Newspaper Closures and Trading in Local Stocks

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ABSTRACT

There is increasing awareness of how local media affects financial markets, but also of the endogeneity of media coverage. We separate the causal impact of local media on financial markets from selection effects using a new, hand-collected database of newspaper closures. We find that at least 29% of local newspaper closures are driven by distress, and thus, likely endogenous to local economic conditions. Return volatility and idiosyncratic risk decrease significantly after non-distress-driven newspaper closures, but increase after distress-driven closures, suggesting the presence of substantial selection effects. We find similar patterns for liquidity and trading. Once we account for selection, the estimated impact of local newspapers on volatility increases by over 40%. The reduction in volatility after non-distress-driven newspaper closures is larger for stocks subject to greater information frictions, lower national media coverage, firms located in remote areas, and during recessions. All these tests suggest that investor information processing is the main channel driving our results. Our findings highlight that the effect of media on financial markets may be larger than previously documented.

JEL Classification: G14, G33, L82

Keywords: media coverage, information processing, financial markets, return volatility, trading

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Local newspapers have been vanishing at an accelerating pace in the United States. In 2022, one in five Americans live in a "news desert", a county with one or no local newspaper. Local newspaper circulation fell 40% from 2015 to 2020, while the number of newsroom employees dropped by 51% between 2008 and 2019, followed by an even larger decline due to the economic impact of COVID-19 (Pew Research Center, 2022). Local newspapers often close due to tough economic conditions (Abernathy, 2022) and re-establishing local media outlets is more difficult in economically disadvantaged areas (Malone, 2022).

Research on geography and finance has shown that investors overweight local stocks in their portfolios (Coval and Moskowitz, 1999, Ivković and Weisbenner, 2005), and pay more attention to local stocks (Cziraki, Mondria, and Wu, 2021). Consequently, local investors may rely heavily on local media sources for their investment decisions, as these sources are more likely to contain high-quality investigative journalism (Abernathy, 2022).

Connecting the above insights, this paper studies the effect of local media closures on financial markets—a treatment effect—, taking into account that such closures are often driven by distress—a selection effect. We collect detailed data on newspaper closures and conduct a narrative classification of the reason for each closure, allowing us to make progress on separating these two effects.

First, we assemble a new sample of newspaper closures for the period 2001-2021. The U.S. News Deserts Project reports that newspapers were closed at an increasing rate during this period. A key advantage of our new dataset is that we register closure dates at monthly frequencies, unlike the vast and widely-used database assembled by Gentzkow, Shapiro, and Sinkinson (2011)—as their source, the Editor and Publisher Yearbook, is published every four years.

Second, we study the *circumstances* of newspaper closures and use a narrative analysis

to identify those that are more likely distress-driven (a closure due to a drop in circulation or declining revenues) from those that occur for other reasons (the death of the founder or the consolidation of newspapers within the same media group). Examining the reasons for newspaper closures helps with the identification of causal effects. For example, local economic decline and cultural factors such as corruption (see, e.g. Parsons, Sulaeman, and Titman, 2018b,a) may lead both to newspaper closures and to various firm-level outcomes such as fraud or investor demand.

We find that at least 29% of newspaper closures are due to financial or economic distress. For brevity, we refer to such closures as 'distress-driven' throughout the paper. We classify a closure as distress-driven if financial or economic distress is mentioned as a reason for the newspaper's closure in any of our data sources. There are two benefits to this approach. First, its simplicity guarantees transparency and ease of replication—including extension to other settings. Second, it is conservative and will underestimate the number of newspapers closed for reasons endogenous to local conditions. The remaining 71% of observations will thus consist of newspaper closures that are truly not distress-driven, as well as closures that are distress-driven but were not reported as such. For brevity, we refer to such events as 'non-distress-driven' closures. As a validity check, we show that local local stock returns decrease significantly in the year before distress-driven newspaper closures, but not before non-distress-driven closures.

We next show that separating distress-driven and non-distress-driven newspaper closures leads to markedly different effects for proxies of individual investor trading and information processing. Return volatility moves in the opposite direction after distress-driven vs. non-distress-driven newspaper closures. Using a stacked differences-in-differences model, we show that stock return volatility and idiosyncratic risk decrease significantly after non-distress-

driven closures but increase significantly after distress-driven closures of newspapers in the same ZIP code as the firm's headquarters. These differences are economically significant. Idiosyncratic volatility declines by 1.3 percentage points, or 4% of one standard deviation, after non-distress-driven closures. However, it *increases* by 1.1 percentage points, or 3% of one standard deviation, after distress-driven closures.

Our results are robust to saturating the model with more granular, state × year, fixed effects, or to using a narrower time period around newspaper closures. Further, firms affected by distress-driven and non-distress-driven closures differ not only with respect to pre-closure stock returns, but also with respect to observable firm characteristics. To account for this, we separately select control firms for each of these two sets. When using matched control firms, we confirm the decrease in return volatility and idiosyncratic risk for non-distress closures at comparable economic magnitudes relative to the results without matching.

Looking at other proxies of investor trading, we find that non-distress-driven closures are followed by statistically significant reductions in trading volume and liquidity. Share turnover declines by 6.8 percent, Amihud (2002) illiquidity increases by 0.45, which represents 5% of one standard deviation, and the proportion of zero-return days (Lesmond, Ogden, and Trzcinka (1999)) increases by one percentage point, or 3% of one standard deviation, suggesting that investors trade less following non-distress-driven newspaper closures. None of these changes are present for distress-driven newspaper closures, and, similarly to the volatility results, the point estimate has the opposite sign for these outcomes.

Further, we document that the decrease in information processing is accompanied by decreases in other measures of investor attention. The number of tweets, and EDGAR visits about the firm decrease significantly after non-distress-driven closures, and with a much smaller economic magnitude after distress-driven closures. Last, a composite measure of in-

vestor sentiment from three major social media platforms, Twitter, StockTwits, and Seeking Alpha (Cookson, Lu, Mullins, and Niessner, 2024) increases after distress-driven newspaper closures and decreases (though not significantly) after non-distress-driven closures. Hence, when a local newspaper shuts down, it is not obvious that other information outlets take its place. These differences further support our claim that it is important to separate distress-driven newspaper closures from other closures.

If the above changes reflect the reduction in local investors' access to information, they should be larger in settings where information about local firms is more valuable. Consistent with this prediction, we find that the reduction in volatility following non-distress-driven newspaper closures is greater for stocks that are subject to larger information frictions. The drop in volatility doubles for small firms and growth firms, triples for stocks of firms located in sparsely-populated areas of the U.S., and more than doubles during recessions. These findings support our interpretation that newspapers contain relevant information about local firms, and that the loss of this information matters for local investors.

Taken together, these results allow us to separate the causal effect of local newspaper closures from the selection effect of which local newspapers are closed. The selection effect is positive, consistent with the intuition of Garcia (2013) and Roth, Settele, and Wohlfart (2022): investors in areas with economic distress have greater incentives to process information about local firms because of personal exposure and uncertainty. However, accounting for this selection effect, the treatment effect of newspaper closures on volatility is negative and significant. We estimate the selection effect for distress-driven closures at 2.4 percentage points in terms of idiosyncratic volatility, which corresponds to 7% of one standard deviation. As an alternative way to show the value of our narrative classification, we pool distress-driven and non-distress-driven newspaper closures, as if we (the researcher) had no further informa-

tion on these events, and re-estimate the same differences-in-differences model. Under this hypothetical, we find a 41-43% attenuation of the estimated treatment effect, suggesting that the selection effect is quantitatively important.[1]

Our work contributes to three strands of the academic literature. First, our results advance the literature on the role of the media in financial markets (Fang and Peress, 2009, Dougal, Engelberg, Garcia, and Parsons, 2012), focusing on local newspapers. Gao, Lee, and Murphy (2020) use the original Gentzkow et al. (2011) data to study municipal bond yields and Heese, Pérez-Cavazos, and Peter (2021) use an additional 33 newspaper closures to examine corporate fraud. We hand-collect all newspaper closures for 2001-2021 and classify them based on the reason for the closure. Our more recent and detailed data allows for a nuanced analysis of the underlying mechanisms.[2] We document that many local newspaper closures—almost one in three—are distress-driven, and, consequently, endogenous to economic conditions. Building on this insight, we separate selection and treatment effects and study how closures affect trading in local stocks.

Second, we contribute to the literature on the endogeneity of media coverage (Gurun and Butler, 2012, Garcıa, 2012, Goldman, Martel, and Schneemeier, 2021). Recognizing this endogeneity, previous studies have used a variety of empirical approaches to infer the causal effect of the media on financial outcomes (Engelberg and Parsons, 2011, Peress, 2014). We provide evidence on the reasons for newspaper closures, and use this variation to gauge the importance of the selection effect of endogenous media coverage. By doing so, we address

[1] As section 3 explains, this estimate is a lower bound on the selection effect assuming perfectly accurate classification of distress-driven and non-distress-driven newspaper closures. If we mistakenly classified some newspaper closures as non-distress-driven, even though in reality they were distress driven, our estimate would understate the true selection effect.

[2] A further benefit of studying stocks is that individual investors are substantially more likely to own stocks than municipal bonds: 61% of Americans report owning stocks (Gallup, 2023), but only 2.4% own government bonds (Olson and Wessel, 2016, Bagley, Vieira, and Hamlin, 2022).

the point raised in Tetlock (2014) that large-scale empirical studies of the media on financial markets report much smaller effects compared to anecdotal studies focusing on the publication of single articles.[3] Our findings on the attenuating effect of selection can help reconcile these two facts.

Finally, our work also adds to the literature on the informational advantage of local investors (Coval and Moskowitz, 2001, Chhaochharia, Kumar, and Niessen-Ruenzi, 2012) and their propensity to pay more attention to local information (Cziraki et al., 2021, Mengoli, Pagano, and Pattitoni, 2024). We contribute to this literature by characterizing the importance of local newspapers in local investors' information processing.[4]

1 Background and predictions

We summarize predictions for our empirical analysis based on previous theoretical and empirical work.[5]

The first set of predictions concerns local investor information processing as information supplied by the media changes—in our setting: a local newspaper closes. Theoretical work predicts that a higher information content of news increases trading activity (Tetlock, 2010), and that media coverage increases stock price informativeness (Goldman et al., 2021). Mondria (2010) and Van Nieuwerburgh and Veldkamp (2009, 2010), show that local investors' information processing and trading decisions endogenously amplify each other. Based on

[3] "First, anecdotal studies suggest the impact of media on asset prices could be enormous, with the publication of single articles causing prices to rise or fall by factors of three to six. But the large-scale evidence from studies using instruments for exogenous changes in media reporting reveals impacts that are smaller by an order of magnitude. One can reconcile these facts by arguing either the anecdotes are unusual or the instruments are weak." (Tetlock, 2014, p.380.)

[4] More broadly, our results also relate to the literature on the reasons for investor disagreement (Cookson and Niessner, 2020, Huang, Hwang, Lou, and Yin, 2020), highlighting the role of differential exposure to local media sources.

[5] See Tetlock (2014) for a review.

this work, we expect a negative treatment effect: local newspaper closures should reduce information processing, and thus trading, by local investors.

The second set of predictions is aimed at understanding a potential selection effect: Newspaper closures can be endogenous to local economic conditions, which in turn also directly influence incentives to process information. Put differently, how does investor information processing change in areas where local newspapers tend to close?

In our setting, we ask whether investors tend to process more or less information about firms in distress, or in areas with a decaying local economy. Allocating different levels of attention to thriving vs. struggling firms is consistent with the model of Peng and Xiong (2006), which shows that limited investor attention leads to category learning. Empirical results on investor processing in good and bad times, or about good and bad firms, are mixed. On the one hand, using time-series variation in the state of the overall economy, Garcia (2013) finds that financial newspaper articles predict stock returns only during recessions. The incentives to process information increase with personal exposure and uncertainty (Roth et al., 2022), which is particularly high during bad times. On the other hand, there is theoretical and empirical evidence that demand for information is greater during booms. Van Nieuwerburgh and Veldkamp (2006) develop a model of asymmetric learning, where greater levels of production lead to more precise information during booms. Studies focusing on firm performance show that individual investors process more information before buying a stock than before making sell decisions (Barber and Odean, 2008, Hartzmark, 2015).[6] Kacperczyk, Van Nieuwerburgh, and Veldkamp (2016) study an attention allocation model in which information production also varies with the business cycle: investors process information about the aggregate shocks in recessions, while processing information about idiosyncratic

[6]In contrast to these findings on individual investors Bowles and Reed (2023) find that *mutual funds* process more information about firms whose stocks they short than firms whose stocks they are long.

shocks in booms, leading to price informativeness varying procyclically as well. In sum, existing theoretical and empirical results yield ambiguous predictions about the sign of the selection effect in our setting.

2 Data on newspaper closures and affected firms

2.1 Data sources on newspaper closures and firm financials

We hand-collect information on newspaper closures identifying the month of each closure. Doing so allows us to link the data to financial market outcomes more precisely relative to the database of Gentzkow et al. who rely on a yearbook published every four years. Our sample covers closures between 2001 and 2021, extending Gentzkow et al.'s database by 16 years. We start with the list of discontinued newspapers published in English in the "Chronicling America" project, which is part of the National Digital Newspaper Program, while we restrict attention to the years 2001 to 2021. We complement this list with the U.S. News Desert Project database[7], which yields 637 potential closures since 2001. For each of these newspapers, we conduct additional manual searches using Factiva, WorldCat.org, the respective newspapers themselves, regional newspaper archives and libraries, local websites, and blogs (accessed through general online search engines), to validate that the newspaper has discontinued, to identify the precise date of the last publication, and to collect additional information about the reasons and circumstances behind the closure. We restrict attention to validated newspapers for which we can identify the closure date and which can be merged to ZIP codes with publicly-traded firms, resulting in a sample of 263 newspaper closures.

We consider a firm to be affected by the newspaper closure if it is headquartered in the

[7]We thank Penelope Muse Abernathy for kindly sharing the U.S. News Deserts database with us.

same ZIP code that the newspaper operated in.[8] We merge these data with information on stock prices from CRSP and accounting data from Compustat. We drop a newspaper closure if there are no publicly-traded firms with Compustat and CRSP data in the ZIP code. Panel A of Table 1 shows the number of newspapers closed each year for which there is at least one affected firm. Newspapers close more frequently starting in 2006. The year 2010 shows the highest number of closures.[9]

Table 1 Panel A also shows the number of firms affected by newspaper closures over time. Overall, 3,717 firms were affected by a closure over the sample period: 1,096 by distress-driven closures and 2,621 by non-distress-driven closures. The year 2010 has the largest number of affected firms with 513, representing approximately 14% of all affected firms.

Panel B of Table 1 shows descriptive statistics of firm and stock characteristics of affected firms and never-affected firms at time $t-1$ prior to the closure event at time t. Affected firms tend to be substantially larger both in terms of total assets (USD 5.1 billion versus USD 2.8 billion) and market capitalization (USD 3.7 billion versus USD 1.9 billion). Tobin's Q of affected firms with an average of 2.0 is also above the average of never-affected firms with 1.7. Affected firms tend to have slightly higher sales growth and report higher R&D expenditures. Despite these differences in firm characteristics, pricing outcomes seem to be rather comparable, as total return volatilities, systematic risk and idiosyncratic risk are quite similar across both groups.[10]

[8]This assumption likely leads us to underestimate the effect of newspaper closures, as many local newspapers circulate in more than one ZIP code and likely also cover local information in surrounding regions.

[9]While newspapers were closed also in 2002, these did not affect any firm by our definition of operating in the same ZIP code.

[10]The level of idiosyncratic risk in our sample is comparable to typical levels found in the literature. Eg.g., Ferreira and Laux (2007) document idiosyncratic volatility (the variance) of 19.4% for the period between 1990 and 2001, which is similar to our squared standard deviation of residuals of 21.0% for affected firms and 23.6% for never-affected firms.

2.2 Newspaper closure motives

2.2.1 Narrative classification of closure motives

To better understand the dynamics of local newspaper closures, we hand-collect information on the reason for the newspaper closure and use a narrative classification method to separate distress-driven and non-distress-driven closures. Ramey (2016) defines a narrative method as "constructing a series from historical documents to identify the reason and/or the quantities associated with a particular change in a variable". Starting with the seminal work of Friedman and Schwartz (1963) such methods have been used to identify various policy shocks—e.g., Poterba (1986), Romer and Romer (1989, 2004, 2010), Ramey and Shapiro (1998), Ramey (2011).

76 closures (29%) are distress-driven. 28 of those discontinued newspapers are the result of a takeover by another newspaper or media holding, while 48 are closures that are not associated with a takeover. We classify newspaper closures as distress-driven if financial distress, or M&A related to distress is one of the stated reasons for the closure. Gilson, Hotchkiss, and Osborn (2016) show that distressed firms have been increasingly relying on M&As as opposed to stand-alone reorganization within the Chapter 11 resolution process during the early part of our sample, 2002-2011.

We classify all other newspaper closures as 'non-distress-driven'. The most common reasons within the set of non-distress-driven closures are consolidation of different products within one media group (56) and mergers that are seemingly not driven by financial distress (49). Personal reasons include the illness or death of (one of) the owner(s), or relocation because of family circumstances. Our classification shows that newspaper closures are often related to distress. Hence, our narrative classification of the reason for closures will help disentangle various mechanisms for any potential correlation between closures and stock

market outcomes.

2.2.2 Validating our classification

To validate our classification of local newspaper closures, we investigate pre-event trends prior to distress and non-distress-driven closures. For each ZIP code, we construct an index of local stock returns, using firms that are located within a 250-mile radius of, following Seasholes and Zhu (2010). We run a stacked difference-in-differences analysis, where we regress ZIP-level stock returns on event-time dummies around the closure year in t, while we distinguish between distress and non-distress-driven closure events. Panel A of Figure 2 shows that local stock returns decline prior to a distress-driven newspaper closure. The event-time coefficients are slightly positive in $t-5$, decrease slightly over the subsequent years, and turn negative in $t-1$. In contrast, local returns prior to non-distress-driven newspaper closures fluctuate rather closely around zero and fail to be statistically significant at conventional levels.

There is substantial heterogeneity in terms of the number of publicly-traded firms located within a 250-mile radius around a newspaper closure, reflecting the diversity in economic density across the country. The number of firms ranges from 1 to 2,117 firms, with a mean of 623 and a median of 417. The differences in the number of firms introduce heterogeneity in the precision at which local returns are measured. In a robustness test we run the same analysis as before, while we restrict attention to ZIP codes for which we observe stock returns of at least 20 firms within a 250-mile radius. The results are shown in Panel B of Figure 2 and confirm the results before: prior to distress-driven closures, local stock returns decline, while there is no such pattern for non-distress-driven closures.

This analysis provides empirical support for the notion that distress-driven closures are

correlated with regional economic trends, in contrast to non-distress-driven ones.

3 Empirical approach

How does the closure of a local newspaper affect return volatility, understanding that such closures are often economically motivated? To answer this question, we estimate a stacked difference-in-difference model with return volatility as the dependent variable, separating distress-driven from non-distress-driven newspaper closures. A two-way fixed effects (TWFE) study, just controlling for unit-fixed and time-fixed effects, is not appropriate in our setting, as the treatment is staggered and the effects are likely to be heterogeneous both across time and also across units, which can incur the problem of the TWFE estimate having the opposite sign of the individual unit-level treatment effects because of the potential of negative weights. We use the approach of stacked difference-in-differences regressions, controlling for separate time and unit fixed effects for each event stack, as in Cengiz, Dube, Lindner, and Zipperer (2019).

To that end, we build stacks consisting of firms affected by newspaper closures in the same ZIP code and those never affected by any such closure for each of the 20 event-years during our sample period. Each stack consists of firm-year observations during the event window spanning six years before to six years around the respective event year of both firms affected by a closure and the never-affected firms. Effectively, we study 20 individual $2x2$ two-way fixed effects models.

We estimate the following equation:

$$Y_{sit} = \beta_1 post_{sit}^{DD} + \beta_2 post_{sit}^{NDD} + \gamma X_{sit} + \nu_{si} + \mu_{st} + \varepsilon_{sit} \tag{1}$$

where i denotes the firm, t denotes the year, s denotes the stack defined by the event year, y is a risk measure, $post^{DD}$ is a dummy variable set to 1 for firms affected by a distress-driven closure in the same ZIP code after the closure, and to zero otherwise, $post^{NDD}$ is a dummy variable set to 1 for firms affected by a non-distress-driven closure in the same ZIP code after the closure and to zero otherwise, ν_i are stack-specific firm fixed effects, and μ_{st} are stack-specific year fixed effects.

In the ideal case, our classification method would be perfect: all distress-driven closures would be truly distress-driven and all non-distress-driven closures would be truly not driven by distress. In this ideal case, we expect that the estimated coefficient for distress-driven closures will reflect both the selection effect (that local newspapers in certain areas and times are more likely to close) and the treatment effect (that a newspaper closure changes stock price risk). In contrast, the estimated coefficient for non-distress-driven closures will reflect only the treatment effect. To the extent that our classification method is imperfect, we expect that *some* closures that we classify as 'non-distress-driven' are, at least in part, distress-driven. This could occur, for example, if the original sources outright missed a distress motive, or because the true 'proximate' reason for the closure was an indirect consequence of distress occurring much earlier. In this case, even the coefficient estimate for non-distress-driven closures will reflect a selection effect, but a smaller one than the coefficient estimate for distress-driven closures. It follows that we can approximate the selection effect as the difference between the coefficient estimate for distress-driven closures less the coefficient estimate for other closures. To the extent that our classification is imprecise, we will underestimate the true selection effect.

4 Newspaper closures and trading in local stocks

4.1 Newspaper closures and volatility

We estimate a stacked differences-in-differences model to study the effect of newspaper closures on the return volatility of local stocks. We use return volatility and idiosyncratic risk as our proxies for investor trading (Foucault, Sraer, and Thesmar, 2011, Griffin, Hirschey, and Kelly, 2011). Andrei and Hasler (2015) show, both theoretically and empirically, that investor information processing increases return volatility. For completeness, we also look at systematic risk as another outcome variable. Return volatility is the annualized standard deviation of stock returns. We estimate idiosyncratic volatility as the annualized standard deviation of residuals of a regression of daily excess stock returns over one full year on the daily three Fama-French factors. We calculate systematic volatility as the square root of the difference between the variance of daily stock returns and the variance of daily residuals.

Return volatility moves in opposite directions following distress-driven and non-distress-driven local newspaper closures. Column 1 of Table 3 shows that return volatility increases following distress-driven closures, with the coefficient statistically significant at the 5% level. In contrast, volatility *decreases* significantly following non-distress-driven closures, with the coefficient significant at the 1% level. Column 2 reveals a similar pattern for idiosyncratic risk, but with an even sharper contrast between distress-driven and other closures. The increase in idiosyncratic volatility is significant at the 5% level. The changes in volatility are also economically significant. Looking at column 2, the increase after distress-driven closures is 1.4 percentage points corresponding to 5% of one standard deviation. The decrease in idiosyncratic volatility after non-distress-driven closures is 1.2 percentage points, or 4% of one standard deviation. Last, systematic risk increases slightly, though not statistically

significant after distress-driven closures, while it decreases significantly after non-distress-driven closures (column 3) with a magnitude of 4% of one standard deviation.

These results underscore the importance of accounting for the reason behind a local newspaper closure when considering its effect on financial outcomes. Relying on our narrative classification, we can infer the sign and magnitude of the selection and treatment effects of the loss of a local media outlet on firm risk. Based on our estimates, there is a negative and significant treatment effect of a local newspaper closure on firm risk: stock returns become less volatile when there is less local news coverage. Examining the difference between the coefficients for distress-driven and other closures, we can infer a positive and significant selection effect. The positive sign is consistent with the intuition of Garcia (2013) and Roth et al. (2022): investors in areas with economic distress have greater incentives to process information about local firms because of personal exposure and uncertainty. The selection effect, estimated as the difference between the coefficients for distress-driven and non-distress-driven closures is substantial. Based on the estimates in column 2, the selection effect is 2.6 percentage points, which corresponds to 8% of the standard deviation of idiosyncratic volatility. As discussed in section 3, this estimate is a lower bound on the selection effect assuming perfectly accurate classification of distress-driven and non-distress-driven newspaper closures. If we classified some distress-driven newspaper closures as non-distress-driven, this estimate would understate the true selection effect.[11]

Panel B of Table 3 shows that using all newspaper closures–without classifying them–obfuscates the relation between newspaper closures and risk. Estimating a stacked DiD

[11]Local newspapers that remain open may also cut costs, lay off journalists, and reduce the focus on local news in an effort to increase efficiency. First, if anything, this trend would lead our estimates to understate the true treatment effect of local newspaper closures, because even the control group receives less local information than before. Second, we find no evidence of different pre-trends for non-distress-driven newspaper closures in Figure 3.

method using all newspaper closures in our sample would lead us to find a smaller, and less statistically significant effect of local newspaper closures on firm risk. This model would conflate distress-driven and other closures, and hence the treatment and selection effects of local newspaper closures. The difference is quantitatively important: the coefficient estimates for total return volatility and idiosyncratic volatility equal are 41-43% smaller relative to those estimated for non-distress-driven closures in Panel A of Table 3. Hence, the selection effect leads to a 41-43% attenuation of the estimated treatment effect in the overall sample.

Last, recognizing that firms affected by a closure might differ from never-affected firms, we repeat our analysis using a matched sample of control firms. Panel A of Table 4 documents substantial differences in terms of observable characteristics between firms affected by distress-driven and non-distress-driven closures at $t-1$ before the closure. These differences imply that we require separate models of how observable firm characteristics affect the likelihood of being subject to a distress-driven vs. a non-distress-driven closure. Hence, we separately conduct Mahalanobis matching for the two subsamples, using the natural logarithm of market capitalization, Tobin's Q, sales growth, leverage, profitability, and R&D at time $t-1$ to identify a comparable firm that was not affected by any newspaper closure.

We then repeat our analysis using these matched samples as controls in Panel B of Table 4. The results remain similar for total return volatility and idiosyncratic risk: both decrease significantly after other closures but not distress-driven closures. Interestingly, in this matched sample, we find no significant change in systematic risk for either type of newspaper closure, and virtually no difference between the point estimates of the coefficients for distress-driven and non-distress-driven closures.

4.2 Newspaper closures and measures of trading activity and liquidity

As further evidence that trading in local stocks changes after local newspaper closures, Table 5 studies how measures of trading activity and liquidity change after local newspaper closures. Column 1 shows that stock turnover decreases significantly by 6.8 percent after non-distress-driven newspaper closures, but not after distress-driven closures. Column 2 documents that Amihud (2002) illiquidity increases by 0.45, or 5% of one standard deviation. Last, the results in column 3 show that the proportion of zero-return days, a proxy for days with no trading (Lesmond et al. (1999)) increases by 1 basis point, which represents 3% of one standard deviation.[12] For both of the latter measures, the point estimate of the coefficient for distress-driven closures has the opposite sign, similar to our results for return volatility. In sum, trading activity and liquidity decrease significantly after non-distress-driven closures, but not after distress-driven closures, displaying a similar pattern to return volatility.

4.3 Dynamics of volatility around distress-driven and other newspaper closures

Building on the finding that volatility moves in different directions after distress-driven and other newspaper closures, we now examine the dynamics of volatility these events in more detail. Figure 3 shows event-study graphs with idiosyncratic volatility as the dependent variable. The top panel shows the dynamics of idiosyncratic volatility around distress-driven newspaper closures and the bottom panel around other newspaper closures.

The two graphs show markedly different patterns for distress-driven vs. other closures.

[12]Placing this estimate in the context of our sample: only 5% of observations affected by a closure have any zero-return days, and the sample avearge of the variable is 5 basis points (not shown in a table).

There is a noticeable downward trend in volatility occurring before distress-driven newspaper closures. However, there is a less pronounced, and not statistically significant decrease in idiosyncratic volatility between the year before the closure ($t = -1$) and two years after it ($t = 2$). In contrast, there is no evidence of any pre-trend prior to other newspaper closures in the bottom graph in Figure 3. Idiosyncratic volatility starts to decrease in the year of the newspaper closure and, after a monotonic decline, becomes significantly lower two years after the closure relative to the year before it. This graphic comparison of stock return volatility around distress-driven vs. other closures substantiates the differences between the two events, supporting the notion that the former are likely driven by selection effects whereas the latter are not, or to a lesser degree.

4.4 Robustness

The effect of non-distress-driven newspaper closures on volatility is robust to alternative empirical specifications of the stacked differences-in-differences model. We show that the main results in Section 4.1 in are robust to allowing for time-varying regional trends. Panel A of 6 FE estimates the same model as Panel A of Table 3 but replaces year $\times$ stack fixed effects (FE) with state $\times$ year $\times$ stack FE. We continue to find that return volatility and idiosyncratic risk decrease significantly following non-distress-driven closures, while they increase significantly after distress-driven closures, with two minor differences. First, the positive coefficient for distress-driven closures increases by almost 50% relative to the main specification–e.g., from 1.4 to 2.1 for idiosyncratic volatility, with a similar increase for total return volatility. Second, the standard errors become slightly larger for both return volatility and idiosyncratic risk.

Next, we restrict the time window of the estimates to three years before to three years

after each newspaper closure to focus more closely on the potential impact of the closure. Panel B of Table 6 shows that our main results continue to hold. Moreover, both the statistical and the economic significance of the coefficient increases slightly for non-distress-driven newspaper closures. For the regression of idiosyncratic risk, the absolute value of the coefficient for non-distress-driven closures increases by 12% relative to the main model in Panel A of Table 3.

In sum, our main results remain robust–and, in some cases, become stronger–when we saturate the main model with more granular fixed effects, or use a narrower time window around newspaper closures to capture their effect on volatility.

4.5 Measuring attenuation in our framework

Our narrative classification allows us to measure the attenuation of the estimated effect of local newspaper closures on trading in local stocks. We measure attenuation as the difference between the coefficient of *Post non-distress-driven* and the coefficient of the variable *Post closure*—which pools both distress-driven and non-distress-driven newspaper closures—scaled by the coefficient of *Post non-distress-driven*. The coefficient of *Post closure* is a hypothetical estimate that an econometrician would observe if they had no further information on the reason for the local newspaper closures.

Table 7 summarizes the magnitude of this attenuation across our various specifications for idiosyncratic volatility (Panel A) and for the different outcome variables we study (Panel B). Table 7 Panel A shows that attenuation towards zero is substantial not only in the baseline model shown in Table 3, but across all alternative specifications. Our estimate of attenuation for idiosyncratic volatility ranges from 32% to 62%. Table 7 Panel B confirms that attenuation towards zero is present not only for idiosyncratic volatility, but for all of

the proxies of investor trading we use. We find an attenuation of 26% for Amihud illiquidity, 18% for the proportion of zero-return days, and 12% for stock turnover. The consistent pattern of attenuation across model specifications and outcome variables further highlights the value of our new data and method in quantifying selection effects when considering the impact of (local) media on financial markets.

5 Heterogeneity and mechanism

5.1 Heterogeneity across firms and locations

Which stocks are affected the most by local newspaper closures? If investors use local newspapers to learn about local stocks, then we expect the treatment effect of a newspaper closure on return volatility to be larger for stocks that face greater information frictions, and in areas where information processing costs are higher. Table 8 shows differences in the effect of local newspaper closures on idiosyncratic risk, splitting the sample at the median value by various proxies of information frictions. We focus our subsample analysis on non-distress-driven local newspaper closures, but control for distress-driven closures as well to maintain a specification consistent with our main analysis.

We find that the effect of non-distress-driven newspaper closures is larger for stocks that are subject to greater information frictions. Table 8 Column 1 shows that the reduction in idiosyncratic risk is larger for smaller firms. The coefficient estimate of -2.37 is statistically significant at the 1% level and almost twice as large as our estimate for the overall sample (-1.23 in Table 3 column 2). The effect of other newspaper closures is also significantly larger at firms with higher sales volatility, shown in column 2. The coefficient estimate of -2.24 is significant at the 1% level and nearly doubles the estimate for the overall sample. This result

suggests that information contained in local newspapers is particularly helpful in gauging the prospects of firms with riskier or more cyclical operations. We also see a larger reduction in idiosyncratic volatility following other newspaper closures at firms with a higher market valuation (column 3), suggesting that the information in local newspapers is particularly important for high-growth stocks.

The last two subsample splits in Table 8 capture investor familiarity with the stock. We use advertising expenses to proxy for firms selling products mostly to retail consumers vs. those selling mostly to other businesses. Column 4 shows that the effect of other newspaper closures is more pronounced for stocks of firms with greater advertising expenses. This pattern may reflect a demand effect: investors are more likely to gather and process information from the local newspaper about firms whose brands and services they are familiar with. It may also reflect a supply effect: local newspapers are more likely to contain stories about firms that retail consumers are familiar with. In both cases, the closure of a local newspaper would lead to a greater drop in the availability of information about such firms, as compared to firms whose primary customers are other businesses.

Last, we separate the sample into high and low media coverage based on the number of Dow Jones news items about the firm in the previous year. On the one hand, greater news coverage may substitute for the information contained in a local newspaper. On the other hand, investors may choose to invest less of their portfolio in firms that rarely receive any media coverage, and, as a result, may update their views about the stock very little even with the arrival of new information. Table 8 column 5 shows that idiosyncratic risk declines after other closures both at stocks with low and high previous media coverage, but the effect is more statistically and economically significant for stocks with low media coverage. This finding hints at some degree of substitution between information from local newspapers and

other news sources. We explore this hypothesis in more detail in section 6.1.

Next, we turn to differences based on geographical characteristics. We expect non-distress-driven newspaper closures to have a larger effect on idiosyncratic volatility in remote, sparsely populated areas, as firms in such areas suffer from greater information asymmetries (Coval and Moskowitz (1999, 2001), Ivković and Weisbenner (2005)). We use the same model specification as before, in Table 8 and split the sample of newspaper closures into those in vs. outside of metropolitan areas, as well as three different cutoff values for population density: 750, 1,000, and 2,000 people per square mile.[13]

Table 9 shows that volatility decreases after non-distress-driven newspaper closures are concentrated in rural and sparsely populated areas, consistent with our prediction based on information frictions. Column 1 shows that the reduction in idiosyncratic risk after other newspaper closures is highly statistically significant outside of metropolitan areas. To quantify the differences more precisely, columns 2-4 show that the reduction in volatility is largest for stocks in areas with population density lower than 750 people per square mile. The coefficient of -2.56%, the effect is more than double the effect in the overall sample. As we increase the threshold for population density (to 1,000 and 2,000 people per square mile), the coefficient decreases monotonically in columns 3-4. Across all models, the coefficient estimates for rural and sparsely populated areas are statistically significant at the 1% level. We conclude that the effect of non-distress-driven newspaper closures is higher for stocks subject to greater information frictions.

[13]The average population density inside U.S. cities is 1,600 according to the U.S. Census Bureau.

5.2 Heterogeneity over the business cycle

To further understand the mechanism driving our results, we examine whether the reduction in idiosyncratic risk following non-distress-driven newspaper closures varies over the business cycle. Garcia (2013) shows that financial news articles predict stock returns, but this predictive power concentrates in recessions. In Table 10, we interact the indicator variable for non-distress-driven newspaper closures with an indicator for NBER recessions. We find that the decrease in return volatility and idiosyncratic risk after non-distress-driven newspaper closures is much more pronounced in recessions. The coefficient estimate of -3.38 for idiosyncratic risk (column 2) is more than twice as large as the baseline estimate of 1.41 in Table 3. Non-distress-driven newspaper closures also predict a significant reduction in volatility outside of recessions, but the effect is less significant both statistically and economically. In light of the results of Garcia (2013), our evidence of heterogeneity over the business cycle suggests that investor information processing is driving the patterns we document.

6 Additional tests

6.1 Other measures of information processing

While stock return volatility is an important outcome variable in its own right and easy to interpret, we present further evidence to support the claim that distress-driven vs. other local newspaper closures lead to different patterns in investor information processing. Table 11 shows results from the same differences-in-differences specifications as before using the number of tweets (e.g., Chen and Hwang (2022), Cookson et al. (2024)) and EDGAR downloads (Drake, Roulstone, and Thornock (2015)) to measure investor information processing. Columns 1-3 show that there is no significant change in the number of tweets, retweets,

and total tweets following distress-driven newspaper closures. However, there is a significant drop in all three measures following other newspaper closures. In contrast, we find no statistically significant difference in the number of EDGAR downloads after distress-driven or other newspaper closures.[14]

6.2 Corporate fraud

To further illustrate the importance of tracking the reason for newspaper closures, we use our approach and sample to examine the effect of distress-driven and non-distress-driven local newspaper closures on corporate fraud. This analysis follows the work of Heese et al. (2021), with a few important differences. First and most importantly, we separate local newspaper closures into distress-driven and non-distress-driven events, as before. Second, we apply the stacked differences-in-differences method of Goodman-Bacon (2021) for more reliable inference. Third, our sample ends in 2021, four years later than their period of analysis. Fourth, they have precise information on plant location at the ZIP code level, which we lack: therefore, we aggregate violations at the firm level, effectively assuming that all violations occur at the headquarter ZIP code. In spite of these differences, our results are similar to theirs for the pooled sample, even if the magnitudes cannot be compared directly.

Table A3 columns 1-2 show that the number of legal violations by local firms increases following local newspaper closures, consistent with Heese et al. (2021). However, column 3 shows that this increase in the number of legal violations is statistically significant only after non-distress-driven newspaper closures, and is twice as large for non-distress-driven closures as compared to distress-driven closures. These findings reinforce the importance of

[14]Appendix Table A2 uses news articles from Ravenpack to examine whether media coverage of the firm changes following newspaper closures. We find some evidence of a reduction in media coverage for distress-related media closures.

considering the reason for local newspaper closures: doing so in the case of corporate fraud raises the point estimate and highlights that the true treatment effect may be larger than previously documented.

7 Conclusion

Academics and commentators have been sounding the alarm bells over the disappearance of local newspapers. This paper studies the effect of local newspaper closures on the market for local firms' shares, taking into account that newspaper closures are not random, and many are distress-driven. We use a hand-collected dataset of local newspaper closures from the most recent two decades, coupled with a narrative classification of the reasons for each closure. We separate distress-driven newspaper closures from those occurring for other reasons. Doing so allows us to make progress on separating the selection effect of which newspapers are closed and the treatment effect of a newspaper closure on the market for local stocks.

We find that at least 29% of local newspaper closures are distress-driven. Volatility, trading activity, and liquidity move in the opposite direction after distress-driven vs. non-distress-driven local newspaper closures. Non-distress-driven newspaper closures predict a significant reduction in total return volatility and idiosyncratic risk. In sharp contrast, distress-driven newspaper closures predict increases in return volatility and idiosyncratic volatility. We infer that both selection and treatment effects are present, and the two have the opposite sign: closing a newspaper alone causes volatility to increase, whereas newspapers tend to close in areas with increasing return volatility. We find similar patterns for trading activity and liquidity. Ignoring information on the different reasons for newspaper closures and estimating the same empirical model would lead to an attenuation of the treatment

effect by 41-43%. Last, we find substantial heterogeneity in the treatment effect, consistent with information frictions: Newspaper closures that are not distress-driven are followed by a larger drop in volatility for small stocks, growth stocks, stocks of firms sparsely populated or rural areas, and during recessions.

Overall, our results highlight the importance of separating selection and treatment effects when studying the effect of media on financial markets, and show that local newspapers are an important source of information for investors. Our findings suggest that experts may be right to be worried about the wave of local newspaper closures over the last two decades because local investors use newspapers to process information about the firms in their area. Once we account for the selection effect of which newspapers are closed, the effect of local newspaper closures may be larger than previously documented.

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Figure 1: Closures over Time

This figure shows the number of newspaper closures by motive over the sample period. Information on newspaper closure dates and reasons are collected from Chronicling America, the U.S. News Desert database (usnewsdeserts.com), WorldCat.Org, regional newspaper archives and libraries, local websites, and blogs. Our sample covers the period 2001-2021.

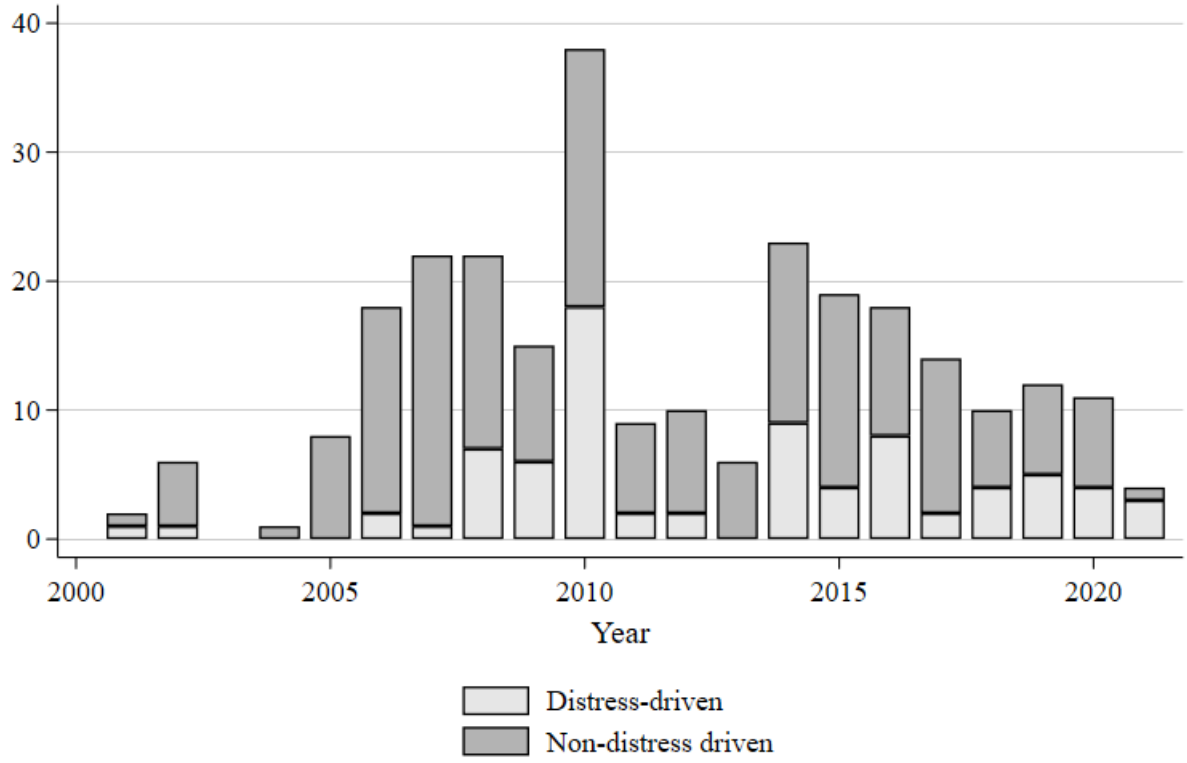
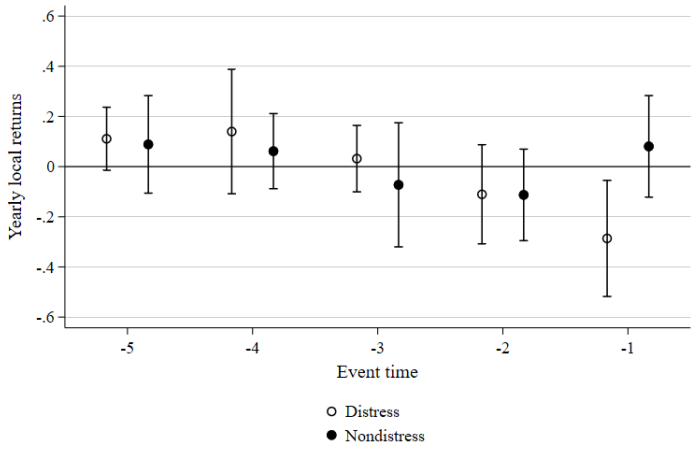


Figure 2: Pre-event local returns prior to newspaper closures

This figure plots equally-weighted yearly stock returns of firms within a 250 mile-radius around the ZIP code of a newspaper closure in the 5 years prior to the closure for distress-driven closures (indicated with hollow circles) and non-distress-driven closures (indicated with solid circles). The graph shows 95% confidence intervals around the coefficient estimate. Panel A uses stock returns of firms located within the 250-mile radius around the center of a ZIP code. Panel B restricts attention to ZIP codes with a minimum number of 20 firms within the 250-miles radius.

Panel A: Baseline result



Panel B: Minimum of 20 firms

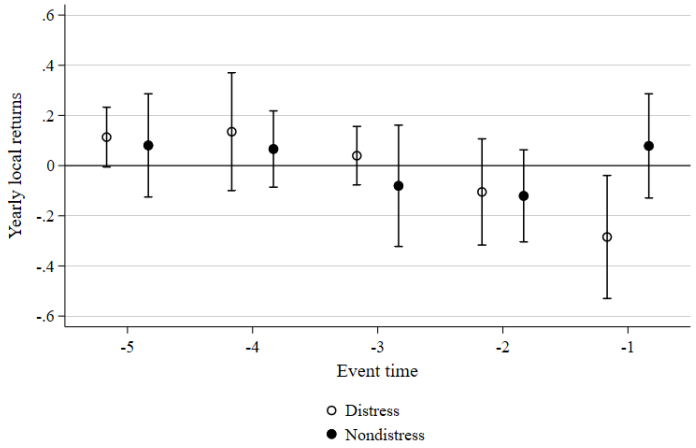
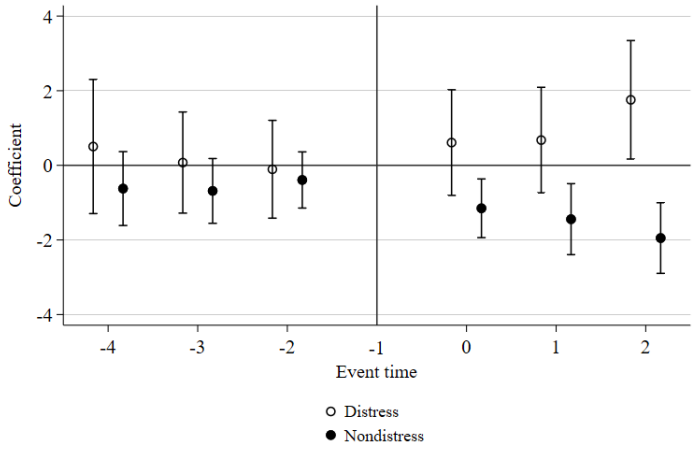


Figure 3: Idiosyncratic volatility around newspaper closures in event-time

This figure plots the coefficient of *beta* from equation 1 over years around the newspaper closure. Panel A shows the estimates for the baseline results and Panel B for matched firms that differ for distress-driven and non-distress-driven closures. The hollow circle markers indicate the coefficient for distress-driven closures, and the solid circle markers indicate the coefficient for non-distress-driven closures.

Panel A: Baseline result



Panel B: Matched control firms

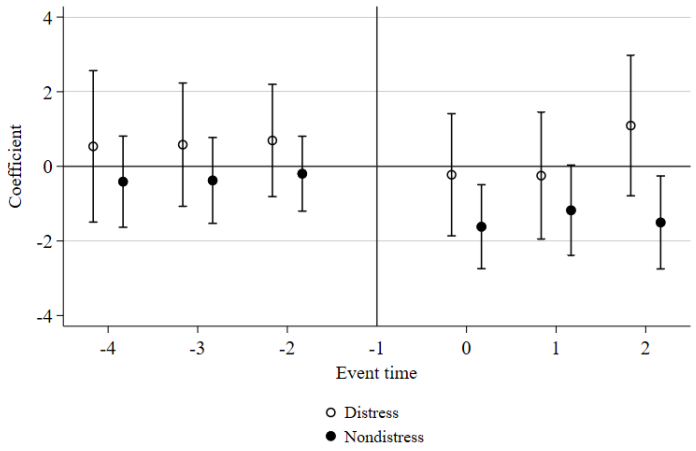


Table 1: Summary statistics of newspaper closures and affected firms

Panel A shows the number of distress and non-distress-driven local newspaper closures, and the number of publicly listed firms headquartered in ZIP codes affected by these closures each year during our sample period. Panel B shows summary statistics for affected and never-affected firms at time $t-1$ around the closure event. A firm is affected by a newspaper closure if it is headquartered in a ZIP code that experiences a newspaper closure. Information on newspaper closure dates and reasons are collected from Chronicling America, the U.S. News Desert Project database (usnewsdeserts.com), WorldCat.Org, regional newspaper archives and libraries, local websites, and blogs. Our sample covers the period 2001-2021.

Panel A: Distress and non-distress-driven newspaper closures

	Total		Distress		Non-distress	
Year	Newspapers	Firms	Newspapers	Firms	Newspapers	Firms
2001	1	37	0	36	1	1
2002	6	73	1	43	5	30
2003	0	0	0	0	0	0
2004	1	19	0	0	1	18
2005	8	162	0	0	8	162
2006	19	226	2	50	17	176
2007	21	278	1	1	20	277
2008	20	473	6	53	14	420
2009	17	115	6	72	11	43
2010	38	513	16	238	22	275
2011	10	115	2	10	8	105
2012	10	106	2	2	8	104
2013	4	106	0	0	4	106
2014	24	307	10	267	14	40
2015	17	441	4	114	13	327
2016	18	92	8	38	10	54
2017	14	334	2	20	12	314
2018	9	23	3	9	6	14
2019	10	159	5	46	5	113
2020	12	87	5	46	7	41
2021	4	52	3	51	1	1
Total	263	3,717	76	1,096	187	2,621

Table 1: Summary statistics of newspaper closures and affected firms (continued)

Panel B: Affected and never-affected firms

	Affected			Control		
	Median	Mean	S.D.	Median	Mean	S.D.
Total assets	704	5,116	11,594	548	2,793	7,524
Market cap	623	3,725	8,367	289	1,906	5,387
R&D scaled by assets	0.00	0.05	0.11	0.00	0.04	0.09
Tobin's Q'	1.42	2.00	1.64	1.23	1.73	1.41
Profitability	0.08	0.02	0.24	0.08	0.05	0.20
Leverage	0.12	0.18	0.19	0.10	0.17	0.18
Sales growth	0.07	0.15	0.51	0.06	0.13	0.43
Return volatility	40.01	48.09	31.29	42.68	53.05	34.82
Systematic risk	14.93	17.60	12.29	14.31	17.03	12.68
Idiosyncratic risk	34.79	43.26	30.79	37.85	48.63	34.64
R^2	17.37	20.84	17.48	11.66	18.12	18.12
Market beta	0.86	0.85	0.52	0.77	0.76	0.55
Turnover	0.59	0.82	0.84	0.39	0.63	0.76
Amihud illiquidity	0.02	2.40	9.29	0.15	4.45	12.29
Zero return fraction	0.00	0.05	0.29	0.00	0.10	0.38
Observations	3,599			33,786		

Table 2: Closure motives

The table below shows the breakdown of distress and non-distress-driven closure motives. Information on newspaper closure dates and reasons are collected from Chronicling America, the U.S. News Desert database (usnewsdeserts.com), WorldCat.Org, regional newspaper archives and libraries, local websites, and blogs. Our sample covers the period 2001-2021.

Motives	Closures
Distress-driven	76
Financial distress	48
Financial distress-related merger	28
Non-distress-driven	187
Within group consolidation	56
Merger not related to distress	49
Personal reasons	7
Other	84
Total	263

Table 3: Distress and non-distress closures and investor information processing

This table regresses stock return volatility, idiosyncratic volatility, and systematic risk on closure indicators, the natural logarithm of market capitalization, Tobin's Q, sales growth, leverage, profitability and R&D scaled by total assets as firm-level control variables, stack fixed effects interacted with firm fixed effects, and stack fixed effects interacted with year fixed effects on the sample of affected firms and for each event-year stack never-affected firms around the event year as control subjects. Panel A separates distress-driven and non-distress-driven closures, while Panel B pools all newspaper closures. Coefficients on control variables are reported in Table A1 in the Appendix. Standard errors are clustered at firm level, and shown in parentheses. *, ***, and *** indicate statistical significance at the 10%, 5%, or 1% level respectively.

Panel A: Separating distress-driven and non-distress-driven closures

	Return volatility	Idiosyncratic risk	Systematic risk
	(1)	(2)	(3)
Post distress closure	1.475**	1.413**	0.183
	(0.588)	(0.573)	(0.278)
Post non-distress closure	-1.353***	-1.234***	-0.501**
	(0.415)	(0.394)	(0.196)
Stack × firm FE	Yes	Yes	Yes
Stack × year FE	Yes	Yes	Yes
Adjusted R^2	0.713	0.724	0.674
N	371,434	371,434	371,434

Panel B: Pooling all closures, irrespective of closure reason

	Return volatility	Idiosyncratic risk	Systematic risk
	(1)	(2)	(3)
Post closure	-0.793**	-0.709*	-0.378**
	(0.386)	(0.367)	(0.183)
Controls	Yes	Yes	Yes
Stack × firm FE	Yes	Yes	Yes
Stack × year FE	Yes	Yes	Yes
Adjusted R^2	0.713	0.724	0.674
N	371,434	371,434	371,434

Table 4: Distress and non-distress closures and investor information processing: matched control firms

This table repeats the regressions from Table 3 while using different matched samples for the set of firms affected by distress-driven newspaper closures and non-distress-driven newspaper closures. Panel A shows the comparison of firm characteristics of firms affected by distress-driven closures and non-distress-driven closures in $t-1$ around the closure. The matched firms are selected using Malahanobis distance matching on the following covariates: the natural logarithm of market capitalization, Tobin's Q, sales growth, leverage, profitability and R&D scaled by total assets. Standard errors are clustered at firm level, and given in parentheses. *, ***, and *** indicate statistical significance at the 10%, 5%, or 1% level respectively.

Panel A: Comparison between firms affected by distress and non-distress closures

	Distress		Non-distress		Comparison	
	Mean	S.D.	Mean	S.D.	Difference	t-test
Total assets	8,312	23,780	6,972	22,195	-2,022.9	-2.47
Market capitalization	4,976	13,319	4,725	13,293	-533.9	-1.09
RD scaled by assets	0.04	0.10	0.06	0.12	0.02	5.49
Tobin's Q	1.77	1.43	2.05	1.69	0.31	4.95
Profitability	0.03	0.22	0.01	0.25	-0.02	-1.63
Leverage	0.20	0.19	0.18	0.20	-0.02	-2.63
Sales growth	0.09	0.47	0.16	0.53	0.07	3.74
Total return volatility	52.74	38.14	46.23	29.51	-6.31	-5.35
Systematic risk	20.54	16.06	16.37	10.54	-3.98	-8.65
Idiosyncratic risk	46.47	37.11	41.97	29.32	-4.36	-3.75
Market beta	0.83	0.53	0.85	0.53	0.03	1.58
R^2	23.18	20.04	19.97	16.59	-3.18	-4.81

Table 4: Distress and non-distress closures and investor information processing: matched control firms (continued)

Panel B: Information processing with matched controls

	Return volatility	Idiosyncratic risk	Systematic risk
	(1)	(2)	(3)
Post distress closure	0.176	0.286	-0.112
	(0.646)	(0.615)	(0.335)
Post non-distress closure	-1.210**	-1.234**	-0.184
	(0.547)	(0.519)	(0.262)
Controls	Yes	Yes	Yes
Stack × firm FE	Yes	Yes	Yes
Stack × year FE	Yes	Yes	Yes
Adjusted R^2	0.710	0.721	0.678
Obs	55,614	55,614	55,614

Table 5: Distress and non-distress-driven newspaper closures, trading activity, and liquidity

This table shows the regression of log turnover, Amihud (2002) illiquidity, and the fraction of zero return days (Lesmond et al. (1999)) on a variable that is set to 1 for affected firms after a distress-driven closure and to zero otherwise, on a dummy variable set to 1 after a non-distress-driven closure and to zero otherwise, the natural logarithm of market capitalization, Tobin's Q, sales growth, leverage, profitability and R&D scaled by total assets as firm-level control variables, stack-specific firm fixed effects interacted, and stack fixed effects interacted with year fixed effects on the sample of affected firms and for each event-year stack never-affected firms around the event year as control subjects in columns 1 to 3. Standard errors are clustered at firm level, and given in parentheses. *, ***, and *** indicate statistical significance at the 10%, 5%, or 1% level respectively.

	Log turnover	Amihud	Zero return fraction
	(1)	(2)	(3)
Post distress closure	-0.022	-0.121	-0.002
	(0.022)	(0.250)	(0.007)
Post non-distress closure	-0.068***	0.454***	0.009**
	(0.015)	(0.156)	(0.004)
Stack × firm FE	Yes	Yes	Yes
Stack × year FE	Yes	Yes	Yes
Adjusted R^2	0.741	0.674	0.262
N	510,938	409,286	535,879

Table 6: Tighter time intervals and controlling for time-varying heterogeneity at the state-level

This table repeats the regressions from Table 3 while using tighter intervals of -3 to +3 years around the event year (Panel A) and adding state × year fixed effects (Panel B). Standard errors are clustered at firm level, and given in parentheses. *, ***, and *** indicate statistical significance at the 10%, 5%, or 1% level respectively.

Panel A: Controlling for state × year fixed effects

	Return volatility	Idiosyncratic risk	Systematic risk
	(1)	(2)	(3)
Post distress closure	2.092**	2.091**	0.252
	(0.833)	(0.824)	(0.405)
Post non-distress closure	-1.080*	-1.364**	0.223
	(0.627)	(0.596)	(0.298)
Controls	Yes	Yes	Yes
Stack × firm FE	Yes	Yes	Yes
Stack × state × year FE	Yes	Yes	Yes
Adjusted R^2	0.713	0.724	0.674
N	371,434	371,434	371,434

Panel B: Tighter time interval of -3 to +3 years

	Return volatility	Idiosyncratic risk	Systematic risk
	(1)	(2)	(3)
Post distress closure	1.001	1.076	-0.036
	(0.710)	(0.679)	(0.356)
Post non-distress closure	-1.400***	-1.388***	-0.209
	(0.408)	(0.395)	(0.198)
Controls	Yes	Yes	Yes
Stack × firm FE	Yes	Yes	Yes
Stack × year FE	Yes	Yes	Yes
Adjusted R^2	0.739	0.745	0.718
N	204,341	204,341	204,341

Table 7: Attenuation for different model specifications and dependent variables

This table summarizes the attenuation across different models using idiosyncratic risk as an outcome variable (Panel A) and using different outcome variables (Panel B). Column 1 shows attenuation in percent, calculated as the difference in the coefficient of *Post non-distress-driven*, reported in column 2, and the coefficient of the pooled variable *Post closure*, reported in column 3, which captures both distress and non-distress-driven closures, scaled by the coefficient of *Post non-distress-driven*.

	Attenuation (%)	Coefficient on post closure	
		Non-distress	Pooled
	(1)	(2)	(3)
Panel A: By model			
Baseline	42.5	-1.234	-0.709
State × year FE	62.0	-1.364	-0.518
Tighter interval (-3 to +3)	32.2	-1.388	-0.941
Matching	39.8	-1.234	-0.743
Panel B: By outcome variable			
Total volatility	41.4	-1.353	-0.793
Idiosyncratic risk	42.5	-1.234	-0.709
Systematic risk	24.6	-0.501	-0.378
Log turnover	11.6	-0.068	-0.060
Amihud illiquidity	25.5	0.454	0.338
Zero return fraction	17.8	0.009	0.007

Table 8: Heterogeneity in information processing across firms

This table repeats the regressions from Table 3 Panel B, while interacting the dummy variables set to 1 after non-distress-driven closures with a dummy indicating that the respective firm is above the median of a certain firm characteristic, and, separately, with a dummy variable indicating that the firm has a value below the median. Column 1 splits the sample at the median value of market capitalization, column 2 at the median of the industry-level sales volatility, column 3 at the median of Tobin's Q, column 4 at the median of non-missing advertising expenditures scaled by total assets, column 5 at the median value of the number of yearly Dow Jones news articles. Standard errors are clustered at firm level, and given in parentheses. *, ***, and *** indicate statistical significance at the 10%, 5%, or 1% level respectively.

	Idiosyncratic risk				
Sample split by	Market cap	Sales vola	Tobin's Q	Advertising	Media
	(1)	(2)	(3)	(4)	(5)
Post distress closure	1.374**	1.361**	1.390**	1.963**	1.424**
	(0.572)	(0.574)	(0.574)	(0.851)	(0.573)
Post non-distress closure × low	-2.370***	0.116	-0.349	-0.462	-2.083***
	(0.631)	(0.524)	(0.526)	(0.843)	(0.579)
Post non-distress closure × high	-0.353	-2.238***	-2.035***	-2.132***	-0.960**
	(0.371)	(0.509)	(0.430)	(0.668)	(0.392)
Controls	Yes	Yes	Yes	Yes	Yes
Stack × firm FE	Yes	Yes	Yes	Yes	Yes
Stack × year FE	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.724	0.724	0.724	0.719	0.724
N	371,434	371,434	371,434	169,654	371,434

Table 9: Geographic heterogeneity in information processing

This table repeats the regressions from Table 3 Panel B, while interacting the dummy variables set to 1 after non-distress-driven closures with a dummy indicating a certain geographical feature. Low in column 1 indicates that the firm is not located in one of the top 10 metropolitan areas in the U.S., high indicates that the firm is located in one of those areas. Low in column 2 indicates that the firm is located in an area with a population of less than 750 people per square mile, and high indicates the firm is located in an area with a greater density. The corresponding density thresholds in columns 3 and 4 are 1,000 and 2,000. Standard errors are clustered at firm level, and given in parentheses. *, ***, and *** indicate statistical significance at the 10%, 5%, or 1% level respectively.

	Idiosyncratic risk			
Sample split by	Metro area	Density 750	Density 1000	Density 2000
	(1)	(2)	(3)	(4)
Post distress closure	1.394*	1.418*	1.411*	1.400*
	(2.43)	(2.47)	(2.46)	(2.44)
Post non-distress closure × low	-1.746***	-2.364**	-2.277***	-1.593***
	(-4.04)	(-3.29)	(-3.71)	(-3.46)
Post non-distress closure × high	1.012	-0.906*	-0.726	-0.694
	(1.35)	(-2.07)	(-1.57)	(-1.14)
Controls	Yes	Yes	Yes	Yes
Stack × firm FE	Yes	Yes	Yes	Yes
Stack × year FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.724	0.724	0.724	0.724
N	371,434	371,434	371,434	371,434

Table 10: Heterogeneity in information processing over the business cycle

This table repeats the regressions from Table 3 Panel B, while interacting the distress and non-distress-driven closure dummy variables with an indicator for NBER recessions. Standard errors are clustered at firm level, and given in parentheses. *, ***, and *** indicate statistical significance at the 10%, 5%, or 1% level respectively.

	Return volatility	Idiosyncratic risk	Systematic risk
	(1)	(2)	(3)
Post distress closure × recession	-0.173	0.052	-0.289
	(1.362)	(1.354)	(0.621)
Post non-distress closure × recession	-3.856***	-3.376***	-1.585***
	(0.871)	(0.840)	(0.373)
Post distress closure × no recession	1.844***	1.714***	0.267
	(0.633)	(0.612)	(0.305)
Post non-distress closure × no recession	-0.860*	-0.812*	-0.283
	(0.441)	(0.418)	(0.213)
Controls	Yes	Yes	Yes
Stack × firm FE	Yes	Yes	Yes
Stack × year FE	Yes	Yes	Yes
Adjusted R^2	0.713	0.724	0.674
N	371,434	371,434	371,434

Table 11: Distress and non-distress-driven newspaper closures and other proxies of investor information processing

This table shows the results of Poisson pseudo-maximum likelihood regressions of certain count-like outcome variables on a dummy variable that is set to 1 for affected firms after a distress-driven closure and to zero otherwise, on a dummy variable set to 1 after a non-distress-driven closure and to zero otherwise, the natural logarithm of market capitalization, Tobin's Q, sales growth, leverage, profitability and R&D scaled by total assets as firm-level control variables, stack-specific firm fixed effects interacted, and stack fixed effects interacted with year fixed effects on the sample of affected firms and for each event-year stack never-affected firms around the event year as control subjects in columns 1 to 3. Column 1 uses the number of yearly tweets on the firm, column 2 the number of yearly retweets, using the data provided by Chen and Hwang (2022). Column 3 uses the number of yearly EDGAR logs as an outcome variable. Columns 4 and 5 estimate OLS regressions with the same set of independent variables, yet monthly measures of attention and sentiment from three major social platforms (Twitter, StockTwits, and Seeking Alpha) from Cookson et al. (2024) as outcome variables. Standard errors are clustered at firm level, and given in parentheses. *, ***, and *** indicate statistical significance at the 10%, 5%, or 1% level respectively.

	Tweets	Retweets	EDGAR	Attention	Sentiment
Estimation	PPML	PPML	PPML	OLS	OLS
Data frequency	Year	Year	Year	Month	Month
	(1)	(2)	(3)	(4)	(5)
Post distress closure	-0.191**	-0.102	-0.176	-0.072	0.083*
	(0.097)	(0.240)	(0.118)	(0.049)	(0.048)
Post non-distress closure	-0.351***	-0.440**	-0.370***	-0.181	-0.052
	(0.087)	(0.189)	(0.105)	(0.138)	(0.047)
Stack × firm FE	Yes	Yes	Yes	Yes	Yes
Stack × year FE	Yes	Yes	Yes		
Stack × year-month FE				Yes	Yes
Adjusted R^2				0.404	0.119
N	64,220	64,170	64,220	382,656	382,656

Table A1: Variable definitions

This table shows variable definitions of pricing outcomes, closure variables, firm characteristics, and geographical characteristics.

Variable	Definition	Source
Pricing outcomes		
Return volatility	The variance of daily stock returns over a full calendar year.	CRSP
Idiosyncratic risk	Standard deviation of residuals from a regression of daily excess returns on the Fama-French 3 factors for the full calendar year	CRSP
Systematic risk	Standard deviation of predicted returns from a regression of daily excess returns on the Fama-French 3 factors for the full calendar year	CRSP
Market beta	Coefficient on the market premium from a regression of daily excess returns on the Fama-French 3 factors for the full calendar year	CRSP
R-squared	Share of predicted variation from a regression of daily excess returns on the Fama-French 3 factors for the full calendar year	CRSP
Closure variables		
Post closure	Dummy equal to 1 if firm is affected by a newspaper closure, 0 otherwise	Manual
Post distress closure	Dummy equal to 1 if firm is affected by a distress-driven newspaper closure, 0 otherwise	Manual search
Post nondistress closure	Dummy equal to 1 if firm is affected by a non-distress-driven newspaper closure, 0 otherwise	Manual
Affected	Dummy equal to 1 the firm has ever been affected by a distress-or non-distress-driven newspaper closure over the entire sample period, and equal to 0 otherwise	Manual
Geographical characteristics		
Density	Population per square kilometer	Opendatasoft geographical repository
Metro area	Dummy variable set to 1 if the firm is located in one of the top 10 U.S. metropolitan areas (New York City, Los Angeles, Chicago, Houston, Phoenix, Philadelphia, San Antonio, San Diego, Dallas, San Jose)	Compustat
Violation	Dummy variable set to 1 if there was a violation occurring in the given ZIP code, and to 0 otherwise	Violation-Tracker

Table A1: Variable definitions (continued)

Variable	Definition	Source
Firm characteristics		
Total assets	Book value of total assets in USD millions	Compustat
Market capitalization	Market capitalization in USD millions	Compustat
R&D scaled by assets	Research and development expenditures scaled by the book value of total assets	Compustat
Tobin's Q	Book value of total assets minus the book value of total equity plus market capitalization, scaled by the book value of total assets	Compustat
Profitability	Operating income scaled by total assets	
Sales growth	Sales divided by sales in the prior fiscal year minus 1	Compustat
Leverage	Book value of long-term liabilities plus book value of short-term liabilities scaled by the book value of total assets	Compustat
Advertising	Advertising expenses scaled by total assets	
Turnover	Average daily shares traded scaled by total shares outstanding over one quarter	CRSP
Amihud	Average of absolute daily return scaled by the dollar volume of traded shares over one quarter in US dollar thousands	CRSP
Zero return fraction	Share of days with a zero return scaled by the number of trading days in a quarter	CRSP

Table A1: Variable definitions (continued)

Variable	Definition	Source
Firm-level media activities		
Tweets	Number of Tweets	Chen and Hwang (2022)
Retweets	Number of Retweets	Chen and Hwang (2022)
Attention	a composite measure of investor attention from three major social media platforms, Twitter, StockTwits, and Seeking Alpha	Cookson et al. (2024)
Sentiment	a composite measure of investor sentiment from three major social media platforms, Twitter, StockTwits, and Seeking Alpha	Cookson et al. (2024)
News items	Yearly number of news items	Ravenpack
Relevance	Yearly news items with relevance score of 100	Ravenpack
Novelty	Yearly news items with event novelty score of 100	Ravenpack
Pos (Neg)	Yearly news items with Composite sentiment score strictly above (below) 50	Ravenpack
EDGAR	Number of EDGAR logs	SEC

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Table A1: Coefficients on control variables for Table 3

This table shows the same regressions as in Table 3, while reporting the coefficients on the control variables. Standard errors are clustered at firm level, and given in parentheses. *, ***, and *** indicate statistical significance at the 10%, 5%, or 1% level respectively.

	Return volatility	Idiosyncratic risk	Systematic risk
	(1)	(2)	(3)
Post distress closure	1.475**	1.413**	0.183
	(0.588)	(0.573)	(0.278)
Post non-distress closure	-1.353***	-1.234***	-0.501**
	(0.415)	(0.394)	(0.196)
Log market cap	-10.484***	-11.461***	0.623***
	(0.353)	(0.346)	(0.150)
Tobin's Q	2.977***	2.674***	1.233***
	(0.238)	(0.232)	(0.085)
Sales growth	0.658*	0.634*	0.215*
	(0.361)	(0.357)	(0.130)
Leverage	9.665***	9.032***	3.857***
	(1.547)	(1.506)	(0.707)
Profitability	-26.544***	-24.780***	-5.478***
	(2.338)	(2.258)	(0.846)
R&D	-15.964***	-10.879*	-14.522***
	(6.163)	(6.081)	(2.157)
Stack × firm FE	Yes	Yes	Yes
Stack × year FE	Yes	Yes	Yes
Adjusted R^2	0.713	0.724	0.674
N	371,434	371,434	371,434

Table A2: Distress and non-distress-driven newspaper closures and media coverage

This table reports the results of a regression of media coverage measures on a dummy variable that is set to 1 for affected firms after a distress-driven closure and to zero otherwise, on a dummy variable set to 10 for non-distress-driven closures and to zero otherwise, the natural logarithm of market capitalization, Tobin's Q, sales growth, leverage, profitability and R&D scaled by total assets as firm-level control variables, stack fixed effects interacted with firm fixed effects, and stack fixed effects interacted with year fixed effects on the sample of affected firms and for each event-year stack never-affected firms around the event year as control subjects, using Poisson pseudo-maximum likelihood regressions. The news analytics measures are based on the PR Edition, which aggregates information from global media organizations including PRNewswire, Canadian News Wire, and LSE Regulatory News Service. Standard errors are clustered at firm level, and given in parentheses. *, ***, and *** indicate statistical significance at the 10%, 5%, or 1% level respectively.

	News items	Relevance	Novelty	Pos	Neg
	(1)	(2)	(3)	(4)	(5)
Post distress closure	-0.016	0.033	0.019	0.030	0.024
	(0.058)	(0.046)	(0.032)	(0.048)	(0.084)
Post non-distress closure	-0.105***	-0.083***	-0.059***	-0.081***	-0.053
	(0.040)	(0.026)	(0.019)	(0.028)	(0.067)
Stack × firm FE	Yes	Yes	Yes	Yes	Yes
Stack × year FE	Yes	Yes	Yes	Yes	Yes
N	202,080	201,219	201,219	201,561	186,439

Table A3: Distress and non-distress-driven newspaper closures and ZIP-level misconduct rates

This table shows the results OLS regressions of a dummy indicating whether a respective ZIP code-year observation was associated with a corporate violation or misconduct on a dummy variable that is set to 1 for a ZIP code after a newspaper closure, ZIP code fixed effects, and state × year fixed effects in column 1, where the sample only consists of affected ZIP code. Column 2 adds control group observations to the sample consisting of ZIP codes never affected by newspaper closures around the respective closure year for every event-year stack. State × year fixed effects are replaced by stack × state × year fixed effects. *, ***, and *** indicate statistical significance at the 10%, 5%, or 1% level respectively.

	Idiosyncratic risk		
Sample	Affected	Affected + control	Affected + control
	(1)	(2)	(3)
Post closure	0.003*	0.008***	
	(0.002)	(0.002)	
Post distress closure			0.004
			(0.004)
Post non-distress closure			0.009***
			(0.002)
Stack × ZIP FE	Yes	Yes	Yes
State × year FE	Yes		
Stack × state × year FE		Yes	yes
Adjusted R2	0.415	0.410	0.410
Obs	1,209,905	7,121,558	7,121,558