Smoke without fire? Reassessing empirical evidence of firesales*
PRELIMINARY AND INCOMPLETE, DO NOT CITE

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

[a] *KBS, University of Cambridge, Janeway Institute*
[b] *London School of Economics*
[c] *International Monetary Fund*
[d] *Bank of England*

Abstract

Until recently the literature on firesales had reached a sort of consensus but now, however, new research has illustrated various empirical problems with traditional identification methods and proxies for forced selling. As such, the literature has been thrown into flux, with some even questioning if firesales exist. Since firesales are often used as exogenous events in a broader literature examining the effect of financial markets on firms' real decision, the implications of this are substantial. We show statistically and economically significant evidence of firesales using novel techniques that avoid the econometric concerns recently raised. Instrumenting sales of bonds (corporate and sovereign) with the sales by traders *other than the bond in question*, and by exploiting within issuer-time variation (comparing Bond A and Bond B of Issuer X at t), we obtain a reliable measure of non-fundamental selling pressure. That is, selling pressure divorced from price-relevant bond fundamentals - which is the essence of firesales. Interestingly, as recent work has suggested, the connection of firesales to distressed funds' actions appears rather weak, in contrast to long held consensus views on this matter. However, looking more broadly, non-fundamental sales from other 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. Notably we examine the 'dash for cash' period in March 2020 and thus also contribute to the important literature on COVID-induced financial market disruption.

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1. Introduction

Firesales 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 firesales is now in a state of flux and several key questions no longer have settled answers. Do firesales 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 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 holding 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 typically by investors who are heavily selling other 'unrelated' bonds, then it proxies for the sort of distress that underpins firesales. The key dataset we use is the universe of transactions in government and corporate bonds by FCA-regulated entities, now required to be submitted under the MiFID 2 directives.

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 'other' 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 held by investors holding bond A or bond B. Beyond this, though most identification-confounding stories would naturally be at the

[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.

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 firesales effects to be picked up, even if they are true? In fact, our results suggest that our approach retains ample non-fundamental variation and display interesting economic and statistical significance. We find an important role for firesales, 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 $60bps$ 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. 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 been concentrated on data in relation to mutual funds. In turn, the recent re-evaluation of the evidence of firesales has been focused in this domain, apparently questioning whether there are substantial effects after all. When we condition on sales by mutual funds, we obtain fairly muted effects, in line with the recent literature, though with the added benefit of using our carefully constructed non-fundamental sales measure that avoids criticisms leveled at other studies. However, an important contribution of our paper is that our data, to be discussed further below, features the whole gamut of bond market participants. Conditioning on sales by these other investors, we find price impacts five times larger. As such, we reconcile existing mutual fund-centric results with the intuitively convincing theoretical models that would suggest an important role for firesales, and with the outsized 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 a decline in the 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 a capital losses (Greenwood, Landier, and Thesmar (2015)). At the same time, while agents are forced to sell assets, natural buyers may be constrained (Shleifer and Vishny (1992)). Furthermore, 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 sheet and the market disruption observed in the COVID-19 crisis.[2]

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 firesales. 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) provided an analysis of the price effect of firesales by open ended mutual funds induced by capital 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 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 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 firesales. 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,

[2]During the onset of the epidemic, dealers were not willing or able to fully absorb the increase in flows 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 below.

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.

Though their analysis is still subject to the Wardlaw critique to some extent, Choi, Hoseinzade, Shin, and Tehranian (2020) use issuer-level fixed effects are used as a powerful way of eliminating 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.[3] Under this approach, which we extend, evidence of firesales effects appears minimal.[4] Our results are in line with their findings on the limited price impact from funds sales. However, when looking at a broader set of investor types (including banks, hedge funds and other asset managers) we find that price impact can be much larger. Our approach is indeed general enough to be applicable to any investor type, allowing us to exploit the richness of our datasets.

Further, we analyse how our results change in times of stress. We find that during the 'dash for cash' 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 safe haven providing a hedging instrument against stock market falls (see Cheema, Faff, and Szulczyk (2020)). While these studies nicely document a

[3]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 on the basis of 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.

[4]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 than threatening market stability. More recent research on the effects of fund-induced firesales also find a limited price effect - see Czech, Koosakul, and Vause (2021) for example.

deterioration in liquidity conditions for bond markets during March 2020, they do not provide conclusive evidence of firesales. However, as emphasized in Falato, Hortacsu, Li, and Shin (forthcoming), bond markets are arguably especially prone to firesales and spillovers and given the degree of disruption in even traditionally safe segments, it is plausible that they were at play.[5]

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), now required to be submitted under the MiFID II directives. 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.

Around $90k$ bonds are traded in our data, of which around 80% are corporate (the remainder are government) with the number of observed trades orders of magnitude greater. There is substantial heterogeneity in the currencies of the bonds traded with Dollar, Euro and Sterling transactions accounting for approximately 45%, 30% and 10% of trades, respectively. The breakdown of traders, conditioning on corporate vs government bonds, sees close to half of transactions featuring a dealer, with non-dealer banks, asset managers (notably mutual funds), insurers and pension funds also featuring prominently.

We adopt a much finer observation frequency than is typical in the literature (which typically is at quarterly or monthly frequency) in aggregating trade data to the weekly frequency. Our sample period runs from January 1^{st} 2019, to the July 1^{st} 2020, although we note that our identification will ultimately come from the (enormous) cross section as we absorb time (indeed issuer-time) components in our analysis, as discussed in section 4.

We merge this data with bond information from Eikon fixed income data, providing key characteristics of the securities. Note that information on maturity, credit enhancement and callability will feature as controls, later in our analysis.

In complementary analysis, we also make use of information on open-ended mutual funds' total net assets and estimated net flows, at weekly frequency, and quarterly data on their portfolios, obtained from

[5]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.

Morningstar. Additionally, (in future work) we will exploit a novel confidential dataset based on filings (of the Form AS) submitted by UK Monetary Financial Institutions (MFIs) to the Bank of England, which contain security level detail on banks' portfolios. As such, for two important classes of market participant, we not only observe their trading flows but, to an extent, can also observe their stocks of asset holdings. In ongoing work, we are exploring the role of banks in holding assets and the role of their accounting and capital regulations in influencing how they behave in firesale periods (see Chodorow-Reich, Ghent, and Haddad (2020) for related work on 'asset insulation').

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_{it}, first by calculating net sales and transactions of all bonds, $k \neq i$ as[6]

$$
\begin{aligned}
z_{ijt}^{NS} &\equiv \sum_{k \neq i} s_{kjt} &\quad (2)\\
z_{ijt}^{T} &\equiv \sum_{k \neq i} |s_{kjt}| &\quad (3)
\end{aligned}
$$

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

$$
z_{it} \equiv \frac{\sum_j \mathbb{1}_{s_{ijt} \neq 0} z_{ijt}^{NS}}{\sum_j \mathbb{1}_{s_{ijt} \neq 0} z_{ijt}^{T}} \quad (4)
$$

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_{it} should be highly correlated s_{it}, and thus a candidate instrument, clearly the requirement that the other bonds being sold are 'unrelated' is unlikely to be satisfied in 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_{it} encodes price relevant fundamental information regarding i.

At this point, one might attempt to refine z_{it} by adopting a selection rule that filters the trades that feature in the calculation of z_{it} - exploiting information about the traders, bonds or the context of the trade. While in section 4 we will explore more elaborate selection rules to gain further insight into the nature of the non-

[6]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.

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_{it}, constructed as described above and, additionally, include a set of demanding fixed effects and controls to eliminate endogeneity concerns. Specifically, we will consider the following 2SLS specification

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

$$s_{i,t} = \alpha z_{i,t} + \delta' X_{i,t} + \nu_{i,t} \quad (6)$$

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 include controls that are at the instrument level, varying within issuer-time. Specifically, we include a measure of time to maturity, a dummy for callability and a proxy for credit enhancement / collateralization which could conceivably be sources of fundamental variation and, perhaps less plausibly, variation that correlates both with returns and with z_{it}. Once we have added our fixed effects and the aforementioned controls, our identification 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 firesale studies. However, it is of course possible that we have thrown the baby out with the bathwater - it could well be that investors are fireselling all bonds from a given issuer to the same degree - in which case we would have eliminated 'desired' variation. However, as shown below in our results section, we retain ample non-fundamental variation for precise estimation of the effects of non-fundamental sales.

We now briefly discuss properties of $z_{i,t}$. In Figure 1 we plot the median of the measure over time, along with the fraction of securities experiencing a greater than x percentile degree of outside selling pressure, based on the empirical distribution over the whole sample. 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 'dash for cash' (March 2021) period which, anecdotally and in aforementioned academic studies, has been argued was associated with fireselling pressure.

In addition, our measure is positively correlated with the 'flow-to-stock' (F2S) and 'flow-to-volume' (F2V) constructed in Wardlaw (2020) as alternatives to the mechanically flawed 'mutual-fund-flow' (MFF) measure of selling pressure advocated in Coval and Stafford (2007) and derivatives. However, we note that there is no requirement for our measure to be strongly correlated with these proxies - and their utility is even questioned in Wardlaw (2020) - to be considered an effective measure of non-fundamental sales. Likely various proxies can be constructed that correlate imperfectly with a true, but latent, concept of non-fundamental sales. Additionally, as aforementioned, our demanding identification approach also likely strips out some desired variation in sales, so that comparability of measures across papers is challenging, and likely would lead to the weak correlation we observe. One benefit of exploiting sales of 'other' bonds, in constructing OSP is that we avoid the more mechanical connections between the firesale proxy and returns on bond i that Wardlaw (2020) criticizes in other approaches.

5. Results

In ongoing work, we are exploring additional avenues related to the holding of securities by banks (making use of *security level* confidential balance sheet data). Additionally, we are actively exploring how to refine our measure so that not only is it non-fundamental, which our methods essentially ensure, but perhaps more interpretable as fireselling *per se*, that is *distressed* non-fundamental sales and possibly those that induced spillovers and sustained deviations from fundamentals. Our results already, however, are extremely promising and interesting in and of themselves.

We begin with our baseline regression framework, with Figure 1 showing results from our regressions of returns on sales (OLS and with sales instrumented by OSP) and, for completeness, Figure 2 the first stage regression for the two stage approach.[7] The importance of instrumenting is shown that, while both key coefficients of returns on sales are negative and significant, as would be intuitive if firesales are at play, the two stage least squares deviates by an economically meaningful amount. Note that both of these regressions include our (extremely) demanding sets of fixed effects. For interpretability of the key coefficient (-0.0039), we note that this would imply that a move from the 5^{th} to the 95^{th} percentile of selling *causes* a $60bps$ 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 firesales imply heterogeneous effects of sales, depending on the basis of the broader context of the selling, the type of assets sold, and the nature of the agent selling the asset. We address these in turn, beginning with Figure 3 in which we focus on how our results depend on whether we only use observations from the 'dash for cash' episode in 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. This is a striking and reassuringly intuitive result and again helps restore the empirical evidence in favor of the existence of firesales, but now on a more sound empirical footing.

Turning to the dependence of our results on the type of securities sold, a natural division within fixed income is between corporate and government bonds (in ongoing work we hope to divide securities more finely - such as in dimensions of complexity and credit quality). Again, our results, shown in Figure 4 are intuitive and we draw confidence from the fact that we are not observing uniform results or heterogenous results inconsistent with a 'model' of firesales. We find that the coefficient on corporate bonds essentially aligns with our whole 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 firesale-depressed prices.

[7] *Caveat: Please note that at submission the specification of bond-timer controls (maturity and credit enhancement etc.) is unclear and we will soon correct/clarify it.*

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 (this will be ongoing work, particularly in slicing by the type of government bond). That is, we examine how corporate and government bonds prices reacted to our instrumented selling, during the dash for cash. As shown in Figure 5 show that the coefficient on corporates is estimated to be even larger than its full sample counterpart, while there remains no significant result for sovereigns.

Finally, let us turn to a sub-sample that is of especial relevance in terms of comparability with much of the literature. In Figure 6 we restrict the calculation of our selling pressure measure to take account only of trades by asset managers. 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 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). We are (as yet) unable to precisely identify the subset of asset managers that are open ended mutual funds. Nevertheless this segment of our data is presumably most comparable to the existing literature. Interestingly, when we condition on examining only pressure from asset managers, we lose our negative effect. Indeed there is an estimate economically tiny though statistically significant positive effect. As such, 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 fo mutual fund selling) on a firmer footing.

6. Conclusions

We have shown that there is statistically and economically significant evidence of firesales, based on a rigorous approach to identifying non-fundamental selling pressure. Interestingly, as recent work has suggested, the connection of firesales 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 from other 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 firesale 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.

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Figures

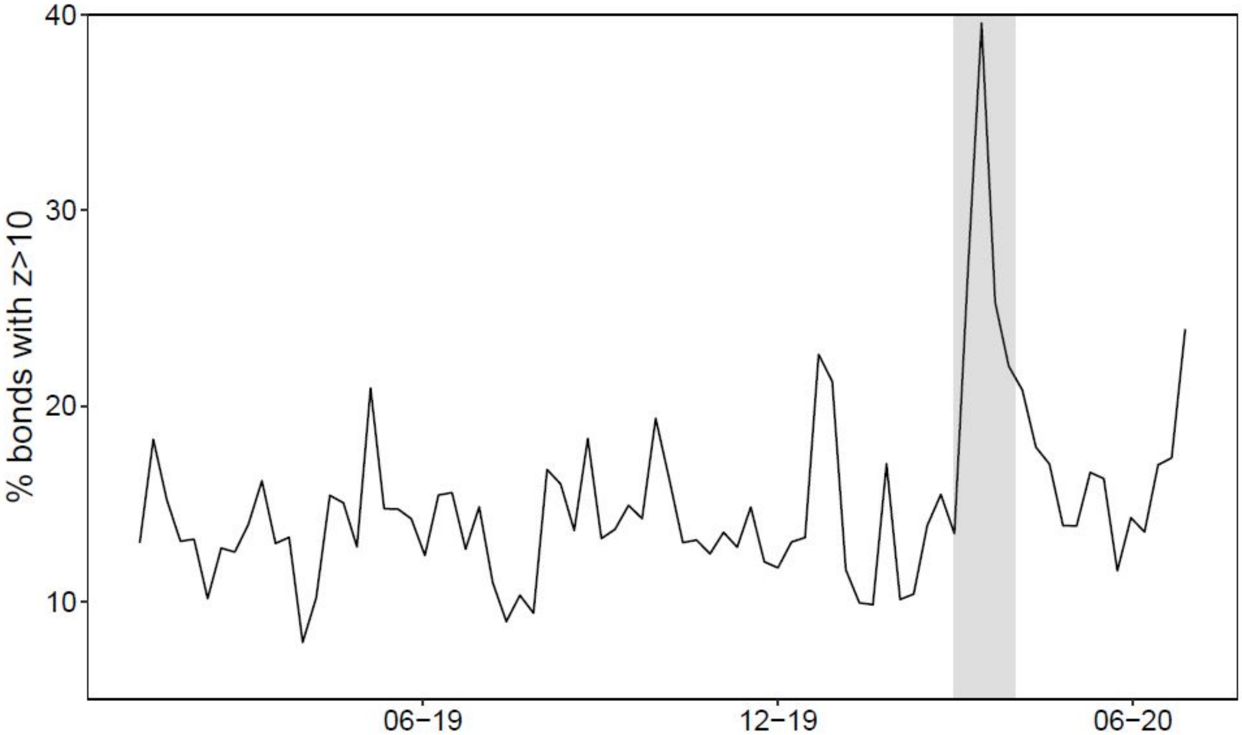
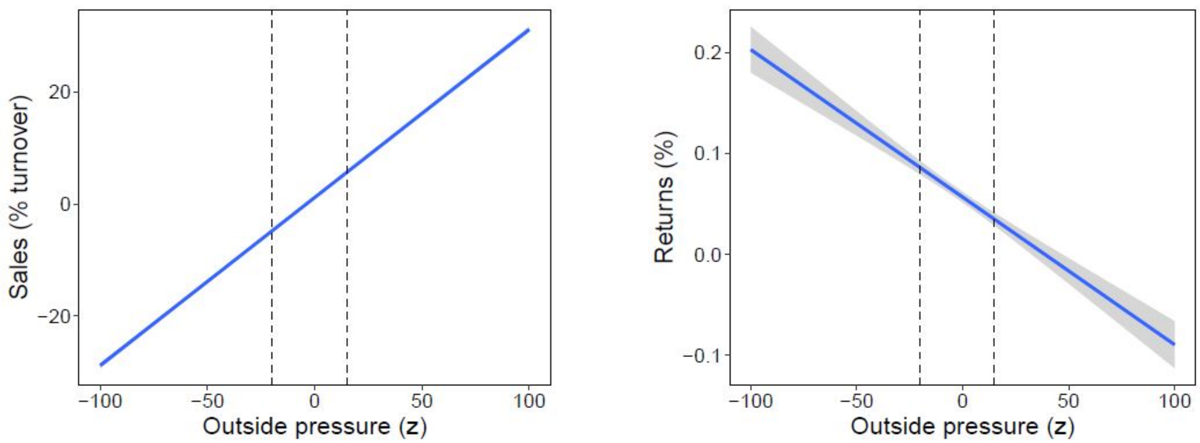


Figure 1: Selling pressure through time: Fraction of bonds traded that are in upper tail of distribution



Pressure & sales

Pressure & returns

Figure 2: Estimated relationship between sales (first stage, normalized by turnover) and bond returns (implied second stage, percent).

Tables

Dependent Variable:	Return r_{it} (%)	
	OLS	2SLS
Model:	(1)	(2)
Variables		
Sales (% turnover)	-0.0003***	-0.0039***
Fixed-effects		
issuer-week	Yes	Yes
instrument	Yes	Yes
Fit statistics		
R^2	0.33633	0.33175
Observations	1,727,856	1,719,907
F-test (1st stage)		6,134.4

One-way (issuer-week) standard-errors in parentheses
Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 1: Regression results: No interactions, whole sample, OLS and 2SLS - Effect of sales and sales instrumented by selling pressure on returns

Dependent Variable:	Sales (% turnover)	
Model:	(1)	(2)
Variables		
Pressure z_{it} (%)	0.2949***	0.2608***
Fixed-effects		
issuer-week	Yes	Yes
instrument		Yes
Fit statistics		
R^2	0.19736	0.24818
Observations	1,719,907	1,719,907

One-way (issuer-week) standard-errors in parentheses
Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 2: First stage regression of 2SLS - Regression of sales on outside selling pressure

Dependent Variable:	Return r_{it} (%)	
	Full sample	March 2020
Model:	(1)	(2)
Variables		
Sales (% turnover)	-0.0039***	-0.0156***
Fixed-effects		
issuer-week	Yes	Yes
instrument	Yes	Yes
Fit statistics		
R^2	0.33175	0.66894
Observations	1,719,907	93,426
F-test (1st stage)	6,134.4	443.80

One-way (issuer-week) standard-errors in parentheses
Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3: Regression results: Sample split into stress (March 2020, dash for cash) and remainder of sample (2SLS)

Dependent Variable:	Return r_{it} (%)	
	Corporate	Government
Model:	(1)	(2)
Variables		
Sales (% turnover)	-0.0041***	-0.0028
Fixed-effects		
issuer-week	Yes	Yes
instrument	Yes	Yes
Fit statistics		
R^2	0.38133	0.18556
Observations	1,291,997	342,870
F-test (1st stage)	4,587.2	1,373.2

One-way (issuer-week) standard-errors in parentheses
Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 4: Regression results: Sample split into trades of corporate and government bonds (2SLS)

Dependent Variable:	Return r_{it} (%)	
	Corporate	Government
Model:	(1)	(2)
Variables		
Sales (% turnover)	-0.0210***	-0.0022
Fixed-effects		
issuer-week	Yes	Yes
instrument	Yes	Yes
Fit statistics		
Standard-Errors	issuer-week	issuer-week
R^2	0.67671	0.58114
Observations	70,072	18,688
F-test (1st stage)	314.07	129.23

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 5: Regression results: Sample split by type of bond and restricted to stressed period in March 2020 (2SLS)

Dependent Variable:	Return r_{it} (%)	
	All traders	Funds
Model:	(1)	(2)
Variables		
Sales (% turnover)	-0.0039***	0.0008***
Fixed-effects		
Issuer-week	Yes	Yes
Instrument	Yes	Yes
Fit statistics		
R^2	0.33175	0.41814
Observations	1,719,907	1,034,330
F-test (1st stage)	6,134.4	3,088.5

One-way (Issuer-week) standard-errors in parentheses
Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 6: Regression results: Contrasting results from outside selling pressure comprising only asset managers' trades vs other participants (2SLS)