Whose asset sales matter?*

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

[a] *King's Business School, QCGBF*
[b] *Imperial College London*
[c] *International Monetary Fund*
[d] *Bank of England*

Abstract

Using novel data on bond trading in the UK, we develop a new measure of selling pressure that can be applied to any trader. We identify exogenous selling pressure in a bond using traders' sales of other, unrelated bonds. The price impact of a sale depends on who is selling: sales by dealers and hedge funds generate significantly larger impacts than equally sized sales by other investors. We rationalise our findings using a model of differentially informed investors. All else equal, our results suggest that more attention should be devoted to risks to financial stability from these impactful sellers.

Keywords: fire sales, liquidity, fixed income, financial stability

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1. Introduction

When an asset is sold, what happens to its price? Various important economic and financial stability issues depend on the answers to these questions, including the existence and impact of asset fire sales, the real effects of financial market fluctuations, and the determinants of market liquidity. Much empirical research into this question has been narrowly focused on particular classes of investors, owing to the lack of a general measure of selling pressure applicable to a wide range of market participants. Answers obtained from these analyses are necessarily incomplete as the effect of sales may vary depending on who is doing the selling.

We address this limitation by developing a new measure of selling pressure that is unrelated to an asset's fundamental value, which can be used to study the impact of sales in any context featuring transactions data with identifiable counterparties. We apply our measure to transaction-level data on trading in corporate and government bonds. Our key finding is that the price impact of selling a bond depends critically on who is selling: sales by dealers and hedge funds having greater impacts than other types of firm. This result has implications for recent debates about the importance of asset fire sales (Wardlaw, 2020; Choi et al., 2020), and policymakers concerned with understanding and mitigating these risks.

Researchers studying the impact of asset sales are confronted with a basic problem of endogeneity. Suppose we observe an investor selling an asset. In this case, we do not know if the sale was because of a signal about the asset's fundamental value or if it was extrinsically motivated by factors unrelated to the asset's value. Studying price impacts requires isolating the latter motivation.

Our basic insight is this: if, at the same time as selling an asset, an investor is selling many other unrelated assets, then the sale is more likely driven by the investor, rather than the particular features of the asset in question. Intuitively, if we see someone selling her 'golf clubs' then they may be bad clubs. But if that person is also selling, say, her house, her car, her wine collection,. . . then it might simply be that she has lost her job and needs to raise funds quickly. This selling is unrelated to the golf clubs' intrinsic value.

Our paper formalises this basic intuition, and applies it to the bond market. Our empirical work is based on regulatory data on transactions in corporate and government bonds by financial firms in the United Kingdom from 2019 to 2020. The key advantage of this dataset, relative to others that are commonly used, is that it includes the trades of all types of firms that trade bonds, not just a single type of trader. Specifically, our dataset covers trades by dealers, non-dealer banks, hedge funds, asset managers (including mutual funds) and other types of firm.

To study the price impact of market participants' sales of bond i, we define a new measure of selling pressure – *outside selling pressure* – based on these market participants' net sales of bonds *other than bond* i. To control for correlated shocks to bond i and other related bonds we follow Choi et al. (2020) by including issuer-time fixed effects as controls, along with bond fixed effects and the time since the bond was issued. We then use our selling pressure measure as an instrumental variable for investors' sales of bond i.

To build intuition, consider two bonds issued by Dell: Dell A and Dell B. If those traders selling Dell A are net sellers of other bonds to a greater extent than are the sellers of Dell B, our instrumental variable will identify Dell A as facing greater non-fundamental selling pressure than Dell B.[1] The exclusion restriction is that any correlation between the fundamentals of Dell bonds and other bonds is swept up by our fixed effects and controls. If this assumption is satisfied, investors' net demand for Dell A has received an exogenous shock relative to Dell B, and we can use this to identify causally the price impact of sales.

This general approach to deriving exogenous selling pressure is the first main contribution of our paper. Extant work often exploits idiosyncrasies of a particular investor class to obtain identification (such as relying upon mutual funds), which limits their scope to that investor class. In contrast, our approach can be applied to any trade repository that identifies counterparties and the assets being traded, and can be used to compare price impacts across investors of any type.

We regress prices on our selling pressure measure, showing that there are statistically and economically significant impacts on price. Moving from the 5^{th} to the 95^{th} percentile of outside selling pressure is associated with a 25 basis point fall in prices. These effects persist, but halve after a couple of weeks and disappear within five weeks. Price impacts are greater in corporate than government bonds, and greater during the dash-for-cash than in the rest of our sample period. These findings support the exogeneity of our pressure measure, as were selling pressure correlated with news about a bond's fundamentals we would expect to see permanent price effects.[2]

Key to our work, however, is that we extend this analysis by regressing prices on sales by investors of particular types, instrumenting with type-specific outside selling pressure. We show that the price impact of dealers' selling is much larger than that of any other sector, with hedge funds the second most impactful

[1] By non-fundamental selling we mean factors driving sales that are exogenous to the asset *per se*. Distressed selling that is motivated by idiosyncratic shocks to the trader, systemic shocks, or regulatory fiat may be 'fundamental' in a broader sense, but for our purposes will be treated as non-fundamental, as the source of the selling is not the asset's own intrinsic properties.

[2] Indeed, as noted by Wardlaw (2020) in his critique of Coval and Stafford (2007), overshooting in price reversion would also raise concerns.

sector. All else equal, sales by the asset management companies that house mutual funds have relatively minor effects. We show these results are robust to different ways of measuring prices and selling pressure, and to different regression specifications.

This result is our second main contribution. It is consistent with the great emphasis placed by regulators on limits to arbitrage and - *in extremis* - fire sales. Yet it is also consistent with a recent literature (see Wardlaw (2020) and Choi et al. (2020)) questioning evidence of fire sales based on mutual funds' activities, where attention has been focused since Coval and Stafford (2007). The result is made possible by our new measure of selling pressure, which enables comparisons of price impacts across all investor types, rather than relying on the idiosyncrasies of a particular investor class to obtain identification.

Why does price impact depend on who is selling? We offer one explanation, related to information in over-the-counter markets. While our empirical approach enables us, the econometricians, to identify selling that is unrelated to fundamentals, the counterparties to the selling do not have this knowledge. Hence they may demand a price discount when trading with counterparties they believe might have private information, either about the fundamentals of the asset or future trading in the asset – and thus its value in future exchange. Dealers' business models give them access to private information about trading flows and the financial conditions of bond issuers, while hedge funds' business models are based around gaining and benefiting from an informational advantage. As a result, when these informed traders sell assets, counterparties may be more wary and demand price discounts that are greater.

We formalise this logic in a variant of the framework in Kyle (1985). Investors trade an asset in a setting with imperfect information. As in a standard Kyle model there are noise traders preventing full revelation of information. However, we augment the information received by informed traders (liquidity 'demanders') and their counterparties (liquidity 'suppliers'), such that they receive correlated signals about the fundamental value of the security.

The model rationalises our results, and also justifies our instrumental variable approach to estimating price impact. In equilibrium, the price impact of a sale is an increasing function of how well informed is the liquidity demander. To the extent that dealers and hedge funds have better information than other traders, this induces their greater price impact. Price impact is decreasing in how well informed is the liquidity supplier. This reinforces our results – when informed traders such as dealers and hedge funds are forced to sell, this must be facilitated by less informed investors buying at a greater discount. Finally, we show that estimating price impact by OLS is inconsistent where there are public signals driving both price and trading that are unobserved by the econometrician. Estimating Kyle's lambda is thus a question of identification, and requires an instrumental variable such as our measure of selling pressure.

Finally, we show that there is empirical support for this mechanism. The traders we identify as having the highest price impact – dealers – trade with more counterparties in a month than any other type of trader. If any traders have the ability to predict future order flow, it is likely to be dealers. On the other hand, hedge funds trade at the most favorable prices in our data: in any given month they buy bonds at low prices and sell them at high prices. This favorable trading record is consistent with their possessing superior information.

Our results have clear implications for policymakers tasked with monitoring market liquidity and, in particular, minimising risks from fire sales. First, our measure provides a useful tool for analysing liquidity risks stemming from a wide range of financial institutions, which is vital for macroprudential regulation. Second, a key takeaway is not to focus *only* on mutual funds, but to consider risks from sales by a wide range of investors. There is, of course, some logic to the attention traditionally paid to mutual funds, as their simple balance sheet structure and stark redemption policies may force them into sales of illiquid assets (Feroli et al., 2014; Goldstein et al., 2017; Baranova et al., 2017). But policymakers should also consider the *impact* of fire sales conditional on them being triggered, even when the probability of them happening seems small.

2. Related literature

A large empirical literature studies the impact of forced sales of assets on prices and firm outcomes (Ellul et al., 2011; Edmans et al., 2012; Dessaint et al., 2018; Falato et al., 2021; Kundu, 2023a,b). Much of this literature is based on the initial insight of Coval and Stafford (2007) that flows from mutual funds can be used to identify non-discretionary sales by funds, and shows that such sales can have large impacts on price. In recent years, work by Wardlaw (2020) and Choi et al. (2020) has disputed this, suggesting that, after resolving issues with the methodology in earlier works and including more restrictive controls, the price impacts of flow-induced trading seem smaller.[3]

Work in this area has tended to focus narrowly on a particular type of trader in a particular setting. This reflects partly data constraints and partly reliance on mutual funds' simple funding and balance sheet structure

[3] Specifically, Wardlaw (2020) demonstrates that standard flow-induced price pressure measures are a direct function of assets' actual realised returns during the outflow quarter, and hence reflect their fundamentals.

to (ostensibly) identify exogenous sales. Our *methodological* contribution to this literature is to propose a new measure of selling pressure which can be applied consistently across all types of traders. Our approach is striking in its generality. Whereas various methods have been used to construct identification schemes for particular traders in particular markets, our method simply requires transactions data with identifiable counterparties in OTC markets– and, within reason, is agnostic on what is traded and who is trading.

Our *empirical* contribution is to show that selling by dealers and hedge funds is more impactful than selling by other investor types, including segments that encompass mutual funds. We believe further attention in this literature should be devoted to these sectors. We note that which traders 'matter most' for fire sales depends not just on the impact of their selling, but also their propensity to engage in fire sales. Our results speak to the former only and thus complement the work of Ellul et al. (2011) and Manconi et al. (2012) who make clear that selling behavior differs across classes of institution (see also Chodorow-Reich et al. (2020)). Heterogeneous selling behavior of investors makes it important to know heterogeneous *effects* of that selling.

Another literature studies the changing nature of liquidity in over-the-counter markets in recent years. Various papers have pointed to a decrease in the willingness of dealers – the traditional suppliers of liquidity in over-the-counter markets – to intermediate (Duffie, 2020; He et al., 2021). Post-crisis regulation is often cited as an explanation for this (Duffie, 2017). Recent work has highlighted that the traditional view of over-the-counter markets, where dealers only supply liquidity and everybody else only demands liquidity, may no longer apply, with non-dealers playing a significant role in supplying liquidity (Choi et al., 2024).

Our contribution to this literature is to connect it with the literature on fire selling, and to show that the types of traders we typically expect to provide liquidity are those who have the greatest impact on asset prices when they sell. This points to the propensity of traditional liquidity suppliers to become liquidity demanders – whether due to fire sales or sales for other non-fundamental reasons like inventory management – as a key determinant of liquidity and overall market functioning. This is in the spirit of early models where price falls following asset sales are determined by the extent to which 'natural' buyers of assets are constrained or unwilling to buy the assets sold (Shleifer and Vishny, 1992; Kiyotaki and Moore, 1997).

A third literature studies prices, liquidity (Ebsim et al., 2020; Haddad et al., 2020; Schrimpf et al., 2020) and selling behaviour (Barth and Kahn, 2021; Czech et al., 2021a,b) during stress episodes and, in particular, during the 'dash-for-cash' in March 2020.[4] These papers typically either study aggregate market conditions,

[4] Another recent episode is the 2022 gilt market crisis, studied by Pinter (2023).

the role of a particular trader in contributing to distress, or in the case of Czech et al. (2021a) the extent to which different investor types sold assets during the turmoil. Our contribution is different: we highlight the importance of *who* is selling in determining the extent of price falls. We also have a broader focus, studying trading behavior across all market participants in both government and corporate bonds.

A fourth literature builds stress testing models that simulate fire sales and estimate the resulting losses for financial institutions (Baranova et al., 2019; Breuer et al., 2023; Coen et al., 2019). An important but challenging component of these models is the estimation of the price impact of asset sales. Our findings suggest that price impacts in these models should vary according to the type of the seller. Further, our results have implications for which types of investor this literature should focus on.

Finally, a growing literature takes a 'demand system' approach to studying asset pricing and portfolios, building on Koijen and Yogo (2019). Bretscher et al. (2022) apply this methodology to bonds. Their paper, like ours, finds differential price responses to different types of investor adjusting their portfolios. Our approaches are substantively different, but complementary. We focus on empirically isolating exogenous sales and measuring their impact directly. Bretscher et al. (2022) infer demand elasticities from portfolio holdings, combined with a structural model. While we strip out bond fixed effects, they explicitly model them. This is a key element of the the demand system approach, with demand modeled as functions of observable bond characteristics. Furthermore, we highlight the primary importance of dealer trading, a category of investor that does not appear in their data.

3. Data & institutional setting

The core dataset we use is the universe of transactions in government and corporate bonds by entities regulated by the Financial Conduct Authority (FCA). These data are required to be submitted under the MiFID II directives. In practice, this includes nearly all financial firms operating in the UK, including subsidiaries of foreign firms. Note that only one counterparty in each transaction needs be regulated by the FCA in this way, so many non-FCA-regulated entities feature in the data. The bonds traded include those of both British and foreign issuers and include bonds denominated in sterling and other currencies.

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 (i.e., dealers, funds, banks, hedge funds and others). It should be noted that the 'funds' category includes a wide range of fund types. This is due to the fact that in our transaction-level data we can identify only asset management firms, who might be trading on behalf of mutual funds as well as other fund types.

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 being government bonds. Government bonds are traded more frequently, and account for slightly over half the trades in our sample. These statistics highlight the relative turnover of government and corporate bonds, with corporate bonds being traded less despite accounting for the largest majority of instrument captured in our dataset. 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 funds, banks and others, which includes trading services firms such as brokerage firms. This is consistent with the fact that both government and corporate bonds are mainly traded bilaterally over-the-counter (OTC), and rely on dealers to intermediate between buyers and sellers.[5,6] Dealers are therefore key players as they observe the order flow of a wide range of sectors, whilst their clients may be less informed due to the lack of transparency in the market.[7]

Our dataset covers the period from 1st January 2019 to 1st July 2020. This includes March 2020, which was a time of high volatility and low liquidity for financial markets following the onset of the COVID-19 pandemic, and more benign periods before and after. For further details on this episode see Czech et al. (2021a) for sterling markets; and Ebsim et al. (2020), Ma et al. (2022) and Schrimpf et al. (2020) for US markets.

We adopt a much finer observation frequency than is typical in the literature (which generally employs monthly, or even quarterly, frequency) in aggregating trade data to weekly observations. Table 2 summarises trading in our weekly dataset. A large number of unique bonds and traders trade each week.[8] Each trader on average trades 78 bonds a week, and each bond is traded by 10 traders each week conditional on being traded

[5]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 all conditions. For more information see the UK Debt Management Office website. The firms we identify as dealers include GEMMs as well as other large US dealer-banks.

[6]For more details on the gilt market see Benos and Žikeš (2018). For more details on the sterling corporate bond market see Coen and Coen (2022), Mallaburn et al. (2019) and Czech and Roberts-Sklar (2019).

[7]Whilst in US fixed income markets the Trade Reporting and Compliance Engine (TRACE) gives post-trade transparency, there is no equivalent in the UK. For more details and a comparison on trade reporting in the US and the UK we refer to Ivanov et al. (2023).

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

at all that week. These features of trading – the fact that on average 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 the trading data with bond-level information from Eikon Fixed Income data, providing key characteristics of the securities. These characteristics will feature as controls in our analysis.

In complementary analysis, we also make use of data on mutual funds to replicate some of the price pressure measures associated with mutual fund outflows that have been developed in the literature. Specifically, we use data on total net assets, net flows and portfolio holdings data of mutual funds from Morningstar to construct fund-based price pressure measures between 2019 Q3 and 2020 Q2. The funds selected in Morningstar hold between 38-52% of bonds traded in our transaction dataset, as shown in Table A4 in the Appendix.

4. Research design

Let us imagine that we have been able to identify bonds that are 'unrelated', in the sense that their price-relevant fundamentals are uncorrelated. If an investor trading bond i at time t is selling many other unrelated assets at the same time, then it suggests that her trades in i are driven, to a large degree, by the investor's condition, rather than by 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 formalise this intuition as follows: let $s_{i,j,t}$ be the net sales of bond i by trader j at time t, where $s_{i,j,t} > 0$ indicates the trader is net selling the asset, and let iss_i be the issuer of bond i. For bond i and trader j at time t we first define net sales ($z^{NS}_{i,j,t}$) and transactions ($z^{T}_{i,j,t}$) of bonds k issued by entities other than iss_i:

$$\begin{aligned} z^{NS}_{i,j,t} &= \sum_k \mathbf{1}(iss_i \neq iss_k) s_{k,j,t} \\ z^{T}_{i,j,t} &= \sum_k \mathbf{1}(iss_i \neq iss_k) |s_{k,j,t}| \end{aligned}$$

where $\mathbf{1}()$ is the indicator function. We then compute our measure of selling pressure – *outside selling pressure* $z_{i,t}$ – as the percentage net sales of bonds other than those issued by iss_i by all investors $j \in \mathcal{J}$ that are selling bond i:

$$z_{i,t,\mathcal{J}} = \frac{\sum_{j \in \mathcal{J}} \mathbf{1}(s_{i,j,t} > 0) z_{i,j,t}^{NS}}{\sum_{j \in \mathcal{J}} \mathbf{1}(s_{i,j,t} > 0) z_{i,j,t}^{T}} \tag{1}$$

where $\mathcal{J}$ is a set of investors of a particular type. In our initial regressions we take $\mathcal{J}$ to be all traders in our data, and in our sector-level analysis we split traders up into dealers, banks, funds, hedge funds, and others.

In our analysis below, we run two types of regressions. The first is a two-stage least squares specification, where the equation we are estimating is:

$$p_{i,t} = \sum_{\mathcal{J}} \beta_{\mathcal{J}} s_{i,t,\mathcal{J}}^{V} + X_{i,t}\gamma + \epsilon_{i,t} \tag{2}$$

where $s_{i,t,\mathcal{J}}^{V}$ denotes net sales of i at time t by investor type $\mathcal{J}$ as a percentage of the average weekly trading volume in that bond by all investors, where this average is taken over all weeks in which that bond trades. $p_{i,t}$ denotes the bond's price, $X_{i,t}$ is a set of control variables and $\epsilon_{i,t}$ is an error term.

We use our selling pressure measure $z_{i,t,\mathcal{J}}$ as an instrumental variable for these sales $s_{i,t,\mathcal{J}}^{V}$. The coefficient $\beta_{\mathcal{J}}$ can then be interpreted as the marginal causal effect of sales by sector $\mathcal{J}$ on prices.

Our instrumental variable approach can be thought of as solving two classic problems with estimating a regression like equation (2) using OLS. The first is a problem of simultaneity: price and quantity are jointly determined by the interaction of demand and supply, and simply regressing price on quantity does not recover any structural parameters. To identify structural parameters one needs an exogenous shifter. This is what outside selling pressure gives us: an exogenous shift in a set of traders' demand for a bond. The second issue is that of endogeneity or reverse causality discussed above: perhaps sales were driven by a signal about the bond's value. This problem is solved by having an instrumental variable for sales. Again, this is provided by outside selling pressure.

The second type of specification we run is the reduced-form of our two-stage least squares regressions:

$$p_{i,t} = \sum_{\mathcal{J}} \delta_{\mathcal{J}} z_{i,t,\mathcal{J}} + X_{i,t}\eta + \nu_{i,t} \tag{3}$$

Here $\delta_{\mathcal{J}}$ is the marginal effect of *outside selling pressure* from sector $\mathcal{J}$ on prices.

We reiterate that in all our analysis our pressure measure for bond i is computed based on investors' net sales of bonds *other* than i. As such, we use minimal information about investors' behavior with respect to bond i while capturing the natural requirement that they are reducing their position in the bond. This minimal use of bond i information (other than the sign of the trade) protects against concerns that our pressure measure is directly related to i's fundamentals. Furthermore, in our regressions we use notional net sales of bonds rather than value-based measures, helping us to avoid mechanical correlations of the sort identified by Wardlaw (2020).

Clearly the requirement that the other bonds being sold are 'unrelated' to i is unlikely to be satisfied by simply considering bonds that are issued by a different issuer. Sales of securities from different issuers may reflect shared 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 time-varying industry fundamentals. 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.

We include a set of demanding fixed effects and controls that, together with our instrumental variable, eliminate these endogeneity concerns.[9] Specifically, in each regression we include issuer-time fixed effects. That is, we exploit within issuer-time variation, such that even if $z_{i,t}$ encodes confounding variation about the issuer, this variation should be absorbed. This powerful approach, following Choi et al. (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-time-level is stripped out. *A fortiori* this will absorb time varying factors common to all issuers, such as industry-specific or economy-wide trends, as these are constant within issuer-time.

It is difficult to think of remaining fundamental variation that would survive both our instrumental variable approach and this fixed effect, though not impossible. For this reason, we include instrument fixed effects and control for the time since issuance of the bond. Once we have added our fixed effects and the aforementioned controls, our identification assumption is $cov(z_{i,t}, \epsilon_{i,t}|X_{i,t}) = 0$. At this point, the main concern with the specification is perhaps whether we retain enough non-absorbed variation to allow us to assess the effect of non-fundamental sales.[10] However, as shown below in our results section, it turns out that we retain ample

[9] An alternative would be to refine z_{it} by adopting a selection rule that filters the trades of 'other bonds' that feature in the calculation of z_{it}, exploiting information about the traders, bonds or the context of the trade. However, this 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.

[10] Our issuer-time fixed effects will certainly absorb some variation at the issuer-time level that *is* non-fundamental and thus

non-fundamental variation for precise estimation of the effects of non-fundamental sales.

We now briefly discuss properties of our outside selling pressure measure $z_{i,t}$. Table 3 summarises the distribution of prices $p_{i,t}$, outside selling pressure $z_{i,t}$ and sales $s^V_{i,t}$. In Figure 1 we plot the distribution of outside selling pressure $z_{i,t}$ through time for all traders. Our identification draws on the enormous cross sectional dimension of our data, rather than the time series alone. Notwithstanding this, it is reassuring that our measure exhibits a spike during the 'dash for cash' in March 2020 period which, anecdotally and in aforementioned academic studies, has been argued was associated with fire selling pressure.[11]

Our measure is positively – but weakly – correlated with various measures of fire selling pressure in the literature derived from mutual fund flows. Table 4 shows the correlation between outside selling pressure – calculated only for funds – with the flow-induced pressure measure introduced in Coval and Stafford (2007) and the 'flow-to-stock' (F2S) and 'flow-to-volume' (F2V) measures constructed in Wardlaw (2020). The correlations indicate that there is *some* common variation, perhaps indicating that some of the variation in outside selling pressure comes from fire sales, and that the sales by the asset management firms in our transaction data – whom we label 'funds' – reflect the trading of the mutual funds they contain as a subset. Notwithstanding these results, we note that there is no requirement for our measure to be strongly correlated with these proxies. Indeed, following Wardlaw (2020), it is unclear that any of these alternative series are appropriate measures of non-fundamental sales. Furthermore, the demanding set of fixed effects we use likely absorbs a substantial component of any fire-selling pressure by these mutual funds, and our measure includes other non-fundamental trading by mutual funds that are not linked to flows.

Non-fundamental selling does not necessariy originate from distress, as is typically emphasised in the fire sales literature. In fact, there are many sources that spring to mind as driving non-fundamental sales: mechanical rebalancing of portfolios to meet regulatory/calendar deadlines, mandate shifts from clients, or changes in their demand for liquidity. In this paper, given the generality of our measure, we are agnostic about the drivers of non-fundamental sales. Our focus is on measuring their impacts, conditional on them being triggered.

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ideally could be exploited to estimate the effect of selling, but it is impossible to extract this variation without opening the door to confounding variation.

[11] The dispersion of selling pressure also increases at the end of the calendar year. Trading activity tends to drop around the end of the year, so this could partly be an artifact of relatively few trades. Alternatively, it could represent firms seeking to 'window dress' their balance sheets at the end of the year, a process which is facilitated by other traders (van Horen and Kotidis, 2018; Morey and O'Neal, 2006).

5. Results

In this section we use our new measure of selling pressure to study the price impacts of selling. We first study how selling pressure aggregated over all sectors affects asset prices, and how this varies across assets and according to financial market conditions. We then study how the price impact of selling depends on the type of investor that is doing the selling.

5.1. Price impacts

Table 5 shows results from our reduced-form regression of equation (3) of asset prices on the selling pressure of all traders, plus controls.[12] Our preferred specification is shown in the final column, but for completeness we show our estimated coefficient as we build up from week fixed effects to our most stringent specification of issuer-week fixed effects. Our coefficient estimates are relatively stable across specifications.

According to our preferred specification in the final column of Table 5, selling pressure has a statistically significant negative impact on asset prices. The magnitudes are meaningful: moving from the 5^{th} to the 95^{th} percentile of selling pressure causes a fall in price of $0.37 \times 0.68 = 0.25\%$, so 25 basis points. This is a meaningful effect relative to the median absolute weekly change in the price of a bond, which is 70 basis points. The significance of these effects is striking given the extremely demanding set of fixed effects we include in our regressions.

Table 6 shows how the impact of selling pressure varies across bonds and across time periods. A natural division within fixed income is between corporate and government bonds. In the first two columns, we show results distinguishing between these two types of securities. The coefficient on pressure for corporate bonds is over four times greater than that for government bonds, which is not statistically different from zero. This is consistent with the view that liquid assets should not exhibit as much of a price effect when sold.[13]

Various models of fire sales and liquidity suggest the price impact of forced selling will depend on the financial condition of traders and assets as a whole. The final two columns of Table 6 show how the coefficient on selling pressure differs between the 'dash for cash' episode in March 2020 and the rest of the sample. The coefficient during the dash-for-cash is 50% greater than the coefficient in the rest of the sample period, in line with what models of fire sales would predict (Kyle, 1985).

[12] Note that we cannot run the two-stage least squares specification here, as the net sales of all investor types by definition sum to zero, meaning there is no variation in the main regressor of interest. Hence using the reduced form specification.

[13] Indeed, various models (for example Coen et al. (2019)) suggest that this is precisely why liquid assets might be sold by distressed firms, before less liquid assets where they might be forced to realise a loss due to fire sale-depressed prices.

Figure 2 shows how the price impacts of selling pressure persist through time. In particular, we run the following version of our reduced-form regression:

$$p_{i,t+\tau} = \delta z_{i,t} + X_{i,t}\eta + \nu_{i,t} \tag{4}$$

where $\tau = 0, 1, ..., 6$ weeks and $z_{i,t}$ is computed for all investor types.[14]

The coefficient dies away over time: after 2 weeks the effect of an increase in selling pressure halves relative to the contemporaneous effect, and after 5 weeks the effect of selling pressure on prices is statistically insignificant. Non-fundamental sales should not have permanent effects, as they by definition should not reveal anything about the future cashflows of the bond. By contrast, sales based on a bond's fundamentals should have permanent effects. As a result, Figure 2 provides reassuring evidence that our measure isolates non-fundamental trading, as it is inconsistent with *any* alternative rationale for our sales that is based on the bond's fundamentals.

5.2. Whose sales matter?

In the previous section we showed that non-fundamental selling can have large impacts on asset prices, and that the nature of these price impacts are consistent with the sales being unrelated to the bond's fundamentals: they have greater impacts in stress and in less liquid bonds, and their impact on prices dies away over time.

How can these results be reconciled with recent studies based on mutual funds showing sales induced by fund outflows have relatively modest impacts on price (Wardlaw, 2020; Choi et al., 2020)? In this section we seek to answer that question, by showing that sales by different types of firms have radically different impacts on price. To do this, we run two-stage least squares specifications as set out in equation (2), where we instrument for sectors' sales of a bond using outside selling pressure measure $z_{i,t}$.[15] As in all our specifications, we include issuer-time and instrument fixed effects as well as controlling for the time since the bond was issued.

Figure 3 shows the estimated price impact coefficients for each sector. The underlying regression table is shown in Table A1 in the appendix.[16] To facilitate interpretation, summary statistics of sales and selling

[14] To ensure we have a consistent sample of bonds and weeks for these regressions, we only include observations where a bond was traded (at least) 7 weeks in a row. As a result the coefficient for $\tau = 0$ does not need to match the coefficient in Table 5.

[15] We are able to do this for the sector-level regressions as there is variation in sales across instruments and time. By contrast, when we include all traders in our specifications, total net sales are by definition zero.

[16] The first-stage regressions – regressing sector-level sales on sector-level pressure – and the reduced-form regressions – regressing price directly on sector-level pressure $z_{i,t}$ – are shown in Tables A2 and A3 in the appendix.

pressure are given in Table 7. These coefficient estimates can be interpreted as the marginal impact on price of an extra unit of sales of an asset by a given sector, scaled by the asset's average trading volume by all traders.

There is a clear ordering across sectors: non-fundamental sales by dealers have the largest impact on price, followed by hedge funds and banks, followed by funds and other firms. The magnitudes for the most impactful sectors are substantial: a one standard deviation increase in dealers' net sales of a bond decreases its price by $0.10 \times 69 = 6.9$pp, which is greater than one standard deviation of prices. Hedge funds have the second highest price impact: sales of the same magnitude by hedge funds would decrease prices by $0.046 \times 69 = 3.2$pp. The *differences* in impacts across different type of investors are large: a sale by funds has a 9 times smaller impact than sales by a dealer. To be clear, regardless of sector, we are identifying non-fundamental sales - it is the impact that differs.

In Section 6 we discuss these results in more detail, propose a model for why it matters who is selling, and provide empirical evidence in favour of this mechanism.

5.3. Robustness

Our key empirical result is to show the stark differences in price impacts across types of firm. Here we demonstrate that this result is robust along a number of dimensions, with details given in the Appendix:

- *Fixed effects.* We show that our results are robust to varying the fixed effects we include in our regressions.

- *Calculation of selling pressure.* We show that our results are robust to using an alternative measure of selling pressure to that shown in Equation 1. Our baseline measure weights firms by the size of their sales. We define an alternative measure that weights firms equally, and show our results do not change when we use this alternative measure.

- *Alternative measures of bond prices.* We show that our results do not change when we use log prices – rather than prices – as the dependent variable.

- *Nonlinear regression specifications.* We show that there is no evidence of nonlinear price impacts that would affect our results.

6. Why does it matter who is selling?

We have used our measure of outside selling pressure to isolate exogenous selling pressure in a bond, and used this to study the price impacts of sales. Our key result is that a sale of an asset can have a very different impact on its price depending on who is selling it. Why should this be? We offer an explanation based on perceived differences in how informed counterparties are.

6.1. Intuition

The key innovation of our instrumental variables approach is that it identifies selling that we, the econometrician, know to be unrelated to the asset's fundamentals. However, the counterparties to this selling – and the market as a whole – do not know this at the time of the trade. The counterparty is then faced with an inference problem, where they must establish the extent to which this sale – which *we* know to be non-fundamental but *they* do not – reveals some private information.

For our purposes, it is useful to distinguish two types of 'fundamental' properties of an asset (Amihud et al., 2005). The first relate to cash flows of the asset – something that in any model would naturally be interpreted as fundamental, and which prevails even in the absence of any trading frictions. The second emerges as a fundamental component in the realistic case of markets exhibiting frictions that imply liquidity or convenience premia in asset values (Amihud et al., 2005; Acharya and Pedersen, 2005; Krishnamurthy and Vissing-Jorgensen, 2012). In this context, fundamental information might relate to knowledge of order flow or, more broadly, superior awareness of likely changes in future trading conditions that might influence the asset's price. A trader may ask herself whether her counterparty's selling signals there will be further sales of the asset in the future, either by the counterparty or by other market participants.

Our results are consistent with these stories. Dealers have many informational advantages over other investors. They may have direct lending relationships with the issuers of the bonds they are trading, from which they can gather information about their probability of default. They may also have been involved in underwriting the initial issuance of the bond in question. As shown by Goldstein et al. (2021), underwriters benefit from the informational advantage gained during the book-building process which extends to secondary markets.

Dealers also observe a large portion of the order flow for the bonds in which they trade, since much trading occurs via them. This has been shown to give dealers an informational advantage over other traders in opaque over-the-counter markets (Bessembinder et al., 2006; Kondor and Pinter, 2022; Pagano and Roell, 1996). This is particularly important for the OTC bond markets we consider where transactions are not publicly reported.[17]

As a result of these informational advantages, when another trader observes a sale by a dealer, it is reasonable for them to conjecture that this potentially encodes fundamental information about the asset, and to demand a discount as a result.

Similarly, hedge funds are typically considered to be informed traders. This is perhaps most natural to envisage in terms of the cash-flow aspect of fundamentals, rather than in terms of order flow, given the research intensity of many hedge funds. Nevertheless, Czech et al. (2021c) show that hedge funds trade in a manner that also suggests informational advantages regarding future trading flows and bond fundamentals.

6.2. Model

Taking stock, it is plausible that a trader should demand a higher discount to buy from a hedge fund or dealer, than a less informed trader. More generally, this story suggests that there should be differential price discounts that emerge from non-fundamental selling. We now formalize these intuitions in a model building on Kyle (1985) where both fundamental and non-fundamental sources of selling are captured in a stylized manner, and where we can structurally draw out sources of heterogeneous price impact of selling. In Section 6.3, we provide empirical evidence in favour of these mechanisms.

6.2.1. Setting

There are three traders in our model. We refer to them as a liquidity demander (associated with subscript D), a liquidity supplier (associated with subscript S), and a noise trader. There is a single asset, with end-of-period value v. The prior distribution of v is Gaussian, with mean p_0 and variance Σ. The prior mean, p_0, is observed by all traders, but not by the econometrician.

Demanders and suppliers each receive signals of the asset's value:

$$\begin{aligned} v_D &= v + \epsilon_D; \\ v_S &= v + \epsilon_S; \end{aligned} \tag{5}$$

[17]We note that this is different from the US markets where transactions are publicly reported in TRACE. For more details and a comparison on trade reporting in the US and the UK we refer to Ivanov et al. (2023).

where ϵ_D and ϵ_S are uncorrelated error terms with mean zero and respective variances σ_D^2 and σ_S^2. Both demander and supplier are risk neutral.

The liquidity demander submits a market order of x. Noise traders submit a market order of u, which has mean 0 and variance σ_u^2. The liquidity supplier observes the sum of the market orders $y = x + u$ but not its constituent parts.

A key feature of the Kyle model that we retain is that the noise traders and the informed liquidity demander are indistinguishable from the perspective of their counterparty, the liquidity supplier. We deviate from the standard Kyle model by introducing noise to the informed agent's signal and by introducing a signal (again with noise) for the supplier. This allows us to parameterize how *informed* is a given agent relative to their counterparty.

Our framework also admits an alternative interpretation, whereby the liquidity demander and noise trader are both components of a single agent whose trades are partly motivated by an understanding of asset-specific fundamentals, but also partly by factors orthogonal to the asset – such as shocks to their funding, financial distress, regulatory requirements and so forth. Under our alternative interpretation, for a given trader, sales for fundamental and non-fundamental reasons are indistinguishable, from the perspective of their counterparty.

In either interpretation, it remains the case that the liquidity supplier faces a signal extraction problem. The attraction of the second interpretation is that it brings us closer to our empirical application. Even informed traders may often trade for reasons unrelated to the asset – noise, in our setting – but the relative importance of the two motivations for selling will vary across types of trader, as discussed above.

Informed by their signal extraction solution, the liquidity supplier sets the price p and takes the other side of the trade. As is standard, we assume liquidity supply is perfectly competitive, such that they make zero profits on average. Liquidity demanders make profits on average, reflecting their informational advantage, leaving noise traders to incur offsetting losses.

The price impact – Kyle's lambda – is the amount by which the price falls as a function of the market order $y = u + x$. We now show how this will depends on how informed the liquidity demander is perceived to be.

6.2.2. Equilibrium

There is an equilibrium where prices are linear functions of the prior of the asset's value, the liquidity supplier's signal, and the trading quantity. As the logic of the equilibrium is much the same as in (Kyle, 1985), we leave the details of the proof to the appendix. Here we only show and interpret the results.

Prices take the following linear form:

$$P(y, v_S) = p_0(1-\gamma) + \gamma v_s + \lambda y \tag{6}$$

where

$$\gamma = \frac{\Sigma(2\sigma_D^2 + \Sigma)}{2(\sigma_D^2 + \Sigma)(\sigma_S^2 + \Sigma) - \Sigma^2} \tag{7}$$

$$\lambda = \frac{1}{2\sigma_u} \frac{(1-\gamma)\Sigma}{\sqrt{\sigma_D^2 + \Sigma}} \tag{8}$$

The final term is a modified version of Kyle's lambda. It is straightforward to show that price impact is:

1. decreasing in the variance of noise trading σ_u^2
2. increasing in the precision of the liquidity demander's signal v_D
3. decreasing in the precision of the liquidity supplier's signal v_S

We note that when the liquidity supplier receives no signal and the information of the liquidity demander is perfect, this setting simplifies to Kyle's original setup. In that case, we obtain the classical Kyle's lambda, where price impact is half the ratio of the standard deviations of the asset value to noise trading. In this more general version the price impact is additionally a function of the precision of traders' signals.

6.2.3. Implications

These theoretical results can help us understand the source of our empirical findings. We focus on three implications - two explicitly about heterogeneous price impacts and one that further justifies our instrumental variable approach.

Implication 1. Traders that are more informed will have greater price impact when selling. Price impact is increasing in the precision of the liquidity demander's signal. As a result, the price impact of sales by traders typically seen to be more informed will be larger. This will be true when these firms trade on information, but also when they trade for non-fundamental reasons – that is, the sort of trading that our pressure measure picks up. Traders would like to limit their price impact by claiming not to be informed, but cannot credibly do so. To emphasize, informed traders' trades have price impact, *even if the trade in question is not based on superior information.*

Implication 2. Better informed liquidity suppliers will lead to lower price impact. Price impact is decreasing in the precision of the liquidity supplier's signal. Where a liquidity supplier has less precise information, they are at greater danger from adverse selection, and thus will only supply liquidity at worse prices.

This result is similar in spirit to models of price dislocation when 'specialists' are constrained or exit the market (Shleifer and Vishny, 1992; Kiyotaki and Moore, 1997). Here a specialist liquidity supplier is one who has good information. In the model the information is about the asset's fundamental value. Again, this value may reflect cash flows or 'costs avoided' in transactions predictable by current and anticipated order-flow. When specialist liquidity suppliers are constrained, and other less informed traders take their place, price impacts will be greater.

Implication 3. Regressing price on trading quantity gives inconsistent estimates of price impact. Our simple model not only provides conceptual insight, but also bolsters the justification for the instrumentation variable approach we have taken in our empirical analysis. Rather than appealing to general sources of endogeneity in OLS regression specifications, we can demonstrate precisely one source of bias.

Suppose we wished to estimate price impact of selling by a given set of traders, using data on asset prices p_t and trades y_t. Suppose also that these data are generated by our model. As such, we take y_t to be the sum of informed trading and noise trading, and p_t the equilibrium price. It follows that:

$$p_t = p_0 + \lambda y_t + e_t \tag{9}$$

where:

$$e_t = \gamma(v_{s,t} - p_0) \tag{10}$$

For ease of exposition, denote the asset's value $v_t = p_0 + \epsilon_{v,t}$, where $\epsilon_{v,t}$ has mean 0 and variance Σ. In this case informed trading is given by

$$x_t = \frac{\sigma_u}{\sqrt{\sigma_D^2 + \Sigma}}(\epsilon_{v,t} + \epsilon_{D,t})$$

with

$$e_t = \gamma(\epsilon_{v,t} + \epsilon_{S,t})$$

The presence of $\epsilon_{v,t}$ in both the regressor (via the informed trading component of y_t) *and* the error term, shows that OLS would be inconsistent. This is due to shocks to asset value (unobserved by the econometrician) driving both price and trading. This makes clear that estimating Kyle's lambda is a question of identification, and to estimate it we need an instrumental variable. The ideal instrumental variable is something that captures noise trading but is uncorrelated with information-based trading. This is precisely what our empirical approach achieves.

6.3. Empirical evidence on the mechanism

The model above identifies information – or more specifically, who is perceived to be informed – as a potential mechanism underlying our results. In this section we provide evidence in favour of this mechanism. As discussed above, in bond markets informational advantages can take two forms. Firstly, traders may have better information on the future cash flows of a bond. Secondly, traders could have better information on future trading in a bond. In Figure 4 we provide evidence that the types of firms with the largest price impacts are those that appear to benefit from these types of informational advantages.

The first panel shows that firms from certain sectors are able to trade at more favourable prices. For each firm that buys and sells a bond in a given month, we compute the difference between the average price at which they sell the bond and the average price at which they buy the bond. We then regress this variable on dummy variables for each sector in our data. We plot the results in the first panel of Figure 4. Our estimates suggest hedge funds trade at the most favourable prices. This is consistent with their role as arbitrageurs, and with the evidence elsewhere in the literature that they are informed traders (Czech et al., 2021c). This lends empirical support to the argument that hedge fund sales have relatively large price impacts because of their informational advantage.

The second panel shows how the number of trading counterparties varies across different sectors. For each firm trading a bond in a given month, we compute the number of counterparties that firm trades that bond with. We then regress this variable on dummy variables for each sector in our data. We plot the results in the second panel of Figure 4. Dealers are, unsurprisingly, the most connected type of sector, consistent with their role as intermediaries in these markets. As argued elsewhere in the literature, dealers are able to use these connections to extract valuable information (Kacperczyk and Pagnotta, 2019; Brancaccio et al., 2017). This superior connectedness supports our argument that dealer sales have large price impacts due to their informational advantages.

7. Conclusion

We have proposed a new method for identifying exogenous selling pressure, and used it to explore how bond sales affect prices. We have shown that sales have significant impacts on bond prices, that are greater in less liquid markets and during stressed periods, and dissipate through time. This is consistent with the intuition behind how markets should react to non-fundamental sales.

Most strikingly, we have shown that the price impact of an asset sale depends critically on the type of institution selling. Sales by dealers and hedge funds generate significantly larger impacts than sales of the same size by other investor types. All else equal, our results suggest that more attention should be devoted to risks stemming from these more impactful sellers.

We have provided a theoretical framework to propose a mechanism for our results. The patterns we identify can be generated in a model of market liquidity where agents differ in the degree to which they are perceived to be informed traders. Regardless of whether they are actually trading on the basis of superior information, counterparties cannot infer their motivations and thus demand a larger price discount when dealing with a more informed trader.

Our method can be applied to transaction-level data on any market where traders have identifiers. As a result, we hope this work can be of use to researchers and practitioners looking to quantify the price impacts of asset sales in a broad array of markets and applications.

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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		
Fund	43	15
Bank	9	14
Dealer	3	51
Hedge Fund	6	2
Other	39	18

Note: 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 funds. The second numeric column shows the percentage of total trades accounted for by each bond and trader type. 'Other' traders include pensions funds, liability-driven investment funds, central counterparties, principal trading firms, brokerage firms, and sovereign wealth funds, among other firm types.

Table 2: Instruments & Traders per week

	Number
Instruments	23,588
Traders	2,922
Instruments per Trader	78
Traders per Instrument	10

Note: 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, and excluding traders with missing trader IDs. The table shows averages of the statistics across the weeks in the sample.

Table 3: Prices, Sales & Pressure

	Mean	Std. dev.	95^{th} - 5^{th} pctile
Prices $p_{i,t}$	99.82	4.86	5.65
Sales $s^V_{i,t}$	0.36	67.73	144.06
Pressure $z_{i,t}$	0.02	0.22	0.68

Note: Table summarises the distributions of asset prices, sales, and selling pressure. Prices $p_{i,t}$ are expressed as a percentage of par. Sales $s^V_{i,t}$ are net sales as a percentage of average trading volume in the instrument. Pressure $z_{i,t}$ is defined in Equation 1, and takes values between -1 and 1. The distributions of prices are within-bond distributions. For example, to compute the standard deviation of prices we first compute the standard deviation of weekly prices for each bond i, and then take the average of this across bonds.

Table 4: Outside selling pressure and fund-flow-based measures

	Fund outside selling pressure $z^F_{i,t}$					
	(1)	(2)	(3)	(4)	(5)	(6)
Coval-Stafford	0.049***			0.007***		
	(0.001)			(0.002)		
Wardlaw F2V		0.018***			0.0002	
		(0.0005)			(0.0007)	
Wardlaw F2S			0.019***			0.003***
			(0.0005)			(0.0008)
R^2	0.005	0.002	0.002	0.38	0.30	0.30
Observations	335,335	830,292	830,292	335,335	830,292	830,292
Issuer-Week fixed effects	No	No	No	Yes	Yes	Yes
Instrument fixed effects	No	No	No	Yes	Yes	Yes

Note: Table summarises the positive correlation between outside selling pressure – computed solely for funds – and various measures of selling pressure induced by mutual fund flows defined in the literature. The Coval-Stafford measure is the measure introduced by Coval and Stafford (2007). The two Wardlaw measures are introduced by Wardlaw (2020), with F2V denoting his flow-to-volume measure and F2S denoting his flow-to-stock measure. We multiply each measure by -1 such that a large positive number entails positive selling pressure. For each week, we then take the percentile rank of each measure, such that each week our regressors are distributed between 0 and 1. For further details on the pressure measures see Appendix C.3 .

Table 5: Price changes and selling pressure

	Price (%)			
	(1)	(2)	(3)	(4)
Pressure $z_{i,t}$	-0.3208**	-0.2486***	-0.2991***	-0.3727***
	(0.1366)	(0.0961)	(0.0552)	(0.0521)
R^2	0.80541	0.82310	0.84703	0.89582
Observations	1,514,387	1,514,387	1,514,387	1,514,387
Instrument fixed effects	Yes	Yes	Yes	Yes
Week fixed effects	Yes	No	No	No
Country-Week fixed effects	No	Yes	No	No
Country-Sector-Week fixed effects	No	No	Yes	No
Issuer-Week fixed effects	No	No	No	Yes

Note: Table shows the results of our reduced-form regression of prices on selling pressure and controls, where pressure is computed for all types of investor, together with different combinations of fixed effects. Time since issuance is included as an additional control. Standard errors are shown in parentheses, and are twoway clustered at the level of the fixed effects. ***, ** and * respectively denote significance at the 0.1%, 1% and 5% levels of significance.

Table 6: Heterogeneity: bond type and stressed periods

	Price (%)			
	Corporate	Government	March 2020	Rest of sample
	(1)	(2)	(3)	(4)
Pressure $z_{i,t}$	-0.468***	-0.102	-0.593***	-0.402***
	(0.055)	(0.114)	(0.176)	(0.052)
R^2	0.89	0.90	0.97	0.90
Observations	1,193,684	320,703	80,541	1,433,846
Issuer-Week fixed effects	Yes	Yes	Yes	Yes
Instrument fixed effects	Yes	Yes	Yes	Yes

Note: Table summarises how the price impacts of selling pressure vary according to the type of bond and the state of the economy. The first column runs our reduced-form regression only for corporate bonds, and the second column includes only government bonds. The third column runs the regression only for March 2020, a time of great financial stress, and the fourth runs the regression for all periods except March 2020. Time since issuance is included as an additional control.

Table 7: Sector sales & pressure

Sector	Mean	Std dev	95^{th} - 5^{th} pctile
Sales $s^V_{i,t}$			
Bank	-0.6	46.0	66.5
Dealer	-0.5	68.7	149.8
Fund	0.5	48.3	78.4
Hedge fund	0.1	14.4	3.5
Other	0.3	42.6	52.4
Pressure $z_{i,t}$			
Bank	-0.01	0.14	0.40
Dealer	0.00	0.07	0.12
Fund	0.01	0.16	0.40
Hedge fund	0.00	0.07	0.00
Other	0.01	0.16	0.32

Note: Table summarises the distributions of sales and selling pressure by investor type. Sales $s^V_{i,t}$ are net sales by investors of a given type as a percentage of average trading volume in the instrument. Pressure $z_{i,t}$ is defined in Equation 1 as the net selling of bonds other than i by investors of a given type, and takes values between -1 and 1.

Figures

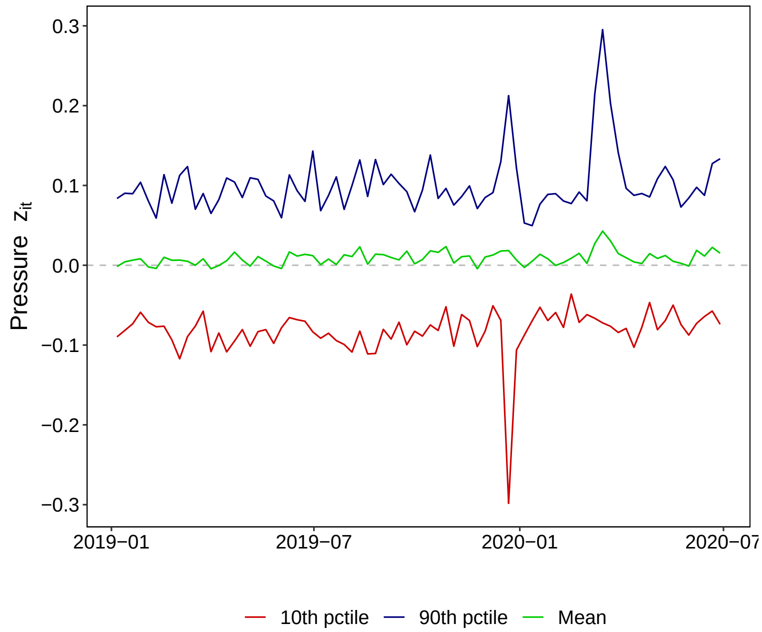

Figure 1: Outside selling pressure through time

Note: This figure summarises the distribution of outside selling pressure $z_{i,t}$ through time. For each week we compute the mean, 10^{th} and 90^{th} percentiles of $z_{i,t}$ across bonds, where $z_{i,t}$ is computed across all traders. We then plot these series through time.

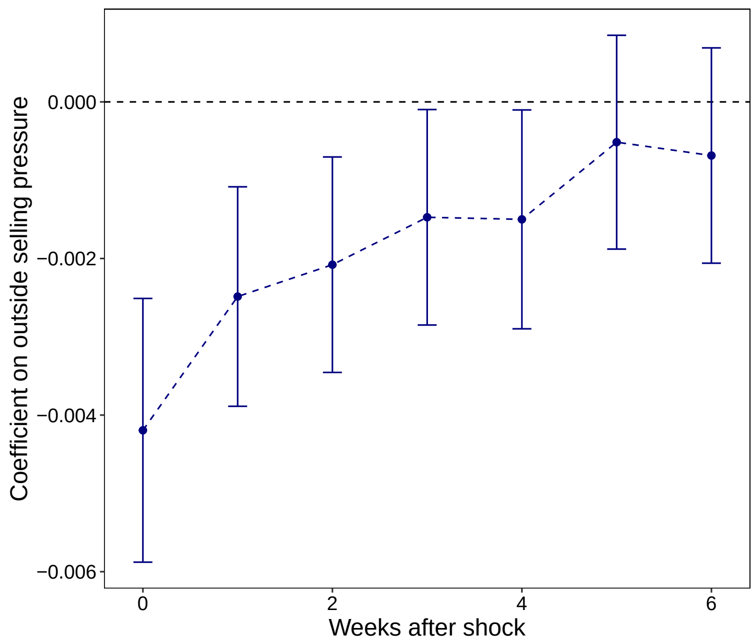

Figure 2: Price impacts through time

Note: This figure shows the duration of the price effects of outside selling pressure $z_{i,t}$. In particular we run regressions as set out in Equation 4, where we related price at time $t + \tau$ to pressure and controls at time t. The x-axis plots τ and the y-axis plots the coefficient estimate δ. To ensure we have a consistent sample of bonds and weeks for these regressions, we only include bonds that are traded on (at least) 7 consecutive weeks. As a result the coefficient for $\tau = 0$ need not match the coefficient in the final column of Table 5.

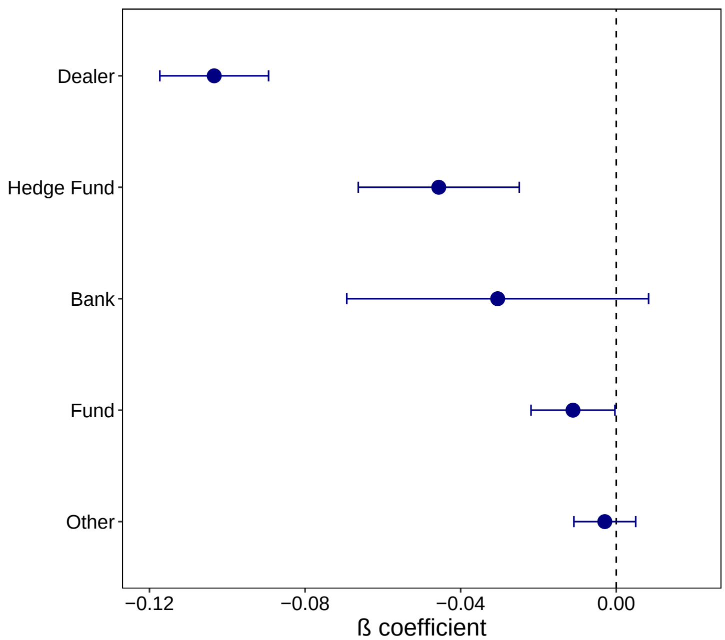

Figure 3: Price impact by sector

Note: This figure shows the price impact of sales by different types of investor. The x-axis shows the estimated coefficient β in Equation 2, and can be interpreted as the marginal effect of increasing the sales of asset i by a given sector on the asset's price. Error bars show 95% confidence intervals. Prices are expressed as a percentage of par. Sales are net sales by investors of a given type as a percentage of average trading volume in the instrument. Controls include issuer-time and instrument fixed effects as well as the time since a bond was issued. Standard errors are clustered at the issuer-time and instrument levels.

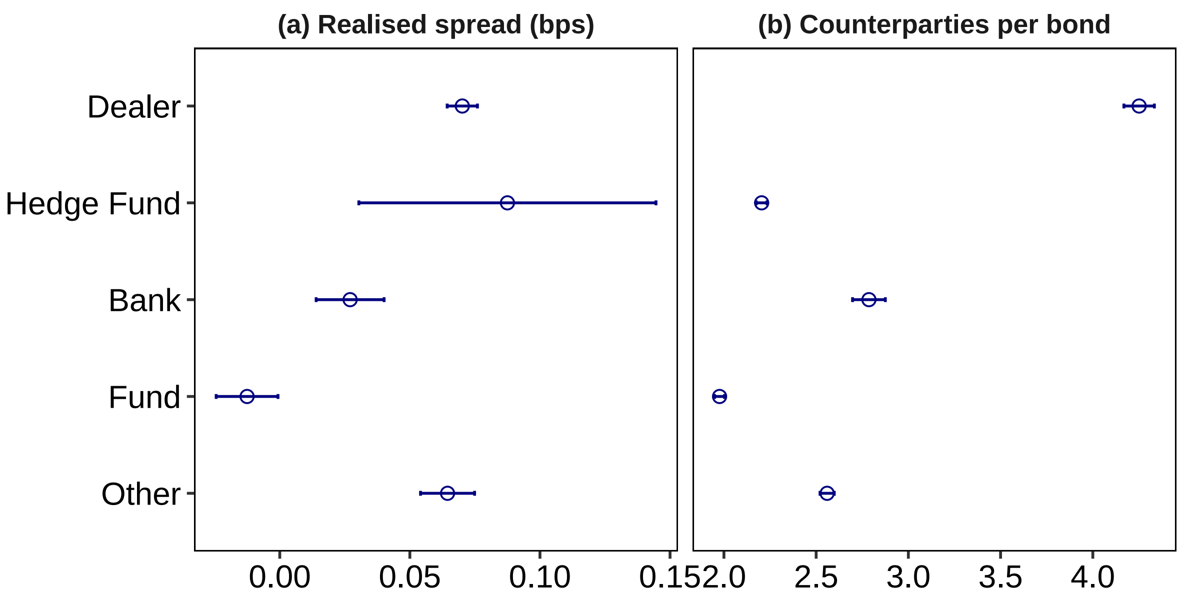

Figure 4: Trading performance & network connections by sector

Note: Figure shows how sectors vary in terms of the prices at which they trade and the number of counterparties they trade with. For each firm that buys and sells a bond in a given month, we compute the difference between the average price at which they sell the bond and the average price at which they buy the bond. We then average this over all months and bonds, and show averages for firms in each of our 5 sectors in the left panel. Horizontal bars show 95% confidence intervals. In the right panel we take each firm in each month and, for each bond they traded, count the unique counterparties with which they traded. We then average these numbers across all bonds and months, and show averages for firms in each of our 5 sectors in the left panel. Horizontal bars show 95% confidence intervals.

Appendix A. Proofs for Model in Section 6.2

Appendix A.1. Equilibrium

We begin by noting that

$$\mathbb{E}(v|v_j) = w_j p_0 + (1 - w_j)v_j,$$

for $j = D, S$, where:

$$w_j = \frac{\sigma_j^2}{\sigma_j^2 + \Sigma}. \tag{A.1}$$

Now, following the steps in Kyle (1985), conjecture that the pricing rule takes the following linear form:

$$P(y, v_S) = \mu + \gamma v_s + \lambda y, \tag{A.2}$$

where μ, γ and λ are constants. The liquidity demander's problem is then as follows:

$$\max_x \mathbb{E}\Big[(v - \mu - \gamma v_s - \lambda y)x|v_D\Big]. \tag{A.3}$$

Taking the first-order condition, noting that $\mathbb{E}(v_s|v_D) = \mathbb{E}(v|v_D)$, and rearranging gives:

$$x = \alpha + \beta v_D, \tag{A.4}$$

where:

$$\alpha = \frac{w_D p_0(1-\gamma) - \mu}{2\lambda}, \tag{A.5}$$

$$\beta = \frac{(1-w_D)(1-\gamma)}{2\lambda}. \tag{A.6}$$

We now turn to the liquidity supplier's problem. Following the logic in Kyle (1985), the pricing function parameters solve:

$$\min_{\mu,\gamma,\lambda} \mathbb{E}\Big[\Big(v - P(y, v_S)\Big)^2 |v_S\Big].$$

We can rewrite this as:

$$\min_{\mu,\gamma,\lambda} [(\gamma v_s + \mu + \lambda\alpha)^2 + (1-\lambda\beta)^2 \mathbb{E}(v^2|v_S) + \lambda^2\sigma_u^2 + \lambda^2\beta^2\sigma_D^2 - 2(\gamma v_s + \mu + \lambda\alpha)(1-\lambda\beta)\mathbb{E}(v|v_S)]. \tag{A.7}$$

Taking the first-order condition relative to μ gives:

$$\gamma v_s + \mu + \lambda\alpha = (1-\lambda\beta)\mathbb{E}(v|v_S). \tag{A.8}$$

We can plug in the formulas for α and β given in Equation A.5 and Equation A.6 and rearrange to obtain:

$$\gamma v_S + \frac{\mu}{2} = \Big(1 - \frac{(1-w_D)(1-\gamma)}{2}\Big)w_S p_0 - \frac{w_D p_0(1-\gamma)}{2} + \Big(1 - \frac{(1-w_D)(1-\gamma)}{2}\Big)(1-w_S)v_S.$$

This must hold for all values of v_S. Matching coefficients, this implies the following must hold:

$$\frac{\mu}{2} = \Big(1 - \frac{(1-w_D)(1-\gamma)}{2}\Big) w_S p_0 - \frac{w_D p_0 (1-\gamma)}{2}, \tag{A.9}$$

$$\gamma = \Big(1 - \frac{(1-w_D)(1-\gamma)}{2}\Big)(1 - w_S). \tag{A.10}$$

Rearranging Equation A.10 yields:

$$\gamma = \frac{(1-w_S)(1+w_D)}{2 - (1-w_S)(1-w_D)}. \tag{A.11}$$

Substituting in the expression for w_D and w_S gives the expression in Equation 7. For clarity, we also rearrange to note:

$$1 - \gamma = \frac{2w_S}{2 - (1-w_S)(1-w_D)}. \tag{A.12}$$

Taking the first-order condition of Equation A.7 relative to λ and rearranging gives:

$$\alpha(\gamma v_s + \mu + \lambda\alpha) - \beta(1-\lambda\beta)(E)(v^2|v_S) + \lambda\sigma_u^2 + \lambda\beta^2\sigma_D^2 - \Big(\alpha(1-\lambda\beta) - \beta(\gamma v_s + \mu + \lambda\alpha)\Big)\mathbb{E}(v|v_S).$$

We can use Equation A.8 and the fact that $\mathbb{E}(v^2|v_S) = Var(v|v_S) + \mathbb{E}(v|v_S)^2$ to obtain:

$$\lambda = \frac{\beta Var(v|v_S)}{\sigma_u^2 + \beta^2\sigma_D^2 + \beta^2 Var(v|v_S)}.$$

Plugging Equation A.5 and Equation A.6, noting that $Var(v|v_S) = w_S\Sigma$ and rearranging yields:

$$4\lambda^2\sigma_u^2 = 2(1-w_D)(1-\gamma)w_S\Sigma - (1-w_D)^2(1-\gamma)^2\Big(\sigma_D^2 + w_S\Sigma\Big).$$

Multiplying and dividing the first term on the right-hand side by $2 - (1-w_S)(1-w_D)$, using Equation A.12 and rearranging yields:

$$4\lambda^2\sigma_u^2 = (1-\gamma)^2(1-w_D)\Bigg(\Sigma\Big(2 - (1-w_S)(1-w_D)\Big) - (1-w_D)(\sigma_D^2 + w_S)\Bigg).$$

Rearranging and plugging in for w_D yields:

$$4\lambda^2\sigma_u^2 = \frac{\Sigma^2(1-\gamma)^2}{\sigma_D^2 + \Sigma}$$

Rearranging and taking the square root gives the expression for λ in Section 6.2.

Finally, we need to solve for μ. Plugging Equation A.10 into Equation A.9 yields:

$$\frac{\mu}{2} = \frac{\gamma w_S p_0}{1 - w_S} - \frac{w_D p_0(1-\gamma)}{2}.$$

Using our expression for γ in Equation A.11 and Equation A.12 it is straightforward to simplify this to:

$$\mu = p_0(1-\gamma).$$

Finally, plugging in Equation A.4 the formulae for γ and λ into α and β and using Equation A.1 gives:

$$x = \frac{\sigma_u}{\sqrt{\sigma_D^2 + \Sigma}}(v_D - p_0).$$

Appendix A.2. Comparative Statics

For convenience, we recall the expression for price impact λ and express the weight in the pricing function on the prior value $1-\gamma$ as a function of fundamental variances:

$$1 - \gamma = \frac{2\sigma_S^2(\sigma_D^2 + \Sigma)}{2(\sigma_D^2 + \Sigma)(\sigma_S^2 + \Sigma) - \Sigma^2},$$
$$\lambda = \frac{1}{2\sigma_u}\frac{(1-\gamma)\Sigma}{\sqrt{\sigma_D^2 + \Sigma}}.$$

Our first comparative static relates to the effect of noise trading on price impact. It is immediate from the expression for λ that when the variance of noise trading σ_u^2 increases the price impact decreases.

Note that we can rewrite $1-\gamma$ as follows:

$$1 - \gamma = \frac{2\sigma_S^2\sigma_D^2 + 2\Sigma\sigma_S^2}{2(\sigma_S^2 + \Sigma)\sigma_D^2 + 2\Sigma\sigma_S^2 + \Sigma^2}.$$

This expression is decreasing in σ_D^2. This, combined with the fact that σ_D^2 enters the expression for λ in the denominator, establishes our second comparative static: as the precision of the demander's signal increases (or its variance decreases), price impact increases.

Finally, we can also rewrite $1-\gamma$ as follows:

$$1 - \gamma = \frac{2\sigma_S^2(\sigma_D^2 + \Sigma)}{2\sigma_S^2(\sigma_D^2 + \Sigma) + 2\Sigma\sigma_D^2 + \Sigma^2}$$

This is increasing in σ_S^2. Given σ_S^2 only affects price impact via the term γ, this establishes our third comparative static: as the precision of the supplier's signal increases (or its variance decreases), price impact decreases.

Appendix B. Details on construction of dataset

In this section we set out the key steps we take to process and clean our bond transaction data. In particular, we describe (a) how we deal with duplicate transaction reports and implausible transaction reports and (b) how we deal with trades undertaken on an agency basis.

Appendix B.1. Duplicate transactions and data cleaning

As in other transaction datasets (Dick-Nielsen and Poulsen, 2019) the same trade can be reported multiple times by different participants in the trade. Left unaddressed, this would lead to double counting. To account for this, we identify duplicates as any reports where we observe two instances of the same two firms trading the same quantity of the same bond at the same point in time, with different firms reporting the trade. We keep only a single instance of these trades.[18]

Finally, we winsorize trading quantities at the [0.1%, 99.9%] levels, and remove any trade reports where the transaction price (as a % of par) is listed as under 20 or over 250.

Appendix B.2. Agency trading

The simplest type of transaction is when one trader sells an asset outright to another trader. There are, however, other types of trades, where dealers act as middlemen for their clients within a single trade. For example, a dealer might buy a trade on behalf of their client. Alternatively, a dealer may organise two exactly offsetting trades, whereby they buy an asset from one client and at the same instant sell the asset to another client.[19] In either case, the dealer has played a role in the transaction but is neither a net seller nor a net buyer.

Given our main analysis is based on *net* selling and buying, these agency-based trades will drop out of most of our analysis. Trades in which dealers act as middlemen do not impact net selling,[20] which is the explanatory variable in our two-stage least squares regression. Nor do they show up as net sales in our selling pressure measure: these trades drop out of the numerator in Equation 1.

Appendix C. Additional results

Appendix C.1. Supporting material

In this section we include analysis supporting our main findings on price impacts by sector, given in Section 5.2. Table A1 shows the full results of the regressions underlying Figure 3, which are two-stage least squares regressions of prices on sector-level sales and controls, where we instrument for sales using outside selling pressure. The most impactful sales are by dealers, followed by hedge funds, followed by banks.

Table A2 summarises the results of the first stages of these regressions. The first stages for dealers and hedge funds are particularly strong, and are moderately strong for banks and funds. The first stage for other traders is weak.

[18] For further details on cleaning this transaction dataset see Jurkatis (2024).

[19] See https://www.esma.europa.eu/sites/default/files/library/2016-1452_guidelines_mifid_ii_transaction_reporting.pdf for details.

[20] Given we aggregate trades to a weekly frequency, this is also true of cases where dealers don't offset trades instantly but do offset them within a short period of time (Choi et al., 2024).

Table A3 shows the results of reduced-form regressions of price directly on sector-level outside selling pressure. These reduced-form results can be rationalised with the two-stage least squares results as follows: dealer selling pressure is associated with around a 25 times greater price effect than fund pressure (Table A3). Dealer selling pressure is also associated with greater asset sales than fund pressure, though this difference is smaller than the price effects (Table A2). Together these are consistent with the greater price impact per unit sold for dealers than funds found using our two-stage least squares regressions (Table A1).

Appendix C.2. Robustness

In this section we demonstrate the robustness of our key finding that the price impact of sales differs according to the seller. To do so, we change an aspect of the specification in Equation 2, run this amended regression, and display the results in Figure A1. To facilitate comparison across different regressions, we normalise the coefficient on dealers' sales to 1.

Our main regressions include issuer-week fixed effects following Choi et al. (2020). In Table 5 we showed that the price impact of aggregate selling pressure is relatively similar for less demanding fixed effects. In Figure A1 we show that our results on price impacts across sectors are also robust to the choice effects. To do so, we re-run the regression with sector-week fixed effects, and plot the relative price impacts across sectors in grey in Figure A1. The results are very similar to our baseline results. Dealers continue to have the largest fixed effects. Our point estimate of the price impact of hedge fund sales is now marginally higher than that for banks, though the latter is estimated imprecisely. Other sectors continue to have minor price impacts.

We define our selling pressure measure in Equation 1. This measure weights traders' sales by their trading of other instruments: large traders will have a big impact on this measure. Here we define an alternative measure:

$$z_{i,t,\mathcal{J}}^{Alt} = \frac{\sum\limits_{j\in\mathcal{J}} \mathbf{1}(s_{i,j,t} > 0) z_{i,j,t}^{NS} / z_{i,j,t}^{T}}{\sum\limits_{j\in\mathcal{J}} \mathbf{1}(s_{i,j,t} > 0)} \tag{C.1}$$

Here the net sales of all traders j who sell bond i are weighted equally. Like our baseline pressure measure, this takes values between -1 and 1. We ran an alternative specification where our instrumental variable is this alternative measure of selling pressure. The relative price impacts across sectors are plotted in red in Figure A1. The results are very similar to our baseline results.

Our main regressions include bond prices in levels. We also ran the same regressions with log prices on the left-hand side. The results are shown in green in Figure A1. The results are essentially unchanged.

As a final robustness check, we assessed whether our different price impacts across sectors could be driven by a combination of (a) nonlinear impacts of sales on prices and (b) variation in average trading quantity across sectors. To do so, we include quadratic sales as an additional regressor in our sector-specific regressions, and include squared selling pressure as an additional instrument. The results are shown in Table A5. None of the coefficients on the quadratic terms are statistically significant, and dealers' price impact remains significantly higher than other classes of investor.

Appendix C.3. Mutual fund selling pressure

We use our data on mutual 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 A4 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 C.3.1. Coval and Stafford (2007) selling pressure measure

Coval and Stafford (2007) develop a selling pressure measure using data on mutual fund holdings. For a given instrument the measure is defined as the difference between total purchases of that instrument by mutual funds that experience extreme inflows and total sales of that instrument by mutual 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 mutual fund total net assets (TNA) and quarterly data on mutual fund holdings. Specifically, for a given bond i and week t we evaluate:

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

where $f_{jt} = F_{jt}/TNA_{jt-1}$ are mutual fund j's weekly flows as a percentage of lagged total net assets; weekly purchases and sales of bond i by mutual fund j are evaluated from weekly data on TNAs and quarterly portfolio weights w_{jiq_t} as follows:[21]

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

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

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 mutual funds that hold a given bond i.

When identifying mutual 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 flows 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 $Pressure_{it}^{CS}$ in the lowest decile of its distribution across bonds and time periods.

Appendix C.3.2. Wardlaw (2020) selling pressure measures

Wardlaw (2020) shows that the measure introduced by Edmans et al. (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:

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

where $OutShare_{it}$ is the amount outstanding of bond i at time t, and $shares_{ijq_t}$ are the portfolio weights

[21] Following Coval and Stafford (2007) Equation C.2 and Equation C.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.

as in the previous quarter q_t.[22]

2. The *flow-to-volume* measure

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

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

[22] 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: $w^*_{ijq_t} TNA_{jq_t} = shares_{ijq_t} PRC_{iq_t}$.

Appendix Figures & Tables

Table A1: Prices & sector-level sales: two-stage least squares results

	Price (%)				
	(1)	(2)	(3)	(4)	(5)
Dealer sales	-0.1034***				
	(0.0071)				
Bank sales		-0.0305			
		(0.0198)			
Fund sales			-0.0111**		
			(0.0055)		
Hedge fund sales				-0.0456***	
				(0.0106)	
Other sales					-0.0029
					(0.0041)
R^2	0.68722	0.88544	0.89221	0.89152	0.89323
Observations	1,591,470	1,591,470	1,591,470	1,591,470	1,591,470
Issuer-Week fixed effects	Yes	Yes	Yes	Yes	Yes
Instrument fixed effects	Yes	Yes	Yes	Yes	Yes

Note: Table shows the results of two-stage least squares regressions of prices on sales by different investors and controls. The coefficient estimates show the estimated coefficient β in Equation 2, and can be interpreted as the marginal effect of increasing the sales of asset i by a given sector on the asset's price. Prices are expressed as a percentage of par. Sales are net sales by investors of a given type as a percentage of average trading volume in the instrument. Time since issuance is included as an additional control. Standard errors are clustered at the issuer-time and instrument levels.

Table A2: First stage summary: sales & pressure

Sector	Coeff ($z_{i,t}$)	t-stat	R-squared	F-stat
Dealer	22.7	13.9	0.25	606.4
Hedge fund	6.6	15.1	0.27	2,266.6
Bank	1.9	4.7	0.28	62.5
Fund	6.2	14.0	0.29	740.0
Other	8.0	17.3	0.28	1,643.1

Notes: Table summarises the first stage of the two-stage least squares regressions in Equation 2. The second column shows the estimated coefficient on the pressure in a regression of sector-level sales on sector-level pressure and fixed effects, where the sector is given by the row. The third shows the t-statistic. The fourth shows the R-squared on this regression, and the final column reports the F-statistics for these first-stage regressions. Standard errors are clustered at the issuer-time and instrument levels.

Table A3: Prices and selling pressure

	Price (%)					
	(1)	(2)	(3)	(4)	(5)	(6)
Dealer pressure	-2.147***					-2.147***
	(0.1110)					(0.1110)
Bank pressure		-0.0615*				-0.0579
		(0.0355)				(0.0355)
Fund pressure			-0.0889***			-0.0861**
			(0.0337)			(0.0337)
Hedge fund pressure				-0.3509***		-0.3496***
				(0.0677)		(0.0677)
Other pressure					-0.0647**	-0.0625*
					(0.0322)	(0.0322)
R^2	0.88798	0.88791	0.88791	0.88791	0.88791	0.88798
Observations	1,864,873	1,864,873	1,864,873	1,864,873	1,864,873	1,864,873
Issuer-Week fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Instrument fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table summarises the results of reduced-form regressions of prices on sector-level pressure and controls. The coefficient estimates show the estimated coefficients $\delta_{\mathcal{J}}$ shown in Equation 3, and can be interpreted as the marginal effect of increasing the selling pressure in asset i for a given sector on the asset's price. Prices are expressed as a percentage of par. Time since issuance is included as an additional control. Standard errors are clustered at the issuer-time and instrument levels.

Table A4: 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.

Table A5: Evidence of nonlinearities

	Price (%)				
	(1)	(2)	(3)	(4)	(5)
Dealer sales	-0.1260***				
	(0.0453)				
Dealer sales^2	0.0004				
	(0.0005)				
Bank sales		-0.0597			
		(0.1767)			
Bank sales^2		0.0004			
		(0.0009)			
Fund sales			-0.0019		
			(0.0090)		
Fund sales^2			-5.08×10^{-5}		
			(7.65×10^{-5})		
Hedge fund sales				-0.0752	
				(0.0521)	
Hedge fund sales^2				0.0005	
				(0.0005)	
Other sales					-0.0089
					(0.0781)
Other sales^2					3.24×10^{-5}
					(0.0004)
R^2	-0.92071	-0.26533	0.85459	0.74671	0.88652
Observations	1,591,470	1,591,470	1,591,470	1,591,470	1,591,470
Issuer-Week fixed effects	Yes	Yes	Yes	Yes	Yes
Instrument fixed effects	Yes	Yes	Yes	Yes	Yes

Notes: Table shows the results of adding quadratic terms to our two-stage least squares regressions by sector. The regression setting is the same as in Table A1, except for (a) the addition of squared sales as a regressor in each regression and (b) the addition of squared pressor as an additional instrument in each regression.

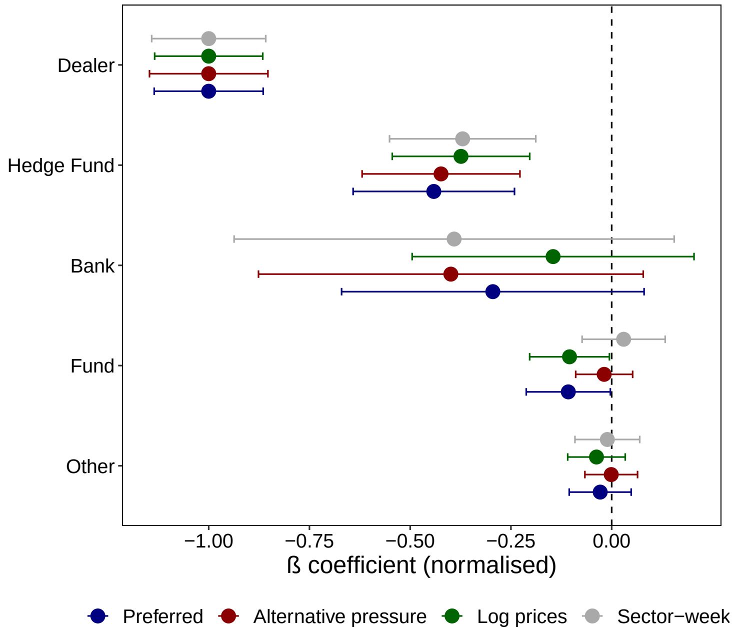

Figure A1: Robustness: Price impact by sector

Note: Figure shows the results of our various robustness checks relative to our baseline results. The blue dots show the estimated coefficient β in Equation 2, normalised by the estimated coefficient for dealers, with the error bars showing 95% confidence intervals. The other colours repeat the exercise for the various robustness checks described in Appendix C.2. Controls include issuer-time and instrument fixed effects as well as the time since a bond was issued. Standard errors are clustered at the issuer-time and instrument levels.