The Causal Impact of Media in Financial Markets

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ABSTRACT

Disentangling the causal impact of media reporting from the impact of the events being reported is challenging. We solve this problem by comparing the behaviors of investors with access to *different* media coverage of the *same* information event. We use zip codes to identify 19 mutually exclusive trading regions corresponding with large U.S. cities. For all earnings announcements of S&P 500 Index firms, we find that *local* media coverage strongly predicts *local* trading, after controlling for earnings, investor, and newspaper characteristics. Moreover, local trading is strongly related to the timing of local reporting, a particular challenge to non-media explanations.

*Engelberg and Parsons are with Kenan-Flagler Business School, University of North Carolina at Chapel Hill. We thank Aydoğan Altı, Nick Barberis, Stefano DellaVigna (discussant), Mike Fishman, Matthew Gentzkow, Jay Hartzell, David Hirshleifer, David Matsa, Mitchell Petersen, Robert McDonald, Jesse Shapiro, Paul Tetlock (discussant), Sheridan Titman, Luigi Zingales, two anonymous referees, the associate editor and the editor. We also thank seminar participants at the NBER Behavioral Economics Meeting, the Financial Research Association Conference, Northwestern University, and Texas A&M University for helpful feedback and discussions. We thank Terry Odean for providing the large discount brokerage data. All errors are our own.

A number of recent studies demonstrate strong correlations between stories reported by the media and stock market reactions.[1] This paper addresses the *causal* relation between the two. Specifically, we ask whether media coverage of a financial event can alter investor behavior.

Showing such a causal relation is challenging because media coverage is not random, but rather the product of profit maximization by newspapers, television, magazines, etc. (Gentzkow and Shapiro (2010)). Because a number of unobservable factors that influence these coverage decisions also affect investor behavior, an identification problem arises. For any "news" event, how can we tell whether the media's coverage changed the market's response, or whether some unobserved aspect of the event simultaneously drove both media coverage and market reaction?

Researchers have followed two approaches to address this problem. The first is to select events for which determinants of media coverage and market responses can be decoupled. A well-known example is Huberman and Regev (2001), which details how a feature story in the *New York Times* caused the stock of Entremed Inc. to increase fourfold overnight. Yet, as the authors carefully show, virtually all of the facts reported in the *NYT* story had been previously published in scientific journals. While convincing in this case, the drawback to such a brute-force approach goes beyond the impracticality of requiring clinical dissection for each candidate event, a requirement that would seemingly permit only anecdotal evidence. More generally, how can one ever be certain that he or she has completely controlled for all other simultaneous determinants of investor demand and media coverage, particularly when these other determinants are measured with noise or are even unobservable?

The second cross-sectional approach sidesteps this criticism completely. The basic idea is to take two groups of agents and, *for the same information event*, vary only media exposure. DellaVigna and Kaplan (2007) employ this strategy with television viewers, showing that voting patterns can be predicted by whether an area's local cable company carries Fox News. Likewise, a field study by Gerber, Karlan and Bergan (2009) randomly assign households in the

[1] See, for example, Tetlock (2007); Tetlock, Saar-Tsechansky, and Macskassy (2008); and Peress (2008).

Washington D.C. area to receive a subscription of either the *Washington Times* or the *Washington Post*, and finds that political attitudes were altered. By holding constant the set of underlying facts, each study can convincingly identify a causal impact of media coverage.

Our study takes the second approach and is, to our knowledge, the first to do so within the context of financial markets. Using retail brokerage accounts, we first identify 19 local, non-overlapping trading markets surrounding major U.S. cities. Then, for each of these markets, we identify a local information source: the daily newspaper of that city. Especially during our pre-internet sample period of 1991 to 1996, investors near Minneapolis (for instance) were more likely to read the *Minneapolis Star Tribune* than the local papers of other regions, such as the *Seattle Post Intelligencer*. This linkage, and the fact that local media outlets often differ in their coverage of the same underlying events, affords a powerful test of the media's effects on financial market participants.

The main result is that for an earnings announcement by a given S&P 500 Index firm, trading in each of the 19 local markets is strongly related to whether the local paper covers the announcement. All else equal, local press coverage increases the daily trading volume of local retail investors, from 8% to nearly 50% depending on the specification. Although somewhat stronger for buying activity, the local media-local trading effect remains significant for selling activity as well.

We interpret these results causally, with media coverage stimulating local trading activity. The main alternative is that even though the underlying event is fixed, a local paper's coverage decisions may be related to unobserved local determinants of local trading. For example, we might imagine that geographic proximity matters: the *Star Tribune* might be more likely to report on Minnesota-based ADC Telecom because of the interests of local investors. Both here and more generally, *any* factor that makes a company's earnings more interesting to local readers is presumably a more attractive story subject for a local media outlet. If such

factors also determine local trading, then the omitted variables problem that we wish to resolve remains.

We address this possibility three ways. First, we include a number of controls intended to control for determinants of local coverage and local trading. For instance, we collect data on the local stock portfolio positions of Minnesota investors, using recent transactions and holdings to measure existing interest. The local coverage-local trading effect remains strong despite the inclusion of local investors' positions. For robustness, we also estimate our regressions including fixed effects for every (1) firm-city, (2) firm-earnings date, and (3) city-earnings date pairing. The first set of fixed effects controls for the proximity between each newspaper (or city) and firm in our sample, capturing any home bias on the part of investors or local media (Coval and Moskowitz (1999, 2001), Ivkovic and Weisbenner (2005)). The second set of fixed effects is equally important. Other information is often released around earnings dates, including guidance from the firm itself, management interviews, and analyst recommendations. Because these coincident news releases may change over time for a given firm, we include a dummy variable for, say, ADC Telecom's earnings announcement in the third quarter of 1993. The final set of fixed effects models any remaining city-level heterogeneity that may influence trading at a given point in time, such as time-varying local economic conditions or fluctuations in a city's investor participation. Even when all three are included simultaneously, local media coverage remains a strong predictor of local trading.

Second, we identify media effects from the correlation between local trading and exogenous variation in local media coverage, fluctuation that cannot plausibly be related to other determinants of trading activity. We develop two approaches. First, we exploit the fact that during our sample period, investors depend on physical delivery of their print media sources. We collect data on the daily weather in each local market, and identify weather events severe enough to likely disrupt or delay delivery of the local newspapers: blizzards and hailstorms. The idea here is that although a blizzard in Minneapolis may prevent the *Star*

Tribune from reaching its readers in a timely manner, it is clearly unrelated to either the *Star Tribune*'s financial reporting (perhaps a story on the earnings of Starbucks some 1,500 miles away) or any unmodeled demands of Minneapolis investors. In keeping with this intuition, we find that days of extreme weather sever the link between locally reported content and local trading.

Our next test is similar in spirit to the one above, but instead of relying on extreme weather, we exploit micro-level variation in the timing of a story's publication. Overwhelmingly, if an earnings story is going to be published locally, it will be within three days of the announcement. About 40% of such stories break on the first day (call this day one), another 50% on day two, and the balance on day three. One possible explanation for this pattern is that newspapers have different print deadlines, potentially influenced by differences in time zones. For example, one can imagine the earnings of Dell Computer released late Wednesday afternoon meeting the print deadline for the *Seattle Post-Intelligencer*'s Thursday morning edition, but missing it for the *Minneapolis Star Tribune* (two time zones ahead), which carries the story on Friday. Similarly, local non-financial news may compete for printing space, and may push an earnings report forward a day or two.

Regardless of the specific mechanism, we posit that whether a story breaks locally on day one, two, or three is almost certainly uncorrelated with unobservable determinants of local investor demand. The specific question we ask is, after controlling for all the fixed effects (e.g., firm-city, firm-date), is local trading best predicted on the *exact* day that local coverage occurs? It is. In regressions of local trading on *day one* (the first possible day coverage can occur), only local coverage on *day one* predicts trading. Coverage initiated on (future) days two or three has no effect on trading. Note that in addition to providing a robustness check, this is a powerful test for falsification. If we had instead found that trading on day one could be predicted from coverage on future days, the model would certainly be misspecified. Identical evidence is found

for the day two and day three trading regressions. That is, for these specifications as well, only local media coverage on the specific day of interest predicts local trading on that day.

By identifying a causal effect of the media on trading volume, our study contributes to a burgeoning literature that explores the media's influence on real outcomes (e.g., Stromberg (2004); Gentzkow and Shapiro (2004); Gentzkow (2006); and Gerber, Karlan and Bergan (2009)). Our results constitute the first systematic identification of the media's causal effects on investor behavior, and lay the groundwork for future work that explores in more detail the specific mechanisms underlying its influence.[2] In addition, our results call for a greater understanding of the media's incentives, in particular whether the media can be manipulated (Reuter and Zitzewitz (2006), Butler and Gurun (2009)).

The remainder of the paper is organized as follows. In Section I we describe in more detail the nature of the identification problem we wish to solve, and in Section II we briefly describe the data and its sources. Next, in Section III, we examine trading of retail investors in a number of local markets, with a particular eye on local media coverage. We then consider how the endogeneity of local media coverage affects our conclusions in Section IV. Section V deals with issues related to robustness and timing of the delivery of media coverage, and Section VI concludes.

I. Identifying Media Effects: An Example

To illustrate the main identification problem, consider the following example. On November 9, 2007, the *Detroit News* ran a story about local firm DTE Energy, which had reported third quarter earnings just the day before. The headline read "DTE reports 3rd-quarter gains over '06 in earnings, revenue" and the story compared DTE's third quarter EPS of $1.16 with the $1.06 that the firm had earned a year earlier. On February 21, 2008, DTE released

[2] In a contemporaneous paper, Miller and Shanthikumar (2010) also examine the relationship between investor behavior and local press coverage. Whereas our paper focuses on establishing causality, their paper focuses on understanding local and non-local investor preferences for information in local and national newspapers.

fourth quarter earnings of $1.56 per share, compared with $0.80 a year earlier. The *Detroit News* did not cover this second story. Although EPS was higher in both cases – and even higher in the second – overall trading volume was 21% higher in the three trading days after the first announcement, compared with the same interval after the second announcement.

This example is typical: many studies find a correlation between media coverage and trading volume. However, correlation alone allows us to conclude little about causation, i.e., whether media coverage contributes at all to the increased trading activity. It is certainly possible that the *Detroit News*'s coverage or that of other media outlets alters investors' responses to the earnings event. On the other hand, it is possible that aspects surrounding the first event jointly determined the likelihood of coverage and the amount of trading.

To formalize the problem, consider the following model of investor demand D:

$$D(X, M(X,Y)),$$

where M is media coverage, X is a set of characteristics that potentially determine both media coverage and investor demand, and Y is a set of characteristics that only influence media coverage. Although both X and Y are publicly observable – in the sense that they are available to the typical investor – the econometrician has little hope of perfectly controlling for them in a regression. For example, the vector X could capture innovations in firm or market fundamentals (e.g., intangible aspects of DTE's earnings news). By contrast, Y pertains to factors that influence *only* the media's objective function, but otherwise have no bearing on the behavior of traders (e.g., Detroit mayor Kwame Kilpatrick's sex scandal around the time of DTE's fourth quarter earnings announcement in February).[3]

The null hypothesis is that $D=D(X)$, that is, the second argument in $D(\cdot)$ has no impact on investor behavior. But given how broadly X is defined, the identification problem is

[3] http://www.newsweek.com/id/104993.

apparent: observing a correlation between *D* and *M* in the data, how can we be sure that failure to measure *X* isn't the culprit?

Taking a total derivative yields

$$dD = \frac{\partial D}{\partial X} dX + \frac{\partial D}{\partial M} \bullet \frac{\partial M}{\partial X} dX + \frac{\partial D}{\partial M} \bullet \frac{\partial M}{\partial Y} dY.$$

The first term is agnostic with respect to how investors learn about underlying events; it simply says that when firm or market fundamentals change, investors respond. Under the null of no media influence in this process, the second and third terms are zero. According to the second term, holding constant the set of accessible facts *X*, the media's process of reporting those facts matters. In other words, while the first term pertains to innovations in knowable facts, the second term captures the idea that the media makes *knowable facts actually known*. Additionally, their reporting may be biased (Gentzkow and Shapiro (2006)), perhaps even to the point of not reporting a relevant story at all. The final term allows the media to have an effect, irrespective of any underlying events. Spreading rumors (e.g., Van Bommel (2003)) would qualify.[4] The goal of our analysis is to empirically separate the second and third terms – the media effects – from the market's reactions to the underlying events (first term).

Our basic assumption is that investor reactions to innovations in *X* are similar in the cross-section. Following the example above, whatever DTE's 2008-Q1 earnings may imply about its prospects, it is difficult to envision reasons why Houston and Denver investors would systematically interpret this event differently, assuming the event was presented identically to them. To the extent that such similarity exists, we control for the first term (reaction to the underlying event). However, supposing that the *Denver Post* reported DTE's earnings and the

[4] Strictly speaking, the media may also be involved with the first term, to the extent that it expands the set of knowable facts. By design, our study holds constant the set of knowable facts across investors. Instances of investigative journalism would lead to an even larger impact of the media on investor behavior.

Houston Chronicle did not, we have identified a laboratory for identifying the media's marginal effect. This example summarizes our basic identification strategy.

The second part of the paper is concerned with the possibility that even this test is misspecified. Specifically, it remains possible that we may have missed unobserved elements of *X* that determine both trading and media coverage at the local level. For example, suppose that DTE planned to expand to Denver (it did not), based on its stellar earnings during the first quarter of 2008. Presumably, this fact would be more relevant for Denver investors and could explain why the *Denver Post* might have carried the story, as well as why Denver investors might have traded the stock more aggressively.

To address this concern, our most precise tests focus not on the coverage decisions of individual newspapers, but on factors that affect only the *timing of delivery* to investors. This is captured in the vector *Y* above, and might include things like a newspaper's print deadline (which would affect whether an event received late Wednesday is printed on Thursday or Friday). Such tests eliminate any correlation between coverage and trading, other than through exogenous factors that cannot plausibly be related to both.

II. Data

Our analysis requires data of three types: 1) earnings announcements, 2) local media coverage (around those earnings events), and 3) trading of retail investors.

We first collect all earning announcements dates from S&P 500 firms between January 1991 and December 2007. Earnings announcement dates and S&P 500 membership are taken from COMPUSTAT. Because we are concerned about the appropriate timing of earnings announcement dates, we crosscheck the dates in COMPUSTAT with those in I/B/E/S and only keep the dates for which we find a match. For each earnings announcement we calculate the earnings surprise (standard unexpected earnings, or SUE) based on the random walk model with price as the deflator (Livnat and Mendenhall (2006)).

We download our media coverage data from ProQuest's newspaper database. We have articles from the following cities (newspapers): Boston (*Globe*), Denver (*Post*), Detroit (*News*), Houston (*Chronicle*), Las Vegas (*Review Journal*), New York (*Times*), Pittsburgh (*Post Gazette*), San Antonio (*Express News*), San Diego (*Union Tribune*), San Francisco (*Chronicle*), Seattle (*Post Intelligencer*), St. Louis (*Post Dispatch*), St. Petersburg (*Times*), Minneapolis (*Star Tribune*), Atlanta (*Journal Constitution*), Sacramento (*Bee*), Washington (*Post*), and New Orleans (*Times Picayune*). We also collect data from two newspapers with national audiences: *USA Today* and *Wall Street Journal*.

Articles about firms in ProQuest are indexed by company name. Using the COMPUSTAT firm name we manually match each S&P 500 firm to its indexed firm name in ProQuest (some firms are linked to multiple ProQuest company names). Using the indexed firm names and the advanced search option in ProQuest, we search and download all articles in the newspaper database between 1991 and 2007 for each firm.[5] The result is a database of newspaper articles linked to S&P 500 firms. When we say that a newspaper *N* "covers" firm *F's* earnings announcement, we mean that ProQuest indexes an article in newspaper *N* on day 0, 1, or 2 after firm *F's* earnings announcement.

Our trading data come from the large discount brokerage database used by Barber and Odean (2000), with data available between 1991 and 1996. The database consists of the holdings and trading behavior of 77,795 households of which 54,297 have valid zip code information. Among these 54,297 households, 43,198 hold at least one common stock for which we have matched COMPUSTAT/CRSP information. Among these 43,198 households, 15,951 are located within 100 km of one of our 19 local newspapers. Portfolio holdings are available monthly between January 31, 1991 and December 31, 1996, while account-level trading data are available between January 1, 1991 and November 30, 1996. We use the trading data as the

[5] Some articles in ProQuest are full-text while others provide only the headline and a summary or abstract.

dependent variable in the majority of our tests, and use the holdings data as controls when we consider investor predisposition to trading certain stocks.

Table I provides summary statistics of our data. Not surprisingly, sample household accounts tend to cluster in well-populated areas like San Francisco (4,076), Los Angeles (1,913) and New York (2,808), with generally more accounts in the western United States (as shown by Zhu (2002)). However, we still have a considerable number of brokerage accounts in other major U.S. cities like Boston (635), Washington D.C. (983) and Houston (607). From the third column of the top panel in Table I, we see that for those households that hold common stock, the average number of stocks held is about 2 in every city. (Of course, this does not include mutual fund ownership or household holdings in other brokerage accounts.)

Table I also demonstrates a strong local bias in nearly every city. To measure local bias we first pool the accounts of all investors in a city and compute the number of local stocks (within a 100-km radius of the local newspaper's headquarters) held by this set; we then scale this number by the number of local stocks *available* (as inferred from COMPUSTAT). We compute a similar statistic for non-local firms (the number of non-local stocks held scaled by the number of non-local stocks available). Comparing the statistics for local and non-local stocks reveals a strong local bias that has been documented in other papers using the same database (Ivković and Weisbenner (2005), Seasholes and Zhu (2009), and Zhu (2002)). For example, the Minneapolis portfolio holds 21.8% of local stocks compared to 3.6% of non-local stocks, and the San Francisco portfolio holds 44.4% of local stocks compared to 15.1% of non-local stocks.

Local papers also report disproportionately about local firms. For example, Table I indicates that the *San Antonio Express News* covers 3.5% of earnings announcements reported by non-local firms, but over 40% for those by firms located near San Antonio. This relationship holds for each of our 19 papers and, like the San Antonio example, the differences in frequency are often quite large. The paper exhibiting the smallest bias towards local firm coverage is the *New York Times*, which reports the earnings of New York firms twice as often as those of firms

located remotely. The *Times Picayune*, at the other extreme, is over 100 times less likely to cover earnings of a firm located outside the New Orleans area.

III. Local Trading Responses to Local Media

The appeal of studying local media coverage is that it allows us to examine the behavior of traders subjected to different media coverage of the same information event. Presumably, this situation describes virtually every trader each time a piece of information is released into the market. However, we almost never observe a given investor's information sources (television, radio, print media, internet, personal advice, etc.), and even if we did, we usually do not observe the investor's responses to information. In aggregate data, this greatly limits our ability to make causal inferences about the media's influence on financial market participants.

Our empirical setting is fortunate in that it allows us to identify specific groups of investors who are more or less likely to receive coverage by specific media outlets. Identification here relies on the assumption that an investor living in a given metro area is more likely to read the local newspaper than another regional paper (i.e., a different city's local paper). Importantly, identification does not require investors to rely solely on local papers for their financial information, although some may. More exclusive reliance on local media would increase the power of our tests, but is not necessary to achieve identification.

As seen in Table I, the mean value of *Local Media Coverage* is only 2.8% (standard deviation 16.6%), indicating that roughly one in 35 quarterly announcements is reported in local newspapers. By contrast, the *Wall Street Journa*l accompanies an earnings release with a story or report 29% of the time in our sample (1991 to 1996) and 33% of the time over the period 1991 to 2007.

A. Methodology and Specification

The relation between local media coverage and local trading is evident from simple correlations. On days in which earnings are reported in the local paper, the average absolute dollar volume of local trading in the mentioned stock is $2,200. By contrast, the average local trading on "non-news" days at the local level is only $290. (The small absolute magnitudes are of course due to the fact that the unit of observation is at the firm-city-date level.) While a sizeable difference, local trading is likely determined by a number of other factors, some of which may be correlated with local media coverage. Thus, in our main tests we estimate the following multivariate regression:

$$\begin{aligned} &Log(Local\,Trading\,Volume_{i,j,t}) \mid Earnings_{j,t} = \alpha + \delta \bullet Local\,Media\,Coverage_{i,t} + \\ &\phi \bullet Firm\,Attributes_{i,t} + \zeta \bullet Earnings\,Surprise_{j,t} + \\ &\gamma \bullet Media\,Fixed\,Effects_{i} + \sigma \bullet City\,Fixed\,Effects + \varepsilon_{i,t} \end{aligned} \quad (1)$$

The dependent variable measures the trading responses of retail investors in each of the 19 local markets, *i*, to S&P 500 firm *j*'s earnings release at time *t*. All local markets are mutually exclusive, so that trading of firm *j*'s stock may occur in local market 1, but not in local market 2, and so on. The goal of equation (1) is to understand what firm, media, investor, and earnings characteristics explain these cross-sectional differences in local trading behavior.

The dependent variable is constructed over a three-day trading window, so that if firm *j*'s earnings are released on Wednesday, local trading is recorded if it occurs on that Wednesday, Thursday, or Friday. In each specification of Table II, the trading variable is the natural logarithm of one plus the absolute dollar trading volume in firm *j*'s stock, aggregated across all investors (for which we have records) in region *i* within three days (0, 1, or 2) of the earnings announcement. The main explanatory variable of interest is *Local Media Coverage*, which takes a value of one if region *i*'s local newspaper reports firm *j*'s earnings (also within three days of its announcement), and zero otherwise.

Crucially, equation (1) also includes paper fixed effects for each of the 19 local newspapers. This means that *Local Media Coverage* is identified solely from the differential responses between a newspaper's local readers and its non-local readership. For example, *Media Fixed Effects* includes a dummy variable for the *Houston Chronicle* that takes a value of one, for each of the 19 local markets, whenever the *Chronicle* covers an earnings announcement. Because there are 19 such dummy variables, each local paper is allowed to have a differential influence on trader behavior. However, only for the observations corresponding to Houston investors does *Local Media Coverage* equal one, allowing the Houston paper to have an *additional* impact on those investors most likely to be exposed to the story (local investors).

We also include controls for media mentions in either of two national media: *Wall Street Journal* and *USA Today*. Both papers have national readership and thus, resist the linkage to specific investors that is possible for local papers. Nonetheless, their inclusion in our regressions controls for any omitted correlation between local and national media, affording us the ability to uniquely identify media effects through local channels.

Other important control variables include *Firm Attributes*, which includes each firm's market capitalization (measured at the end of the most recent fiscal year) as well its Fama-French 30 industry classification. By clustering residuals by firm, we compute standard errors that allow each firm *i* to have its own unobserved effect on the likelihood that a newspaper covers its earnings, and that allow such firm-specific heterogeneity to change over time (Petersen (2009)).[6] Some of our robustness tests (see Table VII below) include firm fixed effects, firm-city fixed effects, and other similarly constructed variables intended to capture unobserved heterogeneity.

[6] Clustering by firm allows standard errors to be correctly computed in the presence of a firm fixed effect or a temporary effect (e.g., a firm effect that decays over time). In unreported tests, we also find that the inclusion of firm fixed effects in the estimation of equation (1) makes no qualitative difference for our results.

The *Earnings Surprise (SUE)* control variables account for the fact that some earnings events lead investors to revise their expectations more than others, and therefore are more likely to generate trade. To capture these differences, we calculate *SUE* as the difference between actual earnings and earnings four quarters ago divided by price (see Livnat and Mendenhall (2006) for a detailed discussion of *SUE* construction). We form quintiles of this surprise variable after pooling all earnings announcements.[7]

B. Results

Table II presents the results of linear regressions of equation (1). The first and second columns include only firm clustering and date fixed effects. As can be seen, *Local Media Coverage* increases local trading volume in the typical market by almost 75%. Column 2 adds industry controls and firm size, and although larger firms are associated with more trading (p=0.000), this has only a trivial effect on both the economic and statistical significance of *Local Media Coverage.*

Controls for the magnitude of *Earnings Surprise* reveal an intuitive finding. The omitted dummy is the middle quintile of least surprising earnings announcements, relative to earnings four quarters ago. Column 3 indicates that trading is highest following the most surprising earnings events, both positive and negative. The coefficients on SUE_1 and SUE_5 suggest increases in trading volume of about 7% to 8%, both effects highly significant. In contrast, those corresponding to less extreme earnings surprises, SUE_2 and SUE_4, are much less statistically and economically significant. A rational interpretation is that *SUE* captures an investor's surprise about cash flows, so that SUE_1 and SUE_5 events are associated with significant portfolio rebalancing. These results are consistent with a number of behavioral explanations. In either case, the coefficient of interest on *Local Media Coverage* remains essentially unchanged.

[7] The results are nearly unchanged if we use alternative ranking criteria, such as within-industry ranks, within industry-year ranks, and ranks based on deciles rather than quintiles. The results are also unchanged if we use a definition of earnings surprise based on the median analyst forecast (rather than the random walk model).

In unreported tests, we have also run a number of regressions with an analyst-based definition of earnings surprise (Livnat and Mendenhall (2006)) and finer breakpoints, for example, deciles instead of quintiles. The magnitude on any *SUE* variable never exceeds 0.15 in any regression specification. In other words, when we compare the effect of the information in *SUE* with the effect of media coverage (*Local Media Coverage*), the media effect is consistently three to 10 times larger than the information effect. At least for retail investors, demand appears to be far more sensitive to media coverage than to the underlying information.

While *SUE* is a conventional way to measure the information content of an earnings event, other salient facts also disclosed at the earnings announcement date may affect trading. To capture newsworthy earnings announcements, column 4 includes a dummy variable for each of the 19 local and two national media outlets, allowing them to affect trading in any market. The marginal effects shown for each paper are relative to the benchmark case in which neither a local nor a national media outlet covers the earnings announcement.[8] As can be seen, coefficients on most of the individual papers are not statistically different from zero, indicating that most have only a trivial effect on their non-local investor community. The few exceptions include the *New York Times* and the *San Francisco Chronicle*, newspapers widely read outside of their immediate catchment area.

In the final column of Table II, we include fixed effects for each of the 19 local markets, allowing average trading volumes to vary across cities. This set of controls is important, increasing the R^2 by almost two percentage points. The results here are intuitive. We see that relative to investors in Denver (the arbitrarily omitted city), local trading volume is higher in larger markets and/or those overrepresented by the specific broker in our data such as Boston, Houston, New York, San Francisco, and San Diego, and is lower in smaller ones such as Las Vegas, San Antonio, and St. Louis. Moreover, with such city controls, the coefficient on *Local Media Coverage* is now identified purely off of differences-in-differences between city-

[8] Note that the "dummy trap" does not apply among the set of newspaper fixed effects because multiple local newspapers may cover a firm earnings announcement.

newspaper pairs. That is, the coefficient on *Local Media Coverage* picks up only the average marginal impact of the media source being local, not the average effects due to larger cities, more influential papers, etc. Taking the final column as the most indicative of the underlying behavior, local media is associated with a 48% increase in trading activity.

The estimates in Table II are presented for all transactions lumped together. In Table III, we disaggregate them, showing the results for buys and sells in Panels A and B, respectively. A priori, one might expect the effects to be stronger on the buying side, given the evidence that retails investors are unlikely to sell stocks short (Barber and Odean (2008)). If retail investors do not sell short, then they can only sell when they hold the stock, which presents fewer occasions upon which to respond negatively to local media coverage.

As expected, the coefficients in this table approximately sum to those in the previous table. However, Panel A shows that although buying is responsible for more than half of all trading activity, with coefficients ranging from 0.46 (column 1) to 0.29 (column 5), the disparity over selling activity is small. In Panel B, we observe slightly lower magnitudes (0.38 to 0.26) for the selling/shorting regressions, but *Local Media Coverage* remains highly significant in each specification. Thus, Table III shows clearly that the effects of the media, or at least the local media, are pervasive across both additions and subtractions to investor portfolios.

IV. Endogeneity of Media Coverage

The primary advantage of our cross-sectional approach is that, by construction, it eliminates the typical concern that reactions to underlying events, rather than to media coverage, are driving the result. In this section, we discuss a second type of endogeneity particularly relevant for analysis of local media coverage. Specifically, because we are analyzing a number of local newspapers that likely cater to the interests of local investors, the possibility arises that local media may simply *reflect* rather than *cause* the trading patterns we observe. For example, consider the hypothetical case in which a local newspaper polls its readers, asking

which stocks they would like the paper to cover. If the paper heeded these suggestions, then the observed correlation between local coverage and local trading would not be spurious but the causation would run in the reverse direction.

Fortunately, our data are well suited to address this possibility. The analysis in this section is organized into three parts. First, we collect a number of additional control variables designed to measure each local market's pre-existing interest in certain stocks. As we will see, many of these measures are very precise, allowing us to argue that any remaining relation between trading and media coverage can be interpreted in the desired (causal) way. Our second and third tests allow for even more precise identification. We identify two characteristics that cause interference with the transmission of media coverage to investors, but leaves unchanged both the underlying content (e.g., information, media spin) as well as the pre-existing investor demand. As we will see, such exogenous variation strongly predicts trading, posing a significant challenge to alternative interpretations.

A. Local Demand

Our first set of tests is motivated by the observation that both retail and institutional investors appear to tilt their portfolios toward geographically local stocks at the expense of their remote counterparts (Coval and Moskowitz (1999, 2001), Ivković and Weisbenner (2005), Zhu (2002), Seasholes and Zhu (2009)). Regardless of why such "home bias" exists, the concern is apparent.[9] In addition to being more widely held by local investors, local firms are more likely to be covered by local newspapers. Thus, what we interpret causally as a media effect may reflect little more than the tendency of both local papers and local investors to pay attention to local stocks.

[9] Coval and Moskowitz (1999, 2001) argue for an information-based explanation of the home bias among institutional investors. Ivković and Weisbenner (2005) make a similar argument for retail traders. Zhu (2002) and Seasholes and Zhu (2009) provide a behavioral explanation of the home bias of retail traders based on familiarity.

To address this possibility, for each of the 19 regions *i*, we augment equation (1) to include a dummy variable indicating whether firm *j*'s headquarters is within 100 km of the local newspaper.[10] This designation allows us to identify stocks likely to be of particular interest to each cohort of local investors. Confirming the home bias found in previous studies, we find that investors are more likely to both hold (0.43% vs. 0.16%) and trade (9.9% vs. 2.0%) the stocks of local firms following earnings announcements. Local coverage is tilted even more toward local firms (19.2% vs. 2.7%).

Even so, column 1 of Table IV indicates that even when all local firms (<100 km of the paper's headquarters) are excluded, *Local Media Coverage* remains strongly related to local trading. Here, the thought experiment is to compare the trading patterns between a Houston and San Antonio investor, after Ohio-based Proctor and Gamble (P&G) releases its quarterly earnings. From the perspective of each investor, P&G is a non-local firm, with headquarters removed by over 1,000 miles. However, if the *Houston Chronicle* reports P&G's earnings while the *San Antonio Express News* does not, trading volume in Houston increases 28.3% relative to its normal volume, while no similar increase is seen in San Antonio. Although roughly half the magnitude observed in the final column of Table II (all firms), this estimate shows that the media has a substantial influence on demand for stocks that are less likely to be of local interest.

The second column presents this analysis in a slightly different way, including a dummy variable for a local firm, and returns to the original, full sample. Adding this variable permits us to observe a "local firm effect" of nearly 50%, confirming the previous findings of Ivković and Weisbenner (2005), Zhu (2002), and Seasholes and Zhu (2009), who document a home bias for retail traders using the same database. However, our primary interest is in the *Local Media Coverage* variable; the second column reveals that it strengthens slightly to over 0.3, and remains highly significant.

[10] Alternative breakpoints (e.g., 200 km, 50 km) yield nearly identical results.

The third and fourth columns consider more precise proxies for the pre-existing demands of local traders. Although a firm's geographic proximity may influence an investor's willingness to hold or trade its stock, other factors may generate cross-region differences in investor interest. For example, it would not be surprising for investors in St. Louis (home to beer brewer Anheuser-Busch during our sample period) to be interested in the fortunes of other brewers or distillers, even those located remotely. Similar arguments can be made for other cities, whose investors may cluster along certain stocks or industries. If local media outlets understand such preferences, and consider them in their coverage decisions, then the same identification remains - media coverage may simply reflect existing investor interest rather than cause it.

To address this concern, we examine the individual portfolio holdings of each investor within each of the 19 local markets. This exercise allows us to identify stocks likely to be traded in each market, based directly on which stocks are already held or frequently traded. For each region *i*, we construct two additional local variables: 1) the fraction of investors within region *i* that own stock *j* at the beginning of the trading month, and 2) the fraction of investors within region *i* that traded stock *j* the previous month.

The third column shows the results of the regression, once we add the first control variable. As can be seen, although *Fraction Locally Held* is observed as being strongly related to local trading, its inclusion actually strengthens the local media variable, which remains highly significant. The fourth column is similar, except that *Fraction Locally Traded* is the key control variable. It has a similarly strong effect (as expected), although the coefficient on *Local Media Coverage* remains strong. The final column includes all three controls for pre-existing local demand simultaneously, and reports an R^2 of over 8%, some 50% larger than the base regression in the final column in Table II. The increase in explanatory power, along with the extremely high significance of each control, suggests that we have controlled for a large portion

of the pre-existing local demand for each stock *j* and region *i.* If so, then the coefficient on *Local Media Coverage* (roughly 28%) represents the pure effects of media coverage.

In unreported tests, we conduct a number of robustness checks similar to the ones reported here. For example, varying the definition of a "local" firm (e.g., 200 km, 50 km) makes very little difference. Similarly, we experiment with including longer time periods, for example, averaging the fraction of local investors that hold a given stock over several months up to a year, and using dummy variables instead of continuous variables in the regressions. None of these alterations make a meaningful difference.

B. Extreme Weather and Transmission of Media Content

The strategy in the previous section was to explicitly control for each market's existing demand for certain securities, so that any remaining relation between trading and media can be interpreted causally. Here, we attempt to achieve identification through different means. The goal of this analysis is to find exogenous variation in the transmission of media stories to investors, but crucially, to leave both the content of the media story and the demands of local traders constant. Moreover, we look for high-frequency variation, which is particularly convenient in attempting to hold constant investor demands, which may change over longer time horizons.

In our search for exogenous variation in the dissemination of media content to investors, we are aided by the fact that our sample period (1991 to 1996) is largely pre-Internet.[11] This implies that in contrast to the modern electronic era: 1) printed content had to be processed and transferred to an actual printed media (the paper), and 2) the printed newspaper needed to be physically delivered, usually door to door by a delivery person, or more likely, and in the personal experience of one of the authors, a "paper boy." Disruptions in either step would delay

[11] While our time period is largely pre-Internet it is not *entirely* pre-Internet. For example, Barber and Odean (2002) note that of the almost 78,000 households in the large discount brokerage database, 1,607 switch from phone-based to Internet-based trading at some point during the 1991 to 1996 period.

or altogether prevent media content from reaching investors, but because both are unrelated to either the paper's content or the investor demand for financial securities, they represent the ideal type of exogenous variation that makes identification feasible.

Our first source of variation is extreme weather. For each of our 19 local markets, we collect weather data. Weather data are taken from the National Climatic Data Center at ftp://ftp.ncdc.noaa.gov/pub/data/gsod, which provides daily data for each weather station as well as the coordinates (longitude and latitude) of each station. For each trading area, we use weather from the station closest to the local newspaper's zip code. The weather data include high and low temperatures, inches of snow, precipitation, and indicators of extreme weather.

From these data, we identify two types of weather events likely to impede or significantly delay delivery of the local daily newspaper: 1) hailstorms, and 2) freezing rain and blizzards. Following Loughran and Schultz (2004), we define a *Blizzard* as greater than or equal to eight inches of new snow, although other definitions give similar results. *Blizzard* and *Hail* are both dummy variables that take a value of one if the extreme weather event occurs on either the earnings announcement day or the day that immediately follows. Extreme weather events, as expected, are clustered in the Northeast and Midwest (e.g., Boston, Minneapolis, Detroit). Across all markets, we identify 736 observations that include a *Blizzard*, and 1,188 that include *Hail*. There is one overlap, so their union, *Extreme Weather*, includes 1,923 city-level observations.

The first column of Table V shows the results of estimating (1),with two additional variables: *Hail* and *Hail*Local Media Coverage*. The full set of controls from the final column in Table II is deployed. With the presence of the interaction term, the coefficient on *Local Media Coverage* now corresponds to the effect of local media on local trading on days without *Hail*. As can be seen, this is nearly identical to the baseline case, shown in the final column of Table II, with a point estimate of 0.48 ($p<0.001$). The presence of *Hail* does not have a meaningful impact on local retail trading; only the weakest inference can be made regarding the

positive point estimate (p=.17). However, we are interested in the sign on the interaction term, which is negative and highly significant, even more so than the original *Local Media Coverage* coefficient itself. This indicates that on extreme weather days, the relation between media coverage and trading is severed entirely.

Importantly, the interaction here has nothing to do with *Hail* lowering the probability of local financial reporting, because indicators of a mention in each of our 21 papers are still included. Instead, the negative sign on the interaction demonstrates that local media content pertaining to the firm of interest, *on days when it is unlikely to be delivered to local investors*, has no effect on trading. This finding, we argue, isolates precisely a media effect – it holds the underlying information constant (as do our previous regressions), but also keeps constant the unobservable demands of local traders.

The second column presents the same analysis, except for our second type of extreme weather event, *Blizzards*. The findings and their interpretation are nearly identical. We see that *Blizzards* alone are not observed to be associated with local retail trading. That is, on days when the local paper doesn't report any news about the firm of interest (recall that the presence of the interaction leads to this interpretation of *Blizzards*), the presence or absence of a *Blizzard* makes no difference to trading decisions.[12] However, as above, the interaction is strongly negative, more than offsetting the positive effects of *Local Media Coverage*.

The final column aggregates all instances of *Extreme Weather* and delivers nearly identical results. On days when the delivery of local news is exogenously delayed or prevented, investors do not respond to any content. This evidence strongly suggests that the media is not

[12] Note that this finding implies at least some reliance on local newspapers for financial information. Instances of extreme weather (e.g., *Blizzards* and *Hail*) are temporary disruptions in the delivery of local news sources, but are unlikely to be correlated with, for example, weekly subscriptions to financial magazines. The fact that inclement weather only dampens trading on days when local papers report news thus suggests that local papers fill this function for at least some retail investors.

passively correlated with a pre-existing relation between local trading and the information contained in media reports.[13]

V. Robustness and Timing

In this section we consider a number of robustness issues, related to how we measure local trading, as well as to how we control for unobserved heterogeneity in the determinants of local trade.

A. Alternative Definitions of Local Trade

In Table VI, our aim is to reduce the potential for a few large orders to generate the results we find. Instead of trading volume, in the first three columns we alternatively define as the dependent variable the natural logarithm of the number of local accounts that trade the stock of interest. This in turn changes the interpretation of the local media coefficient. Instead of the percentage change in absolute trading volume, the coefficient on *Local Media Coverage* represents the increase in the number of local traders who trade, regardless of the size of their order.

The first column, for example, indicates that a mention in the local newspaper increases by approximately 4.67% the number of accounts trading the stock of interest within three days of its earnings being locally reported. The mean number of households in each local market is 840 (standard deviation=1,028), ranging from only 134 accounts in New Orleans to 4,078 in the San Francisco Bay Area. Thus, local news mentions affect, on average, as few as two traders (4.67%*37=1.7) to well over 100. The second and third columns paint a similar picture as Table

[13] The standard errors in Table V are calculated assuming independence in the error terms across cities, but not necessarily across stocks (standard errors are clustered by firm). Given that extreme weather may affect trading of many stocks within a given region, we verify that the extreme weather results hold when standard errors are clustered by city. The interaction term in each column of Table V is still significant at better than the 1% level. Bootstrapping standard errors (n=50, 100, and 250) gives nearly identical results to those seen in Table 5.

III; the percentage change in the number of buy and sell orders are significantly related to *Local Media Coverage*, and in approximately equal magnitudes.

The next specification, shown in the final three columns, further reduces any residual size effects. Here, we map the underlying trading behavior to a discrete representation. The dependent variable takes a value of one if there is *any* local trading in the stock corresponding to the earnings announcement, regardless of either the number of traders or size of their trades. For example, suppose that IBM released its earnings on a given date, and that the following day 100 traders in San Francisco traded some $100,000 worth of IBM stock, while in New Orleans only two trades summing to $500 are observed. Despite the substantial size differences, the dependent variable takes a value of one in each case. As before, all estimates here include the full assortment of control variables shown in the final columns of Tables II and III.

Columns 4 to 6 of Table VI show the results of linear probability models for the discrete variable specification (although probits present similar estimates).[14] As in column 1, column 4 aggregates all transactions together, indicating that *Local Media Coverage* increases the probability of observing any local trade by about 5%. The second and third columns separate this effect into buy and sell transactions, where, also as before, the effect is relatively equal between the two and highly significant.

B. Fixed Effects

The various sets of control variables employed in Tables II through VI attempt to control for any simultaneous determinants of local media coverage and local trading. Underlying these regressions is the OLS consistency assumption: after the inclusion of these controls, a media's decision to report an earnings announcement is unrelated to local trading. The plausibility of this assumption is, of course, difficult to judge for the same reasons that an identification

[14] We use the LPM specification to accommodate the large number (1,350) of date fixed effects in our sample.

problem exists at all. In general, we never perfectly observe the complete set of determinants expected to influence local coverage and trading decisions.

In Table VII, rather than attempt to control for these determinants with observables, we include a large sets of fixed effects to sweep out such unobserved heterogeneity. Recalling that our unit of observation is defined at the city-earnings date-firm level, we can still estimate equation (1) with fixed effects for any pairwise combination of these. For example, city-firm pair fixed effects account for time invariant relations between a given city's investor group and a given firm. Obviously, this accounts for any home bias, but is considerably more general.

The first column shows the results when firm-earnings date fixed effects are included, along with the complete set of controls employed in the final column of Table II. Given that date effects were already present, the addition of firm-date dummies simply allows for trading in any market to depend on a given firm at a given time. This obviously subsumes the *SUE* variables, and controls for any unobserved events that influence a firm's cross-sectional appeal to local traders. However, as can be seen, this set of controls makes very little difference, with the coefficient on *Local Media Coverage* being almost identical to that in the final column in Table II.

The second column of Table VII includes firm-city dummies. While still highly significant, the magnitude on local coverage is cut nearly sixfold. This is not surprising, given that in Table IV we saw that a city's past trading activity in a stock strongly predicts future trading. Nonetheless, it is striking that even for a given firm-city combination, a local trader's exposure to the firm's events strongly predicts additional trading. The third column shows the results when we include fixed effects for each city-earnings date. As can be seen, this gives very similar estimates to both the first column of Table VII and to the final column of Table II.

We include each of the pairwise fixed effects in the final column, and therefore identify media effects net of city-firm, city-date, and firm-date fixed effects. The magnitude of the coefficient indicates an 8% increase in local trading activity, after sweeping out the unobserved

determinants of trading and coverage captured in the fixed effects. Although the magnitude falls sharply (from 47% to 8%) when city-firm fixed effects are included, the purpose of Table VII is identification rather than specification of the true underlying model. For example, suppose a newspaper covers every earnings announcement of its favorite firm. Even if this was the *reason* trading volume for the firm was high in the newspaper's city, including a city-firm fixed effect will mask this causal relationship.

C. Timing of Media Stories and Timing of Trading

Our final identification test builds upon the results in Table VII, and like the weather tests in Table V, exploits a second source of exogenous variation in the transmission of media coverage to investors – a newspaper's "time-to-print." In contrast to the modern electronic information age where web-based news outlets can collect and disseminate stories in hours or minutes, in our sample period print newspapers require substantial lead time in order for information on day t to be printed on day t+1. Fortunately, this requirement introduces a further source of variation that, in theory, is perfectly uncorrelated with either firm-side or investor-side determinants of trading decisions.

We do not observe individual papers' deadline times, and even if we did, we do not observe the exact times when firms release earnings. However, we can infer the necessary information from the empirical distribution of earnings dates and story dates. We are specifically interested in whether micro-variation (e.g., over one or two days) across local papers predicts similar micro-variation in local trading. For example, if San Diego's *Union Tribune* reports Home Depot's earnings on Tuesday, and the *Boston Globe* reports it on Wednesday, do we observe abnormal trading volume of Home Depot in San Diego on Tuesday, and abnormal trading volume in Boston on Wednesday? Like the tests of extreme weather tests, such idiosyncratic, high frequency variation is difficult to claim as correlated with unobserved determinants of local trading.

To conduct this analysis, we first take the sample of all earnings announcements and determine whether the first local report (if it exists) ran on the same day of the release (t), the day immediately following the release (t+1), or the day thereafter (t+2). We then take each of the 19 markets and separate local trading into that occurring on day t, day t+1, or day t+2. The question we wish to answer is whether a newspaper's decision to run an earnings story on any particular day, say on day t+2, predicts trading on *that day*.

The data reveal substantial variation across newspapers in the time between earnings announcements and news coverage. About 40% of reported stories are broken on day t, about 50% on day t+1, and the balance on t+2. However, this ratio differs considerably by newspaper. For example, papers on the West Coast have the benefit of a later time zone (PST), and would be expected to "pick up" late-day announcements more easily than their peers on the East Coast. For example, the t+1 to t+2 ratio for three large PST newspapers, the *Seattle Post Intelligencer*, *San Francisco Chronicle*, and *Los Angeles Times*, is 4.25, 3.63, and 4.75, respectively. By comparison, the same ratios for the EST *New York Times, Atlanta Journal Constitution*, and *Pittsburgh Post Gazette are* 3.09, 2.76, and 3.81, respectively. However, there are presumably paper-specific issues unrelated to time zones that also influence deadlines.[15] Regardless of the reason, our tests simply require that such differences be exogenous to the underlying content, as well as to local investor preferences for certain stocks.

In Table VIII, we show three separate regressions for each of the three days following an earnings release. Each of the regressions includes the three pairwise fixed effects from the final column in Table VII, the most stringent set of controls allowable given the city-earnings date-firm unit observation. The first column shows that when attempting to predict trading on day t, only coverage on day t matters. Coverage on future dates t+1 or t+2 are not related to day t trading, with point estimates roughly one-third of the standard error. On the other hand,

[15] For example, the San Diego *Union Tribune* has the highest percentage of stories reported on day t+2 in our sample, despite being located on the West Coast.

simultaneous news coverage (t) is strongly related to local trading, with a magnitude of roughly 9%, virtually identical to the magnitude seen in the last column of Table VII.

This result is important for assessing model misspecification. If, instead, we had found that future coverage – in this case, coverage on either days t+1 or day t+2 – predicted day t trading, this would be strong evidence of poor specification. One could imagine, for example, that even after city-firm fixed effects, there could be particular earnings announcements of special relevance for some cities. Note that this is not accounted for in either city-date or firm-date fixed effects, as the concern would require the union of a particular city, a particular date, and a particular firm.

However, this criticism pertains to the newspaper's coverage decision, and is unlikely to be related to single-day variation around its reporting date. This assumption allows us to simultaneously test for causal media effects (by looking for same-day correlation between reporting dates and trading) and for model misspecification (by looking for a lack of correlation between trading and reporting on future days).

The second column repeats the same analysis, but considers only trading one day after the firm's earnings release. Here, we have the opportunity to test not only for misspecification by allowing future media coverage to affect trading, but also to check for lagged effects by looking at past media coverage. Neither is seen to matter, although a significant coefficient on day t would not be evidence of misspecification, but rather would suggest delayed reaction to media coverage. The final column considers trading two days after the earnings release, and confirms the evidence of the previous two columns. As before, only local media coverage on day t+2 predicts local trading on t+2. Unless such micro-variation is correlated with high-frequency changes in local investor demand, this result provides strong evidence that media activity can influence the behavior of investors.

VI. Conclusion

We exploit the geographic variation of local paper readership to design and implement empirical tests that allow the media's effect on investors to be identified, distinct from responses to the underlying events. Analyzing the simultaneous reactions of investors in 19 local markets to the same set of information events (earnings releases of S&P 500 Index firms), we find that the presence or absence of local media coverage is strongly related to the probability and magnitude of local trading. Although we would expect stories reported by local newspapers to reflect the investment interests of local readers, this does not explain the evidence. When we examine the individual portfolios of each local investor, we find that the local media coverage-local trading relation remains strong for non-local firms, as well as for those sparsely held or traded by investors in the local market.

Perhaps the strongest causal evidence comes from examining exogenous shocks to the transmission of media coverage to local investors. On days when extreme weather events (hailstorms and/or blizzards) are likely to disrupt the normal delivery of daily newspapers, the link between media coverage and trading is broken. This is an important test, because weather shocks are not correlated with either underlying content or unobservable determinants of investor demand. A complementary test takes advantage of small (one- or two-day) differences between days in which earnings stories are carried by newspapers, presumably because of differences in time zones, printing technology, and so forth. We find that trading patterns are strongly related to the local patterns of media coverage. For example, an earnings report by the *San Francisco Chronicle* on Wednesday stimulates trade in the Bay Area on Wednesday, whereas the entire set of events is shifted in Atlanta if the *Journal Constitution* reports the same event Thursday. This pair of tests is difficult to reconcile with alternatives to a pure media effect on financial market participants.

The cross-sectional nature of our empirical design largely eliminates the usual omitted variable concern; however, the corresponding improvement in identification is not free. By

carving up the trading space into small regions (recall that the unit of observation is defined by firm-date-city triple), we have limited the ability of any one group to directly influence aggregate market outcomes. Specifically, our largest local market is the San Francisco Bay Area, for which we observe the actions of some 4,000 individual investors. But on any given day, most of these investors are not active, even less so in any particular stock. Considering that our universe is the set of large, liquid firms comprising the S&P 500 Index, it is clear that we cannot link our results directly to prices, liquidity, etc.

On the other hand, it seems equally clear that the effects we identify at the local level should apply generally, that is, to national media outlets with audiences large enough to meaningfully impact capital allocation. Staring with Tetlock (2007) and Tetlock, Saar-Tsechansky, and Macskassy (2008), a number of papers have shown that news stories in national newspapers are associated with substantial price responses. Here, identification usually focuses on what kind of information a story conveys – for example, a firm's cash flows, risk, or sentiment. Less explored is the possibility that the story's very existence – a media effect – may generate a response independent of these channels. One could imagine, for example, decomposing the "news response coefficients" estimated in such studies into "media effects" and "content effects."

While we stop short of formally attempting such a decomposition here, we can say something about their relative sizes. Throughout our tests we include the earnings surprise (*SUE*) as a control in the regression. We find that extreme earnings surprises are related to the volume of retail trade. However, the media effect we identify is several times larger than this information effect no matter how we define our earnings surprise. Simply put, in our setting, the media is at least, and sometimes more, likely to drive trade than information. If these

generalize (even partially) to the aggregate level, they easily are capable of influencing prices and allocations.[16]

[16] For example, see Dougal, Engelberg, Garcia, and Parsons (2010).

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Table 1: Summary Statistics

Account and trading data are taken from the large discount brokerage database with demographic information in Barber and Odean (2000). Accounts in Area is the number of accounts within 100 kilometers of the city's local paper headquarters. Local stocks are stocks of firms within 100 kilometers of the city's local paper headquarters. A paper "covers" an earnings announcement in our sample if a story about that firm appears on day 0, 1 or 2 after the firm's earnings announcement in the ProQuest database. Earnings announcement dates are taken from COMPUSTAT and confirmed with I/B/E/S. Pr(covering a non-local EA) is the frequency in which that paper covered a non-local earnings announcement. Pr(covering a local EA) is the frequency in which that paper covered a local earnings announcement.

	Accounts				Paper	
	Households in Area	Avg # of stocks held	Local Stocks Held / Local Stocks Avail	Non-Local Stocks Held / Non-Local Stocks Avail	Pr(covering a local EA)	Pr(covering a non-local EA)
Atlanta (JOURNAL-CONSTITUTION)	398	2.35	0.135	0.035	0.716	0.019
Boston (GLOBE)	635	2.12	0.137	0.050	0.225	0.015
Denver (POST)	488	2.08	0.109	0.041	0.263	0.015
Detroit (NEWS)	317	2.63	0.130	0.036	0.317	0.006
Houston (CHRONICLE)	607	2.18	0.164	0.047	0.372	0.011
Las Vegas (REVIEW-JOURNAL)	167	2.20	0.087	0.018	0.282	0.010
Los Angeles (TIMES)	1913	2.13	0.218	0.112	0.279	0.047
Minneapolis (STAR-TRIBUNE)	445	2.08	0.218	0.036	0.305	0.012
New Orleans (TIMES-PICAYUNE)	134	2.41	0.086	0.016	0.657	0.006
New York (TIMES)	2808	2.28	0.211	0.152	0.300	0.154
Pittsburgh (POST-GAZETTE)	318	2.28	0.151	0.031	0.365	0.038
Sacramento (BEE)	500	2.20	0.128	0.077	0.011	0.002
San Antonio (EXPRESS NEWS)	142	2.31	0.091	0.016	0.401	0.035
San Diego (UNION-TRIBUNE)	517	2.14	0.161	0.047	0.500	0.029
San Francisco (CHRONICLE)	4076	2.17	0.444	0.151	0.260	0.008
Seattle (POST-INTELLIGENCER)	1019	2.11	0.301	0.060	0.461	0.013
St. Louis (POST-DISPATCH)	241	2.06	0.143	0.021	0.628	0.008
St. Petersburg (TIMES)	243	2.52	0.069	0.028	0.243	0.015
Washington D.C. (POST)	983	2.42	0.188	0.079	0.513	0.029
USA TODAY	--	--	--	--	0.023	0.041
WALL STREET JOURNAL	--	--	--	--	0.438	0.318

	Mean	Standard Deviation	1st Percentile	10th Percentile	Median	90th Percentile	99th Percentile
Market Capitalization (in millions)	5660	10676	62	466	2513	12825	54479
SUE	0.001	0.065	-0.120	-0.014	0.002	0.013	0.113
Stocks Held (per household)	2.263	2.406	1.000	1.000	1.000	4.000	12.00
Stocks Traded (per month per household)	1.75	1.78	1.00	1.00	1.00	3.00	9.00
Number of papers that cover EA	0.879	1.534	0.000	0.000	0.000	3.000	7.00
Local Media Coverage (Dummy)	0.028	0.166	0.000	0.000	0.000	0.000	1.000

Table 2: Media Effects and the Trading of Households

Every firm earnings announcement in our sample corresponds to nineteen distinct observations representing trading in each of nineteen major U.S. cities which we call "trading areas." The dependent variable is the natural logarithm of one plus the dollar trading volume in each trading area for the firm that makes the earnings announcement where local trading volume is available between January 1991 and November 1996 and is taken from the discount brokerage database in Barber and Odean (2000). Trading volume is considered on day 0, 1 or 2 following the earnings announcement date as identified by COMPUSTAT and I/B/E/S. *Local Media Coverage* takes the value one if the local newspaper (i.e. the newspaper that corresponds to the trading area) wrote a story about the firm on day +0, +1 or +2 following the earnings announcement. Controls include the natural logarithm of market capitalization (size), dummy variables for varying quintiles of earnings surprise (SUE quintiles), dummy variables for coverage in each of our local and national newspapers, dummy variables for each of the nineteen trading areas and date fixed effects. Robust standard errors clustered by firm are in parentheses. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

Dependent Variable: Log Dollar Trading Volume					
Local Media Coverage	0.746***	0.658***	0.648***	0.370***	0.477***
	(0.0830)	(0.0739)	(0.0739)	(0.0458)	(0.0593)
Firm Size		0.0700***	0.0761***	0.0506***	0.0577***
		(0.0103)	(0.0108)	(0.0100)	(0.0113)
SUE Quintile = Lowest			0.0758***	0.0381***	0.0526***
			(0.0158)	(0.0153)	(0.0171)
SUE Quintile = 2			0.0128	0.00223	0.00988
			(0.00950)	(0.0084)	(0.00921)
SUE Quintile = 4			0.0308**	0.00688	0.0235**
			(0.0119)	(0.0093)	(0.0107)
SUE Quintile = Highest			0.0845***	0.0502***	0.0661***
			(0.0185)	(0.0177)	(0.0197)
Coverage in Boston Globe				-0.235**	0.0305
				(0.101)	(0.0426)
Coverage in Denver Post				0.122	-0.00496
				(0.111)	(0.0501)
Coverage in Detroit News				-0.102	0.0814
				(0.0953)	(0.0641)
Coverage in Houston Chronicle				0.148	0.0257
				(0.0990)	(0.0431)
Coverage in Las Vegas Review-Journal				-0.122	-0.0671
				(0.111)	(0.0474)
Coverage in Los Angeles Times				0.0416	0.0618*
				(0.0974)	(0.0356)
Coverage in New York Times				-0.0618	0.0341**
				(0.0386)	(0.0154)
Coverage in Pittsburgh Post-Gazette				-0.00631	0.0891*
				(0.0860)	(0.0457)
Coverage in San Antonio Express News				0.00900	-0.0298
				(0.120)	(0.0419)
Coverage in San Diego Union Tribune				0.0384	0.0531
				(0.159)	(0.0612)
Coverage in San Francisco Chronicle				-0.420**	0.261***
				(0.208)	(0.0759)
Coverage in Seattle Post-Intelligencer				-0.107	0.168
				(0.376)	(0.187)
Coverage in St. Louis Post-Dispatch				0.00945	-0.0794**
				(0.0908)	(0.0381)
Coverage in St. Petersburg Times				-2.118***	0.0519
				(0.321)	(0.140)
Coverage in Minneapolis Star-Tribune				0.0453	0.0580
				(0.0788)	(0.0401)
Coverage in Atlanta Journal-Constitution				-0.188**	-0.0780**
				(0.0776)	(0.0353)
Coverage in Sacramento Bee				-0.166	-0.152
				(0.328)	(0.259)
Coverage in Washington Post				0.209**	0.0587**
				(0.0813)	(0.0288)
Coverage in New Orleans Times-Picayune				-0.251***	-0.104***
				(0.0850)	(0.0326)
Coverage in USA Today				0.0742	0.173***
				(0.120)	(0.0382)
Coverage in Wall Street Journal				-0.0733**	-0.00561
				(0.0347)	(0.0145)
Industry Fixed Effects	NO	NO	YES	YES	YES
City Fixed Effects	NO	NO	NO	NO	YES
Date Fixed Effects	YES	YES	YES	YES	YES
Observations	276982	276982	273999	265928	273999
Adjusted R^2	0.026	0.039	0.041	0.040	0.058

Table 3: Media Effects among Buys and Sells

Every firm earnings announcement in our sample corresponds to nineteen distinct observations representing trading in each of nineteen major U.S. cities which we call "trading areas." The dependent variable is the natural logarithm of one plus the dollar trading volume in each trading area for the firm that makes the earnings announcement where local trading volume is available between January 1991 and November 1996 and is taken from the discount brokerage database in Barber and Odean (2000). Trading volume is considered on day 0, 1 or 2 following the earnings announcement date as identified by COMPUSTAT and I/B/E/S. Local Covered takes the value one if the local newspaper (i.e. the newspaper that corresponds to the trading area) wrote a story about the firm on day 0, 1 or 2 following the earnings announcement. The first five columns consider only buy volume while the second five columns only consider sell volume. Controls include the natural logarithm of market capitalization (size), dummy variables for varying quintiles of earnings surprise (SUE quintiles), dummy variables for coverage in each of our local and national newspapers, dummy variables for each of the nineteen trading areas and date fixed effects. Robust standard errors clustered by firm are in parentheses. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	Dependent Variable: Log Dollar Trading Volume (Only Buys)					Dependent Variable: Log Dollar Trading Volume (Only Sells)				
Local Media Coverage	0.462***	0.408***	0.402***	0.339***	0.290***	0.381***	0.341***	0.335***	0.307***	0.260***
	(0.0588)	(0.0524)	(0.0521)	(0.0428)	(0.0406)	(0.0464)	(0.0426)	(0.0430)	(0.0391)	(0.0374)
Firm Size		0.0446***	0.0480***	0.0341***	0.0341***		0.0312***	0.0342***	0.0268***	0.0268***
		(0.00684)	(0.00714)	(0.00686)	(0.00686)		(0.00515)	(0.00540)	(0.00581)	(0.00581)
SUE Quintile = Lowest			0.0388***	0.0191*	0.0191*			0.0438***	0.0321***	0.0321***
			(0.0104)	(0.0107)	(0.0107)			(0.00864)	(0.00932)	(0.00932)
SUE Quintile = 2			0.000173	-0.00407	-0.00407			0.0150***	0.0130**	0.0130**
			(0.00716)	(0.00672)	(0.00672)			(0.00564)	(0.00561)	(0.00561)
SUE Quintile = 4			0.00898	0.00340	0.00340			0.0261***	0.0211***	0.0211***
			(0.00885)	(0.00786)	(0.00786)			(0.00690)	(0.00623)	(0.00623)
SUE Quintile = Highest			0.0457***	0.0309***	0.0309***			0.0469***	0.0386***	0.0386***
			(0.0120)	(0.0116)	(0.0116)			(0.0113)	(0.0123)	(0.0123)
Industry Fixed Effects	NO	NO	YES	YES	YES	NO	NO	YES	YES	YES
Paper Fixed Effects	NO	NO	NO	YES	YES	NO	NO	NO	YES	YES
City Fixed Effects	NO	NO	NO	NO	YES	NO	NO	NO	NO	YES
Date Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	276982	276982	273999	273999	273999	276982	276982	273999	273999	273999
Adjusted R2	0.022	0.031	0.032	0.041	0.048	0.015	0.021	0.022	0.026	0.034

Table 4: Media Effects and Determinants of Local Trading

Every firm earnings announcement in our sample corresponds to nineteen distinct observations representing trading in each of nineteen major U.S. cities which we call "trading areas." The dependent variable is the natural logarithm of one plus the dollar trading volume in each trading area for the firm that makes the earnings announcement where local trading volume is taken from the discount brokerage database in Barber and Odean (2000). Trading volume is considered on day 0, 1 or 2 following the earnings announcement date as identified by COMPUSTAT and I/B/E/S. Local Covered takes the value one if the local newspaper (i.e. the newspaper that corresponds to the trading area) wrote a story about the firm on day 0, 1 or 2 following the earnings announcement. Local Firm Dummy takes the value one if the firm is local to the trading market. Fraction of Accounts Holding is the fraction of accounts in the trading area holding the stock as of the first day of the month. Fraction of Accounts Trading is the fraction of accounts in the trading area that traded the stock in the prior month. Controls include the natural logarithm of market capitalization (size), dummy variables for varying quintiles of earnings surprise (SUE quintiles), dummy variables for coverage in each of our local and national newspapers, dummy variables for each of the nineteen trading areas and date fixed effects. Robust standard errors clustered by firm are in parentheses. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	Dependent Variable: Log Dollar Trading Volume				
	ONLY NON-LOCAL FIRMS				
Local Media Coverage	0.283***	0.327***	0.382***	0.454***	0.281***
	(0.0412)	(0.0527)	(0.0570)	(0.0569)	(0.0510)
Firm Size	0.0508***	0.0565***	0.0234***	0.0476***	0.0230***
	(0.00999)	(0.0112)	(0.00532)	(0.00922)	(0.00495)
SUE Quintile = Lowest	0.0384**	0.0479***	0.0283***	0.0401***	0.0255***
	(0.0154)	(0.0170)	(0.0101)	(0.0141)	(0.00940)
SUE Quintile = 2	0.00275	0.00740	0.00113	0.00532	0.000547
	(0.00838)	(0.00907)	(0.00789)	(0.00853)	(0.00772)
SUE Quintile = 4	0.00732	0.0212**	0.0231***	0.0197**	0.0212**
	(0.00934)	(0.0105)	(0.00873)	(0.00955)	(0.00839)
SUE Quintile = Highest	0.0509***	0.0622***	0.0474***	0.0568***	0.0449***
	(0.0178)	(0.0196)	(0.0124)	(0.0173)	(0.0119)
Local Firm Dummy		0.492***			0.336***
		(0.0598)			(0.0518)
Fraction of Accounts Holding			28.34***		23.16***
			(3.030)		(2.764)
Fraction of Accounts Trading				69.78***	50.79***
				(6.974)	(4.976)
Industry Fixed Effects	YES	YES	YES	YES	YES
Paper Fixed Effects	YES	YES	YES	YES	YES
City Fixed Effects	YES	YES	YES	YES	YES
Date Fixed Effects	YES	YES	YES	YES	YES
Observations	265928	273999	273999	273999	273999
Adjusted R2	0.049	0.068	0.081	0.073	0.089

Table 5: Media Effects, Newspaper Delivery and Local Weather

Every firm earnings announcement in our sample corresponds to nineteen distinct observations representing trading in each of nineteen major U.S. cities which we call "trading areas." The dependent variable is the natural logarithm of one plus the dollar trading volume in each trading area for the firm that makes the earnings announcement and is taken from the discount brokerage database in Barber and Odean (2000). Local Covered takes the value one if the local newspaper (i.e. the newspaper that corresponds to the trading area) wrote a story about the firm on day 0, 1 or 2 following the earnings announcement date as identified by COMPUSTAT and I/B/E/S. Weather data are taken from the National Climatic Data Center at ftp://ftp.ncdc.noaa.gov/pub/data/gsod. For each trading area, we use weather from the station closest to the local newspaper's zip code. Local Hail is a dummy variable that takes the value one if there was hail during the two days after the earnings announcement. Local Snow is a dummy variable that takes the value one if there was at least 8 inches of new snowfall during the two days after the earnings announcement. Extreme Weather is a dummy variable takes the value of one if either Local Hail or Local Snow take the value of one. Controls include the natural logarithm of market capitalization (size), dummy variables for varying quintiles of earnings surprise (SUE quintiles), dummy variables for coverage in each of our local and national newspapers, dummy variables for each of the nineteen trading areas and date fixed effects. Robust standard errors clustered by firm are in parentheses. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	Dependent Variable: Log Dollar Trading Volume		
Local Media Coverage	0.478***	0.478***	0.479***
	(0.0594)	(0.0593)	(0.0593)
Firm Size	0.0459***	0.0565***	0.0565***
	(0.0104)	(0.0113)	(0.0113)
SUE Quintile = Lowest	0.0381**	0.0484***	0.0484***
	(0.0157)	(0.0170)	(0.0170)
SUE Quintile = 2	-0.00504	0.00754	0.00752
	(0.00913)	(0.00908)	(0.00908)
SUE Quintile = 4	0.0193*	0.0216**	0.0216**
	(0.0114)	(0.0105)	(0.0105)
SUE Quintile = Highest	0.0522***	0.0633***	0.0632***
	(0.0186)	(0.0197)	(0.0197)
Local Hail	0.0998		
	(0.0745)		
Local Hail * Local Media Coverage	-0.922***		
	(0.129)		
Local Snow		0.0486	
		(0.0352)	
Local Snow * Local Media Coverage		-0.774***	
		(0.135)	
Local Extreme Weather			0.0641*
			(0.0344)
Local Extreme Weather * Local Media Coverage			-0.790***
			(0.105)
Industry Fixed Effects	YES	YES	YES
Paper Fixed Effects	YES	YES	YES
City Fixed Effects	YES	YES	YES
Date Fixed Effects	YES	YES	YES
Observations	273999	273999	273999
Adjusted R2	0.051	0.063	0.063

Table 6: Media Effects and Alternative Definitions of Household Trading

Every firm earnings announcement in our sample corresponds to nineteen distinct observations representing trading in each of nineteen major U.S. cities which we call "trading areas." The dependent variable is some measure of trading volume in each trading area for the firm that makes the earnings announcement where local trading volume is available between January 1991 and November 1996 and is taken from the discount brokerage database in Barber and Odean (2000). Trading volume is considered on day 0, 1 or 2 following the earnings announcement date as identified by COMPUSTAT and I/B/E/S. Local Covered takes the value one if the local newspaper (i.e. the newspaper that corresponds to the trading area) wrote a story about the firm on day 0, 1 or 2 following the earnings announcement. In the first three columns, the dependent variable is a dummy variable that takes the value one if there was any trading in the firm making the earnings announcement. In the second three columns, the dependent variable is the natural logarithm of one plus the number of accounts that traded in the firm making the earnings announcement. Controls include the natural logarithm of market capitalization (size), dummy variables for varying quintiles of earnings surprise (SUE quintiles), dummy variables for coverage in each of our local and national newspapers, dummy variables for each of the nineteen trading areas and date fixed effects. Robust standard errors clustered by firm are in parentheses. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	Dependent Variable: Log Number of Accounts Traded			Dependent Variable: Local Trading Dummy		
	ALL TRADES	BUYS ONLY	SELLS ONLY	ALL TRADES	BUYS ONLY	SELLS ONLY
Local Media Coverage	0.0467***	0.0272***	0.0230***	0.0501***	0.0311***	0.0277***
	(0.00673)	(0.00427)	(0.00369)	(0.00591)	(0.00421)	(0.00389)
Firm Size	0.00487***	0.00281***	0.00224***	0.00604***	0.00368***	0.00286***
	(0.00103)	(0.000576)	(0.000531)	(0.00118)	(0.000729)	(0.000613)
SUE Quintile = Lowest	0.00422***	0.00153*	0.00279***	0.00556***	0.00226*	0.00363***
	(0.00156)	(0.000926)	(0.000832)	(0.00182)	(0.00116)	(0.00101)
SUE Quintile = 2	0.000766	-0.000240	0.00107**	0.000937	-0.000387	0.00148**
	(0.000800)	(0.000588)	(0.000457)	(0.000993)	(0.000738)	(0.000617)
SUE Quintile = 4	0.00203**	0.000382	0.00179***	0.00220**	0.000269	0.00224***
	(0.000965)	(0.000735)	(0.000512)	(0.00110)	(0.000833)	(0.000660)
SUE Quintile = Highest	0.00544***	0.00241**	0.00327***	0.00669***	0.00327***	0.00411***
	(0.00183)	(0.00102)	(0.00111)	(0.00205)	(0.00123)	(0.00130)
Industry Fixed Effects	YES	YES	YES	YES	YES	YES
Paper Fixed Effects	YES	YES	YES	YES	YES	YES
City Fixed Effects	YES	YES	YES	YES	YES	YES
Date Fixed Effects	YES	YES	YES	YES	YES	YES
Observations	273999	273999	273999	273999	273999	273999
Adjusted R2	0.070	0.054	0.035	0.060	0.047	0.033

Table 7: Media Effects and Robustness

Every firm earnings announcement in our sample corresponds to nineteen distinct observations representing trading in each of nineteen major U.S. cities which we call "trading areas." The dependent variable is the natural logarithm of one plus the dollar trading volume in each trading area for the firm that makes the earnings announcement where local trading volume is available between January 1991 and November 1996 and is taken from the discount brokerage database in Barber and Odean (2000). Controls include Firm-Date, Firm-City and City-Date fixed effects. Robust standard errors clustered by firm are in parentheses. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	Dependent Variable: Log Dollar Trading Volume			
Local Coverage	0.477***	0.0755**	0.473***	0.0796***
	(0.0587)	(0.0295)	(0.0535)	(0.0281)
Firm-Date Fixed Effects	YES	NO	NO	YES
Firm-City Fixed Effects	NO	YES	NO	YES
City-Date Fixed Effects	NO	NO	YES	YES
Observations	273999	273999	273999	273999
Adjusted R2	0.093	0.164	0.106	0.289

Table 8: Media Effects, the Timing of Stories and Falsification

Every firm earnings announcement in our sample corresponds to nineteen distinct observations representing trading in each of nineteen major U.S. cities which we call "trading areas." The dependent variable is the natural logarithm of one plus the dollar trading volume in each trading area for the firm that makes the earnings announcement where local trading volume is available between January 1991 and November 1996 and is taken from the discount brokerage database in Barber and Odean (2000). In column 1 (2, 3)only trading volume on day 0 (1, 2) following the earnings announcement is considered. Local Coverage on Day 0 (1, 2) takes the value 1 if there was a news article in the local newspaper on day 0 (1, 2). Local Coverage on Non-Matched Day takes the value 1 if there was a news article on a different day than the trading day (1 day after or 2 days after). Controls include Firm-Date, Firm-City and City-Date fixed effects. Robust standard errors clustered by firm are in parentheses. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	Dependent Variable: Log Dollar Trading Volume		
	On Day 0	On Day 1	On Day 2
Local Coverage on Day 0	0.0915**	-0.0154	-0.00914
	(0.0355)	(0.0282)	(0.0240)
Local Coverage on Day 1	0.0112	0.0688**	0.0111
	(0.0316)	(0.0287)	(0.0193)
Local Coverage on Day 2	0.0161	0.00941	0.0643
	(0.0395)	(0.0551)	(0.0408)
Firm-Date Fixed Effects	YES	YES	YES
Firm-City Fixed Effects	YES	YES	YES
City-Date Fixed Effects	YES	YES	YES
Observations	273999	273999	273999
Adjusted R2	0.269	0.264	0.222