Noisy prices and return-based anomalies in corporate bonds*

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ABSTRACT

We argue that the documented large abnormal returns to investors from corporate bond anomalies such as return reversals and momentum mainly stem from ignoring market microstructure noise in transaction-based bond prices and relying on ad hoc return winsorization. To address these issues, we construct bond data that is largely free of microstructure noise and closely mimics industry-grade quote data. We revisit prior findings in the literature and provide conclusive evidence that return-based anomalies, once properly constructed, generate negligible average returns and alphas. Finally, we show that the considered return-based factors (and their underlying signals) are not related to average bond returns.

Keywords: Empirical asset pricing; Corporate bond anomalies; Market microstructure noise; Bid-ask bias; Sharpe ratio.

JEL classification: C12; C13; G12.

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1 Introduction

Corporate bonds are characterized by complex differentiating features that set them apart from equities or other asset classes. Despite this, many insights and findings for equities have been applied verbatim to corporate bonds. The general theme in the empirical asset pricing literature as it relates to bonds follows a fairly standard blueprint, that is, the same anomalies that have been shown to predict stock returns or form the basis for candidate risk factors in equities are also overwhelmingly employed to investigate the time-series and cross-sectional properties of corporate bond returns.[1] By taking this approach, it is likely that salient features involved in conducting rigorous research in corporate bonds have been overlooked over the past decade. In this article, we address some of the serious and consequential missteps committed in bond pricing with a focus on return-based anomalies such as short- and long-term reversals and momentum.

Within the context of corporate bonds, we carefully characterize the notion of market microstructure noise (MMN), which is primarily comprised of bid-ask bias. MMN is found to be strongly time-varying, although it has steadily declined since the Trade Reporting and Compliance Engine (TRACE) system was introduced for corporate bond trading in July 2002. We show, comprehensively, that the results related to bond short-term reversals in the majority of existing studies based on the transaction-based TRACE database are systematically biased because they do not account for the complex over-the-counter (OTC) trading dynamics of corporate bonds. For example, MMN is so pervasive that after correcting for it, the average returns on all bond short-term reversal factors are close to zero and statistically insignificant. This is in sharp contrast to prior work, where bond short-term reversal strategies yield annualized returns of over 10% with Sharpe ratios greater than 1. Since most empirical bond pricing research makes use of the TRACE data, we provide a novel correction for the transmission of MMN bias and make all of our data and codes publicly available to researchers.

The effect of purging bond returns of MMN is so drastic that there is approximately a 90% reduction in the premium on the bond short-term reversal factor. (The monthly premium is reduced from 0.675% to 0.066%.) As a benchmark, the estimated biases in return premia related to equity price-based anomalies such as firm size, share price, and illiquidity are roughly 50% of

[1] This does not imply that all published papers in corporate bonds follow this trend.

the corrected premium estimates (Asparouhova, Bessembinder, and Kalcheva, 2013). On average, across all bonds, the level of bid-ask bias accounts for close to 10% of the average daily bond return. For speculative-grade bonds, this bias is much bigger. Our findings are consistent with those for small-cap stock returns (Blume and Stambaugh,1983) and equities in general (Asparouhova, Bessembinder, and Kalcheva, 2013). Over time, and post-financial crisis in particular, the level of bid-ask bias present in the TRACE data for both investment grade and speculative grade bonds is found to converge to zero.

Almost all prior research, as it relates to identifying risk premia in the cross-section of corporate bond returns, does not make an allowance for the possibly severe and consequential impact of MMN on transaction-based bond prices and metrics derived from these prices (e.g., bond yields and credit spreads).[2] While our interest lies in estimating premiums associated with return-based bond anomalies, the market microstructure issues we analyse are potentially relevant to a wide range of empirical asset pricing applications as they relate to corporate bonds. We do not claim that all of the findings in these studies would necessarily be overturned. However, in many instances, results are likely to be overstated due to the systematic biases induced by MMN, as emphasized in Appendix C. Overall, given that the corporate bond market is inherently less liquid than the stock market, ignoring the noise in observed prices can potentially have detrimental effects on results. On the other hand, for both momentum and long-term reversals, we show that asymmetric ex-post winsorization in either one of the tails of the pooled distribution of returns is the only way to generate a sizable and statistically significant average return spread across decile-sorted portfolios over all sample periods. Once the data is cleaned appropriately and the cross-sectional distribution of returns is not selectively trimmed, both of the aforementioned anomalies cease to exist.

The first part of the paper explores the effects that MMN has on daily and monthly bond returns over the past two decades, since the introduction of the TRACE system in July 2002. To this end, we employ the bond short-term reversal factor as a laboratory, and develop a simple and effective method to eliminate most of the measurement error inherent in the bond short-term reversal signal when using transaction-based bond prices. Across MMN-corrected short-term bond reversal decile portfolios, prior-month losers (i.e., bonds that had very poor relative returns) perform worse than prior-month winners (i.e., bonds that had very high relative returns).[3] In other words, bonds with

[2]Bartram, Grinblatt, and Nozawa (2021) is the only exception.

[3]When using the MMN-adjusted bond returns, on average, prior-month losers earn a monthly return of 0.59%

high returns in the prior month also earn high returns in the following month – a continuation or short-term momentum effect, which is the exact opposite of what the short-term reversal anomaly seeks to capture. This finding is in stark contrast to prior work that does not adjust for MMN, where average portfolio returns sorted on prior bond returns are monotonically decreasing. The relationship between the prior one-month return and the subsequent return on corporate bonds is nonlinear or U-shaped: past winners and losers in month t perform equally as well over month $t+1$. The vastly different findings for bond short-term reversal strategies are mainly due to the adverse effects of MMN on the unadjusted short-term reversal signals and the efficacy of our proposed method to mitigate MMN. While the detrimental impact of MMN on the results can (intuitively) be dampened by simply using industry-grade dealer quote data, this data is expensive and is not available to most academic researchers. Our simple correction procedure to the TRACE data delivers pricing results that are broadly aligned with those from quote-based databases, and we make this data publicly available.

In the second part of the paper, we provide thorough evidence that the bond momentum anomaly, first identified by Jostova, Nikolova, Philipov, and Stahel (2013, henceforth JNPS) yields a negative average return across all bond rating categories, sample periods, and databases. Surprisingly, bond losers perform better than winners. This finding is robust across both transaction- and quote-based bond databases using the same specification of corporate bond momentum that was employed in prior research. We comprehensively show that the results in JNPS are almost entirely driven by their asymmetric (and ex-post) data elimination scheme, whereby any return that is greater than the 99.5^{th} percentile is removed. This attenuates the effect of short-selling bonds with positive returns that end up in the loser decile, which artificially inflates the average high-minus-low return spread across momentum-sorted bond decile portfolios. The past 6- and 12-month bond losers outperform winners by 5 and 34 basis points per month, respectively, the opposite of what has been found in JNPS and Bali, Subrahmanyam, and Wen (2017). Our results broadly confirm those of Ehsani and Linnainmaa (2022) for the stock market, implying that the corporate bond momentum factor is certainly not a distinct risk factor.

From a structural modeling perspective, a corporate bond is equivalent to a portfolio that is long a risk-free bond of the same maturity and short a put option on the underlying assets of the

compared to 0.64% for prior-month winners.

firm with a strike price that is equal to the face value of the debt (Merton, 1974). As such, and given that bond payoffs are capped at par value, it would seem intuitive that the momentum effect is not present in highly-rated (investment grade) bonds that are trading close to par. Using the same argument, momentum should be more pervasive for bonds with a greater degree of default risk. However, we empirically demonstrate that the momentum premium for bonds with adverse credit ratings is similar to that of bonds with higher quality ratings, that is, close to zero. Even after data mining, in the spirit of Goyal and Wahal (2015), and forming over 50 momentum strategies, we are unable to find a single momentum specification where bond winners outperform bond losers in a significant way. These results are consistent with the seminal work on corporate bond momentum by Gebhardt, Hvidkjaer, and Swaminathan (2005b), who show that the spread between past investment-grade bond winners and losers is -0.45% per month over the period January 1973 to December 1996. Moreover, in contrast with the large and highly statistically significant momentum premium identified by JNPS for non investment-grade bonds, our results suggest that this premium is indeed close to zero. Our results are particularly surprising given the pervasive presence of momentum documented in the equity literature and prior empirical work on corporate bonds.[4]

Since the bond loser portfolio seems to perform as well as the winner portfolio, we conduct an additional data mining exercise and trim bond returns (ex-post) at the 99.5^{th} percentile and the $+30\%$ level as in JNPS and many follow-up studies in bonds. When this approach is implemented, we do observe the momentum effect documented by JNPS and many others after them. In other words, by artificially dampening the effects of bonds that perform well in the loser decile, ex-post trimming seems to play a defining role in teasing out a momentum spread. Asness, Moskowitz, and Pedersen (2013) find consistent momentum return premia across eight distinct markets and asset classes. It is quite revealing that corporate bonds, a major asset class, were excluded from their empirical analyses as it relates to identifying momentum.

We adopt a similar approach in an attempt to pin down an economically meaningful (and statistically significant) bond long-term reversal premium. Prior work by Bali, Subrahmanyam, and Wen (2021, henceforth BSW) documents that over an extended sample period, bond long-term reversals are a powerful predictor of future bond excess returns. Furthermore, they estimate an economically large long-term reversal premium of 0.47% per month (t-statistic of 6.12) with a large

[4]Equity momentum spillover, however, is an important predictor of future corporate bond returns. See Gebhardt, Hvidkjaer, and Swaminathan (2005b) and Choi and Kim (2018).

annualized Sharpe ratio of 1.11 (almost three times that of the bond market factor). Interestingly, over their shorter sample period (July 2002 to December 2017), using TRACE-based transaction data only, their long-term reversal premium is even larger. In contrast, the long-term reversal factor we construct yields a monthly premium of 0.13%, far less than what is reported in prior work. The single-factor bond capital asset pricing model (CAPMB) alpha is also statistically indistinguishable from zero. We then undertake another data mining exercise and construct over 100 different long-term reversal strategies. None of them yields sizable average return spreads that are statistically significant at any conventional significance level. As part of our data mining exercise, we winsorize bond returns at the -10% level. Once this is implemented, the high-minus-low decile spread is large and statistically significant across all sample periods. Again, ex-post winsorization rears its head, this time in the left tail of the return distribution. By attenuating the effect of bond losers that keep on losing, long-term reversals suddenly become significant across all data and time periods. Furthermore, in an extended sample analysis, we show that long-term reversals are not present in corporate bonds in the Lehman, Intercontinental Exchange (ICE), and Wharton Research Data Services (WRDS) databases as they consistently generate an average return spread that is statistically indistinguishable from zero.

We also show that the considered return-based corporate bond anomalies are not attractive from the perspective of a mean-variance investor. Our analysis provides evidence that the additional return-based factors proposed in prior work do not outperform the value-weighted bond market factor from an investment perspective. That is, the improvement to an investor's portfolio from including these factors above and beyond holding the bond market portfolio is statistically and economically marginal, at best. In other words, a prospective bond investor is better off from simply holding the bond market portfolio, a result which confirms prior work by Dickerson, Mueller, and Robotti (2023, henceforth DMR).

Finally, in the last part of the paper, we delve into the debate over whether it is exposure to systematic risk (factor loadings) or to characteristics that drives expected corporate bond returns, building on earlier work by Gebhardt, Hvidkjaer, and Swaminathan (2005a). In doing so, we employ the factor protocol methodology of Pukthuanthong, Roll, and Subrahmanyam (2019, henceforth PRS), and we extend our analyses to additional factors related to corporate bonds. Interestingly, none of the previously documented bond risk factors (return-based or otherwise) are priced in the cross-section of bond returns once controlling for the underlying characteristics. The characteristics related to reversals and momentum are also not significant predictors of future bond returns, indicating it is neither a risk nor a characteristic-based story. The only bond characteristics for which we find marginal support for predicting future bond returns (via multivariate Fama-MacBeth regressions) are ratings and historical value-at-risk. However, all characteristic-sorted portfolio alphas (and their associated high-minus-low spreads) are spanned by the bond market factor.

We are not the first to explore the role played by MMN in asset prices and its impact on biasing cross-sectional asset pricing tests. Since the seminal work on the topic by Blume and Stambaugh (1983), the majority of work on correcting asset prices for microstructure noise has been performed in equities, which is surprising given that potential noise issues in bonds are exacerbated by their OTC market structure and relatively low trading frequency.[5] Asparouhova, Bessembinder, and Kalcheva (2010) explore the role played by MMN in inducing an upward bias in the liquidity premium while focusing on the cross-section of equity returns. Asparouhova, Bessembinder, and Kalcheva (2013) build on their previous insights and apply several novel MMN corrections to an array of firm characteristics.[6] In corporate bonds, Bartram, Grinblatt, and Nozawa (2021) is the only paper that adjusts the (book-to-market) signal for bond price measurement error.

Reversals and momentum have been shown to predict future excess stock returns, and they have often been used to create long-short anomaly portfolios. For corporate bonds, most studies employ price-based investment signals that have not been adjusted for microstructure bias inherent in the TRACE data. This is again surprising given that Asparouhova, Bessembinder, and Kalcheva (2013) show that the estimated biases in return premia related to equity price-based anomalies (such as firm size, share price, and illiquidity) are substantial and in some cases account for more than 50% of the corrected premium estimates. Many studies document that (MMN-biased) bond short-term reversals yield extremely large means and Sharpe ratios. The list of papers that use unadjusted bond short-term reversals or price-based metrics as characteristics or risk factors is far more extensive. (See Lin, Wang, and Wu, 2014, Lin, Wu, and Zhou, 2018, Bai, Bali, and Wen, 2019, Bali, Goyal, Huang, Jiang, and Wen, 2020, Bali, Subrahmanyam, and Wen, 2021, Guo, Lin, Wu, and Zhou, 2022, Kelly and Pruitt, 2022, Ceballos, 2022, Cao, Goyal, Xiao, and Zhan, 2023,

[5] See Bessembinder, Spatt, and Venkataraman (2020) for a detailed review of the microstructure of fixed income markets.

[6] Of the various characteristics considered, those related to stock prices and illiquidity yield premia that exhibit the highest upward bias when MMN is not corrected for.

Li, Yuan, and Zhou, 2023, Duan, Li, and Wen, 2023, Bali, Beckmeyer, Goyal, and Wen, 2023, and Bai, Bali, and Wen, 2023, among others.)

The failure to adjust for market microstructure noise in TRACE-based bond prices also extends to a large body of research that relates to predicting future corporate bond excess returns with unadjusted predictors. In recent work on predicting the cross-sections of both stock and bond excess returns with machine learning methods, short-term reversals and momentum (in equities) are often the most important predictors for month-ahead returns. (See Gu, Kelly, and Xiu, 2020, and Bali, Goyal, Huang, Jiang, and Wen, 2020.) We argue that from a bond perspective, the finding of a short-term reversal effect is due to using the same noisy transaction-based prices in both signal formation and out-of-sample return computation. In essence, several crucial features of OTC bond trading have been ignored in the literature, and this has contributed to grossly overstating the predictive ability of price-based bond predictors for future returns.

On the other hand, momentum and long-term reversals, given the k-month skip period(s), are explicitly designed to avoid the short-term reversal effect (and momentum effect for the case of long-term reversals). Given that both momentum and long-term reversals can be considered the empirical cornerstones of behavioural finance (George and Hwang, 2007), it would seem plausible that these anomalies manifest themselves in corporate bonds as well. In fact, many of the papers that are able to pin down a profitable momentum strategy are those that introduce bias into the cross-sectional distribution of returns by winsorizing with ad hoc thresholds. For example, Galvani and Li (2023) show that the momentum findings are heavily influenced by how bond returns are trimmed/winsorized and that the majority of momentum profits are concentrated in retail-based trades, i.e., those below $100,000. Li and Galvani (2021) also find a positive momentum spread only after winsorizing in the left and right tails at the 1% level. In these studies, momentum strategies become profitable only when winsorizing the return distribution ex-post, which implies that the resultant momentum strategies cannot implemented in real time. Our findings are consistent with those of Khang and King (2004), Gebhardt, Hvidkjaer, and Swaminathan (2005b), Kelly and Pruitt (2022), Dick-Nielsen, Feldhütter, Pedersen, and Stolborg (2023), Kelly, Palhares, and Pruitt (2023), Dang, Hollstein, and Prokopczuk (2023), and Li, Yuan, and Zhou (2023), among others, who convincingly identify a negative average return spread across momentum-sorted portfolios.

This paper comprehensively investigates the role played by return-based bond anomalies in the cross-section of corporate bond returns. We offer a simple solution to addressing measurement error inherent in the transactions-based TRACE database, and make our MMN-corrected TRACE database and associated code publicly available to all researchers. In doing so, we properly construct the bond short-term reversal signal and associated factor, and show that short-term reversals do not improve investment performance. The effect of market microstructure noise is so strong that bonds exhibit a continuation (short-term momentum) effect instead of a short-term reversal effect, although the effect is not significantly different from zero. Long-term reversals and momentum are shown to also be largely redundant in forming corporate bond portfolios. Finally, we contribute to the debate surrounding whether it is systematic exposure to candidate risk factors or characteristics that explain expected bond returns, and find that neither the considered return-based bond factors nor their underlying signals are convincingly related to the cross-section of expected bond returns.

The paper is organised as follows. In Section 2, we discuss the relevant data used in numerous studies on corporate bond pricing. In Section 3, we characterise the extent of market microstructure noise present in transaction-based corporate bond data. In Section 4, we link the level of measurement error to the performance of the bond short-term reversal factor, and we offer a simple method to correct the short-term reversal signal when constructing bond short-term reversal factors with transaction-based price data. Sections 5 and 6 document the absence of momentum and long-term reversals in corporate bonds. In Section 7, we implement the PRS factor protocol and contribute to the debate on risk vs. characteristics for expected bond returns. Section 8 concludes. Additional data descriptions and results can be found in the Appendix.

2 Data

We rely on the intraday TRACE data, the WRDS monthly TRACE data, the monthly Lehman Brothers database (LHM), the daily Bank of America Merrill Lynch (BAML) quote data provided by ICE, and the Mergent Fixed Income Securities Database (FISD).[7]

For our main set of results, we rely on the constituents of the corporate bond database from BAML High Yield (H0A0) and Investment Grade (C0A0) indices, which is available through ICE with a start date of January 1997 and an end date of September 2022. To allow for a sample

[7]See Appendix A for a detailed description of the five databases and the filters that we apply to them to alleviate potential data errors.

start date that coincides with the beginning of the publicly available WRDS version of the TRACE data, we set our sample start date to August 2002 for our main results. In robustness exercises, we extend the data back to January 1977 and use alternative databases, all yielding very similar results to those in the main text. The choice of the ICE database has several key advantages: i) it serves as a common, error-free database given that it is constructed by a professional data provider; ii) monthly prices in the ICE database are sampled exactly at the end of each month, thus allowing to calculate exact monthly returns from month t to month $t+1$. This means that bond returns are sampled contiguously month-to-month with month-end prices. This is not the case for the TRACE data, where a bond must trade (i.e., transact) sufficiently close to the end of the month to be included in the monthly dataset; and iii) ICE provides pre-computed corporate bond characteristics including total bond returns, duration-adjusted returns, bond duration and convexity, and credit spreads that have not been winsorized or truncated.

As mentioned above, in robustness exercises, we extend our data back to January 1977 by using LHM. We also use the TRACE data provided by WRDS, covering the sample August 2002 to September 2022. After applying the filters recommended by Andreani, Palhares, and Richardson (2023), the findings based on the ICE and WRDS databases are almost identical both quantitatively and qualitatively.[8]

Throughout our main analyses, we define bond excess returns as the total bond returns in excess of the one-month risk-free rate. The long-short factors are computed with total bond returns. For our main analyses, we do not winsorize or filter the monthly excess bond returns used in our out-of-sample asset pricing tests.[9] For LHM, we carefully remove obvious data errors using a similar methodology to Chordia, Goyal, Nozawa, Subrahmanyam, and Tong (2017).

[8]We carefully adjust for the truncation of bonds with returns greater than +100% imposed by WRDS. Specifically, we set a truncated WRDS bond return equal to the return observed in the ICE database, i.e., if the truncated WRDS bond return is equal to 100%, we set it equal to the bond return from ICE. Moreover, if the ICE return is missing, we set the bond return equal to the return computed from the TRACE data itself. In total, we identify only 94 cases where the WRDS truncation occurs, and we are able to address 91 of them. The remaining three cases are removed. These adjustments do not make any material difference to the results.

[9]Winsorizing or trimming asset excess returns in an ex-post fashion (in the left or right tails of the distribution) would render the out-of-sample investment performance of any anomaly characteristic or long-short factor spurious by mechanically inflating the factor mean and deflating the volatility. This is fundamentally different from the return truncation imposed by WRDS, which aims at dampening the effects of spurious outliers/data errors in the TRACE data.

3 Characterising TRACE market microstructure noise

We now characterise the level of market microstructure noise present in the TRACE data and subsequently link it to the bond short-term reversal factor, which we use as a laboratory to study the effects of MMN on price-based signals and the returns from trading on these signals.

3.1 The bias in computed returns

The true price of bond i at time t is defined as $P_{i,t}$. This is the price at which a bond can simultaneously be bought or sold at exactly the same price posted by the dealer. Unlike most equities, bonds trade in an OTC dealer-driven market without a limit order book (LOB). As such, we define the daily closing bond price, $\hat{P}_{i,t}$, as the volume-weighted average of all prices on the intraday trades on a given day. $\hat{P}_{i,t}$ can and will differ from $P_{i,t}$ due to the bid-ask effect.[10]

Following Blume and Stambaugh (1983), the bid-ask effect is modeled as

$$\hat{P}_{i,t} = [1 + \delta_{i,t}]P_{i,t}, \tag{1}$$

where $\mathbb{E}[\delta_{i,t}] = 0$, $\delta_{i,t}$ is independently distributed across t, and $\delta_{i,t}$ is independent of $P_{i,\tau}$ for all τ.[11] The bid-ask effect is not only a cause of negative first-order serial autocorrelation in daily individual bond (and stock) returns. It also directly affects the rates of return for individual assets by inducing an upward bias (level shift) in the computed rate of return. To see this, denote the true return on bond i for period t by $r_{i,t} = \frac{P_{i,t}}{P_{i,t-1}} - 1$. The bid-ask effect only affects the clean price of the bond (the transaction price). As a consequence, bond accrued interest and any coupon payments are suppressed from the equation. Using Eq. (1), the computed return is $\hat{r}_{i,t} = \frac{\hat{P}_{i,t}}{\hat{P}_{i,t-1}} - 1 = \frac{[1+\delta_{i,t}]P_{i,t}}{[1+\delta_{i,t-1}]P_{i,t-1}} - 1$.

Combining the true and computed returns and taking expectations yields

$$\mathbb{E}[\hat{r}_{i,t}] = \mathbb{E}\left[\frac{1 + \delta_{i,t}}{1 + \delta_{i,t-1}}\right](1 + \mathbb{E}[r_{i,t}]) - 1. \tag{2}$$

By Jensen's inequality, $\mathbb{E}[\hat{r}_{i,t}] > \mathbb{E}[r_{i,t}]$. Following the final steps of Blume and Stambaugh (1983),

[10] This encompasses a variety of frictions that would render $P \neq \hat{P}$.

[11] Asparouhova, Bessembinder, and Kalcheva (2010) and Asparouhova, Bessembinder, and Kalcheva (2013) relax the independence assumption to allow for serial correlation in the true return process.

the bid-ask bias is approximated using a Taylor series expansion as

$$\mathbb{E}[\hat{r}_{i,t}] \approx \mathbb{E}[r_{i,t}] + \sigma^2[\delta_{i,t-1}], \tag{3}$$

where $\sigma^2[\cdot]$ denotes the variance and provides a lower bound for the bias induced by the bid-ask effect.[12]

3.2 Approximating the bid-ask bias in TRACE

To approximate the bid-ask bias in the TRACE data, denote by $\bar{P}_A$ and $\bar{P}_B$ the volume-weighted averages of all ask and bid prices on the intraday trades on a given day and assume that they occur with equal probability. Since the expected closing price is assumed to be equal to the true price, $P = (\bar{P}_A + \bar{P}_B)/2$. δ_i is then approximated as either plus or minus $(\bar{P}_A - \bar{P}_B)/2P$. In this case, the bid-ask bias in Eq. (3) becomes

$$\sigma^2[\delta_i] = \left(\frac{\bar{P}_A - \bar{P}_B}{\bar{P}_A + \bar{P}_B} \right)^2, \tag{4}$$

where $\sigma^2[\delta_i]$ is our proxy for the daily bid-ask bias in the TRACE data. The inputs to compute the level of bid-ask bias in Eq. (4) are readily available from the intraday TRACE database. For each day d, we follow Bessembinder, Kahle, Maxwell, and Xu (2008, henceforth BKMX) and compute the volume-weighted average bid ($\bar{P}_B$) and ask ($\bar{P}_A$) prices with trades that have a volume greater than \$10,000.[13] Note that by volume-weighting the intraday trade prices, we are inherently down-weighting the potential bias in daily bond prices, with the implication that our estimates serve as a lower bound for the level of noise in daily data.

Since bonds do not trade every day, we follow Bao, Pan, and Wang (2011) and require the number of business days between two trades to be less than or equal to five for the return to be valid. We also require bonds to have at least five trades per month, such that they trade on average approximately 25% of the business days in a given month, and to have a maturity of more than one year.[14] This further tilts our daily sample toward a more liquid subset of corporate bond prices. We also implement the data correction procedures in BKMX and eliminate large return reversals, defined as a 20% or greater return followed by a 20% or greater return of the opposite sign. Finally,

[12] This argument requires that the closing price is greater than zero and less than twice the true price.

[13] Results are similar when we remove trades less than \$100,000, with the bid-ask bias being mechanically smaller.

[14] Because of liquidity reasons, bonds with less than one year to maturity are typically removed from industry-grade bond indices.

we require that the absolute value of the return is less than 20%. We do not exclude observations for which the bid-ask spread is negative.[15] However, we do winsorize the distribution of the bid-ask bias at the 99.5% level as it involves a square, is bounded by zero in the left tail, and has a few extreme outliers. To be clear, we do not use winsorized or trimmed returns in our monthly asset pricing tests. To capture credit risk, we merge the sample to S&P bond credit ratings as provided by FISD. We also compute daily bond credit spreads, which are defined as the difference between daily bond yields and a duration-matched portfolio of U.S. Treasury Bonds. A valid observation requires non-missing information on bond rating, credit spread, return, and bid-ask spread.

To illustrate the general decline over time in both the level of the bid-ask bias and of the bid-ask spread, we split the daily sample into various subperiods following Bessembinder, Jacobsen, Maxwell, and Venkataraman (2018) and Wu (2022), among others. The subperiods include the pre-crisis period (July 2002 – June 2007), the crisis period (July 2007 – April 2009), the post-crisis period (May 2009 – May 2012), post Basel II.5 and III (June 2012 – March 2014), and post Volcker (April 2014 – September 2022).[16] In Table 1, we report the time-series averages of the cross-sectional means of the daily bid-ask bias, return, bid-ask spread, and credit spread in basis points (bps).

Table 1 about here

Over the full sample, the average level of the daily bid-ask bias across all bonds is 0.148 basis points (bps) relative to an average return of 1.703 bps. This implies that close to 10% of the average daily bond returns are comprised of bid-ask bias as per Eq. (4). The bias in bonds that are closer to default (ratings below BBB−) is much larger, with an average bias of 0.204 bps and average daily return of 1.665 bps. The higher level of bid-ask bias in speculative grade debt is also associated with a credit spread that is higher than for investment grade bonds (691 bps vs. 138 bps for A and above and 234 bps for BBB). The bid-ask spread itself is also higher for bonds with a higher credit risk (43 bps for speculative grade vs. 30 bps for investment grade).

Turning to the sub-sample analysis, during the crisis period, the level of bid-ask bias is substan-

[15]Recent work by Choi, Huh, and Shin (2023) documents that customers increasingly provide liquidity to the market following the adoption of post-2008 banking regulations, implying that the bid-ask spread can be negative.

[16]For a detailed description of the rule changes and their effects on bond liquidity, trading delays, and the liquidity risk premium, see Wu (2022).

tial and, as expected, the average daily bond returns are all negative except for the highly-rated A and above bonds. In this regard, the average bid-ask bias across all bonds almost doubles relative to the full sample (from 0.148 to 0.278 bps). The bias for bonds with adverse credit ratings during the financial crisis is highest at 0.373 bps. Post financial crisis, we see a systematic decline in the level of the bid-ask bias across all rating groups. The bid-ask bias drops to 0.168 post-crisis, to 0.081 post Basel II.5 and III, and to 0.049 after the introduction of the Volcker rule – an overall decline of 82% from pre-crisis to post-Volcker levels. The level of the bias after the introduction of the Volcker rule is close to zero and negligible relative to the average daily bond returns (0.049 bps vs. an average daily return of 0.768 bps). This holds true across all rating groups. However, at 0.079 bps, the bid-ask bias for junk bonds remains 61% higher than that of all bonds.

To further highlight the systematic decline in the level of the bid-ask bias in daily bond returns, we present the within-month averages of the daily bid-ask bias (Panel A) and the 12-month moving average of the within-month daily bond return autocorrelations in Fig. 1.

Figure 1 about here

In July 2002, the monthly bid-ask bias equals 0.667 bps, while in September 2022 (the most recent Enhanced TRACE data point) the bias equals 0.026 bps, a percentage decrease in the bias of about 96%. Turning to Panel B, it is clear that the level of serial autocorrelation in bond returns is trending upward, thus implying that the level of microstructure noise is strongly declining. A greater degree of negative return serial autocorrelation implies a greater degree of market microstructure noise and trading frictions. (See Admati and Pfleiderer, 1989, Mech, 1993, and Nagel, 2012, among others.)

It is important to note that the aforementioned results are not a statement about the level or change in the liquidity premium from the perspective of the bond dealer. As comprehensively shown by Wu (2022), most metrics related to liquidity (such as the bid-ask spread) indicate a general increase in the level of liquidity. Trading delays and the liquidity premium, however, have increased. Our argument is linked to the level of market microstructure noise, the level of the bid-ask bias, and its implications for trading upon short-term reversal signals and any other bond trading signals that rely on bid-ask averaged transaction-based price data. Regardless of trading delays and the elevated level of the liquidity premium, the level of MMN and bid-ask bias have systematically declined over time.[17]

In summary, the level of MMN, as measured by the bid-ask bias, in the corporate bond market has steadily declined after the financial crisis. However, failing to adjust for it when constructing short-term reversal signals, even at the monthly rebalancing frequency, materially changes the achievable investment returns for the implied short-term reversal strategies. We now turn our attention toward constructing bond short-term reversal portfolios with MMN-adjusted and unadjusted short-term reversal signals.

4 Microstructure noise and bond short-term reversals

The bond short-term reversal factor, constructed using the transaction-based TRACE data without adjustments, almost exclusively captures market microstructure noise/measurement error. Even at the monthly rebalancing horizon, the bid-ask bias has large and detrimental effects on the interpretation of short-term reversal portfolios.

Prior work in corporate bonds related to constructing reversal factors follows the methodology applied to equities by Jegadeesh (1990). The short-term reversal investment signal available at the end of month t is computed using a bond's prior 1-month return ($R_{t-1:t}$), with bond prices that comprise both dealer bids and asks. As already alluded to in the previous sections, bonds are a distinct asset class with a very different trading environment compared to equities, and Jegadeesh (1990) should not have been applied verbatim to bonds.

When dealing with transaction-based bond data, the transaction price at t, used to form the bond short-term reversal signal, should not also be used to compute the ex-ante return that the investor will realise over $t : t+1$. As noted by Bartram, Grinblatt, and Nozawa (2021) (henceforth BGN), price measurement error shared by the month-end signal and the subsequent return generates spuriously high correlation between the two when the same set of prices is used for both signal generation and ex-ante return computation (even at the monthly horizon).[18] By using the transac-

[17] The reduction in MMN is consistent with the reduction in corporate bond bid-ask spreads over time. In Eq. (4), MMN is essentially one half of the bid-ask spread squared. Our findings are also consistent with Brogaard, Nguyen, Putnins, and Wu (2022) who show a drastic decline in microstructure noise for the equity market since the mid 1990s.

[18] With quote-based data, this is not an issue as all market participants observe the same quote for the price of the bond at the end of month t, can use this price to compute the short-term reversal signal, and trade at the given price posted by the dealers.

tion price observed at t as the reversal signal, the implicit assumption is that this is the same price that investors use to purchase the bond. Given that most bonds do not trade on a given day, this is highly problematic.[19] This is precisely the reason why the short-term reversal anomaly has not been identified in the ICE quote-based data, which is the primary information source used by hedge fund and asset managers. Unlike the TRACE transaction data, for each bond in the sample there will always be a valid quote at the end of month t. This results in a synchronous time-series of bond prices, sampled at exactly the end of each and every month. Given that the data is quote-based, it is largely free of microstructure noise and bid-ask bias issues by construction.

We address the market microstructure noise (MMN) inherent in the TRACE data when constructing short-term reversal signals by designing an augmented correction procedure relative to BGN. To illustrate the efficacy of our approach in eliminating MMN, we compare the correctly-constructed bond reversal factor to the same factor constructed with the ICE quote-based data (which is largely free of market microstructure bias).

4.1 Correcting the short-term reversal signal

For the MMN-corrected TRACE-based reversal factor (REV^*), we propose an intuitive and effective way to compute the bond short-term reversal signals available to the investor in month t. Specifically, we source the daily bond price on the first available trading day of month t, which we denote by $p_{t,d}$ (it includes any accrued interest on day d). Thereafter, we locate the bond price at least one trading day before the end-of-the-month bond transaction price (P_t) used to compute the bond return from $t : t + 1$.[20] We denote this price by $p_{t,d+n}$ (it includes any accrued interest on day $d + n$). Critically, this methodology ensures that the prices used to construct the short-term reversal signal are not the prices used to construct the bond return that is earned from trading on the signal.[21] Our minimum one business day gap increases the probability that the correlations between estimated signals and estimated returns stem from signals that truly predict

[19] In a LOB-driven market as is the case for equities, this is a perfectly reasonable assumption. Work on corporate bonds by Palhares and Richardson (2020) suggests that the fraction of no trade days for IG (HY) bonds averages 69 (66) percent.

[20] We directly source the trade date used for the month-end price (P_t) from the WRDS bond database. The variable name is encoded as `T_DATE`.

[21] Note that the one business day gap implicitly assumes away any trade splitting or bond trades by a single investor that can take several days to materialize. Since the subprime crisis, trading into a corporate bond position (especially high-yield debt) can take several weeks, as emphasized by Wu (2022).

returns rather than any microstructure bias. Scenarios that artificially induce correlation between short-term reversal signals and subsequent returns become less likely the larger the gap between the prices used for signals and returns. Naturally, increasing the gap will reduce market microstructure noise but may also give rise to a more stale signal, and this is precisely the reason why we consider a one-business day gap in our main analyses.[22] Fig. 2 illustrates the transaction timing of prices that are used to construct the bond short-term reversal signal.

Figure 2 about here

We use the bond prices p to compute the short-term reversal signal for each bond i in month t as follows:

$$STREV^{*}_{i,t} = \frac{p_{i,t,d+n} + c_{i,t}}{p_{i,t,d}} - 1, \tag{5}$$

where $p_{i,t,d+n}$ is the bond transaction price (including any accrued interest on day $d+n$) at least one business day before the month end price $P_{i,t}$, $p_{i,t,d}$ is the bond transaction price (including any accrued interest on day d) closest to the beginning of month t, and $c_{i,t}$ is the coupon payment (if any). Note that this method delivers a short-term reversal signal that is very different from the one based on the prior-month bond price as in Jegadeesh (1990) for equities:

$$STREV_{i,t} = \frac{P_{i,t} + c_{i,t}}{P_{i,t-1}} - 1, \tag{6}$$

where prices are inclusive of any accrued interest. P_t now serves as the transaction price used to compute the signal as well as the price that is employed to buy into the position (and compute the associated return over the following month). Note the computation of $STREV$ in Eq. (6) is rendered even more problematic by virtue of P being computed using average intraday bid and ask prices at the end of month t. For the quote-based ICE $STREV$ signal ($STREV$), we can use Eq. (6) based on bond prices that are, by construction, largely free of bid-ask bias and market microstructure noise.[23] Finally, we denote the signal that uses the noisy TRACE data as $STREV^{MMN}$.

We also implement the method of Asparouhova, Bessembinder, and Kalcheva (2010) and use prior-month gross returns as the value-weighting scheme, and show that while this method slightly attenuates the $STREV^{MMN}$-based return spread, it cannot efficiently eliminate MMN in bond data.

[22] In Panel B of Table 3, we find that increasing the required gap beyond one business day has no material impact on the results.

[23] The bond prices provided by ICE are bid-side prices only.

4.2 Portfolio sorts based on short-term reversal signals

We now sort bonds into decile portfolios based on their respective prior-month short-term reversal signal. For all variants of the reversal signal ($STREV^{*}$, $STREV$, and $STREV^{MMN}$), we sort bonds into deciles using the short-term reversal signal observable to the investor at the end of month t, and we hold these bonds for a single month, recording the ex-ante return over month $t : t+1$. The portfolios are rebalanced monthly with average returns weighted by bond amount outstanding. In Panels A and B of Table 2, we report the average excess returns and one-factor CAPMB alphas, respectively. Panel C replicates Panels A and B with the noisy $STREV^{MMN}$ signal with weights determined by prior gross returns. Asparouhova, Bessembinder, and Kalcheva (2010) show that for equities, this weighting mechanism is effective in reducing noise.

Table 2 about here

Strikingly, the average return on decile 10 (bonds that performed very well in the prior month), for both the $STREV^{*}$ and $STREV$-sorted portfolios, is almost identical to the average return on decile 1 (bonds that had the worst prior-month returns). On average, when correcting for measurement error in the TRACE data, purchasing bonds that had high prior-month returns yields an average return of 0.64% as opposed to 0.59% for bonds that had poor prior-month returns. For the ICE-based short-term reversal signal, these average returns are remarkably similar. Deciles 1 and 10 earn the same average excess return, i.e., 0.58% per month. The two methods yield a high-minus-low spread of close to 0% per month, which is not statistically different from zero at all nominal levels of the tests.

This initial finding highlights the efficacy of the approach we employ to reduce MMN in the TRACE-based short-term reversal signals ($STREV^{MMN}$). In Panel B, the Q10-Q1 single-factor CAPMB alphas for the $STREV^{*}$ and $STREV$ signals are also close to each other at 0.22% and 0.15% per month, and they are both statistically insignificant at the 5% nominal level. These results indicate that there is no short-term reversal pattern in bonds: past losers perform about the same as past winners.[24]

[24]If anything, upon examining the alphas across Q1 to Q10, the short-term momentum effect dominates the reversal effect. The TRACE (ICE) alpha for Q10 is positive at 0.16% (0.12%) as opposed to −0.06% (−0.02%) for Q1. However, both alphas are statistically indistinguishable from zero.

Turning to the decile portfolios sorted on the unadjusted prior-month bond returns from TRACE ($STREV^{MMN}$), we observe a very large and statistically significant average monthly high-minus-low spread of -0.69% (-8.28% annualized) and corresponding CAPMB alpha of -0.46% (-5.52% annualized). As we will show in the next section, the upward level shift in the average returns on the $STREV^{MMN}$-sorted portfolios is almost entirely driven by MMN, even at the monthly rebalancing frequency.

To investigate whether MMN adjustments that have been found to be successful for equities are also effective in reducing noise in bonds, we report the $STREV^{MMN}$-sorted average decile returns and alphas using prior-month gross bond returns as the weighting scheme. The results are presented in Panel C. The high-minus-low spread is marginally reduced to -0.61% from -0.69%. However the spread remains large in absolute value and statistically significant. The CAPMB alpha also remains negative and significant. This shows that while very effective in reducing noise in equities, the use of prior-month gross returns as a weighting scheme is not as effective as our proposed method in reducing the impact of measurement error on signal formation and ex-ante return computation.

In Table 3, we also follow Kaul and Nimalendran (1990) and Blume and Stambaugh (1983) and compute short-term reversal signals with TRACE ask- and bid-side only prices ($STREV_{ASK}$ and $STREV_{BID}$, respectively), which mechanically removes the bid-ask effect in the computation of the short-term reversal signal.[25]

Table 3 about here

In Panel C, we report the average excess returns across decile portfolios that are formed based on $STREV_{ASK}$ and $STREV_{BID}$. Not surprisingly, the results of this analysis confirm our previous findings with TRACE MMN-corrected and ICE-based short-term reversal signals. In other words, researchers could also rely on bid- or ask-side only prices and use the same transaction price in both signal formation and ex-ante return computation. However, in this latter scenario, the sample is drastically reduced by about 22% for bids and 47% for asks, as in many cases there are no bids or asks within the last five business days of the month.[26] Overall, by failing to account

[25]The signals are computed based on prior-month prices without any intra-month business day gap.

[26]In a similar fashion, one could use interdealer prices. Duffie, Gârleanu, and Pedersen (2005) show that the interdealer price may be closer to the true asset price, and MMN would be less pervasive for this set of prices.

for the OTC dynamics and transaction-based pricing environment of corporate bonds from the TRACE database, the results from constructing short-term reversal anomaly portfolios are almost completely spurious.[27]

4.3 Short-term reversal factors in corporate bonds

We now construct bond short-term reversal factors for both the TRACE and ICE databases using the $STREV$, $STREV^{MMN}$, and the MMN-adjusted $STREV^{*}$ signals. We follow the standard methodology for constructing risk factors in the corporate bond literature by independently sorting bonds into 5×5 portfolios according to their ratings and their respective $STREV$, $STREV^{MMN}$, or $STREV^{*}$ signals. Then, for each rating quintile, we calculate the weighted-average return difference between the lowest-return quintile and the highest-return quintile. Finally, the factor is computed as the average long-short portfolio return across all rating quintiles. Before analyzing investment performance in detail, we graphically show the efficacy of our market microstructure adjustments in Fig. 3.

Figure 3 about here

In Panel A, we present the cumulative value of \$1 invested in the reversal factor computed using the TRACE data (REV^{*}, formed with the microstructure adjusted $STREV^{*}$ signal) and the reversal factor computed using ICE data (REV), which is inherently free of MMN and serves as our benchmark. The cumulative dollar value from investing in the TRACE (ICE) REV^{*} (REV) factor is \$1.185 (\$1.507), which is clearly a poor return on an investment. Note how the cumulative return lines are tightly aligned with very similar cumulative values. In Panel B, we present the monthly returns with a correlation coefficient of 0.894, as emphasized by Panel B of Table 3.

In Panel C, we present the cumulative return of \$1 invested in the incorrectly constructed and market microstructure bias-prone reversal factor (REV^{MMN}, formed using the unadjusted $STREV^{MMN}$ signal) and the REV^{*} factor as a benchmark. Unlike in Panel A, the cumulative return line is very steep and returns a dollar value of \$5.07, which is about five times what is achievable with the REV or REV^{*} factors. In Panel D, there is a clear level shift in the returns

[27] In Panel A of Table 18 in the Appendix, we use an extended sample that starts in January 1977 and document a high-minus-low spread of -0.04% (t-statistic of -0.43) with a CAPMB alpha of 0.00% (t-statistic of 0.03).

on the unadjusted MMN-prone REV^{MMN} factor, especially toward the beginning of the TRACE sample. This level shift is the likely manifestation of not correcting for measurement error in signal formation. The evidence presented in this figure shows that the TRACE short-term reversal factor (REV^*) performs similarly to the ICE factor (REV), which is largely free of MMN by construction.

We further examine the level shift across the REV^* and REV^{MMN} factors by plotting the monthly factor returns over four sub-samples in Fig. 4.

Figure 4 about here

In Panel A, spanning the sample period 2002:09 to 2007:08, the reversal strategy returns are tightly correlated, but the REV^{MMN} factor is almost always positive and exhibits a large level shift relative to the MMN-adjusted REV^* factor. Across Panels B to D, this level shift starts to fade. The convergence can be clearly seen in Panel D, where the level shift induced by computing returns with unadjusted price signals completely disappears. These findings are consistent with the decreasing over time TRACE bid-ask bias documented in Table 1 and Fig. 1.

In Table 4, we report the average short-term reversal factor returns, CAPMB alphas, and squared Sharpe ratios.

Table 4 about here

The MMN-adjusted REV^* and the ICE-based REV factors both exhibit means that are close to zero (0.066% and 0.170% per month, respectively) and are statistically insignificant at conventional nominal levels of the tests. The bias-adjusted squared Sharpe ratios proposed by Barillas, Kan, Robotti, and Shanken (2020, henceforth BKRS) and single-factor CAPMB alphas are also statistically indistinguishable from zero. In contrast, the unadjusted REV^{MMN} factor yields a large average return (alpha) of 0.675% (0.564%) per month, with a similar standard deviation to the MMN-adjusted REV^* and quote-based REV factors, and a very large and significant bias-adjusted squared Sharpe ratio of 0.101. Thus, we confirm and reinforce the DMR findings by showing that properly-constructed short-term reversal factors are spanned by the bond market factor. Overall, we are unable to detect short-term reversals in corporate bonds once the pervasive measurement error in transaction-based prices is accounted for. Bond short-term momentum appears to be as strong as short-term reversal.

5 Momentum lost (and not found)

We now instigate the corporate bond momentum anomaly and show, comprehensively, that losers outperform winners, such that the resultant long-short portfolio yields a negative average spread. We follow the momentum specification employed by JNPS and form the momentum signal for each bond i at time t as the cumulative return sum

$$MOM^s_{i,t} = \sum_{l=t-6}^{t-1} R_{i,l}, \tag{7}$$

where l is the look-back (formation) period which comprises a total of 6 months of data.[28]

Different from short-term reversals, MMN is unlikely to play an important role for momentum given the one-month skip period in the formation window. Similar to the previous section, we sort all bonds into deciles at time t based on their respective momentum signals, and we then compute the one-month ahead average excess returns.[29] We report the average excess returns and the single-factor model alphas in Panel A of Table 5.

Table 5 about here

It is clear from the decile sorts based on momentum signals that the pattern in the average decile returns is U-shaped. Past losers earn an average return of 0.58% compared to past winners that earn 0.53%. The resultant high-minus-low spread is negative (−0.05%) and not statistically different from zero, thus confirming the finding of Gebhardt, Hvidkjaer, and Swaminathan (2005b) on corporate bond momentum. The bond market alpha on the high-minus-low spread is positive at 0.16% but statistically insignificant at conventional nominal levels. In Panel B, when we use a 12-month formation period, we observe a similar set of results with a high-minus-low spread that is even more negative. Past bond losers earn 0.79% per month compared to past winners that earn 0.45%, implying that past losers outperform past winners by 34 basis points per month.

In Panel B of Table 18 in Appendix B, we use an extended sample that starts in January 1977 and confirm that the high-minus-low decile spread is −0.01% (t-statistic of −0.09) with a CAPMB

[28] In the subsequent analysis, we also data mine with several other formation period specifications for the momentum signal as in JNPS and Bai, Bali, and Wen (2019).

[29] To be consistent with the signal computation for the other factors in the analysis, we rebalance the portfolios every month. However, changing the holding period from one to three or six months leaves the results materially unchanged.

alpha of 0.06% (t-statistic of 0.52). This preliminary finding severely questions the presence of a momentum effect in corporate bonds. Both past bond winners (high momentum) and losers (low momentum) perform about the same, and we cannot reject the null of a zero average high-minus-low spread.

In Table 6, we report results for the momentum specification employed by JNPS with overlapping strategy returns as in Jegadeesh and Titman (1993), using equally-weighted portfolios. In doing so we also extend our sample back to January 1974 (the start date used by JNPS).

Table 6 about here

In Panels A and B, the bond loser decile performs about the same as the winner decile, thus confirming that the relationship between momentum and future bond excess returns is U-shaped. In Panel C, at 0.09, the average high-minus-low spread is positive but insignificantly so.

To reconcile our vastly different findings with JNPS, we ex-post winsorize/trim the return distributions as recommended in their footnote 16. Specifically, we ex-post trim any bond excess return that is greater than the pooled (over bonds and time) 99.5^{th} return percentile or the +30% return threshold in Panels D and E, respectively.[30] Naturally, this dampens the effects of shorting bonds that realize high returns in decile 1, the loser decile. In Panel D, the high-minus-low spread is now 0.35% with a 12-lag Newey-West t-statistic of 2.60. This is roughly the same as what is presented in JNPS who report a spread of 0.37% with a t-statistic of 3.90. (JNPS do not account for possible serial correlation in the data in the computation of the t-statistic.) In Panel E, after trimming at the +30% level, we are only able to generate a spread of 0.18% (t-statistic of 1.83), indicating that a lower threshold is needed to generate a momentum spread with our bond data. In a nutshell, the only way to observe momentum in the cross-section of corporate bond returns is to asymmetrically trim the data in an ex-post fashion by eliminating bonds that perform relatively well, thus dampening their effect if they end up in the loser decile portfolio.

To graphically illustrate the effects of the asymmetric ex-post trimming of bond excess returns, we compare the high-minus-low cumulative dollar values for the momentum spread computed with-

[30]Crucially, and sometimes misunderstood in the literature, this trimming is performed in a forward looking manner (what we refer to as ex-post), with the implication that the resultant strategy is not tradable by a potential investor ex-ante.

out (with) trimming at the 99.5^{th} return percentile with the publicly available JNPS momentum spread in Panel A (C) of Fig. 5.[31]

Figure 5 about here

In Panel A, the cumulative dollar value of our (DRR) compounded momentum spread is \$0.84, implying that \$1 invested in 1974:01 would be worth 0.84 U.S. cents in 2011:06. In contrast, at \$4.84, the cumulative dollar value of the compounded JNPS momentum spread is an order of magnitude larger In Panel B, the time series of the monthly returns appear tightly aligned, except that the drawdown during market crashes is much larger for the untrimmed DRR momentum strategy. (The correlation between the untrimmed DRR and JNPS momentum spreads is 0.66.) Turning to Panel C, we plot the compounded and asymmetrically trimmed DRR spread and the same JNPS spread from Panel A. The cumulative return curves are much more aligned, and the cumulative dollar value of our new compounded momentum spread becomes \$4.48, much closer to the \$4.84 earned by the JNPS compounded momentum spread.[32] Importantly, when comparing the time series of the monthly untrimmed (trimmed) returns in Panel B (D), the 99.5^{th} trimmed DRR momentum spread is far more aligned with the publicly available JNPS momentum spread in terms of maximum drawdown, i.e., the effect of shorting losers that generate returns above the 99.5^{th} percentile is attenuated by the trim. Although momentum has been shown to be a pervasive phenomenon in the equity literature, momentum strategies exhibit infrequent but persistent periods of negative returns. (See Daniel and Moskowitz, 2016.) These periods, denoted as momentum crashes, usually occur in extreme market environments and induce a negative skewness in momentum returns. Thus, trimming returns might mechanically remove (ex-post) these episodes where losers experience strong gains, therefore artificially inflating the overall performance of the strategies.

5.1 Momentum factors

To form a bond momentum factor, consistent with the standard approach in the literature, we first form 5×5 bivariate portfolios of credit rating and bond momentum as defined in Eq. (7).

[31] The publicly available high-minus-low momentum spread data was downloaded from Gergana Jostova's webpage.

[32] Even after trimming bond returns, we cannot generate a correlation coefficient above 0.90. But, the average high-minus-low momentum spread and cumulative return curve are much closer to those presented in JNPS.

Then, the momentum factor is the average across the high-minus-low sorts on momentum across the rating portfolios. We also employ illiquidity ($ILLIQ$) and bond maturity to construct additional momentum factors. In Table 7, we report the high-minus-low averages for momentum portfolios across the rating, $ILLIQ$, and maturity sorts and the respective momentum factor (MOM) in the last column.

Table 7 about here

The primary momentum specification is the one based on double sorts across ratings. In stark contrast to JNPS, the momentum effect appears to be absent across all rating quintiles.[33] The high-minus-low momentum spread ranges from -0.05% for bonds with strong credit ratings (Q1) to -0.08% for bonds with adverse ratings (Q5). This finding is in stark contrast with earlier work that shows that bond momentum is strongly present for bonds with poor ratings. Without asymmetrically trimming, if anything, the momentum effect is weaker for junk bonds (those in quintiles Q4 and Q5) than for investment grade bonds (Q1 and Q2). The average high-minus-low momentum spread across rating quintiles forms the rating-based MOM factor, which yields a statistically insignificant average return of -0.14% per month. This average high-minus-low spread forms the primary momentum factor specification for later analyses. The average high-minus-low momentum spreads across $ILLIQ$ and maturity quintile sorts exhibit similar patterns with associated MOM factor means of -0.04% and -0.15%, respectively. The single-factor CAPMB alphas for the rating-based MOM factor confirm the results for the average returns. The MOM factor yields a statistically insignificant alpha of -0.02%. These results hold true for $ILLIQ$- and maturity-based MOM factors.

5.2 Is bond momentum an echo?

The short answer is that it is not. Before dismissing momentum in corporate bonds, it would be prudent to try as many formation periods as possible. We follow a similar approach to Goyal and Wahal (2015) who data mine for momentum, and compute all possible corporate bond momentum strategy combinations, MOM^s, with a formation period of l months, where $l \in 1, \ldots, 12$, and

[33] This finding confirms results by Gebhardt, Hvidkjaer, and Swaminathan (2005b) for investment grade bonds, who show that for bonds rated AAA/AA, A, and BBB, the momentum spread is negative over the sample period 1973:01 to 1996:12.

a skip period of k months, where $k \in 1, \ldots, 11$ and $l > k$. This yields a total of $MOM^s = 66$ distinct corporate bond momentum strategies. For example, the strategy denoted by (12,1) is the momentum strategy with a formation period of 12 months and a skip period of 1 month as in Jegadeesh and Titman (1993), whereas the strategy denoted by (12,7) would be classified as intermediate momentum from Novy-Marx (2012). The strategy denoted by (6,1) is the one used by JNPS as presented in the previous section. For each momentum strategy combination, (l, k), we form the standard MOM risk factor by double-sorting on ratings and the momentum signal, MOM^s. The MOM factor for each momentum strategy combination is presented in the last column and is the average of the high-minus-low momentum spreads across the rating quintiles. In Panels A and B of Table 8, we report the average returns and CAPMB alphas for the five best performing momentum strategies.

Table 8 about here

Surprisingly, in Panel A, not a single momentum strategy yields a statistically significant factor mean. Of the 66 data mined momentum factors, only 6 manage to generate positive premia. The best performing strategy, $s = (6, 4)$, yields an economically negligible monthly factor mean of 0.08% (t-statistic of 0.67). On average, the high-minus-low momentum spreads are close to zero across all rating categories. In Panel B, the analysis based on the single-factor CAPMB alphas broadly confirm the results for the factor means. The (6,4) momentum factor generates an alpha of 0.19%, which is statistically indistinguishable from zero. None of the other alphas are statistically significant at conventional nominal levels.

Overall, contrary to earlier empirical evidence, corporate bond momentum does not seem to exist. Even after extensive data mining, the largest momentum factor mean is only 0.08%. Across the classical (6,1) momentum decile sorts, past losers roughly earn the same premium as past winners. Unlike the equity market, where momentum has been found to be one of the most economically and statistically robust anomalies, momentum in bonds seems to be largely missing. Furthermore, we provide robust evidence that corporate bond momentum is an artefact of asymmetrically (ex-post) trimming the distribution of bond returns, thus attenuating the performance of past losers that end up in the short leg of the portfolio.

6 Long-term reversals in corporate bonds

BSW show that the long-term reversal anomaly generates a large premium over an extended sample period. We closely follow their methodology and compute the long-term reversal signal for each bond i at time t as

$$LTREV_{i,t}^{s} = \sum_{l=t-48}^{t-13} R_{i,l},$$

where $LTREV_{i,t}^{s}$ is the 36-month cumulative return sum from month $t-48$ to $t-13$, skipping the 12-month momentum effect (from month $t-12$ to $t-1$) and the short-term reversal month (from month $t-1$ to t). Similar to bond short-term reversals and momentum, at each month t we sort bonds into deciles based on the $LTREV$ signal and compute the average excess returns over the following month. In Panel C of Table 5, we report the average excess returns and single-factor CAPMB alphas across the $LTREV^{s}$ deciles. The average high-minus-low spread (alpha) for the $(48, 13)$ decile portfolio strategy is -0.26% (-0.28%) and both estimates are statistically insignificant at all nominal level of the tests. The average return spread is much smaller than the -0.78% value reported by BSW over their TRACE-based sample period. However, unlike momentum, the spread in returns has the correct sign; past long-term losers outperform past long-term winners.

Given that BSW use an extended sample that begins in January 1977, we extend the $LTREV^{s}$ sorts back to this date and present the results in Panel C of Table 18 of Appendix B. For the extended sample, we document a spread of -0.19% (t-statistic of -1.84) and CAPMB alpha of -0.20% (t-statistic of -1.90). Confirming our results over the shorter sample, the spread is negative but smaller in absolute value and statistically insignificant. In an attempt to find a significant spread, we report the average returns across the $LTREV^{s}$-sorted portfolios in Panels A to E of Table 19 of Appendix B. Across all samples and databases, the average high-minus-low spread is small in absolute value (always less than -0.35%) and always statistically insignificant. In Panels A and B of Table 19, we have high-minus-low spreads of -0.09% and -0.14% for the sample periods 1977:01 to 1996:12 and 1977:01 to 2002:07, respectively. For Panels C through E, which use data from 1997:01 onward, the average spreads are not statistically different from zero at all nominal levels of the tests.

In a similar vein to the ex-post trimming implemented to generate a meaningful momentum spread, when we winsorize bond returns at the -10% level in Table 20, we document a large and statistically significant spread across all sample periods except for the LHM sample in Panel A. We choose the −10% level by following the June 2023 version of Bai, Bali, and Wen (2023) who implement ex-post winsorization to classify distressed bonds. Using other asymmetric cut-offs, trimming (as opposed to winsorizing) can generate similar or higher high-minus-low spreads. As for bond momentum, the presence of the long-term reversal anomaly seems to crucially depend on nonstandard asymmetric winsorization.

6.1 Long-term reversal factors

To investigate whether the $LTREV$ signal can be used to construct a set of profitable factors, we employ double sorts using ratings, illiquidity, and maturity, as in the previous section on momentum. The long-term reversal factors (LTR) are the average across the high-minus-low sorts on the $LTREV$ signals across ratings, illiquidity, or maturity. To be consistent with BSW, we use the original $LTREV^s$ specification that is based on a formation period of 48 months and a skip period of 12 months, $LTREV^s = (48, 13)$. In Table 9 below, we report the high-minus-low averages for long-term reversal portfolios across the rating, $ILLIQ$, and maturity sorts and the corresponding long-term reversal factors in the last column. We sign-correct the factors such that they are positive, i.e., they reflect the return from a long (short) position in past long-term losers (winners).

Table 9 about here

In both Panels A (average returns) and B (alphas), none of the high-minus-low $LTREV^s$-sorted spreads are statistically significant. The LTR factor across ratings yields an insignificant factor mean of 0.14% per month. Bonds with adverse credit ratings (Q4 and Q5) generate monthly premiums of 0.29% and 0.24% across the $LTREV^s$-sorted quintiles, whereas those with better ratings (Q1 and Q2) generate average returns of only 0.03% and 0.01% indicating that the effect, if any, seems to be relegated to non-investment grade debt. The average return differences across the $LTREV^s$-sorted quintiles when we first sort on either $ILLIQ$ or bond maturity do not exhibit a clear pattern, are small in magnitude, and are all statistically insignificant.

The construction of the return-based long-term reversal factor (that will be employed in subsequent analyses) closely follows BSW. First, bonds are categorized into terciles based on their credit ratings. Next, within each credit rating portfolio, bonds are further allocated to sub-terciles according to their time to maturity. Last, bonds are once again categorized into terciles based on the long-term reversal signal $LTREV^s = (48, 13)$, i.e., we consider $3 \times 3 \times 3$ sorted portfolios. Different from the previous unconditional sorts, we employ conditional tercile sorts. Therefore, for every month, LTR is computed as the value-weighted average return differential between the portfolio with the lowest $LTREV$ signal and the one with the highest $LTREV$ signal within the rating and maturity portfolios. The LTR monthly factor mean (alpha) is 0.122% (0.082%), and both estimates are statistically insignificant at conventional nominal levels. When we expand the sample to that of BSW (1977:01 to 2017:12), the LTR factor mean (alpha) is reduced to 0.081% (0.059%), both statistically insignificant at all nominal levels of the tests. The bond market factor mean is 0.295% over the same sample period. Our LTR premium is an order of magnitude smaller than that presented in BSW who report a premium of 0.47% per month with a t-statistic of 6.12. (BSW do not account for possible serial correlation in the data in the computation of the t-statistic.)

6.2 Data mining for long-term reversals

We now apply the momentum echo approach of Goyal and Wahal (2015) for constructing corporate bond long-term reversal factors. We compute all possible long-term reversal signal combinations, $LTREV^s$, with a formation period of l months, where $l \in 30, \ldots, 48$, and a skip period of k months, where $k \in 6, \ldots, 12$. This yields a total of $LTREV^s = 133$ possible corporate bond long-term reversal strategies. For example, the strategy denoted by (48,13) is the strategy considered in BSW. In Panels A and B of Table 10, we report the results for the factor means and single factor CAPMB alphas for the five best performing long-term reversal strategies.

$\boxed{\text{Table 10 about here}}$

In Panel A, none of the $LTREV^s$-sorted portfolio means across ratings and LTR factor means are significantly different from zero. Most of the strategies do, however, have the correct sign, implying that on average there is a long-term reversal effect but that it is not statistically or economically sizeable. The top performing strategy is based on a formation period of 45 months with a 6-month skip period. In Panel B, the single-factor CAPMB alphas confirm the results for the factor means, and all of the strategies are spanned by the bond market factor.

Overall, identifying a pervasive LTR factor in corporate bonds seems to be a challenging task. Even after extensive data mining that produces more than 130 possible LTR factors, none of the factors yields an economically large and statistically significant average return or single-factor CAPMB alpha. As external validation for the lack of evidence of predictive power of long-term reversals in the cross-section of corporate bond returns, we source the publicly available long-term reversal quintile portfolio data from Elkamhi, Jo, and Nozawa (2023, henceforth EJN).[34] The $LTREV$ signal of EJN is constructed exactly as described by BSW. Over the BSW sample period, the average return and CAPMB alpha from the high-minus-low spread portfolio of EJN are 0.234% per month (t-statistic of 2.00) and 0.203% (t-statistic of 1.76), respectively. Over a sample period that closely lines up with ours (2002:09 to 2020:09), the average return and alpha are 0.278% and 0.314% with t-statistics of 1.34 and 1.42, respectively. As for bond momentum, the finding of a statistically significant long-term reversal anomaly explicitly hinges on whether one (ex-post) winsorizes or trims the left tail of the return distribution at ad hoc thresholds.

7 Characteristics vs. risk

In the following analysis, we investigate whether bond short-term reversal, momentum, and long-term reversal betas (and the underlying return-based characteristics themselves) are economically and statistically related to future bond excess returns. Their failure to produce successful investment strategies may signal that these factors do not adequately capture the risk/return trade-off in the corporate bond market. To investigate whether popular bond factors proxy for systematic or idiosyncratic risk (or neither), we adopt the methodology proposed by PRS. This allows us to assess whether candidate factors are i) related to the covariance matrix of bond returns (i.e., they proxy for systematic risk); ii) carry a risk premium as compensation for systematic risk; and iii) their Sharpe ratios do not exceed reasonable pricing bounds.

7.1 PRS factor protocol first stage

To implement the canonical correlation method of PRS (factor protocol stage one), we extract the first seven principal components from a large panel of corporate bond portfolios, and we compute

[34] The data is available here.

the canonical correlations between the estimated principal components and the factor candidates.[35] For all the remaining sections, the momentum factor, MOM, is formed from the 5×5 sort on ratings and the past 6-month momentum signal, skipping the prior-month return. The short-term reversal factor, REV, is also formed from a 5×5 sort on rating and the ICE prior one-month return signal.[36] Finally, the long-term reversal factor, LTR, is formed from a $3 \times 3 \times 3$ conditional sort on ratings, maturity, and the $LTREV^s$ signal, where $LTREV^s$ is computed as the cumulative sum of the past 48 months return data, skipping the returns in the prior 12 months to account for momentum and short-term reversal. For both REV and LTR, the tradable factor returns are generated from a long (short) position in past losers (winners), implying that the factor premium should be positive if the strategy is indeed effective. The test portfolios comprise 132 portfolios from bonds sorted on credit spreads (25), size $\times$ maturity (25), $ILLIQ$ $\times$ rating (25), momentum, long- and short-term reversals (30), and value-at-risk (10). Finally, we include portfolios based on the Fama-French industry classification (17). Besides the bond market factor ($MKTB$), we consider the downside, credit, and liquidity risk-factors (DRF, CRF, and LRF, respectively) following DMR.[37]

Table 11 reports the canonical correlations computed from the covariance matrix of the 132 bond portfolio excess returns and factors (Panel A), t-statistics (Panel B), and the factor correlation matrix (Panel C).

Table 11 about here

The first canonical correlation is close to 1 and highly statistically significant, as highlighted by the corresponding z-statistic, and we find that the first six canonical correlations are statistically significant. Following PRS, we compute the average t-statistic from regressing each canonical variate on the proposed factors as a proxy for the significance of the factor candidates, first using all the canonical variates (first row of Panel B) and then conditional on the canonical correlation being significant (second row of Panel B).

We find that for all factors, the average t-statistics exceed the 1.96 value and therefore they signal significance at the 2.5% confidence level.[38] By naively examining the average t-statistics, $MKTB$ is

[35] The number of principal components corresponds to the numbers of factors we subject to scrutiny (seven).

[36] We no longer consider the unadjusted REV factor because it almost exclusively captures market microstructure noise, as shown in the previous sections.

[37] These factors stem from work by Bai, Bali, and Wen (2019, henceforth BBW).

[38] As emphasized by PRS, the estimated coefficients in the regression of the canonical variates on the factor candi-

clearly dominant with t-statistics greater than 30. For the other return-based factors, REV, LTR, and MOM, the t-statistics are one order of magnitude smaller. Overall, all of the factors pass the first stage of the PRS protocol. This merely implies that the factors are statistically related to the covariance of the 132 bond portfolio excess returns in a meaningful way and can be considered potential risk factors. Turning to Panel C, the correlations between the return-based factors and the remaining factors are low and, in some cases, negative. Some of the BBW factors, such as DRF and LRF, exhibit substantial correlations with the bond market factor, thus suggesting that they may be spanned in a single-factor CAPMB specification.

To determine whether the return-based factors significantly add to the bond market factor, we augment the first stage of the PRS protocol with a squared Sharpe ratio analysis. We compute bias-adjusted squared Sharpe ratios for individual factors, for the return-based factor model (RFM, which combines REV, LTR, and MOM), and for the BBW four-factor model with and without the RFM factors. In Table 12, we report the factor means, single-factor CAPMB alphas, volatilities, and the bias-adjusted squared Sharpe ratios. The associated p-values are in square brackets.

Table 12 about here

None of the return-based factor means are significantly different from zero at the 5% nominal level. The factor means for the market, downside, credit, and liquidity factors are similar to those reported in DMR. Next, we use the $MKTB$ factor to compute the one-factor alphas across the remaining factors. All of the factor alphas are close to zero or negative and statistically indistinguishable from zero. The largest CAPMB alpha is 14 basis points per month (for CRF). All of the bias-adjusted squared Sharpe ratios for the return-based factors are either negative or positive but close to zero.

In Panel B, we conduct pairwise model comparison tests based on squared Sharpe ratios following Barillas, Kan, Robotti, and Shanken (2020), who focus on a comparison of models' maximum squared Sharpe ratios in an asymptotic analysis under general distributional assumptions. The differences in bias-adjusted squared Sharpe ratios with their associated p-values are presented in Panel B of Table 12. The bias-adjusted squared Sharpe ratio of CAPMB (the single-factor model with the $MKTB$ factor) is higher than that of any other factor or model combination. Even

dates represent the square roots of the eigenvalues, and they are therefore always positive. The one-tailed cutoffs are hence employed.

after combining the three return-based factors with the BBW model, the CAPMB bias-adjusted squared Sharpe ratio is higher. This evidence suggests that the inclusion of return-based bond factors/anomalies does not lead to an improvement in the risk-return tradeoff of an investor's portfolio. In the following analyses, we dig deeper into the pricing ability of the various factors and also investigate whether the underlying characteristics (that are employed in factor construction) are important drivers of excess bond returns in the cross-section.

7.2 PRS factor protocol second stage

The second step of the PRS protocol encompasses the ability of the candidate factors to price the cross-section of bond returns. To this end, we estimate the risk premia on candidate factors based on two different methodologies. First, we estimate the risk premia on candidate factors using standard two-pass Fama-MacBeth regressions at the bond level. Second, we estimate risk premia based on the three-pass regression framework of Giglio and Xiu (2021) and the 132 bond portfolios as basis assets.

7.2.1 Bond-level Fama-MacBeth regressions

Using bond-level data, we run time-series regressions of excess bond returns on the seven factor candidates and collect the estimated beta coefficients. We follow PRS and estimate the betas for each bond in the sample using all available information for that bond,

$$r_{i,t} = \alpha_i + \boldsymbol{\beta}_i \boldsymbol{f}_t + \epsilon_{i,t}, \tag{8}$$

where $r_{i,t}$ denotes the excess return on bond i at time t, $\boldsymbol{f}_t$ denotes the considered time-t K-vector of factors, and $\boldsymbol{\beta}_i$ is the matrix of factor betas. For a bond to be included in the sample, we require a minimum of 24 monthly observations. This procedure yields a set of seven unconditional factor betas for each bond in the sample.[39] Then, for each month, we run a cross-sectional regression of bond excess returns on the estimated factor betas with and without characteristics,

$$r_{i,t+1} = \gamma_{0,t} + \hat{\boldsymbol{\beta}}_i \boldsymbol{\gamma}_t + \boldsymbol{z}_{i,t} \boldsymbol{\lambda}_t + \epsilon_{i,t+1}, \tag{9}$$

[39] This approach reduces the attenuation bias in the factor betas induced from using fixed rolling windows and delivers more granular betas (at the bond level) than the post-formation betas based on portfolio sorts. However, using rolling betas over a 24-month period or post-formation betas delivers results that are qualitatively similar to those we report.

where $r_{i,t+1}$ is the excess return on bond i at time $t+1$, $\boldsymbol{z}_{i,t}$ is a time-t vector of bond characteristics used to form the underlying factors, and $\hat{\boldsymbol{\beta}}_i$ is the matrix of estimated factor betas. The time-series averages of the estimated cross-sectional coefficients $\hat{\boldsymbol{\gamma}}_t$ and $\hat{\boldsymbol{\lambda}}_t$ denote the risk and characteristic premia, respectively.

Controlling for the underlying characteristics that are used to form the considered factor candidates allows us to run a horse race between factor betas and the characteristics in the spirit of Daniel and Titman (1997). The included characteristics encompass those associated with the return-based factors ($STREV$, $LTREV^s$, and MOM^s) and those associated with the downside, credit, and liquidity factors (bond ratings, *Rating*, value-at-risk, VaR, and illiquidity, $ILLIQ$).[40] Table 13 reports the time-series averages of the estimated cross-sectional coefficients, along with associated sampling statistics.

$\boxed{\text{Table 13 about here}}$

In models 1 and 2, we present the results for the factor premia associated with the seven factors by first excluding and then including the underlying characteristics (leaving the long-term reversal characteristic out of the analysis in model 2). In model 3, we also include the long-term reversal characteristic. For this specification, the sample is reduced by close to 40% because of the long rolling period required to compute $LTREV^s$.[41] Finally, in model 4, we include the estimated factor betas and the characteristics that have been found to be statistically significant at the 5% nominal level based on model 3. In model 1, none of the return-based factors generate sizeable and statistically significant risk premia, with the estimated coefficients being close to zero. Similarly, after including the underlying characteristics in model 2, none of the factors generate any risk premia. The only characteristic that seems to matter is *Rating*, which is statistically significant at the 1% level. In model 3, after including $LTREV^s$ and losing close to 40% of the sample, the results largely confirm those for model 2. Finally, in model 4, after excluding all characteristics except for *Rating*, the only characteristic or risk exposure that is economically and statistically associated with one-month ahead bond excess returns are bond credit ratings. Overall, neither the return-based factor risk premia nor the premia on their underlying characteristics (except for bond

[40] We only winsorize $ILLIQ$ at the 1% level because the series contains some extreme outliers.

[41] The median maturity of the bonds in our sample is 60 months and the $LTREV$ signal requires a rolling period of 48 months.

rating) are sizeable and statistically significant at conventional nominal levels of the tests.

7.2.2 Three-pass regression framework of Giglio and Xiu (2021)

In Table 14, we report risk premium estimates and associated t-statistics for the seven candidate factors when three, five, and seven principal components are extracted from our panel of 132 portfolio returns.[42]

Table 14 about here

The estimated risk premia at the portfolio level are broadly consistent with the premia estimated at a bond level in the previous table. All of the return-based factors yield economically small and statistically insignificant risk premia. The $MKTB$, CRF, and DRF candidate factors yield economically large risk premia estimates but are statistically insignificant. We do not find support for the liquidity factor being priced.[43] Both the magnitudes and the levels of statistical significance are strongly aligned regardless of the number of principal components used to estimate the risk premia.

Overall, the only two factor candidates that consistently command a risk premium that is economically (although not statistically) significant at the portfolio and bond level (without including any characteristics) are the $MKTB$ and DRF factors. However, given that the DRF factor is arguably a more levered version of $MKTB$ (the correlation between the two factors is close to 90%), the incremental pricing ability of DRF over $MKTB$ is very weak.[44] The main message from this analysis is that none of the return-based factor betas nor return-based characteristics are useful in explaining the cross-section of excess bond returns.

[42] The number of latent factors based on the estimator proposed by Giglio and Xiu (2021) is found to be equal to three. We also include results for five and seven latent factors as a robustness check.

[43] These univariate results confirm the mean return analysis in Table 12. As emphasized by PRS, if one factor is similar to another factor, it is of interest to determine which of them carries stronger evidence of being priced, and this is best achieved in a multiple regression.

[44] In DMR, the price of covariance risk associated with DRF is shown to be negative and insignificantly different from zero.

7.3 PRS factor protocol third stage

To further explore whether the various factors/characteristics are priced and generate Sharpe ratios that are within reasonable magnitudes, we now form hedge portfolios by sorting bonds on the rolling 24-month beta exposures to each factor.[45] For each of the seven factors and for each bond in the sample, we estimate the univariate rolling regression $r_{i,t} = \alpha_{i,t} + \beta_{i,t} f_t + \epsilon_{i,t}$, where f_t represents, in turn, each of the seven factor candidates. We then create decile and quintile sorts based on the factor betas and create a high-minus-low hedge portfolio, which is long (short) bonds with the highest (lowest) factor exposures. The portfolios are value-weighted using bond amount outstanding. In Table 15, we report the results in Panels A and B for deciles and C and D for quintiles.

Table 15 about here

Once again, the high-minus-low spreads for the sorts on the univariate exposures to the return-based factors are economically small and statistically insignificant. Confirming the important role of the bond market factor, the high-minus-low spread from sorting on the $MKTB$ beta is 0.62% per month with a t-statistic of 1.93 in Panel A. The high-minus-low decile spreads from sorting on the DRF and LRF univariate factor betas are the only other spreads that are statistically significant at the 10% level. The spread for CRF is not statistically different from zero at conventional significance levels.

The last step of the PRS factor protocol checks whether the Sharpe ratios achieved by the proposed factors that pass the previous stages do not exceed a threshold that is consistent with risk pricing. We first augment this step by simply computing the single-factor CAPMB alphas and investigate whether any of the high-minus-low spreads are consistent with a CAPM-style risk model. Across all of the high-minus-low spreads, none of them have abnormal average returns (the single-factor CAPMB alphas) that are statistically different from zero, implying that the CAPM explains away the high-minus-low spreads across factors. Next, following PRS, for each factor candidate, we combine its hedge portfolio returns with the market factor, $MKTB$, and test whether the annualized Sharpe ratio of this investment strategy is within the chosen threshold of 0.80.[46]

[45] We estimate a rolling regression with a minimum required number of observations equalling 24, which expands up to 36 and then rolls forward. See Binsbergen, Nozawa, and Schwert (2023).

[46] Following MacKinlay (1995), PRS argue that the annualized Sharpe ratio of the combined portfolio should not

Panel B of Table 15 presents the means, standard deviations (SD), and annualized Sharpe ratios for investment strategies than combine $MKTB$ with the high-minus-low spread portfolios from Panel A. We report annualized Sharpe ratios (SR) of $MKTB$ augmented the high-minus-low spread portfolio and the t-statistic associated with the null of $H_0 : SR = 0.80$ under the assumption of i.i.d. returns following Lo (2002). We find that all factor candidates that successfully pass the previous stages achieve a risk-reward ratio that is consistent with risk pricing (i.e., the Sharpe ratio does not exceed the 0.80 threshold). For the return-based factors, REV, LTR, and MOM, the annualized Sharpe ratios are significantly smaller than the 0.80 threshold. This confirms the results for the differences between the squared Sharpe ratios for these factors and that for CAPMB presented in Table 12.

We repeat the same analysis in Panels C and D, the only difference being in how hedge portfolios are constructed. We now form portfolios based on the top/bottom quintiles of corporate bonds sorted on factors' betas. The results are qualitatively similar to those discussed previously. None of the return-based hedge portfolios deliver an average (risk-adjusted) return that is statistically different from zero, and, again, the combined investments in the market and the hedge portfolios of $MKTB$ and LRF achieve Sharpe ratios in line with risk pricing. CAPMB also explains the spread across quintile portfolios.

We now turn to decile and quintile portfolios sorted on the underlying characteristics that are used to form the seven factors and report the results in Table 16. These characteristics comprise the short-term reversal, $STREV$, long-term reversal, $LTREV^s$, momentum, MOM^s, 95% historical value-at-risk, VaR, and illiquidity, $ILLIQ$. For each characteristic, we form value-weighted decile and quintile portfolios. We then compute the average return to the high-minus-low portfolio which is long (short) the portfolio with the highest (lowest) characteristic. Panels A and B report results for deciles while Panels C and D are for quintiles.

Table 16 about here

All return-based characteristics ($STREV$, MOM^s, and $LTREV^s$) yield statistically insignificant

exceed 0.60. This is also the bound used by Chordia, Goyal, Nozawa, Subrahmanyam, and Tong (2017) in their analyses. We argue that for corporate bonds, the threshold should be higher and therefore we choose a bound of 0.80 in the tests presented in this section. See Dickerson, Fournier, Jeanneret, and Mueller (2022) for both theoretical and empirical evidence on why corporate bond Sharpe ratios exceed those of stocks.

high-minus-low spreads. The negative MOM^s spread indicates that past losers outperform winners by 1 basis point per month. The VaR characteristic generates a spread that is large (0.57%) but not statistically significant at the conventional nominal levels of the tests.

For the reversal-based decile and quintile sorts in Panels B and D, we multiply their average high-minus-low spread by minus one (losers are meant to outperform winners), such that the expected return on these strategies should be positive and aligned with that of $MKTB$. In terms of Sharpe ratios, none of the characteristic-sorted portfolios, once combined with $MKTB$, significantly exceed the 0.80 cutoff. Since the average return for the decile high-minus-low spread on $STREV$ is close to zero in magnitude, the $MKTB$-augmented strategy yields a return that is the same as simply holding the bond market portfolio. The same holds true for MOM^s, where the strategy of buying winners and shorting losers generates a hedge portfolio return of -0.01%. Once combined with $MKTB$, the associated Sharpe ratios are systematically smaller than the 0.80 threshold. Similar conclusions are drawn when hedge portfolios are constructed using quintiles as in Panels C and D.

7.4 Robustness checks

Holcblat, Lioui, and Weber (2023) propose a testing methodology to formally assess whether a given factor candidate can be considered a valid risk factor. Given the short and long legs of a proposed investment strategy, if there exists at least one risk-averse individual characterized by a concave and increasing von Neumann-Morgenstern utility function who is willing to forsake the higher return from the long leg in favor of the lower but less risky return from the short leg, then this implies that the factor's expected return is a possible compensation for the higher risk of the long leg with respect to the short leg. Based on their proposed statistical test, a rejection of the null hypothesis of strict preference for the long portfolio leg signals that the spread in expected returns is a form of compensation for risk. In this particular context, an anomaly is defined by the authors as a "deviation from the risk-return tradeoff." Due to its appealing properties, which notably include the absence of parametric assumptions on return distributions and agents' utility functions, as well as the avoidance of issues related to multiple hypothesis testing and pre-testing biases, we consider their unconditional test to determine whether popular factors in corporate bond pricing command a nonzero average return as a compensation for bearing systematic risk.[47]

[47]It is important to note that the rejection of the null hypothesis could potentially be affected by the lack of

The null hypothesis is given by

$$H_0 : \forall u \in \mathbb{U}_2, \mathbb{E}[u(R_S)] < \mathbb{E}[u(R_L)], \tag{10}$$

where $\mathbb{U}_2$ indicates the set of all concave and increasing functions, and R_L and R_S denote the gross returns of the long and short portfolios, respectively. Holcblat, Lioui, and Weber (2023) show that testing Eq. 10 is equivalent to testing whether R_L strongly second order stochastically dominates R_S. Table 17 reports the average returns for both the long and short legs of the factors (either the factors themselves or the hedge portfolios constructed in the final stage of the PRS protocol) along with the associated p-values from the unconditional test of Holcblat, Lioui, and Weber (2023).

Table 17 about here

In this context, a rejection of the null hypothesis suggests that the candidate factor may be considered a true risk factor, thereby implying that the positive returns generated from the investment strategies can be attributed to compensation for systematic risk. If the strategy under investigation yields a negative average return, we do not perform the test as a negative return spread implies that the short position outperforms its long counterpart. Testing whether individuals favor the long position over the short one in such instances would invariably result in the rejection of the null, even if this preference does not pertain to risk or any other related explanation.

Panel A of Table 17 shows that for $MKTB$, DRF, and LRF, we can reject the null hypothesis at the 5% nominal level. These results are fairly consistent with our earlier findings for the factor squared Sharpe ratios presented in Table 12, thus suggesting that these factors could indeed be regarded as potential risk factors. Once again, when examining the hedge portfolios created through univariate sorts on risk exposures (as shown in Panels B and D), the same conclusions hold. Conversely, we fail to reject the null hypothesis for CRF. The lack of rejection of the null for these factors, coupled with their statistically insignificant spreads (Panels A and C of Table 15), suggests that these candidate factors may not represent true anomalies or risk factors. For quintile and decile hedge portfolios sorted on characteristics, our results indicate that only VaR exhibits some potential as a risk factor at the 5% nominal level for both decile and quintile sorts. When

significance in return spreads. Since several investment strategies analyzed in our study exhibit either statistically insignificant or only marginally significant return spreads, a rejection of the null in this context does not necessarily imply that the candidate factor impacts expected returns due to its association with systematic risk.

considering the $ILLIQ$ characteristic, the null hypothesis is rejected at the 5% significance level for quintile sorted portfolios, but this rejection does not hold when bonds are sorted into deciles. Additionally, for the *Rating* characteristic, while we observe a rejection at the 10% significance level (p-value of 0.08), the spread in Table 16 is relatively small and its t-statistic is 1.20, which is not supportive of this characteristic being a potential risk factor. In summary, the results from the unconditional test of Holcblat, Lioui, and Weber (2023) confirm and reinforce our earlier findings.

8 Conclusion

Corporate bond pricing has consistently relied on very similar techniques and assumptions that were found to be appropriate and successful for equities. This approach, whilst enticing, should not have been followed. Given the relatively short sample periods and the relevant liquidity issues that arise from the over-the-counter bond trading, corporate bonds provide a unique platform to study characteristics and risks. The excessive borrowing from the equity literature has led to a series of questionable results. In this article, we provide evidence that none of the previously documented return-based corporate bond anomalies earn significant spreads nor do they help explain the cross-section of corporate bond returns.

In our analysis, we carefully quantify the level of market microstructure noise inherent in the transaction-based TRACE data and its effect in distorting the bond short-term reversal signal. As a possible solution, we correct the short-term reversal signal for the presence of measurement error in transaction-based prices/returns so that researchers do not have to purchase expensive quote-based data from Intercontinental Exchange. After adjusting bond signals for market microstructure noise, bonds, if anything, exhibit a short-term momentum effect as opposed to a short-term reversal.

Corporate bond momentum and long-term reversal signals have both been documented as extremely strong predictors of future bond returns in prior empirical investigations. Both have been used to form factors with apparently large mean returns that are not spanned by most of the existing risk factors. We comprehensively show that the momentum and long-term reversal bond anomalies hinge entirely on asymmetric (ex post) trimming/winsorizing of the distribution of corporate bond returns. Finally, in implementing and augmenting the Pukthuanthong, Roll, and Subrahmanyam (2019) factor protocol, we show that the considered return-based bond anomalies are not priced risk factors and that their underlying signals (characteristics) are not related to average excess bond returns. Trying to identify characteristics/factors that drive the cross-section of corporate bond returns is a fruitful avenue for future research. Recognising the unique market structure of corporate bonds is the first and essential step in this direction, and our hope is that our paper can lay the groundwork for this process.

Appendix A

Corporate bond databases

In Appendix A, we describe the five databases that we employ in the paper. Across all databases, we filter out bonds that have a time to maturity of less than 1 year. Furthermore, for consistency across databases, we define bond ratings as those provided by Standard & Poor's (S&P). We include the full spectrum of ratings (AAA to D), but we exclude bonds that are unrated. For each database that we consider, we do not winsorize or trim bond returns in any way.

WRDS bond database

The Wharton Research Data Services (WRDS) bond database is a pre-processed monthly bond database based on the Enhanced Trade Reporting and Compliance Engine (TRACE) data and the Mergent Fixed Income Securities Database (FISD). It was introduced by WRDS in April 2017. The data is publicly available (requires a valid subscription to WRDS). After logging into WRDS, the data is available here. We use the version of the WRDS database that has a sample end date of September 2022.

WRDS bond returns. The WRDS data team provides us with three different bond return variables: `RET_EOM` (returns are computed using bond prices that land on any day of the month), `RET_L5M` (a bond must trade on the last five days of the month), and `RET_LDM` (a bond must trade on the last day of the month). For the results based on the WRDS bond database, we always use `RET_L5M`, i.e., a return is valid if the bond trades on the last five days of month t and month $t-1$. However, the publicly available data we use from WRDS, imposes a data filter that sets any bond return that is greater than 100% equal to 100%, i.e., the data is truncated/trimmed at this level. Although this does not make any material difference to our results, we carefully address the issue below.

WRDS bond returns truncation correction. We carefully adjust for the truncation of bonds returns greater than +100% imposed by WRDS by setting any bond return that is truncated to the return observed in the Intercontinental Exchange (ICE) database, i.e., if the truncated WRDS bond return is equal to 100%, we set this value equal to the bond return from ICE as the true bond return. If the ICE return is missing, we set the value equal to the return computed from the TRACE data itself. In total, we identify only 94 cases where the truncation occurs, and we are able to address 91 of them. The remaining 3 cases are removed.

WRDS bond filters. To align the data to the Bank of America Merrill Lynch (BAML) corporate bond database provided by ICE, we follow Andreani, Palhares, and Richardson (2023) and use the following filters (all using data provided by WRDS):

1. Remove investment grade (IG) rated bonds that have less than USD 150 million outstanding prior to, and including, November 2004, and less than USD 250 million after November 2004.
2. Remove non-investment grade (HY) rated bonds that have less than USD 100 million outstanding prior to, and including, September 2016, and less than USD 250 million after September 2016.
3. Remove bonds that are classified as zero-coupon, `bond_type == 'CMTZ'`.
4. Remove bonds that are classified as convertible, `conv == 'N'`.

We merge the WRDS data with FISD (also publicly available via the WRDS data platform) and apply the following filters that are all standard in the literature:

1. Only keep bonds that are issued by firms domiciled in the United States of America, `COUNTRY_DOMICILE == 'USA'`.
2. Remove bonds that are private placements, `PRIVATE_PLACEMENT == 'N'`.
3. Only keep bonds that are traded in U.S. Dollars, `FOREIGN_CURRENCY == 'N'`.
4. Bonds that trade under the 144A Rule are discarded, `RULE_144A == 'N'`.
5. Remove all asset-backed bonds, `ASSET_BACKED == 'N'`.
6. Remove convertible bonds, `CONVERTIBLE == 'N'`.

7. Only keep bonds with a fixed or zero coupon payment structure, i.e., remove bonds with a floating (variable) coupon, `COUPON_TYPE != 'V'`.

8. Remove bonds that are equity linked, agency-backed, U.S. Government, and mortgage-backed, based on their `BOND_TYPE`.

9. Remove bonds that have a "nonstandard" interest payment structure or bonds not caught by the variable coupon filter (`COUPON_TYPE`). This affects a tiny fraction of bonds ($\sim 0.10\%$ or 142 bonds) of the FISD data file. We remove bonds that have an `INTEREST_FREQUENCY` equal to -1 (N/A), 13 (Variable Coupon), 14 (Bi-Monthly), and 15 and 16 (undocumented by FISD). Additional information on `INTEREST_FREQUENCY` is available on Page 60 of 67 of the FISD Data Dictionary 2012 document.

10. Remove a small fraction of bonds that do not have the required (and crucial information) to compute accrued interest. Bonds that do not have a valid `DATED_DATE` are removed (3,051 bonds). The `DATED_DATE` variable is the date from which bond interest accrues. Bonds without a valid `INTEREST_FREQUENCY`, `DAY_COUNT_BASIS`, `OFFERING_DATE`, `COUPON_TYPE`, and `COUPON` are also removed (425 bonds in total).

For bonds with missing amount outstanding information in the file, we set the amount outstanding equal to the face value at issuance.

Enhanced TRACE (TRACE) data

TRACE provides intraday bond clean prices, trading volumes, and buy-and-sell indicators. We apply the standard bond filtering procedure used by Binsbergen, Nozawa, and Schwert (2023).

TRACE bond filters. We apply the following filters in cleaning the intraday TRACE data for the pre-2012 database. (For detailed descriptions of the TRACE data changes, see Dick-Nielsen, 2014.)

1. Keep all trades that have less than two days to settlement, `days_to_sttl_ct == '002'`, `days_to_sttl_ct == '001'`, `days_to_sttl_ct == '000'` or `days_to_sttl_ct == 'None'`.

2. Remove trade records with the 'when-issued' indicator, `wis_fl != 'Y'`.

3. Remove trade records with the 'locked-in' indicator, `lckd_in_ind != 'Y'`.

4. Keep trade records which do not have special conditions, `sale_cndtn_cd == 'None'` or `sale_cndtn_cd == '@'`.

Thereafter, we clean the bond trades for reversals, corrections, and cancellations in the standard manner as prescribed by Dick-Nielsen (2009) and Dick-Nielsen (2014). The end-of-day bond clean price is the volume-weighted price of all eligible trades within each day d of month t that register par volume equal to or greater than \$10,000, `entrd_vol_qt` $\geq$ `$10,000`. We are careful to not filter out bond trades (`rptd_pr`) that have a price of less than \$5 or greater than \$1,000. Imposing this filter introduces look-ahead bias into the out-of-sample portfolio results based on monthly data.[48]

Bank of America Merrill Lynch (BAML) database

The BAML data is provided by ICE and contains daily bond price quotes, accrued interest, and a host of pre-computed corporate bond characteristics such as the bond option-adjusted credit spread (OAS), the asset swap spread, duration, convexity, and bond returns in excess of a portfolio of duration-matched Treasuries. The ICE sample spans the time period 1997:01 to 2022:12 and includes constituent bonds from the ICE Bank of America High Yield (H0A0) and Investment Grade (C0A0) Corporate Bond Indices. To align the ICE sample with the WRDS sample, we use a sample end date of September 2022.

ICE bond filters. We follow Binsbergen, Nozawa, and Schwert (2023) and take the last quote of each month to form the bond-month panel. We then merge the ICE data with FISD, which has had the same filters applied to it as discussed above. The following ICE-specific filters are then applied:

1. Only include corporate bonds, `Ind_Lvl_1 == 'corporate'`.

2. Only include bonds issued by U.S. firms, `Country == 'US'`.

3. Only include corporate bonds denominated in U.S. Dollars, `Currency == 'USD'`.

[48]We thank Avanidhar Subrahmanyam for pointing this out to us.

ICE bond returns. Total bond returns are computed in a standard manner in ICE, and no assumptions about the timing of the last trading day of the month are made because the data is quote based, i.e., there is always a valid quote at month-end to compute a bond return. This means that each bond return is computed using a price quote at exactly the end of the month, each and every month. This introduces homogeneity into the bond returns because prices are sampled at exactly the same time each month. ICE only provides bid-side pricing, meaning bid-ask bias is inherently not present in the monthly sampled prices, returns, and credit spreads. The monthly ICE return variable (as denoted in the original database) is `trr_mtd_loc`, which is the month-to-date return on the last business day of month t.

Lehman Brothers (LHM) database

The Lehman Brothers bond database contains monthly price data for corporate (and other) bonds starting from January 1973 until December 1997. The database categorizes the prices as either quote or matrix prices and identifies whether the bonds are callable or not. However, as per the findings of Chordia, Goyal, Nozawa, Subrahmanyam, and Tong (2017), the difference between quote and matrix prices or callable and non-callable bonds does not have a material impact on cross-sectional return predictability. Hence, we include both types of observations. This is consistent with both Jostova, Nikolova, Philipov, and Stahel (2013) (momentum) and Bali, Subrahmanyam, and Wen (2021) (long-term reversals) who also use the LHM database. In addition, the LHM data provides key bond details such as the amount outstanding, credit rating, offering date, and maturity date. For the extended dataset, we use the LHM data from 1973:01 to 1996:12.

LHM filters. As for the other databases, we merge the LHM data with the pre-filtered FISD and then apply the following LHM-specific filtering guidance provided by Elkamhi, Jo, and Nozawa (2023):

1. Only include corporate bonds classified as 'industrial', 'telephone utility', 'electric utility', 'utility (other)', and 'finance', as per the LHM industry classification system, `icode ==` {`3 | 4 | 5 | 6 | 7`}.

2. Remove the following dates for which there are no observations or valid return data, `date ==`

`{1975-08 | 1975-09 | 1984-12 | 1985-01}`.

We also follow Bessembinder, Kahle, Maxwell, and Xu (2008) and Chordia, Goyal, Nozawa, Subrahmanyam, and Tong (2017) and apply the following filters to the LHM data to account for potential data errors:

1. Remove observations with large return reversals, defined as a 20% or greater return followed by a 20% or greater return of the opposite sign.

2. Remove observations if the prices appear to bounce back in an extreme fashion relative to preceding days. Denote R_t as the month t return, we exclude an observation at month t if $R_t \times R_{t-k} < -0.02$ for $k = 1, \ldots, 12$.

3. Remove observations if prices do not change for more than three months, i.e., $\frac{P_t}{P_{t-3}} - 1$ `!= 0`, where P is the quoted or matrix price.

LHM returns. The LHM bond database includes corporate bond returns that have been pre-computed. The accuracy of the LHM return computation has been verified empirically by Elkamhi, Jo, and Nozawa (2023).

Combined data

To affirm the main results, we rely on an extended dataset that combines the LHM and ICE datasets over the sample period 1973:01 to 2022:09. The data is spliced together as follows:

1. From 1973:01–1996:12 we use the LHM data.

2. From 1997:01–2022:09 we use the ICE data.

Appendix B

Results with extended data

The main results presented in several of the papers that we consider (including those by Jostova, Nikolova, Philipov, and Stahel, 2013 and Bali, Subrahmanyam, and Wen, 2021) use an extended sample that includes the LHM database. To show that our findings are not materially affected by extending the sample, we replicate several of our main results related to short- and long-term reversals and momentum with data from 1977:01 to 2022:09.

Portfolio sorts

In Table 18, we present average decile portfolio returns and the high-minus-low spreads associated with MMN-adjusted short-term reversals (Panel A), momentum (Panel B), and long-term reversals (Panel C).

Table 18 about here

In both Panels A and B, we confirm the results based on the TRACE-only sample. The average high-minus-low spread for short-term reversals is −0.04% per month, which is insignificantly different from zero at all nominal levels. The CAPMB alpha is practically zero (0.00% per month).

In Panel B, the average high-minus-low momentum (6,1) spread is −0.01%, thus indicating that with the extended sample past bond losers continue to outperform past bond winners. In Panel C, we show that with the extended sample there is a meaningful high-minus-low spread in the long-term reversal decile portfolios. Past long-term losers earn 0.46% per month compared to past long-term winners that earn 0.27%. However, this results in an average Q10-Q1 spread of −0.19% that is statistically insignificant at the 5% nominal level of the test. The resultant CAPMB alpha on this latter spread is practically unchanged at −0.20% and is still insignificant at the 5% level.

Digging into long-term reversals across different samples/databases

We revisit the long-term reversal analyses in the main text by forming decile sorts across different sample periods and databases. We report the results in Table 19.

Table 19 about here

Overall, we find no evidence of long-term reversals across time and databases. In Panels A and B, we find average high-minus-low long-term reversal spreads of −0.09% and −0.14% (both statistically insignificant) for data up until 1996:12 and 2002:07, respectively. The average high-minus-low spreads for the WRDS data (Panel C), the ICE sample that aligns with WRDS (Panel D), and the full ICE sample (Panel E) are −0.31%, −0.25%, and −0.27%, respectively, and all of them are not statistically different from zero at the conventional nominal levels of the tests.

BSW report an average high-minus-low quintile spread of −0.78% per month (t-statistic of −2.89) over their 2002:07 to 2017:12 sample period, which is more than two times as large than what we observe over a similar sample period based on the WRDS data. To ensure that our results are not driven by the quantile choice (we use deciles) or the sample end date (our sample ends in 2022:09), we redo the portfolio sorts using quintiles with the TRACE data as processed by WRDS and restrict the sample to 2002:07–2017:12. At −0.27% (t-statistic of −1.28), the resultant average quintile spread is found to be even lower than before and about three times smaller than the BSW spread of −0.78%. The associated CAPMB alpha is −0.04% (t-statistic of −0.32), an order of magnitude smaller than the BSW 11-factor alpha of −0.67% (t-statistic of −2.99).

This said, significant high-minus-low spreads in long-term reversal decile portfolios can be found if we asymmetrically (ex-post) winsorize excess bond returns that are smaller than −10%. We report the results in Table 20.

Table 20 about here

As one would expect, ex-post winsorizing very negative returns generates statistically significant spreads across all databases and sample periods, except for Panel A that employs the LHM data from 1977:01 to 1996:12. In Panel C, the average high-minus-low spread for the WRDS data almost doubles (in absolute value) from −0.31% to −0.51% per month (t-statistic of −2.48). We observe a similar jump in the ICE data in Panel D over the same 2002:08–2022:09 sample period. Using quintile sorts with winsorized returns over the BSW 2002:07–2017:12 sample period generates an average high-minus-low spread of −0.46% (t-statistic of −1.96) and CAPMB alpha of −0.28% (t-statistic of −1.79).

Appendix C

Effects of market microstructure noise on price-based characteristics

To illustrate the effects of using bond price-based characteristics that have not been adjusted for MMN, we employ an augmented procedure from Bartram, Grinblatt, and Nozawa (2021). Fig. 6 shows an hypothetical example of how daily bond prices are used to construct bond price-based signals for month t that are purged of MMN.

Figure 6 about here

For any month t, an investor sources the daily bond price at least one trading day d before the end-of-the-month transaction price P_t. This price, denoted by $p_{t,d-1}$, is used to compute any price-based signal, i.e., credit spreads or bond yields. The trader observes the signal and purchases the given bond at price P_t, and she will realize a return over the following month of $\frac{P_{t+1}}{P_t} - 1$. The end-of-the-month transaction prices P_t must be within the last five business days of the month. This ensures that the signal prices $p_{t,d-1}$ are never included in the realized out-of-sample bond returns that use the bond prices P_t in the last five business days of month t.

In Table 21, we report results for the CAPMB alphas across decile portfolios sorted on bond yield (Panel A), credit spread (Panel B), the 6-month change in the log of bond credit spreads (see Kelly, Palhares, and Pruitt, 2023 and Kelly and Pruitt, 2022, Panel C), and bond price (Panel D).

Table 21 about here

Across all panels, we denote the alphas from using the unadjusted (MMN-adjusted) characteristics by TRACE (TRACE*). The alphas for the portfolios formed with the ICE data are denoted by ICE. Given that noise in bond prices can also affect the portfolio weights, we weight the TRACE (TRACE*) portfolios by bond market capitalization with unadjusted (adjusted for MMN) prices.

For all of the decile sorts, the Q10-Q1 CAPMB alphas are statistically significant at the 10% (5%) nominal levels in Panels A and B (C and D) when the noisy TRACE signals are used. On the other-hand, the denoised TRACE* and ICE-based sorts are much more aligned, implying that our correction procedure does a reasonably good job at aligning the transaction-based TRACE data to that of the industry-grade ICE data. Except for the sort on the six-month change in log credit spreads for ICE, none of the price-based CAPMB alphas are statistically significant at the 5% nominal level once we adjust for MMN. Furthermore, by examining the alphas in the lowest and highest deciles, the CAPMB alphas for the TRACE* and ICE data are very similar across all panels.

Going from the noisy TRACE to the MMN-adjusted TRACE*, the alphas on the average Q10-Q1 spreads decrease by 43% for bond yields, 41% for credit spreads, 64% for the six-month change in log credit spreads, and 37% for bond prices. Clearly, the impact of MMN on bond price-based signals is pervasive and can be potentially mitigated by the MMN-reduction procedure we prescribe along with prior work by Bartram, Grinblatt, and Nozawa (2021).

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Table 1: TRACE bid-ask bias across time. The table presents the time-series averages of the cross-sectional means of the daily bid-ask bias, return, bid-ask spread, and credit spread in basis points (bps) for the full sample period and various sub-samples based on post financial crisis regulations as specified in Wu (2022). The bid-ask bias is defined as $\sigma^2[\delta_i] = [(\bar{P}_A - \bar{P}_B)/(\bar{P}_A + \bar{P}_B)]^2$ based on Eq. (4). The bid-ask spread is defined as the difference between the daily volume-weighted average bid and ask prices scaled by their average. The daily return is computed using the clean (transaction) bond prices. The credit spread is computed as the difference between the bond yield and a duration-matched portfolio of U.S. Treasury Bonds. The full sample is daily and spans the period 2002:07 to 2022:09. The daily data is from the enhanced version of the TRACE database.

	All	A and above	BBB	Junk		All	A and above	BBB	Junk
Full sample: Jul 2002 – Sept 2022					Post-Crisis: May 2009 – May 2012				
Bid-ask bias (bps)	0.148	0.114	0.138	0.204	Bid-ask bias (bps)	0.168	0.149	0.138	0.217
Return (bps)	1.703	1.022	0.898	1.665	Return (bps)	6.899	4.572	6.194	10.50
Bid-ask spread (bps)	35.13	29.90	34.23	42.77	Bid-ask spread (bps)	42.47	40.98	38.80	48.28
Credit spread (bps)	323.6	137.5	234.3	691.3	Credit spread (bps)	440.6	211.9	297.6	835.1
Pre-crisis: Jul 2002 – Jun 2007					Basel II.5 & III: Jun 2012 – Mar 2014				
Bid-ask bias (bps)	0.278	0.202	0.270	0.373	Bid-ask bias (bps)	0.081	0.061	0.075	0.113
Return (bps)	1.374	−0.450	−1.424	2.980	Return (bps)	1.914	1.356	2.083	1.851
Bid-ask spread (bps)	51.80	42.00	50.78	63.32	Bid-ask spread (bps)	29.52	24.87	29.96	36.26
Credit spread (bps)	314.7	106.9	218.2	653.2	Credit spread (bps)	305.5	141.3	229.9	625.2
Crisis: Jul 2007 – Apr 2009					Post-Volcker: Apr 2014 – Sept 2022				
Bid-ask bias (bps)	0.323	0.285	0.287	0.429	Bid-ask bias (bps)	0.049	0.029	0.046	0.079
Return (bps)	−2.499	1.538	−1.460	−11.23	Return (bps)	0.768	0.291	0.420	0.375
Bid-ask spread (bps)	59.04	57.60	53.03	66.28	Bid-ask spread (bps)	19.87	14.86	20.67	26.20
Credit spread (bps)	602.9	318.0	475.2	1309.6	Credit spread (bps)	238.0	91.51	173.7	555.3

Table 2: Decile portfolios based on short-term reversal. The table presents average excess returns (Panel A) and single-factor CAPMB alphas (Panel B) across decile portfolios sorted on prior returns. The $STREV^{*}$ portfolios are constructed using the MMN-adjusted short-term reversal signals based on the Trade Reporting and Compliance Engine (TRACE) data combined with bond returns computed by WRDS while the $STREV$ portfolios are constructed using the prior-month returns based on the Bank of America Merrill Lynch database provided by Intercontinental Exchange (ICE). The $STREV^{MMN}$ portfolios are constructed using the unadjusted prior-month returns based on the TRACE data from WRDS. Panel A reports the average one-month ahead excess returns on decile portfolios sorted from low to high on the respective short-term reversal signal. The portfolios are value-weighted by amount outstanding. In Panel B, we report the single-factor CAPMB alphas. In Panel C we report the average returns (Ave. Return) and single-factor CAPMB alphas from sorting on $STREV^{MMN}$ with prior-month gross bond returns as the weights following Asparouhova, Bessembinder, and Kalcheva (2013). We report Newey-West t-statistics with 12 lags in parentheses. Panels A, B, and C are based on the sample period 2002:09 to 2022:09 (241 months).

Panel A: Average returns

	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
$STREV^{*}$	0.59	0.29	0.22	0.24	0.24	0.26	0.34	0.38	0.46	0.64	0.05
	(2.03)	(1.85)	(1.64)	(2.00)	(2.06)	(2.23)	(2.66)	(2.72)	(2.84)	(2.41)	(0.41)
$STREV$	0.58	0.25	0.22	0.25	0.26	0.29	0.32	0.35	0.42	0.58	−0.01
	(2.09)	(1.41)	(1.40)	(1.89)	(1.98)	(2.27)	(2.39)	(2.51)	(2.41)	(2.12)	(−0.07)
$STREV^{MMN}$	0.99	0.40	0.30	0.26	0.24	0.25	0.28	0.30	0.32	0.31	−0.69
	(2.88)	(2.41)	(2.25)	(2.25)	(2.10)	(2.13)	(2.25)	(2.18)	(1.97)	(1.22)	(−3.38)

Panel B: One-factor model alphas

	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
$STREV^{*}$	−0.06	−0.09	−0.09	−0.04	−0.02	−0.00	0.06	0.09	0.13	0.16	0.22
	(−0.40)	(−2.28)	(−2.38)	(−1.19)	(−0.54)	(−0.05)	(2.32)	(3.81)	(3.96)	(1.57)	(1.57)
$STREV$	−0.02	−0.14	−0.10	−0.03	−0.01	0.03	0.05	0.06	0.08	0.12	0.15
	(−0.18)	(−2.47)	(−1.44)	(−0.86)	(−0.39)	(1.04)	(1.42)	(2.10)	(2.05)	(1.21)	(1.05)
$STREV^{MMN}$	0.31	0.01	−0.02	−0.02	−0.02	−0.01	0.01	0.00	−0.02	−0.15	−0.46
	(1.63)	(0.21)	(−0.45)	(−0.46)	(−0.44)	(−0.35)	(0.18)	(0.14)	(−0.51)	(−1.35)	(−2.04)

Panel C: Prior-month gross return-weighted $STREV^{MMN}$-sorted portfolios

	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
Ave. Return	0.94	0.43	0.32	0.28	0.24	0.26	0.28	0.29	0.30	0.33	−0.61
	(2.78)	(2.48)	(2.35)	(2.28)	(2.01)	(2.17)	(2.22)	(2.13)	(1.82)	(1.20)	(−3.07)
Alpha	0.31	0.05	0.02	0.00	−0.02	0.00	0.01	0.00	−0.03	−0.12	−0.43
	(1.66)	(0.94)	(0.44)	(0.13)	(−0.47)	(0.15)	(0.34)	(0.02)	(−0.73)	(−0.99)	(−2.01)

Table 3: Short-term reversals – alternative intra-month day gaps and bid- or ask-side prices only. The table presents average excess returns (Panels A through C) across decile portfolios sorted on various short-term reversal signals. In Panel A, we present our baseline results for the MMN-polluted signal, $STREV^{MMN}$, the quote-based ICE signal, $STREV$, and the MMN-corrected signal that uses a minimum of one business day gap between signal formation and ex-ante return computation, $STREV^{*,(1)}$. In Panel B, we report average excess returns from sorting on the MMN-adjusted signals that use a minimum of three and five business days gap between signal formation and ex-ante return computation ($STREV^{*,(3)}$ and $STREV^{*,(5)}$, respectively). Finally, in Panel C, we report average excess returns from sorting on the $STREV$ signals computed using TRACE month-end bid or ask prices only ($STREV_{BID}$ and $STREV_{ASK}$, respectively). The latter signals are computed based on the prior-month prices without any intra-month gap. The $STREV^{MMN}$ signal uses TRACE data, and the signal is computed based on the prior-month price. The $STREV$ signal uses the Bank of America Merrill Lynch data provided by Intercontinental Exchange (ICE), and the signal is computed based on the prior-month price. The $STREV^{*}$ signal uses the TRACE data with the intra-month gap. Panels A through C are based on the sample period 2002:09 to 2022:09 (241 months). We report Newey-West t-statistics with 12 lags in parentheses.

Panel A: Baseline results

	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
$STREV^{MMN}$	0.99	0.40	0.30	0.26	0.24	0.25	0.28	0.30	0.32	0.31	−0.69
	(2.88)	(2.41)	(2.25)	(2.25)	(2.10)	(2.13)	(2.25)	(2.18)	(1.97)	(1.22)	(−3.38)
$STREV$	0.58	0.25	0.22	0.25	0.26	0.29	0.32	0.35	0.42	0.58	−0.01
	(2.09)	(1.41)	(1.40)	(1.89)	(1.98)	(2.27)	(2.39)	(2.51)	(2.41)	(2.12)	(−0.07)
$STREV^{*,(1)}$	0.59	0.29	0.22	0.24	0.24	0.26	0.34	0.38	0.46	0.64	0.05
	(2.03)	(1.85)	(1.64)	(2.00)	(2.06)	(2.23)	(2.66)	(2.72)	(2.84)	(2.41)	(0.41)

Panel B: $STREV^{*}$ with different day gaps

	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
$STREV^{*,(3)}$	0.59	0.28	0.22	0.22	0.24	0.26	0.33	0.41	0.47	0.64	0.06
	(1.96)	(1.54)	(1.58)	(1.81)	(2.02)	(2.31)	(2.79)	(2.94)	(3.02)	(2.52)	(0.42)
$STREV^{*,(5)}$	0.57	0.30	0.24	0.23	0.23	0.26	0.33	0.40	0.48	0.63	0.06
	(1.90)	(1.65)	(1.66)	(1.88)	(2.06)	(2.35)	(2.82)	(2.97)	(2.99)	(2.44)	(0.43)

Panel C: $STREV$ computed with bid or ask prices only

	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
$STREV_{BID}$	0.61	0.21	0.21	0.23	0.25	0.29	0.31	0.39	0.42	0.62	0.01
	(2.07)	(1.30)	(1.71)	(2.15)	(2.27)	(2.51)	(2.55)	(2.85)	(2.64)	(2.30)	(0.09)
$STREV_{ASK}$	0.59	0.23	0.21	0.21	0.26	0.30	0.31	0.39	0.44	0.60	0.02
	(2.02)	(1.45)	(1.81)	(1.85)	(2.32)	(2.61)	(2.55)	(2.85)	(2.77)	(2.24)	(0.11)

Table 4: Summary statistics and performance tests for the short-term reversal factors. Panel A reports the short-term reversal factor means (Mean), the single-factor CAPMB alphas (Alpha), the bias-adjusted factor squared Sharpe ratios (Sh^2) proposed by Barillas, Kan, Robotti, and Shanken (2020), the factor standard deviations (SD), and the minimum (Min) and maximum (Max) values. The REV^* factor is constructed using the Trade Reporting and Compliance Engine (TRACE) data based on an MMN-adjusted short-term reversal signal. The REV factor is constructed using the Bank of America Merrill Lynch database provided by Intercontinental Exchange (ICE). The REV^{MMN} factor is the incorrectly constructed short-term reversal factor using the TRACE data based on a short-term reversal signal that has not been corrected for bid-ask bias. Panels A and B are based on the sample period 2002:09 to 2022:09 (241 months). p-values are in square brackets.

Panel A: Short-term reversal factor statistics and squared Sharpe ratios						
	Mean	Alpha	Sh^2	SD	Min	Max
REV^*	0.066	−0.012	−0.003	1.635	−6.478	9.534
	[0.452]	[0.913]	[0.529]			
REV	0.170	0.077	0.003	1.987	−6.686	12.602
	[0.138]	[0.554]	[0.183]			
REV^{MMN}	0.675	0.564	0.101	2.073	−6.256	11.423
	[0.000]	[0.000]	[0.000]			

Panel B: Short-term reversal factor correlations			
	REV^*	REV	REV^{MMN}
REV^*	1	0.894	0.900
REV		1	0.881
REV^{MMN}			1

Table 5: Momentum and long-term reversal decile portfolios. The table presents average excess returns (Ave. Return) and alphas (Alpha) across decile portfolios sorted on momentum (Panels A and B) and long-term reversal signals (Panels C). The first row of each panel reports the average one-month ahead ($t+1$) excess returns on decile portfolios sorted from low to high on the respective signal. The portfolios are value-weighted by bond amount outstanding. In the second row, we report the alphas from the single-factor CAPMB model which includes the bond market factor as the only control. We report Newey-West t-statistics with 12 lags in parentheses. Panels A–C are based on the sample period 2002:09 to 2022:09 (241 months) using the Bank of America Merrill Lynch database provided by Intercontinental Exchange (ICE).

	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
Panel A: Momentum (6,1) decile sorts											
Ave. Return	0.58	0.42	0.27	0.26	0.28	0.28	0.28	0.34	0.37	0.53	−0.05
	(1.71)	(1.85)	(1.56)	(1.82)	(2.17)	(2.16)	(2.17)	(2.36)	(2.24)	(2.17)	(−0.22)
Alpha	−0.01	0.00	−0.08	−0.04	0.01	−0.01	−0.00	0.04	0.04	0.16	0.16
	(−0.04)	(0.05)	(−1.34)	(−0.85)	(0.14)	(−0.14)	(−0.00)	(0.86)	(0.59)	(1.08)	(0.87)
Panel B: Momentum (12,1) decile sorts											
	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
Ave. Return	0.79	0.45	0.35	0.29	0.24	0.25	0.26	0.27	0.3	0.45	−0.34
	(1.84)	(1.92)	(1.87)	(1.84)	(1.82)	(1.98)	(2.25)	(2.15)	(2.24)	(2.58)	(−1.02)
Alpha	0.20	0.03	0.00	−0.04	−0.06	−0.04	−0.02	−0.02	0.00	0.13	−0.07
	(1.03)	(0.49)	(0.02)	(−0.98)	(−1.60)	(−1.48)	(−0.39)	(−0.30)	(0.05)	(1.58)	(−0.31)
Panel C: Long-term reversal (48,13) decile sorts											
	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
Ave. Return	0.62	0.51	0.41	0.33	0.32	0.30	0.25	0.24	0.30	0.36	−0.26
	(2.00)	(2.35)	(2.73)	(2.44)	(2.44)	(2.21)	(1.61)	(1.50)	(1.67)	(1.78)	(−1.20)
Alpha	0.19	0.16	0.17	0.08	0.08	0.02	−0.07	−0.10	−0.10	−0.09	−0.28
	(1.00)	(1.71)	(2.51)	(2.36)	(1.20)	(0.43)	(−1.11)	(−1.61)	(−1.39)	(−0.96)	(−1.21)

Table 6: Momentum decile portfolios across databases – extended sample with and without trimming. The table presents average excess returns (Ave. Return) and alphas (Alpha) across decile portfolios sorted on momentum following the methodology of Jegadeesh and Titman (1993) that is implemented by Jostova, Nikolova, Philipov, and Stahel (2013) (JNPS). The sample is aligned to the JNPS start date of January 1974 and equal weights are used to form portfolios. All panels employ the Lehman Brothers database (LHM) spanning the period 1974:01 to 1996:12 and the Bank of America Merrill Lynch database provided by Intercontinental Exchange (ICE) spanning 1997:01 to 2022:09. Panel A reports the full sample results spanning 1974:01 to 2022:09, Panel B uses the same sample as in JNPS spanning 1974:01 to 2011:06, Panel C uses the LHM data spanning 1974-01 to 1996:12, Panels D and E replicate Panel B with trimmed data. In Panel D, we ex-post trim the return data at the 99.5^{th} percentile over the JNPS sample. In Panel E we ex-post trim the return data above the +30% threshold over the JNPS sample. The first row of each panel reports the average one-month ahead excess returns on decile portfolios sorted from low to high on the respective signal. In the second row, we report the alphas from the single-factor CAPMB model, which includes the bond market factor as the only control. We report Newey-West t-statistics with 12 lags in parentheses.

Panel A: 1974:01–2022:09

	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
Ave. Return	0.39	0.30	0.27	0.25	0.25	0.25	0.25	0.26	0.27	0.33	−0.06
	(2.10)	(2.41)	(2.33)	(2.25)	(2.26)	(2.27)	(2.27)	(2.28)	(2.30)	(2.52)	(−0.54)
Alpha	0.09	0.04	0.01	−0.01	−0.01	−0.00	−0.00	0.00	0.01	0.08	−0.00
	(0.93)	(1.13)	(0.46)	(−0.32)	(−0.29)	(−0.13)	(−0.09)	(0.08)	(0.52)	(1.84)	(−0.04)

Panel B: 1974:01–2011:06

	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
Ave. Return	0.37	0.30	0.28	0.27	0.27	0.28	0.28	0.29	0.29	0.35	−0.02
	(1.68)	(1.97)	(2.00)	(1.99)	(2.02)	(2.05)	(2.10)	(2.11)	(2.10)	(2.24)	(−0.14)
Alpha	0.07	0.01	0.00	−0.01	−0.00	0.01	0.02	0.02	0.03	0.11	0.04
	(0.61)	(0.32)	(0.00)	(−0.31)	(−0.00)	(0.29)	(0.68)	(0.92)	(1.31)	(2.04)	(0.41)

Panel C: 1974:01–1996:12

	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
Ave. Return	0.22	0.23	0.23	0.23	0.23	0.23	0.23	0.23	0.24	0.32	0.09
	(1.09)	(1.19)	(1.17)	(1.17)	(1.15)	(1.12)	(1.14)	(1.13)	(1.18)	(1.54)	(1.30)
Alpha	0.02	0.00	−0.00	−0.00	−0.01	−0.01	−0.00	−0.00	0.01	0.11	0.10
	(0.29)	(0.11)	(−0.14)	(−0.19)	(−0.32)	(−0.58)	(−0.22)	(−0.08)	(0.61)	(2.64)	(1.41)

Panel D: 1974:01–2011:06 with ex-post winsorizing at the 99.5^{th} percentile

	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
Ave. Return	−0.14	0.18	0.21	0.22	0.24	0.25	0.25	0.25	0.25	0.20	0.35
	(−0.64)	(1.27)	(1.59)	(1.67)	(1.76)	(1.82)	(1.87)	(1.86)	(1.77)	(1.34)	(2.60)
Alpha	−0.41	−0.09	−0.06	−0.04	−0.03	−0.02	−0.01	−0.01	−0.01	−0.04	0.37
	(−2.18)	(−1.47)	(−1.52)	(−1.47)	(−1.17)	(−0.78)	(−0.46)	(−0.34)	(−0.45)	(−0.61)	(2.53)

Panel E: 1974:01–2011:06 with ex-post trimming at +30%

	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
Ave. Return	0.12	0.26	0.26	0.26	0.27	0.27	0.28	0.28	0.28	0.30	0.18
	(0.62)	(1.78)	(1.93)	(1.93)	(1.99)	(2.02)	(2.06)	(2.07)	(2.03)	(1.96)	(1.83)
Alpha	−0.17	−0.03	−0.01	−0.02	−0.01	0.00	0.01	0.02	0.02	0.05	0.22
	(−1.23)	(−0.66)	(−0.66)	(−0.73)	(−0.3)	(0.04)	(0.42)	(0.66)	(0.77)	(1.02)	(2.00)

Table 7: Average returns for momentum double sorts. Panel A reports the momentum factor means for long-short positions in bonds with high and low momentum across ratings, illiquidity ($ILLIQ$), and maturity. The momentum factor (MOM) means are presented in the last column. Panel B reports the single-factor CAPMB alphas. Panels A and B are based on the sample period 2002:09 to 2022:09 (241 months) using the Bank of America Merrill Lynch database provided by Intercontinental Exchange (ICE). We report Newey-West t-statistics with 12 lags in parentheses.

Panel A: Average returns across rating, illiquidity, and maturity

	Q1 (Low)	Q2	Q3	Q4	Q5 (High)	MOM
Rating	−0.05	−0.17	−0.22	−0.14	−0.08	−0.14
	(−0.37)	(−0.82)	(−0.94)	(−0.65)	(−0.26)	(−0.80)
$ILLIQ$	0.03	0.13	−0.00	−0.16	−0.22	−0.04
	(0.20)	(1.02)	(−0.03)	(−0.63)	(−0.81)	(−0.25)
Maturity	−0.20	0.11	0.07	−0.34	−0.34	−0.15
	(−0.33)	(0.44)	(0.29)	(−1.72)	(−1.34)	(−0.67)

Panel B: Alphas across rating, illiquidity, and maturity

	Q1 (Low)	Q2	Q3	Q4	Q5 (High)	MOM
Rating	0.00	−0.08	−0.15	−0.07	0.21	−0.02
	(0.00)	(−0.42)	(−0.69)	(−0.28)	(0.79)	(−0.13)
$ILLIQ$	0.23	0.27	0.10	0.05	−0.02	0.13
	(1.71)	(1.94)	(0.76)	(0.33)	(−0.10)	(0.98)
Maturity	−0.14	0.31	0.27	−0.17	−0.19	0.01
	(−0.19)	(1.15)	(1.32)	(−1.00)	(−0.89)	(0.02)

Table 8: Momentum echoes. Panel A reports the momentum factor means for long-short positions in bonds with high and low momentum across ratings, for different momentum strategies, MOM^s. The momentum factor (MOM) means are presented in the last column. The strategies are ranked from high to low based on their MOM factor average. Panel B reports the single-factor CAPMB alphas. Panels A and B are based on the sample period 2002:09 to 2022:09 (241 months) using the Bank of America Merrill Lynch database provided by Intercontinental Exchange (ICE). We report Newey-West t-statistics with 12 lags in parentheses.

Panel A: Average high-minus-low momentum return spreads across ratings

MOM^s	Q1 (Low)	Q2	Q3	Q4	Q5 (High)	MOM
(6,4)	0.13	0.08	−0.02	0.06	0.17	0.08
	(0.96)	(0.61)	(−0.16)	(0.41)	(0.74)	(0.67)
(8,7)	0.05	0.10	0.02	−0.03	0.17	0.06
	(0.40)	(0.57)	(0.15)	(−0.21)	(0.74)	(0.43)
(6,5)	0.14	0.17	−0.05	−0.05	0.05	0.05
	(1.13)	(1.15)	(−0.43)	(−0.53)	(0.23)	(0.49)
(3,2)	0.01	0.12	−0.01	−0.08	0.14	0.04
	(0.06)	(0.71)	(−0.10)	(−0.49)	(0.68)	(0.34)
(12,11)	0.17	0.07	0.16	0.06	−0.24	0.04
	(1.64)	(0.56)	(1.53)	(0.60)	(−0.98)	(0.43)

Panel B: Average high-minus-low momentum alphas across ratings

MOM^s	Q1 (Low)	Q2	Q3	Q4	Q5 (High)	MOM
(6,4)	0.15	0.14	0.01	0.13	0.50	0.19
	(1.15)	(0.89)	(0.06)	(0.87)	(1.88)	(1.37)
(8,7)	−0.01	0.11	0.04	0.01	0.22	0.07
	(−0.04)	(0.51)	(0.25)	(0.04)	(1.27)	(0.52)
(6,5)	0.20	0.28	−0.02	0.04	0.30	0.16
	(1.29)	(1.28)	(−0.17)	(0.32)	(1.26)	(1.14)
(3,2)	0.03	0.16	0.03	−0.05	0.37	0.12
	(0.23)	(1.08)	(0.35)	(−0.29)	(1.76)	(1.07)
(12,11)	0.06	−0.11	0.04	−0.08	−0.16	−0.05
	(0.48)	(−0.53)	(0.35)	(−0.57)	(−0.76)	(−0.39)

Table 9: Average returns for long-term reversal double sorts. Panel A reports the long-term reversal (LTR) factor means for a long (short) position in past long-term bond losers (winners) across bond ratings, illiquidity ($ILLIQ$), and maturity. The LTR factor means are presented in the last column. Panel B reports the single-factor CAPMB alphas. We use the original $LTREV^s$ specification which is based on a look-back period of 48 months and a skip period of 12 months, $LTREV^s = (48, 13)$. Panels A and B are based on the sample period 2002:09 to 2022:09 (241 months) using the Bank of America Merrill Lynch database provided by Intercontinental Exchange (ICE). We report Newey-West t-statistics with 12 lags in parentheses.

Panel A: Average returns across rating, illiquidity ($ILLIQ$) and maturity

	Q1 (Low)	Q2	Q3	Q4	Q5 (High)	LTR
Rating	0.03	0.01	0.15	0.29	0.24	0.14
	(0.17)	(0.06)	(1.00)	(1.91)	(0.76)	(0.92)
$ILLIQ$	0.27	0.14	0.10	0.25	0.14	0.18
	(1.47)	(0.98)	(0.69)	(1.48)	(0.62)	(1.25)
Maturity	0.16	0.31	0.23	0.36	0.28	0.27
	(0.72)	(1.29)	(1.29)	(2.05)	(1.70)	(1.65)

Panel B: Alphas across rating, illiquidity ($ILLIQ$) and maturity

	Q1 (Low)	Q2	Q3	Q4	Q5 (High)	LTR
Rating	0.23	0.10	0.26	0.38	−0.04	0.19
	(1.39)	(0.48)	(1.90)	(2.31)	(−0.14)	(1.16)
$ILLIQ$	0.26	0.25	0.19	0.21	0.10	0.20
	(1.27)	(1.53)	(1.16)	(1.24)	(0.40)	(1.20)
Maturity	0.03	0.19	0.14	0.35	0.25	0.19
	(0.22)	(1.04)	(0.94)	(2.18)	(1.32)	(1.44)

Table 10: Best performing long-term reversal strategies. Panel A reports the long-term reversal portfolio means for a long (short) position in bonds with high (low) long-term reversal across bond ratings, for different long-term reversal signals, $LTREV^s$. The long-term reversal factor means are presented in the last column. The strategies are ranked from high to low based on their LTR factor average. Panel B reports the single-factor CAPMB alphas. Panels A and B are based on the sample period 2002:09 to 2022:09 (241 months) using the Bank of America Merrill Lynch database provided by Intercontinental Exchange (ICE). We report Newey-West t-statistics with 12 lags in parentheses.

Panel A: Average high-minus-low long-term reversal return spreads across ratings

$LTREV^s$	Q1 (Low)	Q2	Q3	Q4	Q5 (High)	LTR
(45,6)	0.13	0.23	0.25	0.43	0.18	0.25
	(0.65)	(1.34)	(1.49)	(2.23)	(0.47)	(1.27)
(35,9)	0.04	0.17	0.11	0.32	0.52	0.23
	(0.23)	(1.02)	(0.71)	(1.84)	(1.49)	(1.35)
(47,6)	0.07	0.20	0.21	0.46	0.23	0.23
	(0.36)	(1.18)	(1.40)	(2.37)	(0.62)	(1.27)
(32,12)	0.02	0.18	0.10	0.32	0.56	0.23
	(0.13)	(1.20)	(0.72)	(1.75)	(1.37)	(1.33)
(45,7)	0.07	0.24	0.20	0.43	0.22	0.23
	(0.37)	(1.28)	(1.18)	(2.27)	(0.58)	(1.20)

Panel B: Average high-minus-low long-term reversal alphas across ratings

$LTREV^s$	Q1 (Low)	Q2	Q3	Q4	Q5 (High)	LTR
(45,6)	0.23	0.35	0.34	0.56	−0.08	0.28
	(1.24)	(1.77)	(1.85)	(3.05)	(−0.21)	(1.40)
(35,9)	0.13	0.29	0.20	0.45	0.40	0.29
	(0.72)	(1.67)	(1.12)	(2.85)	(1.17)	(1.79)
(47,6)	0.20	0.33	0.31	0.59	−0.05	0.28
	(1.13)	(1.68)	(1.89)	(3.38)	(−0.14)	(1.49)
(32,12)	0.17	0.27	0.18	0.47	0.30	0.28
	(1.24)	(1.87)	(1.33)	(2.78)	(0.89)	(1.84)
(45,7)	0.20	0.36	0.30	0.58	−0.05	0.28
	(1.18)	(1.87)	(1.61)	(3.09)	(−0.12)	(1.42)

Table 11: Canonical correlations. The table presents the results of the first stage of the Pukthuanthong, Roll, and Subrahmanyam (2019) factor identification protocol. In Panel A, we report the canonical correlations (Canon. Corr.) of the 7 principal components extracted from the set of 132 basis assets and the seven factor candidates. The test assets comprise portfolios sorted on credit spreads (25), size × maturity (25), $ILLIQ$ × rating (25), momentum, long- and short-term reversals (30), value-at-risk (10), and the Fama-French industry portfolios (17) formed with the Bank of America Merrill Lynch database provided by Intercontinental Exchange (ICE). In Panel B, we present the results of the factor identification protocol, that is, we regress the seven canonical variates on a constant and the full set of factor candidates. For each factor, we report the average t-statistic from a regression with all of the canonical variates (Ave. t-stat) and the average t-statistic from a regression with those variates that are associated with significant (at the 5% nominal level) canonical correlations (Ave. t-stat$_{cond}$). The final row of Panel B indicates whether a given factor passes the first stage of the identification protocol. Panel C presents the candidate factor correlations. Panels A through C are based on the sample period 2002:09 to 2022:09 (241 months). t/z-values are in parentheses.

Panel A: Canonical correlations							
	1	2	3	4	5	6	7
Canon. Corr.	1.00	0.99	0.93	0.86	0.74	0.60	0.06
z-stat.	(74.07)	(54.55)	(38.96)	(29.15)	(20.08)	(11.96)	(0.20)

Panel B: Significance levels for the factor candidates							
	REV	LTR	MOM	$MKTB$	DRF	CRF	LRF
Ave. t-stat	(4.10)	(7.31)	(5.70)	(35.20)	(6.96)	(17.49)	(4.65)
Ave. t-stat$_{cond}$	(4.67)	(8.47)	(6.59)	(41.07)	(8.08)	(20.40)	(5.36)
Pass/Fail	Pass	Pass	Pass	Pass	Pass	Pass	Pass

Panel C: Factor correlations							
	REV	LTR	MOM	$MKTB$	DRF	CRF	LRF
REV	1.00						
LTR	0.09	1.00					
MOM	−0.30	−0.42	1.00				
$MKTB$	0.26	0.18	−0.27	1.00			
DRF	0.31	0.39	−0.42	0.86	1.00		
CRF	0.05	0.33	−0.21	0.22	0.21	1.00	
LRF	0.17	0.20	−0.15	0.62	0.69	0.06	1.00

Table 12: Summary statistics and performance tests. Panel A reports means (Mean), single-factor bond market alphas (Alpha), bias-adjusted squared Sharpe ratios (Sh^2), and standard deviations (SD) for short-term reversal factor (REV), the long-term reversal factor (LTR), the momentum factor (MOM), the bond market factor ($MKTB$), the downside risk factor (DRF), the credit risk factor (CRF), and the liquidity risk factor (LRF). In Panel B, we report pairwise model comparison tests based on bias-adjusted squared Sharpe ratios (the differences are computed between the squared Sharpe ratios in row i and column j). Panels A and B are based on the sample period 2002:09 to 2022:09 (241 months) using the Bank of America Merrill Lynch database provided by Intercontinental Exchange (ICE). p-values are in square brackets.

Panel A: Factor statistics and squared Sharpe ratios

	REV	LTR	MOM	$MKTB$	DRF	CRF	LRF
Mean	0.170	0.122	−0.139	0.344	0.422	0.253	0.167
	[0.138]	[0.204]	[0.420]	[0.032]	[0.065]	[0.188]	[0.111]
Alpha	0.077	0.082	−0.021	–	−0.010	0.142	0.041
	[0.554]	[0.345]	[0.906]	–	[0.934]	[0.456]	[0.478]
Sh^2	0.003	0.006	−0.001	0.029	0.019	0.004	0.018
	[0.183]	[0.115]	[0.376]	[0.005]	[0.018]	[0.152]	[0.021]
SD	1.987	1.202	2.442	1.890	2.767	2.752	1.131

Panel B: Differences in squared Sharpe ratios

	REV	LTR	MOM	RFM	BBW+RFM	BBW
CAPMB	0.026	0.023	0.030	0.025	0.012	0.007
	[0.429]	[0.439]	[0.333]	[0.051]	[0.847]	[0.691]

Table 13: Bond-level Fama-MacBeth regressions. The table reports factor and characteristic premia (Coef., percent per month) estimated using two-pass cross-sectional Fama-MacBeth regressions. In model 1, only factor betas are considered. The factors include the MMN-adjusted short-term reversal (REV), long-term reversal (LTR), momentum (MOM), bond market ($MKTB$), downside (DRF), credit (CRF), and liquidity (LRF) factors. In model 2, we include bond characteristics such as $STREV$, MOM^s, historical bond value-at-risk (VaR), illiquidity ($ILLIQ$), and ratings ($Rating$). In model 3, we augment model 2 with the long-term reversal characteristic ($LTREV^s$). In model 4, we only include the characteristics from model 3 that are statistically significant at the 5% nominal level. For each specification, we report the adjusted R^2 (R^2_{Adj}) and the total numbers of observations (Obs.) The sample period is 2002:09 to 2022:09 (241 months) using the Bank of America Merrill Lynch database provided by Intercontinental Exchange (ICE). t-statistics (t-stat) in parentheses are based on a 12-lag Newey-West adjustment.

	model 1		model 2		model 3		model 4	
	Coef.	t-stat	Coef.	t-stat	Coef.	t-stat	Coef.	t-stat
REV	−0.00	(−0.01)	0.01	(0.07)	0.06	(0.32)	−0.05	(−0.40)
LTR	0.09	(1.07)	0.07	(0.60)	0.06	(0.49)	0.07	(0.81)
MOM	−0.06	(−0.37)	−0.08	(−0.43)	−0.08	(−0.43)	−0.02	(−0.10)
$MKTB$	0.16	(1.18)	0.17	(1.21)	0.09	(0.67)	0.16	(1.22)
DRF	0.23	(1.17)	0.24	(1.16)	0.22	(1.05)	0.21	(1.09)
CRF	0.11	(0.55)	−0.13	(−0.66)	−0.16	(−0.85)	−0.10	(−0.50)
LRF	0.12	(1.26)	0.17	(1.52)	0.16	(1.47)	0.13	(1.43)
$STREV$			−0.02	(−1.01)	−0.02	(−1.21)		
$LTREV^s$					−0.20	(−0.83)		
MOM^s			0.25	(0.39)	−0.15	(−0.23)		
VaR			0.01	(0.95)	0.01	(1.02)		
$Rating$			0.03	(5.89)	0.03	(4.31)	0.04	(4.56)
$ILLIQ$			−0.01	(−0.46)	−0.00	(−0.24)		
Constant	0.24	(5.90)	−0.08	(−1.40)	0.05	(0.45)	−0.12	(−1.81)
R^2_{Adj}	0.320		0.405		0.430		0.331	
Obs.	569,990		569,990		342,661		569,990	

Table 14: Portfolio-level three-pass regressions. The table reports factor premiums (Coef., percent per month) based on the Giglio and Xiu (2021) three-pass regression procedure. The test assets comprise 132 portfolios sorted on credit spreads (25), size × maturity (25), $ILLIQ$ × rating (25), momentum, long- and short-term reversal (30), value-at-risk (10), and the Fama-French industry portfolios (17). The seven factors include short- and long-term reversals (REV and LTR), momentum (MOM), bond market risk ($MKTB$), and downside, credit, and liquidity risks (DRF, CRF, and LRF). The three-pass regressions are based on three, five, and seven principal components (PCs). The sample period is 2002:09 to 2022:09 (241 months) using the Bank of America Merrill Lynch database provided by Intercontinental Exchange (ICE). t-statistics (t-stat) are in parentheses and are based on a 12-lag Newey-West adjustment.

	3 PCs		5 PCs		7 PCs	
	Coef.	t-stat	Coef.	t-stat	Coef.	t-stat
REV	0.07	(1.71)	0.05	(0.90)	0.02	(0.24)
LTR	0.08	(1.10)	0.09	(1.29)	0.11	(1.28)
MOM	−0.14	(−1.63)	−0.14	(−1.35)	−0.15	(−1.36)
$MKTB$	0.22	(1.39)	0.23	(1.43)	0.25	(1.52)
DRF	0.32	(1.47)	0.31	(1.44)	0.33	(1.47)
CRF	0.28	(1.36)	0.28	(1.44)	0.27	(1.43)
LRF	0.08	(1.16)	0.08	(1.10)	0.11	(1.19)
Constant	0.13	(7.13)	0.12	(6.68)	0.11	(4.37)

Table 15: Hedge portfolio returns for bond factors. The table presents average returns (Ave. Return) and CAPMB alphas (Alpha) for the high-minus-low decile and quintile hedge portfolios associated with the univariate REV, LTR, MOM, $MKTB$, DRF, CRF, and LRF betas. Panel A (C) reports results when portfolios are constructed from top and bottom deciles (quintiles). Panel B (D) presents average returns, standard deviations (SD), and Sharpe ratios (SR) for the $MKTB$ factor augmented with the portfolio returns presented in Panel A (C). The sample period is 2004:09 to 2022:09 (217 months) using the Bank of America Merrill Lynch database provided by Intercontinental Exchange (ICE). We report the annualized Sharpe ratios and associated t-statistics for the null $H_0 : SR = 0.80$ computed under the assumption of i.i.d. returns following Lo (2002). t-statistics for average returns and alphas are based on a 12-lag Newey-West adjustment.

Panel A: Decile hedge portfolio returns

	REV	LTR	MOM	$MKTB$	DRF	CRF	LRF
Ave. Return	−0.06	0.07	0.13	0.62	0.53	0.40	0.55
	(−0.32)	(0.24)	(0.46)	(1.93)	(1.83)	(1.16)	(1.78)
Alpha	−0.21	0.08	0.16	0.29	0.16	0.20	0.30
	(−0.99)	(0.30)	(0.57)	(1.50)	(0.89)	(0.63)	(1.17)

Panel B: $MTKB$ plus decile hedge portfolio returns

		Augmented returns						
	$MKTB$	REV	LTR	MOM	$MKTB$	DRF	CRF	LRF
Ave. Return	0.29	0.23	0.36	0.42	0.91	0.82	0.69	0.84
	(1.69)	(0.86)	(1.06)	(1.22)	(1.92)	(1.89)	(1.52)	(1.88)
SD	1.91	4.23	3.99	4.05	4.86	5.02	5.04	4.51
SR	0.53	0.19	0.31	0.36	0.65	0.57	0.48	0.64
	(−3.75)	(−8.97)	(−6.99)	(−6.33)	(−2.02)	(−3.19)	(−4.52)	(−2.08)

Panel C: Quintile hedge portfolio returns

	REV	LTR	MOM	$MKTB$	DRF	CRF	LRF
Ave. Return	−0.09	0.06	0.04	0.41	0.35	0.29	0.37
	(−0.56)	(0.29)	(0.23)	(1.64)	(1.58)	(1.16)	(1.56)
Alpha	−0.18	0.08	0.06	0.10	0.02	0.14	0.12
	(−1.11)	(0.42)	(0.26)	(0.85)	(0.14)	(0.63)	(0.68)

Panel D: $MTKB$ plus quintile hedge portfolio returns

		Augmented returns						
	$MKTB$	REV	LTR	MOM	$MKTB$	DRF	CRF	LRF
Ave. Return	0.29	0.20	0.35	0.34	0.70	0.64	0.58	0.66
	(1.69)	(0.86)	(1.31)	(1.23)	(1.71)	(1.70)	(1.59)	(1.70)
SD	1.91	3.28	3.05	3.27	4.28	4.43	4.00	4.05
SR	0.53	0.21	0.40	0.36	0.56	0.50	0.50	0.57
	(−3.75)	(−8.56)	(−5.71)	(−6.36)	(−3.25)	(−4.13)	(−4.18)	(−3.20)

Table 16: Hedge portfolio returns for bond characteristics. The table presents average returns (Ave. Return) and CAPMB alphas (Alpha) for the high-minus-low decile and quintile hedge portfolios associated with the $STREV$, $LTREV^s$, MOM^s, VaR, $ILLIQ$, and *Rating* characteristics. Panel A (C) reports results when portfolios are constructed from the top and bottom deciles (quintiles). Panel B (D) displays returns for the $MKTB$ factor augmented with the portfolio returns presented in Panel A (C). For these panels, we multiply the high-minus-low decile (quintile) $STREV$ and $LTREV$ spreads by -1 such that they reflect the underlying investment strategy of a long (short) position in prior losers (winners). Since the distribution of *Rating* is not big enough to form deciles, we only form *Rating* quintiles. The sample period is 2004:09 to 2022:09 (217 months) using the Bank of America Merrill Lynch database provided by Intercontinental Exchange (ICE). We report the annualized Sharpe ratios and associated t-statistics of the null of $H_0 : SR = 0.80$ computed under the assumption of i.i.d. returns following Lo (2002). t-statistics for the average returns and alphas are based on a 12-lag Newey-West adjustment.

Panel A: Decile hedge portfolio returns

	$STREV$	$LTREV^s$	MOM^s	VaR	$ILLIQ$
Ave. Return	−0.01	−0.17	−0.01	0.57	0.20
	(−0.05)	(−0.80)	(−0.06)	(1.43)	(1.35)
Alpha	0.13	−0.21	0.17	0.07	0.13
	(0.88)	(−0.87)	(0.85)	(0.36)	(1.14)

Panel B: $MTKB$ plus decile hedge portfolio returns

		Augmented returns				
	$MKTB$	$STREV$	$LTREV^s$	MOM^s	VaR	$ILLIQ$
Ave. Return	0.29	0.30	0.47	0.28	0.86	0.49
	(1.69)	(1.31)	(1.51)	(1.21)	(1.55)	(1.66)
SD	1.91	4.27	3.25	3.91	5.95	2.79
SR	0.53	0.24	0.50	0.24	0.50	0.61
	(−3.75)	(−8.12)	(−4.23)	(−8.06)	(−4.13)	(−2.61)

Panel C: Quintile hedge portfolio returns

	$STREV$	$LTREV^s$	MOM^s	VaR	$ILLIQ$	*Rating*
Ave. Return	0.08	−0.19	−0.04	0.43	0.22	0.30
	(0.82)	(−0.98)	(−0.17)	(1.45)	(1.55)	(1.20)
Alpha	0.17	−0.23	0.10	0.02	0.09	0.10
	(1.72)	(−1.21)	(0.59)	(0.17)	(1.19)	(0.47)

Panel D: $MTKB$ plus quintile hedge portfolio returns

		Augmented returns					
	$MKTB$	$STREV$	$LTREV^s$	MOM^s	VaR	$ILLIQ$	*Rating*
Ave. Return	0.29	0.21	0.48	0.25	0.72	0.51	0.59
	(1.69)	(1.04)	(1.72)	(1.21)	(1.57)	(1.68)	(1.57)
SD	1.91	3.39	2.88	3.11	4.96	2.94	4.35
SR	0.53	0.21	0.58	0.28	0.51	0.60	0.47
	(−3.75)	(−8.52)	(−3.05)	(−7.45)	(−4.09)	(−2.69)	(−4.61)

Table 17: Unconditional test of Holcblat, Lioui, and Weber (2023). The table presents the results of the unconditional test of Holcblat, Lioui, and Weber (2023). In each panel, we report the average returns (Ave. Return) on the long and short legs for each factor (Panel A) and hedge portfolio (Panels B to E) along with the $p-$value of the test (in square brackets). In Panel A, we present the results for the REV, LTR, MOM, $MKTB$, DRF, CRF, and LRF factors. In Panel B (D), we present the results for hedge portfolios associated with the univariate REV, LTR, MOM, $MKTB$, DRF, CRF, and LRF factor betas when portfolios are constructed from top and bottom deciles (quintiles). In Panel C (E), we present the results for hedge portfolios associated with the $STREV$, $LTREV^s$, MOM^s, VaR, and $ILLIQ$ characteristics when portfolios are constructed from top and bottom deciles (quintiles). Since the distribution of *Rating* is not big enough to form deciles, we only form *Rating* quintiles. The sample period is 2004:09 to 2022:09 (217 months) using the Bank of America Merrill Lynch database provided by Intercontinental Exchange (ICE).

Panel A: Factor returns

	REV	LTR	MOM	$MKTB$	DRF	CRF	LRF
Ave. Return (long leg)	0.53	0.48	0.41	0.39	0.61	0.50	0.53
Ave. Return (short leg)	0.34	0.39	0.47	0.09	0.27	0.33	0.37
p-value	[0.23]	[0.09]	–	[0.00]	[0.00]	[0.11]	[0.01]

Panel B: Decile returns on hedge portfolios sorted on betas

	REV	LTR	MOM	$MKTB$	DRF	CRF	LRF
Ave. Return (long leg)	0.45	0.58	0.51	0.74	0.75	0.65	0.77
Ave. Return (short leg)	0.52	0.51	0.38	0.11	0.22	0.24	0.22
p-value	–	[0.48]	[1.00]	[0.02]	[0.00]	[0.19]	[0.00]

Panel C: Decile returns on hedge portfolios sorted on characteristics

	$STREV$	$LTREV^s$	MOM^s	VaR	$ILLIQ$
Ave. Return (long leg)	0.48	0.48	0.43	0.69	0.51
Ave. Return (short leg)	0.48	0.30	0.45	0.11	0.31
p-value	–	[0.65]	–	[0.04]	[0.16]

Panel D: Quintile returns on hedge portfolios sorted on betas

	REV	LTR	MOM	$MKTB$	DRF	CRF	LRF
Ave. Return (long leg)	0.37	0.45	0.42	0.56	0.56	0.54	0.59
Ave. Return (short leg)	0.46	0.39	0.38	0.15	0.21	0.26	0.22
p-value	–	[0.65]	[0.63]	[0.00]	[0.00]	[0.22]	[0.00]

Panel E: Quintile returns on hedge portfolios sorted on characteristics

	$STREV$	$LTREV^s$	MOM^s	VaR	$ILLIQ$	*Rating*
Ave. Return (long leg)	0.42	0.47	0.37	0.57	0.46	0.49
Ave. Return (short leg)	0.34	0.28	0.41	0.14	0.24	0.19
p-value	[1.00]	[1.00]	–	[0.02]	[0.03]	[0.08]

Table 18: Short-term reversal, momentum and long-term reversal decile portfolios – extended sample. The table presents average excess returns (Ave. Return) and alphas (Alpha) across decile portfolios sorted on short-term reversals (Panel A), momentum (Panel B), and long-term reversal signals (Panels C). The first row of each panel reports the average one-month ahead excess returns on decile portfolios sorted from low to high on the respective signal. The portfolios are value-weighted by bond amount outstanding. In the second row, we report the alphas from the single-factor CAPMB model which includes the bond market factor as the only control. We report Newey-West t-statistics with 12 lags in parentheses. Panels A–C are based on the sample period 1977:01 to 2022:09 (549 months). The data comprises the Lehman Brothers database (LHM) spanning 1977:01 to 1996:12 and the Bank of America Merrill Lynch database provided by Intercontinental Exchange (ICE) spanning 1997:01 to 2022:09.

Panel A: Short-term reversal decile sorts

	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
Ave. Return	0.36	0.25	0.22	0.26	0.25	0.25	0.26	0.26	0.29	0.32	−0.04
	(2.16)	(2.08)	(1.97)	(2.30)	(2.20)	(2.23)	(2.24)	(2.25)	(2.27)	(1.95)	(−0.43)
Alpha	0.02	−0.03	−0.03	0.01	−0.00	0.01	0.01	0.01	0.02	0.02	0.00
	(0.22)	(−0.75)	(−0.96)	(0.24)	(−0.01)	(0.25)	(0.31)	(0.38)	(0.73)	(0.39)	(0.03)

Panel B: Momentum decile sorts (6,1)

	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
Ave. Return	0.33	0.31	0.26	0.25	0.25	0.25	0.24	0.24	0.24	0.32	−0.01
	(1.81)	(2.27)	(2.14)	(2.21)	(2.25)	(2.22)	(2.18)	(2.11)	(1.99)	(2.07)	(−0.09)
Alpha	−0.00	0.03	−0.00	0.00	0.01	0.00	−0.00	−0.01	−0.01	0.06	0.06
	(−0.01)	(0.75)	(−0.11)	(0.03)	(0.19)	(0.04)	(−0.08)	(−0.24)	(−0.47)	(0.83)	(0.52)

Panel C: Long-term reversal (48,13) decile sorts

	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
Ave. Return	0.46	0.35	0.30	0.28	0.27	0.26	0.23	0.22	0.24	0.27	−0.19
	(2.74)	(2.62)	(2.64)	(2.43)	(2.37)	(2.30)	(1.99)	(1.83)	(1.96)	(2.08)	(−1.84)
Alpha	0.19	0.09	0.07	0.03	0.03	0.01	−0.02	−0.05	−0.03	−0.01	−0.20
	(2.10)	(1.94)	(1.72)	(1.18)	(0.74)	(0.44)	(−0.77)	(−1.43)	(−0.94)	(−0.16)	(−1.90)

Table 19: Long-term reversal decile portfolios across databases – extended sample. The table presents average excess returns (Ave. Return) and alphas (Alpha) across decile portfolios sorted on long-term reversals. Panel A reports results for the Lehman Brothers database (LHM) spanning 1977:01 to 1996:12, Panel B uses the LHM and the Bank of America Merrill Lynch database provided by Intercontinental Exchange (ICE) spanning 1977:01 to 2002:07, Panel C is based on the TRACE data as provided by WRDS spanning 2002:08 to 2022:09, Panel D uses the ICE data spanning 2002:08 to 2022:09, and Panel E refers to the full sample of the ICE data from 1997:01 to 2022:09. The first row of each panel reports the average one-month ahead excess returns on decile portfolios sorted from low to high on the respective signal. The portfolios are value-weighted by bond amount outstanding. In the second row, we report the alphas from the single-factor CAPMB model, which includes the bond market factor as the only control. We report Newey-West t-statistics with 12 lags in parentheses.

Panel A: LHM data 1977:01–1996:12

	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
Ave. Return	0.32	0.22	0.21	0.21	0.21	0.22	0.23	0.20	0.21	0.23	−0.09
	(1.48)	(1.04)	(1.00)	(0.98)	(0.98)	(1.02)	(1.06)	(0.94)	(0.99)	(1.09)	(−1.08)
Alpha	0.07	−0.04	−0.05	−0.06	−0.06	−0.05	−0.04	−0.07	−0.05	−0.02	−0.09
	(1.17)	(−1.02)	(−1.44)	(−1.68)	(−1.86)	(−1.72)	(−1.43)	(−2.35)	(−1.51)	(−0.40)	(−1.04)

Panel B: LHM and ICE data 1977:01–2002:07

	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
Ave. Return	0.34	0.21	0.21	0.22	0.21	0.22	0.22	0.19	0.19	0.20	−0.14
	(1.95)	(1.28)	(1.28)	(1.25)	(1.24)	(1.28)	(1.26)	(1.13)	(1.12)	(1.16)	(−1.92)
Alpha	0.13	−0.01	−0.01	−0.01	−0.02	−0.01	−0.02	−0.04	−0.04	−0.02	−0.15
	(2.06)	(−0.26)	(−0.42)	(−0.41)	(−0.69)	(−0.45)	(−0.60)	(−1.45)	(−1.27)	(−0.53)	(−1.90)

Panel C: WRDS data 2002:08–2022:09

	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
Ave. Return	0.70	0.52	0.46	0.34	0.32	0.30	0.30	0.25	0.28	0.39	−0.31
	(2.28)	(2.46)	(2.51)	(2.59)	(2.52)	(2.51)	(2.43)	(1.59)	(1.65)	(2.00)	(−1.62)
Alpha	0.24	0.10	0.09	0.07	0.05	0.05	0.02	−0.09	−0.13	−0.09	−0.32
	(1.60)	(0.95)	(1.28)	(1.56)	(1.67)	(1.06)	(0.53)	(−1.63)	(−1.78)	(−0.92)	(−1.80)

Panel D: ICE data 2002:08–2022:09

	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
Ave. Return	0.61	0.53	0.42	0.35	0.33	0.31	0.25	0.25	0.31	0.36	−0.25
	(1.99)	(2.40)	(2.77)	(2.52)	(2.51)	(2.29)	(1.66)	(1.54)	(1.70)	(1.80)	(−1.16)
Alpha	0.18	0.16	0.17	0.09	0.09	0.02	−0.07	−0.11	−0.10	−0.10	−0.28
	(0.93)	(1.73)	(2.52)	(2.44)	(1.23)	(0.49)	(−1.13)	(−1.66)	(−1.46)	(−1.02)	(−1.18)

Panel E: ICE data 1997:01–2022:09

	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
Ave. Return	0.57	0.45	0.38	0.33	0.31	0.29	0.24	0.23	0.27	0.30	−0.27
	(2.33)	(2.64)	(3.10)	(2.91)	(2.85)	(2.61)	(1.92)	(1.76)	(1.81)	(1.86)	(−1.58)
Alpha	0.22	0.15	0.16	0.10	0.09	0.05	−0.03	−0.07	−0.08	−0.08	−0.30
	(1.39)	(2.04)	(3.00)	(3.13)	(1.69)	(1.16)	(−0.65)	(−1.29)	(−1.39)	(−1.09)	(−1.61)

Table 20: Long-term reversal decile portfolios across databases – extended sample winsorized at the −10% level. The table presents average excess returns (Ave. Return) and alphas (Alpha) across decile portfolios sorted on long-term reversals. We winsorize excess bond returns at the −10% level by setting any excess bond return that is less than −10% to −10%. Panel A reports results for the Lehman Brothers database (LHM) spanning 1977:01 to 1996:12, Panel B uses the LHM and the Bank of America Merrill Lynch database provided by Intercontinental Exchange (ICE) spanning 1977:01 to 2002:07, Panel C uses the TRACE data as provided by WRDS spanning 2002:08 to 2022:09, Panel D uses the ICE data spanning 2002:08 to 2022:09, and Panel E uses the full sample of the ICE data from 1997:01 to 2022:09. The first row of each panel reports the average one-month ahead winsorized excess returns on decile portfolios sorted from low to high on the respective signal. The portfolios are value-weighted by bond amount outstanding. In the second row, we report the alphas from the single-factor CAPMB model, which includes the bond market factor as the only control. We report Newey-West t-statistics with 12 lags in parentheses.

Panel A: LHM data 1977:01–1996:12

	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
Ave. Return	0.40	0.22	0.22	0.22	0.22	0.22	0.24	0.21	0.22	0.27	−0.13
	(1.82)	(1.08)	(1.04)	(1.01)	(1.02)	(1.05)	(1.10)	(0.98)	(1.08)	(1.30)	(−1.68)
Alpha	0.15	−0.03	−0.05	−0.05	−0.05	−0.04	−0.03	−0.06	−0.04	0.03	−0.13
	(2.47)	(−0.82)	(−1.24)	(−1.48)	(−1.62)	(−1.46)	(−1.04)	(−1.96)	(−0.98)	(0.72)	(−1.56)

Panel B: LHM and ICE data 1977:01–2002:07

	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
Ave. Return	0.47	0.25	0.24	0.24	0.23	0.23	0.23	0.21	0.21	0.26	−0.21
	(2.56)	(1.50)	(1.43)	(1.39)	(1.35)	(1.36)	(1.38)	(1.23)	(1.29)	(1.52)	(−2.67)
Alpha	0.26	0.03	0.01	0.01	−0.00	0.00	0.00	−0.02	−0.01	0.04	−0.22
	(3.23)	(0.64)	(0.31)	(0.24)	(−0.02)	(0.02)	(0.15)	(−0.75)	(−0.37)	(1.28)	(−2.59)

Panel C: WRDS data 2002:08–2022:09

	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
Ave. Return	1.04	0.64	0.53	0.40	0.37	0.33	0.34	0.31	0.36	0.53	−0.51
	(3.21)	(2.77)	(2.58)	(2.91)	(2.77)	(2.70)	(2.70)	(2.08)	(2.12)	(2.63)	(−2.48)
Alpha	0.62	0.27	0.18	0.14	0.11	0.09	0.07	−0.01	−0.02	0.11	−0.51
	(3.54)	(2.90)	(2.26)	(2.94)	(2.44)	(1.45)	(1.19)	(−0.08)	(−0.25)	(1.34)	(−3.03)

Panel D: ICE data 2002:08–2022:09

	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
Ave. Return	0.95	0.65	0.46	0.39	0.37	0.35	0.29	0.30	0.38	0.51	−0.44
	(2.88)	(2.54)	(2.86)	(2.78)	(2.64)	(2.51)	(1.96)	(1.94)	(2.11)	(2.52)	(−1.78)
Alpha	0.57	0.33	0.21	0.14	0.13	0.07	−0.01	−0.04	0.00	0.11	−0.45
	(3.19)	(2.41)	(2.45)	(2.65)	(1.42)	(1.05)	(−0.20)	(−0.51)	(0.02)	(1.42)	(−2.19)

Panel E: ICE data 1997:01–2022:09

	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
Ave. Return	0.90	0.58	0.43	0.38	0.35	0.33	0.28	0.28	0.34	0.44	−0.46
	(3.40)	(2.87)	(3.29)	(3.27)	(3.05)	(2.89)	(2.29)	(2.20)	(2.29)	(2.68)	(−2.32)
Alpha	0.59	0.31	0.22	0.16	0.14	0.09	0.02	−0.00	0.02	0.11	−0.48
	(3.96)	(2.91)	(3.04)	(3.43)	(1.96)	(1.70)	(0.39)	(−0.06)	(0.25)	(1.74)	(−2.85)

Table 21: Market microstructure adjusted vs. unadjusted portfolio sorts. The table presents single-factor CAPMB alphas across decile portfolios sorted on bond yield (Panel A), credit spread (Panel B), the six-month change in log credit spreads (Panel C), and bond price (Panel D). The row denoted by TRACE presents the alphas across the decile portfolios using the TRACE transaction-based bond data with characteristics that have not been purged of MMN. The row denoted by TRACE* forms portfolios with characteristics that have been adjusted for MMN. The row denoted by ICE forms portfolios with the Bank of America Merrill Lynch data provided by Intercontinental Exchange (ICE), which is largely free of MMN. For all rows, the data spans the period 2002:09 to 2022:09 (241 months). The TRACE (TRACE*) portfolios are value-weighted by their unadjusted (MMN-adjusted) bond market capitalization. We report Newey-West t-statistics with 12 lags in parentheses.

Panel A: Bond yield

	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
TRACE	−0.10	−0.03	−0.03	−0.04	−0.09	−0.08	−0.02	0.05	0.15	0.40	0.49
	(−2.07)	(−1.13)	(−0.82)	(−1.07)	(−2.19)	(−1.50)	(−0.34)	(1.22)	(2.51)	(1.64)	(1.86)
TRACE*	0.02	0.01	0.00	−0.03	−0.08	−0.09	−0.05	0.02	0.11	0.30	0.28
	(0.66)	(0.47)	(0.01)	(−0.91)	(−2.10)	(−1.90)	(−0.93)	(0.60)	(1.81)	(1.28)	(1.12)
ICE	−0.01	−0.00	−0.01	−0.05	−0.10	−0.13	−0.07	0.00	0.15	0.33	0.34
	(−0.34)	(−0.07)	(−0.26)	(−1.30)	(−2.57)	(−2.37)	(−1.38)	(0.13)	(2.68)	(1.48)	(1.43)

Panel B: Bond credit spread

	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
TRACE	−0.09	−0.08	−0.08	−0.05	−0.03	−0.02	0.02	0.08	0.16	0.37	0.46
	(−2.71)	(−2.09)	(−1.71)	(−1.15)	(−0.63)	(−0.45)	(0.39)	(1.50)	(2.21)	(1.55)	(1.75)
TRACE*	0.01	−0.01	−0.03	−0.03	−0.05	−0.07	−0.03	0.03	0.12	0.28	0.27
	(0.35)	(−0.39)	(−0.81)	(−0.76)	(−0.94)	(−1.59)	(−0.79)	(0.65)	(1.73)	(1.23)	(1.08)
ICE	−0.03	−0.03	−0.06	−0.05	−0.05	−0.06	−0.08	0.06	0.14	0.30	0.33
	(−0.90)	(−1.00)	(−1.44)	(−1.07)	(−0.98)	(−1.21)	(−1.35)	(1.17)	(2.31)	(1.38)	(1.38)

Panel C: 6-month change in log credit spread

	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
TRACE	−0.28	−0.22	−0.17	−0.13	−0.06	0.03	0.06	0.14	0.23	0.42	0.70
	(−3.62)	(−3.98)	(−3.53)	(−2.6)	(−1.42)	(0.60)	(1.64)	(2.57)	(3.58)	(2.68)	(3.46)
TRACE*	−0.08	−0.09	−0.07	−0.05	−0.05	0.00	0.05	0.07	0.12	0.18	0.25
	(−1.24)	(−1.81)	(−1.39)	(−1.02)	(−1.35)	(0.15)	(1.2)	(1.5)	(1.64)	(1.14)	(1.35)
ICE	−0.18	−0.13	−0.11	−0.08	−0.03	0.02	0.06	0.10	0.12	0.25	0.44
	(−2.17)	(−2.79)	(−2.43)	(−1.70)	(−0.77)	(0.55)	(1.41)	(2.38)	(2.00)	(1.77)	(2.22)

Panel D: Bond price

	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q10-Q1
TRACE	0.37	0.12	0.03	0.01	−0.01	−0.00	−0.01	−0.03	−0.09	−0.22	−0.59
	(1.82)	(1.86)	(0.84)	(0.42)	(−0.29)	(−0.12)	(−0.37)	(−0.62)	(−1.17)	(−2.57)	(−2.18)
TRACE*	0.28	0.07	−0.02	−0.02	−0.01	−0.00	0.01	−0.01	−0.05	−0.09	−0.37
	(1.46)	(1.11)	(−0.34)	(−0.44)	(−0.33)	(−0.04)	(0.33)	(−0.17)	(−0.69)	(−1.15)	(−1.48)
ICE	0.32	0.10	0.00	−0.02	0.02	−0.00	−0.02	−0.05	−0.09	−0.14	−0.45
	(1.75)	(1.61)	(0.07)	(−0.37)	(0.69)	(−0.10)	(−0.63)	(−0.95)	(−1.19)	(−1.67)	(−1.83)

Figure 1: Bond bid-ask bias and serial autocorrelation over time.

The figure plots the within-month averages of the daily bond bid-ask bias in basis points (Panel A) and the 12-month moving average of within-month bond return serial autocorrelation (Panel B). The sample period is from 2002:07 to 2022:09 (243 months). The daily data is from the Trade Reporting and Compliance Engine (TRACE) database.

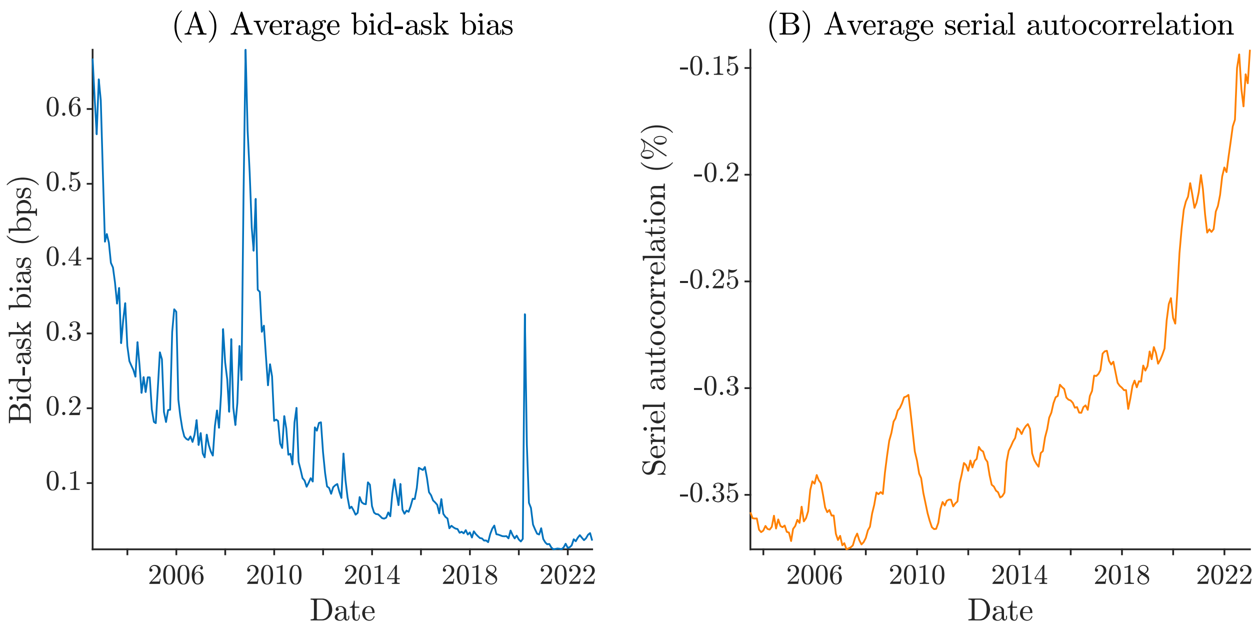

Figure 2: Transaction timing of prices used for the bond short-term reversal signal.

Transaction timing of prices used for the bond short-term reversal signal. The figure shows an hypothetical example of how daily bond prices are used to construct the bond short-term reversal signal for month t that is purged of MMN. For any month t, an investor sources the daily bond price on the first available trading day d, $p_{t,d}$. Thereafter, she locates the bond price at least one trading day before the end-of-the-month transaction price P_t, which is denoted by $p_{t,d+n}$. The end-of-the-month transaction price P_t must be within the last five business days of the month. The bond short-term reversal signal for month t is then defined as $\frac{p_{t,d+n}+c_t}{p_{t,d}} - 1$, where c_t is the coupon payment (if any) over the month. We define p to include any accrued interest. This ensures that the signal prices p are never included in the realized out-of-sample bond returns that use the bond prices P_t in the last five business days of month t.

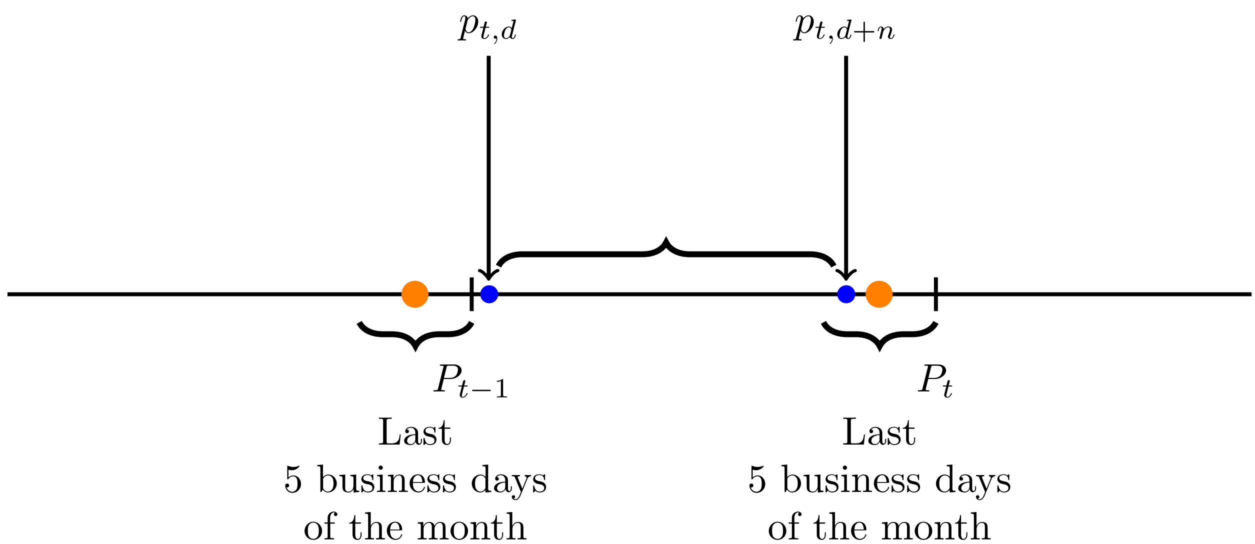

Figure 3: Bond short-term reversal factors.

The figure plots the three short-term reversal factors. Panels A and B present the reversal factors constructed with the Trade Reporting and Compliance Engine (TRACE) database (where the reversal signal is purged of microstructure noise, REV^*) and with the Bank of America Merrill Lynch database provided by Intercontinental Exchange (ICE), REV. Panels C and D present the reversal factor (REV^{MMN}) based on the TRACE database where the reversal signal is not purged of microstructure noise along with the MMN-corrected REV^* factor. The sample period for all factors is 2002:09 to 2022:09 (241 months).

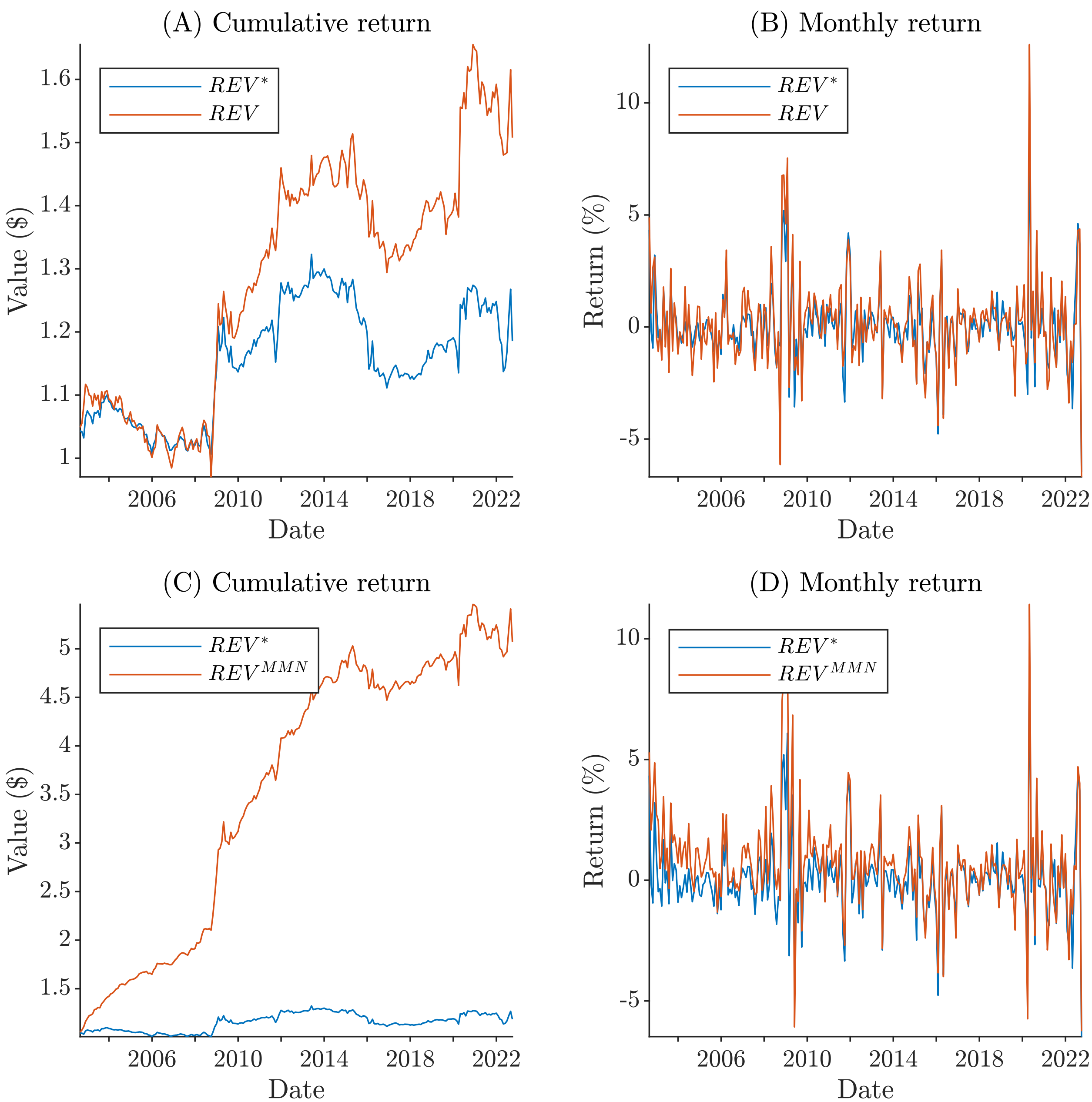

Figure 4: Bond short-term reversal factors over time.

The figure plots the Trade Reporting and Compliance Engine (TRACE) bond short-term reversal factor (REV^{*}, blue line) that is purged of market microstructure noise (MMN) and the corresponding TRACE factor with MMN (REV^{MMN}, red line) over four sub-periods. The REV^{*} factor is based on a short-term reversal signal that is largely free of microstructure bias. REV^{MMN} is constructed using the prior-month bond returns as the signal and contains noise. The full sample period is from 2002:09 to 2022:09 (241 months). The returns are monthly and presented in percent.

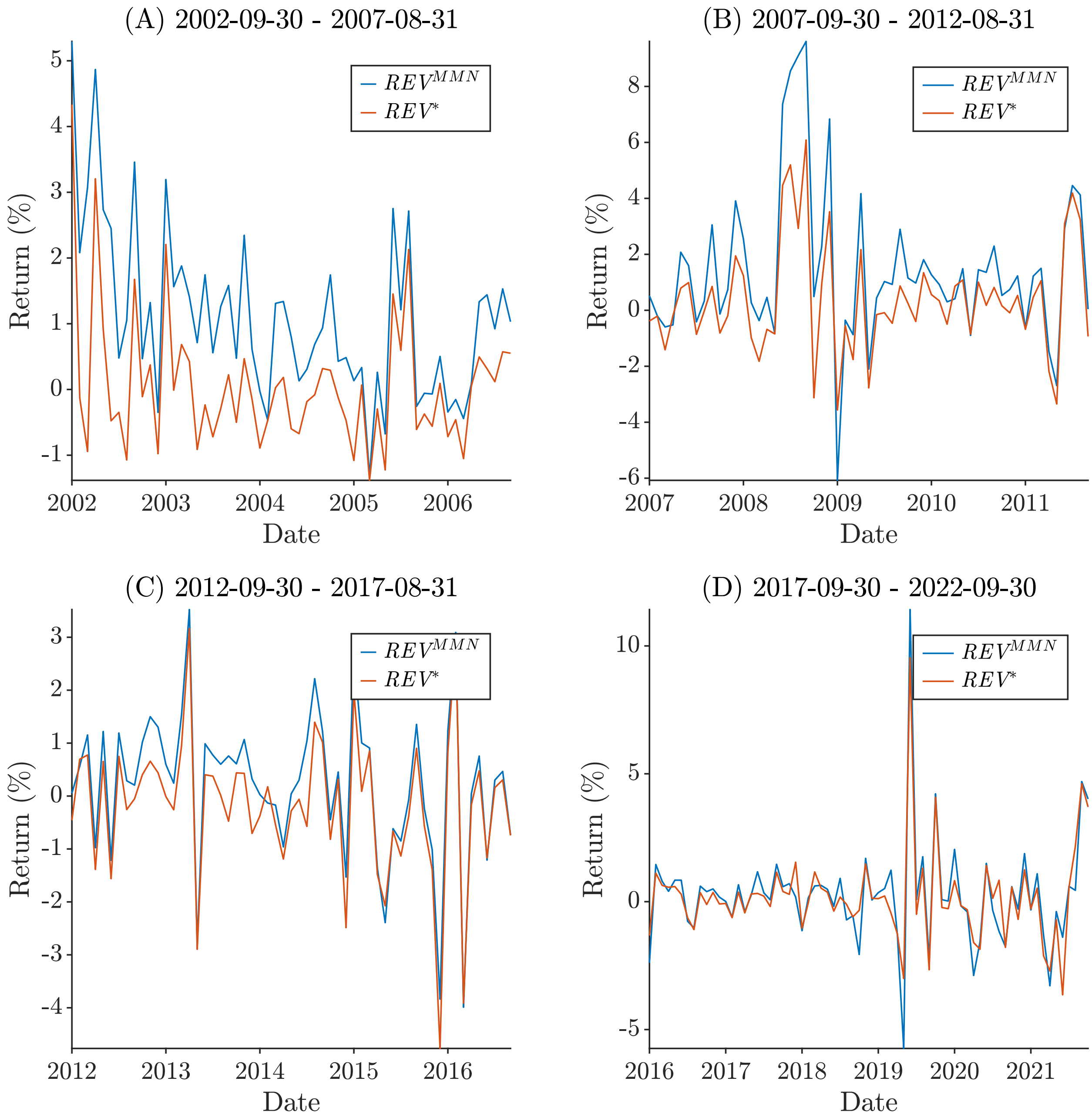

Figure 5: Jostova, Nikolova, Philipov, and Stahel (2013, JNPS) momentum comparison.

The figure plots the publicly available JNPS momentum (MOM) spread (blue line) and our spread (DRR, orange line). We follow the method outlined in JNPS to construct the momentum-sorted decile portfolios and associated high-minus-low (HL) spread. In Panels A and B, when computing the DRR MOM spread, we use excess returns that have not been trimmed, winsorized, or altered in any way. In Panels C and D, when computing the DRR MOM spread, we use excess returns that have been trimmed (ex-post) at the 99.5$^{\text{th}}$ percentile. Panels A and C report the cumulative dollar value of the JNPS MOM spread and the untrimmed (trimmed) DRR MOM spread, respectively. Panels B and D report the monthly excess returns (in percent) on the JNPS MOM spread and the untrimmed (trimmed) DRR MOM spread, respectively. The sample period is 1974:01 to 2011:06 as in JNPS. The data comprises the Lehman Brothers database (LHM) spanning 1974:01 to 1996:12 and the Bank of America Merrill Lynch database provided by Intercontinental Exchange (ICE) spanning 1997:01 to 2011:06.

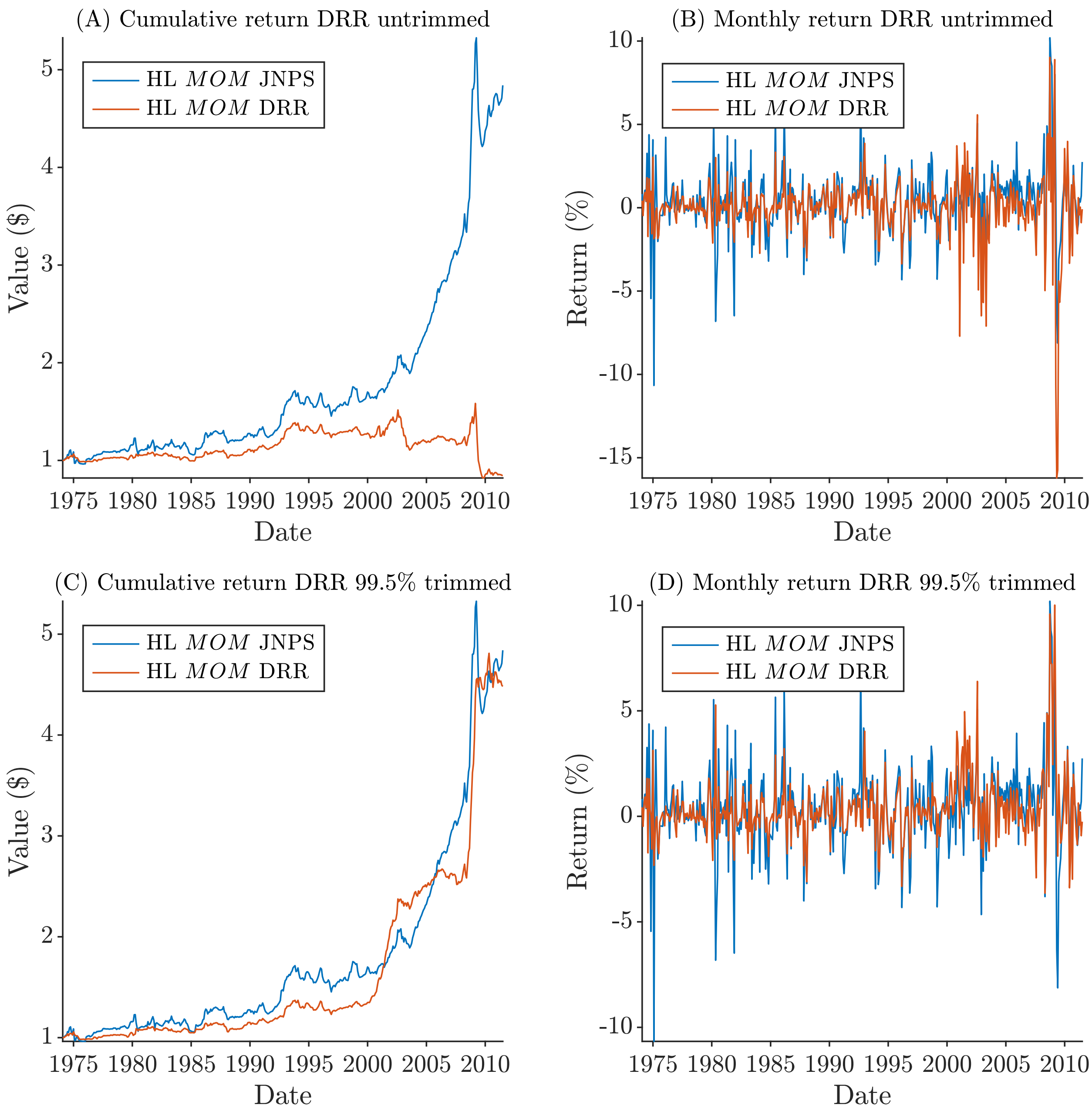

Figure 6: Transaction timing of prices used for MMN-adjusted priced-based signals.

Transaction timing of prices used for MMN-adjusted priced-based signals. The figure shows an hypothetical example of how daily bond prices are used to construct bond price-based signals for month t that are purged of MMN. For any month t, an investor sources the daily bond price at least one trading day d before the end-of-the-month transaction price P_t. This price, denoted by $p_{t,d-1}$, is used to compute any price-based signal, i.e., credit spreads or bond yields. The trader observes the signal and purchases the given bond at price P_t, and she will realize a return over the following month of $\frac{P_{t+1}}{P_t} - 1$. The end-of-the-month transaction prices P_t must be within the last five business days of the month. This ensures that the signal prices $p_{t,d-1}$ are never included in the realized out-of-sample bond returns that use the bond prices P_t in the last five business days of month t.