2009) show that this matrix pricing bias in prices is still very much present in daily corporate bond prices from Datastream. Bond matrix pricing may be sensible to use for bonds that trade very infrequently or for a fair value price of the bond in a legal sense as under the FAS 157<sup>2</sup>, but it is inappropriate for research on a transaction level. This makes the disseminated TRACE data even more valuable especially for microstructure research of bond market liquidity.

In section 1.2 we give a brief summary of the dissemination history of the TRACE data and explain in depth how the design of the TRACE system accumulates reporting errors. In section 1.3 we explain how and under what assumptions we can identify the errors. We set up an algorithm that filters out the reporting errors and assess the performance of the algorithm. In section 1.4 we look at the potential biases from the reporting errors. Ignoring the errors will make the corporate bond market seems more liquid than it actually is. Section 1.5 explains the problems with the recording of agency transactions in the TRACE system. In some cases it is not enough just to filter out the error reports, it may also be necessary to carefully consider the handling of agency transactions.

## 1.2 Reporting Errors

This section gives a brief introduction to the phases in the TRACE initiative and goes on to describe how trade report filings by brokers accumulate errors

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<sup>2</sup>Statement of Financial Accounting Standards No. 157, September 2006.

in the disseminated TRACE data.

### 1.2.1 Trace History

In 1998 the Security and Exchange Commission (SEC) began reviewing the debt market with a particular focus on price transparency. But as early as 1994 the NASD<sup>3</sup> (now FINRA) started a surveillance system for 50 high yield bonds known as the fixed income pricing system (FIPS). In January 2001 the SEC went on to approve rules that required NASD members to report all over-the-counter (OTC) secondary market transactions in corporate bonds. The actual reporting started in July 2002 with the launch of the TRACE system.

The TRACE system allows the regulatory authorities to keep a complete trail of audit and enhances the transparency of the market by disseminating transactions. The amount of disseminated information has gradually increased since the launch. The reason for not just offering full disclosure from the start was conflicting views about the effect of improving transparency.

1. July 1, 2002  
Dissemination of all investment grade issues with initial issuance above \$1 billion and of the former FIPS bonds.
2. March and April 2003  
Dissemination of all investment grade issues with initial issuance above \$100 million and at least an A- rating. In addition to the FIPS speculative grade bonds another 120 BBB rated bonds were added for dissemination.
3. October 1, 2004.  
Trades in all bonds are disseminated (99% in real time).
4. November 3, 2008.  
Buy-Sell side information is disseminated for trades from this date on.

Since July 2005 all transactions must be reported within 15 minutes of the actual transaction.

### 1.2.2 Reporting and Dissemination

According to the FINRA rule 6700 series (formerly NASD rule 6200 series) all members are required to report an OTC corporate bond transaction in the secondary market using the TRACE system. In case of a buy or a sell from a customer the broker that facilitates the trade has to file a report with detailed information about the transaction. If two brokers trade with each

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<sup>3</sup>National Association of Securities Dealers now called The Financial Industry Regulatory Authority.

other (i.e. an interdealer trade) both of them file a report resulting in two reports referring to the same transaction. However, it is important to note, that only one of the two interdealer reports is actually disseminated<sup>4</sup>.

The TRACE system is essentially a one-day system and reporting is only possible within the system operating hours<sup>5</sup>. This means that a reporting error can easily be corrected if the correction is made within the same day as the report was filed. In this case the broker files a new report, but the old report containing the error still remains in TRACE. In the disseminated data this yields two reports. The first report contains the error, the second report either cancels or corrects the wrong report but none of them replaces the first report. Panel A of table 1.1 shows an example of a same-day cancelation of a reported trade and panel B shows a same-day correction of a reported trade. It is easy to identify both the cancelation and correction through the trade status of the report and to link them to their original reports through the original message sequence number, which is unique on an intraday level. With a cancelation as in panel A we should delete both reports, whereas with a correction as in panel B we should delete the original report and keep the correction.

Bond ID	Date	Time	Price	Par volume	Yield	Sequence Number	Original SN	Status
Panel A								
AA.HM	20070105	15:05:57	99.356	800000	5.3167	17473	.	T
AA.HM	20070105	15:05:57	99.356	800000	5.3167	17478	17473	C
Panel B								
AA.HM	20070112	12:21:45	99.404	600000	5.2690	10405	.	T
AA.HM	20070112	12:21:45	99.381	600000	5.3100	11788	10405	W

**Table 1.1: Disseminated Trade Reports with same-day corrections.** This table contains two typical examples of how same-day corrections are disseminated in TRACE. Panel A shows a cancelation of a trade. This can be seen from the 'C' in trade status of the second report. The original report can be identified by the original message sequence number. Panel B is a correction of an original report. This can be seen from the 'W' in the trade status for the second report in panel B. The table only displays a selection of the disseminated information about each report. These are bond ID, Date, Time, Price, Par Volume, Yield, Message Sequence Number, Original Message Sequence Number and Trade Status.

Trade reports filed on a later day than that of the actual transaction still contain the information of the actual transaction<sup>6</sup> but are marked in

<sup>4</sup>See TRACE FAQ on Reporting at the FINRA TRACE website.

<sup>5</sup>Reporting outside system hours follows special rules.

<sup>6</sup>The date and time of the actual transaction and not the date and time of the filing of the report.

the disseminated data as an as-of trade. This plays a role for reporting errors detected on a later date. If a broker has to cancel a report filed on an earlier date, this is done by filing a report *identical* to the original report but marked as a reversal<sup>7</sup>. A correction on a later date is done by first filing the reversal, thereby canceling the report containing the error, and then the broker has to file the right report but now marked as an as-of trade. In the disseminated data a reversal implies that we should delete both the original report and the reversal report. Table 1.2 shows an example of a report that has been canceled by a reversal and then followed up by an as-of trade report containing the correct information. In this case we cannot identify the original report by its message sequence number, because it is an intraday number and the reversal and the original are not filed on the same day. Instead the original report is identified as a report that is identical to the reversal (except that the original is not marked as a reversal). Note how small the error is in the original trade report compared to the correction both for the same-day correction and for the reversal. It is not clear that we could have identified any of these trades as being errors based on the prices or yields alone. In this sense most errors in TRACE are not outliers deviating from the surrounding reports.

Bond ID	Date	Time	Price	Par volume	Yield	As-Of
CSCO.GB	20060221	15:21:42	100.348	1000000	5.17050	
CSCO.GB	20060221	15:21:42	100.348	1000000	5.17050	R
CSCO.GB	20060221	15:21:42	100.348	1000000	5.17042	A

**Table 1.2: Disseminated Trade Reports with non-identical reversals.** This table contains a typical example of how a reversal is disseminated in TRACE. The reversal report is an identical copy of the original report but with a 'R' in the as-of indicator. Some reversals are followed up by an as-of report indicated by the 'A' in the as-of indicator. The follow up report then contains the correct trade report information. The table only displays a selection of the disseminated information about each report. These are bond ID, Date, Time, Price, Par Volume, Yield and As-Of indicator.

Failing to delete the error reports will yield a substantial amount of double counting<sup>8</sup>. We show in section 1.4 that this double counting will severely overstate the trading activity and depth of the market.

<sup>7</sup>See FINRA (2008) page 31.

<sup>8</sup>In a strategic analysis of the transaction prices it is debatable which of the reports to use but not without taking a stand on whether or not the market found the given report credible or not at the time of the filing of the original report.



### 1.3 Error Filter

This section describes a simple algorithm that detects and deletes the reporting errors. Based on the disseminated transaction information it is not possible to set up a perfect filter, so we test the performance of the proposed error filter by comparing with the statistics from the official TRACE fact book.

#### 1.3.1 Description

The filtering of the reporting errors in the disseminated data takes place in three steps.

**1** Deleting true duplicates.

In the disseminated data each report has an intra-day unique message sequence number. We delete any duplicates identified by the message sequence number<sup>9</sup>.

**2** Deleting reversals.

Since reversals are typed in later than same-day corrections we start by deleting those which are newest in a chronological sense. All reports marked as a reversal are deleted and for each reversal we also delete the original report. Each reversal should exactly match one original report<sup>10</sup>.

**3** Deleting same-day corrections.

There are two types of same-day corrections in the disseminated data. These can be identified by the trade status of the report. If the correction is a cancelation, both reports should be deleted and if it is a correction only the original should be deleted. Contrary to reversals the original can be identified through the original message sequence number which is given as part of the correcting report.

#### 1.3.2 Stylized facts

Even though the outline of the filter is quite simple in principle, it is not possible to actually implement it in that exact form. Particularly the second step of the algorithm can be problematic. When a broker files a reversal she

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<sup>9</sup>A duplicate in this step indicates that two interdealer reports have been disseminated, even though only one should have been according to the description of the disseminated data. In the TRACE data disseminated through WRDS, this is no longer a big issue. However, it used to be. At some point in 2007 the database was altered and almost all these duplicates removed. Before that point in time almost 25% of all reports were duplicates of this type.

<sup>10</sup>It is not possible to partially reverse a trade report or to have a reversal canceling more than one original report. See FINRA (2008) page 31.

has to supply a 10-digit TRACE-assigned control number from the original trade allowing the SEC to keep a linkage between the reversal and the original report. The only way to make the same linkage within the disseminated data, between an original trade report and a reversal, is to rely on brokers filing the reversal as an exact replica of the original (as they should do according to guidelines<sup>11</sup> and as we saw it in table 1.2). But not all reversals can be matched with an identical original report. In panel A of table 1.3 we have a reversal without any identical report but with a possible original report. These two reports differ on two variables. The differences may just be a matter of rounding, but more than one report may match if we were to round the numbers. In panel B we have another reversal with a possible original report. Again the two reports are not identical<sup>12</sup>. In these two examples the reversals differ on three variables all together and in the second example the original cannot be matched by rounding either the reversal or the original. This makes it hard to set up a rule for identifying an original report when the reversal does not match any reports exactly.

Bond ID	Date	Time	Price	Par volume	Yield	As-Of
Panel A						
CSCO.GB	20060412	11:39:03	99.250	3000000	5.42600	
CSCO.GB	20060412	11:39:00	99.250	3000000	5.42635	R
Panel B						
CSCO.GB	20060215	13:45:33	99.809	1MM+	5.29398	
CSCO.GB	20060215	13:45:33	99.809	2000000	5.29398	R

**Table 1.3: Disseminated Trade Reports with non-identical reversals.** This table contains two examples of reversals for which the original trade report is not identical to the reversal. This makes the identification of the original report hard or even impossible. The table only displays a selection of the disseminated information about each report. These are bond ID, Date, Time, Price, Par Volume, Yield and As-Of indicator.

In the TRACE User Guide it is stressed that one reversal report can only cancel one original report. This may be the rule, but in some cases as in table 1.4 it could be, that the reversal is meant to cancel more than just one report, since the reversal matches four other reports. It is rather unusual in the data to have more trades in the same second with matching prices

<sup>11</sup>According to FINRA (2008) page 31 a reversal should be exactly identical to the report it is reversing.

<sup>12</sup>The second example in table 1.3 is due to a rating change between the date of the first report and the date of the reversal report. Trading volumes are censored for investment grade bonds at \$5 millions and for speculative grade at \$1 million.

but different quantities. This makes the trade sequence seems suspicious. On the other hand one could argue that a broker might be chopping up a deal into smaller pieces but actually selling it all to just one customer for some reason. If this is the case the price sequence is misleading since the negotiated price is not for separate small trades but for a larger package i.e. a larger volume, in which case we do not have 9 consecutive trades with the same the price but one larger trade.

Bond ID	Date	Time	Price	Par volume	Yield	As-Of
CSCO.GB	20060217	14:25:00	101.000	10000	5.02100	
CSCO.GB	20060217	14:25:00	101.000	10000	5.02100	
CSCO.GB	20060217	14:25:00	101.000	20000	5.02100	
CSCO.GB	20060217	14:25:00	101.000	20000	5.02100	
CSCO.GB	20060217	14:25:00	101.000	10000	5.02100	
CSCO.GB	20060217	14:25:00	101.000	20000	5.02100	
CSCO.GB	20060217	14:25:00	101.000	10000	5.02100	
CSCO.GB	20060217	14:25:00	101.000	10000	5.02100	R
CSCO.GB	20060217	16:01:09	101.000	10000	5.02100	

**Table 1.4: Disseminated Trade Reports with non-identical reversals.** This table shows an example where the reversal matches more than just one trade report. According to TRACE guidelines one reversal is only meant to cancel one original report. The table only displays a selection of the disseminated information about each report. These are bond ID, Date, Time, Price, Par Volume, Yield and As-Of indicator.

On a more technical note the disseminated TRACE data from WRDS do not include the filing date of the reports, which would be helpful when we want to match the reversal with the original report<sup>13</sup>. In panel A of table 1.5 the reversal matches the as-of trade report and not what is likely to be the original report. In this case the bond has gone from a speculative grade rating to an investment grade rating in the time between the original report filing and the filing of the reversal report. At the time of the original filing the volume then was censored at \$1 million dollars whereas at the time of the reversal report the censoring was at \$5 million. Looking at the message sequence numbers (not shown in the table) the reversal and the as-of trade have consecutive numbers with the reversal having the lower number. This indicates that the reversal is filed before the as-of trade report and therefore meant to cancel the third report in panel A<sup>14</sup>. In panel B the as-of trade matching the reversal was filed with a wrong time record. Then the reversal

<sup>13</sup>The filing date is actually public information in the way that it is disseminated for example as part of the FISD time sales data.

<sup>14</sup>There is a possibility of the two reports being filed at different days and still getting consecutive number, but it is a rather small possibility. This question could be resolved with the information about filing date as for example FISD contains. If the reports are

is canceling the as-of trade that it matches and the last as-of trade is the one with the correct time record. In this case it is safe to assume that the reversal is referring to the as-of trade. In panel C of table 1.5 it is again not clear which report the reversal is meant to cancel. The reversal does not actually match any of the other reports but comes closest to matching the as-of report. As in panel A the message sequence numbers indicate that the reversal is filed before the as-of trade in which case it could not be canceling the as-of trade. It then seems most likely that the reversal is meant to cancel one (or both) of the other reports. In conclusion, since we are not allowed to see the direct link between the reversal and the original report in the disseminated data, the filing date (which is public information but not provided by WRDS) would be helpful when making the indirect link.

Bond ID	Date	Time	Price	Par volume	Yield	As-Of
Panel A						
CSCO.GB	20060215	15:01:40	100.028	5000000	5.24356	R
CSCO.GB	20060215	15:01:40	100.028	5000000	5.24356	A
CSCO.GB	20060215	15:01:40	100.028	1MM+	5.24356	
Panel B						
CSCO.GB	20060330	12:54:00	99.370	25000	5.39700	R
CSCO.GB	20060330	12:54:00	99.370	25000	5.39700	A
CSCO.GB	20060330	13:12:00	99.370	25000	5.39700	A
Panel C						
CSCO.GB	20060828	16:53:00	99.656	15000	5.33700	
CSCO.GB	20060828	16:53:00	99.656	15000	5.33700	
CSCO.GB	20060828	17:13:57	99.656	15000	5.33702	R
CSCO.GB	20060828	17:13:57	99.656	15000	5.33700	A

**Table 1.5: Disseminated Trade Reports with reversals of as-of trades.** This table shows three examples where it is not clear which reports the reversals are meant to cancel. It could look like the reversals are canceling the as-of reports, which is only true in panel B. The table only displays a selection of the disseminated information about each report. These are bond ID, Date, Time, Price, Par Volume, Yield and As-Of indicator.

When using the disseminated TRACE data from WRDS we make the following choices when implementing the filter. If there is no identical match for a reversal based on the parameters shown in table 1.3-1.5 we only delete the reversal because the original report cannot be identified with certainty. Second, if there is more than one trade report, that matches the reversal

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filed on the same day and the reversal has the lower message sequence number then the reversal was filed before the as-of report for sure. In this case the reversal could not be canceling the as-of trade, since the as-of trade was not yet filed.

we delete all of them not including any as-of trades that might match the reversal. That is, we do not delete any as-of reports that match the reversal on the chosen set of parameters. Then we would not make a mistake in panel A and C of table 1.5 but we do make an error in panel B of the same table.

We delete too few reports when we cannot find an original report and when we do not delete as-of trades that are later reversed. On the other hand we delete too many reports when we delete all ordinary reports that match a reversal.

### 1.3.3 Performance

Each year FINRA publishes a TRACE Fact Book with summary statistics of trading over the past year. We test the performance of our error filter by matching the number of trades post filtering for the 10 most frequently traded investment grade bonds in 2007 with the numbers for the same bonds from the official fact book. In table 1.6 we can see that the algorithm performs fairly well. The most traded bond that year was a General Electric bond with an official number of 12,857 trades. In the raw data file the same bond has 13,479 trade reports, but after the filtering the number of trade reports is 12,856, i.e. only 1 report short of the official number. Apparently, there is some discrepancy between the fact book of 2007 and the number of disseminated reports in that the second most traded bond according to the fact book was a Morgan Stanley bond. However for unknown reasons, the raw TRACE file only displays one third of the transactions reported for the bond in the TRACE fact book. All of the other bonds are close to the official number of trades. The largest deviation is 1.5%. Looking at the sign of the deviations we can see that the filter most commonly deletes too few observations. This happens because the dominating problem is that we cannot match a reversal with an original report. For the few bonds where we delete too many observations some reversals have matched more than just one original report. When we compare this with the number of unmatched reversals the latter problem seems most important. If we only deleted one report each time we had an identical match to a reversal and refrained from deleting more than one if there were more matches, we would still get a small error percentage for the filter.

The first step of the algorithm deletes 2,532 reports<sup>15</sup>. These are the reports which match in pairs on all parameters including the unique intra-day message sequence number. When two reports filed on the same day have matching message sequence numbers it means that they are referring to the same trade and one of them has to be deleted if we want to avoid double counting. These reports are interdealer trades where both reports are accidentally disseminated for some reason. In the second step of the algorithm we delete reversals and matching original reports. There are a

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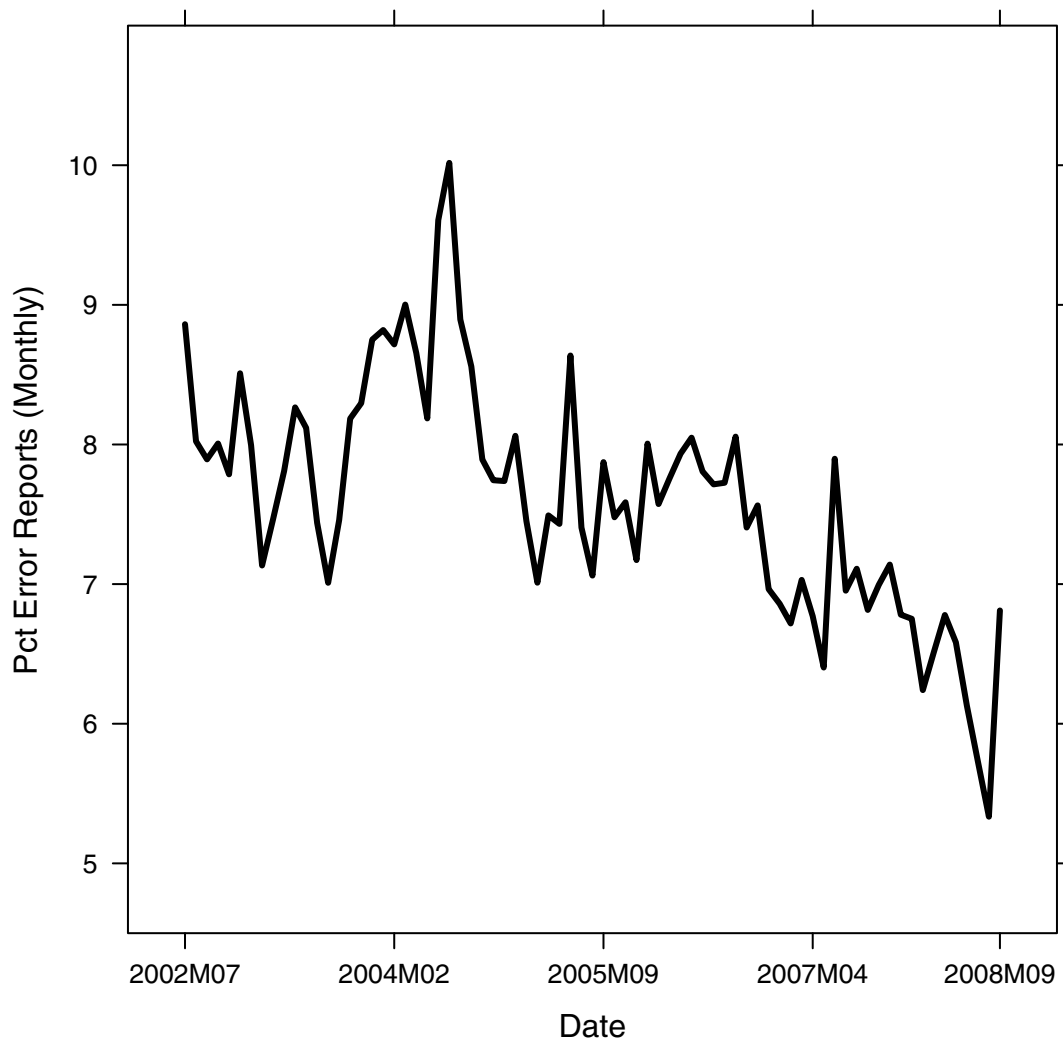
<sup>15</sup>Our data sample covers transactions up to and including 2008Q3.

Symbol	Issuer Name	Coupon	Maturity	Actual	Raw Reports	Post Filter	Rev. Missing	Dev. Pct.
GE.ADF	GE Company	5.000	2/1/13	12,857	13,479	12,856	12	-0.01
MS.QP	Morgan Stanley	4.750	4/1/14	12,333	3940	3788	11	-
GS.OU	Goldman Sachs Group	5.700	9/1/12	11,573	12,061	11,577	16	0.03
C.HEF	Citygroup	5.000	9/15/14	11,212	11,693	11,217	19	0.04
GE.AAD	GE Capital Corp.	6.000	6/15/12	11,085	11,511	11,086	21	0.01
BLS.HW	Bellsouth Corp.	6.000	11/15/34	10,450	11,072	10,594	148	1.38
WMT.HN	Wal-Mart Stores	4.550	5/1/13	9,681	10,052	9,678	18	-0.03
GE.WB	GE Corp.	5.875	2/15/12	9,468	9,957	9,470	16	0.02
GS.WL	Goldman Sachs Group	5.625	1/15/17	8,108	8,655	8,115	15	0.09
JPM.QP	J. P. Morgan Chase	5.750	1/2/13	8,051	8,365	8,048	10	-0.04

**Table 1.6: Performance of the error filter.** This table shows the error filter performance on a selection of bonds for which the actual number of trades are known from the Trace Fact Year Book 2007. The selected bonds are the top 10 most traded investment grade issues in 2007. The differences between the filtered disseminated data and the actual number of trades arise because a perfect filtering is not possible using only the disseminated data. In the table 'actual' refers to the actual number of trades, 'raw reports' refers to the number of reports disseminated in Trace, 'post filter' refers to the number of reports or trades left after applying the filter, 'rev. missing' refers to the number of reversals where it was not possible to find an identical original report and 'the deviation in percentage' is between the actual number of trades and the post filtered amount.

total of 418,626 reversal reports and we end up deleting 763,013 reports in this second step. As in table 1.6 we are not able to match all of the reversals with an original report. Table 1.7 shows a summary of the filtering process. Note that a total of 120,420 reversals remains unmatched in the filtering. Finally, in the third step we delete same-day cancelations and corrections. We end up with a total of 26,943,152 trade reports having deleted 1,404,978 reports in the third step. The database contains a total of 29,113,675 raw reports of which we have dropped 7.5% in our filtering. The official error rate is 7.7% which is slightly higher than ours since we still lack to match some reversals to their original reports.

When introducing a new reporting system such as the TRACE system it is natural for the users to make errors simply because they are not familiar with the system yet. Figure 1.1 shows a time series plot of the monthly reporting error rate. There is a clear downward trend in the error rate over time, but it is still far from zero. So even if we decided only to look at a new data sample from TRACE, we would still need to filter the data before use.



**Figure 1.1: Monthly Percentage of Error Reports.** This graph shows the monthly percentage of error reports in the disseminated TRACE data. There is a small decline in errors over time. The error rate peaks after the start of the last phase of the dissemination in 2004Q4.

Description	Trade reports
Raw reports	29,113,675
Step 1	
Deleted	2,532
Post step 1	29,111,143
Step 2	
Deleted	763,013
Reversals	418,626
Unmatched Reversals	120,421
Post step 2	28,348,130
Step 3	
Deleted	1,404,978
Post step 3	26,943,152

**Table 1.7: Filtering Summary.** This table shows how many reports that are deleted in each step of the error filter and for step 2 the table lists the number of unmatched reversals. The unmatched reversals are the main problem for the filter as seen in table 1.6

## 1.4 Error Impact

In this section we first show that ignoring the reporting errors will bias some of the most commonly used liquidity measures. Secondly, we show that replacing the error filter with a standard stock market filter is not a viable alternative. In stock market research there are well tested filters which screen the price sequences for outliers. However, applying a similar approach to the TRACE data will have almost no effect on the biases from section 1.3.

### 1.4.1 Market Liquidity

If the TRACE data are not cleaned up before use, the number of transactions will be too high. For many research applications this will lead to a bias in the results<sup>16</sup>. Furthermore, the reporting error percentage is increasing in the trade size of the bonds. In table 1.8 the error percentage is listed for different trade sizes. For trades of par value \$1,000,000 and above the amount of error reports is 13.2%. Most studies find larger trades more interesting than smaller trades, because these trades are likely to have been carried out by well informed institutional traders with high bargaining power and 90%-

<sup>16</sup>This section is in part inspired by the SAS-programs that WRDS has on their website. These are very helpful for first time users but they are ignoring the problems with reporting errors. Using their code for research *will* result in the biases of this section.



95% of all trading measured by volume takes place in institutional trade sizes (typically a institutional trade is defined as a trade with par value size above \$100,000).

Trade Size	Error Pct
-5,000	5.27
5,000-10,000	4.60
10,000-25,000	4.71
25,000-250,000	6.86
250,000-1,000,000	11.68
1,000,000-	13.24

**Table 1.8: Errors as a function of trade size.** This table shows the error percentage as a function of the trading size. As the trading size increases so does the number of corrections and cancelations.

A typical measure of liquidity is the turnover of an asset. We define the turnover for a bond as the daily average of total trading volume taken over days with at least one trade<sup>17</sup>. When the error reports are deleted from the raw TRACE data the turnover will be lower. That is, if we do not delete the errors the trading activity on the market will appear higher than it actually is. From table 1.9 we can see that the median turnover deviation is 7.2%. A large part of the bonds have the same turnover measure before and after the filtering simply because these bonds have very little or no reports deleted in the filtering. But at the other end of the scale a quarter of the bonds will have the turnover overestimated by more than 14%.

Another popular measure of liquidity is the Amihud price impact measure. We calculate a quarterly measure for each bond by first calculating a daily Amihud measure and then taking the median across days with a non-zero measure. The daily measure is given on the form:

$$\text{Amihud}_{bond,t} = \frac{1}{N} \sum \frac{|r_{ij}|}{Q_j}$$

where N is the number of trades on day t for the bond,  $r_{ij}$  is the return between consecutive trades  $j$  and  $i$  and  $Q_i$  is the dollar par volume for trade  $i$ . Note that taking the median rather than the mean across the quarter makes the measure far more robust. But even with this robust definition of the Amihud measure a large part of the bonds have a too low measure pre filtering. In table 1.9 we can see that the median bias is at 0.0% but that for more than a quarter of the quarterly measures the bias is as high

<sup>17</sup>This definition is taken from one of the example programs for TRACE on the WRDS website. Since the majority of the bonds trade infrequently a more proper definition would be to take the average over all days.

as 14.6%. The same bias is 34.5% for the Amihud measure calculated using only institutional trade sizes. A lot of the error reports are duplicates of the original reports (at least on the part of the report that enters in the calculation of the Amihud measure). Inserting these duplicate reports in the price sequence underestimates the price impact, because there will be too many zero returns between consecutive trades.

We have only showed that there will be a bias for the turnover measure and for the Amihud price impact measure, but the bias is likely to extend to all liquidity measures based on either the trading activity or the price sequence.

Quantile	Turnover	Amihud All	Amihud Large
Q3	14.3%	-14.6%	-34.5%
Median	7.2%	-0.0%	-6.9%
Q1	0.1%	-0.0%	-0.0%

**Table 1.9: Bias distributions.** This table shows the distribution of the size of the bias in percent. The bias is calculated as the deviation between the measures calculated with and without filtering the data. The bias distribution is calculated for the turnover, the Amihud measure using all trades and the Amihud measure using only trades above \$100,000 in par volume.

### 1.4.2 Price Sequence Filter

It is typical in stock market research to set up a filter based on the price sequence. These filters aim at detecting outliers that potentially would bias results. Typically an observation is classified as an outlier if the price falls outside a certain price range, because the stock might then be trading under special conditions, or if the price deviates too much from the other prices close in sequence. It makes good sense to use a price filter like this to ensure that research results are not influenced by a small number of questionable observations. The same kind of filter can be applied to the TRACE data but it is in no way a substitute for the error filter from section 1.3. The price sequence based filter could be motivated by the desire to detect broker typos. But it is actually hard for a broker to file a trade report to the TRACE system containing a typo. Once a broker files a report to the TRACE system, the system automatically assesses the report and compares the reported price to other reported prices for the same issue. If the price falls outside a range based on these prices the report is not approved and the broker has to either change the price or overwrite the system. In case that the broker overwrites the system and files the report anyway, the system then widens its price

range. If the price is still outside this new range the report is dismissed again and the broker has to report and explain the price by phone to FINRA<sup>18</sup>.

In order to test the performance of a typical price based filter we apply the price filter<sup>19</sup> from Han and Zhou (2008). We identify an outlier and delete it using the following criteria: trade size is missing or zero; price is less than \$1 or greater than \$500; price is more than 20 percent away from the median price in a day; or price is more than 20 percent away from the previous trading price. In table 1.10 we can see the cross section between the price sequence based filter and the error filter from section 1.3. Only a very small number of reports are categorized as errors/outliers with the price sequence based filter and of the reports deleted by the price sequence filter only 19% percent were also marked as errors by the error filter from section 1.3. More than 99.6% percent of the errors detected by the error filter from section 1.3 did not show up in the price sequence based filter. This shows that the price sequence based filter can in no way substitute for the original error filter. The majority of the reporting errors are still present after applying the price sequence filter. Still it may be valuable to apply a price sequence based filter after the error filter in order to delete potential influential outliers. These outliers may just be actual trades that traded under some special conditions. We will briefly return to the question of special conditions below.

	EF: Error	EF: Non-Error	Total
PSF: Error	7,782	33,548	41,330
PSF: Non-Error	2,162,741	26,909,604	29,072,345
Total	2,170,523	26,943,152	29,113,675

**Table 1.10: Price sequence based filter.** This table shows how the error filter (EF) from section 1.3 and the price sequence based filter (PSF) from section 1.4.2 intersect.

## 1.5 Remaining Issues

Even after we have applied the error filter there may still be a substantial bias in some liquidity measures. This is due to certain reporting rules special for the TRACE system. The Amihud price impact measure will have a median downward bias at 7.7% in the post filter sample, which actually increases the formerly stated bias when ignoring all errors. In this section we explain

<sup>18</sup>See FINRA (2008) page 17.

<sup>19</sup>This price filter is just one of the filters that Han and Zhou (2008) use to clean up the data.

why using the post filter sample is not enough to ensure unbiased results. Further filtering will however, cause a bias in the turnover measure, when comparing to the official fact book statistics. If the goal is to match the turnover statistics from the fact book, then we should not make any further filtering. For studies of liquidity where the price sequence matters we will argue that further filtering is required. This suggests that the Amihud price impact measure and the turnover measure should not be calculated on the same data sample.

### 1.5.1 Agency Transactions

In the post filter data sample there is still a lot of double reports. These reports match on every parameter except the message sequence number, which means that they are *not* referring to the same transaction i.e. they are not the result of double reporting in interdealer transactions<sup>20</sup>. The double reports are for the most part a result of agency transactions where an introducing broker passes on a trade from an executing broker to a customer. In general, transactions between the broker and a customer can either be a principal transaction or an agency transaction. In a principal transaction the dealer trades with the customer against her own inventory. In an agency transaction the dealer does not have the bond in question on inventory, but buys it from another dealer (from the other dealers view this is a principal transaction) and passes it on to the customers account. In this transaction the dealer that has the contact with the customer is the introducing dealer and the dealer selling the bond to the introducing dealer is denoted the executing dealer. This agency transaction requires three reports to be filled to the TRACE system and two of the reports will be disseminated. First the interdealer transaction gives two reports (one principal sell and one agency buy) and then the introducing dealer files an agency sell report<sup>21</sup>. The introducing broker is most commonly compensated by taking a commission on the sale but technically passes the bond on at the same price as she bought it at without a markup or a markdown. In TRACE bond prices are disseminated as the price including any commission (see FINRA (2008) page 49). A separate field indicates whether or not the price includes a commission but the actual size of the commission is not reported. For smaller

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<sup>20</sup>One could conject that these reports are then unmatched interdealer reports which by accident are reported as two separate transactions. However, brokers filing unmatched interdealer reports are alerted by the TRACE system, which should eliminate the possibility of such unmatched reports (see FINRA (2008) page 41).

<sup>21</sup>Green, Hollifield, and Schürhoff (2007a) provide an excellent discussion on reporting problems with agency trades in the municipal bond market and the same discussion applies to the corporate bond market. However, the problem with agency transactions is likely to be more pronounced for research dealing with the TRACE data because Green, Hollifield, and Schürhoff (2007a) investigate the primary market whereas TRACE has reports on the secondary market where agency transactions are far more common.

trade sizes a commission can have a huge impact on the actual price, but since we do not know the size of the commission it may be preferable to just delete reports that include commission. But the introducing broker does not always charge a commission per trade instead the customer can have an account with the broker where he pays an annual wrap fee which is usually some percentage of the value of the assets on the account. In these cases the broker transfers assets to the account without taking any transaction based commission. Since the broker is only allowed to include *bona fide* commissions when reporting to TRACE<sup>22</sup> these agency transactions appear to have no commission in TRACE. In the disseminated TRACE data this will give two reports that are identical on all aspects except the message sequence number.

Note that based on table 1.6, the post filter data can be regarded free of any error reports, since we are very close to replicate the official number of trades in the TRACE fact book. But this is before we consider deleting any further duplicates that come from agency transactions. If we want to compare our estimates with the same data sample that is used in the official statistics on for example the total market turnover we should not delete any more reports. In the official turnover statistics an agency transaction counts as two transactions. This means that we are going to underestimate the turnover if we delete one of the duplicates. On the other hand the Amihud price impact measure will be too low thus giving us a bias in the direction of a more liquid market<sup>23</sup>. How we treat the agency transactions clearly should depend on what the data are used for.

Quantile	Turnover	Amihud All	Amihud Large
Q3	9.7%	-34.3%	-15.4%
Median	4.9%	-7.7%	-0.0%
Q1	1.4%	-0.0%	-0.0%

**Table 1.11: Agency transaction bias distributions.** This table shows the distribution of the differences between the post error filter data and the post error filter data with only one agency transaction report per agency trade. The Amihud measure will be biased downwards if the agency trades are not removed. The bias is most significant for the sample using all trades since agency trades are likely to be small in trade size. The turnover will be lower once the agency transactions are taken out. However, this will bias the turnover downwards compared to the FINRA statistics.

<sup>22</sup>See FINRA (2003).

<sup>23</sup>It is possible to construct a price sequence where this reasoning does not hold and the Amihud measure would be too high, but empirically the Amihud measure does increase in value when we delete the agency trades.

Each agency transaction with no commission returns a zero return for consecutive trades (empirically) lowering the Amihud measure. This bias can be avoided by deleting one of the agency reports but remember that this just causes another bias in the turnover. In table 1.11 we can see that this approach (agency filtering) will raise the Amihud measure based on all trades by 7.7% for the median bond when we compare to the Amihud measures based on the post error filter sample. The agency effect is greater for the measure based on all trades since agency trades are most likely to be small in trade size. One explanation could be that retail investors only have accounts with one broker whereas larger institutional traders have a better opportunity to screen the market by themselves thereby mitigating any agency fee. If we were to calculate the daily turnover on the same sample as the Amihud measure this will lower the turnover by 4.9% for the median bond. But as discussed before this will be a bias compared to the FINRA statistics.

Another problem with the agency transactions is that we cannot identify them directly in the disseminated data because the agency trade indicator is not disseminated. We can only indirectly identify them as the duplicates and the reports that include a commission. The buy-and-sell side information is only disclosed from November 3th of 2009 and onwards. In table 1.12 we take a look at the distribution of trading partners in all pairwise duplicate transactions of November and December 2009. According to our agency transaction identification argument these trades should all be pairs of one interdealer trade and one dealer-costumer trade. As can be seen from the table this is the case for 71% of the pairs. The remaining pairs consists of 16% with two interdealer trades and only 13% with two dealer-costumer trades. Primarily the latter kind of pairs are wrong to delete since they are not part of any agency transactions as we have defined them. The transaction pairs with two interdealer transactions with the same information are usually part of a special construction eg. a one-sided automatic give up reported with anonymous partners (see FINRA (2008) page 23.). As with agency transactions it seems reasonable to delete one of the interdealer reports. The error rate using our approach then lies around 13% when identifying the agency transactions. Once buy-sell side information is known this kind of errors can of cause be avoided.

Not all pair of agency reports are filed with the same transaction time record. A lot of the transactions have 1 to 30 seconds between the first and the second report. So if we really want to get rid of the agency report bias, we should look for identical reports within for example a 60 second time slot. After the first error filter we have 26,943,152 trade reports left. If we delete one of the two reports in an agency transaction but require that the trades match within the second we are left with 24,702,916. Widening the time frame to 60 seconds reduces this number to 23,824,156. Only deleting the agency trades matching exactly on the second still leaves a substantial

Trader Identity	Transaction pairs	Percentage
2 Dealer-Costumer	10,499	12.8%
Interdealer/Dealer-Customer	58,614	71.5%
2 Interdealer	12,855	15.7%

**Table 1.12: Distribution of pair reports.** This table shows the distribution of the trading partners in pair trades executed with the same information ie. two trades with the same volume, price, time stamp etc. The sample is taken from November and December of 2009, at which point the buy-and-sell side information was disclosed. The pair transactions can either be interdealer/interdealer, interdealer/dealer-costumer, or two dealer-costumer.

amount of agency transactions left. Of cause since we cannot actually identify an agency report we cannot be absolutely sure that the 60 second range is the right approach.

### 1.5.2 Special Conditions

A final thing to consider before using the data is whether the bond in the particular trade was trading under some form of special condition. In the disseminated data an indicator tells if the report is for a trade with special conditions.

The most usual special condition is that the bond has an odd number of days to settlement (i.e. different from 3 days). It could also be special conditions classified in TRACE as a cash trade, the price was a weighted average, the seller had some kind of special option etc.

## 1.6 Conclusion

The disseminated corporate bond transactions data from TRACE have to be cleaned up before use. Any result obtained with the raw data file stands a chance of being biased. We show that two popular liquidity measures are biased towards a more liquid market if the reporting errors are not filtered out of the data. The median bias for the turnover is around 7.4% and for a quarter of the bonds the Amihud price impact measure will be more than 14.6% too low. Just filtering out the reporting errors is not enough to ensure that the results will be unbiased. The filing rules for agency transactions can also cause a bias in some cases. Deleting the duplicate agency transaction reports possibly reduces this bias for measures where the price sequence is important, as in the case with the Amihud measure. However, one has to be careful before deleting the agency transactions if the total turnover

is important. The agency trades are part of the official turnover statistics from the TRACE fact book. Deleting them will give a bias when comparing turnover with the official FINRA statistics.

With the disseminated data none of the filters can be constructed to eliminate all errors because the information linking the transactions together is not disseminated. Our filter relies on a number of assumptions that are needed in order to indirectly identify the reports with errors. All of the assumptions we use are fairly conservative and since we are able to closely replicate the official trade statistics, the assumptions seem appropriate<sup>24</sup>.

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<sup>24</sup>SAS programs, with both kinds of filters implemented and easy to use, are available from the author upon request.



## Essay 2

# Corporate Bond Liquidity Before and After the Onset of the Subprime Crisis<sup>1</sup>

Co-authored with Peter Feldhütter and David Lando, Copenhagen Business School.

### Abstract

We analyze liquidity components of corporate bond spreads by combining the superior data quality of transaction-level corporate bond prices from TRACE with the increase in bond spreads caused by the crisis. A single linear combination of four liquidity proxies captures most of the liquidity-related variation of spreads before and during the crisis. The contribution to spreads from illiquidity increases dramatically with the crisis. We use our measure to shed new light on flight-to-quality, liquidity risk, the impact of trading frequency, the role of funding shocks to lead underwriters, and the liquidity of corporate bonds issued by financial firms.

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<sup>1</sup>We thank Yakov Amihud, Sreedhar Bharath, Michael Brennan, Tom Engsted, Edie Hotchkiss, Marco Pagano, Lasse Pedersen, Ilya Strebulaev and seminar participants at seminars at the Goethe University in Frankfurt, Deutsche Bundesbank, ECB, Oesterreichische Nationalbank, CBS, NYU, and at conferences in Bergen (EFA), Konstanz, Florence, London and Venice for helpful comments.

## 2.1 Introduction

The onset of the subprime crisis caused a dramatic widening of corporate bond spreads. In light of the strong evidence that illiquidity in addition to credit risk contributes to corporate bond spreads, it is reasonable to believe that at least part of the spread widening can be attributed to a decrease in bond liquidity, and perhaps to an increase in liquidity *risk* as well. To show this we need robust measures of liquidity and liquidity risk which enable us to disentangle the credit risk component and the liquidity component of corporate bond spreads. Ideally, a robust measure should be significant before and after the crisis, and we would expect it to reveal a strong decrease in liquidity around the onset of the crisis.

We show in this paper that a sum of four liquidity proxies has been a consistent contributor to corporate bond spreads both before and after the onset of the crisis and across rating categories. The four variables are Amihud's measure of price impact, a measure of roundtrip cost of trading, and the variability of each of these two measures. We can think of the Amihud measure and the roundtrip cost measure as measuring liquidity, and the two variability measures as representing liquidity risk.

We arrive at our liquidity measure through a principal component analysis which reveals that the first principal component among eight liquidity variables is almost the same before and after the onset of the crisis and it is close to being an equally weighted sum of the four variables mentioned above. When we regress corporate bond spreads on the principal components, and control for credit risk, only the first component contributes to corporate bond spreads consistently across ratings and regime. In this sense our liquidity measure dominates trading frequency of bonds used in Chen, Lesmond, and Wei (2007) and Roll's bid-ask measure used by Bao, Pan, and Wang (2009). This consistency is important for drawing conclusions when we split the sample by industry and lead underwriter as explained below.

We use our liquidity measure to identify the contribution of liquidity to corporate bond spreads before and after the onset of the crisis, across different rating categories and across maturity. The procedure we use is to first compute our liquidity measure for each bond in the sample. Within a rating category, we then order the bonds according to this measure. Higher values correspond to lower liquidity. We then compute the difference between the 5% and the 50% quantiles and multiply by the regression coefficient for that rating category. The result is the liquidity-related difference between the spread of bonds with median and with high liquidity.

How large is then the effect of liquidity on spreads? Before the crisis, it was small for investment grade bonds both as a fraction of the yield spread and measured in basis points. The contribution to spreads from lack of liquidity rose through both an increase in our liquidity measure and in the sensitivity to this measure across all rating categories at the onset of

the crisis, although the AAA contribution remains small during the crisis. Our finding that liquidity components in AAA-rated bonds are small even after the onset of the crisis is consistent with a flight-to-quality into those bonds. Measured as a fraction of spreads, there was almost no change in the liquidity component for speculative grade bonds. When we zoom in on the time series behavior of liquidity premia, we find that they persistently increase for investment grade bonds during the crisis and peak around the rapid stock market decline in the first quarter of 2009. For speculative grade bonds, premia are less persistent, peak around the Lehman default in the fall of 2008, and returned almost to pre-crisis levels in the summer of 2009.

Our measure is also useful for analyzing other aspects of corporate bond illiquidity. We construct a liquidity beta, i.e. a measure for the covariation of an individual bond's liquidity with that of the entire corporate bond market. We show that this liquidity beta is not a significant contributor to spreads before the onset of the crisis but it does contribute to spreads for bonds except for AAA-rated bonds after the onset of the crisis. This indicates that the flight-to-quality effect in investment grade bonds found in Acharya, Amihud, and Bharath (2010) is confined to AAA-rated bonds. We also ask whether financial distress of a lead underwriter of a corporate bond issue affects the liquidity of the bond in the secondary market. If lead underwriters are providers of liquidity of the bond in secondary market trading, it is conceivable that if a lead underwriter is in financial distress, the liquidity of the bond decreases relative to other bonds. We show that bonds which had Bear Stearns as lead underwriter had lower liquidity during the take-over of Bear Stearns and bonds with Lehman as lead underwriter had lower liquidity around the bankruptcy of Lehman. Finally, we investigate whether the time series variation of liquidity of corporate bonds issued by financial firms is different from the variation for bonds issued by industrial firms. There is conflicting empirical evidence on this issue: Longstaff, Mithal, and Neis (2005) find that bonds issued by financial firms are more illiquid than other bonds, while Friewald, Jankowitsch, and Subrahmanyam (2009) find this not to be the case. Our time series study reveals that bonds issued by financial firms have similar liquidity as bonds issued by industrial firms, except in extreme stress periods, where bonds of financial firms become very illiquid, overall and compared to bonds issued by industrial firms.

The detailed trading data for corporate bonds available from the TRACE database are critical for our ability to measure liquidity proxies properly, and they help us shed new light on previous results on liquidity in corporate bonds. We show that Datastream's record of zero return days for a bond, which in Chen, Lesmond, and Wei (2007) is used to proxy for days when the bond does not trade, has little connection to the actual trades recorded in TRACE. With actual trades, the LOT measure employed in Chen, Lesmond, and Wei (2007) becomes unrealistically large. We also show that the Amihud measure is strongly influenced by restricting the universe of trades to large

trades, as we do in this paper. Using large trades only, the median price impact of a 300,000 dollar trade is roughly 0.1%, whereas Han and Zhou (2008) using all trades obtain an impact of 10.2%.

To support the claim that our measure is not measuring credit risk, we run regressions on a matched sample of corporate bonds using pairs of bonds issued by the same firm with maturity close to each other. Instead of credit controls, we use a dummy variable for each matched pair and estimate the response of spreads to our liquidity measure. The measure remains significant. In an appendix, we also show that our regression results change only slightly if we choose Treasury instead of swap rates as our riskless rates, and we test for simultaneous equation bias arising from joint determination of credit and liquidity risk and for omitted variables.

The flow of our paper is as follows. We describe our data set and how we define the eight liquidity variables that enter into the regressions. After providing summary statistics of our liquidity proxies, we run regressions on the eight liquidity variables one at a time while controlling for credit risk. We see that four variables stand out as significant predictors of spreads. Remarkably, these four variables also form the first component in a principal component decomposition of the standardized liquidity variables - and this decomposition is stable before and after the onset of the crisis. We then perform the same regressions as above - using one principal component at a time instead of the liquidity proxies. The first principal component is the only consistently significant regressor variable. Since the principal component is close to being an equally weighted sum of the four liquidity variables, we define an operational measure of liquidity as the sum of the four variables. This measure is then used to measure the contribution of illiquidity to corporate bond spreads across ratings and maturities, before and after the onset of the crisis. Furthermore, we use our measure to examine how the covariance between bond-specific liquidity and market-wide liquidity affects bond spreads, and how financial distress of a lead underwriter and the type of firm issuing the bond affect bond liquidity.

## 2.2 Literature review

It has been recognized for a long time that the ease with which a security is traded influences its price. A comprehensive survey of both the different notions of and the empirical evidence on liquidity can be found in Amihud, Mendelson, and Pedersen (2005). Here, we will focus on the growing literature that deals specifically with corporate bonds.

In recent years, the illiquidity of corporate bonds has been seen as a possible explanation for the 'credit risk puzzle', i.e. the claim that yield spreads on corporate bonds are larger than what can be explained by default risk - even after adjusting for recovery risk and compensation for bearing default risk. Huang and Huang (2003) calibrate structural default risk models

to match the default probabilities and recoveries of corporate bonds. They use a specification of the risk premium - learned from equity markets - to price the default risk in corporate bonds and show that the resulting credit spreads are smaller than the observed spreads. Other works supporting the idea that there are components of credit spreads that are unrelated to default risk include Elton, Gruber, Agrawal, and Mann (2001) who show that yield spreads cannot entirely be explained by credit risk and tax effects, and Collin-Dufresne, Goldstein, and Martin (2001) who show that changes in credit spreads cannot be explained by credit risk alone. Covitz and Downing (2007) study credit spread components in the short maturity commercial paper market and while they do find evidence of a contribution to spreads from illiquidity, they find credit risk to be the main determinant of spreads for short maturities. Longstaff, Mithal, and Neis (2005) subtract CDS premia from bond spreads to extract a non-default component of a corporate bond spread. They show that this component is correlated with proxies for liquidity both in the cross section of corporate bond spreads and in the time series evolution of spreads. In our paper, we cover a larger segment of the corporate bond market than those for which CDS premia exist. Also, it is frequently the case that the CDS spread is larger than the comparable bond spread indicating that the CDS market may also be prone to buying and selling pressures. This suggests that there are also liquidity components in CDS spreads as confirmed by Bongaerts, Driessen, and de Jong (2009).

Earlier papers which show that liquidity proxies are significant explanatory variables for corporate bond spreads and bond returns are Houweling, Mentink, and Vorst (2005), Downing, Underwood, and Xing (2005), and de Jong and Driessen (2006). An early contribution, which also stresses the importance of matrix pricing for empirical studies of bond liquidity, is Sarig and Warga (1989).

TRACE transactions data became available only recently, and therefore few studies have used the data set. Bao, Pan, and Wang (2009) use TRACE data to study liquidity effects focusing in particular on a transformation of the Roll measure. There are several studies on the effects of the introduction of TRACE. These show that the enhanced price transparency following the dissemination of prices has lowered transaction costs for investors, see Edwards, Harris, and Piwowar (2007), Goldstein, Hotchkiss, and Sirri (2007), and Bessembinder, Maxwell, and Venkaraman (2006). This would suggest that liquidity has increased. However, as shown in Goldstein, Hotchkiss, and Sirri (2007) trading volume and trading frequency have not increased as a consequence of bond price dissemination, and it is still the case that a large number of bonds trade very infrequently. This is also confirmed by Mahanti, Nashikkar, Subramanyam, Chacko, and Mallik (2008). They combine data on holdings on corporate bonds by different investors with turnover measures of these investor's portfolios to infer a turnover measure for bonds, called latent liquidity. This measure is shown to have predictive power for

other measures of liquidity. However, since we are interested in yield spread effects of illiquidity, we must confine ourselves to the more liquid segment of the corporate bond market for which we can actually observe some trading and therefore some prices and price changes. Friewald, Jankowitsch, and Subrahmanyam (2009) and Han and Zhou (2008) are other papers using the TRACE data set.

## 2.3 Data description

Since January 2001 members of the Financial Industry Regulatory Authority (FINRA) have been required to report their secondary over-the-counter corporate bond transactions through TRACE (Trade Reporting and Compliance Engine). Because of the uncertain benefit to investors of price transparency not all trades reported to TRACE were initially disseminated at the launch of TRACE July 1, 2002. Beginning October 1, 2004 trades in almost all bonds except some lightly traded bonds are disseminated (see Goldstein and Hotchkiss (2008) for details). Therefore our sample starts on this date.

We use a sample of straight coupon bullet bonds with trade reports from October 1, 2004 to June 30, 2009. That is, we require that bonds are fixed rate bullet bonds that are not callable, convertible, putable, or have sinking fund provisions. We obtain bond information from Bloomberg, and this provides us initially with 10,785 bond issues. We use rating from Datastream and bonds with missing rating are excluded.<sup>2</sup> This reduces the sample to 5,376 bonds. For these bonds we collect the trading history from TRACE covering the period from October 1, 2004 to June 30, 2009 and after filtering out erroneous trades, as described in Dick-Nielsen (2009), we are left with 8,212,990 trades. Finally we collect analysts' forecast dispersion from IBES, share prices for the issuing firms and firm accounting figures from Bloomberg, swap rates from Datastream, Treasury yields consisting of the most recently auctioned issues adjusted to constant maturities published by the Federal Reserve in the H-15 release<sup>3</sup> and LIBOR rates from British Bankers' Association. If forecast dispersion, share prices, or firm accounting figures are not available, we drop the corresponding observations from the sample.

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<sup>2</sup>We use the rating from S&P. If this rating is missing we use the rating from Moody's and if this is missing the rating from Fitch. If we still do not have a rating we use the company rating.

<sup>3</sup>Further information about the Treasury yield curve methodology can be found on the United States Department of Treasury's web page <http://www.treas.gov/offices/domestic-finance/debt-management/interest-rate/yieldmethod.html>.

## 2.4 Empirical methodology

This section provides details on the regression analysis conducted in the next section and defines the set of liquidity variables we use.

### 2.4.1 Regression

As dependent variable we use the yield spread for every bond at the end of each quarter in the regressions. We calculate the quarter-end yield as the average yield for all trades on the last day in the quarter where the bond traded. If a bond did not trade during the last month of the quarter, it is excluded from that quarter. Retail-sized trades (trade below \$100,000 in volume) are discarded. Yield spreads are calculated as the difference between the quarter-end yield and the interpolated maturity-matched swap rate calculated on the same day as the yield is measured. We exclude yield spreads for bonds that have less than one month to maturity or have a time to maturity when issued of more than 30 years.

To control for credit risk, we follow Blume, Lim, and MacKinlay (1998) and others and add the ratio of operating income to sales, ratio of long term debt to assets, leverage ratio, equity volatility and four pretax interest coverage dummies to the regressions.<sup>4</sup> In order to capture effects of the general economic environment on the credit risk of firms we include the level and slope of the swap curve, defined as the 10-year swap rate and the difference between the 10-year and 1-year swap rate. Duffie and Lando (2001) show that credit spreads may increase when there is incomplete information on the firm's true credit quality. To proxy for this effect, we follow Guntay and Hackbarth (2006) and use dispersion in earnings forecasts as a measure of incomplete information.

Finally we add bond age, time-to-maturity, and size of coupon to the regressions - see for example Sarig and Warga (1989), Houweling, Mentink, and Vorst (2005) and Longstaff, Mithal, and Neis (2005).

For each rating class we run separate regressions using quarterly obser-

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<sup>4</sup>The pretax interest coverage dummies are defined as follows. We define the pretax interest rate coverage (IRC) ratio as EBIT divided by interest expenses. It expresses how easily the company can cover its interest rate expenses. However, the distribution is highly skewed. As in Blume, Lim, and MacKinlay (1998) we control for this skewness by creating four dummies (pretax dummies) which allows for a non-linear relationship with the spread. The first dummy is set to the IRC ratio if it is less than 5 and 5 if it is above. The second dummy is set to 0 if IRC is below 5, to the IRC ratio minus 5 if it lies between 5 and 10 and 5 if it lies above. The third dummy is set to 0 if IRC is below 10, to the IRC ratio minus 10 if it lies between 10 and 20 and 10 if it lies above. The fourth dummy is set to 0 if IRC is below 20 and is set to IRC minus 20 if it lies above 20 (truncating the dummy value at 80).

ventions. The regressions are

$$\begin{aligned}
\text{Spread}_{it} = & \alpha + \gamma \text{Liquidity}_{it} + \beta_1 \text{Bond Age}_{it} + \beta_2 \text{Amount Issued}_{it} \\
& + \beta_3 \text{Coupon}_{it} + \beta_4 \text{Time-to-Maturity}_{it} + \beta_5 \text{Eq.Vol}_{it} \\
& + \beta_6 \text{Operating}_{it} + \beta_7 \text{Leverage}_{it} + \beta_8 \text{Long Debt}_{it} \\
& + \beta_{9,\text{pretax}} \text{Pretax dummies}_{it} + \beta_{10} \text{10y Swap}_t \\
& + \beta_{11} \text{10y-2y Swap}_t + \beta_{12} \text{forecast dispersion}_{it} + \epsilon_{it} \quad (2.1)
\end{aligned}$$

where  $i$  is bond issue,  $t$  is quarter, and  $\text{Liquidity}_{it}$  contains one of the liquidity proxies defined below. Since we have panel data set of yield spreads with each issuer potentially having more than one bond outstanding at any point in time we calculate two-dimensional cluster robust standard errors (see Petersen (2009)). This corrects for time series effects, firm fixed effects and heteroscedasticity in the residuals.

#### 2.4.2 Liquidity Measures

Since there is no single measure that adequately describes the liquidity of an asset, we define several liquidity-related measures for corporate bonds in this section. We winsorize the 0.5% highest values of every liquidity variable, meaning that all values above the 99.5% percentile are set to the 99.5% percentile.

##### Amihud measure (price impact of trades)

Amihud (2002) constructs an illiquidity measure that is based on the theoretical model of Kyle (1985). It measures the price impact of a trade per unit traded and we use a slightly modified version of this measure. For each corporate bond the measure is the daily average of absolute returns  $r_j$  divided by trading volume  $Q_j$  (in million \$) of consecutive transactions:

$$\text{Amihud}_t = \frac{1}{N_t} \sum_{j=1}^{N_t} \frac{|r_j|}{Q_j} = \frac{1}{N_t} \sum_{j=1}^{N_t} \frac{\left| \frac{P_j - P_{j-1}}{P_{j-1}} \right|}{Q_j}$$

where  $N_t$  is the number of returns on day  $t$ . At least two transactions are required on a given day in order to calculate the measure, and we define a quarterly Amihud measure by taking the median of daily measures within the quarter.

##### Roll measure (bid-ask spread)

A liquid asset can be bought or sold close to the fundamental price of the asset, implying that roundtrip costs are small. A proxy for roundtrip costs



is the bid-ask spread, but bid-ask spreads are not available in TRACE. Since November 2008, buy-sell indicators are available, but this covers only a fraction of our sample. Roll (1984) finds that under certain assumptions the effective bid-ask spread equals two times the square root of minus the covariance between adjacent price changes:

$$Roll_t = 2\sqrt{-cov(\Delta P_i, \Delta P_{i-1})}$$

where  $t$  is the time period for which the measure is calculated. The intuition is that the bond price bounces back and forth within the bid-ask band, and higher bid-ask bands lead to higher negative covariance between adjacent price changes. We define a daily Roll measure using a rolling window of 21 trading days, and the measure is only well-defined if there are at least four transactions in the window. We define a quarterly Roll measure by taking the median of daily measures within the quarter.

#### Unique roundtrip cost (bid-ask spread)

An alternative measure of transaction costs, proposed in Feldhütter (2009), is calculated using *unique roundtrip trades* (URT). Often, we see a corporate bond trading two or three times within a very short period of time after a longer period with no trades. This is likely to occur because a dealer matches a buyer and a seller and collects the bid-ask spread as a fee. When a dealer has found a match, a trade between seller and dealer along with a trade between buyer and dealer are carried out. Possibly, the matching occurs through a second dealer in which case there is also a transaction between the two dealers. If two or three trades in a given bond with the same volume take place on the same day, and there are no other trades with the same volume on that day, we define the transactions as part of a URT. For a URT we define the unique roundtrip cost (URC) as

$$\frac{P_{max} - P_{min}}{P_{max}}$$

where  $P_{max}$  is the largest price in the URT and  $P_{min}$  is the smallest price in the URT. A daily estimate of roundtrip costs is the average of roundtrip costs on this day for different volumes, and we estimate quarterly roundtrip costs by averaging over daily estimates. URC overcomes the problem that we only have information on trading volume and not, as in Green, Hollifield, and Schürhoff (2007b), on bid and ask prices or dealer identity. Feldhütter (2009) examines the properties of URTs in detail, including how much of total trading volume is captured and for a subsample of TRACE data with buy-sell indicators available, to what extent URTs capture full roundtrip costs.

### Turnover (trading intensity)

Assets that trade frequently are intuitively more liquid than assets that only trade on rare occasions. We therefore consider the quarterly turnover of the bond:

$$\text{Turnover}_t = \frac{\text{Total trading volume}_t}{\text{Amount outstanding}}$$

where  $t$  is the quarter. We can interpret the inverse of the turnover as the average holding time of the bond, i.e. a turnover of 1 implies an average holding time of about 3 months.

### Zero trading days (trading intensity)

An alternative trading intensity measure is the number of days where a bond did not trade. We calculate *bond zero-trading days* as the percentage of days during a quarter where the bond did not trade. We also calculate *firm zero-trading days* as the percentage of days during a quarter where none of the issuing firm's bonds traded. Clearly, this is a firm-specific rather than a bond-specific measure, and it is therefore the same for different bonds issued by the same firm. Even though a single bond seldomly trades, the issuing firm often has bonds of many different maturities outstanding. It may therefore be the case that the waiting time between trades in any of the firm's bond issues is much shorter and that there is relatively frequent new information about the issuing firm and frequent trading in close substitutes. Firm zero trading days addresses this issue.

### Variability of Amihud and unique roundtrip costs (liquidity risk)

It is likely that investors consider not only the current level of bond liquidity but also the possible future levels in case the investor needs to sell the bond. The variability of both the Amihud measure and unique roundtrip costs may therefore play a role for liquidity spreads. Thus, we include in our regressions the standard deviations of the daily Amihud measure and unique roundtrip costs measured over one quarter. These two measures do not separate total liquidity risk into a systematic and unsystematic component. Arguably, only the systematic component is important for pricing, but since it is difficult to measure this component on a quarterly basis, we calculate the total component and address the systematic component later in the paper.

## 2.5 Liquidity premia

### 2.5.1 Summary statistics

Table 2.1 shows summary statistics for the liquidity variables. We see that the median quarterly turnover is 4.5%, meaning that for the average bond

Panel A: Summary statistics for liquidity proxies

	Amihud	Roll	firm zero	bond zero	turnover	URC	Amihud risk	URC risk
99th	0.0813	8.39	92.1	96.8	0.247	0.0156	0.1592	0.01702
95th	0.0427	3.16	76.2	93.5	0.136	0.0096	0.0792	0.00997
75th	0.0120	1.05	12.5	79.7	0.070	0.0041	0.0298	0.00427
50th	0.0044	0.53	0.0	60.7	0.045	0.0022	0.0147	0.00220
25th	0.0015	0.29	0.0	31.7	0.028	0.0012	0.0064	0.00102
5th	0.0003	0.12	0.0	6.3	0.012	0.0005	0.0011	0.00024
1st	0.0000	0.06	0.0	0.0	0.005	0.0002	0.0002	0.00003

Panel B: Correlation matrix for liquidity proxies

	Amihud	Roll	firm zero	bond zero	turnover	URC	Amihud risk	URC risk
Amihud	1.00							
Roll	0.16	1.00						
firm zero	-0.08	0.11	1.00					
bond zero	-0.08	0.18	0.46	1.00				
turnover	-0.20	0.04	0.03	0.04	1.00			
URC	0.72	0.20	-0.03	-0.03	-0.13	1.00		
Amihud risk	0.61	0.10	-0.12	-0.12	-0.11	0.69	1.00	
URC risk	0.57	0.14	-0.12	-0.19	-0.11	0.87	0.69	1.00

**Table 2.1: Statistics for liquidity proxies.** This table shows statistics for corporate bond liquidity proxies. The proxies are described in detail in Section 2.4 and are calculated quarterly from 2004:Q4 to 2009:Q2. Panel A shows quantiles for the proxies. Panel B shows correlations among the proxies. There is a total of 2,224 bond issues and 380 bond issuers in our sample.

in the sample it takes 5-6 years to turn over once. The turnover is a lower bound on the actual turnover since trade sizes above \$1mio (\$5mio) for speculative (investment) grade bonds are registered as trades of size \$1mio (\$5mio). The median number of bond zero-trading days is 60.7% consistent with the notion that the corporate bond market is an illiquid market. We also see that the median number of firm zero-trading days is 0%. This shows that although a given corporate bond might not trade very often, the issuing firm has *some* bond that is trading. It is likely that the number of bond zero-trading days overstates the difficulty of finding a trading partner when buying or selling the bond, since the bond is a close substitute to a number of other bonds.

The median Amihud measure is 0.0044 implying that a trade of \$300,000 in an average bond moves price by roughly 0.13%. Han and Zhou (2008) also calculate the Amihud measure for corporate bond data using TRACE data and find a much stronger price effect of a trade. For example, they find that a trade in an average bond of \$300,000 moves the price by 10.2%. The reason for this discrepancy is largely due to the exclusion of small trades in our sample and underscores the importance of filtering out retail trades when estimating transaction costs of institutional investors<sup>5</sup>.

The median roundtrip cost in percentage of the price is 0.22% according to the URC measure, while the roundtrip cost is less than 0.05% for the 5% most liquid bonds. Thus, transaction costs are modest for a large part of the corporate bond market consistent with findings in Edwards, Harris, and Piwowar (2007), Goldstein, Hotchkiss, and Sirri (2007), and Bessembinder, Maxwell, and Venkaraman (2006). The roundtrip cost measured using URC is lower than the median roundtrip cost of 0.53% when estimated using the Roll measure.

The correlations of the liquidity measures in Panel B of Table 2.1 reveal several interesting aspects of liquidity and liquidity risk. The correlation of 87% between URC and URC risk and 61% between Amihud and Amihud risk shows that liquidity and liquidity risk are highly correlated. This is consistent with results in Acharya and Pedersen (2005) who likewise find a high correlation between liquidity and liquidity risk. Interestingly, there is a high correlation of 72% between market depth (Amihud) and bid/ask spread (URC).

The correlations also show that the Amihud measure is negatively correlated with firm zero, bond zero, and turnover, while the Roll measure has positive correlations with the three trading activity variables. We would expect negative correlations for both the Roll and Amihud measure, since more traded bonds are likely to have lower bid/ask spreads and higher mar-

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<sup>5</sup>A second reason for the discrepancy is that we estimate a quarterly Amihud measure by taking the median of daily measures, while Han and Zhou (2008) estimate a monthly measure by taking the mean of daily measures. The effect of filtering out small trades is by far the most important reason for the discrepancy.

ket depth. The positive correlations for the Roll measure might be explained by the statistical properties of the Roll measure. Harris (1990) finds that the serial covariance estimator can be severely biased in small samples. The bias decreases in the number of observations, and this might explain the positive correlations between the Roll measure and trading activity measures. The bias might also explain why the Amihud measure is slightly more successful in explaining spreads than the Roll measure in the next section.

Having defined the individual liquidity measures and looked at some descriptive statistics, we now turn to the effects on bond spreads of these variables one at a time.

### 2.5.2 The effect of liquidity proxies

We have defined eight liquidity proxies and in this section we ask if the proxies affect spreads. For each variable we run the pooled regression in Equation (2.1) for each of the seven rating categories and before and after the onset of the subprime crisis. We winsorize the 0.5% highest and lowest spreads to make the results robust to outliers. Running separate regressions for different rating categories shows us to what extent the variables affect bonds of various credit quality and how robust our results are. In addition, the effect of liquidity on corporate bond spreads might be different in periods of rich liquidity and periods of little liquidity. By splitting the sample into pre- and post-subprime, we see how liquidity is priced in two such different regimes; the pre-subprime period was a period with plenty of liquidity while the market in the post-subprime period has suffered from a lack of liquidity. Table 2.2 shows the regression coefficients for each of the variables.<sup>6</sup>

For the pre-subprime period both measures of transaction costs, Roll and URC, have positive coefficients for every rating category. All five coefficients are significant for URC while the results are mixed in case of the Roll measure. We obtain similar results in the post-subprime period, four out of five coefficients positive and statistically highly significant in case of the URC measure and mostly insignificant results for the Roll measure. Transaction costs are clearly priced, at least when we proxy bid-ask spreads with the URC measure, which is consistent with the results in Chen, Lesmond, and Wei (2007) who find that bid/ask spreads are priced. We also see that the Amihud measure has positive regression coefficients across all ratings pre- and post-subprime and 6 out of 10 are statistically significant.

Figure 2.1 shows that bid-ask spreads (URC) and the lack of market depth (Amihud) have increased strongly during the subprime crisis. The increase from the beginning of the crisis to the end of 2008 in bid-ask spreads

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<sup>6</sup>We only use observations for which an estimate for all measures exists. This ensures that the regression coefficients for all proxies are based on the same sample. We have also run the regressions where we allow an observation to enter a regression if the observation has an estimate for this liquidity proxy, although it might not have estimates of some of the other proxies. The results are very similar.

Panel A: Marginal liquidity regressions, pre-subprime (2004:Q4-2007:Q1)

	AAA	AA	A	BBB	spec
Amihud	1.15*** (4.87)	2.08*** (3.85)	4.14*** (3.18)	3.68 (1.52)	14.12 (1.63)
Roll	0.02*** (3.18)	0.02*** (3.48)	0.01 (1.48)	0.02 (0.53)	0.05 (1.26)
firm zero	0.000 (0.46)	-0.001 (-1.42)	0.000 (0.74)	-0.001* (-1.66)	-0.005 (-1.60)
bond zero	-0.000 (-0.09)	-0.000 (-0.86)	0.000 (1.13)	-0.003** (-2.22)	-0.012** (-2.33)
turnover	-0.27*** (-6.52)	-0.12 (-0.97)	-0.03 (-0.31)	-0.03 (-0.18)	-0.05 (-0.09)
URC	3.83** (2.03)	7.11*** (2.66)	18.91*** (2.61)	47.47*** (3.76)	69.29** (2.26)
Amihud risk	0.39* (1.82)	0.55* (1.87)	1.43** (2.42)	3.46*** (3.46)	9.48** (2.29)
URC risk	2.08** (2.30)	3.98* (1.95)	9.16** (2.29)	25.99*** (3.18)	57.20*** (3.67)

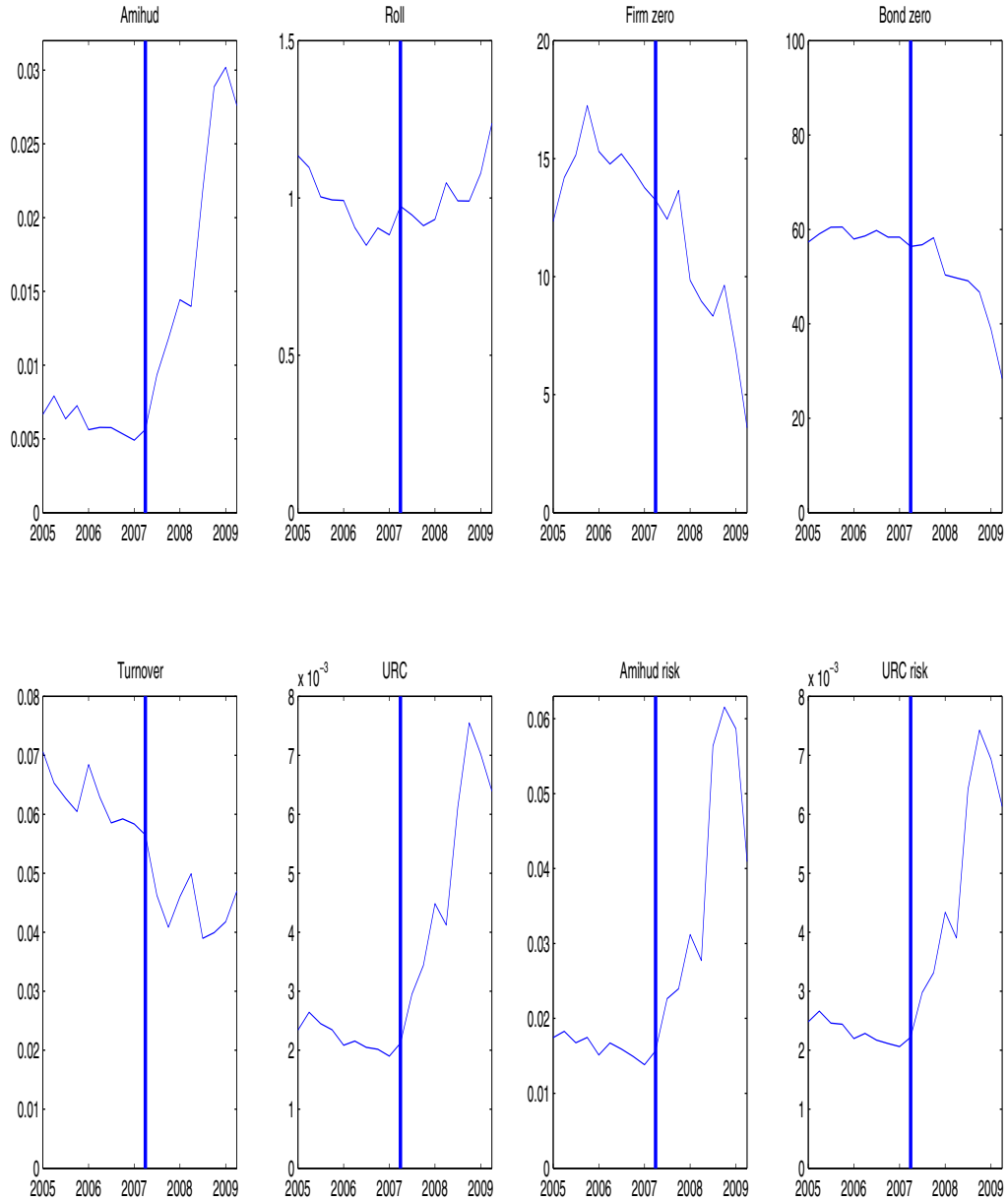
Panel B: Marginal liquidity regressions, post-subprime (2007:Q2-2009:Q2)

	AAA	AA	A	BBB	spec
Amihud	2.93*** (2.98)	18.40*** (2.94)	6.80 (0.82)	21.94** (2.54)	22.47 (1.52)
Roll	0.04*** (2.58)	-0.02 (-1.55)	0.04 (0.87)	0.19* (1.76)	-0.73 (-1.47)
firm zero	-0.016 (-1.46)	-0.000 (-0.03)	-0.000 (-0.07)	-0.023** (-2.22)	-0.047** (-2.05)
bond zero	0.007*** (7.26)	0.002 (0.73)	0.013** (2.31)	-0.016 (-0.53)	-0.087 (-1.49)
turnover	-2.95*** (-11.87)	-2.12 (-1.11)	-0.74 (-0.31)	-2.97 (-0.33)	14.47 (0.82)
URC	20.50*** (2.88)	191.63*** (3.08)	209.47*** (4.74)	212.15*** (2.96)	-143.70 (-0.57)
Amihud risk	1.99 (1.25)	18.87*** (4.74)	20.66*** (3.26)	21.42** (2.22)	24.11** (2.43)
URC risk	17.40** (2.07)	167.60*** (3.71)	190.46*** (4.03)	270.28*** (4.23)	233.16** (2.13)

**Table 2.2: Marginal liquidity regressions.** For each rating class  $R$  and each liquidity variable  $L$  a pooled regression is run with credit risk controls

$$Spread_{it}^R = \alpha^R + \gamma^R L_{it} + \text{credit risk controls}_{it} + \epsilon_{it}$$

where  $i$  is for bond in rating  $R$  and  $t$  is time measured in quarter. In total 40 regressions are run (8 liquidity variables  $\times$  5 rating classes). This table shows for each regression the coefficient and t-statistics in parenthesis for the liquidity variable,  $\gamma$ . The proxies are described in detail in Section 2.4 and are calculated quarterly from 2004 : Q4 to 2009 : Q2. Panel A shows the coefficients using data before the subprime crisis, while Panel B shows the coefficients using data after the onset of the subprime crisis. Standard errors are corrected for time series effects, firm fixed effects, and heteroscedasticity, and significance at 10% level is marked '\*, at 5% marked '\*\*', and at 1% marked '\*\*\*'.



**Figure 2.1: Time series of liquidity variables in the regression sample.** This graph plots the time series of liquidity variables along with a line marking the start of the subprime crisis (beginning in 2007Q2). Liquidity variables are measured quarterly, and for every liquidity variable the mean value of the variable across all observations each quarter is graphed. For each quarter a bond observation requires a full set of accounting variables and at least four transactions during the quarter.

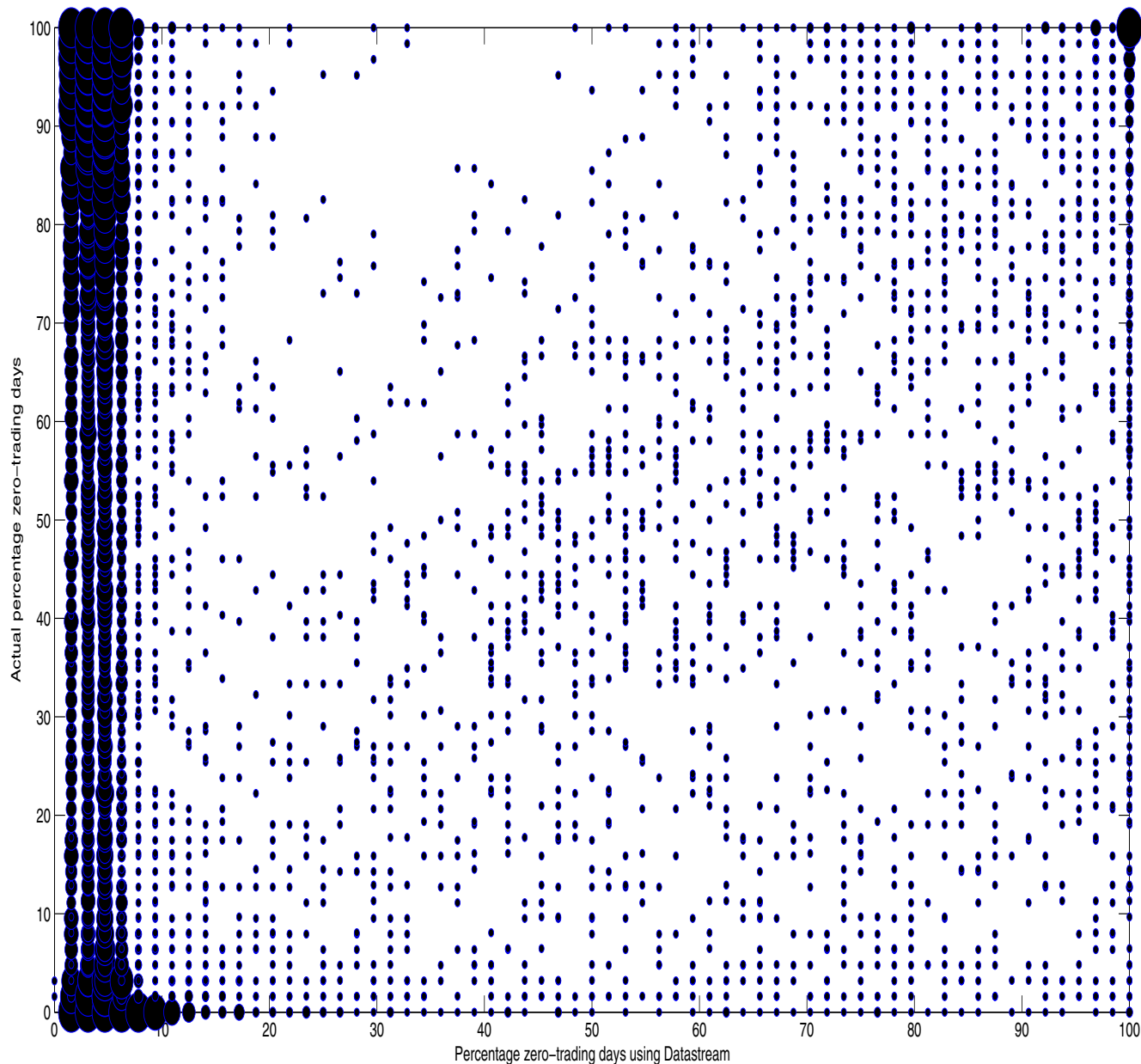
is approximately a factor of 4 for bid-ask spreads and a factor of 8 for lack of market depth. That is, not only have bid-ask spreads widened strongly during the crisis but the ability to sell large notional amounts of bonds without a sizeable discount has disappeared. We see that liquidity in the second quarter of 2009 slowly returns to the market since Amihud and URC are finally decreasing after the increase in previous years.

Volume has traditionally been regarded as a proxy for liquidity, since it should be easier to trade when markets are more active. However, Johnson (2008) finds in a simple frictionless model that volume is unrelated to the level of liquidity but related to liquidity risk as measured by the variance of liquidity. Table 2.2 shows that 9 out of 10 regression coefficients for volume are negative indicating that large volumes tend to reduce credit spreads. The significance of the coefficients is modest though, so the evidence is not conclusive. Liquidity risk is clearly priced since Amihud and URC risk have significantly positive regression coefficients in 19 out of 20 cases. Interestingly, all coefficients increase strongly in size post-subprime. Thus, investors require a larger compensation post-subprime for investing in bonds with a high uncertainty about the liquidity discount when selling the bond. Since liquidity risk has increased strongly as Figure 2.1 shows, the impact of liquidity risk is twofold; through a larger level of liquidity risk *and* through a higher risk premium on liquidity risk.

Turning to zero trading days Table 2.2 shows surprisingly that there is no consistent relationship between the number of zero trading days and spreads. If anything, the relationship tends to be negative since 14 out of 20 bond and firm zero regression coefficients are negative. Constantinides (1986) finds theoretically that in the presence of transaction costs, investors will trade infrequently, and consistent with this line of reasoning Chen, Lesmond, and Wei (2007) find that corporate bond spreads - when controlling for credit risk - depend positively on the number of zero trading days.

The difference between our results and those of Chen, Lesmond, and Wei (2007) is likely to be the data source. While we use actual transaction data and can directly detect when a trade occurs, Chen, Lesmond, and Wei (2007) use data from Datastream and define a zero trading day as a day where the price does not change. We find that Datastream corporate bond data can differ substantially from actual transaction data in non-predictable ways. To illustrate this, we calculate for each bond quarter the percentage zero trading days using Datastream, and Figure 2.2 plots all pairs of TRACE and Datastream percentage zero trading days. The figure shows that there is very little relation between actual and Datastream zero trading days, and while Datastream often understates the number of zero trading days, they are also overstated for some observations. Although zero-trading days are not correctly identified in Datastream, the LOT measure of Chen, Lesmond, and Wei (2007) could be a relevant measure to include in our analysis. Therefore we have calculated a yearly LOT measure as in Chen, Lesmond, and Wei





**Figure 2.2: Zero-trading days using Datastream.** This graph plots for every bond in the sample and every quarter from 2005:Q1 to 2007:Q4 the percentage zero-trading days using Datastream on the x-axis and actual percentage zero-trading days (based on all trades in TRACE) on the y-axis. The thickness of a point depends on the number of observations in that point. The total number of observations is 60,680.

(2007) for all TRACE bonds for the years 2005, 2006, and 2007 based on all TRACE trades. The median roundtrip cost is 237 basis points, which appears too high compared to findings in Edwards, Harris, and Piwowar (2007), Goldstein, Hotchkiss, and Sirri (2007), and Bessembinder, Maxwell, and Venkaraman (2006).

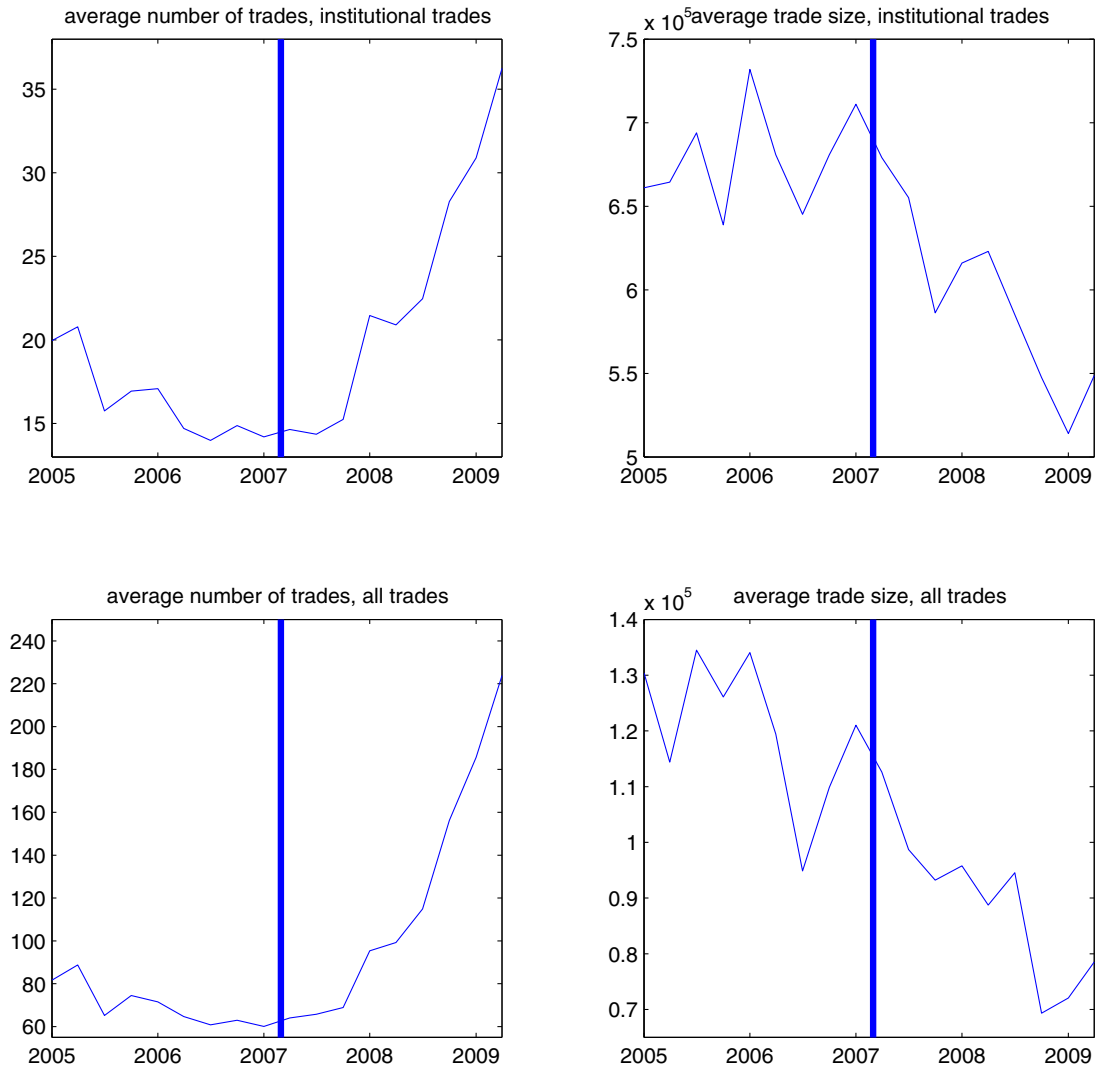
From a theoretical point of view the mixed results regarding the impact of zero trading days on spreads can be explained by results in Huberman and Stanzl (2005). They show that an investor trades more often when price impact of trades is high, because he attempts to reduce the total price impact by submitting more but smaller orders. All else equal more trades therefore occur in illiquid bonds since it is necessary to split a sell order in many small trades, while it can be executed in a single trade in a liquid bond.<sup>7</sup> If this explanation holds true we should expect to see less zero trading days in illiquid times without an increase in the total trading volume. As Figure 2.1 shows this happens during the subprime crisis. The top-right graph shows that the median number of percentage zeros in the regression sample decreases during the subprime crisis. For example, the median number of percentage zeros is 30% in the last quarter of 2008 while it is 62% in the first quarter of 2007. Also, we see in the bottom-left graph that volume in our regression sample decreases slightly during the crisis.

Drawing conclusions from Figure 2.1 might be misleading since a bond in a given quarter is only included in the regression sample if it has a full set of accounting variables and trades at least four times that quarter (otherwise the Roll measure cannot be calculated). Thus, it is only the most liquid bonds that are included and there are less bonds included post-subprime than pre-subprime. The decrease in zero trading days might therefore be due to a smaller number of bonds included in the sample. To address this concern, Figure 2.3 shows time series of the quarterly average number of trades and average trade size for all straight coupon bullet bond transactions in our sample period. The top graphs are based on transactions of size \$100,000 or more, which our regression results are based on, while the bottom graphs are based on all transactions. In both cases we clearly see an increase on the average number of trades and a decrease in the average trade size after the onset of the subprime crises.

Overall, there is theoretical evidence both in favor of and against the

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<sup>7</sup>Goldstein, Hotchkiss, and Sirri (2007) find that dealers behave differently when trading liquid and illiquid bonds. When trading liquid bonds they are more likely to buy the bond, have it as inventory and sell it in smaller amounts. When trading illiquid bonds they more often quickly sell the entire position, so they perform more of a matching function in these bonds. This is consistent with our argument that illiquid bonds trade more often, which can be illustrated with the following example. In a liquid bond the investor sells \$1,000,000 to a dealer, who sells it to investors in two amounts of \$500,000. In an illiquid bond the investor sells 500,000 to two different dealers, who each sells the \$500,000 to an investor. The total number of trades in the illiquid bond is four while it is three in the liquid bond.



**Figure 2.3: Time series of average number of trades and average trade size in the full sample.** This graph plots the time series of average number of trades in a quarter and average trade size along with a line marking the start of the subprime crisis (beginning in 2007.Q2). Number of trades and trade size are measured quarterly and the mean value across all observations each quarter is graphed. A bond is included in every quarter if it traded at least one time during the sample period 2005:Q1-2008:Q4. The top graphs is based on institutional trades, i.e. trades of size \$100,000 or more, while the bottom graphs are based on all trades.

use of zero trading days as a measure of illiquidity. Our empirical evidence is also mixed. We show that trading activity increases when the market becomes more illiquid, while at the same time Table 2.2 shows that bond zero trading days do tend to predict investment grade spreads after the onset of the subprime crisis. In any case, we do not find that zero trading days can be consistently used as a predictor of spreads.

### 2.5.3 Principal component analysis of liquidity

In our analysis we include eight liquidity proxies that measure different aspects of liquidity. To see if most of the relevant information in the proxies can be captured by a few factors, we conduct a principal component analysis. Table 2.3 shows the loadings and the explanatory power of the eight principal components. We see that both the explanatory power and the loadings of each PC component are very stable in the two subperiods. Also we see that the PC components have clear interpretations. The first component explains 40% of the variation in the liquidity variables and is close to being an equally-weighted linear combination of the Amihud and URC measures and their associated liquidity risk measures. The second PC explains 20% and is a zero trading days measure, the third PC explains 13% and is a volume measure, and the fourth PC explains 9% and is a Roll measure. The last four PCs explain less than 20% and do not have clear interpretations.

Table 2.4 shows results of adding each of the PCs in turn to our regression in the same way as we did with each liquidity variable in Table 2.2. Strikingly, the first PC is significant for all rating categories pre- and post-subprime. For 9 out of 10 regression coefficients the significance is at a 1% level. In addition, the remaining seven PCs are mostly insignificant and often with conflicting signs. This suggests that although liquidity has many different aspects, a single linear combination of measures of transaction costs, market depth, and liquidity risk explains much of the impact of liquidity on yield spreads. The factor is priced at all ratings pre- and post-subprime in contrast to previously proposed liquidity proxies, zero-trading days (Chen, Lesmond, and Wei (2007)) and the Roll measure (Bao, Pan, and Wang (2009)).

The principal component loadings on the first PC in Table 2.3 lead us to define a factor that loads evenly on Amihud, URC, Amihud risk, and URC risk, and does not load on any of the other liquidity measures. The factor is simpler to calculate than the first PC while retaining its properties. We use this factor in our subsequent analysis and call it  $\lambda$ . To be precise: for each bond  $i$  and quarter  $t$  we calculate the measure  $L_{it}^j$  where  $j = 1, \dots, 4$  is an index for Amihud, URC, Amihud risk, and URC risk. We normalize each measure  $\tilde{L}_{it}^j = \frac{L_{it}^j - \mu^j}{\sigma^j}$  where  $\mu^j$  and  $\sigma^j$  are the mean and standard deviation of  $L^j$  across bonds and quarters and define our liquidity measure for each

Panel A: Principal Component loadings, pre-subprime (2004:Q4-2007:Q1)								
	1PC	2PC	3PC	4PC	5PC	6PC	7PC	8PC
Amihud	0.45	0.05	-0.12	-0.05	0.44	0.70	-0.12	0.28
Roll	0.26	0.33	0.08	-0.86	-0.27	-0.06	0.06	0.02
firm zero	-0.04	0.64	-0.02	0.39	-0.56	0.36	0.07	0.02
bond zero	-0.00	0.67	-0.10	0.10	0.56	-0.45	0.05	0.11
turnover	-0.02	0.07	0.98	0.07	0.15	0.08	0.01	0.03
URC	0.52	0.06	0.03	0.15	0.00	-0.10	-0.39	-0.73
Amihud risk	0.47	-0.11	0.01	0.16	-0.01	-0.09	0.85	-0.09
URC risk	0.49	-0.12	0.06	0.21	-0.29	-0.40	-0.31	0.60
cum. % explained	39%	59%	72%	81%	89%	94%	99%	100%

Panel B: Principal Component loadings, post-subprime (2007:Q2-2009:Q2)								
	1PC	2PC	3PC	4PC	5PC	6PC	7PC	8PC
Amihud	0.46	0.04	-0.10	-0.10	-0.07	0.73	0.43	0.21
Roll	0.06	0.47	0.35	-0.78	0.10	-0.02	-0.17	0.02
firm zero	-0.11	0.59	-0.28	0.33	0.62	0.20	-0.17	0.00
bond zero	-0.12	0.64	-0.07	0.21	-0.67	-0.16	0.21	0.12
turnover	-0.14	0.05	0.88	0.39	0.08	0.20	0.12	0.01
URC	0.52	0.15	0.06	0.09	0.09	-0.26	0.28	-0.73
Amihud risk	0.46	0.03	0.07	0.21	-0.30	0.19	-0.78	-0.04
URC risk	0.51	0.02	0.09	0.13	0.23	-0.51	0.10	0.63
cum. % explained	39%	58%	71%	81%	88%	94%	99%	100%

**Table 2.3: Principal component loadings on the liquidity variables.** This table shows the principal component analysis loadings on each of the eight liquidity variables along with the cumulative explanatory power of the components.

bond and quarter as

$$\lambda_{it} = \sum_{j=1}^4 \tilde{L}_{it}^j$$

#### 2.5.4 Size of liquidity component

To calculate the impact of corporate bond illiquidity on yield spreads we do the following. For each rating  $R$  we run the pooled regression

$$spread_{it}^R = \alpha^R + \beta^R \lambda_{it} + \text{credit risk controls}_{it} + \epsilon_{it}$$

where  $i$  refers to bond,  $t$  to time (measured in quarters of year), and  $\lambda_{it}$  is our liquidity measure. We define the liquidity score for a bond in a given quarter as  $\beta^R \lambda_{it}$ . Within each rating (AAA, AA, A, BBB, spec), period (pre- or post subprime), and maturity (0-2y, 2-5y, 5-30y) we sort all observations according to their liquidity score. The liquidity component of an average bond is defined as the 50% quantile minus the 5% quantile of the liquidity score distribution. Thus, the liquidity component measures the difference in bond yields between a bond with average liquidity and a very liquid bond. Following Cameron, Gelbach, and Miller (2008) we calculate

Panel A: Multivariate liquidity regressions, pre-subprime (2004:Q4-2007:Q1)					
	AAA	AA	A	BBB	spec
intercept	-0.4 (-1.24)	0.2 (1.20)	-0.5 (-1.62)	2.2*** (2.84)	-0.1 (-0.03)
1PCA	0.01*** (3.22)	0.02*** (12.31)	0.03*** (3.28)	0.05*** (2.88)	0.30*** (5.65)
2PCA	0.01 (0.58)	-0.00 (-0.09)	0.04*** (3.41)	-0.06 (-1.30)	-0.19 (-1.19)
3PCA	-0.014*** (-4.20)	-0.006 (-0.72)	0.018*** (2.66)	-0.005 (-0.21)	0.093 (0.88)
4PCA	-0.020** (-2.32)	-0.022*** (-2.94)	-0.002 (-0.18)	-0.015 (-0.67)	0.112* (1.92)
5PCA	0.00 (0.01)	0.02*** (3.08)	0.03* (1.88)	-0.05 (-1.22)	-0.02 (-0.16)
6PCA	0.00 (0.69)	0.01 (0.81)	0.03*** (4.19)	0.03 (0.65)	0.24* (1.91)
7PCA	0.00 (0.27)	-0.00 (-0.28)	-0.00 (-0.55)	-0.02* (-1.70)	-0.10* (-1.68)
8PCA	0.02*** (3.07)	0.02 (1.43)	-0.01 (-0.74)	-0.23*** (-2.58)	-0.17 (-1.56)
age	0.00 (0.08)	-0.00 (-0.96)	0.00 (1.12)	-0.01 (-1.26)	-0.00 (-0.12)
amount issued	-0.025*** (-3.52)	-0.012 (-1.34)	0.032** (2.57)	-0.108*** (-2.65)	-0.143 (-0.87)
forecast dispersion	3.05 (1.64)	0.02 (1.30)	0.73** (2.12)	0.65** (2.04)	1.21 (1.37)
coupon	0.02** (1.99)	0.02*** (4.00)	0.01* (1.79)	0.07*** (4.46)	0.29*** (3.62)
10y swap	-0.05* (-1.82)	-0.03*** (-3.76)	-0.05*** (-4.23)	-0.06*** (-4.03)	-0.26 (-1.33)
10y-2y swap	0.005 (0.79)	-0.030** (-2.28)	-0.020*** (-2.89)	-0.107*** (-5.31)	-0.132 (-0.44)
equity vol	-0.002 (-0.33)	0.008*** (15.21)	0.006* (1.68)	0.011*** (4.17)	0.093*** (5.88)
pretax1	0.344*** (3.53)	0.023*** (2.88)	0.010 (0.57)	-0.026 (-1.36)	0.027 (0.44)
pretax2	-0.051*** (-3.06)	-0.016*** (-4.90)	-0.011* (-1.90)	-0.013 (-1.54)	-0.068 (-0.90)
pretax3	-0.007 (-1.00)	0.000 (0.18)	-0.001 (-0.35)	0.011** (2.18)	0.048 (0.95)
pretax4	-0.003*** (-3.78)	0.000 (0.03)	0.000 (0.26)	-0.005*** (-3.31)	-0.022 (-1.30)
sales to income	-0.002 (-1.14)	-0.000 (-0.53)	-0.000 (-0.01)	-0.005** (-2.14)	-0.003** (-1.97)
long term debt to asset	-0.016** (-2.49)	-0.002*** (-4.13)	0.001 (1.16)	0.008*** (2.92)	-0.001 (-0.02)
leverage ratio	0.009*** (3.04)	0.001 (1.58)	-0.001 (-1.00)	0.000 (0.10)	0.023 (0.91)
time-to-maturity	0.016*** (3.50)	0.019*** (18.21)	0.022*** (15.21)	0.040*** (7.95)	0.043*** (2.99)
$N$	533	1869	4148	1340	1075
$R^2$	0.46	0.53	0.47	0.60	0.61

**Table 2.4: Multivariate liquidity regressions.** For each of the five rating classes a pooled regression with quarterly observations is run with variables measuring both liquidity and credit risk. Panel A shows the regression coefficients and t-statistics in parenthesis when using data from 2004:Q4 to 2007:Q1, while Panel B shows the results for data from 2007:Q2 to 2009:Q2. Standard errors are corrected for time series effects, firm fixed effects, and heteroscedasticity, and significance at 10% level is marked '\*, at 5% marked '\*\*', and at 1% marked \*\*\*'.

Panel B: Multivariate liquidity regressions, post-subprime (2007:Q2-2009:Q2)					
	AAA	AA	A	BBB	spec
intercept	-2.5** (-2.00)	-2.6 (-1.00)	1.0*** (2.66)	24.9 (1.42)	30.2* (1.65)
1PCA	0.05* (1.91)	0.48*** (4.50)	0.45*** (4.64)	0.67*** (3.18)	1.16*** (4.33)
2PCA	-0.08 (-0.57)	0.15 (1.60)	0.26** (2.27)	-0.03 (-0.05)	-0.73 (-1.21)
3PCA	0.066 (1.21)	0.153*** (2.96)	0.146*** (3.27)	0.389* (1.75)	0.349 (0.90)
4PCA	-0.125 (-1.35)	0.283*** (5.14)	0.267*** (4.07)	0.110* (1.81)	0.900 (1.40)
5PCA	-0.35*** (-2.75)	-0.18 (-1.17)	-0.17*** (-7.65)	-0.46 (-0.90)	0.52 (0.97)
6PCA	-0.09* (-1.76)	-0.17 (-1.30)	-0.41* (-1.67)	-0.30* (-1.70)	1.00** (2.57)
7PCA	0.07 (0.68)	-0.39* (-1.79)	-0.22 (-1.24)	-0.44 (-1.08)	-0.58** (-1.98)
8PCA	0.12* (1.72)	0.07 (0.30)	-0.29** (-2.14)	1.04 (1.11)	0.63 (0.54)
age	-0.03*** (-4.83)	-0.02 (-0.84)	0.02 (0.52)	0.10 (1.02)	0.18*** (3.12)
amount issued	0.087*** (4.22)	0.101 (1.27)	0.009 (0.09)	-0.715 (-1.04)	-0.571 (-0.72)
forecast dispersion	18.32*** (3.07)	0.13*** (3.75)	0.15*** (3.34)	0.76*** (7.31)	1.06*** (4.31)
coupon	0.10*** (4.46)	0.10** (2.07)	0.02 (0.17)	-0.50 (-1.34)	-0.09 (-0.19)
10y swap	-0.32*** (-6.18)	0.07 (0.24)	-0.09 (-0.22)	-1.33*** (-3.25)	-3.18*** (-3.05)
10y-2y swap	-0.400** (-2.17)	-0.490 (-1.58)	-0.820* (-1.95)	-0.962 (-1.23)	-1.962*** (-2.59)
equity vol	0.096*** (6.22)	0.055*** (3.82)	0.050*** (3.64)	0.050*** (3.06)	0.097*** (3.24)
pretax1	-0.836** (-2.17)	0.004 (0.21)	-0.098* (-1.80)	-0.051 (-0.53)	0.001 (0.44)
pretax2	0.422*** (5.33)	0.033 (0.93)	-0.000 (-0.00)	-0.073 (-0.53)	-0.442 (-0.53)
pretax3	0.144 (0.78)	-0.041*** (-2.59)	-0.003 (-0.37)	0.076 (0.81)	0.000 (NaN)
pretax4	0.003 (0.65)	0.052* (1.83)	0.008 (0.50)	-0.067 (-0.62)	0.000 (NaN)
sales to income	-0.108* (-1.68)	-0.003*** (-4.56)	-0.001*** (-3.79)	-0.002*** (-7.81)	-0.013 (-1.25)
long term debt to asset	-0.256*** (-2.67)	-0.009 (-0.71)	0.044** (2.40)	0.058 (1.56)	-0.108*** (-4.57)
leverage ratio	0.184* (1.92)	0.000 (0.00)	-0.026*** (-3.55)	-0.005 (-0.17)	0.106*** (13.08)
time-to-maturity	0.024*** (6.00)	-0.015 (-0.96)	-0.035* (-1.72)	-0.064 (-1.43)	-0.124*** (-2.63)
<i>N</i>	414	1549	2533	539	464
<i>R</i> <sup>2</sup>	0.84	0.71	0.67	0.79	0.72

Table 2.4: continued.

confidence bands by performing a wild cluster bootstrap of the regression residuals.

Table 2.5 shows the size of the liquidity component. We see that the liquidity component becomes larger as the rating quality of the bond decreases. For investment grade ratings, the component is small with an average pre-subprime across maturity of 0.8bp for AAA, 1.0bp for AA, 2.4bp for A, and 3.9bp for BBB. For speculative grade the liquidity component is larger and estimated to be 57.6bp.

Panel A: Liquidity component in basis points, pre-subprime  
(2004Q4-2007:Q1)

	average	0-2y	2-5y	5-30y	N 0-2y	N 2-5y	N 5-30y
AAA	0.8	0.6 (0.3;0.8)	0.9 (0.5;1.3)	1.1 (0.6;1.5)	162	178	193
AA	1.0	0.7 (0.3;1.1)	1.0 (0.4;1.7)	1.3 (0.5;2.2)	704	667	498
A	2.4	1.5 (0.6;2.3)	2.5 (1.1;3.9)	3.2 (1.4;4.9)	1540	1346	1260
BBB	3.9	2.8 (1.4;4.4)	4.0 (1.9;6.2)	4.7 (2.3;7.3)	517	270	553
spec	57.6	45.0 (32.3;57.4)	44.0 (31.5;56.0)	83.9 (60.2;106.8)	270	324	480

Panel B: Liquidity component in basis points, post-subprime  
(2007:Q2-2009:Q2)

	average	0-2y	2-5y	5-30y	N 0-2y	N 2-5y	N 5-30y
AAA	4.9	2.5 (0.5;4.4)	4.5 (0.9;8.0)	7.9 (1.7;14.1)	110	149	155
AA	41.8	23.5 (12.9;33.2)	37.1 (20.3;52.4)	64.7 (35.5;91.4)	493	572	483
A	50.7	26.6 (15.3;39.2)	51.0 (29.3;75.1)	74.5 (42.9;109.7)	762	878	890
BBB	92.7	64.3 (36.5;92.7)	115.6 (65.6;166.6)	98.1 (55.7;141.4)	123	159	256
spec	196.8	123.6 (80.2;157.3)	224.0 (145.3;285.1)	242.7 (157.4;308.8)	133	129	201

**Table 2.5: Liquidity Component in basis points.** For each rating  $R$  we run the pooled regression

$$spread_{it}^R = \alpha^R + \beta^R \lambda_{it} + \text{credit risk controls}_{it} + \epsilon_{it}$$

where  $i$  refers to bond,  $t$  to time, and  $\lambda_{it}$  is our liquidity measure. The bond spread is measured with respect to the swap rate. Within each rating and maturity bucket (0-2y, 2-5y, and 5-30y) we sort increasingly all values of  $\lambda_{it}$  and find the median value  $\lambda_{50}$  and the 5% value  $\lambda_5$ . The liquidity component in the bucket is defined as  $\beta(\lambda_{50} - \lambda_5)$ . This table shows for all buckets the liquidity component with standard errors in parenthesis. Confidence bands are found by a wild cluster bootstrap.



There is a strong increase in the liquidity component in the post-subprime period as Panel B in Table 2.5 shows. The component increases by a factor 10 or more in investment grade bonds of rating AA, A, and BBB while it increases by a factor 3-4 in speculative grade bonds. This shows that liquidity has dried out under the subprime crisis and part of the spread widening for bonds is due to a higher liquidity premium. Figure 2.1 shows the evolution of liquidity variables over the sample, and we see that the liquidity variables entering our measure of liquidity (Amihud, URC, Amihud risk, URC risk) all increase strongly after the onset of the subprime crisis. Thus, the higher liquidity premium is due to an increase in the sensitivity of spreads to illiquidity as well as higher levels of illiquidity.

While liquidity components in all ratings increase, we see that in absolute terms the increase in AAA bonds is modest. Even after the onset of the subprime crisis the component is 8 basis points or less, which is small compared to the component of other bonds. We see in Table 2.5 that the regression coefficient for AAA on the first principal component is small post-subprime compared to those of other rating classes, so the sensitivity of AAA-rated bonds to liquidity is small.<sup>8</sup> This suggests that there is a flight-to-quality into AAA bonds, namely that investors are buying high-quality AAA-rated bonds regardless of their liquidity.

The average liquidity premium in speculative grade bonds was 57.6bp pre-subprime, so even in this liquidity-rich period speculative grade bonds commanded a sizeable liquidity premium. Post-subprime the liquidity premium increased to 196.8bp for speculative grade bonds. An A rated bond has an average liquidity premium of 50.7bp post-subprime, so the illiquidity of such a bond post-subprime is similar to that of a speculative grade bond pre-subprime.

The size of the liquidity component pre-subprime is comparable in magnitude to the nondefault component in investment grade corporate bond spreads found by subtracting the CDS premium from the corporate - swap spread (swap basis), see Longstaff, Mithal, and Neis (2005), Blanco, Brennan, and Marsh (2005), and Han and Zhou (2008).<sup>9</sup> These papers look at recent periods before the subprime crisis and our pre-subprime results agree with their results in that there is a modest liquidity premium in investment grade corporate bond yields. The nondefault component for speculative bonds extracted from the swap basis is smaller and often negative, and the evidence presented here suggests that other factors than corporate bond liq-

<sup>8</sup>Strictly speaking, we use our measure  $\lambda$  to calculate liquidity components, but the regression coefficient on  $\lambda$  is close to the coefficient on IPC.

<sup>9</sup>Longstaff, Mithal, and Neis (2005) find an average nondefault component of -7.2bp for AAA/AA, 10.5bp for A, and 9.7bp for BBB, Han and Zhou (2008) find the nondefault component to be 0.3bp for AAA, 3.3bp for AA, 6.7bp for A, and 23.5bp for BBB, while Blanco, Brennan, and Marsh (2005) find it to be 6.9bp for AAA/AA, 0.5bp for A, and 14.9bp for BBB.

uidity are important for explaining the basis for speculative grade bonds.<sup>10</sup>

Turning to the term structure of liquidity, the general pattern across ratings and regime is that the liquidity component increases as maturity becomes higher. Overall, the premium in basis points is around twice as high for long maturity bonds compared to short maturity bonds. This seemingly contrasts the work of Ericsson and Renault (2006) who find a downward sloping term structure of liquidity. However, they use two data sets: one is transaction data from NAIC and the other is Datastream data, and only find support for a downward sloping liquidity effect in the Datastream data set. In light of the quality of Datastream data discussed earlier in this paper, we find it likely that conclusions based on actual transaction data are more reliable than those based on Datastream data.

To address how much of the corporate bond spread is due to liquidity, we find the fraction of the liquidity component to the total spread. For each bond we proceed as follows. We define the bond's liquidity component as  $\beta^R(\lambda_{it} - \lambda_{5t})$  where  $\lambda_{5t}$  is the 5% quantile of the liquidity scores. The liquidity component is then divided by the bond's yield spread to give an estimate of the fraction of the total yield spread that is due to illiquidity. Within each group we find the median liquidity fraction. We show later that the size of the liquidity component is robust to the choice of benchmark riskfree rate, but the liquidity fraction of the total spread is sensitive to the benchmark. The swap rate is chosen because there is mounting evidence that swap rates historically have been a better proxy for riskfree rates than Treasury yields (see for example Hull, Predescu, and White (2004) and Feldhütter and Lando (2008)).

Table 2.6 shows the fraction of the liquidity component to the total corporate-swap spread. The first parts of Panel A and B sort according to rating. We see that the fraction of spreads due to illiquidity is small for investment grade bonds, 11% or less. Using the ratio of the swap basis relative to the total spread, Longstaff, Mithal, and Neis (2005) and Han and Zhou (2008) find the fraction of spread due to liquidity at the 5-year maturity to be 2% respectively 19% consistent with our finding that it is relatively small. In speculative grade bonds the fraction due to liquidity is 24%. Post-subprime the fractions increase and range from 23 to 42 % in all ratings but AAA where it is only 7%. That the liquidity fractions of spreads in AAA are small in percent relative to other bonds underscores that there is a flight-to-quality effect in AAA bonds. A consistent finding from Tables 2.5 and 2.6 is that for investment grade bonds the importance of liquidity has increased after the onset of the subprime crisis both in absolute size (basis points) and relative to credit risk (fraction of spread). For speculative grade bonds the liquidity component in basis points has increased but it is stable measured as the fraction of total yield spread.

<sup>10</sup>Longstaff, Mithal, and Neis (2005) report an average of 17.6bp for BB, while Han and Zhou (2008) estimate it to be 2.8bp for BB, -53.5bp for B, and -75.4bp for CCC.

Panel A: Liquidity component in fraction of spread, pre-subprime  
(2005:Q1-2007:Q1)

	rating	AAA	AA	A	BBB	spec
fraction in pct		3 (2;5)	4 (2;7)	11 (5;18)	8 (3;12)	24 (18;30)
<i>N</i>		533	1869	4148	1340	1075

maturity	0-1y	1-2y	2-3y	3-4y	4-5y	5-8y	8-10y	10-30y
fraction in pct	3 (2;4)	7 (4;9)	13 (8;17)	13 (8;18)	13 (8;17)	11 (7;15)	8 (5;11)	10 (7;14)
<i>N</i>	1596	1613	1241	891	641	1187	578	1218

Panel B: Liquidity component in fraction of spread, post-subprime  
(2007:Q2-2009:Q2)

	rating	AAA	AA	A	BBB	spec
fraction in pct		7 (1;12)	42 (23;60)	26 (14;39)	29 (16;41)	23 (16;30)
<i>N</i>		414	1549	2533	539	464

maturity	0-1y	1-2y	2-3y	3-4y	4-5y	5-8y	8-10y	10-30y
fraction in pct	11 (7;14)	20 (13;27)	23 (15;31)	27 (18;38)	31 (20;42)	44 (28;60)	33 (21;44)	43 (28;53)
<i>N</i>	809	819	675	657	556	817	568	598

**Table 2.6: Liquidity component in fraction of spread.** For each rating  $R$  we run the pooled regression

$$spread_{it}^R = \alpha^R + \beta^R \lambda_{it} + \text{credit risk controls}_{it} + \epsilon_{it}$$

where  $i$  refers to bond,  $t$  to time, and  $\lambda_{it}$  is our liquidity measure. Within each rating we sort increasingly all values of  $\lambda_{it}$  and find the 5% value  $\lambda_5$ . For each bond we define the liquidity fraction of the total spread as  $\frac{\beta^R(\lambda_{it} - \lambda_5)}{spread_{it}^R}$ . The estimated fractions in the table are for each entry the median fraction. Confidence bands are found by a wild cluster bootstrap.

The last parts of Panel A and B in Table 2.6 show the liquidity fraction of total spread as a function of maturity. We introduce a fine maturity grid but do not sort according to rating in order to have a reasonable sample size in each bucket. We see that the fraction of the spread due to liquidity is small at short maturities and becomes larger as maturity increases. This is the case both pre- and post-subprime, although the fraction is higher post-subprime for all maturities. For example, post-subprime the fraction of spread due to liquidity is 43% for bonds with a maturity more than 10 years while it is 11 % for maturities less than 1 year. The fraction increases at maturities shorter than 5 years and thereafter flattens. The slight dip at the 8-10 year maturity both pre- and post subprime is due to an on-the-run effect; many bonds are issued with a maturity of 10 years and are more liquid right after

issuance.<sup>11</sup>

We find strong differences in the pre- and post-subprime periods, and in order to examine potential variation within the two periods more closely, we estimate monthly variations in liquidity and spreads as follows. Each month we a) find a regression coefficient  $\beta_t$  by regressing spreads on  $\lambda$  while controlling for credit risk, b) calculate for each bond the fraction due to illiquidity,  $\frac{\beta_t(\lambda_{it}-\lambda_{5t})}{spread_{it}}$ , c) find the median fraction, and d) multiply this fraction by the median spread. This gives us the total liquidity premium in basis points on a monthly basis. We do this for investment grade and speculative grade bonds separately.<sup>12</sup> This measures the amount of the total spread that is due to illiquidity. Figure 2.4 shows the time series variation in the median spread and the amount of the spread due to illiquidity.

The liquidity premium in investment grade bonds is persistent and steadily increasing during the subprime crisis and peaks in the first quarter of 2009 when stock prices decreased strongly. We see that the co-movement between the liquidity premium and credit spread is quite high. For speculative grade bonds, the liquidity premium peaks around the bankruptcy of Lehman and shows less persistence. Furthermore, the co-movement between the liquidity premium and the spread is less pronounced than for investment grade bonds, and the premium at the end of the sample period is almost down to pre-crisis levels even though the spread is still higher than before the crisis.

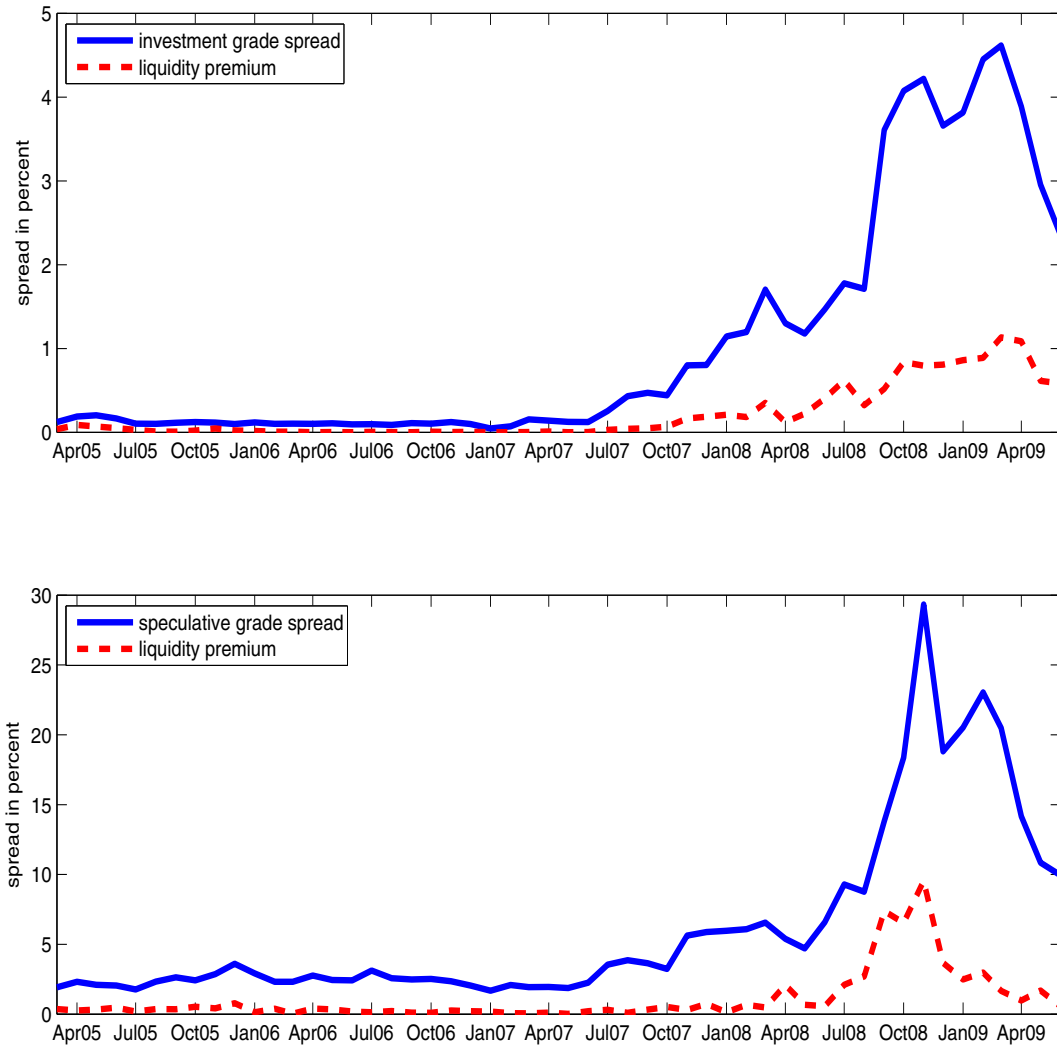
### 2.5.5 Robustness checks

In Appendix 2.8 we carry out a series of robustness checks. We test for potential endogeneity bias and find that endogeneity is not a major concern. We calculate liquidity premia using corporate bond spreads to Treasury rates instead of swap rates and find that our conclusions still hold. And we examine an alternative definition of our liquidity component and find results to be robust to this definition. We have also tried to exclude bonds with an age less than one or two years and find that our conclusions hold, a result not in the Appendix but available on request.

As a further test showing that our regression results are robust, we provide a different methodology for controlling for credit risk in this section. Longstaff, Mithal, and Neis (2005), Blanco, Brennan, and Marsh (2005), and Han and Zhou (2008) control for credit risk by assuming that the premium in a credit default swap is a pure measure of credit default risk. However, credit default swaps are shown also to contain a liquidity component (Tang and Yan (2006) and Bongaerts, Driessen, and de Jong (2009)) and

<sup>11</sup>To support this claim we additionally sorted according to bond age (older and younger than 2 years). After this sort, the dip at the 8-10 year maturity was not present. Results are available on request.

<sup>12</sup>The results become unstable if we split into finer rating categories. While the regression coefficient  $\beta_t^R$  can be determined reasonable well, the 5% quantile  $\lambda_{5t}$  becomes too noisy.



**Figure 2.4: Liquidity premium and total spread for investment grade and speculative grade bonds.** This graph shows for investment grade and speculative grade yield spreads the variation over time in the amount of the spread that is due to illiquidity and the total yield spread. On a monthly basis, the fraction of the yield spread that is due to illiquidity is calculated as explained in Section 2.5.4. This fraction multiplied by the median yield spread is the amount of the spread due to illiquidity and plotted along with the median yield spread.

this component is likely to have increased after the onset of the subprime crisis. Furthermore, only a small number of firms have actively traded credit default swaps, and those that have typically only have liquid swaps at a maturity of five years. Using credit default swaps would severely reduce our sample size.

We use an alternative approach to check our credit risk controls. The idea is that any yield spread difference between two fixed rate bullet bonds with the same maturity and issued by the same firm must be due to liquidity differences and not differences in credit risk. This intuition is formalized in the following regression.

We conduct rating-wise "paired" regressions of yield spreads on dummy variables and one liquidity measure at the time. The regression is

$$spread_{it}^R = dummy_{Gt}^R + \beta^R \lambda_{it}$$

where  $dummy_{Gt}^R$  is the same for all bonds with the same rating  $R$  and approximately the same maturity. The grid of maturities is 0-0.5y, 0.5-1y, 1-3y, 3-5y, 5-7y, 7-10y, and more than 10y. For example, if firm  $y$  in quarter  $t$  has three bonds issued with maturities 5y, 5.5y, and 6y, the bonds have the same dummy in that quarter, and we assume that any yield spread difference between the bonds is due to liquidity. There are separate dummies for each quarter. Once we have dummied out credit risk in the regressions, estimated coefficients for the liquidity measure are not inconsistent because of possibly omitted credit risk variables. Hence, the paired regression is free of any endogeneity bias due to credit risk. Only groups with two or more spreads contribute to the liquidity coefficient reducing the sample compared to former regressions. Therefore, we only look at two rating groups, investment grade and speculative grade.

Table 2.7 shows the regression coefficients in the paired regression. We see that  $\lambda$  is significant in all regressions while zero trading days, the Roll measure, and turnover are only significant in some of the regressions. This supports our finding that  $\lambda$  is a more consistent measure of corporate bond liquidity compared to previously proposed measures.

## 2.6 Determinants of bond illiquidity

In this section, we show that our measure is also useful for analyzing other aspects of corporate bond illiquidity. Specifically, we focus on liquidity betas, the liquidity of bonds with a lead underwriter in financial distress, and the liquidity of bonds issued by financial firms relative to bonds issued by industrial firms.

### 2.6.1 Liquidity betas

We estimate bond-specific liquidity betas by calculating a monthly time series of corporate bond market illiquidity, and for each bond estimate the

	pre-subprime		post-subprime	
	investment	spec	investment	spec
$\lambda$	0.01*** (3.79)	0.09** (2.43)	0.12*** (3.58)	0.41* (1.95)
Amihud	2.26*** (5.11)	16.80*** (3.51)	16.10*** (3.04)	54.65 (1.54)
Roll	0.03*** (3.56)	0.16** (2.54)	0.05** (2.14)	0.39 (1.44)
bond zero	0.00*** (5.85)	0.01** (2.28)	0.00 (0.78)	0.03 (1.12)
turnover	0.11* (1.87)	1.48* (1.72)	-3.21 (-1.46)	72.74 (1.63)
URC	8.48*** (3.72)	125.03** (2.55)	104.34** (2.43)	-95.04 (-0.58)
URC risk	1.30 (0.69)	57.15** (2.15)	39.09*** (2.97)	-103.42 (-0.74)
Amihud risk	0.64*** (4.21)	9.44*** (2.79)	6.56*** (3.19)	39.63*** (4.60)

**Table 2.7: Paired regression.** We pair bonds from the same firm with similar maturity and regress their yield spreads on liquidity variables one at a time and add a dummy for a given firm and maturity combination. Since bonds with similar maturity and issued by the same firm have similar credit risk characteristics, the dummy controls for credit risk. Significance at 10% level is marked '\*', at 5% marked '\*\*', and at 1% marked '\*\*\*'.

correlation between market-wide illiquidity and bond-specific illiquidity. The market-wide time series is calculated by averaging on a monthly basis across all observations of bond-specific  $\lambda_i$  using amount outstanding as weight. Bond-specific beta is estimated through the slope coefficient in the regression of bond-specific  $\lambda_i$  on market-wide  $\lambda$ , where the regression is based on all months where a bond-specific  $\lambda_i$  can be calculated. We calculate the betas using the whole sample period 2004Q4-2009Q2, because estimating betas separately for the pre- and post subprime periods leads to noisier estimates.

For each rating class  $R$  pooled regressions are run where yield spreads are regressed on each bonds liquidity  $\beta$  and our liquidity measure  $\lambda_t$  with credit risk controls

$$Spread_{it}^R = \alpha^R + \gamma_1^R \lambda_{it} + \gamma_2^R \beta_i + \text{credit risk controls}_{it} + \epsilon_{it}$$

where  $i$  is for bond in rating  $R$  and  $t$  is time measured in quarter.

The result of the regression is reported in Table 2.8. Our regressions are run both 'marginally', i.e. with our liquidity beta as the only regressor in addition to the credit risk controls, and with our liquidity measure included as additional regressor.

Both marginally and with  $\lambda$  included, there is no significance pre-subprime except for the AAA-category. After the onset of the crisis, the picture changes and only spreads in the AAA-category do not depend on our liquidity beta. This is consistent with the regime-dependent importance of liquidity betas noted in Acharya, Amihud, and Bharath (2010). But whereas

	pre-subprime		post-subprime	
	$\beta$	$\lambda$	$\beta$	$\lambda$
AAA	-0.0034 (-1.34) -0.0056*** (-3.26)	0.0033*** (2.65)	-0.0085 (-0.84) 0.0159 (1.26)	0.0234** (2.38)
AA	0.0012 (0.23) 0.0067 (1.06)	0.0017 (0.60)	0.1823* (1.94) 0.1720** (2.14)	0.1712*** (3.82)
A	-0.0004 (-0.14) 0.0021 (0.65)	0.0106** (2.57)	0.2631** (2.22) 0.2314** (2.15)	0.1211** (2.03)
BBB	0.0044 (1.34) 0.0012 (0.34)	0.0254*** (4.33)	0.2171*** (4.05) 0.3187*** (3.44)	0.3242*** (2.91)
spec	0.0102 (0.90) 0.0162 (1.31)	0.1502*** (4.64)	1.3538*** (2.60) 1.3140** (2.73)	0.4155*** (7.08)

**Table 2.8:  $\beta$  regressions.** For each rating class  $R$  pooled regressions are run where yield spreads are regressed on each bonds liquidity  $\beta$  and our liquidity measure  $\lambda_t$  with credit risk controls

$$Spread_{it}^R = \alpha^R + \gamma_1^R \lambda_{it} + \gamma_2^R \beta_i + \text{credit risk controls}_{it} + \epsilon_{it}$$

where  $i$  is for bond in rating  $R$  and  $t$  is time measured in quarter. Each bond's  $\beta_i$  is calculated as the covariance between this bond's monthly  $\lambda_{it}$  and a size-weighted monthly market  $\lambda_{Mt}$ . Two regressions for each rating pre- and post-subprime are run; one with only  $\beta$  included and one with both  $\beta$  and  $\lambda$  included. Standard errors are corrected for time series effects, firm fixed effects, and heteroscedasticity, and significance at 10% level is marked '\*\*', at 5% marked '\*\*\*', and at 1% marked '\*\*\*\*'.

they use stock and Treasury bond market liquidity to measure aggregate liquidity, our measure specifically captures corporate bond market liquidity.

We saw in the previous section that the contribution to spreads of liquidity was small for AAA bonds after the onset of the crisis, and the insignificant liquidity beta coefficient for AAA in the crisis period confirms that there is a flight-to-quality effect in AAA-rated bonds.

## 2.6.2 Lead underwriter

Brunnermeier and Pedersen (2009) provide a model that links an asset's market liquidity and traders' funding liquidity, and find that when funding liquidity is tight, traders become reluctant to take on positions, especially "capital intensive" positions in high-margin securities. This lowers market liquidity. Empirical support for this prediction is found in Comerton-Forde, Hendershott, Jones, Moulton, and Seasholes (2010) who find for equities traded on NYSE that balance sheet and income statement variables for mar-



ket makers explain time variation in liquidity.

Since the TRACE data do not reveal the identity of the traders, we cannot perform direct tests of the Brunnermeier and Pedersen (2009)-model for the U.S. corporate bond market. However, if we assume that the original underwriter is more likely to make a market (as is the case in equity markets, see Ellis, Michaely, and O'Hara (2000)), we can provide indirect evidence by observing bond liquidity of bonds underwritten by Bear Stearns and Lehman Brothers, two financial institutions in distress during the subprime crisis. We therefore calculate for all bonds with Lehman Brothers as lead underwriter their average  $\lambda$  - weighted by amount outstanding - on a monthly basis. Likewise, we do this for bonds with Bear Stearns as lead underwriter and for all bonds in the sample. We obtain underwriter information from FISD. The results are plotted in Figure 2.5.

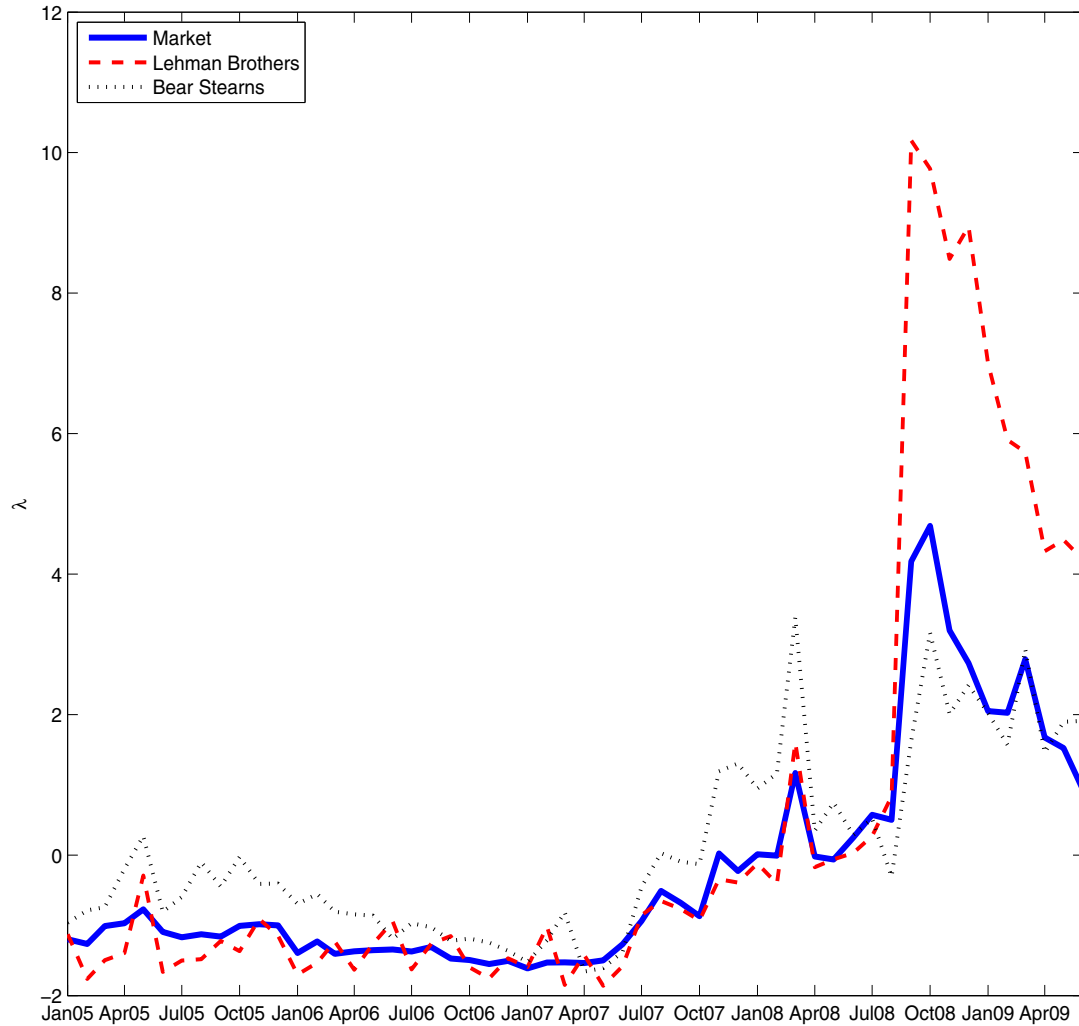
The liquidity of bonds with Bear Stearns as lead underwriter was roughly the same as an average bond entering the summer of 2007. During the week of July 16, 2007 Bear Stearns disclosed that two of their hedge funds had lost nearly all of the value, and the graph shows that the 'illiquidity gap' between Bear Stearns underwritten bonds and average bonds increased this month. On August 6, Bear Stearns said that it was weathering the worst storm in financial markets in more than 20 years, in November 2007 Bear Stearns wrote down \$1.62 billion and booked a fourth quarter loss, and in December 2007 there was a further write-down of \$1.90 billion. During these months, the 'illiquidity gap' steadily increased. Bear Stearns were in severe liquidity problems in beginning of March, and they were taken over by JPMorgan on March 16. In this month the 'illiquidity gap' peaked but returned to zero in June 2008 after Bear Stearns shareholders approved JPMorgan's buyout of the investment bank on May 29.

The liquidity of bonds underwritten by Lehman was close to the liquidity of an average bond in the market up until August 2008, but this changed in September 2008 when the 'liquidity gap' between Lehman underwritten bonds and average market bonds increased strongly in September in response to Lehman filing for bankruptcy on September 15. The gap stayed at high levels during the rest of the sample period suggesting that after the Lehman default, bonds they had underwritten became permanently more illiquid.

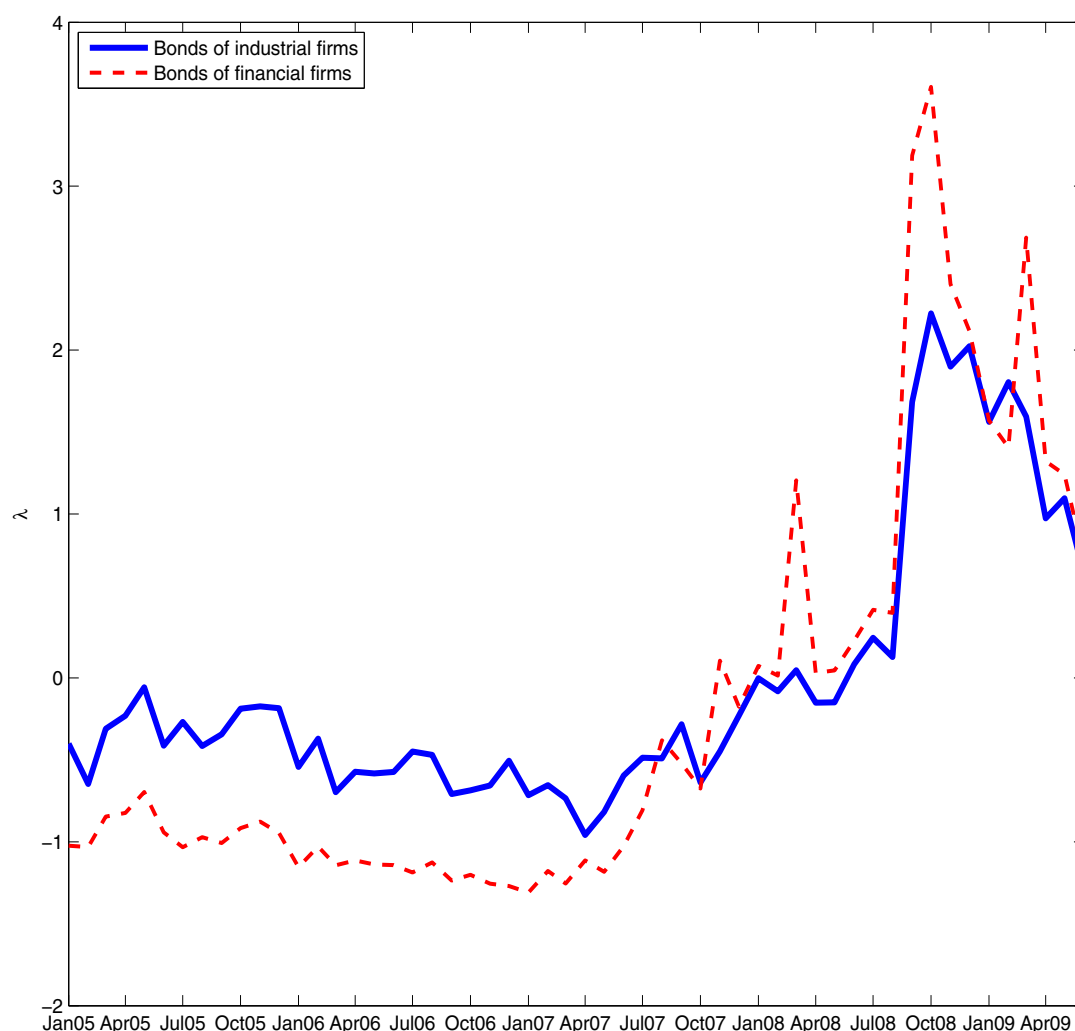
### **2.6.3 Industry**

Bonds issued by financial firms might be more or less liquid compared to bonds issued by industrial firms. They might be less liquid because financial firms are more opaque, especially in times of financial distress, and their bonds might be more affected by asymmetric information. They might be more liquid because financial firms are more connected to capital markets and are liquidity providers to the market.

The empirical evidence is mixed. Longstaff, Mithal, and Neis (2005)



**Figure 2.5: Illiquidity of bonds underwritten by Lehman Brothers and Bear Stearns.** This graph shows the time series variation in illiquidity of bonds with Lehman Brothers as lead underwriter, bonds with Bear Stearns as lead underwriter, and all bonds in the sample. For every bond underwritten by Lehman Brothers their (il)liquidity measure  $\lambda$  is calculated each month and a monthly weighted average is calculated using amount outstanding for each bond as weight. The graph shows the time series of monthly averages. Likewise, a time series of monthly averages is calculated for bonds with Bear Stearns as a lead underwriter and all bonds in the sample. Higher values on the y-axis imply more illiquid bonds.



**Figure 2.6: Illiquidity of bonds of industrial and financial firms.** This graph shows the time series variation in illiquidity of bonds of industrial and financial firms. For every bond issued by a financial firm their (il)liquidity measure  $\lambda$  is calculated each month and a monthly weighted average is calculated using amount outstanding for each bond as weight. The graph shows the time series of monthly averages. Likewise, a time series of monthly averages is calculated for bonds issued by industrial firms. Higher values on the y-axis imply more illiquid bonds.

find in a study of 68 bonds that bonds issued by financial firms are more illiquid and command a higher liquidity premium. In contrast, Friewald, Jankowitsch, and Subrahmanyam (2009) find that there is no difference, except during the subprime crisis where bonds of financial firms are in fact more liquid.

We address the issue by calculating a value-weighted average monthly illiquidity  $\lambda$  of financial respectively industrial firms and plotting the time series behavior in Figure 2.6. We obtain bond industry from FISD. In general, there is little systematic difference. For both financial and industrial bonds, illiquidity goes up at the onset of the crisis. There are, however, additional spikes in illiquidity for financial firms around the takeover of Bear Stearns in March 2008, around the Lehman bankruptcy in September 2008, and around the stock market decline in the first quarter of 2009. That is, in times of severe financial distress, illiquidity of financial bonds increases relative to that of industrial bonds, while in other times illiquidity is similar.

By calculating monthly averages, we are able to draw more high-frequency inferences compared to other papers, since averaging  $\lambda$  over longer periods of time, the approach taken in Longstaff, Mithal, and Neis (2005) and Friewald, Jankowitsch, and Subrahmanyam (2009), would wash out the effects we see.

## 2.7 Conclusion

The subprime crisis dramatically increased corporate bond spreads and while default risk certainly has increased because of funding constraints and the slowing of the real economy, it is also widely believed that deteriorating liquidity has contributed to the widening of spreads. The difficulty is how to measure this contribution.

In this paper, we show that an equally weighted sum of four (normalized) measures of liquidity and liquidity risk consistently contributes to corporate bond spreads across time and across ratings. The four measures are the Amihud measure of price impact, a measure of roundtrip trading costs and the variability of these two measures. The equally weighted sum is a close approximation to the first factor in a principal component analysis of eight liquidity measures, and this is true both before and after the onset of the crisis. Our measure dominates other liquidity measures, such as the Roll measure and zero trading days.

The measure is used to analyze the contribution of illiquidity to corporate bond spreads before and after the onset of the subprime crisis. We find that before the crisis, the contribution to spreads from illiquidity was small for investment grade bonds both measured in basis points and as a fraction of total spreads. The contribution increased strongly at the onset of the crisis for all bonds except AAA-rated bonds, which is consistent with a flight-to-quality into AAA-rated bonds. Liquidity premia in investment grade bonds rose steadily during the crisis and peaked when the stock market declined

strongly in the first quarter of 2009, while premia in speculative grade bonds peaked during the Lehman default and returned almost to pre-crisis levels in mid-2009. The number of zero trading days did not increase with the crisis and we find evidence that this was because trades in less liquid bonds were split into trades of smaller size.

Our measure is useful for analyzing other important aspects of corporate bond liquidity. From the covariation between an individual bond's liquidity measure and a value-weighted average of all bonds' liquidity measures, we define a liquidity beta which is shown to have little effect on spreads before the onset of the crisis, but does have a positive effect for all bonds except AAA-bonds after the crisis. This is consistent with the regime-dependent role of liquidity betas found in Acharya, Amihud, and Bharath (2010) but it narrows the flight-to-quality story from general investment grade bonds to AAA-rated bonds only.

We also use our measure to study the impact on bond liquidity of funding shocks to lead underwriters and to compare illiquidity of corporate bonds issued by financial firms with that of industrial firms. Financial distress of lead underwriters clearly affects the liquidity of the bonds for which they have served as lead underwriters. Bonds issued by financial firms are not permanently more or less liquid than industrials but they do, however, have illiquidity spikes around the take-over of bear Sterns, the collapse of Lehman and the March 2009 rapid stock market decline.

## 2.8 Appendix: Robustness checks

In this Appendix we discuss possible misspecification in our regression analysis. We test for endogeneity, show that our results are robust to the choice of benchmark riskfree rate, and show that results are robust to how we define the liquidity component.

### 2.8.1 Endogeneity

There may be a two way causal relationship between contemporaneous measures of liquidity and credit risk and failing to account for such a relationship in regressions results in inconsistent OLS estimates. This simultaneity bias is not a concern in our regressions since liquidity measures lag our measure of credit spreads. Spreads are measured on the last day in each quarter while liquidity measures are based on transactions during the quarter, so liquidity measures are lagged in time relative to spreads.

To test for potential endogeneity bias, we use a residual augmented two stage least squares t-test as in Davidson and MacKinnon (1993), equivalent to the Durbin-Wu-Hausman test. We do this for every marginal regression in Table 2.2, that is, test every liquidity variable separately. If the test is not significant the liquidity variable can be regarded as exogenous. As instrument we use bond age and therefore exclude it in the yield spread regressions<sup>13</sup>. Table 2.9 shows the  $R^2$ 's for the first stage regressions and the t-statistic tests for endogeneity. Most  $R^2$ 's are relatively high indicating that the control variables including the instrument are able to explain a large portion of the variation in the liquidity measures. Out of the 80 test statistics 80% are insignificant at a 10% level indicating that endogeneity is not a major concern.

### 2.8.2 Benchmark riskfree rate

The size of the nondefault component in corporate bond spreads investigated by among others Huang and Huang (2003) and Longstaff, Mithal, and Neis (2005) depend strongly on the chosen riskfree rate. In Longstaff, Mithal, and Neis (2005) the difference is around 60 basis points. As Table 2.10 shows the estimated liquidity component when the Treasury rate is used as riskfree rate instead of the swap rate does not change much. The change in estimated liquidity is often less than one basis point and is for all rating categories less than 10 basis points. Therefore, our findings on the size of the liquidity premium in basis points are insensitive to the choice of benchmark

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<sup>13</sup>Another potential instrument is amount issued. Since this variable is significant in most of the regressions in Table 2.4, omitting it from the regressions in the test creates a new endogeneity problem. The tests in this case would likely show an endogeneity problem even if it is not there, and if we use amount issued as instrument, this is indeed the case.

Panel A: Endogeneity tests, pre-subprime (2004:Q4-2007:Q1)

	AAA	AA	A	BBB	spec
Amihud	−0.43 (33%)	−1.00 (20%)	0.98 (18%)	1.31 (9%)	0.71 (34%)
Roll	0.66 (47%)	−0.98 (30%)	0.98 (32%)	1.16 (24%)	−0.45 (25%)
firm zero	−0.25 (88%)	1.08 (34%)	−0.83 (23%)	−1.18 (25%)	0.27 (46%)
bond zero	−0.41 (83%)	1.04 (67%)	−0.69 (68%)	0.85 (45%)	−0.87 (61%)
turnover	−0.18 (19%)	−1.13 (28%)	0.86 (15%)	−1.05 (29%)	1.04 (39%)
URC	0.51 (34%)	−1.08 (18%)	0.95 (19%)	1.45 (23%)	0.13 (37%)
Amihud risk	0.45 (19%)	−1.09 (10%)	0.89 (11%)	1.43 (13%)	0.31 (31%)
URC risk	0.46 (13%)	−1.08 (12%)	0.90 (11%)	1.29 (14%)	−0.03 (33%)

Panel B: Endogeneity tests, post-subprime (2007:Q2-2009:Q2)

	AAA	AA	A	BBB	spec
Amihud	−5.03*** (41%)	−1.06 (31%)	−0.20 (30%)	−0.60 (27%)	−2.82*** (42%)
Roll	−5.24*** (33%)	−1.15 (15%)	0.51 (21%)	0.77 (16%)	−2.89*** (23%)
firm zero	5.50*** (87%)	−1.12 (35%)	−0.40 (24%)	−0.82 (44%)	−3.06*** (58%)
bond zero	6.40*** (79%)	1.10 (73%)	−0.21 (70%)	−0.70 (68%)	−3.26*** (76%)
turnover	−6.17*** (27%)	−1.15 (16%)	0.32 (17%)	0.73 (20%)	2.91*** (36%)
URC	−4.94*** (50%)	−0.84 (42%)	−0.26 (49%)	0.77 (39%)	−2.72*** (63%)
Amihud risk	−5.07*** (21%)	−1.05 (22%)	−0.36 (34%)	−0.59 (45%)	−2.69*** (50%)
URC risk	−4.82*** (39%)	−0.74 (34%)	0.57 (48%)	−0.75 (34%)	−2.75*** (55%)

**Table 2.9: Endogeneity tests.** For each rating class  $R$  and each liquidity variable  $L$  we test for potential endogeneity bias by using a Durbin-Wu-Hausman test. In total 56 tests are run (8 liquidity variables  $\times$  5 rating classes) pre- and post-subprime. This table shows for each test the t-statistics and  $R^2$  for the first stage regression in parenthesis. The proxies are described in detail in Section 2.4 and are calculated quarterly from 2004 : Q4 to 2009 : Q2. Panel A shows the coefficients using data before the subprime crisis, while Panel B shows the coefficients using data after the onset of the subprime crisis. Significance at 10% level is marked '\*', at 5% marked '\*\*', and at 1% marked '\*\*\*'.

Panel A: Liquidity component in basis points, pre-subprime  
(2004Q4-2007:Q1)

	average	0-2y	2-5y	5-30y	N 0-2y	N 2-5y	N 5-30y
AAA	1.6	1.1 (0.8;1.4)	1.7 (1.2;2.1)	2.0 (1.4;2.5)	162	178	193
AA	1.7	1.1 (0.8;1.5)	1.8 (1.3;2.3)	2.3 (1.6;3.0)	704	667	498
A	2.8	1.7 (0.9;2.6)	2.9 (1.5;4.3)	3.8 (1.9;5.5)	1540	1346	1260
BBB	4.0	2.9 (1.4;4.4)	4.1 (1.9;6.2)	4.9 (2.3;7.3)	517	270	553
spec	57.8	45.2 (33.9;57.4)	44.1 (33.1;56.0)	84.2 (63.2;106.9)	270	324	480

Panel B: Liquidity component in basis points, post-subprime  
(2007:Q2-2009:Q2)

	average	0-2y	2-5y	5-30y	N 0-2y	N 2-5y	N 5-30y
AAA	1.0	0.5 (0.3;5.4)	0.8 (0.5;8.1)	1.7 (0.9;16.6)	110	149	155
AA	40.6	22.9 (11.5;35.2)	36.1 (18.2;55.5)	63.0 (31.8;96.8)	493	572	483
A	47.6	25.0 (12.9;37.6)	47.9 (24.7;72.1)	70.0 (36.1;105.4)	762	878	890
BBB	94.0	65.2 (36.0;97.4)	117.2 (64.8;175.1)	99.5 (55.0;148.6)	123	159	256
spec	189.9	119.3 (79.4;154.9)	216.3 (144.0;280.9)	234.2 (156.0;304.2)	133	129	201

**Table 2.10: Liquidity Component in basis points when the Treasury rate is used as riskfree rate.** For each rating  $R$  we run the pooled regression

$$spread_{it}^R = \alpha^R + \beta^R \lambda_{it} + \text{credit risk controls}_{it} + \epsilon_{it}$$

where  $i$  refers to bond,  $t$  to time, and  $\lambda_{it}$  is our liquidity measure. The bond spread is measured with respect to the Treasury yield. Within each rating and maturity bucket (0-2y, 2-5y, and 5-30y) we sort increasingly all values of  $\lambda_{it}$  and find the median value  $\lambda_{50}$  and the 5% value  $\lambda_5$ . The liquidity component in the bucket is defined as  $\beta(\lambda_{50} - \lambda_5)$ . This table shows for all buckets the liquidity component with standard errors in parenthesis. Confidence bands are found by a wild cluster bootstrap.



(while our findings on the fraction out of the total spread of course depend on the benchmark riskfree rate).

### **2.8.3 Alternative definition of liquidity component**

The liquidity component is calculated as the the median minus 5% quantile of the liquidity score and has the natural interpretation as the liquidity premium of an average bond in the corporate bond market relative to a very liquid bond. To check that our main results are robust to the definition of the liquidity component, Table 2.11 shows the liquidity component when it is defined as the 75% quantile minus 5% quantile. The component in this table can be interpreted as that of an illiquid bond relative to a very liquid bond. Table 2.11 shows that the liquidity component is larger for an illiquid bond compared to an average bond (which by definition must the case). Also, Table 2.11 shows that the main results of the paper are unchanged: liquidity premia are increasing in maturity, the liquidity premium is higher post-subprime compared to pre-subprime, and the liquidity premium for investment grade bonds is small pre-subprime.

Panel A: Liquidity component in basis points, pre-subprime  
(2004Q4-2007:Q1)

	average	0-2y	2-5y	5-30y	N 0-2y	N 2-5y	N 5-30y
AAA	1.4	1.0 (0.5;1.3)	1.2 (0.7;1.7)	2.0 (1.1;2.8)	162	178	193
AA	1.7	1.1 (0.4;1.7)	1.6 (0.6;2.6)	2.4 (0.9;3.8)	704	667	498
A	4.4	2.8 (1.2;4.3)	4.3 (1.8;6.8)	6.1 (2.6;9.6)	1540	1346	1260
BBB	8.4	5.8 (2.4;9.1)	8.9 (3.6;13.9)	10.4 (4.2;16.3)	517	270	553
spec	117.1	81.5 (61.2;104.4)	90.4 (67.9;115.8)	179.4 (134.6;229.6)	270	324	480

Panel B: Liquidity component in basis points, post-subprime  
(2007:Q2-2009:Q2)

	average	0-2y	2-5y	5-30y	N 0-2y	N 2-5y	N 5-30y
AAA	9.2	4.4 (0.9;7.9)	8.0 (1.7;14.2)	15.2 (3.2;27.3)	110	149	155
AA	68.5	37.8 (21.2;53.4)	64.0 (35.8;90.5)	103.9 (58.1;146.9)	493	572	483
A	92.6	53.8 (29.4;78.8)	95.9 (52.5;140.6)	128.1 (70.1;187.7)	762	878	890
BBB	176.5	138.6 (76.0;203.3)	201.6 (110.5;295.6)	189.4 (103.8;277.8)	123	159	256
spec	420.5	294.0 (196.2;383.0)	390.5 (260.6;508.7)	577.1 (385.2;751.8)	133	129	201

**Table 2.11: Liquidity Component in basis points for an illiquid bond.** For each rating  $R$  we run the pooled regression

$$spread_{it}^R = \alpha^R + \beta^R \lambda_{it} + \text{credit risk controls}_{it} + \epsilon_{it}$$

where  $i$  refers to bond,  $t$  to time, and  $\lambda_{it}$  is our liquidity measure. The bond spread is measured with respect to the swap rate. Within each rating and maturity bucket (0-2y, 2-5y, and 5-30y) we sort increasingly all values of  $\lambda_{it}$  and find the 75% value  $\lambda_{75}$  and the 5% value  $\lambda_5$ . The liquidity component in the bucket is defined as  $\beta(\lambda_{75} - \lambda_5)$ . This table shows for all buckets the liquidity component with standard errors in parenthesis. Confidence bands are found by a wild cluster bootstrap.

## Essay 3

# Index Driven Price Pressure for Corporate Bonds <sup>1</sup>

### Abstract

Revisions of the Lehman/Barclay corporate bond index are completely information free events and have no long term impact on supply and demand. However, both included and excluded bonds experience a temporary price pressure at the revision dates. The temporary change in price is usually fully reversed in the days after the revision. Trading activity spikes at the revision dates consistent with index trackers seeking to minimize tracking error. The price pressure is significant for newly issued bonds included into the index, for maturing bonds excluded from the index and for downgraded bonds excluded from the index. Furthermore, dealers only participate as liquidity providers when the reversal return exceeds the bid-ask spread and dealers otherwise perform a matching function.

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<sup>1</sup>I thank Peter Feldhütter, Søren Hvidkjær, David Lando, Mads Stenbo Nielsen, Lars Jul Overby and Ilya Strebulaev for helpful comments. All errors are my responsibility.

### 3.1 Price Pressure

When a stock enters a major index it experiences a temporary price increase followed by a partial reversal. There exists several competing theories that aim at explaining the stock price reaction following an index revision. This paper is the first to test similar theories for a corporate bond index. Revisions to the Lehman/Barclay Corporate Bond Index mainly happen for four different reasons. Newly issued corporate bonds are included into the index at the last trading day of the month if they meet certain size and rating requirements. Bonds, that fall below one year to maturity, are excluded from the index at the last trading day of the month. Upgraded or downgraded bonds get their index status changed also at the last trading day of the month of which their ratings were changed. Hence, revisions are always known long before the rebalancing date and new information therefore has time to be impounded into bond prices before the index is rebalanced. Contrary to the S&P 500 index, this practice clearly makes bond index revisions information free events. At the same time bond indices are not limited to a certain number bonds. So nothing suggests that index inclusions should raise investor awareness. Any price reaction at corporate bond index revisions therefore has to be driven by changes in demand and market frictions.

Trading activity spikes at the rebalancing dates both for included and excluded bonds. The spike in trading activity is consistent with index trackers seeking to minimize their tracking error by trading as close to the rebalancing date as possible. When the index trackers are buying bonds that enter the index the price goes up in the days leading up to the rebalancing event, only to be followed by a reversal of equal size over the next 5 to 10 days. When index trackers are selling excluded bonds the price first drops and is then also reversed in the days after the event. Both price reactions are consistent with temporary price pressure or short term downward sloping demand curves for bonds. There is no evidence for a longer term shift in demand following the rebalancing event, so it is not possible to study demand curves for bonds in the long run through this event. Newly issued bonds that get included in the index experience an abnormal return of 9.8 bps followed by a reversal of -11.0 bps, both significant at a 1% level. Bonds that get excluded because the maturity falls below 1 year experience an abnormal return of -58.0 bps followed by a reversal of 58.7 bps and again both abnormal returns are significant at a 1% level. The price pressure return for downgraded bonds is -935.1 bps whereas the reversal return is only 348.7 bps. Still, the significant reversal return, which is not otherwise found at bond downgrades, suggests that part of the negative return is price pressure. Only bonds that get included because of an upgrade to investment grade show insignificant abnormal returns at the index inclusion. The insignificant returns for upgraded bonds, should be seen in connection with the trading strategies of index trackers, which make the upgraded bonds less

attractive. In contrast with stock market index tracking strategies, bond index trackers are not replicating the index. To keep transaction costs low, bond index trackers sample the index and only hold about one-third of the bonds in the index, which is still around 800 bonds for the Lehman/Barclay corporate bond index. In line with this, we find that index trackers have an affinity for large bond issues with long maturities that reduce the need for frequent trading. Upgraded bonds are thus competing with newly issued bonds for a place in an index tracker's portfolio. Since newly issued bonds on average have more time to maturity than upgraded bonds, the upgraded bonds are less attractive for index trackers.

The temporary price pressure around index rebalancing is consistent with theoretical models of trading in over-the-counter markets such as Duffie, Gârleanu, and Pedersen (2007). In Duffie, Gârleanu, and Pedersen (2007) buyers and sellers are searching for counterparties with certain intensities. Once they meet a counterparty they negotiate over the price with bargaining powers determined by their respective alternatives. When there is a positive demand or supply shock the bargaining power shifts, causing a price change and trading volume goes up at the same time. Index trackers entering the market around the rebalancing date seeking to buy a bond constitute a positive demand side shock. Following the shock, the supply side has better opportunities of finding counterparties, hence their bargaining power goes up. If this is the case the price should increase temporarily and then return to the pre-shock price once the index trackers are out of the market again. This price pattern assumes that the excess demand from index trackers does not affect the post-shock supply side significantly. Empirically, we find that the longer term post event trading activities are not affected by the index event itself.

Further, we find that when the abnormal temporary return caused by index trackers exceeds the size of an average bid-ask spread, dealers participate in the event as liquidity providers. The dealers trade against their inventory when index trackers buy or sell and in the following days the dealers bring their inventory back to the pre-event level earning a profit from the price reversal. In the other cases, where the bid-ask spread exceeds the abnormal price pressure and reversal return, and also in the case of the downgrade event itself, the dealers only perform a matching service earning the bid-ask spread.

### 3.2 Index Tracking

Abnormal returns around stock index revisions have been intensely studied, but no attention has so far been given to the monthly revisions of corporate bond indices. We therefore turn to the stock market literature on index revisions for evidence on how mutual funds track indices and why there is a price reaction to index revisions. This section starts out by reviewing how

stock index trackers time their trading. In the later empirical parts of the paper we will see that bond index trackers follow a similar strategy and seek to minimize tracking error. Then we discuss how stock prices react to index revisions and review five competing explanations as to why the price reacts. For each explanation we discuss how well it relates to our bond index setup. Our empirical findings in the following sections are, in particular, consistent with the predictions from the price pressure hypothesis. Finally, we discuss two related papers that document price pressure in the corporate bond market in a setting closely connected to index revisions, but without being driven by bond index trackers.

### 3.2.1 Stock index trading volume and timing

Trading volume associated with revisions of the S&P 500 has increased over time in line with the increased interest in index tracking by mutual funds. Beneish and Whaley (1996) report that Vanguard Index Trust-500, which tracks the S&P 500, has had an increase in their portfolio value from \$14 millions in 1976 to \$9,356 millions in 1994. Prior to 1976 index tracking received very little attention and both Shleifer (1986) and Chen, Noronha, and Singal (2004) report that there was no change in trading volume around revision dates before that time.

After the new S&P announcement service from September 1976 trading volume did increase, which is consistent with more mutual funds tracking the index. Since revision announcements at that point were given after the market close, the first day with abnormal trading volume would be the next trading day. Harris and Gurel (1986) report an abnormal trading volume of 1.89 times the normal level for this day on average over the period 1973-1983 for stocks added to the index. They also show that the volume for these stocks over the following week remained 29% above normal. The trading behavior is consistent with index trackers trading aggressively on the day closest to the revision date in order to minimize tracking error. For various sub-periods in the interval from September 1976 to September 1989 Shleifer (1986), Beneish and Whaley (1996) and Chen, Noronha, and Singal (2004) find similar patterns for stocks being added to the index.

Following the separation of the announcement date and the effective date of revisions in October 1989 the trading volume spikes twice around a change - once on the day after the announcement and again on the day prior to the effective day. Chen, Noronha, and Singal (2004) report a trading volume on the day following the announcement that is 3.7 times higher than normal and 12.3 times higher than normal on the day prior to the effective day for stocks added to the index from October 1989 to 2000. The same pattern is reported for various periods from 1989 onwards by Beneish and Whaley (1996), Lynch and Mendenhall (1997), Blume and Edelen (2002), Hegde and McDermott (2003), Elliot and Warr (2003) and Cai (2007).

Lynch and Mendenhall (1997), Blume and Edelen (2002) and Chen, Noronha, and Singal (2004) report similar patterns for stocks being excluded from the index.

Most funds committed to tracking the S&P 500 use a portfolio replication strategy and hold all 500 stocks from the index. Beneish and Whaley (1996) suggest that arbitrageurs buy the stocks that get included in the index on the day after the announcement and then sell them on to the index funds on the day prior to the effectuation which explains the abnormal volume on these two days. Since index trackers can anticipate this behavior Beneish and Whaley (1996) guess that index trackers would move their buying closer to the announcement date and eventually drive arbitrageurs out of the "S&P 500-game". However, Blume and Edelen (2002) show that this has not been the case. Barclays Global Investors, which is a major player among the index funds, have only had an absolute average tracking error per year over the last decade of 2.7 bps. Tracking errors this low can only be accommodated by trading very close to the actual revision time. Even though index trackers could earn a higher abnormal return on a strategy where they traded right after the announcement date (as implicitly suggested by Beneish and Whaley (1996)) they are more focused on minimizing their tracking error. Blume and Edelen (2002) argue that low tracking error is a way for investors to monitor the actions of index funds. This behavior is not unique for funds tracking the S&P 500. Greenwood (2005) finds a similar increase in trading volume following a revision of the Nikkei225 and Mase (2007) and Mazouz and Saadouni (2007) for the FTSE100. In general all the studies find that trading volume increases around index inclusions and exclusions as a result of the change in demand from index funds. Furthermore, for added stocks the post-inclusion trading volume drops from the peak but stays higher than the pre-inclusion trading level (Harris and Gurel (1986) and Beneish and Whaley (1996)).

Part of the stocks included in the S&P 500 are traded on NASDAQ whereas the rest trades on NYSE. One might expect bonds to react more like the NASDAQ stocks since they are both traded in dealer markets. There does seem to be some differences in trading volume around revisions for NASDAQ stocks compared to NYSE traded stocks, but Elliot and Warr (2003) find that NYSE stocks experience a larger abnormal increase in trading volume (13.8 times the normal level) compared to NASDAQ stocks (9.67 times the normal level) for the period 1989-2000, whereas Blume and Edelen (2002) find the opposite relationship for the period 1995-2000. This could indicate that the behavior has shifted over time and that NASDAQ stocks are now traded more aggressively than normal compared to NYSE stocks when included into the S&P 500.

### 3.2.2 Stock index rebalancing returns and explanations.

Whereas all studies agree on the changes in trading volume around revisions, not all agree on how the abnormal returns react and specifically what happens in the longer run. Shleifer (1986) and Chen, Noronha, and Singal (2004) find no significant price changes around index inclusion or exclusion before the introduction of the announcement service in September 1976. Over the period from September 1976 until September 1989 Chen, Noronha, and Singal (2004) report an average abnormal return of 3.7% on the first trading day after the announcement for stocks included into the S&P 500, which is at the level of Shleifer (1986), Harris and Gurel (1986), Jain (1987), Dhillon and Johnson (1991) and Beneish and Whaley (1996) for various sub-periods.

Harris and Gurel (1986) find a complete reversal of the abnormal return over the next 10-14 trading days, whereas the remaining studies only find a partial reversal of the abnormal return. For the newer period from October 1989 until 2000, where the announcement date and the change date for the S&P500 are separated by 5 days on average, Chen, Noronha, and Singal (2004) report a 5.4% abnormal return on the announcement day for stocks being included and a further 3.5% increase from the announcement day to the effectuation day. After the effectuation day, the cumulative abnormal return is again partially reversed over time. This return pattern is also found by Beneish and Whaley (1996), Lynch and Mendenhall (1997), Blume and Edelen (2002), Denis, McConnell, and Ovtchinnikov (2003), Hegde and McDermott (2003), Elliott and Warr (2003), Cai (2007), Petajisto (2009), Elliott, Ness, Walker, and Warr (2006) for S&P 500 inclusions for various periods after 1989. The same pattern is also documented by Madhavan (2003) and Cariño and Pritamani (2007) for inclusions to the Russell-indices, for revisions of the Nikkei225 (Greenwood 2005), for revisions of the Toronto300 index (Kaul, Mehrotra, and Morck 2000) and for inclusions to the FTSE100 (Mase 2007). Even though index inclusions have received the most attention in the literature, index exclusions show roughly the same pattern with an abnormal (negative) return on the announcement date and a further decrease up to the change date followed by a reversal (Lynch and Mendenhall (1997), Blume and Edelen (2002) and Chen, Noronha, and Singal (2004)).

Elliott, Ness, Walker, and Warr (2006) list five competing explanations for the abnormal return patterns around stock index revisions - price pressure, downward-sloping demand curves, improved liquidity, improved operating performance, and increased investor awareness. The explanations are not necessarily exclusive.

According to the price pressure hypothesis the abnormal return at inclusion stems from short-run liquidity constraints temporarily driving prices above the fundamental value in order to compensate liquidity providers. Empirically only Harris and Gurel (1986) find full support of the price pressure hypothesis since they find a complete reversal of the abnormal returns. As



listed above several other studies find that part of the abnormal return is reversed in the days subsequent to the revision, but that a part of the price increase is permanent, at least for the newer periods. This suggests that price pressure at most explains part of the return pattern. Scholes (1972), Kraus and Stoll (1972) and Keim and Madhavan (1998) all study large block sales of stocks and do also find evidence of short-run price pressure in the stock market. However, contrary to the stock market, large trades in the corporate bond market are not expected to move prices apriori. Edwards, Harris, and Piwowar (2007) and Feldhütter (2009) present evidence that transaction costs are a decreasing function of trading volume for corporate bonds and that larger trades are executed at lower bid-ask spreads. This happens because larger trades are carried out by sophisticated investors with high bargaining power in an over-the-counter market as modeled in Duffie, Gârleanu, and Pedersen (2007). Even though corporate bond index trackers are sophisticated investors they may still be subject to price pressure since they are constrained by their investment strategy to minimize tracking error, which could reduce their bargaining power. This leaves the question of index driven price pressure for corporate bonds as an empirical question.

In classical asset pricing theory the demand curves for stocks are horizontal at the risk adjusted fundamental value, which implies that a demand or a supply shift would not affect the price of the stock. To the extent that index inclusions are information free events, they provide an excellent test of the slope of the demand curve for stocks. When stocks are included into an index, demand for the stock will increase as a consequence of index tracking. Shleifer (1986) is the first to suggest this test of downward sloping demand curves and as listed earlier a range of studies do find evidence that part of the price increase following index inclusions is permanent. Especially Greenwood (2005)'s study of the Nikkei225 and Kaul, Mehrotra, and Morck (2000) of the Toronto300 present strong evidence in support of downward sloping demand curves for stocks. In both studies index weights were changed due to an index redefinition which resulted in price changes for stocks still in the index, but now with a different weight and hence a different demand from index trackers. Also Wurgler and Zhuravskaya (2002) find support of downward sloping demand curves for stocks studying S&P 500 inclusions. They find that index arbitrageurs are unable to find close substitutes for the stocks included which leaves the arbitrageurs with unhedged risk that make them unwilling to trade at the pre-inclusion price of the stock.

The improved liquidity hypothesis states that when stocks are included into an index it improves liquidity and reduces asymmetric information permanently. This results in lower transaction costs and in the end a permanently higher price as in the model of Amihud and Mendelson (1986). The liquidity may be improved upon inclusion because the number of institutional investors increases which increases the level of monitoring of the firm thereby lowering the level of asymmetric information. Beneish and Whaley

(1996) find a temporary reduction in bid-ask spreads, Hegde and McDermott (2003) find the same but with a permanent reduction and Madhavan (2003) ascribes the increased trading volume following inclusion into the Russell index family as improved liquidity. It seems unlikely that a bond index inclusion should improve liquidity. Inclusion into major bond indices are based on mechanical rules like that of the Russell indices but the indices are not limited to contain a certain number of bonds. Most of the bonds included are issued by large firms which already have a number of bonds outstanding, so there is no reason to think that the issuance of yet another bond from the same firm would lower the asymmetric information level. If more bonds from the same firm get included into the index it might even lower the liquidity of the bonds, since the issuance of many bonds might indicate that the firm is in financial trouble, which tend to lower the bond liquidity (see e.g. Dick-Nielsen, Feldhütter, and Lando (2009)).

Investors may not be aware of all stocks in the market which would make them overinvested in the stocks of which they are aware (Merton 1987). In order to hold less known stocks investors require a return premium (a shadow cost). Inclusion into a stock index is likely to increase investor awareness of the stock and it thus reduces the shadow costs. For stocks included into the index the empirical implications of this hypothesis and of the downward sloping demand curve hypothesis are identical. However, index exclusions do allow us to empirically separate the investor awareness hypothesis and the downward sloping demand curve hypothesis. Chen, Noronha, and Singal (2004) argue that investor awareness should not decrease after an index exclusion which should result in a smaller price change for stocks removed from the index. Empirically, Chen, Noronha, and Singal (2004) do find an asymmetric price response in favor of the investor awareness hypothesis. For the large majority of corporate bond indices, inclusion is closely related to the issuance of the bond, since the bonds are included close after issuance. This means that index inclusion is not a sudden change in the status of the bond and at the same time index membership is not limited to a selected number as it is for most stocks indices. All together, it is unlikely that investor awareness should change upon a bond index inclusion.

All of the above mentioned hypotheses assume that index inclusion is an information free event in itself. This is a reasonable assumption since index revision rules for the Nikkei225, the Toronto300 and the Russell index family are all based on mechanical rules and Standard and Poor's states that inclusion into the S&P 500 is only based upon publicly available information. However, Standard and Poor's do make extensive analysis of different candidate firms in order to insure a low turnover in index membership. Being a rating company which specializes in financial analysis of firms, Standard and Poor's may unwillingly or unknowingly use the public information in a way superior to other investors and in the end convey information about the firm when they select it for inclusion. Denis, McConnell, and Ovtchinnikov (2003)

present evidence that analyst earnings forecasts rise for firms, whose stocks are included into the S&P 500 and that these firms also realize the higher earnings. The improved operating performance hypothesis states that this may not just be a result of new information about the firm. The improved operating performance is a result of closer scrutiny of the management, which in the end improves the performance (as also suggested by Shleifer 1986). Both Denis, McConnell, and Ovtchinnikov (2003) and Dhillon and Johnson (1991) find empirically that upon inclusion into the S&P 500 the firms' stocks, as well as its bonds, increase in value. The increased bond values indicate that the index inclusion is linked to the fundamental value of the firm and not only isolated frictions relating to stocks as an asset class. As with the Nikkei225, the Toronto300 and the Russell index family, inclusion to or exclusion from corporate bond indices are non-informational events and like the investor awareness hypothesis it seems unlikely that inclusion into the bond index should lead to improved operating performance, since inclusion does not make the firm move visible.

### 3.2.3 Demand Curves for Corporate Bonds

This paper defines index trackers as investors that seek to replicate the return of an index while they seek to minimize their tracking error. The definition captures the fact that index trackers do not seek or wish to outperform the index as documented in Blume and Edelen (2002) for S&P 500 index trackers. Blume and Edelen (2002) show that index trackers actually sacrifice trading gain to reduce the tracking error. No papers have so far looked at the impact of corporate bond index tracking on the individual bonds being included to or excluded from the index. However, two papers, Newman and Rierison (2004) and Chen, Lookman, Schürhoff, and Seppi (2009), present evidence of price pressure or short-term downward sloping demand curves for corporate bonds around events closely related to corporate bond index tracking.

Between October 1999 and July 2001 the European telecom-sector issued bonds which increased the overall amount outstanding by 300% for the sector. The new issues raised funds to the sector that could support bids for government auctions on cellular bandwidth licenses. Newman and Rierison (2004) study the price reaction on the already issued bonds in the sector around the new issuances. They find price pressure on the already issued bonds in the sector that temporarily decreases the price. One explanation for the price pressure could be that the new issues would enter into a corporate bond index. This new entry would then decrease the weight of the other bonds in the index forcing index trackers with limited funds to sell out of the bonds already in the index in order to buy the new issues and hence still track the index. This explanation is in line with what happened in Kaul, Mehrotra, and Morck (2000) and Greenwood (2005). However, whereas Kaul, Mehrotra, and Morck (2000) and Greenwood (2005) docu-

ment the effect of the reweighting in the index to happen on the same day that the reweighting was effectuated, Newman and Riersen (2004) show that the price pressure surrounding the new issuances in the telecom sector was centered around the issuance date of the bonds. As we will describe later on, newly issued bonds will not enter the major indices until the last day of the month in which they are issued. Nothing in Newman and Riersen (2004) suggests that the actual index inclusion day itself should be special. This indicates that the price pressure they find is not driven by index trackers, since we have no reason to think that index trackers would be active around the issuance date (see Blume and Edelen 2002).

Chen, Lookman, Schürhoff, and Seppi (2009) look at the effect of a rating rule change in the Lehman/Barclay index family. In January 2005, Lehman announced that it would change its practise on index rating definitions (only relevant for split-rated bonds). Before the change, a corporate bond was considered investment grade if it had an investment grade rating by both S&P and Moody's. After the change, a corporate bond was considered investment grade if it had an investment grade rating by at least two out the three major rating agencies S&P, Moody's or Fitch. Lehman used the index rating to determine bond membership of its indices, e.g. membership of the major US Aggregate Bond Index and of the Corporate Bond index required an investment grade index rating. The index rating rule change had an effect on bonds that were mechanically upgraded into investment grade because of the rule change and on bonds that now had a more save investment grade rating. Chen, Lookman, Schürhoff, and Seppi (2009) show that the mechanically upgraded bonds experienced a cumulative abnormal return of 200 bps in the two weeks following the announcement. The price pressure was transitory and disappeared again after 20 to 30 trading days. While the index rating rule change without doubt affected the portfolio construction of index trackers it is unlikely that the 200 bps abnormal return was driven by index trackers. The actual rule change was not effectuated until the end of June 2005, long after the announcement return effect in January 2005, had disappeared and Chen, Lookman, Schürhoff, and Seppi (2009) find no abnormal return around the actual index change. As stated in Chen, Lookman, Schürhoff, and Seppi (2009) the abnormal return following the announcement is more likely to have been driven by institutional investors. Institutional investors are in many cases constrained in their portfolio choice to investment grade assets and in the case of split ratings industry practise have been to follow the Lehman index rating rule.

### 3.3 The Lehman/Barclay Corporate Bond Index

The largest bond index funds (i.e. Vanguard, Schwab and Fidelity Total Bond Market Index Funds) are tracking the performance of the Barclays

Capital U.S Aggregated Bond Index (formerly Lehman U.S Aggregated Bond Index). The index is a broad mixture of Government, Agency, Corporate and Mortgage Backed bonds all with an investment grade rating. We focus on the corporate bond part of the index which by itself is known as the Barclays Capital Corporate Bond Index. As of July 1, 2005, the index consists of all corporate bond issues which have an investment grade rating by at least two of the three major rating agencies. The issue size must be \$250 millions or above and time to maturity must be above 1 year. Once a bond is issued and if it complies with all the rules it is included in the index at the next rebalancing date. If the bond at some point no longer fulfills all the criteria, it is excluded from the index at the next rebalancing date.

Reason	N	Average amt. (\$1,000)	Average Duration	Average Coupon
Panel A: Inclusions				
New issue	4007	663,834	7.4	5.6
Upgrade	554	587,277	5.8	7.0
Other	200	676,832	6.7	5.6
Panel B: Exclusions				
Maturity < 1	1817	538,114	0.92	6.0
Called	244	332,439	0.82	7.5
Downgrade	917	587,548	5.0	6.9
Other	1685	247,617	5.8	6.7

**Table 3.1: Corporate Bond Index Inclusions and Exclusions Statistics.** This table shows statistics for inclusions and exclusions from the Lehman/Barclay Corporate Bond Index. The same bonds enters and exits several Lehman/Barclay indices at the same time. The statistics are accumulated over the period from July 2002 to August 2009. Average amt. is the average amount outstanding in \$1,000 at the time of the index revision.

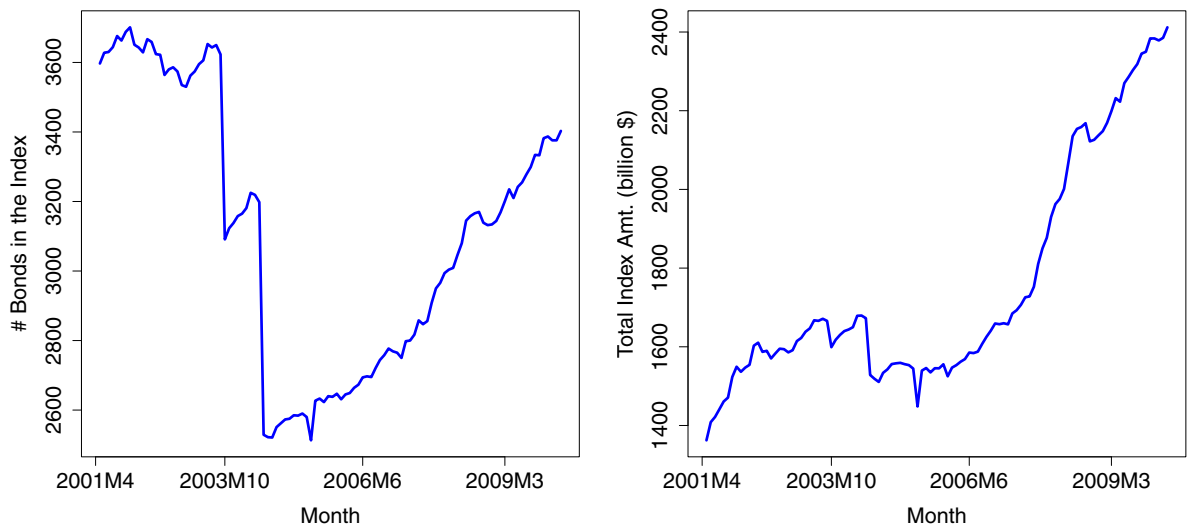
The index is rebalanced on the last trading day of each month at 3:00 PM EST. Inclusions to the index mainly happen because a bond is newly issued or because it is upgraded. Exclusions from the index happen mainly when a bond is downgraded to speculative grade or when the time to maturity drops below 1 year. From July 2002 to August 2009, 4,007 bonds were included as newly issued. In table 3.1 we can see that they had an average duration of 7.4 years, which is higher than the total index duration of around 5 years. 554 bonds were upgraded into the index. These bonds have a lower duration and a higher coupon since most of them were originally issued as speculative grade before they were later upgraded. Effective July 2005 Lehman changed the index rules so that a split rated bond was no longer seen as a speculative grade, but had the middle rating from Standard and Poor's, Moody's and Fitch (the present rating rule). The specific event involving the rule change

is researched in Chen, Lookman, Schürhoff, and Seppi (2009). The other bonds that were included into the index were included for various reasons e.g. they changed status from non-public to publicly traded. 1,817 bonds were excluded from the index because the maturity dropped below one year. A smaller fraction of the bonds were called and 917 bonds left the index because of a downgrade. The average coupon for the downgraded bonds was higher than for the maturing bonds, reflecting that the downgraded bonds were issued with a lower rating in the first place. A large fraction of the bonds were excluded for other reasons. The average amount outstanding for these bonds is below \$ 250 million, which is the lower limit for being in the index today. Most of these bonds were excluded exactly because the amount outstanding dropped below the limit, either because the amount outstanding changed or because the index limit was raised. Since the inception of the index the minimum amount outstanding limit has been raised several times.

Typically, each mutual fund family (Vanguard, Schwab, Fidelity etc.) has more funds tracking different parts of the total aggregated index. Barclays keeps short-term, intermediate and long-term indices, which have similar rules for membership as the aggregate index. The only difference is the maturity of the bonds in the indices.

None of the bond index funds track the indices by replication, which is the most common way for stock index funds to track an index (Blume and Edelen 2002). Instead they are *sampling* the index (see e.g. Vanguard (2009) or Schwab (2009)). Typically the fund chooses a selection of the bonds currently in the index and designs a portfolio of these that match the index with respect to duration, cash flow, quality and callability. As of December 31, 2008 Vanguard Total Bond Market Index Fund held 3,731 different bonds out of the 9,168 bonds present in the index at that time. The corporate bond index has around 3,400 bonds in the index right now (see figure 3.1). Since index inception the rules for index membership have been tightened so that smaller issues have been excluded. It happened once in October, 2003 and again in July, 2004. The motivation has been to keep the index from getting too large, but as can be seen in figure 3.1 the index has still increased immensely both in the number of bonds and the amount outstanding. In order to reduce transaction costs from rebalancing, the index funds also invest outside the index. Typically 80% of the assets in the funds are invested in bonds currently in the index. The remaining 20% are invested outside the index for example in non-public bonds, lower rated bonds, non-corporate bonds or derivatives such as futures, options and swaps. The criteria for investing outside the index are rather loose and in that way it is not possible to know exactly which assets the funds have on their balance sheets. Still, even though the bond funds track by sampling they obtain a rather low tracking error. The average yearly absolute return tracking error for the shares of Vanguard Total Bond Market Fund over 1995-2009 is 23.5 bps. The tracking error record is dominated by 2002 which had

an exceptionally bad tracking error of 2% under the actual return of the Lehman/Barclay index. Without that year the mean absolute error is 11.0 bps. Compared to Fidelity's tracking error of 2.3 bps for tracking the S&P 500, the error for the Vanguard bond fund is much higher. Still, the tracking error record for the Vanguard fund suggests that the goal of the fund is to track the index very closely. In only one of the years between 1995-2009, have the Vanguard fund shares had a higher return than the index it tracks, which is probably due to the transaction costs from the sampling strategy.



**Figure 3.1: Corporate Bond Index Composition.** The left graph shows the evolution of the number of bonds in the index. During the period the index membership rules were changed twice. First in October, 2003 and again in July, 2004. The left graph shows the total amount of debt outstanding in the index.

We obtain information about the index rules and composition of the Lehman/Barclays Capital Corporate Bond Index from their website<sup>2</sup>. From the same place we obtain return series for different benchmark indices. Finally we use the TRACE database from WRDS for transaction level information about the individual bonds in the index. Before using the TRACE data for calculating returns we filter the data as in Dick-Nielsen (2009) in order to avoid biases from the way reporting errors are recorded and cumulating in the TRACE system.

<sup>2</sup>See index rules at [www.lehman.com/fi/indices/pdf/US.Corporate.Index.pdf](http://www.lehman.com/fi/indices/pdf/US.Corporate.Index.pdf) and index dynamics at <https://live.lehman.com/LL/lehmanlive>

### 3.4 Measuring Abnormal Corporate Bond Returns

Calculating daily corporate bond returns provide quite a challenge compared to stock market returns. First, an average corporate bond only trades once in a couple of months making it hard to get time series data. Second, trades are usually clustered and the same bond might trade several times on the same day at different prices (see e.g. Feldhütter (2009)) making it difficult to get a unique daily price. Bessembinder, Kahle, Maxwell, and Xu (2009) show in an extensive simulation analysis of corporate bond event studies that tests relying on transactions data have far more power than tests using daily data quotes from e.g. DataStream. This finding is in line with Sarig and Warga (1989) and Dick-Nielsen, Feldhütter, and Lando (2009) who show that quoted prices often have little connection to actual transaction prices.

For each bond entering or leaving the index we calculate a daily price on days with at least one trade above \$100,000 in nominal value (100 bonds) as the trading volume weighted average price of all trades on that day above \$100,000. This method is suggested by Bessembinder, Kahle, Maxwell, and Xu (2009) as the best way to calculate daily prices. Bonds might not have transactions on all days surrounding our index events. To circumvent this problem we follow the approach in Cai, Helwege, and Warga (2007) and calculate event returns as the logarithmic difference between the price on the closest day prior to the event and the closet day after or including the event date:

$$r_i^{b,a} = \log p_i^b - \log p_i^a$$

where  $i$  is a bond identifier,  $b$  is the event day or the day closest to the event day, but still after the event and  $a$  is the day closest to the event date, but still before the event. If we define day number -1 as the day before the event, day 0 as the event day and day 1 as the day after the event, then

$$a = \max(\text{day number with a return}) < 0$$

$$b = \min(\text{day number with a return}) \geq 0$$

We extend the method to cumulative returns by restricting the window in the following way

$$a = \max(\text{day number with a return}) < c$$

$$b = \min(\text{day number with a return}) \geq d$$

where  $c \leq 0$  and  $d \geq 0$  so that the window spans a period. We restrict the sample to returns calculated using daily prices not more than five trading days away from the event date or restriction points ( $c - a < 6$  and  $b - d < 6$ ). In comparison Ambrose, Cai, and Helwege (2009) use a return window as



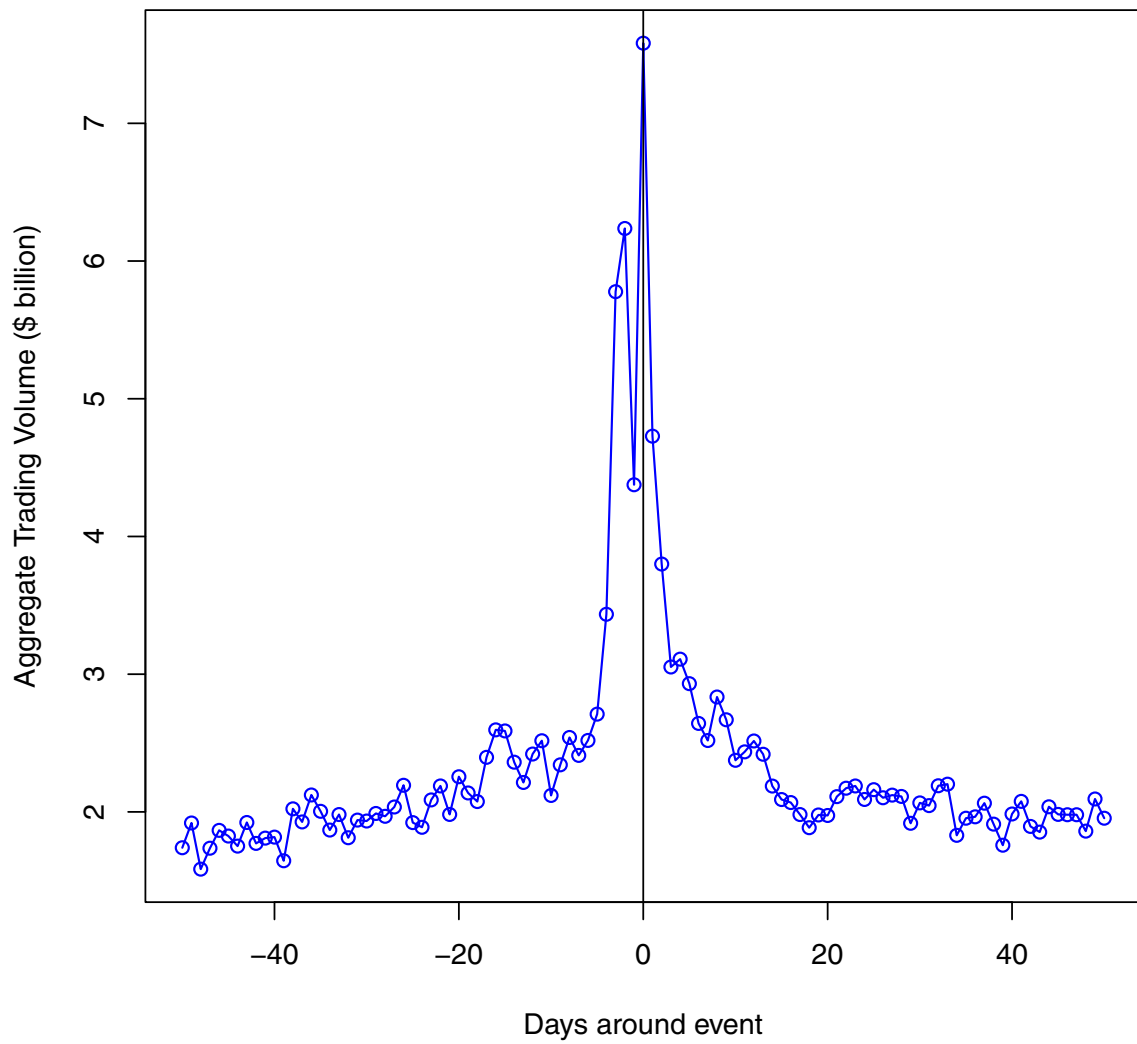
large as  $\min(a) = -100$  and  $\max(b) = 99$ . However, a window that long could have a significant influence on the variance of the return, which is why we choose a shorter window. There could still be a problem with the variance of the return, but Ambrose, Cai, and Helwege (2009) and Cai, Helwege, and Warga (2007) run various robustness checks and show that there are no problems with small windows like the one we use.

Chen, Lookman, Schürhoff, and Seppi (2009) propose a different method to circumvent the infrequent trading problem. They calculate cumulative returns from a given pre-event day to a range of later dates, for each date disregarding bonds that did not trade on that particular date. Then they use a value-weighted average of all returns on each of the later days to get a portfolio return for that day. In this way they get a portfolio time-series, but with a possibly different portfolio on each time series date. However, this method still requires trading on the post-event dates, which is the major problem in our event study.

Bessembinder, Kahle, Maxwell, and Xu (2009) and Chen, Lookman, Schürhoff, and Seppi (2009) argue that the best way to calculate abnormal returns is to use a benchmark portfolio of bonds similar to the individual bond in the event study (in line with Barber and Lyon (1997) for stock returns). We follow these studies and in each of the four following event types we explain which benchmark portfolio we use.

### 3.5 Maturity Less Than 1 year

In contrast to stocks, bonds have a maximum lifetime in an index, since they are excluded 1 year prior to maturity. Corporate bonds that fall below 1 year to maturity are excluded from the corporate bond index at the last trading day of the month. Exclusion because of low maturity is by far the most common reason for a bond to leave the index (as seen in table 3.1). Figure 3.2 shows the aggregate trading volume around the index rebalancing date. The event date is the last day of the month, negative days are before the event date and positive days are after the event date. For each bond excluded because of low maturity the total trading volume (without the sign of the trade direction) is added for each day around the event across bonds and calendar dates. Similar to stock index trackers' timing of their trades (Blume and Edelen (2002)), the trading activity is highest at the date of the exclusion. Because bond funds only sample an index, not all bonds that are excluded from the index exhibits abnormal trading activity. The average turnover on the event day is 1.2% of the volume outstanding for the 1/3 most traded bonds on the event day (as stated in section 3.3 bond index funds typically only hold 1/3 of the bonds in an index). It is clear from figure 3.2 that the event date is the exclusion date and not the day where the maturity actually falls below 1 year. Had the latter been the case, then the trading activity should have been higher 10 to 21 days before the exclusion day, which



**Figure 3.2: Total trading volume for bonds excluded because of low maturity.** Trading volume for each bond is added on a daily basis (without trading direction) and then the daily volume is added across bonds according to distance from the event. 1138 bonds trade on the event date. The average turnover for the 1/3 most traded bonds is 1.3%. The most traded bonds are selected on a monthly basis as the 1/3 bonds with highest turnover that month.

is not the case. Also, it is not just an end-of-month effect, in the sense that the same pattern repeats for the same bonds each month. If it was an end-of-month effect we should also expect to see a spike in trading activity at around plus and minus 21 days from the event date. Since nothing seems to happen at the time where maturity actually falls below 1 year and since it is not just an end-of-month effect, the figure supports that trading activity is driven by investors aiming at a low tracking error (in the sense of Blume and Edelen (2002)). Blume and Edelen (2002) show that the funds with lowest tracking error following the S&P 500 index need to trade exactly at the rebalancing dates. Comparing with figure 3.2 the bond index trackers also trade very close to the event date and as stated before Vanguard Total Bond Market Fund only had a yearly tracking error of 23 bps over the most recent 15 years. Still, the bond trading activity is high from approximately 4 days before to 2 days after the event date. This is most likely due to the illiquidity of the bond market as compared to the stock market and not because bond index trackers are careless about their tracking errors.

Panel A: All Bonds

From (Day)	To (Day)	N	Return (bps)	T-Stat
-10	0	795	-13.6	-6.42***
-10	5	700	-9.8	-3.57***
-10	10	707	-20.4	-6.44***
-10	15	700	-21.1	-5.70***
0	5	870	4.7	2.88***
0	15	808	-6.3	-2.44***

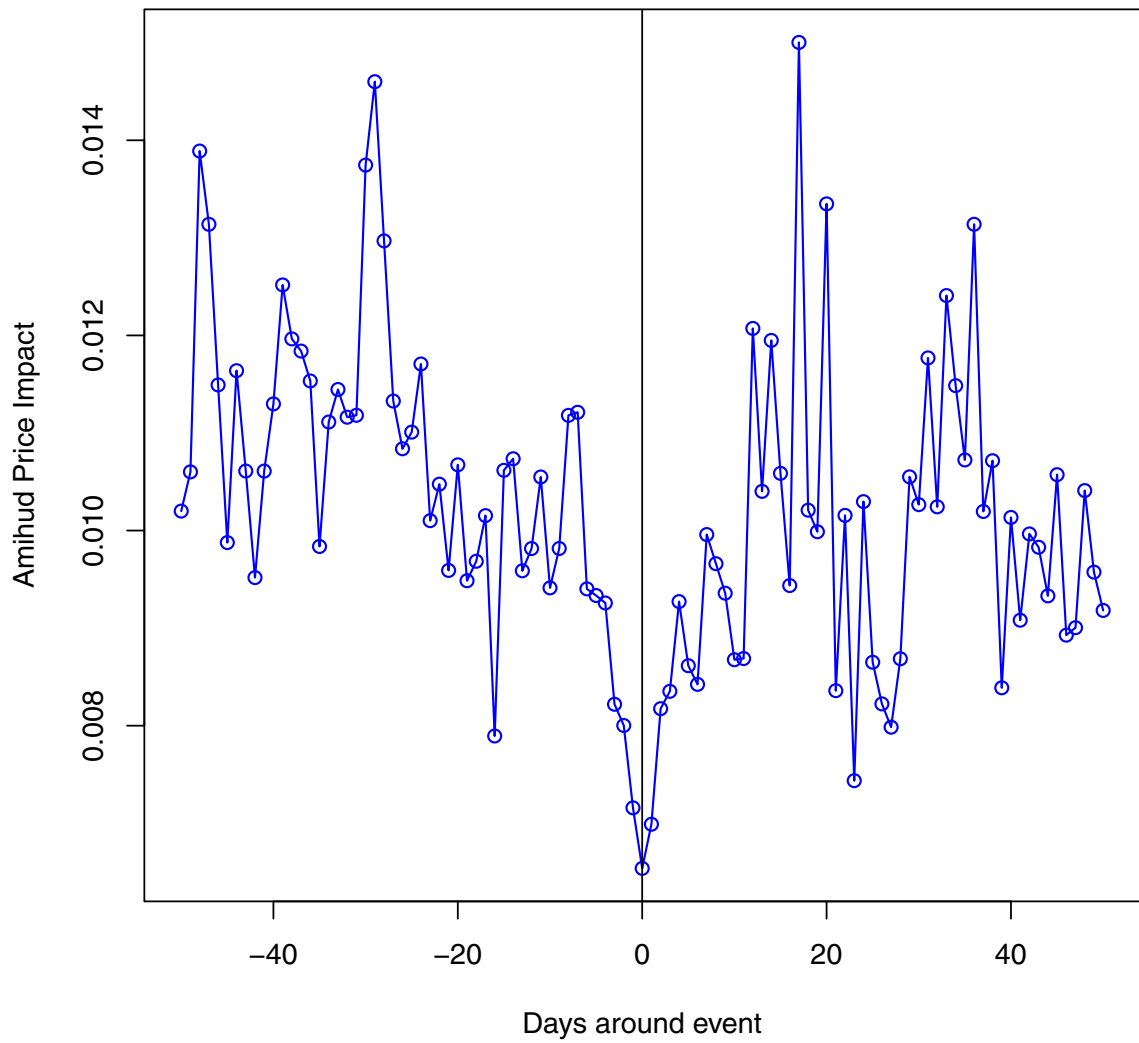
Panel B: From 2008M11 to 2009M08

From (Day)	To (Day)	N	Return (bps)	T-Stat
-10	-2	95	-58.0	-3.79***
-10	5	85	5.5	0.30
-10	10	79	-10.4	-0.39
-10	15	79	4.5	0.18
-2	5	96	60.7	4.34***
-2	15	90	58.7	3.39***

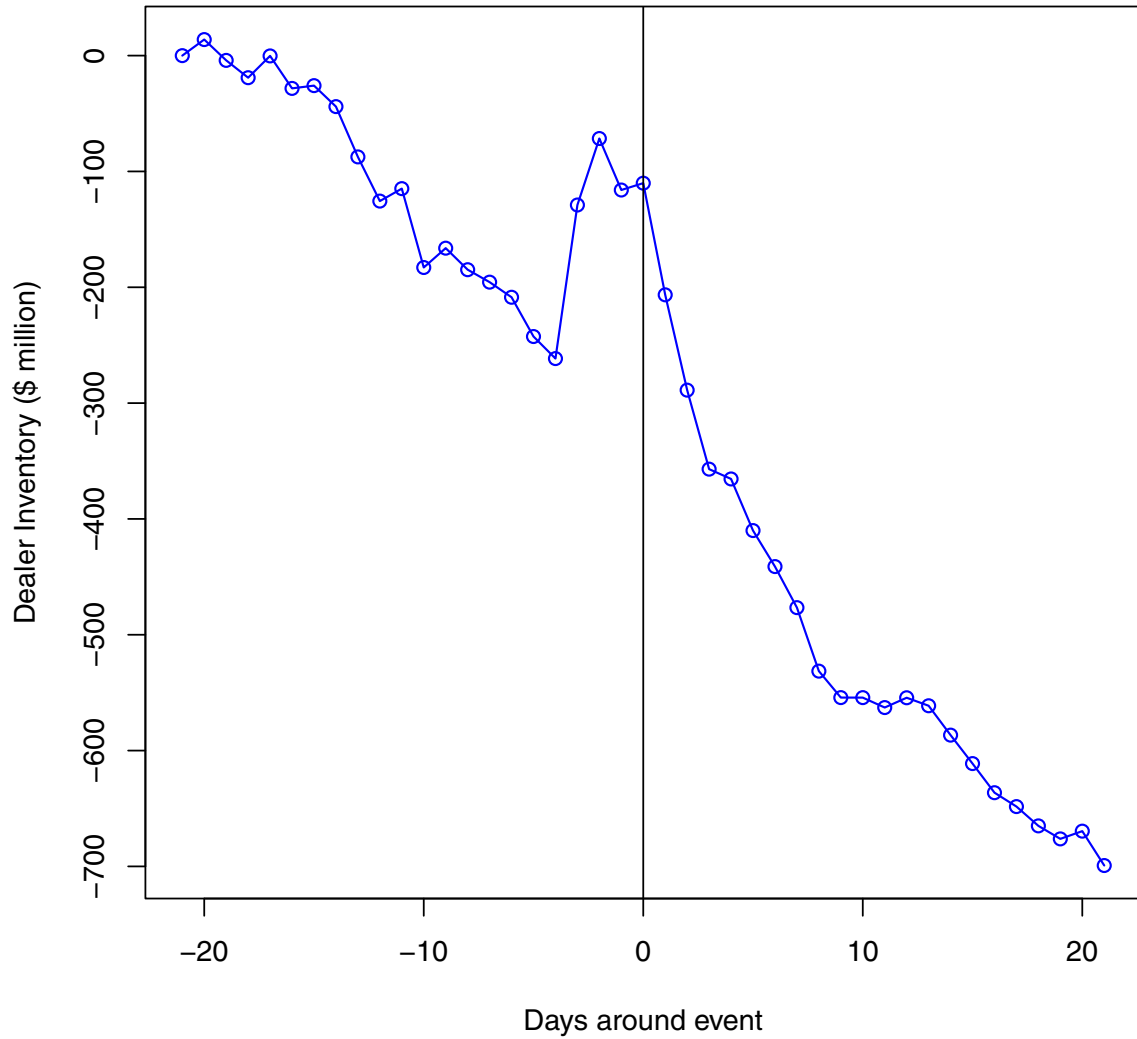
**Table 3.2: Abnormal returns at index exclusion of maturing bonds.** The abnormal return is a size weighted average over all bonds for which it has been possible to calculate a return. The benchmark return for each bond is the short Lehman/Barclays index with maturity between 9-12 months that match on rating. The test statistics are cluster robust to time series and firm fixed effects. Panel A shows the results across all bonds. Panel B shows the results when only the newest period is analyzed.

Bonds are usually issued in the first part of the month specially on the 1st, the 5th and the 15th calendar day of the month, so bonds have an average time to maturity of just above 11 month at the time they are excluded from the index. When calculating abnormal returns as described in section 3.4 we use the return on the short term bond index from Lehman/Barclay as benchmark. This benchmark index consists of all bonds that have been excluded from the investment grade indices and which have a maturity of 9-12 month. Different from the pure corporate bond index, the benchmark index also includes other investment grade bond issues e.g. government bonds.

In table 3.2 abnormal returns are calculated for different periods bracketing the event day. In panel A of table 3.2 the abnormal returns are calculated using all available bonds in the period from July 2002 to August 2009. From day -10 to 0 the bonds fall with 13.6 bps on average (size weighted average). The decrease in price is slightly reversed over the following 5 days, but eventually drops to a level of around -20 bps. The partial (but significant) reversal from day 0 to 5 after the event suggests that there is some price pressure around the exclusion, but that some other effect is giving a more longer term negative effect. There is no obvious reason as to why the price should drop permanently after the index exclusion. The event in itself is completely information free. One explanation could be that the bonds become more illiquid after they are excluded from the index. But that is not true. In figure 3.3 size weighted daily Amihud price impact measures are calculated for each day around the event across all the bonds. The price impact is lowest at the event day indicating that the bonds are more liquid at this time than usually. This could be due to either that there are more buyers and sellers active at this time, so that it is easier for counterparties to meet or it could be because the sellers, which are for the most part the index trackers, are more homogeneous and sophisticated than normal. In both cases, the intraday prices would be more stable than normal and the average price impact would therefore be lower than normal. When comparing the level of the Amihud price impact measures before and after the event there is little economical difference and no statistical significant difference (actually the average price impact level is lower after the event i.e. the bonds are more liquid). Another explanation for the permanent price drop of around -20 bps could be a clientele effect. When the index trackers sell their bond at exclusion there is no natural counterparty for which the bonds are attractive. If the buyers value the bonds less than the sellers the price should drop. Still the price drop should only be permanent if the pre-event clientele is completely different from the post-event clientele. For government bonds, this is to a large extent true, once the maturity falls below 1 year government bonds are said to trade in the money market. Traditional money market funds only buy a very small number of corporate bonds and they only buy the highest rated. However, there do exist funds that buy the



**Figure 3.3: Amihud price impact measure around bond exclusion because of low maturity.** For each bond we calculate an Amihud price impact measure on a daily basis and then we take a size weighted average across bonds to form a daily Amihud price impact measure.



**Figure 3.4: Dealer inventory around bond exclusion because of low maturity.**

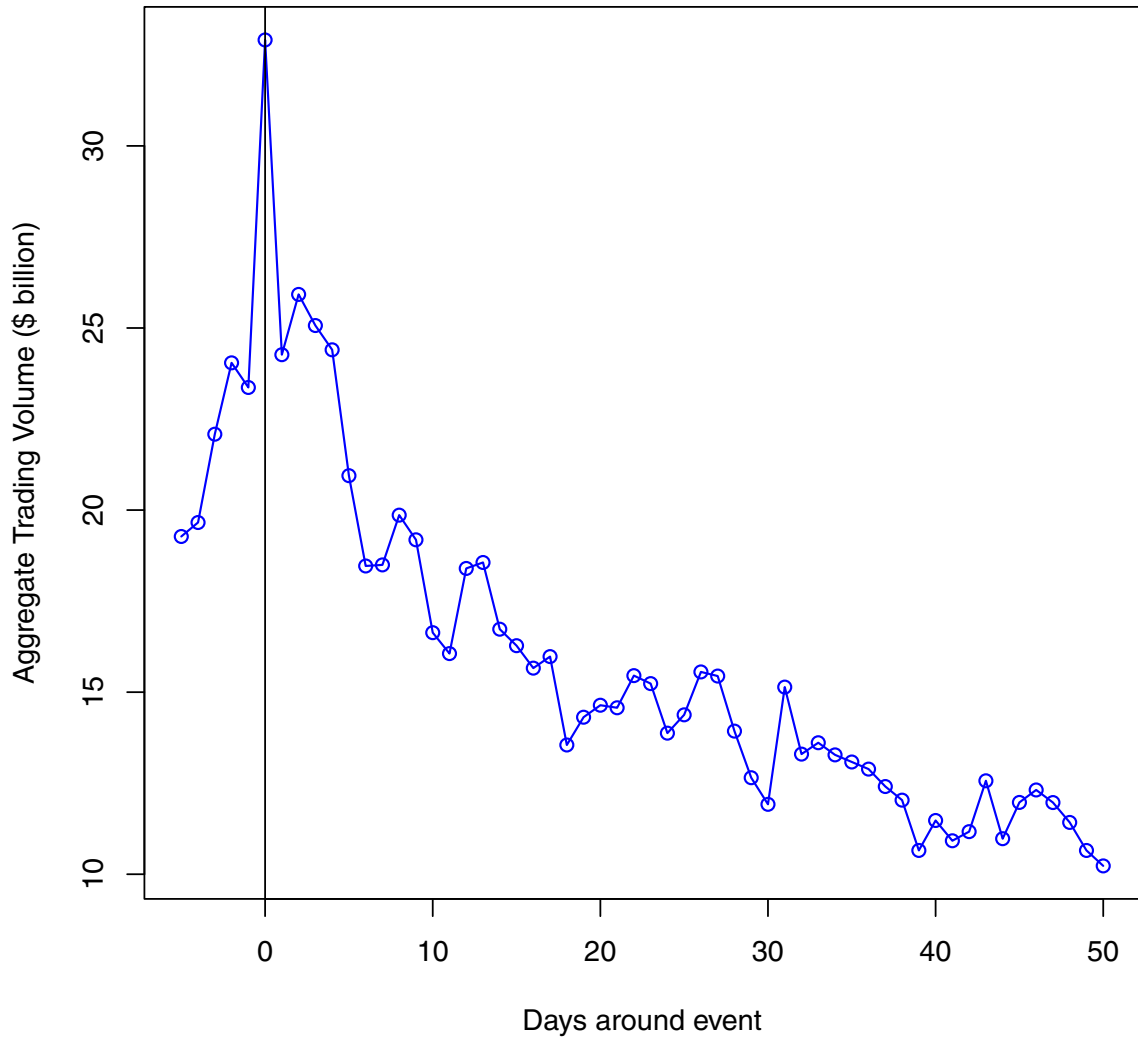
The graph shows the cumulative dealer inventory change. From November 2008 to August 2009 transactions in TRACE are marked as dealers sell, dealer buy or interdealer trade. Using this marking, we calculate dealer inventory change as the volume difference between dealer buys and sells each day for each bond. Then we add the bond specific inventory changes together according to distance from the event. Finally, we cumulate the aggregate changes using day -21 as benchmark.

excluded corporate bonds. These funds are called Ultra Short Bond Funds. The excluded corporate bonds are attractive for this clientele, but they can far from absorb all the bonds that are excluded. If they could we would probably not see any price reaction to an exclusion from the corporate bond index.

For the later period of the transaction data sample (from November 2008 to August 2009) the trade direction is available. Figure 3.4 shows the cumulative dealer inventory around the event. For each bond we calculate the difference between total dealer sells and buys. A negative number indicates that on that day the dealers have net sold from their inventory. We then add the daily net inventory positions across bonds for the different days around the event and finally we calculate the cumulative inventory position across bonds with day -21 as the benchmark zero position. Seen over the period from -21 to +21 days around the event the dealers net sell from their inventory. However, in approximately the same interval as the trading volume spikes in figure 3.2, the dealer inventory increases up to the event and then decreases with an equally amount after the event. This shows, that the dealers are acting as one of the counterparties for the index trackers in the sense that they buy up when the index trackers are selling, only to sell out shortly after. This dealer behavior is not optimal when compared to panel A of table 3.2. The price reversal in the period when the dealers sell out of the bonds again from day 0 to 5 is only 4.7 bps, which is far below the bid-ask spread as reported in Edwards, Harris, and Piwowar (2007) and Feldhütter (2009). However, when we restrict the abnormal return calculation to the same period as that for the dealer inventory graph (figure 3.4), the abnormal return and the reversal do become economically significant. In panel B we can see that when the index trackers sell their positions the price drops by -58 bps. Subsequently the price decrease is fully reversed and for this period there is not any significant longer term price effect. In this period it makes sense for the dealers to participate as counterparties for the index trackers since the average return is about 3 times the bid-ask spread.

### 3.6 Newly Issued Bonds

Most stock indices contain a fixed number of stocks and a new stock is only added when another stock is deleted. The rules for inclusion can either be subjective as for the S&P 500 or objective as for the Russell index family. Bond indices are in general not limited to only containing a fixed number of bonds. Once a new bond has been issued it is included into the corporate bond index at the last trading day of the month if it fulfills the size and rating rules etc. as described in section 3.3. Bonds are issued on all days of the month but the majority of the bonds is issued on the 1st, 5th or 15th calendar day of the month. Both Cai, Helwege, and Warga (2007) and Goldstein and Hotchkiss (2008) look at corporate bonds going from the primary market to



**Figure 3.5: Total trading volume for newly issued bonds included into the index.** Trading volume for each bond is added on a daily basis (without trading direction) and then the daily volume is added across bonds according to distance from the event. 2014 bonds trade on the event date. The average turnover for the 1/3 most traded bonds is 2.8%. The most traded bonds are selected on a monthly basis as the 1/3 bonds with highest turnover that month. We have excluded a few bonds which were issued during the last 10 calendar days of the month in order not to mix up any index effect with the abnormal trading volume on the first days of trading in the secondary market.



the secondary market. They show that the bonds are heavily traded in the first week of trading on the secondary market. In order not to mix up this newly issuance effect from the possible index effect on the last day of the month, all bonds issued (app. 5%) on the last 10 trading days of the month are excluded from the following analysis.

Figure 3.5 shows the trading activity around the index inclusion date (event date) for newly issued bonds. Since no bonds are issued on the last 10 trading days of the month the number of bonds that could possibly trade is the same on all the plotted days. Cai, Helwege, and Warga (2007) and Goldstein and Hotchkiss (2008) make similar graphs with the first day of trading on the secondary market as the event date. On average the trading activity for newly issued bonds is decreasing towards a constant level over the first 60 days of trading. The same decreasing pattern can be recognized from figure 3.5 from +1 to +50 days after the inclusion event. The trading activity increases up to the inclusion day where it spikes. The average turnover on the inclusion day is 2.8% for the 1/3 most actively traded bonds. The spike indicates that some investors are actually tracking the index and are trying to do so with a low tracking error as in Blume and Edelen (2002).

Abnormal returns around the event are shown in table 3.3. The benchmark return is chosen for each bond as the part of the Lehman/Barclay corporate bond index with matching rating and maturity as either intermediate term ( $< 10$  years) or longer term bonds ( $> 10$  years). Panel A of table 3.3 shows that the event return for inclusion is 9.6 bps and that the return stays up around 5 days after the event before it is fully reversed to the pre-event level after 10 days of trading. In panel B the same abnormal returns are calculated, but only for the 1/3 most traded bonds. Each month the 1/3 bonds with highest turnover are selected to be included in this calculation. The reason for picking only the 1/3 most traded bonds is that on average a bond index fund only sample the index, which means that they only hold about 1/3 of all the bonds in the index. The majority of the remaining bonds might not be part of any index tracking strategy, in which case we would not expect them to have an abnormal return on the event date. Leaving them in could bias the abnormal returns towards zero. The abnormal returns are slightly higher for the most traded bonds. From day -1 to +5 the abnormal return is 15.4 bps compared to 9.8 bps for the entire bond index universe. As before the abnormal return is fully reversed, but for the most traded bonds it takes till day +15 after the event. The size of the price pressure and reversal returns can be compared to the underpricing returns found in Cai, Helwege, and Warga (2007) and Goldstein and Hotchkiss (2008). Cai, Helwege, and Warga (2007) find an insignificant first day abnormal return of 2 bps for investment grade bond IPOs in a study with data from 1995 to 1999. For a newer data sample matching the one in this paper, Goldstein and Hotchkiss (2008) find a significant first day return of around 40 bps. However, they do not construct an abnormal return correcting for market

movements.

Panel A: All Bonds

From (Day)	To (Day)	N	Return (bps)	T-Stat
-1	1	2056	9.6	6.50***
-1	5	2050	9.8	5.05***
-1	10	2042	0.3	0.10
5	10	2140	-11.0	-5.58***

Panel B: Most traded

From (Day)	To (Day)	N	Return (bps)	T-Stat
-1	1	808	10.6	4.73***
-1	5	805	15.4	5.00***
-1	10	805	4.7	1.15
-1	15	803	-5.6	-1.18
5	10	827	-9.7	-2.91***
5	15	824	-18.7	-4.24***

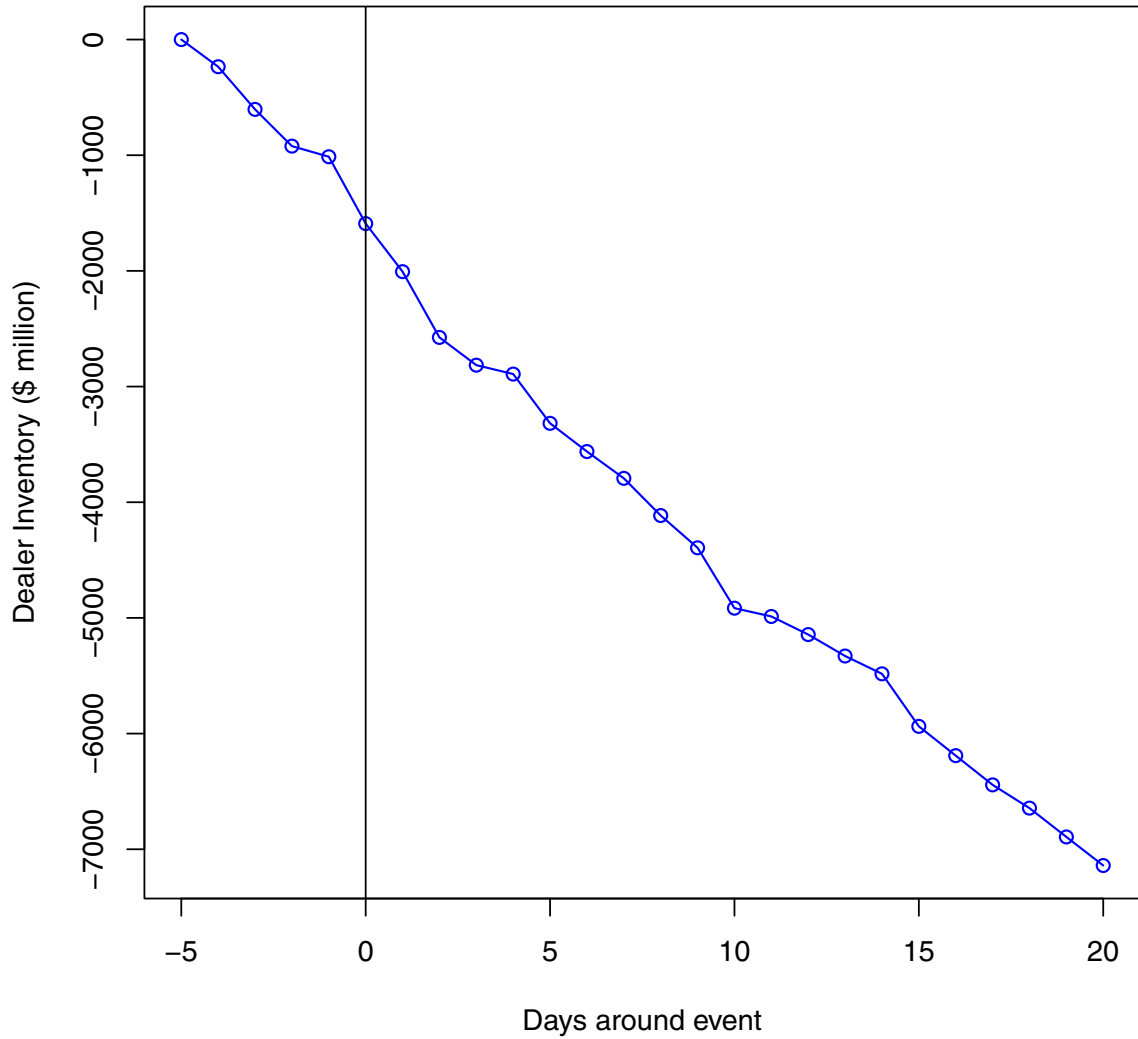
**Table 3.3: Abnormal returns at index inclusion of newly issued bonds.** The abnormal return is a size weighted average over all bonds for which it has been possible to calculate a return. The table excludes a few bonds that were issued on the last 10 trading days of the month in order not to mix up any underpricing with the index effect. The benchmark return for each bond is the corresponding Lehman/Barclays index that match on maturity and rating. The test statistics are cluster robust to time series and firm fixed effects. Panel A shows the results across all bonds. Panel B shows the results when only the most traded bonds are included in the analysis. The most traded bonds are selected on a monthly basis as the 1/3 bonds with highest turnover that month.

The presence of the reversal return in our study supports that the index trackers trading behavior leads to a price pressure, where the price temporarily increases, because of the temporarily increased demand. The inclusion event is completely free of any information content and it is not expected to increase investor awareness, since the index is not limited to a fixed number of bonds. Altogether the return and trading activity evidence support the price pressure hypothesis/short term downward sloping demand curves for bonds. It could also be that demand curves are downward sloping in the long run. The empirical evidence is not contradicting this hypothesis, but since the pre and post event demand in the long run cannot be separated easily from the newly issued bond effect where demand is decreasing (Cai, Helwege, and Warga (2007) and Goldstein and Hotchkiss (2008)) it is hard to tell if there is any demand shift in the long run caused by the index inclusion. However, since there is no reason to expect any increased investor awareness we would not expect any demand shift even if we could separate

the index effect from the newly issued effect in the long run.

For the later period of the sample the transaction data contains information on trade direction where trades are marked as either a dealer sell (to a customer), a dealer buy or an interdealer trade. From this information a cumulative dealer inventory can be calculated. For each bond all sells and buys are added (with sign) on each date, so that a negative number indicates that on that day dealers have been net selling from their inventory. The inventory changes are then added across bonds and cumulated over time with day -5 before the event as the benchmark. Figure 3.6 shows the cumulative dealer inventory for the bonds included to the index over the period November 2008-August 2009. On the event date where the trading activity spiked in figure 3.5, figure 3.6 with the dealer inventory shows no abnormal change. Over the period the dealers seem to sell steadily out of their inventory. The interdealer trading volume does spike on the event date (not shown) which suggests that the dealers are not providing the liquidity for the index trackers but only perform a matching function, unlike what they do in the other three cases of index rebalancing. However, Edwards, Harris, and Piwowar (2007) and Feldhütter (2009) find that the bid-ask spread is around 20 bps for large trades across the period. So the dealers can make a higher profit from just providing the matching function, than they can from selling out of their inventory. Another reason could be that some dealers, usually some of the underwriters (see Dick-Nielsen, Feldhütter, and Lando (2009)), have market making responsibilities in the bond, which can make it unattractive to just unload their entire inventory at once.

It is reasonable to think that index trackers prefer some bonds over others. Since they only sample the index they have to choose bonds that fit into a robust portfolio matching the duration, convexity and rating of the index. If some bonds are more attractive than other we would expect higher temporary demand for these and hence more price pressure. In order to test this hypothesis, we regress the bond specific abnormal return (day -1 to 1) on different bond characteristics and common bond return predictors. Table 3.4 shows the estimates and robust standard errors (see Petersen (2009)) for 4 different specifications of the regression. As bond specific predictors we use bond rating, coupon, maturity and size. We also include systematic abnormal return predictors, which are the slope and level of the treasury curve, credit risk factors (level and monthly change of a BAA yield curve) and illiquidity factors (level and monthly change). The daily illiquidity factor is calculated as a size weighted average over a daily Amihud price impact measure for all corporate bonds on the market. The daily illiquidity factor is then transformed to a monthly factor by taking the median over the month. The levels of the credit risk factor and the illiquidity factor are highly correlated (84%), so only one of the two factor types are included into the regression. In both regression specifications for all bonds, maturity and issue size are significant, which indicates a preference for large bond



**Figure 3.6: Dealer inventory around inclusion of newly issued bonds.** The graph shows the cumulative dealer inventory change. From November 2008 to August 2009 transactions in TRACE are marked as dealers sell, dealer buy or interdealer trade. Using this marking, we calculate dealer inventory change as the volume difference between dealer buys and sells each day for each bond. Then we add the bond specific inventory changes together according to distance from the event. Finally, we cumulate the aggregate changes using day -5 as benchmark.

	All Bonds		Most Traded Bonds		
Intercept	-53.5 (-1.32)	-57.7 (-1.63)	-35.5 (-0.54)	-63.1 (-1.11)	-288.3 (-1.55)
AAA	1.1 (0.12)	-4.6 (-0.5)	13.0 (0.97)	0.5 (0.04)	-35.8 (-0.66)
AA	4.2 (0.75)	-1.1 (-0.21)	10.3 (1.34)	-1.7 (-0.24)	0.7 (0.03)
A	6.1* (1.66)	0.6 (0.16)	7.4 (1.34)	-3.0 (-0.57)	9.4 (0.53)
BAA	- (-)	- (-)	- (-)	- (-)	- (-)
Coupon	2.9* (1.73)	-1.3 (-0.79)	2.1 (0.8)	-4.7** (-1.98)	-4.3 (-0.79)
TTM	0.4** (2.07)	0.6*** (3.08)	0.5* (1.84)	0.8*** (3.14)	0.9 (1.06)
Log(Size)	6.1** (2.56)	6.9*** (2.97)	1.1 (0.29)	5.3 (1.43)	5.0 (0.4)
Treasury Level	-10.6*** (-3.18)	-7.4* (-1.78)	-1.6 (-0.33)	2.4 (0.41)	44.4 (1.12)
Treasury Slope	-7.7** (-2.55)	-7.6** (-2.5)	-2.7 (-0.63)	-1.3 (-0.31)	39.9 (0.75)
BAA yield	0.015 (0.01)		1.9 (0.42)		
$\Delta$ BAA yield	14.2*** (2.71)		6.9 (0.91)		
Illiq		1765.7** (2.06)		2266.2* (1.86)	6178.7 (1.02)
$\Delta$ Illiq		0.022*** (11.59)		0.029*** (10.55)	0.036*** (4.54)
$\Delta$ Inventory					-0.9** (-2.37)
$R^2$	2.6	8.5	1.5	14.0	20.1
N	2055	2053	807	805	164

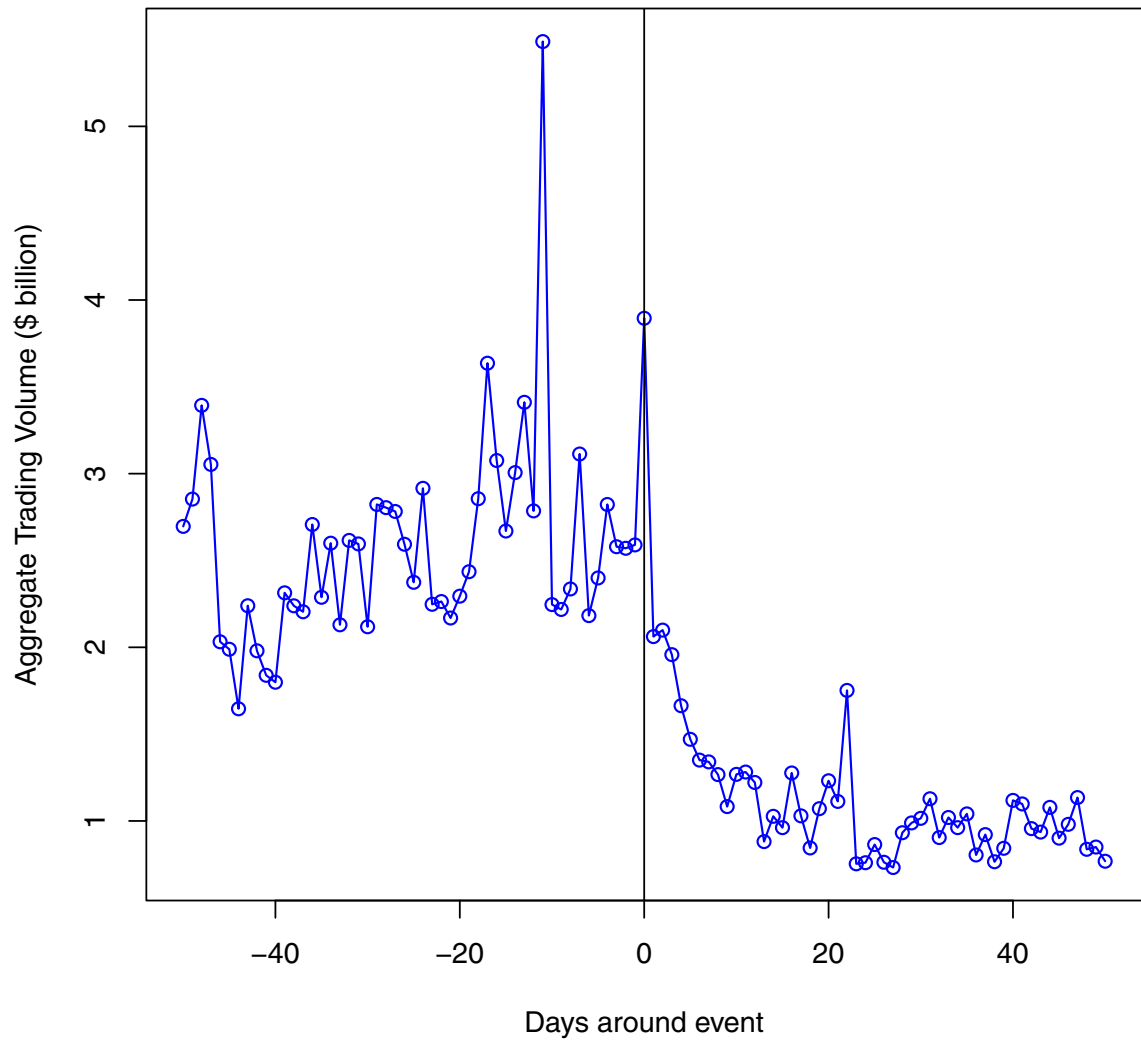
**Table 3.4: Abnormal event return regression for newly issued bonds included in the index.** The table shows the regression coefficients from regressions of the abnormal event return (day -1 to +1) for newly issued bonds included into the index on different issue specific characteristics and common bond return predictors. The bond characteristics are rating, coupon, maturity and the logarithm of the issuance size. The common bond return predictors are the level of a Moody's BAA yield curve, the monthly change in this yield curve, treasury level (1 year yield), treasury slope (10 - 1 year yield), and the level and change of an illiquidity factor. The illiquidity factor is a size weighted average of a bond specific monthly Amihud price impact measure across the entire corporate bond market. The first 4 columns show the results when all bonds are used and the remaining columns show the results for the same regressions when only the most traded bonds are used. The most traded bonds are selected on a monthly basis as the 1/3 bonds with highest turnover that month. All standard error are robust i.e. controlled for time series and firm fixed effects.

issues with long maturities. This makes sense since larger issues weigh more in the index than smaller issues and when buying longer term bonds index trackers reduce transaction costs. Running the same regression on the most heavily traded bonds show that maturity still remains significant. The issuance size has dropped out of the regression but that is mainly due to the conditioning on the most traded bonds, which is mainly the large issues. Even though the levels of the credit risk factor and the illiquidity factor is highly correlated, the illiquidity factors do a better job at explaining the returns than the credit risk factors based on the  $R^2$ . The driving difference is the monthly change in the illiquidity factor. The change in the illiquidity factor is not as much correlated with the corresponding change in the credit risk factor. When the market is illiquid or credit risk is high the abnormal returns are higher. Also when the illiquidity or credit risk have increased over the month abnormal returns are higher. The latter could be seen as extra compensation for the liquidity providers, who have probably bought the bond on the primary market and then hold it to the last day of the month. If liquidity has gone down, then the liquidity provision strategy has become more risky during the month and the index trackers may also have more problems locating the bond, which would lower their bargaining power against the liquidity provider. In the last column of table 3.4 we include the bond specific change to dealer inventory over day -1 to +1 around the inclusion. The inventory change is negative and significant (on a 5% level). This means that the more dealers net sell from their inventory the higher the abnormal return. One could also argue for the causality to be turned around, so that dealers only participate in the liquidity provision when the abnormal return is high (enough).

### 3.7 Downgraded Bonds

When a bond is downgraded from an index rating of investment grade to speculative grade it gets excluded from the index at the last trading day of the month. Still, whereas the downgrade itself could contain new information to the market the subsequent index exclusion should not contain any information. A corporate bond that gets downgraded from investment grade to speculative grade is also known as a fallen angel. Price pressure for fallen angels have been separately studied in Ambrose, Cai, and Helwege (2009), but without paying any attention to the index exclusion event.

Figure 3.7 shows the aggregate trading volume for all bonds excluded because of a downgrade. There is a clear spike on the event date indicating that index trackers sell out of their portfolio in order to minimize their tracking error. The bonds are downgraded between day -21 and -1, with most bonds being downgraded on the 15th calendar day of the month, which explains the tall spike on that day. When the bonds become speculative grade they start trading less than before (even when the period is extended



**Figure 3.7: Total trading volume for bonds excluded because of a downgrade.** Trading volume for each bond is added on a daily basis (without trading direction) and then the daily volume is added across bonds according to distance from the event. 412 bonds trade on the event date. The average turnover is 1.5%. We have excluded a few bonds which were downgraded during the last 5 calendar days of the month in order not to mix up any index effect with the abnormal trading volume around the actual downgrade.

back in time on the graph before any firm distress). Measured by outstanding volume Ford and GM bonds make up a large part of the market and an even larger part when only looking at fallen angels. The large volume spike in the downgrade period is mainly caused by trading in Ford and GM and the spike at day +21 is because Ford gets an index upgrade again the following month when Lehman included Fitch in the determination of spilt ratings (as explained in section 3.3).

Table 3.5 shows the abnormal returns for different periods around the exclusion event. Panel A shows the results for all bonds excluded whereas panel B shows the results without GM and Ford. Since Ford and GM make up such a large part of the market they might trade differently than other bonds. The event return from day -1 to +1 without GM and Ford is -136.0 bps consistent with a price decrease when index trackers are trying to sell the bonds. The negative return is reversed over the next 5 to 10 days. The pattern is the same when Ford and GM are included, although the event return is virtually 0 bps. The price falls on average with -2,145.7 bps in the downgrade period from day -21 to -5. This is not surprising since the downgrade probably contains some information for some of the bonds. Ambrose, Cai, and Helwege (2009) find that not all bonds fall in price at the downgrade to speculative grade, but mainly the bonds for which the stock price also decreases, which suggests that the price only falls when the downgrade conveys information. What is interesting in table 3.5 is that the average bond also decreases in price between day -5 to 0, even though no firms are downgraded in that specific period. It could be because the index trackers are selling out in this period or just because the market is illiquid, so that it takes some time for the downgrade information to get impounded into the price. The following reversal in price indicates that index trackers cause some price pressure. In panel B the event return of -136 bps is almost fully reversed after 10 days.

Figure 3.8 shows the cumulative change in dealer inventory over the shorter period from November 2008 to August 2009. The dealers unload (part of) their inventory before the downgrade, which can be seen from the decrease in inventory between day -30 and -21. It does not mean that the dealers can forecast the downgrade, since the graph is conditional upon downgrade and dealers might also decrease their inventory in other bonds which did not end up being downgraded. Comparing with the abnormal returns in table 3.5 it seems smart to unload inventory in this period, since the abnormal return between day -30 to -21 is 13.3 bps for all bonds and -119.7 when excluding Ford and GM. Both returns are far less than the downgrade month return of more than -2,000 bps. In the downgrade month from day -21 to -5 (where all the bonds in the sample get downgraded), the dealers keep a constant inventory. The constant inventory should be seen in connection with figure 3.9. Figure 3.9 shows the aggregate trading volume across all the bonds with day 0 being the actual downgrade date (and not



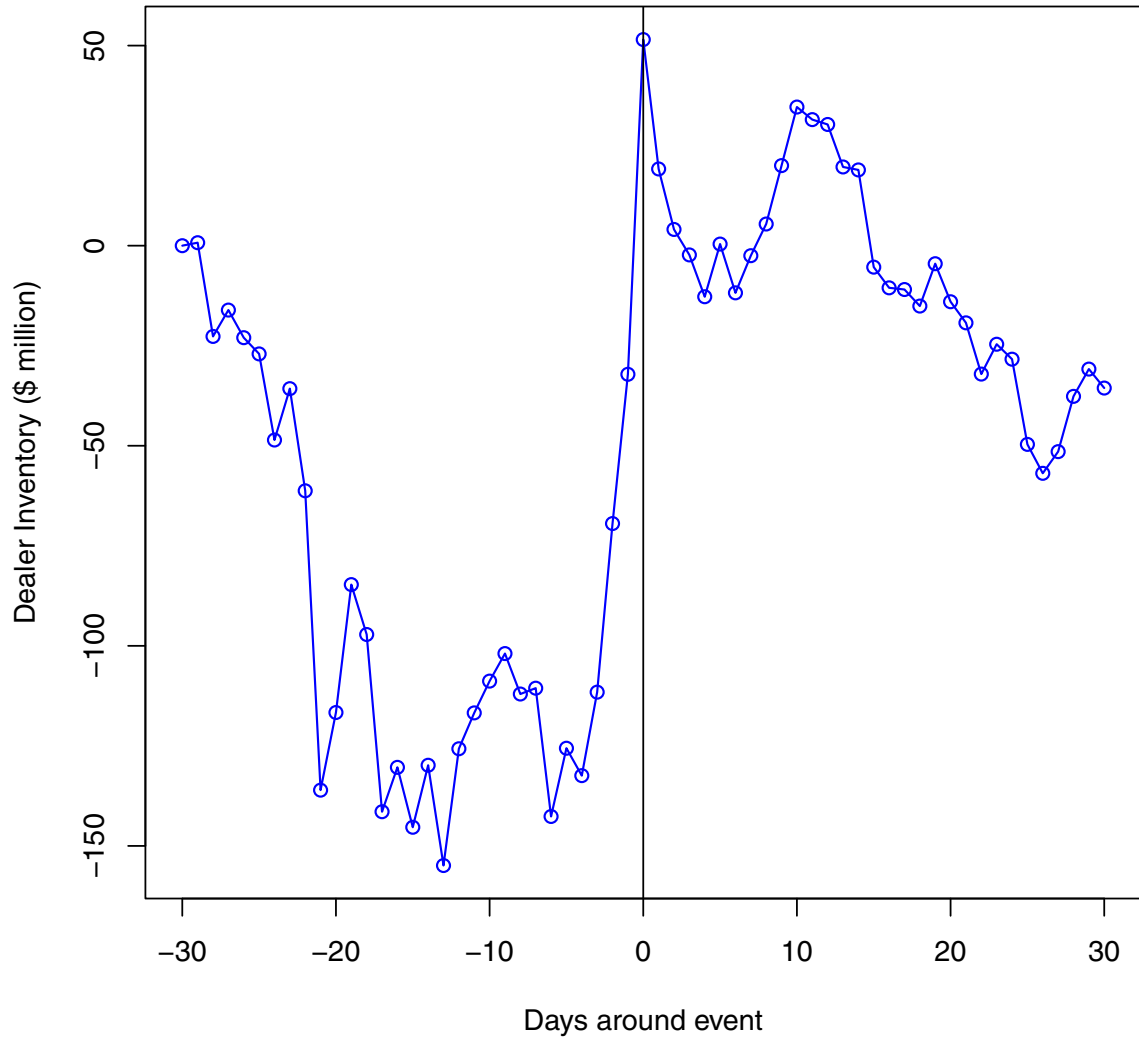
Panel A: All Bonds

From (Day)	To (Day)	N	Return (bps)	T-Stat
-1	1	360	0.2	0.003
-5	0	346	-935.1	-5.28***
-5	10	317	-586.5	-3.14***
0	4	315	348.7	6.17***
0	10	348	255.8	4.28***
-30	-21	279	13.3	0.31
-30	10	271	-2682.2	-6.20***
-21	-5	808	-2145.7	-7.34***

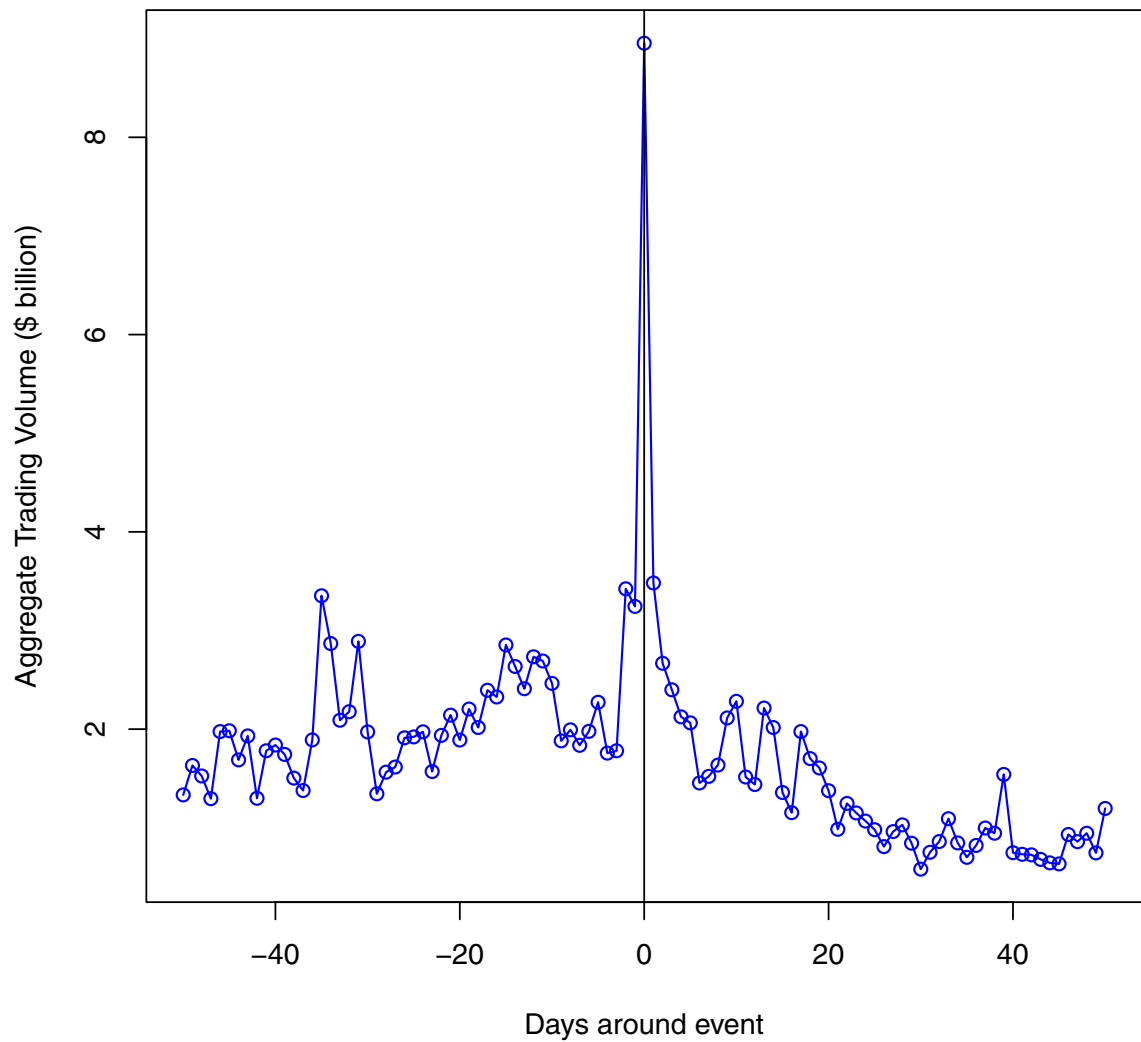
Panel B: Without GM and Ford

From (Day)	To (Day)	N	Return (bps)	T-Stat
-1	1	334	-136.0	-2.06**
-5	0	320	-1303.5	-6.25***
-5	10	291	-930.7	-4.35***
0	4	289	270.2	4.29***
0	10	322	125.0	1.90*
-30	-21	253	-119.7	-2.53**
-30	10	245	-3652.5	-7.28***
-21	-5	808	-2742.3	-8.23***

**Table 3.5: Abnormal returns at index exclusion of downgraded bonds.** The abnormal return is a size weighted average over all bonds for which it has been possible to calculate a return. The table excludes a few bonds that were downgraded on the last 5 trading days of the month in order not to mix up any downgrade day effect with the index effect. The benchmark return for each bond is the corresponding high yield Lehman/Barclays index that match on maturity and rating. The test statistics are cluster robust to time series and firm fixed effects. Panel A shows the results across all bonds. Panel B shows the results without Ford and GM bonds, which together make up a large part of the sample measured by volume.



**Figure 3.8: Dealer inventory around exclusion of downgraded bonds.** The graph shows the cumulative dealer inventory change. From November 2008 to August 2009 transactions in TRACE are marked as dealers sell, dealer buy or interdealer trade. Using this marking, we calculate dealer inventory change as the volume difference between dealer buys and sells each day for each bond. Then we add the bond specific inventory changes together according to distance from the event. Finally, we cumulate the aggregate changes using day -31 as benchmark.



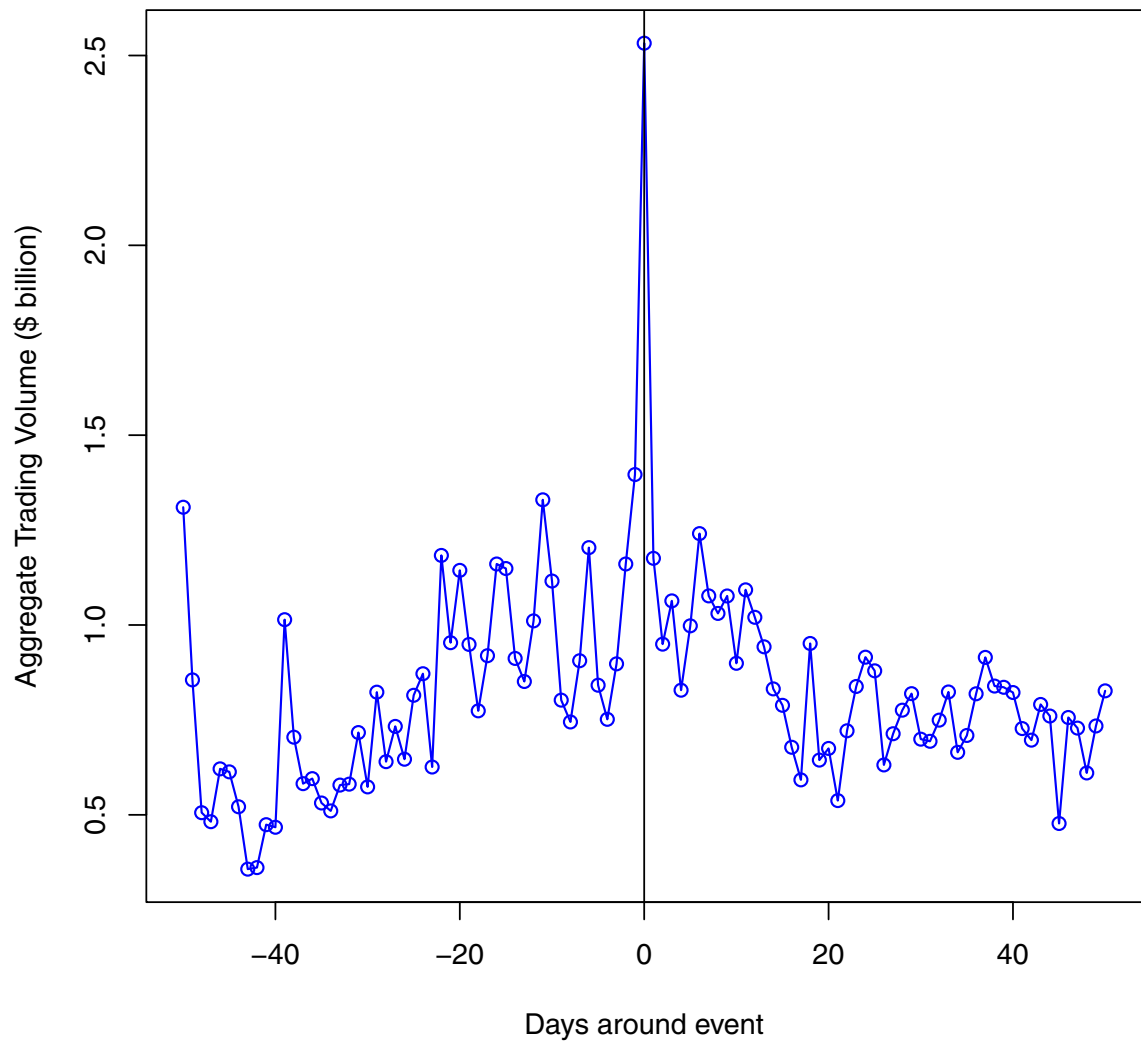
**Figure 3.9: Total trading volume for bonds around the actual downgrade date.** Trading volume for each bond is added on a daily basis (without trading direction) and then the daily volume is added across bonds according to distance from the actual event date. 288 bonds trade on the event date. For a large part of the bonds, we are not able to determine the exact downgrade date because of lack of data. The average turnover on the event day is 14.7%.

the index exclusion date). A lot of institutional investors are selling their positions on the downgrade day because they cannot hold speculative grade securities (even though they might have a grace period of up to 90 days). Despite the heavy trading activity on the downgrade day the dealers keep a constant inventory indicating that they are not absorbing any bonds. Instead they only perform a matching function. In this way the dealers earn the bid-ask spread and avoid holding the bonds when they subsequently fall in value between day -5 to 0. During the period up to the index exclusion where the index trackers are assumed to unload their position, the dealers net buy and fill up their inventory to a position around the original level from day -30. Finally the dealers sell out of their inventory in the days after the exclusion, where the price is partially reversed. In this case, where the reversal return is higher than the bid-ask spread the dealers play the role as liquidity providers for the index trackers. Still, there could also be other investors providing liquidity for the index trackers.

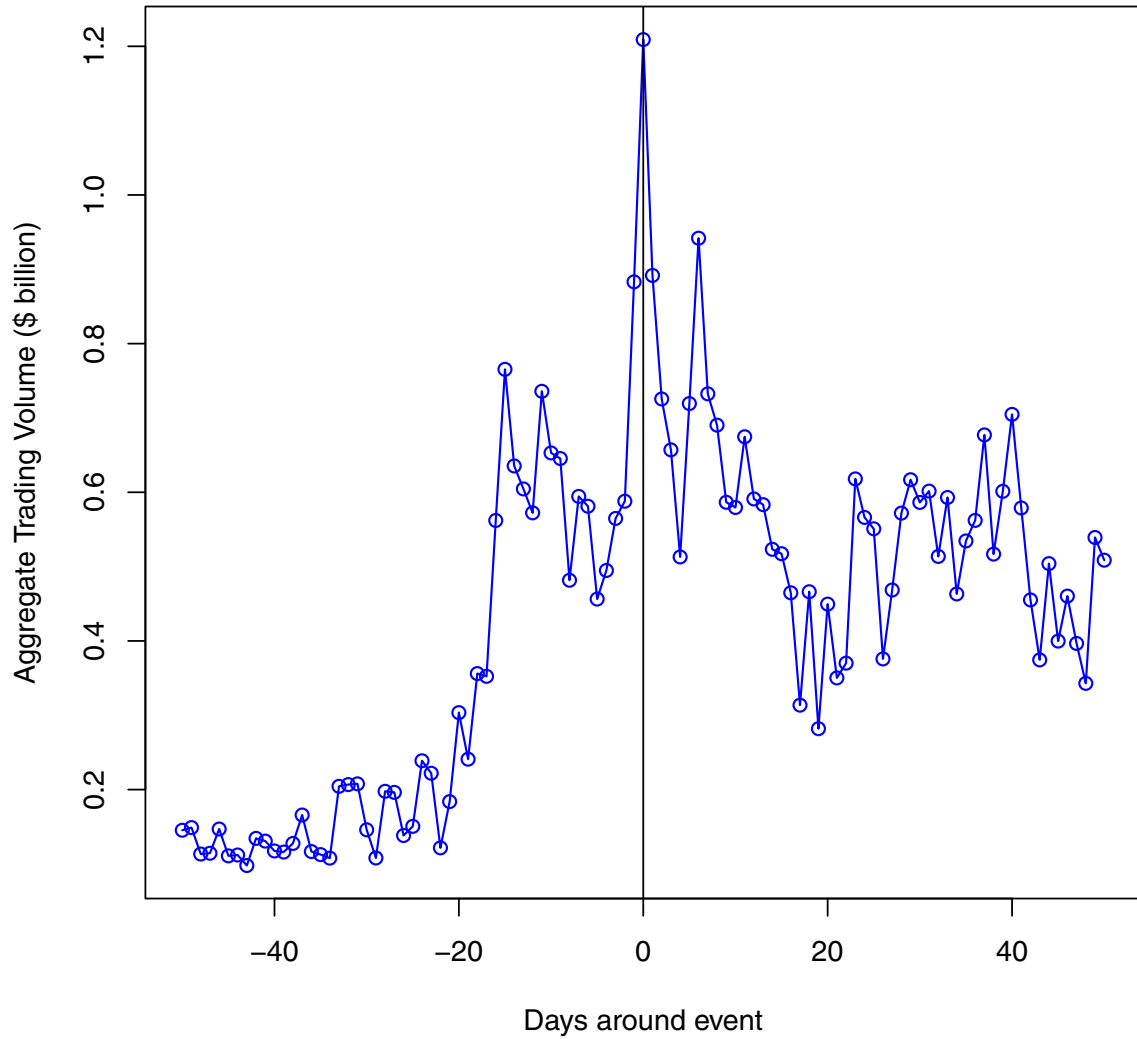
### 3.8 Upgraded Bonds

Bonds that get upgraded from speculative grade to investment grade are included into the index at the last trading day of the month. The index rating rule change in 2005 was an exception from normal upgrades. As described in section 3.3, Lehman included Fitch ratings together with Moody's and Standard and Poor's to determine the index rating of split rated bonds. The rule change has been specifically studied in Chen, Lookman, Schürhoff, and Seppi (2009), but only at the announcement date 5 month prior to the actual index change. Figure 3.10 shows the aggregate trading volume for all bonds around the index inclusion of upgraded bonds. The volume spikes on the inclusion date indicating that index trackers do buy the upgraded bonds at index inclusion. The picture becomes more clear when excluding the bonds that enter the index because of the rule redefinition, which can be seen in figure 3.11. There is an increase in volume between day -21 to 0, where the bond is actually upgraded. The same pattern can be seen in figure 3.12, which shows the aggregate trading volume, but around the actual upgrade day. There is a clear upward shift in trading activity from speculative grade to investment grade, with a spike on the day after the upgrade. The spike after the upgrade day is probably due to the timing of the upgrade, which could happen after market close.

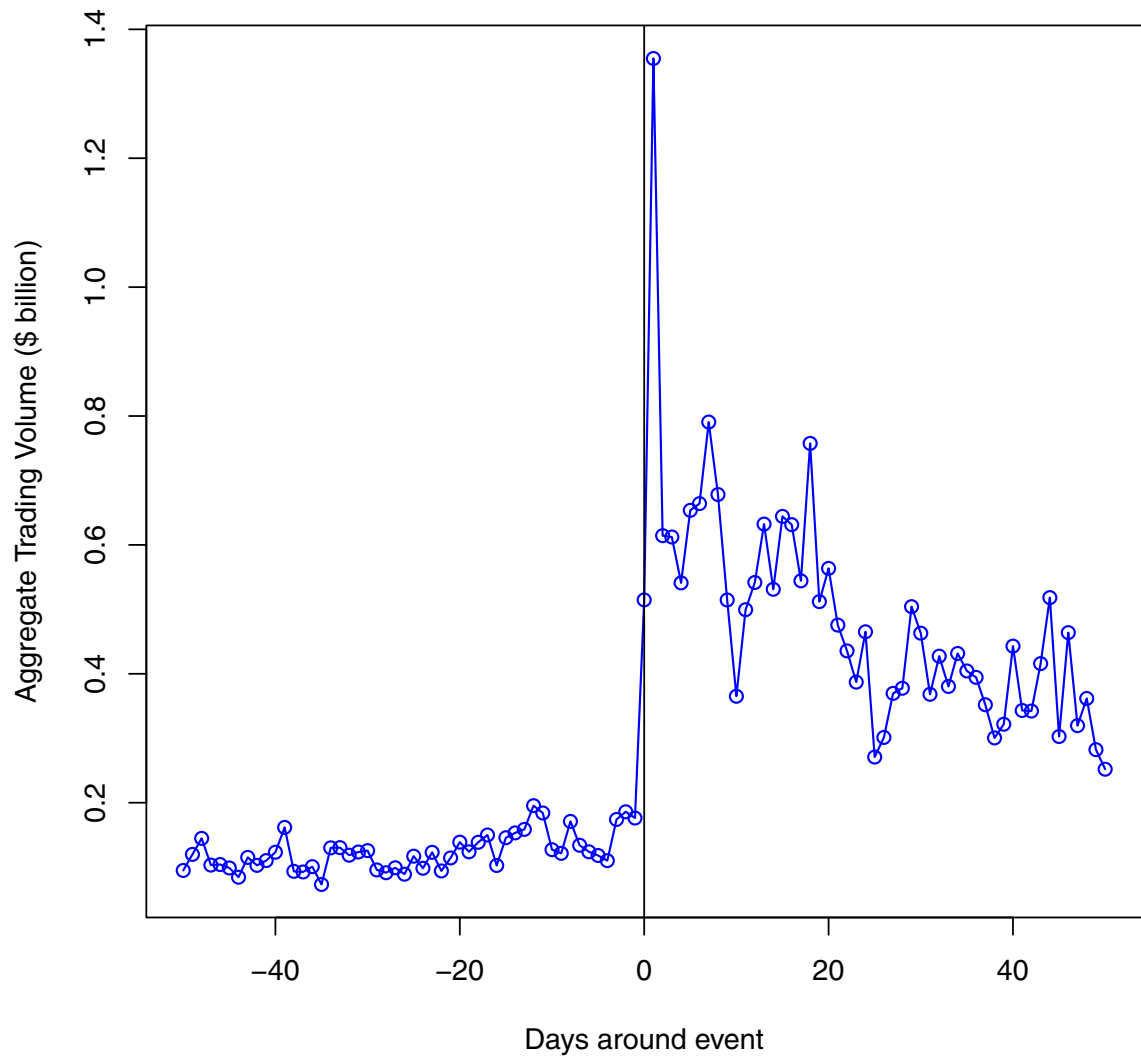
Table 3.6 shows the abnormal returns around the index inclusion date. We use the Lehman/Barclay investment grade corporate bond index as benchmark and match the bonds on rating and maturity (intermediate term ( $< 10$  years) or longer term ( $> 10$  years)). There is a positive abnormal return around the index inclusion indicating that demand is higher than normal at this point. The price increase is followed by a slight price reversal, but the reversal is only partial. When excluding the bonds that are



**Figure 3.10: Total trading volume for bonds included because of an upgrade.** Trading volume for each bond is added on a daily basis (without trading direction) and then the daily volume is added across bonds according to distance from the event. 206 bonds trade on the event date. The average turnover is 1.9%. We have excluded a few bonds which were upgraded during the last 5 calendar days of the month in order not to mix up any index effect with the abnormal trading volume on around the upgrade itself.



**Figure 3.11: Total trading volume for bonds included because of an upgrade without index rule redefinition months.** Trading volume for each bond is added on a daily basis (without trading direction) and then the daily volume is added across bonds according to distance from the event. 147 bonds trade on the event date. We have excluded a few bonds which were upgraded during the last 5 calendar days of the month in order not to mix up any index effect with the abnormal trading volume on around the upgrade itself.



**Figure 3.12: Total trading volume for bonds around the actual upgrade date.** Trading volume for each bond is added on a daily basis (without trading direction) and then the daily volume is added across bonds according to distance from the actual event date. 106 bonds trade on the event date. A large part of the bonds are excluded in the analysis, because of missing rating data. The average turnover for is 8.9% on the day after the event.

Panel A: All Bonds

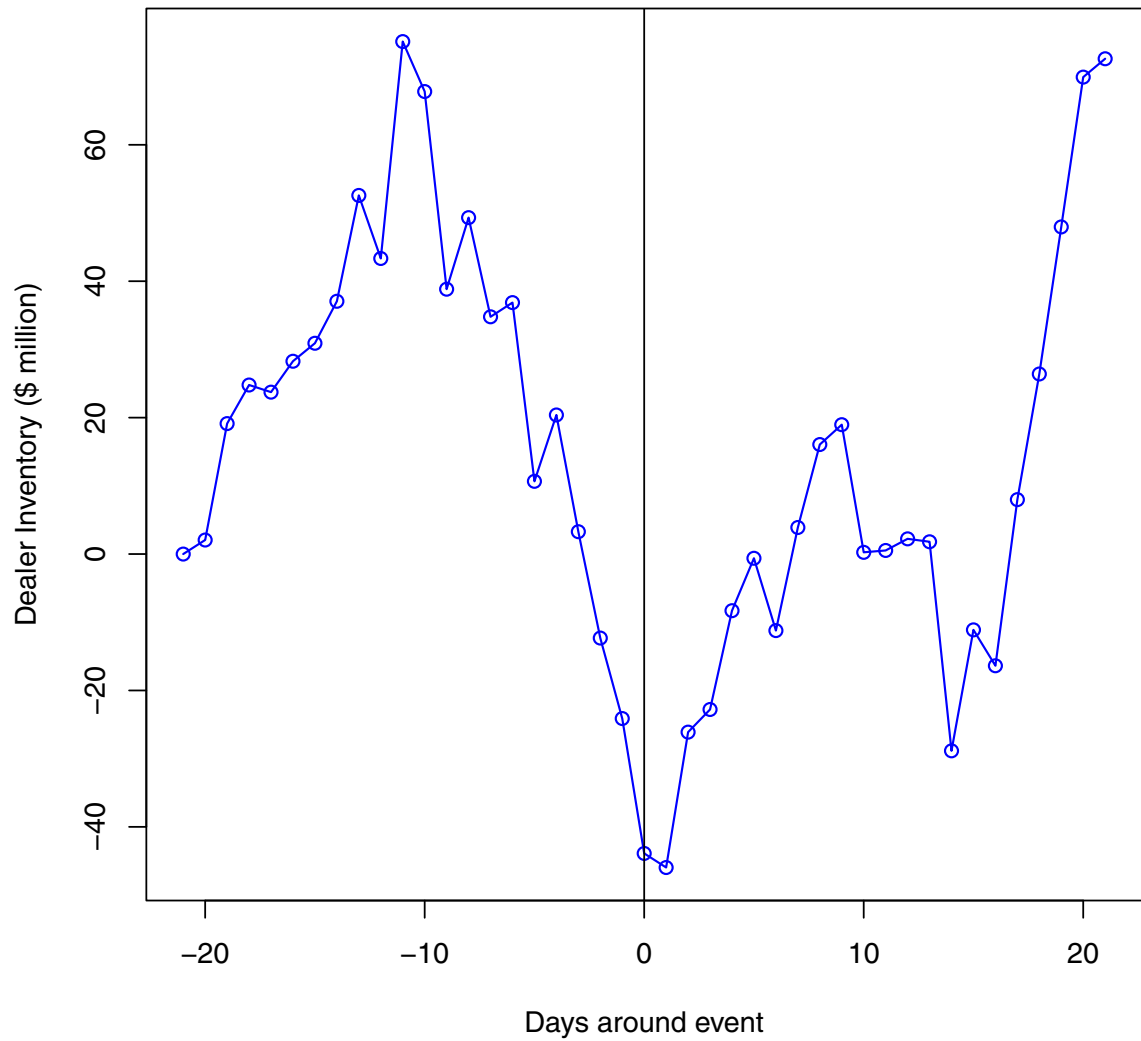
From (Day)	To (Day)	N	Return (bps)	T-Stat
-1	1	188	21.7	3.96***
-5	0	208	32.1	3.96***
-5	10	197	63.6	5.13***
0	4	161	-16.8	-2.60**
0	10	194	32.8	3.83***
-30	-21	165	257.2	8.35***
-30	10	155	259.6	8.31***
-21	-5	186	-13.6	-1.03

Panel B: Without index rule redefinition months

From (Day)	To (Day)	N	Return (bps)	T-Stat
-1	1	136	2.5	0.41
-5	0	152	9.3	1.07
-5	10	140	2.0	0.15
0	4	114	4.8	0.71
0	10	138	-7.4	-0.83
-30	-21	113	16.1	0.97
-30	10	101	53.8	1.95*
-21	-5	128	43.1	2.69***

**Table 3.6: Abnormal returns at index inclusion of upgraded bonds.** The abnormal return is a size weighted average over all bonds for which it has been possible to calculate a return. The table excludes a few bonds that were upgraded on the last 5 trading days of the month in order not to mix up any upgrade day effect with the index effect. The benchmark return for each bond is the corresponding Lehman/Barclays index that match on maturity and rating. The test statistics are cluster robust to time series and firm fixed effects. Panel A shows the results across all bonds. Panel B excludes the bonds that were mechanically upgraded because of the rating rule change in July 2005.





**Figure 3.13: Dealer inventory around inclusion of upgraded bonds.** The graph shows the cumulative dealer inventory change. From November 2008 to August 2009 transactions in TRACE are marked as dealers sell, dealer buy or interdealer trade. Using this marking, we calculate dealer inventory change as the volume difference between dealer buys and sells each day for each bond. Then we add the bond specific inventory changes together according to distance from the event. Finally, we cumulate the aggregate changes using day -21 as benchmark.

mechanically upgraded because of the index rule change in 2005 the event returns become insignificant. The remaining bonds show a positive and significant abnormal return in the period of the actual upgrade of 43.1 bps, which seems to be a lasting price increase. When including all bonds there is a significant and positive return of 257.2 bps from day -30 to -21, which is before the upgrade month. However, since the rule change was announced 5 month prior investors could at this point forecast that specific bonds would be upgraded. The increase in price before the upgrade month, which is mainly driven by the mechanically upgraded bonds could be explained by the incentives for the investors to front run the market in anticipation of a further price increase once the bond is actually upgraded. Figure 3.13 shows the cumulative change in dealer inventory around the index inclusion. The graph is only based on 28 bonds for the period November 2008 to August 2009, so it is not very robust. It seems like the dealers are buying up the bonds around the actual upgrade and then sell out in the days up to the index inclusion. Such a dealer strategy would be consistent with price pressure caused by the index trackers, but we could not see that effect in the returns in panel B of table 3.6.

### 3.9 Conclusion

Similar to stock index trackers, corporate bond index trackers seek to minimize their tracking error. This results in an increased trading activity at the rebalancing date in bonds that are included to or excluded from the Lehman/Barclays index. The trading activity spikes both when bonds are included because they are newly issued, when they are included because they are upgraded, when they are excluded because of low maturity and when they are excluded because they have been downgraded from investment grade to speculative grade. The information deciding whether a bond should be included or excluded is always available before the index revision and in most cases even long before. Hence, the informational content leading to the index revision usually gets impounded into prices before the bond is excluded or included.

Parallel with the increased trading activity from index trackers at the rebalancing date, the bonds experience a price pressure. When index trackers buy up bonds the price temporarily increases, causing an abnormal positive return. The abnormal return is fully reversed 5 to 10 trading days after the index inclusion. The opposite happens when index trackers are selling a bond that leaves the index. The price temporarily decreases, only to be reversed afterwards. The abnormal price pressure return and reversal are significant in all four cases, except for upgraded bonds where there is no price reaction to the index inclusion.

For bonds excluded because of low maturity and for bonds excluded because of a downgrade, the reversal returns are economically significant and

above an average bid-ask spread for large trades. We present empirical evidence that shows dealers only participate as liquidity providers for the index trackers when the reversal return is higher than bid-ask spreads. Hence, dealers increase their inventory up to the rebalancing date, when buying up excluded bonds. After the rebalancing date dealers decrease their inventory again to the pre-event level, so that they profit from the reversal return. Consistent with a profit maximization strategy, dealers do not trade against their inventory, when the reversal return is lower than an average bid-ask spread. This happens when newly issued bonds are included into the index and at the actual downgrade date. In both of these cases dealers only perform a matching function.

Index trackers seek to minimize transaction costs and thus only sample the bond index. In effect they do not hold all bonds in the index. We present evidence on which bonds are most attractive for the index trackers. In a regression using the price pressure return of newly issued bonds included into the index, we find that maturity and issuance size help predict the abnormal return and e.g. bond rating do not. This indicates that index trackers prefer bonds which weigh more in the index and which have a longer time in the index, both characteristics reduce the need for frequent trading.



# Summary

## English Summary

### Essay 1: Liquidity Biases in TRACE

The transactions database TRACE is rapidly becoming the standard data source for empirical research on US corporate bonds. This paper is the first to thoroughly discuss the assumptions needed to clean the disseminated TRACE data and to suggest that different filters should be used depending upon the application. According to the FINRA rule 6700 series all members (dealers) are required to report an over-the-counter corporate bond transaction in the secondary market using the TRACE system. The TRACE system was introduced for the first time in July 2002 and the dissemination of transactions from TRACE has gradually been expanded to include the entire corporate bond universe. The dissemination of the transactions is a great improvement for empirical research, but the construction of the system means that errors are accumulating. Around 7.7% of all trade reports in TRACE are errors and in some cases up to 18% of the reports should be deleted. The errors accumulate because TRACE is essentially a one day system. If a dealer makes a reporting error, he can easily correct it if the correction is made within the same day as the report was filed. In this case the dealer files a new report, but the old report containing the error still remains in TRACE. In the disseminated data this yields two reports. The first report contains the error, the second report either cancels or corrects the wrong report but none of them replaces the first report. Hence, one or both reports should be deleted from the sample before the data is used for research.

While same-day corrections can easily be matched with the original trade report, corrections on a later date cannot. In order to find the original report in the latter case a range of assumptions are needed. This paper explains the assumptions and sets up a filter that deletes almost all error reports from TRACE. For the 10 most frequently traded bonds in 2007 the deviation between filtered TRACE reports and the official FINRA number of reports is in the range of 0.05%. Failing to filter the data before use will most likely result in different biases. As an example this paper shows

that popular liquidity measures will be biased towards a more liquid market. The median bias for the daily turnover will be 7.4% and for a quarter of the bonds the Amihud price impact measure will be underestimated by at least 14.6%. These biases are encountered if nothing is done about the data after download. WRDS supplies the TRACE data for academic research together with some sample programs. However, if one uses the sample programs, they will encounter exactly the biases just described. A naive filter would be to delete all trade reports marked as error reports. This is what Bloomberg does when they report statistics based on TRACE data. However, the naive filter deletes less than half of what should be deleted, since they do nothing about the original report containing the error. Even after applying the error filter there is still a lot of identical trade reports in TRACE. They are a result of the way certain agency transactions are reported. I suggest deleting the duplicates if the price sequence is important, since the sequence otherwise would be biased. Finally, I show that normal price based filters cannot replace the error filter. In a normal price based filter trade reports are deleted if the transaction price falls outside a certain range based on the surrounding price sequence. A standard price sequence filter only deletes a very small fraction of the errors detected by the error filter. Price sequence filters are usually motivated by a desire to detect and delete typing errors from the dealers. However, it is worth noting that the TRACE system by itself performs a price sequence test. If the price deviates too much the trade report is dismissed and the dealer has to overwrite the system. The TRACE system then expands the allowed price range. If the price is still outside the range the report is dismissed again and the dealer has to phone in the report and explain why the price deviates.

## **Essay 2: Corporate Bond Liquidity Before and After the Onset of the Subprime Crisis**

The subprime crisis dramatically increased corporate bond spreads and while default risk certainly has increased because of funding constraints and the slowing of the real economy, it is also widely believed that deteriorating liquidity has contributed to the widening of spreads. The difficulty is how to measure this contribution. We analyze liquidity components of corporate bond spreads by using transaction-level corporate bond prices from TRACE. The high data quality of TRACE allow us to compute a range of different liquidity measures and asses their performance over a period spanning both a pre-crisis period and the onset of the subprime crisis.

When calculating the liquidity measures we restrict the transaction sample to only include trades with a nominal value above 100,000\$. In this way we only look at trades from sophisticated traders, since retail traders rarely trade above 100,000\$. The volume restriction together with the transaction level information significantly reduce the size of the liquidity measures

towards a more liquid pre-crisis market than what is usually found in previous studies. The performance of each bond liquidity measure is assessed by running marginal regressions with only one liquidity measure at the time. We use quarter-end yield spreads as the depended variable in all regressions and a range of different credit risk controls as independent variables. The marginal regressions show that the Amihud price impact measure, a measure of round-trip transaction costs and the standard deviation of these two measure are all significant in explaining the liquidity component in credit spread both before and after the onset of the crisis. The bond turnover, the Roll measure, the number of zero-trading days and the number of firm zero trading days are not consistently significant. Furthermore, we find in a principal component analysis of the liquidity measures that an equally weighted linear combination of the four significant measures captures most of the liquidity-related variation of credit spreads.

Using this new measure of bond liquidity we estimate the absolute and relative size of the liquidity component in credit spreads. Further, we use the measure to shed new light on flight-to-quality, liquidity risk, the impact of trading frequency, the role of funding shocks to lead underwriters, and the liquidity of corporate bonds issued by financial firms. We find that before the crisis, the contribution to spreads from illiquidity was small for investment grade bonds both measured in basis points and as a fraction of total spreads. The contribution increased strongly at the onset of the crisis for all bonds except AAA-rated bonds, which is consistent with a flight-to-quality into AAA-rated bonds. Liquidity premia in investment grade bonds rose steadily during the crisis and peaked when the stock market declined strongly in the first quarter of 2009, while premia in speculative grade bonds peaked during the Lehman default and returned almost to pre-crisis levels in mid-2009. The number of zero trading days did not increase with the crisis and we find evidence that this was because trades in less liquid bonds were split into trades of smaller size.

### **Essay 3: Index Driven Price Pressure for Corporate Bonds**

The impact of stock index tracking has been intensely studied and there exists several competing theories seeking to explain the price reaction at index inclusion. This paper is the first to test similar theories for a corporate bond index. Unlike the S&P 500 index, inclusions and exclusions to the Lehman/Barclays Corporate Bond Index are monthly recurrent events. The rules for inclusion and exclusion are fully transparent and based on bonds characteristics. Another unique feature of bond indices is that they are not limited to a certain number of securities. The main reasons for inclusion are that the bonds are newly issued or that the bonds get upgraded from speculative grade to investment grade. The inclusion itself always happens on the last trading day of the month, which makes the inclusions information

free events.

Trading activity at the date of inclusion is higher than normal and this spike indicates that some investors are in fact tracking the index. The incentive for index trackers to trade close to the rebalancing date is that they want to minimize tracking error (Blume and Edelen (2002)). A similar trading pattern can be observed for excluded bonds. Again, the trading activity spikes at the exclusion date, where index trackers want to sell the bonds. The main reason for bonds to be excluded from the index is because their maturity falls below one year or because they are downgraded from investment grade to speculative grade. As with the inclusions, exclusions are effectuated at the last trading day of the month.

Parallel with the increased trading activity from index trackers, the bonds experience a price pressure. When index trackers buy up bonds the price temporarily increases, causing an abnormal positive return. The abnormal return is fully reversed 5 to 10 trading days after the index inclusion. The opposite happens when index trackers are selling a bond that leaves the index. The price temporarily decreases, only to be fully reversed afterwards. The abnormal price pressure return and reversal are both significant in all four cases, except for upgraded bonds. For bonds excluded because of low maturity and for bonds excluded because of a downgrade, the reversal return is significant and above an average bid-ask spread for large trades over the period. In these two cases, dealers participate as liquidity providers for the index trackers. Dealers increase their inventory up to the rebalancing date, when buying up from the index trackers. After the rebalancing date dealers decrease their inventory again to the pre-event level, while they earn the reversal return. Consistent with a profit maximization strategy, the dealers do not trade against their inventory, when the reversal return is lower than an average bid-ask spread. This happens both when newly issued bonds are included into the index and at the actual downgrade date, where dealers only perform a matching function.

We find that the price pressure for newly issued bonds at index inclusion is positively correlated with maturity and issuance size and not with e.g. bond rating. This indicates that index trackers prefer bonds which weigh more in the index and which have a longer time in the index. These characteristics are important for the bond index trackers, since bond index trackers do not replicate the index, but only sample the index. Sampling the index means that they only hold part of the index, unlike S&P 500 index funds that holds all 500 securities. The Lehman/Barclay corporate bond index has around 3,400 bonds, so holding all the bonds would result in a very high level of transaction costs. In order to avoid the transaction costs, bond index funds only hold about 1/3 of the bonds in the index and they invest 20% of their portfolio outside the index. Despite the sampling strategy, we still find index driven price pressure in corporate bonds.



## Dansk Resumé

### Essay 1: Likviditetsbias' i TRACE

Transaktionsdatabasen TRACE er hurtigt ved at blive standarden indenfor empiriske undersøgelser af amerikanske virksomhedsobligationer. Denne artikel er den første, der grundigt diskuterer de forudsætninger, der ligger til grund for anvendelserne af dette datasæt. Inden man kan benytte den givne data er det nødvendigt at filtrere datasættet og slette alle transaktionsrapporter, der indeholder fejl, hvilket ikke kan gøres uden visse antagelser. Ifølge amerikansk lovgivning kræves det, at alle dealere indberetter handler i virksomhedsobligationer gennem TRACE systemet senest 15 min. efter, handlen er effektueret. TRACE systemet blev introduceret i juli 2002 og transaktionsrapporterne er efterfølgende blevet offentligt tilgængelige. Tilgængeligheden er gradvist blevet øget således, at alle transaktioner i dag er offentligt tilgængelige så snart, de er blevet rapporteret. TRACE systemet er således en kæmpe fordel for empiriske studier, men selve konstruktionen af TRACE gør, at fejl akkumuleres i det offentliggjorte datasæt. Omkring 7.7% af transaktionsrapporterne i TRACE skal slettes før datasættet kan bruges til forskning, og i nogle tilfælde skal helt op til 18% slettes. Fejlrapporterne akkumuleres, fordi TRACE er konstrueret som et samme-dag-system. Hvis en dealer kommer til at sende en rapport med en fejl, kan han nemt rette den indenfor samme dag, som rapporten er sendt. For at rette en fejl indenfor samme dag sender dealeren en ny rapport, men uden at den gamle rapport slettes. Den nye rapport fortæller blot, at den gamle rapport var en fejl, som man skal se bort fra. Men systemet indeholder nu to rapporter for en enkelt handel. Før datasættet kan bruges til forskning skal mindst én af rapporterne slettes. Hvis en dealer først retter en fejl på en dag, der ligger senere end den dag, hvor den oprindelige rapport blev sendt, sker det samme som lige beskrevet. Men i det offentlige datasæt kan vi nemt matche fejlrapporten med original rapporten indenfor samme dag, mens vi ikke kan lave samme match, når fejlrrapporten er sendt på en senere dato. I det sidst nævnte tilfælde opstiller vi en række antagelser, der gør, at vi alligevel kan lave et match til en potentiel original rapport. Ud fra antagelserne opstilles et filter, der fjerner alle fejlrapporter fra det offentlige datasæt. For de 10 mest handlede virksomhedsobligationer i 2007 kan vi sammenligne resultatet af vores filter med det tal, som FINRA selv finder. Den relative forskel mellem vores filter og FINRA's tal ligger alle i omegnen af 0.05%. Hvis man ignorerer filteret og blot bruger data uden nogen modifikationer, vil man højst sandsynligt få systematiske fejl i sine udregninger. Medianfejlen for den daglige omsætning vil ligge på 7.4%, mens Amihud likviditetsmål vil blive mindst 14.6% for småt for hver fjerde obligation. Disse fejl opnås, hvis man bruger datasættet ukritisk uden at gøre noget ved data inden brug. WRDS, som sikre offentlig adgang til datasættet, vedlægger samtidig også

små program eksempler, som anvender data. Disse programmer indeholder netop den slags fejl, som er beskrevet ovenfor. En naiv tilgang til brugen af datasættet, kunne være at slette alle rapporter, der er markeret som fejl-rapporter. Det er, hvad Bloomberg gør, når de bruger TRACE datasættet. Men denne tilgang sletter kun under halvdelen af fejlene, fordi de ikke gør noget ved den oprindelige rapport, som i første omgang indeholdt en fejl. En anden standardtilgang til at fjerne fejlrapporter er at lave et filter baseret på prissekvensen. Men et sådan filter fjerner kun en meget lille andel af de fejl, som vores filter finder, så det er på ingen måde en erstatning. Et prissekvensfilter fjerner rapporter, hvor prisen ligger langt væk fra resten af de omkringliggende rapporters priser. Dette filter bliver ofte motiveret, som et filter, der fjerner tastefejl begået af dealerne. Men faktisk har TRACE allerede et lignende filter implementeret. Så hvis en pris ligger ekstremt bliver rapporten ikke godkendt af systemet og dealeren skal ringe ind til FINRA og forklare prisen.

## **Essay 2: Likviditeten i markedet for virksomhedsobligationer før og efter starten på subprime krisen**

Subprime-krisen har ført til en voldsom stigning i kreditspændene for virksomhedsobligationer. En del af stigningen skyldes helt klart kreditrisiko, som følge af en nedkølning af real økonomien og begrænsninger i likviditeten hos de fleste virksomheder og investorer. Men stigningen i kreditspændene skyldes i høj grad også at selve virksomhedsobligationerne er blevet mindre likvide. Udfordringen i denne forbindelse er, hvorledes likviditetspræmien i kreditspændene kan måles. Vi analyserer likviditetskomponenten i kreditspændene for virksomhedsobligationer ved at bruge transaktionsdata fra TRACE. Den høje datakvalitet i TRACE gør, at vi kan udregne en række forskellige likviditetsmål og teste deres kvalitet over en periode, der både omfatter før og under krisen. Når likviditetsmålene udregnes ses bort fra handler med en nominel værdi på under 100.000\$. Derved bliver målene udelukkende udregnet på baggrund af handler fra sofistikerede investorer, da almindelige investorer sjældent handler over 100.000\$. Denne restriktion i udregningen, sammen med at vi bruger egentlige transaktionspriser, gør, at likviditetsmålene bliver signifikant mindre før krisen end, hvad man tidligere har fundet i litteraturen. For at afgøre hvilke likviditetsmål, der er bedst til at beskrive variationen i kreditspændenes likviditetskomponent, så estimerer vi en række marginale regressioner. Regressionerne er marginale i den forstand, at vi kun inddrager et enkelt likviditetsmål i hver regression. Vi bruger kvartalsvise obligationsspecifikke kreditspænd, som den afhængige variable og inddrager udover likviditetsmålet en række kreditvariable. Disse marginale regressioner afslører, at Amihuds likviditetsmål, et mål for direkte transaktionsomkostninger og disse to måls standardafvigelse er dem, der bedst fanger likviditetsvariationen i kreditspændene, både før og un-

der krisen. De øvrige mål; obligationsomsætningen, antallet af dage uden handler for obligationen, antallet af dage uden handler for virksomheden og Rolls transaktionsomkostningsmål, er enten ikke signifikante eller får inkonsistent fortegn. Vha. en principalkomponentanalyse finder vi yderligere, at en ligevægtet linearkombination af de fire gode likviditetsmål faktisk fanger det meste af den likviditetsrelaterede komponent i kreditspændene. Ved at bruge denne linearkombination og det deraf konstrueret likviditetsmål kan vi give et estimat af både den absolutte og den relative betydning af likviditetskomponenten. Vi bruger også målet til at kaste nyt lys på flight-to-quality, likviditetsrisiko, betydningen af handelsaktivitet, likviditetschok til de ledende obligationshandlere og likviditeten for obligationer udstedt af finansielle virksomheder. Vi finder blandt andet, at før krisen var illikviditetskomponenten i kreditspændene meget små for investment grade obligationer både målt i bps og som en relativ andel af spændet. Bidraget fra illikviditet steg voldsomt efter krisens start undtagen for AAA ratede obligationer, konsistent med en investorflugt væk fra dårligt ratede obligationer og ind i højt ratede obligationer. Illikviditetspræmien steg generelt under krisen for investment grade og toppede i første kvartal 2009, mens illikviditetspræmien for speculative grade obligationer toppede under Lehmans krak og efterfølgende faldt tilbage til før-krise-niveaueuet.

### **Essay 3: Pristryk fra rebalanceringen af virksomhedsobligation-index**

En række investeringsfonde følger en strategi, hvor de replikerer afkastet fra et aktieindeks. For disse fonde er det primære mål at følge afkastene så nøjagtigt som muligt. Det er således dårligt for fondene, både når de klarer sig dårligere end indekset, og når de klarer sig bedre end indekset. Det optimale for fondene bliver at handle nøjagtigt på de tidspunkter, hvor nye aktier bliver tilføjet til indekset. Denne handelsstrategi har en række implikationer for handlen og prisen for de aktier, der bliver inkluderet eller ekskluderet fra fx S&P 500 indekset. I litteraturen findes der en række forskellige teorier, der forklarer priseffekten både på kort- og langtsigt når en aktie bliver tilføjet til et indeks. Denne artikel er den første til at undersøge ligende teorier for et obligationsindeks. For Lehman/Barclay indekset for virksomhedsobligationer er inklusioner og eksklusioner tilbagevendende månedlige begivenheder. Reglerne, for hvilke obligationer der bliver inkluderet og ekskluderet, er fuldt transparente og udelukkende baseret på objektive kriterier. Desuden er antallet af virksomhedsobligationer i indekset ikke begrænset til et fast antal. De primære årsager til at en obligation bliver inkluderet er, at den er nyudstedt, eller at den har fået sin rating opgraderet til investment grade. Selve inkluderingen i indekset sker altid på den sidste handelsdag i måneden, hvilket gør, at inkluderingen i sig selv ikke indeholder nogen speciel information. Empirisk stiger handelsaktiviteten på selve dagen for

inkluderingen til et signifikant højere niveau end normalt, hvilket er konsistent med, at de fonde der følger indeksets afkast, køber obligationerne her. Et tilsvarende mønster finder sted ved ekskludering af obligationer fra indekset. Ekskluderinger sker primært, fordi obligationens løbetid falder under 1 år, eller at den får sat sin rating ned til speculative grade. Selve ekskluderingen finder ligesom inkluderingen sted på den sidste handelsdag i måneden. Samtidig med, at der er en stigning i handelsaktiviteten, der er drevet af indeksfondene, oplever obligationerne et midlertidigt pristryk. Når indeksfondene opkøber obligationer, der indtræder i indekset, så stiger prisen på obligationerne som følge af den øgede efterspørgsel. Efter indeksfondene har købt de obligationer, de skal bruge, så falder efterspørgslen, og prisen falder tilbage til det oprindelige niveau. Det samme sker med modsat fortegn, når indeksfondene sælger obligationer, der bliver ekskluderet af indekset. Både den abnormale stigning i afkastet og det efterfølgende fald er signifikante i alle af de fire tidligere nævnte tilfælde, på nær når obligationer bliver opgraderet ind i indekset. For de obligationer, der bliver ekskluderet, fordi de har lav løbetid eller fordi de er blevet nedgraderet, er prisændringerne større end de direkte transaktionsomkostninger. Derfor ser man også dealerne på markedet deltage aktivt i handelen og handle imod deres lager. Dealerne øger således deres lager når indeksfondene sælger ud af deres obligationer. Kort efter indeksrebalanceringsdagen sælger dealerne ud af deres lager og reducerer niveauet til det oprindelige lagers størrelse. Ved denne strategi tjener dealerne penge på det midlertidige pristryk. Derimod bruger dealerne ikke deres lager når obligationer bliver inkluderet i indekset. Her matcher de udelukkende handlerne, hvorved de tjener på kundernes transaktionsomkostninger. Igen er denne strategi optimal for dealerne, fordi pristrykket er mindre end transaktionsomkostningerne. Transaktionsomkostningerne er også bestemmende for indeksfondenes handelsstrategi. Mens en aktieindeksfond holder en andel af alle aktierne i indekset, så holder obligationsindeksfondene kun andel af cirka 1/3 af obligationerne i indekset. Det gør de for, at holde transaktionsomkostningerne nede. I tråd med dette foretrækker obligationsindeksfondene at holde obligationer med lang løbetid og fra store udstedelser.

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