Smoke without fire? Reassessing empirical evidence of fire sales*

PRELIMINARY AND INCOMPLETE, DO NOT CITE OR CIRCULATE WITHOUT AUTHORS' PERMISSION

R. M. Bidder[a], J. Coen[b], C. Lepore[c], L. Silvestri[d]

[a]*King's Business School*
[b]*London School of Economics*
[c]*International Monetary Fund*
[d]*Bank of England*

Abstract

We set out statistically and economically significant evidence of fire sales in bond markets. Using novel data on the transactions of financial firms in corporate and government bonds in the UK, we develop a new measure of non-fundamental selling pressure. We instrument for firms' sales of a bond with their sales of bonds *other than the bond in question* and exploit within issuer-time variation to identify selling pressure that is unrelated to the price-relevant fundamentals of a bond. We find that exogenous sales of a bond can have large impacts on bond prices. We show how these effects vary according to the type of the bond, the state of the financial system, and the type of firms selling the bond. The effects are amplified in the case of corporate—rather than government—bonds, as well as in times of stress. Sales by asset managers do not appear to cause large price falls, in line with recent work on sales by distressed mutual funds. Thus our results suggest that whilst fire sales by distressed funds may only have minor effects, fire sales by several market participants remain a cause for concern.

*Disclaimer:** Any views expressed are solely those of the authors and so cannot be taken to represent those of the Bank of England or to state Bank of England policy. This paper should therefore not be reported as representing the views of the Bank of England or members of the Monetary Policy Committee, Financial Policy Committee or Prudential Regulation Committee. Nor should they be attributed to the IMF, its board or its management, nor King's Business School. We are grateful to Raouf Atia and Stephen Newell for their help with the data. We are grateful for comments by Saleem Bahaj, Simon Jurkatis, Marcin Kacperczyk, Patrick Coen, Nick Vause and seminar participants at the Bank of England, the Royal Economic Society Annual Conference, the 6th International Workshop on Financial Markets and Nonlinear Dynamics, the 2022 FSB-IOSCO Conference on Vulnerabilities from Liquidity Mismatch in Open-Ended Funds, and the 2022 European Meeting of the Econometric Society.

1. Introduction

Fire sales are feared—by financial institutions selling in them, by regulators who hope to prevent them and by the public caught up in their repercussions. Economists study them closely and, until recently, had ostensibly developed coherent frameworks to understand them and an accompanying set of apparently reliable empirical results (see Shleifer and Vishny (2011) for a thorough survey).

After being fairly stable for some time, however, the empirical literature on fire sales is now in a state of flux and several key questions no longer have settled answers. Do fire sales by mutual funds cause economically significant price impacts? Is the answer the same for sales by a broader class of investors? How might the answer depend on characteristics of the asset or the context in which sales occur? We address all these questions, using granular data on market transactions and methods that avoid existing methodological flaws in the literature.

In terms of methodology, we construct a novel index of 'outside selling pressure' at the bond-time level—so called because it is based on sales, by investors trading a given bond, of bonds *other than the bond of interest*. We then use this to instrument sales in the bond of interest. Intuitively, if a bond is being sold by investors who are typically heavily selling other 'unrelated' bonds, then it proxies for the sort of distress that underpins fire sales. We implement this methodology using a dataset on the universe of transactions in government and corporate bonds by entities regulated by the UK's Financial Conduct Authority.

Of course, the word 'unrelated' is important and, in its simplest form, our measure defines 'unrelated' in a stark way—simply not being the bond in question. One might plausibly be concerned that bonds that are in important respects related (such as coming from common industries) will impart confounding variation to our measure, reflecting information on common price-relevant fundamentals. While one response to this problem would be to define ‘unrelated’ in a more elaborate way (selecting the ‘unrelated’ bonds according to some rule), that approach is fraught with difficulty and would always be subject to concerns of unobserved (or unknown) sources of endogeneity. Instead, we incorporate *issuer-time* fixed effects in our analysis, to eliminate variation common across the securities of a given issuer.[1] Thus, our concept of ‘unrelated’ is ultimately synthetic. Our analysis relies on averaging over the (very) large number in our sample of comparisons of the form ‘Company X bond A vs Company X bond B’ and exploiting how heavily sold are other bonds traded by investors trading bond A or bond B. Beyond this, though most identification-confounding stories would naturally be at the issuer level, we include further controls to absorb any variation arising from credit enhancement or maturity—*fundamental* price relevant factors that may vary across bond *and* within issuer-time.

Having thus eliminated fundamental sources of variation in sales, we are left with a reliable measure of non-fundamental variation. The question then is, might we have absorbed ‘too much’? Is there any non-fundamental variation left (as our identification approach likely eliminates some desired *non-fundamental* variation too) to allow fire sales effects to be picked up, even if they exist? In fact, our approach retains ample non-fundamental variation and display interesting economic and statistical significance. We find an important role for fire sales, but that their prevalence and severity depend very much on context. Our headline ‘unconditional’ result is that a move from the 5^{th} to the 95^{th} percentile of our measure causes a 64bps decline in returns. However, digging into this result reveals interesting patterns. The price impact is four times greater in times of broader market stress (in our case captured around the ‘dash for cash’ in the Spring of 2020) and, again consistent with theories of illiquidity, the impact appears larger for corporate bonds than for sovereigns.

[1] For brevity, we will henceforth compact issuer-time to issuer in our prose, but will be explicit in later sections where this distinction is important.

Indeed, combining these dimensions, it appears that the price impact of selling corporate bonds spikes in times of stress, but the effect for sovereigns does not.

As we will shortly discuss, much existing research in this area has concentrated on data in relation to mutual funds (see for instance Coval and Stafford, 2007; and Edmans, Goldstein, and Jiang, 2012). In turn, the recent re-evaluation of the evidence of fire sales has been focused in this domain, apparently questioning whether there are substantial effects after all (Wardlaw, 2020; Choi, Hoseinzade, Shin, and Tehranian, 2020). When we condition on sales by asset managers, of whom mutual funds are an important subset,[2] we obtain results more in line with the recent literature in that we do not find evidence of this type of selling driving down prices (indeed we estimate a small positive effect). For sales by other market participants, however, we estimate an economically and statistically significant negative effect. As such, we reconcile recent mutual fund-centric (non) results with intuitively convincing theoretical models that would suggest an important role for fire sales, and with the significant emphasis put on them by regulators in their efforts to avoid them.

2. Literature

Theoretical models point to various mechanisms that could 'force' constrained agents to sell assets at prices below 'fundamentals'. For instance, margin calls due to a decline in collateral value as securities prices fall might lead financial institutions to sell securities, leading to further price falls (Brunnermeier and Pedersen, 2009). Leveraged financial institutions, such as banks and hedge funds, might need to deleverage by selling assets when subject to capital losses (Greenwood, Landier, and Thesmar, 2015). There is also empirical evidence

[2]It should be noted that given the way counterparty identities are reported in the data we are not able to separately identify trades by open-ended investment funds from those by other types of mutual fund. We are able to identify asset managers whose trading activity includes that of open-ended funds too.

of investors in corporate bond open-ended funds redeeming when fund performance is poor (Goldstein, Jiang, and Ng, 2017). As open-ended funds offer their investors the possibility of redeeming their shares at any time, they might be forced to sell assets to comply with redemptions. At the same time, while agents are forced to sell assets, natural buyers may be constrained (Shleifer and Vishny, 1992).

Furthermore, there might be limits to dealers' willingness and ability to intermediate between buyers and sellers, due to balance-sheet capacity constraints. This channel has been particularly debated recently, in view of post-crisis regulatory constraints on dealers' balance sheets and the market disruption observed in the COVID-19 crisis.[3]

Empirically, the difficulty of assessing the impact of such 'non-fundamental' sales arises from the plausibly close correlation between the impetus for the sale of an asset and factors that would fundamentally influence its price, even in situations without any frictions or constraints that might lead to fire sales. Simply put, bad assets might be sold and the drop in price observed may simply reflect the encoding of information on impaired cashflows, say, in the clearing price of a well functioning market.

In an influential paper, Coval and Stafford (2007) provide an analysis of the price effect of fire sales in the stock market by open ended mutual funds induced by investor outflows, indicating that there was a non-fundamental negative effect on prices before an eventual return to the fundamentals-determined level. While the analysis has since been refined (notably by Edmans, Goldstein, and Jiang (2012)), the thrust of the paper and its essential method reached a level of consensus and was broadly adopted. Indeed, the applications were very

[3]During the onset of the epidemic, dealers were not willing or able to fully absorb the increased sales onto their balance sheets, blocking the plumbing between market participants. Using a theoretical model, He, Nagel, and Song (2021) explain that post-crisis regulation, in particular the leverage ratio, may have constrained dealers' ability to expand their balance sheets via direct holdings or repo. For empirics and further analysis, see Kargar, Lester, Lindsay, Liu, Weill, and Zúñiga (2020), Schrimpf, Shin, and Sushko (2020) and Duffie (2020). We return to this debate later.

broad: if one can obtain a measure of non-fundamental price changes, it opens the door to studies of the real effects of financial markets, measures of liquidity in markets and the effects or design of regulations. Being able to identify an exogenous shift in supply of assets is vital in getting a sense of the shape of the demand curve—a key object from which many other phenomena derive.

More recently, further analysis has reopened debates on the nature, or even existence, of fire sales. Two papers in particular help set the stage for our own analysis. In a disruptive paper, Wardlaw (2020) notes that the construction of many of the proxies for forced selling used in the literature is fundamentally flawed, primarily because components of the measure are *mechanically* guaranteed to be correlated with realized returns, and those components that are not show very little correlation. That is, when using a measure that is stripped of meaningless correlation, there is little evidence of the price effects previously claimed. Indeed, this would also undermine, *inter alia*, derived research that exploit the claimed price effects to assess induced real impacts on firms—such as buyout activity.

Choi, Hoseinzade, Shin, and Tehranian (2020) use issuer-level fixed effects to eliminate any selection based on fundamentals at the issuer-level. That is, only within-issuer, cross-security variation is exploited such that any concerns that (fundamental) price-relevant variation at the issuer level is eliminated as a source of endogeneity.[4] Under this approach, which we extend, evidence of fire sales effects appears minimal.[5] Our results are in line with their findings on

[4]Note that this is an alternative approach to that of Edmans, Goldstein, and Jiang (2012) who instrument sales in the current period with sales implied under *lagged* portfolio structure interacted with fund size, so as to avoid selection effects due to discretionary selling on the basis of funds' recent/current knowledge. If one regards selection as likely being on an issuer-by-issuer basis, rather than a security-by-security basis, then Choi, Hoseinzade, Shin, and Tehranian (2020) also avoid any contamination from selection. We shall discuss this further below in section 4.

[5]They attribute their findings to bond funds' liquidity management strategies, including maintaining liquidity buffers and selectively trading liquid assets, which allows them to absorb investor redemptions without resorting to fire sales. Relatedly, Anand, Jotikasthira, and Venkataraman (2020) show that liquidity-supplying mutual funds maintain their relative trading style when facing large outflows, thus alleviating rather

the relatively limited price impact from funds' sales—indeed if anything we estimate a small positive effect. However, when looking at a broader set of investor types (including banks, hedge funds and other asset managers) we find a negative and much larger price impact.

Further, we analyse how our results change in times of stress. During the turmoil in March 2020 the price impact of fire sales becomes significantly larger, in particular for corporate bonds. Thus, our paper contributes to the empirical literature analysing selling pressures in bond markets at the height of the COVID-19 pandemic. During this time bond markets showed signs of liquidity pressure, as documented by several recent papers. Kargar, Lester, Lindsay, Liu, Weill, and Zúñiga (2020) show that trading costs for US corporate debt increased in March 2020 and Haddad, Moreira, and Muir (2020) show that market disruptions were most salient in the investment-grade segment. As the crisis progressed in mid-March even advanced economy government bonds, which initially appreciated due to flight-to-safety, experienced a snapback in yields and extreme turbulence. For example, Schrimpf, Shin, and Sushko (2020) report that the spread between Treasury yields and swap rates widened dramatically in mid-March. Dislocations were concentrated in long-dated Treasuries, while T-Bills were less affected and served as a safe haven providing a hedging instrument against stock market falls (Cheema, Faff, and Szulczyk, 2020). While these studies clearly document a deterioration in liquidity conditions for bond markets during March 2020, they do not provide conclusive evidence of fire sales. However, as emphasized in Falato, Hortacsu, Li, and Shin (forthcoming), bond markets are arguably especially prone to fire sales and spillovers and, given the degree of disruption in even traditionally safe segments, it is plausible that they were at play.[6]

than threatening market stability. More recent research on the effects of fund-induced fire sales also finds a limited price effect—see Czech, Koosakul, and Vause (2021) for example.

[6]See also Ambastha, Ben Dor, Dynkin, Hyman, and Konstantinovsky (2010), Bao, Pan, and Wang (2011) and Goldstein, Jiang, and Ng (2017) for analyses of bond market liquidity and fragility.

3. Data

The core dataset we use is the universe of transactions in government and corporate bonds by entities regulated by the Financial Conduct Authority (FCA), required to be submitted under the MiFID II directives.[7] It is worth emphasizing that, while the dataset relates to FCA-regulated entities, the bonds are not required to be those of British issuers. Further, only one counterparty in each transaction need be regulated by the FCA in this way, so many non-FCA-regulated entities feature in the data.

The data contain detailed information on each transaction, such as the timestamp, volume, price, instrument traded and the identity of the two counterparties. We match the data with a hand-collected mapping that associates to each counterparty a financial sector (e.g., dealer, asset-manager, insurance company and pension fund, hedge fund and so on).[8]

Table 1 displays summary statistics on the bonds and traders in our sample. Around 85% of the bonds in our sample are corporate bonds, with the remainder government bonds. Government bonds are traded more frequently, and account for slightly over half the trades in ours sample. 80% of the instruments and over 90% of the trades are in sterling, euro or dollar instruments. 3% of traders are dealers but they account for half of total trading. The rest of trading is accounted for largely by asset managers, non-dealer banks and trading services firms such as brokerage firms. This is consistent with the fact that both government and corporate bonds are mainly traded over-the-counter (OTC) and rely on dealers to intermediate between buyers and sellers. In the sterling government bond (gilt) market there are designated market-makers called Gilt-edged Market Makers (GEMMs) that are required to make markets in

[7]See https://www.esma.europa.eu/policy-rules/mifid-ii-and-mifir.

[8]We are unable to identify individual open-ended funds in our transaction data. Instead we can identify asset managers, who might be trading on behalf of open-ended funds and other types of funds.

all conditions.[9] Most of the transactions in the sterling corporate bond market are OTC (Baranova, Douglas, and Silvestri, 2019).

We adopt a much finer observation frequency than is typical in the literature (which commonly is at quarterly or monthly frequency) in aggregating trade data to the weekly frequency. Table 2 summarises trading in our weekly dataset. A large number of unique bonds and traders trade each week.[10] Each dealer that trades a bond in a week trades a large number of bonds, whilst non-dealers—who we call customers henceforth—on average trade 8 unique bonds per week. Each instrument that trades in a week is on average traded by 4 dealers and 10 customers. These features of trading—the fact that several traders trade the same bond in a week and each trader trades several bonds in a week—will be critical for our approach to identifying exogenous selling pressure in a bond.

We merge this data with bond information from Eikon fixed income data, providing key characteristics of the securities.

In complementary analysis (see the end of section 4) we also make use of data on open-ended funds to replicate some of the price pressure measures associated with open-ended fund outflows that have been developed in the literature. We use daily data on total net assets (TNAs), Estimated Total Net Flows (ENFs) and quarterly data on portfolio holdings of open-ended funds from Morningstar. Daily data on TNAs and ENFs span from 1 January 2019 to 29 November 2020, while portfolio holdings data cover 2019 Q3, 2019 Q4, 2020 Q1 and 2020 Q2. Daily data on TNAs and ENFs are aggregated to a weekly frequency to align with the frequency used with transaction level data. We focus on funds investing in fixed-income instruments that might be covered in our transaction level data. The funds selected

[9]For more information on GEMMs and the gilt market see the UK Debt Management Office website.

[10]Unique bonds are defined by their International Securities Identification Number (ISIN) and unqiue traders by their Legal Entity Identifier (LEI).

in Morningstar hold between 38-52% of bonds traded in our transaction dataset, as shown in Table A1.

4. Research design

We are concerned with estimating the effect of sales of securities on their price, in the case where those sales are motivated by 'non-fundamental' reasons. If we were confident that all sales were for such non-fundamental reasons then the associated regression specification would be

$$r_{i,t+\tau} = \beta s_{i,t} + \gamma' X_{i,t} + \epsilon_{i,t} \tag{1}$$

where $r_{i,t+\tau}$ is the return on security (in our case, bond) i from t to $t+\tau$, $s_{i,t}$ is net sales by non-dealers, scaled by average turnover in the bond, $X_{i,t}$ is a vector of controls and $\epsilon_{i,t}$ is a disturbance term. Note that we construct net sales by non-dealers as, were we to include *all* market participants (i.e. were we to include dealers), *net* sales would be identically zero. Our decision also reflects our desire to focus on participants without a market-making role and, thus, whose trades in some sense reflect their own initiative.

Now, it is unlikely that the covariance of $\epsilon_{i,t}$ with $s_{i,t}$ is zero. Price-relevant, fundamental information—public or private—plausibly influences the decision to sell and thus confounds the effect of selling, *per se*, on the price. As aforementioned, various approaches have been proposed to isolate exogenous variation in $s_{i,t}$, but here we propose a novel approach. Ultimately, our method will entail instrumenting $s_{i,t}$ in a particular way, using a measure we refer to as 'outside selling pressure'. We will first describe the construction of this instrument, before discussing identification at length.

Let us imagine that we have been able to identify 'unrelated' bonds. If an investor trading bond i is selling many other unrelated assets, then it suggests that her trades in

i are presumably driven, to a large degree, by the investor's condition, rather than any idiosyncratic properties of bond i. Conversely, if an investor is trading bond i for purely idiosyncratic (to the bond) reasons then, on average, her sales of other assets should be zero.

We formalize this basic intuition as follows. For bond i and trader j at time t, we compute outside selling pressure $z_{i,t}$, first by calculating net sales and transactions of all bonds, $k \neq i$ as[11]

$$z_{i,j,t}^{NS} \equiv \sum_{k \neq i} s_{k,j,t} \tag{2}$$

$$z_{i,j,t}^{T} \equiv \sum_{k \neq i} |s_{k,j,t}| \tag{3}$$

and then calculating the percentage net sales of bonds other than i by all traders j among non-dealers transacting in bond i

$$z_{i,t} \equiv \frac{\sum_j \mathbb{1}_{s_{ijt} \neq 0} z_{i,j,t}^{NS}}{\sum_j \mathbb{1}_{s_{ijt} \neq 0} z_{i,j,t}^{T}}. \tag{4}$$

Intuitively, we identify traders transacting in bond i and, among those, derive a measure of their selling tendency, using only their transactions in bonds *other* than i. If those traders are heavy sellers (buyers) overall, then the measure will be close to 1 (-1). If there is no general tendency for traders transacting in bond i to be sellers or buyers then $z_{i,t}$ will be close to 0. By construction, we have that $z_{it} \in [-1, 1]$.

While $z_{i,t}$ should be highly correlated with $s_{i,t}$, and is thus a candidate instrument, clearly the requirement that the other bonds being sold are 'unrelated' is unlikely to be satisfied in

[11] Although not strictly necessary, given the nature of the regression specifications we ultimately use, we exclude bonds other than i but which are from the same issuer in Equation 2 and Equation 3.

the first instance by simply considering bonds other than i. One can easily envisage how other assets sold may reflect shared time varying factors that both induce sales and are tied to price-relevant fundamentals. For example, an investor may have acquired a portfolio featuring similar bonds, perhaps from the same industry, so that sales in other assets may reflect the effects of fundamentals, which we must purge from our measure. If an investor is heavily selling bonds issued by Acer, and bond i is issued by Dell, then it is plausible—indeed likely—that $z_{i,t}$ encodes price relevant fundamental information regarding i.

At this point, one might attempt to refine $z_{i,t}$ by adopting a selection rule that filters the trades that feature in the calculation of $z_{i,t}$—exploiting information about the traders, bonds or the context of the trade. While in future work we will explore more elaborate selection rules to gain further insight into the nature of the non-fundamental variation in sales, we choose not to adopt this approach for the purpose of isolating exogenous, non-fundamental variation in sales. The approach relies on observable criteria, such that there would always be the concern that some unobserved factor might correlate with sales and price-relevant information. Instead, we choose to instrument sales of i with $z_{i,t}$ constructed as described above and, additionally, include a set of demanding fixed effects and controls to eliminate endogeneity concerns. Specifically, we consider the following 2SLS specification

$$r_{i,t+\tau} = \beta s_{i,t} + \gamma' X_{i,t} + \epsilon_{i,t} \tag{5}$$

$$s_{i,t} = \alpha z_{i,t} + \delta' X_{i,t} + \nu_{i,t}. \tag{6}$$

Pivotal in our specification is that the set of controls $X_{i,t}$ will include issuer-time fixed effects. That is, we exploit within issuer-time variation such that, even if $z_{i,t}$ encodes confounding variation, this variation should be absorbed. This approach, akin to that of Choi, Hoseinzade, Shin, and Tehranian (2020), means that we only exploit variation that is obtained by contrasting returns from, for example, Dell Bond A vs Dell Bond B in the same period. Any source of fundamental variation in sales that is issuer-level (or, *a fortiori*, industry level) is stripped out.

It is difficult to think of remaining fundamental variation that would survive this fixed effect, though not impossible. As such, we additionally include instrument fixed effects—which control for any time-invariant heterogeneity across bonds—and a measure of the time since issuance.[12] Once we have added these additional controls our identifying assumption is that $cov(z_{i,t}, \epsilon_{i,t}|X_{i,t}) = 0$.

At this point, the main concern with the specification is whether or not we retain enough non-absorbed variation to allow us to assess the effect of non-fundamental sales. Remaining variation is derived from reasons unrelated to the issuer or to bond-specific (fixed or time-varying) fundamentals. That is, we have purged sales of the variation that would be problematic for fire sale studies. However, it is of course possible that we have thrown the baby out with the bathwater—it could well be that investors are fire selling all bonds from a given issuer to the same degree, in which case we would have eliminated 'desired' variation.

Table 3 shows that ample variation in returns and sales remains even once we include a demanding set of fixed effects. The table shows the R-squared from regressions of returns and sales on increasingly demanding sets of fixed effects. Even the most demanding sets of fixed effects leave two-thirds of returns variation and three-quarters of variation in sales unexplained. As shown below in our results section, this remaining variation is sufficient for precise estimation of the effects of non-fundamental sales.

We now briefly discuss properties of $z_{i,t}$. In Table 4 we summarise the distribution of returns, sales and our pressure measure $z_{i,t}$. In Figure 1 we plot the fraction of bonds in a

[12]Given we include instrument fixed effects, for a bond with fixed maturity this is equivalent to controlling for time to maturity.

week whose selling pressure $z_{i,t}$ exceeds the median across the whole sample, a time series measure of average pressure in the market. We emphasize at this point, however, that our identification draws on the enormous cross sectional dimension of our data, rather than the time series alone. Additionally, heavy general selling would also be *mechanically* picked up in this 'tail fraction' based on our measure. Notwithstanding this, it is reassuring that our measure exhibits a spike during the March 2020 turmoil period which, anecdotally and in aforementioned academic studies, has been argued was associated with fire selling pressure.

Figure 2 shows the correlation between sales of an asset and our selling pressure measure, and between returns in an asset and our selling pressure. Returns are defined as the percentage change in an asset's price since the last time it was traded. Intuitively, this is a univariate version of the two-stage least squares approach we will adopt in Section 5, where we use our selling pressure as an instrument to identify the effect of sales on returns. Sales are tightly correlated with the selling pressure measure, which suggests we have a relatively strong instrument, whilst pressure is associated with lower returns.

In addition, our measure is positively correlated with the various measures of fire selling pressure in the literature derived from mutual fund flows. Table 5 shows the correlation between our selling pressure measure—calculated only for asset managers—the 'flow-to-stock' (F2S) and 'flow-to-volume' (F2V) constructed in Wardlaw (2020) and the 'mutual-fund-flow' (MFF) measure of selling pressure advocated in Coval and Stafford (2007).[13] In particular, we define $z_{i,t}^{AM}$ as our measure of outside selling pressure computed based only on sales by asset managers, and regress it on versions of the fund-flow based measures after stripping out instrument and issuer-time fixed effects. The positive relationship we find is reassuring, as it suggests our approach does capture some measure of distressed selling.

[13]See Appendix A for detailed information on these extant selling pressure measures.

5. Results

We begin with our baseline regression framework, with Table 6 showing results from our regressions of returns on sales (OLS and with sales instrumented by outside selling pressure). The importance of instrumenting is shown by the fact that, while both key coefficients of returns on sales are negative and significant, as would be intuitive if fire sales are at play, the two-stage least squares coefficient deviates by an economically meaningful amount. Note that both of these regressions include our (extremely) demanding sets of fixed effects. The F-statistic from the first stage is extremely high, which suggests our instruments are strong. For interpretability of the key coefficient (-0.004), we note that this would imply that a move from the 5^{th} to the 95^{th} percentile of selling *causes* a 64bps decline in the security price, all else equal. Naturally, this is a substantial amount and perhaps might be thought to be in tension with recent mutual fund-based results that suggest weaker or no effects—a point we will return to shortly.

Various models of fire sales imply heterogeneous effects of sales, depending on the basis of the broader context of the selling, the type of asset sold, and the nature of the agent selling the asset. We address these in turn, beginning with Table 7 in which we focus on how our results change when we only use observations from March 2020. Indeed, restricting ourselves to that period (and recalling we still have an enormous number of observations in that case) we obtain a coefficient estimate that is highly significant and is four times greater than that obtained in the whole sample. In Figure 3 we plot how the estimated response of returns to sales varies week-by-week around the 'dash for cash'. Again, we see the price impact of selling rise during the stressed period and return to normal after the financial system stabilised.[14] This

[14] For reference, dislocations at this time induced various central banks to act. For example, on March 11 the Bank of England reduced Bank Rate by 50bps to 0.25%, on March 19 the Bank voted to purchase £200*bn* gilts and reduced Bank Rate by 15bps to 0.1%, and on March 24 the Contingent Term Repo Facility was

is a striking and reassuringly intuitive result and again helps restore the empirical evidence in favor of the existence of fire sales, but now on a more sound empirical footing.

Turning to the dependence of our results on the type of asset sold, a natural division within fixed income is between corporate and government bonds.[15] Table 8 shows how our results vary across these two types of asset. The coefficient on corporate bonds essentially aligns with our full sample coefficient using all bonds. In contrast, despite still a very large sample size, we observe no statistically significant effect (and a smaller, though still negative, point estimate) for government bonds. There remains the consensus view that liquid assets should not exhibit as much of a price effect in times of forced selling. Indeed, various models suggest that this is precisely why high grade assets might be sold by distressed firms, before less liquid assets, where they might be forced to realize a loss due to fire sale-depressed prices (Coen, Lepore, and Schaanning, 2005). We thus again draw confidence from the fact that our results seem to align with what one would expected based on a 'model' of fire sales.

Given the unusual nature of behavior at some maturities of traditional 'safe' Treasuries (see Duffie, 2020) it is natural to 'take the product' of the aforementioned dimensions of heterogeneity. That is, we examine how corporate and government bond prices reacted to our instrumented selling during March 2020. As shown in Table 9, the coefficient on corporates is estimated to be even larger than its full sample counterpart, while there remains no significant result for sovereigns.

Finally, we turn to a subset of trading that is of special relevance to the literature. Mutual funds have been the focus of a large fraction of research on this topic, especially since the seminal work of Coval and Stafford (2007). Recent work, as aforementioned, has to

activated.

[15] In ongoing work we hope to divide securities more finely—such as in dimensions of complexity and credit quality.

some degree overturned—or at least disrupted—consensus that forced sales by such agents induce price effects (see Choi, Hoseinzade, Shin, and Tehranian, 2020; and Wardlaw, 2020 in particular). In Table 10 we restrict the calculation of both sales and our selling pressure measure to take account only of trades by asset managers.[16] Interestingly, when we condition on examining only pressure from asset managers, we lose our negative effect. Indeed there is a statistically significant *positive* effect. While the positive effect may be an estimation artifact, our comprehensive approach, looking across a broader set of traders than is typical—and using a powerful identification method—adds new insights to the literature and puts relatively new insights (the weak effect of mutual fund selling) on a firmer footing.

6. Discussion: Noise vs Fire Sales

Our measure of outside selling pressure, when combined with our demanding set of controls and fixed effects, convincingly extracts non-fundamental variation in trading and allows us to show interesting and theoretically plausible patterns in the effects of such variation on prices. At this point, one might ask what is the nature of the non-fundamental variation. Does it even matter if non-fundamental variation derives from 'fire selling' (constrained selling under distress), or from some other impetus to sell that is orthogonal to price-relevant fundamentals? If it does matter, does our measure plausibly capture the former, rather than the latter?

If one is purely trying to estimate the response of prices to a liquidity 'demand shock' (implicitly a shock that traces out the liquidity 'supply curve'), then our analysis plausibly achieves this, regardless of the interpretation of the non-fundamental shock we implicitly extract. Traditionally, customers (non-dealers) are seen as demanders of liquidity (they trade for their own purposes) whilst dealers are seen as suppliers of liquidity (they facilitate

[16] A single asset manager in our data will likely house open-end funds, closed-end funds and exchange-traded funds, though the bulk of their holdings will be in open-end funds of the type studied in the literature.

customers' trading needs). An OLS regression of price (our returns measure $r_{i,t}$) on a measure of quantity (net sales $s_{i,t}$) has long been understood to suffer from simultaneity bias, as they are both simultaneously determined. Our 2SLS regressions are a standard response to this: we instrument for sales using a demand shifter. As such, we can interpret our regression equation as a liquidity supply equation, $z_{i,t}$ as a demand shifter, and hence our estimates of β as an estimate of the slope of the liquidity supply curve.

Now, it is useful to interpret sales as coming from three sources: (a) 'fundamentals' trading, where firms trade based on news about the asset's future cashflows; (b) 'noise' trading, where firms trade assets *completely* randomly, for reasons uncorrelated with *anything*; and (c) 'correlated' trading, where firms trade assets in a way that is correlated across assets, but for reasons unrelated to the asset's fundamental value. Fire sales are perhaps the clearest example of how 'correlated' trading arises in that, because the firm faces constraints/distress, it sells a host of assets such that $z_{i,t}$ becomes 'large'. It is indeed difficult to envisage another source of correlated non-fundamental selling and even more difficult to articulate one that would pervade our selling pressure measure *and* not have its variation absorbed by our regressions' fixed effects and controls. Consider herding, for example, in which fads or fashion could plausibly induce correlated selling of an instrument across many firms (Cai, Han, Li, and Li, 2019). Such variation would be encoded in our measure of sales, but to the extent that herding, by definition, entails focal behavior related to a given instrument, it would not create the type of cross-instrument selling that causes our pressure measure to be large. Further, to the extent that herding operates at the issuer level, such selling pressure would be absorbed by our fixed effects.

The main concern that remains—one of interpretation, rather than identification—is whether our measure is only or predominantly capturing noise-driven demand fluctuations, which in a structural model might be thought to have different effects from fire sale-induced fluctuations. We noted that our measure seems to spike in periods of distress and dislocation—as captured in Figure 1.We also noted that our results were stronger among corporate bonds and that this was further enhanced under stress. Beyond that, our measure correlates with measures used in the literature that are constructed explicitly to focus on distressed selling. As such, it seems plausible that our measure contains a substantial component reflecting distressed selling, or 'fire sales'.

To the extent that there is both a noise-driven and fire sale-driven component in our selling pressure measure, two questions then arise. First, do the components have the same effect on prices? Second, how large are these components in relation to eachother?

In ongoing work we attempt to formulate answers to these questions. If the answer to the former is in the affirmative then the latter is much less interesting. In a structural model of fire sales, however, it is plausible that the two types of shocks have different impulse responses for a given asset and differ also in the patterns of their effects across assets and in their spillovers. For example, in models designed to characterize fire sales there is a sense in which the shock is focused on a particular set of assets, cross-held across a particular set of investors and that those assets are sold in distress, perhaps in a particular sequence (initially selling the most liquid, for example). That is, the distinction between noise and fire sales is not simply a question of magnitude, but of type. In ongoing work, we hope to refine our measure to explore how much of its variation can be tied to shocks which exhibit characteristics of fire sale shocks suggested by structural models of fire sales.

In terms of the relative size of the components in $z_{i,t}$, it seems plausible that the further one goes into the tail of selling pressure realizations, which aggregate over all traders selling asset i and the assets other than i which they sell, the less likely it is to reflect the aggregation of uncorrelated noise components in individual traders' actions. Intuitively, for our selling pressure to be 'big' one needs a correlated realization across many assets and traders, orthogonal to all of our fixed effects and controls. Of course, one needs an objective sense of 'big' because even the aggregation of noise traders' actions give rise to dispersion. As such, deriving an objective and interpretable decomposition of our pressure measure into noise and firesale pressure is our focus in ongoing work.

7. Conclusions

We have shown that there is statistically and economically significant evidence of fire sales, based on a rigorous approach to identifying non-fundamental selling pressure. Interestingly, as recent work has suggested, the connection of fire sales to distressed mutual funds' actions appears rather weak, in contrast to long held consensus views on this matter. However, looking more broadly, non-fundamental sales across market participants do appear to have substantial effects. Heterogeneity is not only to be found in terms of the type of trader, however. Importantly, we show that the effects are amplified in the case of corporate bonds, rather than sovereigns, and also in times of crisis. This is evidence in favor of fire sale models that emphasize the difficulty of finding substitutes for 'expert' buyers of relatively opaque or complicated assets in times where those experts, or arbitrageurs more generally, are constrained.

References

Ambastha, M., A. Ben Dor, L. Dynkin, J. Hyman, and V. Konstantinovsky (2010): "Empirical Duration of Corporate Bonds and Credit Market Segmentation," *The Journal of Fixed Income*, 20(1), 5–27.

Anand, A., C. Jotikasthira, and K. Venkataraman (2020): "Mutual Fund Trading Style and Bond Market Fragility," *The Review of Financial Studies*, 34(6), 2993–3044.

Bao, J., J. Pan, and J. Wang (2011): "The Illiquidity of Corporate Bonds," *The Journal of Finance*, 66(3), 911–946.

Baranova, Y., G. Douglas, and L. Silvestri (2019): "Simulating stress in the uk corporate bond market: investor behaviour and asset fire-sales," .

Brunnermeier, M. K., and L. H. Pedersen (2009): "Market Liquidity and Funding Liquidity," *The Review of Financial Studies*, 22(6), 2201–2238.

Cai, F., S. Han, D. Li, and Y. Li (2019): "Institutional herding and its price impact: Evidence from the corporate bond market," *Journal of Financial Economics*, 131(1), 139–167.

Cheema, M. A., R. Faff, and K. R. Szulczyk (2020): "The 2008 Global Financial Crisis and COVID-19 Pandemic: How Safe are the Safe Haven Assets?," *Covid Economics*, 34.

Choi, J., S. Hoseinzade, S. S. Shin, and H. Tehranian (2020): "Corporate bond mutual funds and asset fire sales," *Journal of Financial Economics*, 138(2), 432–457.

Coen, J., C. Lepore, and E. Schaanning (2005): "Taking regulation seriously: fire sales under solvency and liquidity constraints," Bank of England Staff Working Paper No. 793.

Coval, J., and E. Stafford (2007): "Asset fire sales (and purchases) in equity markets," *Journal of Financial Economics*, 86(2), 479–512.

Czech, R., J. Koosakul, and N. Vause (2021): "Responses of UK Corporate Bond Funds to Redemptions: Evidence from High-Frequency Data," Unpublished paper, Bank of England.

Duffie, D. (2020): "Still the World's Safe Haven? Redesigning the U.S. Treasury Market After the COVID19 Crisis," Hutchins Center Working Paper 62, Brookings Institution.

Edmans, A., I. Goldstein, and W. Jiang (2012): "The Real Effects of Financial Markets: The Impact of Prices on Takeovers," *The Journal of Finance*, 67(3), 933–971.

Falato, A., A. Hortacsu, D. Li, and C. Shin (forthcoming): "Fire-Sale Spillovers in Debt Markets," *The Journal of Finance*.

Goldstein, I., H. Jiang, and D. T. Ng (2017): "Investor flows and fragility in corporate bond funds," *Journal of Financial Economics*, 126(3), 592–613.

Greenwood, R., A. Landier, and D. Thesmar (2015): "Vulnerable banks," *Journal of Financial Economics*, 115(3), 471–485.

Haddad, V., A. Moreira, and T. Muir (2020): "When Selling Becomes Viral: Disruptions in Debt Markets in the COVID-19 Crisis and the Fed's Response," NBER Working Papers 27168, National Bureau of Economic Research, Inc.

He, Z., S. Nagel, and Z. Song (2021): "Treasury inconvenience yields during the COVID-19 crisis," *Journal of Financial Economics*.

Kargar, M., B. Lester, D. Lindsay, S. Liu, P.-O. Weill, and D. Zúñiga (2020): "Corporate Bond Liquidity During the COVID-19 Crisis," NBER Working Papers 27355, National Bureau of Economic Research, Inc.

Schrimpf, A., H. S. Shin, and V. Sushko (2020): "Leverage and margin spirals in fixed income markets during the Covid-19 crisis," BIS Bulletins 2, Bank for International Settlements.

Shleifer, A., and R. Vishny (2011): "Fire Sales in Finance and Macroeconomics," *Journal of Economic Perspectives*, 25(1), 29–48.

Shleifer, A., and R. W. Vishny (1992): " Liquidation Values and Debt Capacity: A Market Equilibrium Approach," *Journal of Finance*, 47(4), 1343–66.

Wardlaw, M. (2020): "Measuring Mutual Fund Flow Pressure as Shock to Stock Returns," *The Journal of Finance*, 75(6), 3221–3243.

Figures

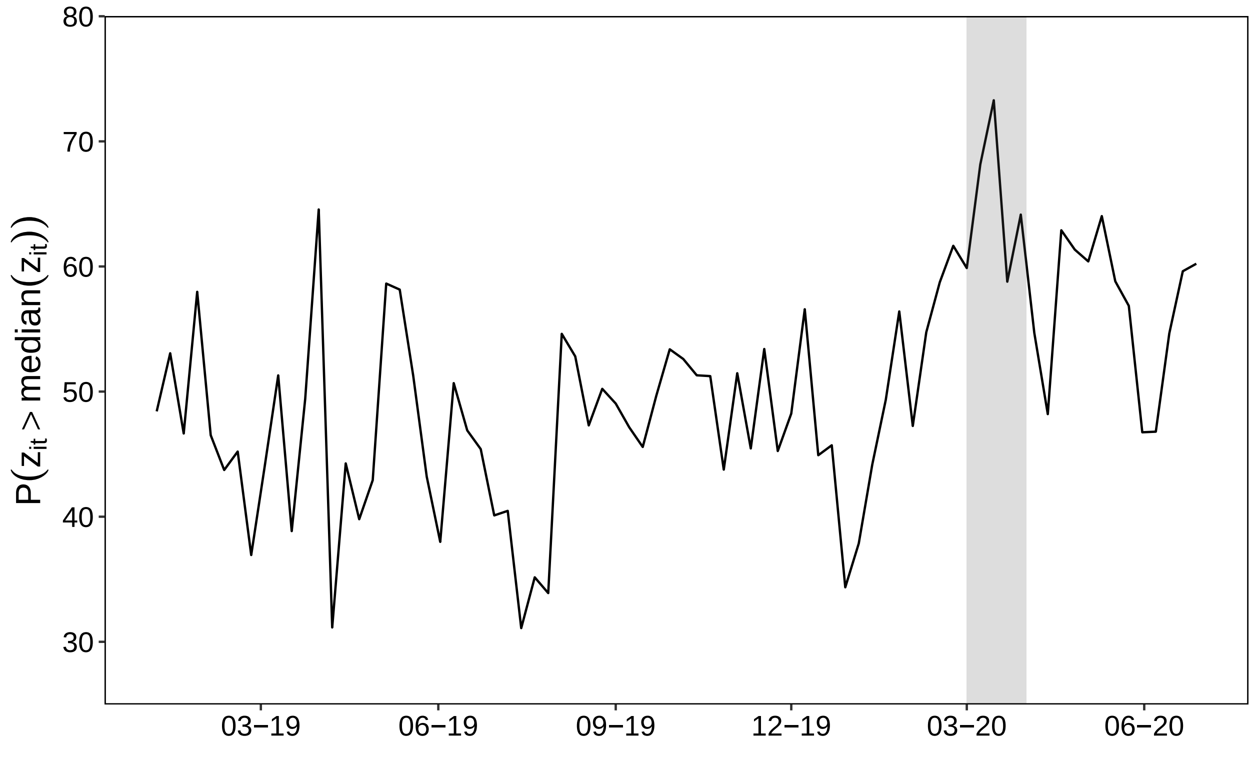
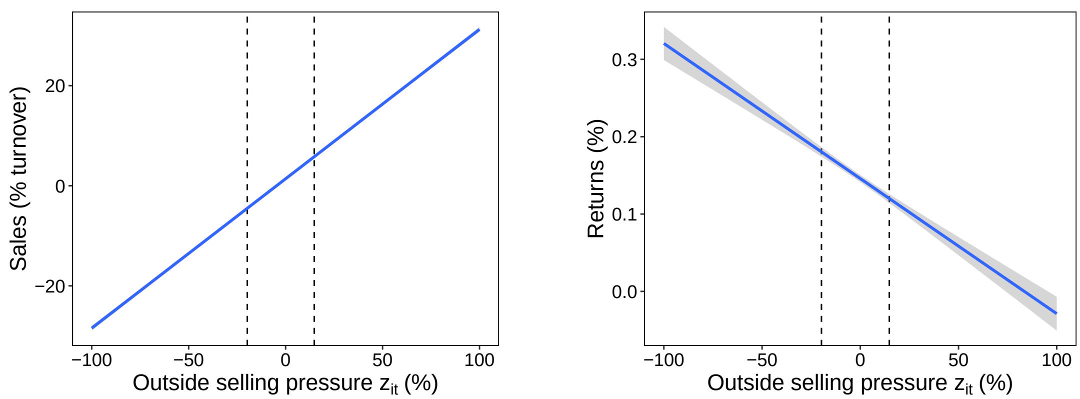


Figure 1: Selling pressure through time

Note: Figure shows the fraction of bonds in a given month that have outside selling pressure $z_{i,t}$ greater than the median in the sample. Grey shaded area denotes March 2020.



(a) Pressure and sales

(b) Pressure and returns

Figure 2: Outside selling pressure, sales and bond returns

Note: Figure shows the relationship between sales of a bond (normalised by turnover) and outside selling pressure (left-hand side) and bond returns (right-hand side) and outside selling pressure (right-hand side). The figures are univariate versions of our instrumental variables approach, with the left-hand figure showing the first stage of the regression and the right-hand side showing the reduced form relationship between returns and the instrument. Dashed vertical lines show the 10^{th} and 90^{th} percentile of outside selling pressure.

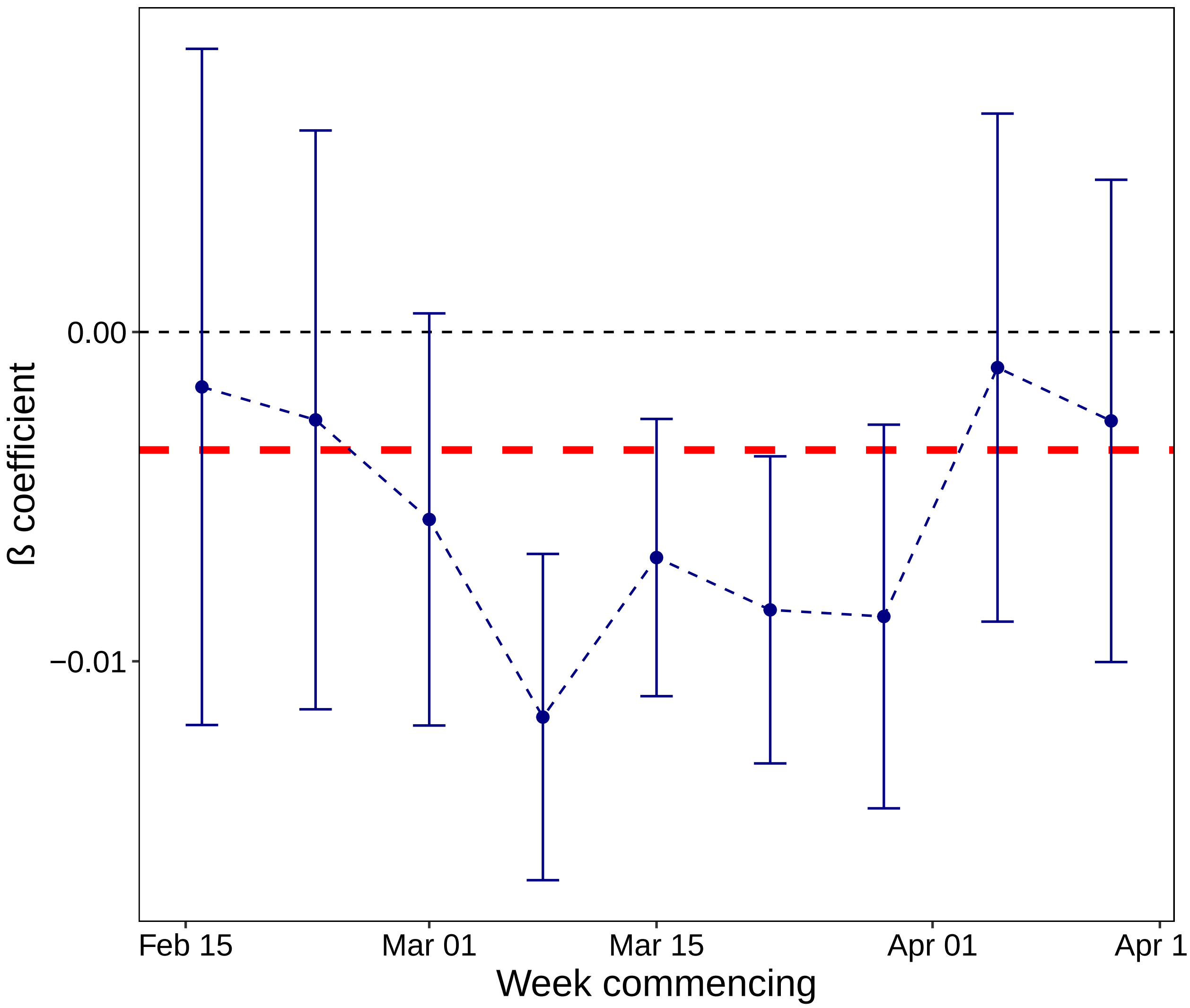


Figure 3: Price changes and selling pressure around the 'Dash for Cash'

Note: Figure shows how the estimated β coefficient in our price impact regressions varies when estimated separately for various weeks in February-April 2020. The error bars show 95% confidence intervals. The dashed red line shows the estimated β coefficient in our baseline results for the whole sample.

Tables

Table 1: Summary Statistics

	Share	Trade Share
PANEL A: BONDS		
Type		
Corporate	85	44
Government	15	56
Currency		
GBP	7	11
EUR	26	44
USD	47	39
Other	20	6
Maturity		
0-5 years	45	21
6-10 years	37	44
11-20 years	7	12
21+ years	11	24
PANEL B: TRADERS		
Sector		
Asset Manager	44	15
Bank	11	14
Dealer	3	51
Hedge Fund	6	2
Non-Financial	2	0
Other Financial	4	8
PFLDI	28	2
State	2	1
Trading Services	1	8

Notes: This table summarises the instruments traded and the types of traders in the dataset. The first numeric column shows raw shares, for example the percentage of bonds that are corporate vs. government, or the percentage of traders that are asset managers. The second numeric column shows the percentage of total trades accounted for by each bond and trader type. State includes trading by central banks, governments, sovereign wealth funds and supranational organisations. PFLDI denotes pension funds and liability-driven investment firms. Trading services firms include brokerage firms. Other financials includes central counterparties and principal trading firms, among other firm types.

Table 2: Instruments & Traders per week

	Number
Instruments traded	24,378
Traders	27,859
Instruments per trader	
Dealers	733
Customers	8
Traders per instrument	
Dealers	4
Customers	10

Notes: Table summarises the number of traders and instruments traded per week. Each statistic in the table is calculated for each week, including only instruments and traders that traded at least once in that week. The table shows averages of the statistics across the weeks in the sample. Customers are defined as all traders except dealers.

Table 3: Explaining Variation in Returns & Sales

	R-squared	
Fixed Effects	Returns r_{it}	Sales s_{it}
Instrument	0.02	0.07
Issuer	0.01	0.01
Week	0.04	0.00
Issuer & Week	0.05	0.02
Issuer-Week	0.31	0.20
Issuer-Week & Instrument	0.32	0.25

Notes: Table shows the variation in returns $r_{i,t}$ and sales $s_{i,t}$ that can be explained by various combinations of fixed effects. The first numeric column shows the R-squared from a regression of returns on the relevant fixed effects, whilst the second shows the R-squared from a regression of sales on the relevant fixed effects.

Table 4: Returns, Sales & Pressure

	Mean	Std. dev.	5^{th} pctile	Median	95^{th} pctile
Returns r_{it}, (%)	0.17	3.04	-7.20	0.07	7.49
Sales s_{it}, (% turnover)	1.12	73.33	-77.15	0.00	83.48
Pressure z_{it}, (%)	-1.12	30.29	-36.61	-0.07	27.70

Notes: Table summarises the three key variables in our regressions: asset returns, sales, and pressure. Returns $r_{i,t}$ are the percentage change in asset i's average trading price at time t relative to the last time it was traded. Sales $s_{i,t}$ are aggregate sales of asset i at time t by non-dealers divided by turnover, which is the average weekly trading volume of asset i in weeks it was traded. Pressure $z_{i,t}$ is our measure of outside selling pressure defined in equation 4. Returns are winsorized at the 5^{th} and 95^{th} percentiles.

Table 5: Outside Selling Pressure and Fund-Flow based pressure

	Asset Manager Pressure z_{it}^{AM}		
	(1)	(2)	(3)
Coval-Stafford (pctile)	0.30^{*}		
	(0.18)		
Wardlaw F2S (pctile)		0.40^{***}	
		(0.12)	
Wardlaw F2V (pctile)			0.35^{***}
			(0.12)
R^2	0.47	0.47	0.47
Observations	526,665	526,665	526,665
Issuer-Week fixed effects	Yes	Yes	Yes
Instrument fixed effects	Yes	Yes	Yes

Notes: Table shows the positive correlation between our measure of outside selling pressure defined in equation 4, $z_{i,t}$—computed solely for asset managers—and various measures of selling pressure induced by mutual fund flows defined in the literature, after including issuer-week and instrument fixed effects. The Coval-Stafford measure is the selling pressure measure introduced by Coval and Stafford (2007), whilst the two Wardlaw measures are those introduced by Wardlaw (2020). We multiply each measure by -1 such that a large positive number entails positive selling pressure, and take the percentile rank of each measure as our regressors. Our regressors thus take values between 0 and 1, and a coefficient of 0.5 means moving from the 1^{st} to the 51^{st} percentile of a fund selling pressure is associated with an increase in outside selling pressure $z_{i,t}$ of $0.5 \times 0.5 = 0.25$. For further details on the pressure measuress see the appendix.

Table 6: Price changes and selling pressure

	OLS Return r_{it} (%) (1)	2SLS 2[nd] stage Return r_{it} (%) (2)	2SLS 1[st] stage Sales (% turnover) (3)
Sales (% turnover)	-0.0004***	-0.004***	
	(4.1×10^{-5})	(0.0006)	
Pressure z_{it} (%)			0.26***
			(0.006)
R^2	0.33	0.33	0.25
F-test (IV only)		61.3	7,476.5
Observations	1,629,220	1,622,762	1,622,762
Issuer-Week fixed effects	Yes	Yes	Yes
Instrument fixed effects	Yes	Yes	Yes

Notes: Table shows the impact of sales on returns using an OLS specification and a 2SLS specification. Sales are instrumented by outside selling pressure. Controls are issuer-week fixed effects, instrument fixed effects, and the time since a bond was issued. The number of observations is marginally lower in the 2SLS specification as computing pressure z_{it} requires that in week t at least one customer trading instrument i traded a bond issued by a different issuer. The 1[st] stage F-statistic tests whether the coefficient on the instrument is 0. Stock & Yogo (2005) suggest an F-statistic lower than 10 indicates weak instruments.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 7: Heterogeneity: Stressed periods

	Return r_{it} (%)	
	Full sample	March 2020
	(1)	(2)
Sales (% turnover)	-0.004***	-0.01***
	(0.0006)	(0.003)
R^2	0.33	0.66
Observations	1,622,762	87,647
Issuer-Week fixed effects	Yes	Yes
Instrument fixed effects	Yes	Yes

Notes: Table shows how the impact of fireselling differs in a period of stress vs the whole sample. 2SLS regressions are run separately for the full sample (first column) and only for March 2020 (second column). Controls are issuer-week fixed effects, instrument fixed effects, and the time since a bond was issued.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 8: Heterogeneity: Corporate vs. Government bonds

	Return r_{it} (%)	
	Corporate	Government
	(1)	(2)
Sales (% turnover)	-0.004***	-0.002
	(0.0007)	(0.002)
R^2	0.36	0.18
Observations	1,290,583	332,179
Issuer-Week fixed effects	Yes	Yes
Instrument fixed effects	Yes	Yes

Notes: Table shows how the impact of fireselling differs between coporate and government bonds. 2SLS regressions are run separately for corporate bonds (first column) and government bonds (second column). Controls are issuer-week fixed effects, instrument fixed effects, and the time since a bond was issued.
*p < 0.1, **p < 0.05, ***p < 0.01.

Table 9: Heterogeneity: Corporate and Government bonds in stress

	Return r_{it} (%)	
	Corporate	Government
	(1)	(2)
Sales (% turnover)	-0.02***	-0.004
	(0.004)	(0.004)
Standard-Errors	Issuer-Week	Instrument
R^2	0.67	0.57
Observations	69,133	18,561
Issuer-Week fixed effects	Yes	Yes
Instrument fixed effects	Yes	Yes

Notes: Table shows how the impact of fireselling in a stress differs between corporate and government bonds. 2SLS regressions are run separately for corporate bonds (first column) and government bonds (second column), with the sample restricted to the stressed period in March 2020. Controls are issuer-week fixed effects, instrument fixed effects, and the time since a bond was issued. *p < 0.1, **p < 0.05, ***p < 0.01.

Table 10: Impact of sales by all traders vs sales by asset managers

	Return r_{it} (%)	
	All traders	Asset Managers
	(1)	(2)
Sales (% turnover)	-0.004***	0.003***
	(0.0006)	(0.001)
R^2	0.33	0.42
F-test (1st stage), Sales (% turnover)	7,476.5	3,039.2
Observations	1,622,762	994,560
Issuer-Week fixed effects	Yes	Yes
Instrument fixed effects	Yes	Yes

Notes: Table compares the impact of all traders sales' of an asset to sales only by asset managers. The first column repeates the results of the baseline 2SLS regression shown in Table 6. The second shows the results of a 2SLS regression where the regressor of interest is sales of a bond by asset managers only (scaled by turnover) and the instrument is outside selling pressure for asset managers only. Controls are issuer-week fixed effects, instrument fixed effects, and the time since a bond was issued. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Appendix A. Mutual Fund Selling Pressure

We use our data on open-ended fund holdings to construct three measures of fire-selling pressure induced by fund flows that are commonly used in the literature, introduced by Coval and Stafford (2007) and Wardlaw (2020). Each of these approaches results in an instrument-level measure of selling pressure, which we then compare to our instrument-level pressure measure based only on transactions data. As Table A1 shows, on average funds in our fund holdings data hold between one-third and one-half of the instruments observed in our transactions data, though they hold only a small percentage of the total amount issued.

Appendix A.1. Coval and Stafford (2007) selling pressure measure

Coval and Stafford (2007) develop a selling pressure measure using data on open-ended fund holdings. For a given instrument the measure is defined as the difference between total purchases of that instrument by funds that experience extreme inflows and total sales of that instrument by funds that experience extreme outflows, normalised by lagged trading volume. Sales and purchases are evaluated using changes in holdings between two consecutive time periods. Extreme outflows and inflows are defined as those below the 10^{th} percentile and above the 90^{th} percentile of the distribution of flows, respectively.

We evaluate the pressure measure developed by Coval and Stafford (2007) at weekly frequency using weekly data on open-ended fund total net assets (TNA) and quarterly data on open-ended fund holdings. Specifically, for a given bond i and week t we evaluate:

$$Press_{it}^{CS} = \frac{\sum_j \left(\max(0, purchases_{jit}) | f_{jt} > 90^{th} \text{ pctile} \right) - \sum_j \left(\max(0, -sales_{jit}) | f_{jt} < 10th \text{ pctile} \right)}{AvgVol_{iq_t}}, \quad \text{(A.1)}$$

where $f_{jt} = F_{jt}/TNA_{jt-1}$ are fund j's weekly flows as a percentage of lagged total net assets; weekly purchases and sales of bond i by fund j are evaluated from weekly data on TNAs and quarterly portfolio weights w_{jiq_t} as follows:[17]

[17] Following Coval and Stafford (2007) Equation A.2 and Equation A.3 are derived as weekly changes in asset holdings assuming that weekly portfolio weights are constant in a given quarter and equal to those at the end of the previous quarter. Therefore we are not including discretionary sales or purchases.

$$sales^{(1)}_{ijt} = \Delta TNA_{jt} w_{jiq_t} \quad \text{when } \Delta TNA_{jt} = TNA_{jt} - TNA_{jt-1} < 0, \tag{A.2}$$

$$purchases^{(1)}_{ijt} = \Delta TNA_{jt} w_{jiq_t} \quad \text{when } \Delta TNA_{jt} = TNA_{jt} - TNA_{jt-1} > 0; \tag{A.3}$$

w_{jiq_t} are portfolio weights as in the previous quarter of any time t; and $AvgVol_{iq_t}$ is the average weekly volume traded in the previous quarter estimated using transaction level data. As in Coval and Stafford (2007) we assume that there should be at least 10 funds that hold a given bond i.

When identifying funds with extreme inflows and outflows we treat each fund separately—namely, funds with extreme inflows and outflows are determined relative the 10^{th} and 90^{th} of the distribution of their own sales in the sample period, and not those of the whole population of funds.

Following Coval and Stafford (2007) bonds with fire sales are those with $Pressure^{CS}_{it}$ in the lowest decile of its distribution across bonds and time periods.

Appendix A.2. Wardlaw (2020) selling pressure measures

Wardlaw (2020) shows that the measure introduced by Edmans, Goldstein, and Jiang (2012) might include a monotonically increasing function of asset returns when the variables used to evaluate it—the amount held in the portfolio and volumes—are marked to market. He suggests two alternative measures that measure holdings and volumes in nominal terms. We follow his approach and evaluate the two alternative measures he proposes as follows:

1. The *flow-to-stock* measure:

$$Press^{F2S}_{it} = \sum_j \frac{F_{jt} shares_{ijq_t}}{TNA_{jt-1} OutShare_{it}} = \sum_j \frac{f_{jt} shares_{ijq_t}}{OutShare_{it}}, \tag{A.4}$$

   where $OutShare_{it}$ is the amount of outstanding of bond i at time t, and $shares_{ijq_t}$ are the portfolio weights as in the previous quarter q_t.[18]

[18] In the original definition of Wardlaw (2020) $shares_{ijq_t}$ are defined to be linked to the quarterly portfolio weights $w^*_{ijq_t}$ and quarterly prices PRC_{iq_t} by the following equation: $w^*_{ijq_t} TNA_{jq_t} = shares_{ijq_t} PRC_{iq_t}$.

2. The *flow-to-volume* measure

$$Press_{it}^{F2V} = \sum_j \frac{F_{jt} shares_{ijq_t}}{TNA_{jt-1} VolShare_{it}} = \sum_j \frac{f_{jt} shares_{ijq_t}}{VolShare_{it}}, \tag{A.5}$$

where again $shares_{ijq_t}$ are the portfolio weights as in the previous quarter q_t and $VolShare_{it}$ is the total volume of bond i traded expressed in units terms. Namely, we can evaluate it from our transaction level data by dividing the traded volume expressed in monetary units by the par value of bond i.

In both Equation A.4 and Equation A.5 only funds in distress carrying out fire sales are included in the summation. Similarly to Coval and Stafford (2007) we identify funds doing fire sales as those experiencing extreme outflows—namely, with outflows below the 10^{th} percentile of their distribution.

Table A1: Funds Data Coverage

	Percentage of instruments held by mutual funds	Percentage of issuance held by mutual funds
2019 Q3	50.1	1.3
2019 Q4	52.1	1.3
2020 Q1	37.8	0.5
2020 Q2	48.0	0.6

Notes: Table shows the percentage of bonds traded in our transactions data held by funds in our mutual fund data. For each quarter-end we compute the number of bonds in our transactions data that are in issue at that time, and the aggregate amount issued of these bonds. We then compute the percentage of these bonds that are recorded as held by mutual funds in our mutual fund holdings data, and the aggregate holdings of these bonds by mutual funds as a percentage of the amount issued.