What Determines Word-of-Mouth Effects in Financial Markets?

This Draft: October 2019*

We study what determines the strength of word-of-mouth effects in financial markets. We identify a set of events that – for plausibly exogenous reasons – changes the information set for certain investors and causes such "treated investors" to trade. We infer the strength of word-of-mouth effects from the degree to which abnormal trading activity spills over from the treated investors to their neighbors and their neighbors' neighbors. Our results suggest that word-of-mouth effects are strongest when financial markets are relatively calm, when investor sentiment is high, when there are few extraneous news events, when the information transmitted through word-of-mouth is positive and when the investors communicating with one another have similar backgrounds.

JEL Classification: G11, G12, G14, G20.

Keywords: Social Interaction, Investor Communication, Information Diffusion.

*Huang is affiliated with the Faculty of Business and Economics, The University of Hong Kong, Hong Kong. Hwang is affiliated with the Cornell SC Johnson College of Business, Cornell University, Warren Hall 310E, Ithaca, NY 14853, USA. Lou's primary affiliation is with the London School of Economics, London, UK; Lou is also affiliated with the CEPR. E-mail: huangsy@hku.hk, bhwang@cornell.edu, and d.lou@lse.ac.uk. The authors would like to thank Snehal Banerjee, Nick Barberis, Justin Birru, John Campbell, James Choi, Daniel Ferreira, Bing Han, David Hirshleifer, Dirk Jenter, Matti Keloharju, Ralph Koijen, Ulrike Malmendier, Daniel Paravisini, Lasse Pedersen, Ville Rantala, Scott Yonker, Qiping Xu, and seminar participants at University of Amsterdam, the Chinese University of Hong Kong (Shatin), the Chinese University of Hong Kong (Shenzhen), Copenhagen Business School, Cornell University, University of Geneva, University of Hong Kong, Hong Kong University of Science and Technology, Imperial College Business School, London School of Economics, National University of Singapore, Purdue University, Renmin University Business School, Rotterdam School of Management, Shanghai Advanced Institute of Finance, Singapore Management University, Tilburg University, Toulouse School of Economics, University of London Birkbeck, Zhejiang University, and participants at the London Empirical Asset Pricing Meeting, China International Conference in Finance, Helsinki Finance Summit, Miami Behavioral Finance Conference, and American Finance Association Annual Meeting for helpful comments and suggestions. We are grateful for funding from the Paul Woolley Center at the London School of Economics.

1. Introduction

Behavioral finance has become an oft-proposed alternative to the traditional framework in finance (Hirshleifer 2001; Barberis and Thaler 2003). In essence, behavioral finance asks whether the behavior of investors and financial markets can be better described using models that are "psychologically more realistic" compared with those relied upon in the traditional framework.

At this point, it appears safe to conclude that behavioral finance has been helpful in accounting for basic facts about the trading behavior of investors, the cross-section of average stock returns, and the aggregate stock market (Barberis and Thaler 2003). Behavioral finance has also shown that it can make concrete, out-of-sample predictions, some of which have already been confirmed in the data (Barberis, 2018).

Despite the many successes that behavioral finance can claim, some have advocated "a need to move from behavioral finance to *social finance*" (Hirshleifer 2015, p. 151). The argument is that the major biases that behavioral finance incorporates to make their models psychologically more realistic represent biases at the individual-person level (e.g., extrapolative beliefs, prospect-theory preferences). Investors do not operate in a vacuum, however, and it appears plausible that biases exist not only at the individual-person level, but also in the manner in which individuals communicate with one another and that such "biases in social interactions" may prove to be highly useful in explaining the behavior of financial markets (Hirshleifer 2015). For instance, Han, Hirshleifer and Walden (2018) develop a model in which information transmission among investors is biased in the sense that "investors like to recount to others their investment victories more than their defeats, and that listeners do not fully discount for this" (p. 3). The authors show that this can lead investors to flock to active investment funds even though they tend to underperform passively managed funds.[1]

[1] In a related model (Han, Hirshleifer and Walden 2019), individuals infer the state of the economy through social interaction, but social learning is biased by the fact that consumption is more salient than non-consumption. For instance, a friend wearing an expensive watch is more noticeable than a friend not wearing an expensive watch. Such bias can lead individuals to significantly overestimate others' consumption, increase their own actual consumption and under-save.

The goal of this paper is to provide guidance regarding the conditions under which investors draw more heavily on word of mouth, making biases in social interactions potentially more important, and to contrast them to the states in which social interactions between and among investors can be disregarded. While prior work has provided evidence that word-of-mouth effects are present in financial markets,[2] we know relatively little about the conditions that make investors rely more or less on word of mouth.[3] In this paper, we provide such basic evidence and list some of the key determinants of the strength of word-of-mouth effects in financial markets.

The ideal experiment to speak to our research question would be to randomly seed investment ideas among investors and track under what conditions such investment ideas diffuse more widely. Our empirical design draws inspiration from this ideal. In particular, we look to the trading behavior of investors around cross-industry stock-financed mergers and acquisitions (M&As). We exploit the fact that, at the completion of a cross-industry stock-financed M&A, investors in the target firm – residing in some industry *x* – receive shares of the acquirer firm – residing in some industry *y*. We conjecture that the endowment of shares from the acquirer industry leads some target investors to collect information and form opinions about the acquirer industry and to start trading on firms from that industry aside from the acquirer firm itself.

If target investors communicate their newly gained industry perspectives to other investors in the same neighborhood, we may observe abnormal trading activity in the acquirer industry not only by target investors, but also by their neighbors and their neighbors' neighbors. Tracing out the "contagion" of abnormal trading activity in the acquirer industry then enables us to quantify the rate at which financial information spreads through word-of-mouth communication. Because in our setting we can point to the event that triggered word-of-mouth communication as well as the person spreading such information, we can quantify how the degree of information diffusion varies with characteristics of the trigger, conditions

[2] For instance, Shiller and Pound (1989); Hong, Kubik and Stein (2004); Brown, Ivković, Smith and Weisbenner (2008); Hvide and Östberg (2015).

[3] As we discuss later, prior research has identified a couple of determinants of the effect of word of mouth communication on investor behavior. For example, Heimer and Simon (2017) find evidence that investors are more likely to discuss investment ideas when they have done well in the past. Ivković and Weisbenner (2007) and Brown, Ivković, Smith and Weisbenner (2008) find that word of mouth among investors is stronger in more sociable states.

of the environment, and, to draw an analogy to epidemiology, characteristics of the "infectives" and "susceptibles."

To implement our empirical tests, we exploit detailed trading records of 78,000 US households from a discount brokerage from 1991 through 1996 and we collect data on all cross-industry M&As that occurred during that same time interval. We separate cross-industry M&As into those that are stock-financed and those that are cash-financed: we define the former as deals that are at least partially equity-financed; the latter comprise 100% cash-financed deals. In cash-financed M&As, target investors receive cash as opposed to shares in an acquirer firm and, as such, are less incentivized to study the corresponding acquirer industry. Cash-financed M&As thus serve as our placebo.

In the first part of our empirical analysis, we conduct a simple difference-in-differences analysis to see how much more target investors trade in the acquirer industry in the post-M&A period compared with the remaining investor population while controlling for a host of investor- and investor-zip-code-level characteristics. We also include M&A fixed effects to absorb any M&A-specific effects. Since target investors are bound to sell their holdings in the acquirer firm, we exclude the acquirer firm itself from our calculation. We further exclude investors in states where target and acquirer firms have any business operations. We repeat the above difference-in-differences analysis for non-target investors who live within three miles of a target investor ("target neighbors").

Our results reveal that in the year following the completion of a cross-industry stock-financed M&A, target investors, compared with other investors, more than double the number of trades they execute in the corresponding acquirer industry. This abnormal trading activity in the acquirer industry dies out within 18 months.

Consistent with the presence of word of mouth, we find that target neighbors also trade substantially more actively in the acquirer industry than investors who do not living within three miles of a target investor. Target investors and target neighbors tend to trade in the same direction; that is, if a target investor is buying in the acquirer industry, so are her neighbors. Consistent with word of mouth playing a role in generating the above pattern, our effect becomes statistically and economically weaker the further away investors reside from a target investor.

In a series of placebo tests to help rule out alternative interpretations, we find that – for both target investors and their neighbors – our documented effect disappears when we consider cash-financed M&As. Our effect also turns substantial in size only after target investors are eventually endowed shares of the acquirer firm (as opposed to when the M&A is announced, which is when media coverage of M&As typically peaks).

The remainder of the paper takes the size of abnormal trading activity in the acquirer industry undertaken by target neighbors as a measure of the strength of word-of-mouth effects in financial markets and asks what factors determine such "size of contagion."

We begin with two determinants already broached by prior literature. Heimer and Simon (2017) find evidence that investors are more likely to discuss investment ideas when their own portfolios have done well in the past. We make similar observations in our setting. The size of contagion, that is, the size of abnormal trading activity in the acquirer industry undertaken by target neighbors, is substantially larger when the corresponding target investors have experienced high recent portfolio returns. Also in line with prior work (Brown, Ivković, Smith and Weisbenner 2008), we find that the size of contagion is larger if target investors and target neighbors reside in more sociable states as captured by the DDB Life Style survey data.

We believe the remaining determinants are new to the literature. Broadly defined, our determinants fall into one of three groups: (a) conditions of the environment, (b) characteristics of the trigger, and (c) investor characteristics.

We start with two intuitive determinants at the environment level. We argue that in times of high uncertainty about the overall stock market and low investor sentiment, investors are wary of new investment ideas and thus less likely to act on such ideas. This makes it hard for any new investment idea to propagate across investors. Consistent with such conjecture, we find that the size of contagion is significantly lower when the CBOE's Volatility Index (VIX) is relatively high and when measures of investor sentiment produce low realizations.

Moreover, Hirshleifer, Lim and Teoh (2009) note that since attention is finite, investors can focus only on a small subset of signals to the exclusion of others. Applying this notion to our setting, we hypothesize that investors are less likely to discuss investment ideas if there are important distractions. To test this idea, we analyze the trading behavior of investors, who, at the time of the M&A event, reside in an area with a local National Football League (NFL) team playing in the playoffs or in an area with a local weather-related emergency, such as a blizzard, tornado, or wildfire. Consistent with our idea, we find that the size of contagion drops to virtually zero if investors face important non-financial-market-related distractions.

Our next class of determinants captures characteristics of the trigger of word-of-mouth communication. A key assumption in the model of Han, Hirshleifer and Walden (2018) is that individuals are more likely to share a story when such story is positive and salient. We do not directly observe the stories shared between and among investors. However, extending the argument of Han, Hirshleifer and Walden, we speculate that a story is more likely to be shared if the trigger for such story, here, the cross-industry stock-financed M&A event, is a more positive and salient experience for the target investor. We approximate positivity by the corresponding target firm's M&A-announcement-day returns and by whether the acquisition represents a friendly deal as opposed to a hostile takeover. We approximate saliency by the weight of a target firm in the target investor's portfolio. In line with expectations, we find that the size of contagion is higher when the target firm experiences higher M&A announcement-day returns, when the acquisition represents a friendly deal, and when the target firm accounts for a larger portion in the target investor's portfolio.

Our third class of determinants captures characteristics of the investors communicating with one another. For reasons detailed in Section 4.2.4, we adopt a dynamic research design to account for indirect interactions among investors. We consider three social characteristics on which we have data: income, age, and gender. We find that investment ideas spread more easily when there are fewer differences in age, gender, or income between senders and receivers of information. Specifically, our results suggest that a ten-year age gap between an investor pair, being of different gender, and a one-step difference in income lowers the contagion rate by 12%, 17%, and 2%, respectively. That is, differences in age and gender represent much higher barriers to communication than differences in socio-economic background. We also find evidence of strong asymmetries. In particular, younger investors are more likely to act on older investors' views than the other way around. Relatedly, the average contagion rate from female to male investor is higher than that from male to female investor. Further, lower-income investors are more likely to act on higher-income investors' views than the other way around.

In summary, our paper provides relatively clean evidence that there are word-of-mouth effects in financial markets. Crucially, there is substantial variation in the degree to which investors rely on word of mouth. We find that the size of contagion is large when financial markets are relatively calm and investor sentiment is high, but that word-of-mouth effects are weak when uncertainty about the overall market is high and investor sentiment is low. The size of contagion is substantial when the trigger for word-of-mouth communication is positive and salient and when the initial idea is seeded within a relatively homogeneous investor population. The size of contagion becomes negligible when there are important non-financial-market-related distractions and when the trigger for word-of-mouth communication is negative and inconspicuous.

2. Literature Review

Our paper builds on two streams of research: the finance literature providing evidence for the presence of word-of-mouth effects in financial markets and the word-of-mouth literature in psychology, marketing and economics.

2.1 Word-of-Mouth Effects in Financial Markets

Shiller and Pound (1989) are perhaps the first to consider word-of-mouth effects in financial markets. Shiller and Pound conduct surveys of both retail investors and institutional investors and they conclude that, in general, investors do not derive investment ideas by themselves. Rather, they are drawn to stocks through word-of-mouth communication.

Evidence in subsequent work supports the idea that word of mouth is both frequent and important in financial markets. Hong, Kubik and Stein (2005) find that a fund manager's purchases of a stock increase by roughly 0.13 percentage points when other managers from different fund families in the same city increase their purchases of the same stock by 1 percentage point. Ivković and Weisbenner (2007) find that the above positive correlation in trading behavior between neighbors extends to retail investors. Hvide and Östberg (2015) utilize micro data, which allow them to identify coworkers at the plant level, and they find that a one-standard-deviation increase in the fraction of coworkers who make a stock purchase in a given month is associated with a 41% increase in the probability of a worker making a stock purchase him-/herself.

2.2 Word-of-Mouth Research in Psychology, Marketing and Economics

Word of mouth has also been examined by researchers in psychology, marketing and economics. Economists primarily study word of mouth from a social network perspective and conclude that "the full network of relationships—how dense it is, whether some groups are segregated, who sits in central positions—affects how information spreads and how people behave" (Jackson 2014, p. 3-4).

Our work relates perhaps more closely to the word-of-mouth literature in psychology and marketing. Similar to the finance literature, this literature points to the relevance of word of mouth. Crucially, this literature notes that there is substantial variation in the strength of word-of-mouth effects and researchers in psychology and marketing have exerted significant efforts to better understand the intervening behavioral processes and environmental conditions that explain such variation (e.g., Fast, Heath and Wu 2009; Berger and Milkman 2012; Lovett, Peres and Shachar 2013; Berger 2014; 2016).

We view our study as an extension of such work to financial markets. While the corresponding finance literature has provided evidence that investors communicate with one another, to the best of our knowledge, our study is the first systematic analysis, which lays out the conditions under which investors rely strongly on word of mouth and contrasts them to the states in which investor interactions can be disregarded. We believe such analysis is critical in understanding when social finance may play an important role in financial markets and when the consideration of social interactions adds little explanatory power.

3. Data

3.1 Data Sources and Descriptive Statistics

We obtain detailed investor-trading records for a subsample of US households for the 1991–1996 period from a discount brokerage firm. These are the same records used by Odean (1998) and Barber and Odean (2001), among others.[4] The brokerage database contains zip code information, which enables us to compute the distance between two investors using the longitude and latitude associated with each zip code, adjusted for curvature.[5] We augment our data with information from the US Census Bureau's zip code database, which, among others, includes the population and average household income for each zip code.

We match our investor-trading records to all M&As that take place from 1991 through 1996. Our data sources are the Security Data Corporation (SDC) and the Center for Research in Security Prices (CRSP) delisting file. We require that the acquirer and target firms reside in separate industries. Industries are defined based on the Fama-French 49-industry classification. Using alternative industry classifications, such as the Fama-French 38- or 30-industry classifications or the Global Industry Classification Standard, does not change the main results of the paper (results available upon request). We exclude M&As for which we cannot identify the acquirer's or the target's industry. We separate M&A deals into those that are stock-financed and those that are cash-financed: we define the former as deals that are at least partially equity-financed; the latter are 100% cash-financed.

[4] In our data, one household can have multiple accounts. We conduct our analyses at the household level; that is, we aggregate all accounts held by the same household into one observation. Henceforth, we use the terms "households" and "investors" interchangeably.

[5] The formula is: distance = $\arccos(\cos(a_1)\cos(a_2)\cos(b_1)\cos(b_2) + \cos(a_1)\sin(a_2)\cos(b_1)\sin(b_2) + \sin(a_1)\sin(b_1)) * 3963$, where a_1 and b_1 (a_2 and b_2) are the latitudes (longitudes) of the two zip codes and 3,963 miles is the Earth's radius.

Our final sample contains 460 M&As executed from 1991 through 1996, of which 317 are stock-financed and 143 are cash-financed. In Panel A of Table 1, we report summary statistics for these M&A deals. For stock-financed M&As, the median acquirer-market capitalization is $951 million and the median target-market capitalization is $74 million. For cash-financed M&As, the median acquirer-market capitalization is $1,561 million and the median target-market capitalization is $93 million.

We end up with a sample of around 70,000 investor accounts. As can be seen from Figure 1, which shows a heat map of the number of investors in each state, the investors in our sample are disproportionately clustered on the East Coast and the West Coast. In Panel B of Table 1, we provide summary characteristics for these accounts. The median and mean portfolio sizes are $13,141 and $41,030, respectively. The average investor holds 3.88 stocks in her portfolio and places 0.47 trades a month, with an average monthly trade value of $5,679. The average investor age in our sample is 42 and the average annual household income is $69,500. Panel C provides summary statistics of households (residents) in each zipcode.

3.2 Benefits and Shortcomings of Our Sample

The perhaps most appealing feature of our data is that our trading records are highly detailed and the median retail investor in our sample holds (only) three stocks. As a result, substituting any one stock position with another stock from a different industry is likely to have a significant effect on investor attention.

Our data are also subject to several caveats, however. First, the set of retail investors in our sample is not randomly drawn as, by construction, they are all clients of the same discount brokerage firm. To the extent that having a common broker is an indication of belonging to the same social network, our sample of households is likely to be better connected to one another than the average US household. This introduces an upward bias in our baseline contagion size estimate.

Second, the landscape of the US equity market has changed dramatically over the past three decades. In particular, the fraction of shares held directly by retail investors has decreased steadily. This raises the question of whether we can extrapolate our results to today’s marketplace. On a related note, one may argue that with the advent of modern communication technologies, the size of contagion is an order of magnitude higher today than it was during our sample period.

We acknowledge these concerns. At the same time, we note that there is research, which tracks individuals' within-day activities and finds evidence that, even in recent years, a mere seven percent of word of mouth happens online (Berger 2016). Moreover, most of our analysis focuses on what determines the size of contagion. To the extent that there are inherent, persistent behavioral components in social structures and norms (e.g., the tendency to interact with people of similar age), the above caveats are less of a concern and the determinants that we find in this study are likely to generalize to other investor groups as well as across time.

4. Research Design and Main Results

One major challenge facing empirical, non-experimental research on word of mouth is what Manski (1993) refers to as the "reflection problem." Much prior work infers word of mouth from correlated trading patterns between investors residing in the same locale. Yet, if two investors in the same locale exhibit correlated trading patterns, how can we be certain that they actually communicate with one another rather than simply exhibit similar backgrounds/tastes/preferences and/or access to the same local information?

Some studies address the reflection problem through natural experiments, which generate random assignments of individuals to classes or cohorts (e.g., Duflo and Saez 2002, 2003; Shue 2013). Others follow a regression discontinuity approach (e.g., Anderson and Magruder 2012; Luca 2016). Still, others conduct field studies. For instance, Banerjee, Chandrasekhar, Duflo, and Jackson (2017) seed the following information in three subsets of rural villages in India: "*anyone who calls a particular phone number will have a chance to win a free cell phone, and if they do not win the phone, they are guaranteed to win some cash*." In the first subset, the information is seeded with randomly selected individuals. In the second subset, the information is seeded with "village elders." In the third subset, the information is seeded with individuals nominated by villagers as the "best gossipers." Banerjee et al. analyze which setting generates the highest information diffusion rate by counting the number of phone calls made.

Our empirical design draws inspiration from that of Banerjee, Chandrasekhar, Duflo, and Jackson (2017). Rather than seed a raffle, our setting seeds attention to an industry among US retail investors. M&As are plausibly exogenous to the backgrounds/tastes/preferences of retail investors. Rather than count the number of phone calls made, we check for abnormal trading activity in the acquirer industry among target neighbors. Rather than compare the extent to which information spreads when information is seeded with random individuals versus with elders, we compare, among others, the extent to which information spreads when a piece of information is seeded among investors who happen to face a local weather-related emergency versus investors who do not. In overall structure, however, we consider our research design to be similar to that of Banerjee et al. and, as a result, we believe we can draw appropriate causal inferences.

4.1 The Size of Contagion

Our empirical design assumes that the endowment of acquirer shares draws target investors' attention to the corresponding acquirer industry; elevated attention, in turn, increases trading activity (Barber and Odean 2007). To test the validity of this assumption, we estimate the following regression equation:

$$Trading\ Act_{i,m} = a_m + \beta_1\, Target\ Investor_{i,m} + CONTROL * \gamma + \varepsilon_{i,j,t}, \quad (1)$$

where *Trading Act*$_{i,m}$ is the number of trades by investor i in the acquirer industry after cross-industry stock-financed M&A *m* as a fraction of her total number of trades across all industries. We also compute *Trading Act* based on the dollar value of trades. Since the exact completion date is missing for many M&As, we examine total trading behavior in months 7 through 18 after the M&A is announced as, on average, it takes six months for an M&A to complete (Giglio and Shue 2014).

Since target investors are bound to sell their holdings in the acquirer firm sooner or later, we exclude the acquirer firm when calculating trading activity in the acquirer industry to avoid any "mechanical" effect. To exclude dormant accounts, we require that investors place at least one trade in either the one-year period prior to the M&A or the one-year period following the M&A. We exclude households in states where the target or acquirer firm has any business operations—identified using both headquarters and factory locations.[6]

We further require that these households have no trading/holdings in the acquirer industry in the year prior to the M&A announcement. We do so for two reasons. First, target investors that have prior holdings in the acquirer industry may "mechanically" sell their existing holdings upon receiving acquirer shares to reduce their overall exposure to the acquirer industry. Second, we conjecture that target investors with no prior trading/holdings in the acquirer industry are more likely to be "shocked/treated" by the endowment of shares in the acquirer industry.

The main independent variable in our regression equation is *Target Investor*$_{i,m}$, which equals one if investor i holds shares in the target firm in the month prior to the M&A announcement, and zero otherwise. Since we require all investors to have no stock holdings/trading in the acquirer industry prior to the M&A, our analysis is essentially a difference-in-differences analysis and the coefficient estimate for *Target Investor*$_{i,m}$ informs us how much more target investors trade in the acquirer industry in the post-M&A period relative to the pre-M&A period, compared with the remaining investor population over the same time frame.[7]

Our control variables fall into one of two groups: (a) investor characteristics and (b) zip code characteristics. The former includes household income, number of children, number of family members, age, gender, and marital status. The latter include zip code population, fraction of male residents, average home value, average number of household members, and average household income. In our full specification, we also include M&A fixed effects to absorb any M&A-specific effects. The standard errors are double clustered at the zip code level and the year-month of the M&A announcement level.

[6] We thank Alok Kumar for sharing his data on firm headquarters and factory locations.

[7] In other words, instead of regression equation (1), we could include observations in the one-year period prior to an M&A announcement and the one-year following M&A completion and estimate a regression of trading activity in the acquirer industry on a target investor indicator and a post-M&A indicator as well as an interaction term between the two indicator variables, along with other controls and fixed effects. The estimate for the interaction term will be identical to our estimate for *Target Investor*$_{i,m}$ in regression equation (1).

The regression results are reported in Panel A of Table 2. The dependent variable in the first three columns is based on the number of trades, while that in the next three columns is based on the dollar value of trades. Column (1), which reports results when controlling for investor and zip code characteristics, shows that target investors increase their trading activity in the acquirer industry by an incremental 2.55% compared with other investors (*t*-statistic = 5.54). To put this number in perspective, the unconditional trading activity in any industry is 2.04% (=100%/49). That is, the endowment of acquirer stocks induces target investors to more than double their normal trading activities in the average industry. As can be seen in Columns (2) and (3), including M&A fixed effects has virtually no impact on our results. The regression coefficients reported in Columns (4)–(6), which are based on the dollar value of trades, are almost identical to those reported in Columns (1)–(3).[8]

Having provided evidence on the validity of our assumption that the endowment of acquirer shares induces at least some target investors to pay greater attention to the acquirer industry and to trade more actively in the acquirer industry, we now estimate the baseline size of contagion through the post-M&A trading behavior in the acquirer industry on the part of target investors' neighbors. We use a narrow definition of "neighbors" – investors who live within a three-mile radius – as we presume that the likelihood of two individuals coming into direct contact with each other rapidly diminishes with distance. We impose the same data requirements as for target investors, that is, we exclude the acquirer firm when calculating trading activity in the acquirer industry, we exclude dormant accounts, we exclude investors in states where the target or acquirer firm has any business operations, and we require that investors have no holdings/trading in the acquirer industry in the prior year. In doing so, we can directly compare the regression coefficients across the two settings.

We estimate a regression equation similar to equation (1):

[8] One potential concern with our target-investor result is that holdings in the target firm are not random. In particular, some investors may build exposure to the acquirer industry by, indirectly, purchasing shares of the target firm in anticipation of the M&A. To assess the relevance of this channel, we define target investors using lagged holdings information. In Online Appendix Table A1, *Target Investor* now take the value of one if an investor holds the target sock one year prior to the M&A announcement. *Target Neighbor* takes the value of one if an investor lives within three miles of such a target investor. It is implausible that retail investors are able to forecast M&As one year in advance. Yet, we find that all of our main results hold under this alternative specification.

$$Trading\ Act_{i,m} = a_m + \beta_1\ Target\ Neighbor_{i,m} + CONTROL * \gamma + \varepsilon_{i,j,t}, \quad (2)$$

where *Target Neighbor*$_{i,m}$ is an indicator variable that takes the value of one if investor *i* lives within three miles of a target investor and is not a target investor herself.[9] If an investor lives within three miles of more than one target investor, we count them only once.[10] The coefficient estimate for *Target Neighbor*$_{i,m}$ informs us how much more investors residing within three miles of a target investor increase their trading in the acquirer industry in the post-M&A period relative to the pre-M&A period, compared with all other investors in our sample over the same time frame. We exclude target investors from our sample when estimating regression equation (2) to ensure that target investors do not enter our counterfactuals.

The results are reported in Panel B of Table 2. When controlling for investor and zip code characteristics, we find that the number of trades increases by 46bps more for investors who live within three miles of a target investor (*t*-statistic = 6.57) than for investors who do not live within three miles of a target investor. After adding M&A fixed effects, the coefficient estimate for *Target Neighbor*$_{i,m}$ turns to 22bps (*t*-statistic = 3.14). Given that the unconditional trading activity in a given industry is 2.04%, our results imply that target neighbors increase their trading activity by more than 10% relative to the unconditional trading activity in any industry. The results based on the dollar value of trades are very similar. For example, the coefficient estimate for *Target Neighbor*$_{i,m}$ in the full specification is now 21bps (*t*-statistic = 3.05).[11]

4.1.1 Extensive vs. Intensive Margins

Comparing the results reported in Panel A of Table 2 with those reported in Panel B, we observe that the effect of stock-financed M&As on target investors' trading activity is about ten times larger than that on target neighbors' trading activity (2.34% vs. 22bps). To better understand the difference in trading behavior

[9] In additional tests, we replace our main independent variable with the number of target investors who live within three miles of an investor. The results are virtually unchanged (Online Appendix Table A2).

[10] In unreported analyses, we assign more weights to neighbors of multiple target investors and the results are by and large unchanged (results available upon request).

[11] As shown in Online Appendix Table A3, target neighbors also significantly increase their trading activity in the acquirer firm itself in the post-M&A period.

between target investors and their neighbors, we decompose trading activity into (a) the probability that an investor places a trade in the acquirer industry (that is, the probability that the investor becomes infected) and (b) the trading intensity once infected. In particular, we re-estimate regression equations (1) and (2), but the dependent variable is now an indicator that equals one if an investor places at least one trade in the acquirer's industry in months 7 through 18 after the M&A is announced, and zero otherwise.

As can be seen in Online Appendix Table A4, irrespective of whether we estimate logit regressions or OLS regressions, our results indicate that the probability of reporting a trade in the acquirer industry is around ten times higher for target investors than for their neighbors. The similarity to the previously observed ten-to-one ratio in trading activity between target investors and their neighbors indicates that, once infected, target neighbors exhibit similar trading intensity as target investors.

On a related note, in our baseline analysis, we simply count the number or dollar value of trades irrespective of whether those trades represent buys or sells. In Online Appendix Table A5, we show that target investors and their neighbors tend to trade in the same direction; that is, if a target investor is buying in the acquirer industry, so are her neighbors. This contagion effect is much stronger on the buy side than on the sell side. This result seems sensible insofar as retail investors rarely short (Barber and Odean 2007). As a result, for retail investors to sell shares in the acquirer industry, they must first have bought shares in that industry to begin with.

4.1.2 Alternative Explanations

As alluded to earlier, M&As are unlikely to be a function of similarities in backgrounds between target investors and their neighbors. In addition, M&As are unlikely to reflect commonalities in preference or taste. Our study is therefore less subject to the aforementioned reflection problem.

There remains the possibility that local media coverage disproportionally draws the attention of both target investors and their neighbors leading to abnormally high trading activity in the acquirer industry without their directly communicating with one another. We deem such a scenario unlikely. Since we are contrasting the trading activity of target investors and their neighbors to that of all other investors, to explain our results, media coverage would need to be substantially more extensive in areas with target investors than in areas without. Media coverage might vary in this particular way for two reasons: (a) The financial press caters to and provides greater coverage of M&As in areas with a greater concentration of target investors, as these are the investors most affected by the M&As. (b) Target investors concentrate in areas that generally feature more concentrated media coverage of M&As, such as perhaps the most populated metropolitan areas.

The former seems unlikely as retail investors' collective holdings in target firms are negligible, making them an unattractive clientele to which to cater. Also, we exclude households in states where target or acquirer firms have any business operations. At odds with the latter and discussed further in Section 4.2.1, our results disappear once we focus on the most populated metropolitan areas.

In additional attempts to gauge the relevance of the local-media explanation, we conduct a placebo test around cash-financed M&As. If, for some reason, media coverage of M&As is greater in areas with a greater concentration of target investors, we should observe similar patterns around cash-financed M&As. In contrast, if our results are driven by the endowment of acquirer shares generating word-of-mouth effects, we should observe no noticeable patterns around cash-financed M&As. The results are reported in Table 3. The coefficient estimates for *Target Investor* are only one-fifth of those found for stock-financed M&As. None of the coefficient estimates is statistically significant. The coefficient estimates for *Target Neighbor* are all close to zero.[12]

We also test what happens during months 1 through 6 after the M&A is announced. An M&A-local-media-coverage explanation implies that our patterns should be significantly stronger around the M&A announcement date, which is when M&A media coverage typically peaks. In sharp contradiction, Panel C of Online Appendix Table A10 reveals that target investors and their neighbors trade in the acquirer

[12] In another placebo test, we consider investors who, at the time of an M&A, hold shares in the target industry, but not in the target firm itself. In particular, for each M&A, we identify the industry peer with the closest market capitalization and book-to-market ratio to the actual target firm ("pseudo target firm") that is not itself being acquired. We then examine whether shareholders of the pseudo target firm and their neighbors change their trading in the acquirer industry. As reported in Panels A and B of Online Appendix Table A6, we observe no increase in trading activity in the acquirer industry on the part of shareholders of pseudo target firms or on the part of their neighbors.

firm's industry substantially more frequently during months 7 through 18 than during months 1 through 6. These results suggest that the endowment of acquirer shares generating word-of-mouth effects has to be part of the explanation of our results.

4.2 Determinants of the Size of Contagion

A key strength of our setting is that we can point to what *triggered* word-of-mouth communication. In this section, we take advantage of this feature and examine how the size of contagion varies with conditions of the environment, characteristics of the trigger, and investor characteristics.

4.2.1 Determinants Broached by Prior Literature

a. Investors' Past Portfolio Performance

We begin with determinants broached by prior literature. Heimer and Simon (2017) find evidence that an investor is more likely to share her investment experiences when her portfolio has performed well in the past. To gauge the relevance of this determinant in our setting, we sort target neighbors into halves based on the corresponding target investors' portfolio returns in the quarter prior to the relevant M&A announcement. We then re-estimate regression equation (2) – including our full set of controls and M&A-fixed effects – for each half. Again, we refer to the coefficient estimate for *Target Neighbor*, which measures the size of abnormal trading activity in the acquirer industry undertaken by target neighbors, as the "size of contagion."

As can be seen in Panel A of Online Appendix Table A7, when target investors have above-median performances, the size of contagion is nearly twice as large as when target investors have below-median performances. These results corroborate the findings in Heimer and Simon (2017).

In Panel B, we extend our comparison to target neighbors' portfolio performances. We find that the size of contagion is substantially larger when target neighbors have above-median performances compared with when target neighbors have below-median performances. That is, we make the novel observation that word of mouth among investors strengthens not only with the sender's past performance, but also the recipient's. One possible explanation for this finding is that target neighbors avoid discussing investment ideas if their overall portfolios have been performing poorly as any such discussion would re-access the negative emotional experiences tied to their investment failures and raise questions about their investment competency.

b. "Sociable Communities"

To assess whether word of mouth is stronger in more sociable communities, we follow prior work (Putnam 2000; Brown, Ivković, Smith and Weisbenner 2008) and consider the following three measures at the state level: seminar or class attendance, club meeting attendance, and community project participation. The data are from www.bowlingalone.com/data.php3. In Panel A of Appendix Table A8, we sort target neighbors based on whether the corresponding target investor resides in a state with above-median sociability, or below-median sociability. We then re-estimate regression equation (2) in each of the two subsamples. In short, we find that the size of contagion is strong in the more sociable states, yet indistinguishable from zero in the less sociable states.

In additional analyses, we test whether the size of contagion varies with how long investors have lived in their respective areas and how densely populated their respective areas are. A target investor's tendency to interact with her neighbors should increase with the number of years such investor has lived in her neighborhood. We label all target investors who have lived in the same neighborhood for more than five years as long-term residents, and those who have lived in the neighborhood for less than five years as short-term residents. We then sort target neighbors based on whether the corresponding target investor is a long-term resident or a short-term resident. We use the five-year cutoff to ensure that we have similar numbers of investors across the two groups. As can be seen in Panel B of Online Appendix Table A8, we find that the size of contagion is about three to five times larger when the corresponding target investor is a long-term resident than when the corresponding target investor is a short-term resident.

We also conjecture that an investor pair living within a three-mile radius in a less populated area is more likely to interact with one another than an investor pair living within a three-mile radius in a more populated area (e.g., certain areas in Upstate New York versus Manhattan). To test this idea, we contrast the behavior of investors residing in metropolitan areas that are in the top quartile in terms of population to that of investors residing in metropolitan areas that are below the 75^{th} percentile in terms of population. Again, we use the top quartile cutoff to ensure that we have similar numbers of investors across the two groups. Consistent with our conjecture, we find that the size of contagion in the less-populated areas is more than twice as large as that in the more populated areas (Panel C of Online Appendix Table A8).

4.2.2 Determinants at the Environment Level

a. Market Uncertainty and Investor Sentiment

We now turn to the determinants that we believe are new to the literature. First, we hypothesize that in times of high uncertainty about the overall market and low investor sentiment, investors are wary of new investment ideas and less likely to act on such ideas. This makes it harder for new investment ideas to propagate among investors. To test this hypothesis, we sort M&As into halves based on the Chicago Board Options Exchange Volatility Index or based on the latest available University of Michigan Consumer Sentiment Index, both as of the week prior to the M&A announcement. We then re-estimate regression equation (2) separately in each half.

As can be seen from Panel A of Table 4, the size of contagion in periods of low market uncertainty is nearly twice as large as that in periods of high market uncertainty. Panel B shows further that the size of contagion in periods of high investor sentiment is nearly four times as large as that in periods of low investor sentiment. We make similar observations when considering the Baker-Wurgler Sentiment Index (2006) or the Consumer Confidence Index compiled by The Conference Board as alternate measures of investor sentiment. The above-noted contagion sizes are statistically different from each other at the 5% level.

b. Extraneous News Events

Next, we turn to extraneous news events that vary not only at the aggregate market level, but also in the cross-section of investors. As argued in Hirshleifer, Lim and Teoh (2009), attention is finite and investors can focus only on a small subset of signals at a time. We build on this argument and conjecture that investors are less likely to discuss investment ideas if there are important distractions. We focus on two types of distractions: NFL playoff games and weather-related emergencies (e.g., blizzards, tornados, or wildfires).

In Panel A of Table 5, we sort target neighbors based on whether the corresponding target investor resides in a metropolitan area with a local NFL team playing in the playoffs in the week before or after the corresponding M&A announcement ("Distracted"), or not ("Not Distracted"). In Panel B, we sort target neighbors based on whether the corresponding target investor resides within 100 miles of the focal point of a weather-related emergency in the week before or after the corresponding M&A announcement ("Distracted"), or not ("Not Distracted"). Our data source for weather-related emergencies is the National Centers for Environmental Information (https://www.ncdc.noaa.gov).[13]

As shown in Panel A of Table 5, our estimate for the size of contagion is highly significant when target investors and their neighbors are not distracted by an NFL playoff game, but insignificant and close to zero when target investors and their neighbors are distracted. Similarly, Panel B of the same table shows that there is sizeable contagion when target investors and their neighbors are not distracted by a weather-related emergency. There is zero contagion when target investors and their neighbors are in no such luck. Again, the above-noted contagion sizes are statistically different from each other at the 5% level.

4.2.3 Determinants at the Trigger Level

a. Valence

A large body of work argues and provides evidence that individuals prefer to share positive stories over negative stories (Berger and Milkman 2012; Berger 2014). While we do not directly observe the stories that target investors share with their neighbors, we extend the argument above and conjecture that stories are more likely to be shared if they are triggered by a positive event. We consider two measures for the

[13] We consider the following weather-related emergencies: Winter storm, blizzard, heavy snow, flood, ice storm, tornado, avalanche, excessive heat, wildfire, dust storm, exceptional drought, tropical storm, and hurricane.

positivity of a trigger: the corresponding target firm's announcement-day return and whether the relevant M&A is considered a friendly deal or a hostile takeover.

In Panel A of Table 6, we report results from sorting M&As into halves based on target-firm-announcement-day returns. We find that the size of contagion within the subsample of above-median announcement-day returns is nearly three times as large as that within the subsample of below-median announcement-day returns.[14] Similarly, Panel B shows that while there is strong contagion ensuing friendly M&As, there is no reliable contagion following hostile takeovers. The above-noted contagion sizes are statistically different from each other at the 1% level.

b. Saliency

Retail investors generally hold a small number of stocks in their portfolios. In our sample, the median retail investor holds three stocks. Any change in one stock position should therefore have a material impact on retail investors' attention and subsequent information-gathering activity. However, there is wide variation in portfolio size across retail investors and we suspect that our effect becomes weaker the more stocks a target investor holds in her portfolio.

In Panel C of Table 6, we compute for each target investor the number of stocks in her portfolio ("portfolio size"). We then sort target neighbors into halves based on the corresponding target investor's portfolio size. In line with expectations, we observe strong contagion when a target investor has a below-median portfolio size and no reliable contagion when a target investor has an above-median portfolio size. The difference in contagion size between the two subsamples is statistically significant at the 5% level.

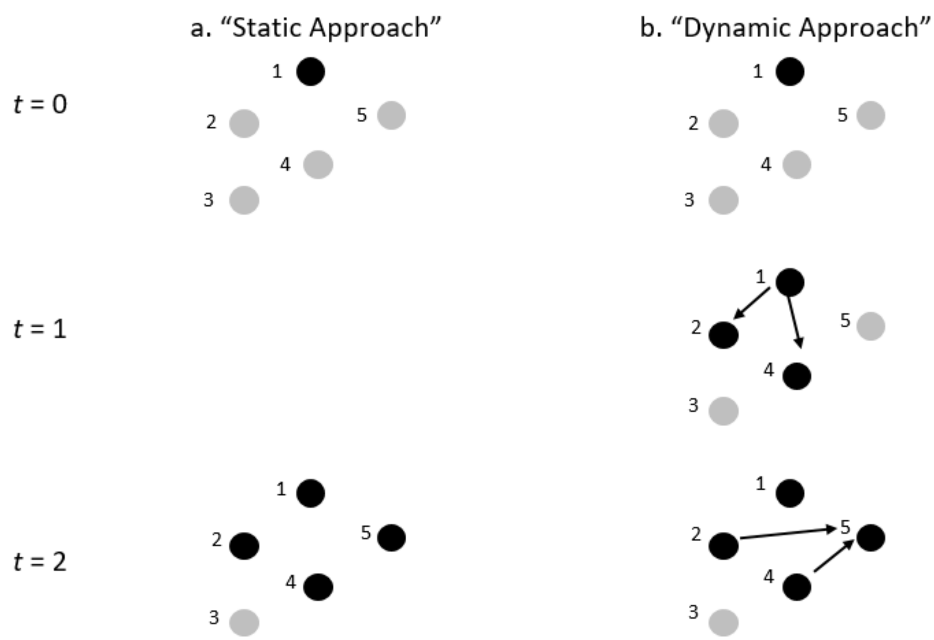
[14] In unreported analyses, we sort all cross-industry stock-financed M&As based on target firms' cumulative returns from the M&A announcement to the estimated M&A-completion date and we arrive at the same conclusion.

4.2.4. Determinants at the Investor Level

a. Dynamic Approach

Our final class of determinants contains characteristics of the investors communicating with one another. We now adopt a "dynamic research design," which we borrow from research that examines the contagion rate of diseases. The reason for our switch in research design is perhaps best illustrated in a figure.

The figure below represents a simple example, which contrasts our previous "static empirical design" with the new dynamic design. Each dot represents an investor. All investors reside within a three-mile radius. Dark dots represent investors with abnormal trading in the acquirer industry; grey dots represent investors with no such abnormal trading activity. Investor 1 is a target investor.



Our static approach captures merely the size of contagion, that is, the fraction or number of target neighbors, who become infected within a given time. The static approach cannot capture whether any such infection is coming straight from the target investor or another infected neighbor. For instance, in the left panel, it is unclear whether Investor 5 becomes infected through Investor 1 or whether abnormal trading in the acquirer industry first spills over from Investor 1 to Investors 2 and 4, who, in turn, infect Investor 5. Such differentiation is not material when gauging what environmental or trigger conditions affect the total size of contagion. However, such differentiation is crucial when trying to assess how the contagion rate varies with characteristics of the infected and the susceptible as, for that, we need to know – at any given point – who the infected and who the susceptibles are.

Our dynamic approach allows for such differentiation albeit at the cost of additional assumptions and high technicality. We defer a full account of our methodology to the Appendix at the end of the paper. Essentially, following each cross-industry stock-financed M&A, we now estimate a transmission matrix and track how trading activity in the acquirer industry percolates across investors from quarter to quarter as illustrated in the right panel of the previous figure. We then examine the degree to which any impact investor *j* has on investor *i* varies with individual characteristics of investors *j* and *i*.

b. Income, Age and Gender

We consider three individual characteristics on which we have detailed data: income, age, and gender. The results are reported in Columns (1) and (2) in Table 7. Our results suggest that a ten-year difference in age, a one-step difference in income, and being of another gender lowers the contagion rate by 12%, 2%, and 17%, respectively (all statistically significant at the 1% level).[15] A one-step difference in household income represents an economically meaningful difference.[16] Our results thus suggest that age and gender represent much higher barriers to communication than differences in socio-economic backgrounds.

Since our empirical design allows us to gauge who the senders and who the receivers are, we can assess whether the contagion rate varies *asymmetrically* with social characteristics of senders and receivers. That is, we can examine whether the contagion rate differs between a receiver being ten years *younger* than a sender and a receiver being ten years *older* than a sender. Empirically, instead of estimating one slope for the absolute distance in a social characteristic between an investor pair, we now estimate two slopes, one for positive differences and another for negative differences.

[15] We discuss our empirical design and how to interpret the coefficient estimates in the Appendix. In short, the base communication rate for Column (1) is 0.452. Since the estimate for $\widehat{Trade}_{j,t}$ *$|Age_i$-$Age_j|$ in Column (1) is -0.006, a ten-year difference in age suggests a 12% drop in the contagion rate ((-0.006 × 10) / 0.452 = 12%).

[16] In Online Appendix Table A9, we show the various income bins provided by our data vendor.

The results, presented in Columns (3) and (4) in Table 7 show strong asymmetries. For instance, consider the case in which i is the receiver and j the sender, the coefficient estimate for $|Age_i - Age_j|^-$ in Column (3) is -0.003. In comparison, the coefficient estimate for $|Age_i - Age_j|^+$ is -0.007. These two estimates are statistically different from each other at the 1% level. To interpret these estimates, consider investor i who is 40 years old. The contagion rate is maximized if investor j is also 40 years old. Our estimates suggest that if investor i is younger than investor j, the contagion rate from j to i declines by 0.003 per one-year age gap. If investor i is older than investor j, the contagion rate from j to i declines by 0.007. That is, when investor i is younger than investor j, the contagion rate from j to i declines by less than when investor i is older than investor j. We can thus infer that, on average, younger investors are more likely to act on older investors' views than the other way around.

Applying the same logic to gender and age, we find that the average contagion rate from female to male is higher than that from male to female (-0.107 vs. -0.018) and that lower-income investors are more likely to act on higher-income investors' views than vice versa (-0.015 vs. -0.010). One implication of these estimates is that information transmission is maximized if the information is seeded with older, female, wealthier members of a community.

In our final experiment involving the dynamic approach, we estimate the residual state fixed effects in the contagion rate after controlling for observable social characteristics. Figures 2 and 3 plot the average contagion rates by state. We observe strong regional differences. Some of the highest contagion rates are in the Southeast (e.g., North and South Carolina, Georgia, Florida). Some of the lowest contagion rates are in the central West/Midwest (e.g., Montana, Wyoming, Kansas). The correlation coefficient between our contagion rate and a state-level measure of sociability drawn from Putnam (2000) is as high as 0.44, confirming our earlier evidence that the size of contagion is larger in regions in which individuals are more sociable.[17]

[17] Putnam (2000) surveys individuals regarding whether they seek advice from friends. The state-level measure of sociability is the state-level average percentage of households that respond with "yes."

5. Additional Analyses

We conclude our study with some additional analyses, which gauge how the size of contagion varies with the distance between target investors and their neighbors, and time since M&A completion. We also consider asset pricing implication of our findings.

5.1 Varying Distance and Time

If social interactions play a major role in generating our results, our documented pattern should vary substantially with our definition of neighbors and with the time horizon over which we analyze trading activity. In our first set of such tests, tabulated in Panel A of Online Appendix Table A10, we vary the distance over which we define neighbors. When we broaden our definition of neighbor to investors who live between three to seven miles from a target investor, the coefficient estimate for *Target Neighbor* in the full regression specification using the dollar-weighted measure of trading activity drops from 21bps to 18bps. As we further increase the distance to between seven to fifteen miles, the coefficient estimate for *Target Neighbor* drops to 15bps only to drop further to 2bps for investors who reside between 15 to 30 miles of a target investor. We make identical observations when basing our dependent variable on the number of trades. This rapid decrease in the coefficient estimates is consistent with the idea that word-of-mouth effects decay quickly with distance.

We also experiment with the time period over which we measure investors' trading activity. Specifically, instead of focusing on the one-year period after the estimated M&A completion (that is, months 7 through 18 after the M&A is announced), we expand our window to years two and three. Irrespective of the dependent variable, we find that target investors gradually reduce their abnormally high trading activity in the acquirer industry. In particular, in our baseline regression, which runs from months 7 through 18 after the M&A is announced, target investors disproportionately increase their trading in the acquirer industry by 2.30% based on their number of trades. As can be seen in Panel B of Online Appendix Table A10, this figure drops to 1.78% in months 19 through 30, and to 1.23% in months 31 through 42. The drop in trading propensity on the part of target neighbors is even more pronounced. The coefficient estimate for *Target Neighbor* drops from 23bps in months 7 through 18, to 5bps in months 19 through 30, and to 1bp in months 31 through 42.

5.2 Dissemination of Value-Relevant Information or Spreading Noise?

Do investors in our setting transmit unique and value-relevant news or simply spread noise? If any newly acquired views about the acquirer industry transmitted through word of mouth represent unique value-relevant information, stocks bought by target investors and their neighbors in the acquirer industry ("long leg") should subsequently outperform stocks sold by target investors and their neighbors in the acquirer industry ("short leg"). On the other hand, if views about the acquirer industry represent mere noise, we should observe no performance differential between the long leg and the short leg.

We experiment with three portfolio construction schemes: (a) For each stock in the acquirer industry traded by target investors and their neighbors from months 7 through 18 after the M&A is announced, we compute the total number of shares bought by target investors and their neighbors minus the total number of shares sold. The long leg contains stocks of which target investors and their neighbors are net buyers; the short leg contains stocks of which they are net sellers. The long and short legs are weighted by the net total number of shares bought (sold) across target investors and their neighbors, and they are held for one month. (b) We repeat the above but we now consider the dollar value of shares rather than the number of shares. (c) For each stock in the acquirer industry traded by target investors and their neighbors from months 7 through 18 after the M&A is announced, we compute the average change in a stock's weight in the portfolios of target investors and their neighbors. The long leg contains stocks that experience an increase; the short leg contains stocks that experience a decrease. The long and short legs are weighted by the corresponding stock's portfolio weight change, and they are held again for one month.

The results are reported in Table 8. Irrespective of the portfolio construction scheme, we find that the long leg underperforms the short leg, albeit not to a statistically significant extent. These results do not support the notion that newly acquired views about firms in the acquirer industry reflect value-relevant information. Our results are similar to observations made by Hvide and Östberg (2015) who also find evidence that information transmitted through word of mouth does not help investors' trading performances.

5.3 Social Interactions and Anomaly Returns

Han, Hirshleifer and Walden (2018) argue that stocks that reside in the short leg of certain anomalies exhibit characteristics that make them more engaging to discuss with other investors (e.g., stocks with high volatility or high skewness). Such "viral" stocks, in turn, become overpriced and, on average, earn low returns going forward.

If word of mouth and a preference to discuss stocks with certain characteristics are responsible for overpricing among short-leg stocks, short-leg stocks should be particularly overpriced when word-of-mouth effects are particularly strong. In this paper, we find evidence that word of mouth is particularly strong when uncertainty about the overall market is low and when investor sentiment is high. Subsequent short-leg returns should therefore be particularly low following episodes of low uncertainty and high sentiment.

To test these predictions, we compile short-leg returns of 172 anomalies[18] and we contrast the subsequent short-leg returns after months the VIX or the University of Michigan Consumer Sentiment Index is in the top versus the bottom quintile. In short, in line with expectations, we find that subsequent short-leg returns are 2.3% a month lower (*t*-statistic = -2.40) when the VIX is in the bottom quintile, that is, when uncertainty is low, and 1.2% lower (*t*-statistic = -1.82) when our measure of investor sentiment is in the top quintile, that is, when sentiment is high. These findings represent initial evidence that investor communication is a relevant factor in explaining the cross-section of average stock returns.

[18] We thank Andrew Chen (https://sites.google.com/site/chenandrewy/) for sharing his short-leg returns with us.

6. Conclusion

The question of how information spreads and how views form lies at the heart of asset pricing and has motivated a significant body of research. Most such research examines how investors react to public news or public opinions such as the publication of sell-side analyst reports (Kothari 2001).

However, much of the information on which investors condition their behavior does not come "straight from the source," but instead reflects information obtained via word-of-mouth communication (Shiller and Pound 1989). Compared with how extensively the accounting and finance literature has been studying how investors react to public announcements of news and opinions, we know relatively little about how information travels privately through word of mouth. Here, we provide an empirical strategy for studying word of mouth among investors and we provide basic evidence on what determines the strength of word-of-mouth effects in financial markets. Further gauging what determines investors' reliance on word of mouth, as well as speaking to related questions, such as what types of content investors prefer to share or to what degree word of mouth causes investors to misperceive reality, should prove to be an interesting avenue for future research.

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Appendix. The Dynamic Setting

In this Appendix, we provide details of our dynamic setting. In essence, we estimate a transmission matrix that quantifies how views and opinions percolate through the investor population from one period to the next:

$$\begin{pmatrix} X_{1,t+1} \\ X_{2,t+1} \\ \cdot \\ \cdot \\ \cdot \\ X_{k,t+1} \end{pmatrix} = \begin{pmatrix} \beta_{1,1},\ \beta_{1,2},\ \ldots,\beta_{1,k} \\ \beta_{2,1},\ \beta_{2,2},\ \ldots,\beta_{2,k} \\ \cdot \\ \cdot \\ \cdot \\ \beta_{k,1},\ \beta_{k,2},\ \ldots,\beta_{k,k} \end{pmatrix} * \begin{pmatrix} X_{1,t} \\ X_{2,t} \\ \cdot \\ \cdot \\ \cdot \\ X_{k,t} \end{pmatrix},$$

where $X_{i,t}$ is the trading activity of investor i in the acquirer industry in period t and $X_{i,t+1}$ is the trading activity of investor i in the acquirer industry in period t+1.

In vector form and over multiple periods, we have

$$X_{t+1} = B * X_t$$

$$X_{t+2} = B * X_{t+1}$$

$$\ldots$$

$$X_{t+p} = B * X_{t+p-1}. \tag{3}$$

The advantage of the dynamic setting is that it enables us to explicitly and dynamically account for "third-party ties," that is, our setting allows for the possibility that investor i transmits her view to investor j through a third party (or a chain of third parties) without being in direct contact with investor j. This, in turn, enables us to quantify how the social distance between any two investors affects the pairwise communication rate between these two investors. In comparison, the static setting captures only the final cumulative communication rate, that is, how many investors are infected by the end of a year irrespective of whether the infection is coming straight from patient zero or whether the investor is infected by a neighbor of patient zero or a neighbor of a neighbor of patient zero.

Compounding the transmission matrix in (3) over p periods, we have

$$X_{t+p} = B * X_{t+p-1} = B^2 * X_{t+p-2} = \cdots = B^p * X_t, \tag{4}$$

where t is the M&A completion date and p is the number of periods after the M&A is completed. If the set of X_{t+p}'s satisfied the exogeneity condition, we could simply estimate a vector auto-regression based on $X_{t+p} = B * X_{t+p-1}$ by stacking our observations both across M&As and across event quarters. However, as the exogeneity condition is unlikely to hold (trading by investors *i* and *j* may be driven by the same information), we instrument the independent variable in each of these equations by the initial portfolio shock induced by the M&A. In other words, we jointly estimate the following set of equations:

$$X_{t+1} = B * \widehat{X_t} + e_{t+1}$$

$$X_{t+2} = B^2 * \widehat{X_t} + e_{t+2},$$

$$\dots$$

$$X_{t+p} = B^p * \widehat{X_t} + e_{t+p},$$

where $\widehat{X_t}$ is the instrumented trading activity in the acquirer industry immediately after the M&A is completed. Estimating this set of equations is computationally challenging as the set contains non-linear terms of an unknown 70,000 x 70,000 matrix (we have roughly 70,000 investors in our sample).

To get around this technical complexity, we instead employ a three-stage approach. In our estimation, we define each period as one quarter, as retail investors in our sample, on average, trade once every quarter. We study the four quarters after each M&A, so *p* ranges from 1 to 4. We restrict ourselves to four quarters as we find in this paper that M&As no longer have a discernible impact on target neighbors' trading activity in years two and three after M&A completion.

In our first stage, we instrument the set of X_{t+p}'s using portfolio shocks experienced by target investors at the M&A completion date. Specifically, we estimate regression equations of investor i's trading activity in the acquirer industry in each quarter *t*+*p* on *Target Investor*$_i$, which equals one if investor i is a target investor and zero otherwise. Trading activity in the acquirer industry is the total number of trades (or total dollar value of trades) in the acquirer industry (excluding the acquirer firm) divided by the total number of trades (or total dollar value of trades) across all industries.

In our second stage, we estimate how trading activity in the acquirer industry in period *t*+*p* (X_{t+p}) relates to the *fitted* trading activity in the acquirer industry in period *t*+p-1 ($\widehat{X_{t+p-1}}$), calculated from the first-stage regression:

$$X_{t+1} = B * \widehat{X_t} + e_{t+1} = B * \widehat{X_t} + e_{t+1}$$

$$X_{t+2} = B^2 * \widehat{X_t} + e_{t+2} = B * \widehat{X_{t+1}} + e_{t+2} \quad (5)$$

$$\ldots$$

$$X_{t+p} = B^p * \widehat{X_t} + e_{t+p} = B * \widehat{X_{t+p-1}} + e_{t+p}.$$ [19]

In our third stage, we improve the efficiency of our estimates for the B matrix using a recursive method. Specifically, in each iteration, we use the B matrix estimated from the previous round to re-estimate a new set of $\widehat{X_{t+p}}$'s. That is, we start with the instrumented $\widehat{X_t}$ and then calculate $\widehat{X_{t+1}} = B * \widehat{X_t}$, $\widehat{X_{t+2}} = B * \widehat{X_{t+1}}$, etc. We then re-estimate the set of equations (5) using $\widehat{X_{t+1}}$, $\widehat{X_{t+2}}$, ..., $\widehat{X_{t+p}}$ to derive a new B. We initialize the process with the B matrix estimated from the second stage and stop the process when we find a fixed point for B.

To facilitate the computation of the transmission matrix, we impose a linear structure on all the off-diagonal elements, $\beta_{i,j}$.[20] In particular, we conjecture that the effective communication rate between any two investors *i* and *j* is a linear function of (a) $|Income_i - Income_i|$, the "income gap" between any two investors, (b) $|Age_i - Age_j|$, the age gap, and (c) $|Gender_i - Gender_j|$, the gender gap. To further reduce complexity, we set all $\beta_{i,j}$ at zero when the physical distance between investors *i* and *j* is greater than three miles; in other words, we focus only on elements that are close to the diagonal. (Our results are by and large unchanged if we use other distance cutoffs, such as four, five, or six miles.)

[19] If we were to stop here, our estimates for the B matrix would be unbiased (to the extent that our instruments are exogenous). However, we lose efficiency as we do not impose the following condition in the first-stage estimation: $\widehat{X_{t+p}} = B * \widehat{X_{t+p-1}} = \cdots = B^p * \widehat{X_t}$.

[20] For all the diagonal terms, $\beta_{i,i}$, which capture the persistence in investor trading behavior, we assume that it is a constant for all investors.

Under these assumptions, rather than having to estimate 70,000 x 70,000 unknowns, we now have to estimate the following linear function with only four unknowns:

$$\beta_{i,j} = b_0 + b_1 * |Income_i - Income_j| + b_2 * |Age_i - Age_j| \\ + b_3 * |Gender_i - Gender_j| + \varepsilon_{i,j} \ \ (i \neq j),$$

where $\varepsilon_{i,j}$ captures the unobserved determinants of $\beta_{i,j}$ and b_0 reflects the baseline effective communication rate (with all social distances set to zero). Scaling our estimates for b_1, b_2, and b_3 by our estimate for b_0 yields the proportional change in the effective communication rate as a function of social distances.

Since our empirical design enables us to pinpoint senders and receivers, we can also examine whether the effective communication rate varies *asymmetrically* with social characteristics of senders and receivers. That is, we can examine whether the communication rate varies when a receiver is ten years younger than the sender compared with when a receiver is ten years older than the sender. Empirically, instead of estimating one slope for the absolute distance in a social characteristic between a pair of investors, we now estimate two slopes, one for positive differences and another for negative differences. With this modification, we now have a linear equation of $\beta_{i,j}$ with seven elements.

Table 1. Summary Statistics

This table reports summary statistics for our various samples. Panel A presents statistics for the M&A sample. Stock-financed M&As are deals that are at least partially equity-financed; cash-financed M&As comprise 100% cash-financed deals. Firm size is the number of shares outstanding multiplied by the share price as of the month prior to an M&A. All observations are at the M&A level. Panel B shows investor- and portfolio characteristics for our retail investor sample. We require that investors place at least one trade in either the one-year period prior to the M&A or the one-year period following the M&A. We exclude households in states where the target or acquirer firm has any business operations—identified using both headquarters and factory locations. We further require that these households have no existing positions in the acquirer industry prior to the M&A announcement. Portfolio size is the dollar value of the stock holdings. Investor income is the annual income of the primary account holder. Investor gender is a dummy that equals one for male and zero for female. All observations are at the account/year-month level. Panel C shows demographic information for each zip code included in our sample. *Seeking Advice from Friends* under "Sociability" comes from Putnam (2000) and captures how often people seek advice from friends. All observations under "Basic Characteristics" are at the zip-code/year-month level; all observations under "Sociability" are at the state/year level.

	N	25%	Median	75%	Mean	Std. Dev.
Panel A: M&A Characteristics						
Stock-Financed M&As						
Acquirer Firm Size ($million)	317	217	951	2,920	2,742	5,504
Target Firm Size ($million)	317	31	74	250	651	2,370
Cash-Financed M&As						
Acquirer Firm Size ($million)	143	391	1,561	4,491	5,541	12,970
Target Firm Size ($million)	143	30	93	216	266	585
Panel B: Investor/Portfolio Characteristics						
Portfolio Size ($)	70,608	5,513	13,141	31,818	41,030	216,539
Number of Stocks Held	70,608	1.00	2.00	5.00	3.88	5.03
Number of Trades Each Month	70,608	0.00	0.00	0.00	0.47	1.76
Value of Trades Each Month ($)	70,608	0	0	0	5,679	76,056
Investor Age	70,608	36.00	46.00	56.00	42.02	21.44
Investor Income ($)	70,608	45,000	62,500	87,500	69,500	30,064
Investor Gender	70,608	1.00	1.00	1.00	0.90	0.30
Panel C: Zip Code Characteristics						
Basic Characteristics						
Population	42,057	785	2,777	11,960	8,965	13,134
No. Household Members	42,057	2.40	2.56	2.73	2.59	0.35
House Value ($)	42,057	58,200	82,900	122,300	105,359	89,589
Household Income ($)	42,057	29,779	36,250	45,750	39,631	16,243
Sociability						
Seeking Advice from Friends	294	2.90	3.07	3.21	3.06	0.31

Table 2. Trading in the Acquirer Industry after Stock-Financed M&As

This table reports coefficient estimates from regressions of investor trading in the acquirer industry on a target investor dummy (Panel A) or a target neighbor dummy (Panel B). We focus on cross-industry stock-financed M&As and the observations are at the M&A/brokerage account/year-month level. The dependent variable in Columns (1)-(3) is the number of trades in the acquirer industry (excluding the acquirer firm) as a fraction of the total number of trades across all industries in months 7 through 18 after the M&A is announced. The dependent variable in Columns (4)-(6) is the dollar value of trades in the acquirer industry (excluding the acquirer firm) as a fraction of the total dollar value of trades across all industries in months 7 through 18 after the M&A is announced. We examine total trading behavior in months 7 through 18 after the M&A is announced since the exact completion date is missing for many M&As, and as, on average, it takes six months for an M&A to be completed (Giglio and Shue, 2014). *Target Investor* is an indicator, which equals one if an investor possesses shares of the target stock at the end of the month prior to the M&A announcement. *Target Neighbor* is an indicator variable that takes the value of one if an investor lives within three miles of a target investor. Investor-level controls include the account holder's income, number of children, number of family members, age, gender, and marital status. Zip-code-level controls include the zip-code population, fraction of male residents, average home value, average number of household members, and average household income. Standard errors, shown in brackets, are clustered at the zip-code- and the year-month-of-an-M&A-announcement level. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

	# Trades			$ Trades		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Target Investors' Trading in the Acquirer Industry						
Target Investor	0.0255***	0.0234***	0.0234***	0.0227***	0.0208***	0.0208***
	[0.0046]	[0.0048]	[0.0048]	[0.0045]	[0.0047]	[0.0047]
Investor Controls	YES	NO	YES	YES	NO	YES
Zip Code Controls	YES	NO	YES	YES	NO	YES
M&A Fixed Effects	NO	YES	YES	NO	YES	YES
Adj. R^2	0.01%	1.65%	1.66%	0.01%	1.58%	1.59%
# Obs.	7,580,936	7,580,936	7,580,936	7,580,936	7,580,936	7,580,936
Panel B: Target Neighbors' Trading in the Acquirer Industry						
Target Neighbor	0.0046***	0.0020***	0.0022***	0.0043***	0.0018***	0.0021***
	[0.0007]	[0.0007]	[0.0007]	[0.0008]	[0.0007]	[0.0007]
Investor Controls	YES	NO	YES	YES	NO	YES
Zip Code Controls	YES	NO	YES	YES	NO	YES
M&A Fixed Effects	NO	YES	YES	NO	YES	YES
Adj. R^2	0.01%	1.65%	1.66%	0.01%	1.58%	1.59%
# Obs.	7,578,642	7,578,642	7,578,642	7,578,642	7,578,642	7,578,642

Table 3. Trading in the Acquirer Industry after *Cash-Financed* M&As – Placebo

This table reports coefficient estimates from regressions of investor trading in the acquirer industry on a target investor dummy (Panel A) or a target neighbor dummy (Panel B). The regressions are identical to those in Table 2, but we now estimate regressions on a sample of *cash-financed* M&As. Standard errors, shown in brackets, are clustered at the zip-code- and the year-month-of-an-M&A-announcement level. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

	# Trades			$ Trades		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Target Investors' Trading in the Acquirer Industry						
Target Investor	0.0046	0.0044	0.0043	0.0061	0.0059	0.0059
	[0.0037]	[0.0035]	[0.0035]	[0.0042]	[0.0040]	[0.0040]
Investor Controls	YES	NO	YES	YES	NO	YES
Zip Code Controls	YES	NO	YES	YES	NO	YES
M&A Fixed Effects	NO	YES	YES	NO	YES	YES
Adj. R^2	0.01%	2.36%	2.37%	0.01%	2.25%	2.26%
# Obs.	3,489,281	3,489,281	3,489,281	3,489,281	3,489,281	3,489,281
Panel B: Target Neighbors' Trading in the Acquirer Industry						
Target Neighbor	0.0015	-0.0001	0.0003	0.0014	-0.0002	0.0002
	[0.0011]	[0.0010]	[0.0010]	[0.0012]	[0.0010]	[0.0010]
Investor Controls	YES	NO	YES	YES	NO	YES
Zip Code Controls	YES	NO	YES	YES	NO	YES
M&A Fixed Effects	NO	YES	YES	NO	YES	YES
Adj. R^2	0.01%	2.36%	2.37%	0.01%	2.25%	2.26%
# Obs.	3,488,558	3,488,558	3,488,558	3,488,558	3,488,558	3,488,558

Table 4. Determinants of the Size of Contagion: Market Uncertainty and Investor Sentiment

This table reports coefficient estimates from regressions of investor trading in the acquirer industry on a target neighbor dummy. The regressions are identical to those in Panel B of Table 2, but we now estimate the regressions separately for various subsamples. In Panel A, we sort M&As into halves based on the Chicago Board Options Exchange Volatility Index as of the week prior to the M&A announcement. In Panel B, we sort M&As into halves based on the latest available University of Michigan Consumer Sentiment Index. “High” and “Low” represent top- and bottom-half observations, respectively. Standard errors, shown in brackets, are clustered at the zip-code- and the year-month-of-an-M&A-announcement level. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

	High		Low	
	# Trades (1)	$ Trades (2)	# Trades (3)	$ Trades (4)
Panel A: Market Uncertainty				
Target Neighbor	0.0015*	0.0016*	0.0028***	0.0025**
	[0.0009]	[0.0009]	[0.0010]	[0.0010]
Investor Controls	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES
M&A Fixed Effects	YES	YES	YES	YES
Adj. R^2	1.62%	1.54%	1.67%	1.62%
# Obs.	3,690,916	3,690,916	3,887,726	3,887,726
Panel B: Investor Sentiment				
Target Neighbor	0.0038***	0.0032***	0.0010	0.0013
	[0.0011]	[0.0011]	[0.0008]	[0.0009]
Investor Controls	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES
M&A Fixed Effects	YES	YES	YES	YES
Adj. R^2	1.76%	1.69%	1.55%	1.49%
# Obs.	3,743,758	3,743,758	3,834,884	3,834,884

Table 5. Determinants of the Size of Contagion: Sports- and Weather-Related Distractions

This table reports coefficient estimates from regressions of investor trading in the acquirer industry on a target neighbor dummy. The regressions are identical to those in Panel B of Table 2, but we now estimate the regressions separately for various subsamples. In Panel A, we sort target neighbors based on whether the corresponding target investor resides in a metropolitan area with a local NFL team playing in the playoffs in the week before or after the corresponding M&A announcement (“Distracted”), or not (“Not Distracted”). In Panel B, we sort target neighbors based on whether the corresponding target investor resides within 100 miles of the focal point of a weather-related emergency in the week before or after the corresponding M&A announcement (“Distracted”), or not (“Not Distracted”). Standard errors, shown in brackets, are clustered at the zip-code- and the year-month-of-an-M&A-announcement level. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

	Distracted		Not Distracted	
	# Trades (1)	$ Trades (2)	# Trades (3)	$ Trades (4)
Panel A: Sports-Related Distractions				
Target Neighbor	-0.0009 [0.0033]	-0.0008 [0.0035]	0.0023*** [0.0007]	0.0022*** [0.0007]
Investor Controls	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES
M&A Fixed Effects	YES	YES	YES	YES
Adj. R^2	1.65%	1.58%	1.65%	1.59%
# Obs.	7,542,361	7,542,361	7,576,766	7,576,766
Panel B: Weather-Related Distractions				
Target Neighbor	0.0007 (0.0014)	0.0009 (0.0015)	0.0026*** (0.0008)	0.0023*** (0.0008)
Investor Controls	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES
M&A Fixed Effects	YES	YES	YES	YES
Adj. R^2	1.65%	1.58%	1.65%	1.58%
# Obs.	7,571,089	7,571,089	7,548,038	7,548,038

Table 6. Determinants of the Size of Contagion: Characteristics of the Trigger

This table reports coefficient estimates from regressions of investor trading in the acquirer industry on a target neighbor dummy. The regressions are identical to those in Panel B of Table 2, but we now estimate the regressions separately for various subsamples. In Panel A, we sort M&As into halves based on the target firm's announcement day returns. "High" and "Low" represent top- and bottom-half observations, respectively. In Panel B, we sort M&As - for which we have the relevant data - based on whether they represent friendly M&As or hostile takeovers. "High" and "Low" represent friendly M&As and hostile takeovers, respectively. In Panel C, we sort target neighbors based on whether the corresponding target investor's portfolio size - in terms of number of stocks - is in the bottom half of its distribution or in the top half of its distribution. As any stock replacement in an investor's portfolio should be more salient when such investor has fewer stocks in her portfolio, bottom-half observations are allocated to the "High"-salience columns and top-half observations are allocated to the "Low"-salience columns. Standard errors, shown in brackets, are clustered at the zip-code- and the year-month-of-an-M&A-announcement level. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

	High		Low	
	# Trades (1)	$ Trades (2)	# Trades (3)	$ Trades (4)
Panel A: Positivity ("High versus Low Announcement Day Return")				
Target Neighbor	0.0034*** [0.0010]	0.0034*** [0.0010]	0.0013 [0.0009]	0.0011 [0.0009]
Investor Controls	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES
M&A Fixed Effects	YES	YES	YES	YES
Adj. R^2	1.84%	1.77%	1.47%	1.41%
# Obs.	3,744,500	3,744,500	3,834,142	3,834,142
Panel B: Positivity ("Friendly versus Hostile Takeover")				
Target Neighbor	0.0025*** [0.0007]	0.0024** [0.0008]	-0.0020 [0.0016]	-0.0021 [0.0017]
Investor Controls	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES
M&A Fixed Effects	YES	YES	YES	YES
Adj. R^2	1.60%	1.53%	2.92%	2.87%
# Obs.	7,251,071	7,251,071	244,852	244,852
Panel C: Salience				
Target Neighbor	0.0026*** [0.0009]	0.0023** [0.0009]	0.0013 [0.0010]	0.0015 [0.0010]
Investor Controls	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES
M&A Fixed Effects	YES	YES	YES	YES
Adj. R^2	1.65%	1.59%	1.65%	1.59%
# Obs.	7,561,064	7,561,064	7,558,063	7,558,063

Table 7. Determinants of the Size of Contagion: Investor Characteristics

This table reports the results of a three-stage estimation of a transmission matrix. The estimation procedure is detailed in the Appendix. In essence, we assess how trading activity in the acquirer industry percolates across investors from quarter to quarter and how any such "contagion rate" varies with differences in income, age and gender between the sender of acquirer-industry information and the receiver of acquirer-industry information. Trading activity is based either on the number of trades (Columns (1) and (3)) or the dollar value of trades (Columns (2) and (4)). In Columns (1) – (2), we assume that any distance in income, age and gender has a symmetric effect on the contagion rate. For instance, whether the sender is ten years older than the receiver or ten years younger than the receiver has the same decreasing effect on the contagion rate. In Columns (3) – (4), we no longer impose such assumption. The differences denoted by "+" take the value of the absolute difference if the difference is greater than zero, and zero otherwise. The differences denoted by "-" take the value of the absolute difference if the difference is smaller than zero, and zero otherwise. Bootstrapped standard errors are shown in brackets. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

	Symmetric		Asymmetric	
	# Trades (1)	$ Trades (2)	# Trades (3)	$ Trades (4)
$\widehat{Trade}_{i,t}$	0.621*** [0.039]	0.602*** [0.040]	0.616*** [0.039]	0.597*** [0.040]
$\widehat{Trade}_{j,t}$	0.452*** [0.047]	0.469*** [0.048]	0.435*** [0.047]	0.452*** [0.048]
$\widehat{Trade}_{j,t} \times \lvert Age_i\text{-}Age_j \rvert$	-0.006*** [0.001]	-0.007*** [0.001]		
$\widehat{Trade}_{j,t} \times \lvert Age_i\text{-}Age_j \rvert^{--}$			-0.003*** [0.001]	-0.003*** [0.001]
$\widehat{Trade}_{j,t} \times \lvert Age_i\text{-}Age_j \rvert^{+}$			-0.007*** [0.001]	-0.007*** [0.001]
$\widehat{Trade}_{j,t} \times \lvert Income_i\text{-}Income_j \rvert$	-0.013** [0.005]	-0.011** [0.005]		
$\widehat{Trade}_{j,t} \times \lvert Income_i\text{-}Income_j \rvert^{--}$			-0.010** [0.004]	-0.010** [0.005]
$\widehat{Trade}_{j,t} \times \lvert Income_i\text{-}Income_j \rvert^{+}$			-0.015** [0.007]	-0.015** [0.006]
$\widehat{Trade}_{j,t} \times \lvert Gender_i\text{-}Gender_j \rvert$	-0.086*** [0.021]	-0.090*** [0.021]		
$\widehat{Trade}_{j,t} \times \lvert Gender_i\text{-}Gender_j \rvert^{--}$			-0.107** [0.050]	-0.112** [0.048]
$\widehat{Trade}_{j,t} \times \lvert Gender_i\text{-}Gender_j \rvert^{+}$			-0.018 [0.070]	-0.019 [0.071]
Adj. R^2	0.016	0.016	0.016	0.016
# Obs.	1,591,710	1,591,710	1,591,710	1,591,710

Table 8. Does Word-of-Mouth Help Investors Make Better Investment Decisions?

This table reports monthly returns of hedge portfolios that (1) go long acquirer-industry stocks bought by target investors and their neighbors ("long leg") and (2) go short acquirer-industry stocks sold by target investors and their neighbors ("short leg"). We experiment with three portfolio construction schemes: In Panel A, for each stock in the acquirer industry traded by target investors and their neighbors from months 7 through 18 after the M&A is announced, we compute the total number of shares bought by target investors and their neighbors minus the total number of shares sold. The long leg contains stocks of which target investors and their neighbors are net buyers; the short leg contains stocks of which they are net sellers. The long and short legs are weighted by the net total number of shares bought (sold) across target investors and their neighbors, and they are held for one month. In Panel B, we repeat the above but we now consider the dollar value of shares as opposed to the number of shares. In Panel C, for each stock in the acquirer industry traded by target investors and their neighbors from months 7 through 18 after the M&A is announced, we compute the equal-weighted average change in a stock's weight in target investors' and target neighbors' portfolios. The long leg contains stocks that experience an increase; the short leg contains stocks that experience a decrease. The long and short legs are weighted by the relevant stock's portfolio weight change, and they are held again for one month. *T*-statistics, shown in parentheses, are computed based on standard errors with Newey-West corrections of twelve lags. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

	Excess Return (1)	CAPM Alpha (2)	Three-Factor Alpha (3)	Four-Factor Alpha (4)
Panel A: Returns to Portfolios Weighted by Shares Traded				
Buy-Sell	-0.35% (-1.01)	-0.24% (-0.53)	-0.15% (-0.42)	-0.13% (-0.29)
Panel B: Returns to Portfolios Weighted by Trading Value				
Buy-Sell	-0.36% (-0.73)	-0.13% (-0.23)	-0.16% (-0.28)	-0.02% (-0.04)
Panel C: Returns to Portfolios Weighted by Portfolio Weight Changes				
Buy-Sell	-1.14% (-0.90)	-1.29% (-1.01)	-0.69% (-0.69)	-0.33% (-0.29)

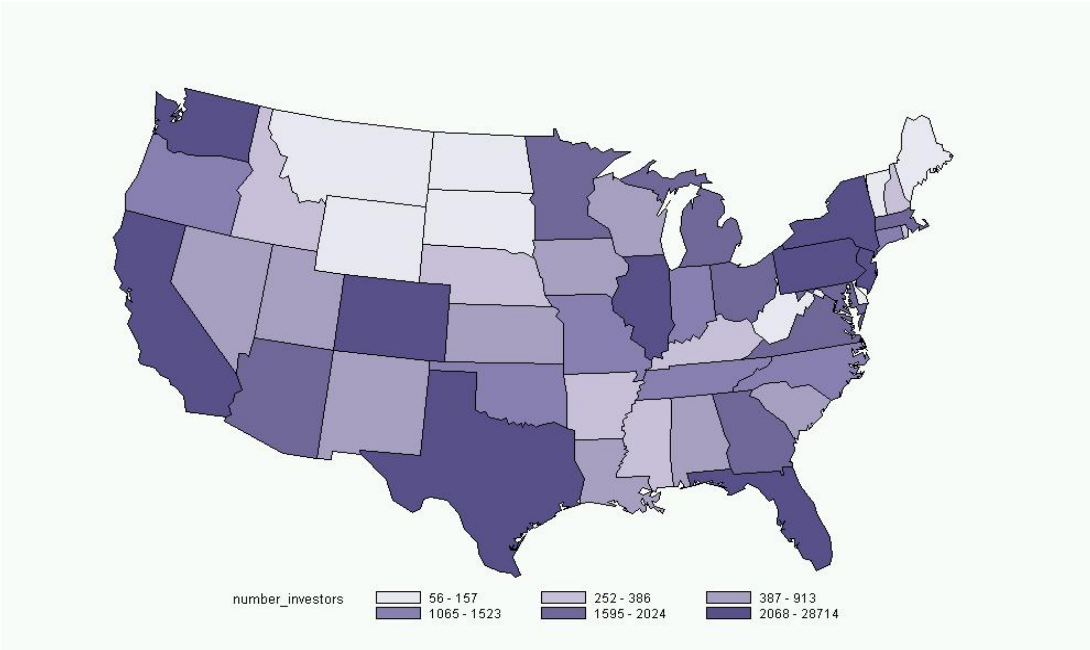

Figure 1. The Number of Investors in Each State

This figure shows the number of investors in each state in our sample. The darker the color of the block, the larger the number of investors in the corresponding state.

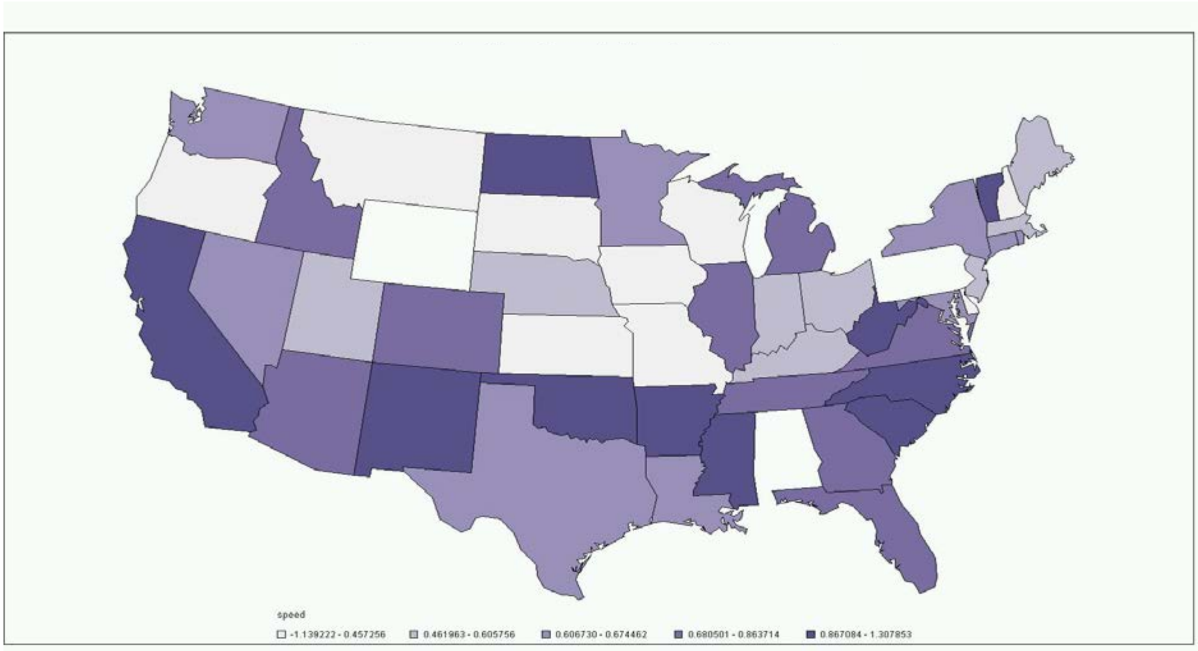

Figure 2. “Heat Map” of Contagion Rates (# Trades)

This figure shows how the contagion rate between and among investors varies across states. The contagion rate is inferred from the number of trades in the acquirer industry as a fraction of the total number of trades across all industries. The darker the color of the block, the higher the average contagion rate in the corresponding state.

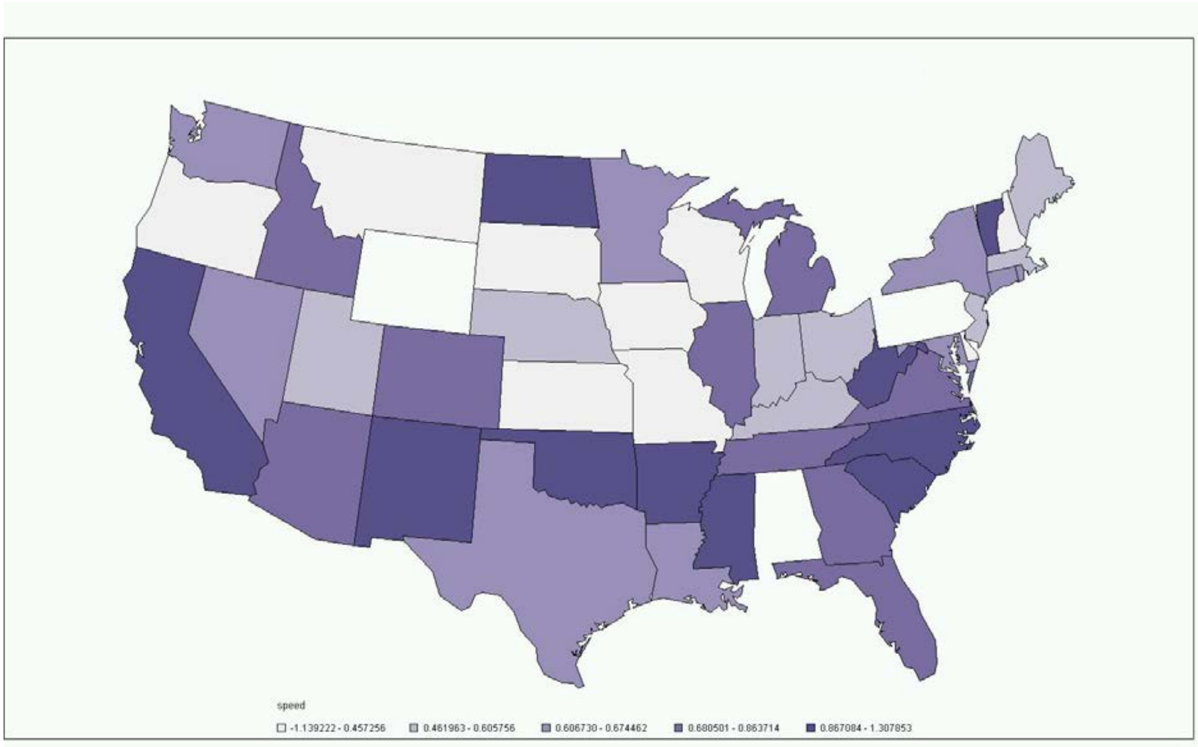

Figure 3. “Heat Map” of Contagion Rates ($ Trades)

This figure shows how the contagion rate between and among investors varies across states. The contagion rate is inferred from the dollar value of trades in the acquirer industry as a fraction of the total dollar value of trades across all industries. The darker the color of the block, the higher the average contagion rate in the corresponding state.

Online Appendix to

"What Determines Word-of-Mouth Effects in Financial Markets?"

Table A1. "Target Investors" Instrumented via Lagged One-Year Holdings

This table reports coefficient estimates from analyses similar to those reported in Tables 2 and 3. *Target Investor* now takes the value of one if an investor holds the target stock one year prior to the M&A announcement. *Target Neighbor* takes the value of one if an investor lives within three miles of such a target investor. Standard errors, shown in brackets, are clustered at the zip-code- and the year-month-of-an-M&A-announcement level. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

	Stock-Financed M&As		Cash-Financed M&As	
	# Trades (1)	$ Trades (2)	# Trades (3)	$ Trades (4)
Panel A: Target Investors				
Target Investor	0.0142***	0.0120***	0.0013	0.0013
	[0.0034]	[0.0033]	[0.0033]	[0.0033]
Investor Controls	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES
M&A Fixed Effects	YES	YES	YES	YES
Adj. R^2	1.50%	1.44%	2.35%	2.24%
# Obs.	6,943,336	6,943,336	3,220,313	3,220,313
Panel B: Target Neighbors				
Target Neighbor	0.0014**	0.0015**	-0.0001	-0.0001
	[0.0006]	[0.0007]	[0.0009]	[0.0009]
Investor Controls	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES
M&A Fixed Effects	YES	YES	YES	YES
Adj. R^2	1.50%	1.45%	2.35%	2.24%
# Obs.	6,941,105	6,941,105	3,219,641	3,219,641

Table A2. The Number of Target Investors in the Neighborhood

This table reports coefficient estimates from analyses similar to those reported in Panel B of Tables 2 and 3. The main independent variable, *# Target Investors*, now is the number of target investors who live within three miles of an investor who is not a target investor him-/herself. Standard errors, shown in brackets, are clustered at the zip-code- and the year-month-of-an-M&A-announcement level. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

	Stock-Financed M&As		Cash-Financed M&As	
	# Trades (1)	$ Trades (2)	# Trades (3)	$ Trades (4)
# Target Investors	0.0013***	0.0011***	0.0006	0.0005
	[0.0004]	[0.0004]	[0.0008]	[0.0008]
Investor Controls	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES
M&A Fixed Effects	YES	YES	YES	YES
Adj. R^2	1.66%	1.59%	2.37%	2.26%
# Obs.	7,578,642	7,578,642	3,488,558	3,488,558

Table A3. Target Neighbors' Trading in the Acquirer Firm

This table reports coefficient estimates from analyses similar to those reported in Panel B of Table 2. The dependent variable now is the number of trades in the acquirer firm itself in months 7 through 18 after the M&A is announced (Column (1)), the dollar value of trades in the acquirer firm itself (Column (2)), or an indicator variable, which takes the value of one if an investor places at least one trade in the acquirer firm itself (Column (3)). Standard errors, shown in brackets, are clustered at the zip-code- and the year-month-of-an-M&A-announcement level. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

	# Trades (1)	$ Trades (2)	I(Trades) (3)
Target Neighbor	0.0016*** [0.0005]	0.0073*** [0.0021]	0.0008*** [0.0002]
Investor Controls	YES	YES	YES
Zip Code Controls	YES	YES	YES
M&A Fixed Effects	YES	YES	YES
Adj. R^2	0.49%	0.76%	0.75%
# Obs.	7,578,642	7,578,642	7,578,642

Table A4. The Likelihood of Trading in the Acquirer Industry

This table reports coefficient estimates from analyses similar to those reported in Panel B of Table 2. The dependent variable now is an indicator variable, which takes the value of one if there is any trading in the acquirer industry in months 7 through 18 after the M&A is announced. We estimate both logit models (Column (1)) and OLS regressions (Columns (2)-(3)). For the logit models, the coefficient estimates are converted into marginal probabilities. Standard errors, shown in brackets, are clustered at the zip-code- and the year-month-of-an-M&A-announcement level. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

	Logit (1)	OLS (2)	OLS (3)
Panel A: Target Investors' Likelihood of Trading in the Acquirer Industry			
Target Investor	0.0625***	0.1028***	0.0974***
	[0.0051]	[0.0111]	[0.0109]
Investor Controls	YES	NO	YES
Zip Code Controls	YES	YES	YES
M&A Fixed Effects	YES	YES	YES
Adj. R^2	4.62%	0.06%	2.44%
# Obs.	7,580,936	7,580,936	7,580,936
Panel B: Target Neighbors' Likelihood of Trading in the Acquirer Industry			
Target Neighbor	0.00635***	0.0133***	0.0071***
	[0.0015]	[0.0019]	[0.0017]
Investor Controls	YES	NO	YES
Zip Code Controls	YES	YES	YES
M&A Fixed Effects	YES	YES	YES
Adj. R^2	4.61%	0.05%	2.43%
# Obs.	7,578,642	7,578,642	7,578,642

Table A5. Buy vs. Sell Transactions

This table reports coefficient estimates from regressions of target neighbor trading in the acquirer industry on target investor trading in the acquirer industry. The dependent variable in Columns (1) and (3) is a target neighbor's number of buy (sell) trades in the acquirer industry (excluding the acquirer firm) as a fraction of the total number of buy (sell) trades across all industries in months 7 through 18 after the M&A is announced. The dependent variable in Columns (2) and (4) is a target neighbor's dollar value of buy (sell) trades in the acquirer industry (excluding the acquirer firm) as a fraction of the total dollar value of buy (sell) trades across all industries in months 7 through 18 after the M&A is announced. The main independent variable, *Target Investor Trading*, is the corresponding target investor's total number or total dollar value of buy (sell) trades in the acquirer industry (excluding the acquirer firm) as a fraction of the total number or total dollar value of buy (sell) trades across all industries in months 7 through 18 after the M&A is announced. Standard errors, shown in brackets, are clustered at the zip-code- and the year-month-of-an-M&A-announcement level. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

	Buy		Sell	
	# Trades (1)	$ Trades (2)	# Trades (3)	$ Trades (4)
Target Investor Trading	0.0175*** [0.0056]	0.0173*** [0.0057]	0.0080** [0.0041]	0.0079* [0.0044]
Investor Controls	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES
M&A Fixed Effects	YES	YES	YES	YES
Adj. R^2	1.64%	1.44%	2.35%	2.24%
# Obs.	7,580,936	7,580,936	7,580,936	7,580,936

Table A6. Pseudo-Target Investors' and Pseudo-Target Neighbors' Trading in the Acquirer Industry

This table reports coefficient estimates from analyses similar to those reported in Tables 2 and 3. Rather than examine the trading behavior of target investors and target neighbors, in Panels A and B, we now consider the trading behavior of pseudo-target investors and pseudo-target neighbors. Specifically, for each M&A, we identify the industry peer that has the closest market capitalization and book-to-market ratio to the actual target firm and that is not being acquired itself (≡ "pseudo target firm"). We then examine whether current shareholders of the pseudo target firm and their neighbors change their trading behavior vis-à-vis the acquirer industry. In Panels C and D, we consider only investors who trade or hold stocks in the acquirer industry in the year prior to the M&A announcement (and, as a result, are much less likely to be positively "shocked" by the endowment of acquirer firm shares). Standard errors, shown in brackets, are clustered at the zip-code- and the year-month-of-an-M&A-announcement level. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

	Stock-Financed M&A		Cash-Financed M&A	
	# Trades (1)	$ Trades (2)	# Trades (3)	$ Trades (4)
Panel A: Pseudo Target Investors' Trading in the Acquirer Industry				
Pseudo Target Investor	0.0006 [0.0018]	-0.0006 [0.0019]	-0.0009 [0.0028]	-0.0003 [0.0030]
Investor Controls	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES
M&A Fixed Effects	YES	YES	YES	YES
Adj. R^2	1.66%	1.59%	2.36%	2.25%
# Obs.	7,558,105	7,558,105	3,476,999	3,476,999
Panel B: Pseudo Target Neighbors' Trading in the Acquirer Industry				
Pseudo Target Neighbor	-0.0003 [0.0006]	-0.0003 [0.0006]	0.0005 [0.0008]	0.0004 [0.0008]
Investor Controls	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES
M&A Fixed Effects	YES	YES	YES	YES
Adj. R^2	1.66%	1.59%	2.36%	2.25%
# Obs.	7,555,604	7,555,604	3,475,477	3,475,477

Table A6. Continued.

Panel C: Target Investors' Trading in the Acquirer Industry among Investors who Trade or Hold Stocks in the Acquire Industry in the Year prior the M&A				
Target Investor	0.0048 [0.0050]	0.0009 [0.0052]	-0.0015 [0.0096]	-0.0040 [0.0098]
Investor Controls	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES
M&A Fixed Effects	YES	YES	YES	YES
Adj. R^2	8.83%	8.68%	9.87%	9.68%
# Obs.	1,551,059	1,551,059	587,642	587,642

Panel D: Target Neighbors' Trading in the Acquirer Industry among Investors who Trade or Hold Stocks in the Acquire Industry in the Year prior the M&A				
Target Neighbor	-0.0034 [0.0029]	-0.0040 [0.0030]	0.0026 [0.0055]	0.0044 [0.0059]
Investor Controls	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES
M&A Fixed Effects	YES	YES	YES	YES
Adj. R^2	8.83%	8.68%	9.87%	9.68%
# Obs.	1,549,568	1,549,568	587,323	587,323

Table A7. Past Investor Performance and the Size of Contagion

This table reports coefficient estimates from regressions of investor trading in the acquirer industry on a target neighbor dummy. The regressions are identical to those in Panel B of Table 2, but we now estimate the regressions separately for various subsamples. In Panel A, we sort target neighbors based on the corresponding target investors' portfolio returns in the quarter prior to the M&A announcement. In Panel B, we sort target neighbors based on target neighbors' portfolio returns in the quarter prior to the M&A announcement. "High" and "Low" represent top- and bottom-half observations, respectively. Standard errors, shown in brackets, are clustered at the zip-code- and the year-month-of-an-M&A-announcement level. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

	High		Low	
	# Trades (1)	$ Trades (2)	# Trades (3)	$ Trades (4)
Panel A: The Effect of Target Investors' Past Portfolio Performance				
Target Neighbor	0.0027***	0.0027***	0.0015*	0.0014
	[0.0010]	[0.0010]	[0.0009]	[0.0009]
Investor control	YES	YES	YES	YES
Zip Code control	YES	YES	YES	YES
M&A Fixed Effects	YES	YES	YES	YES
Adj. R^2	1.65%	1.58%	1.65%	1.58%
# Obs.	7,558,300	7,558,300	7,560,827	7,560,827
Panel B: The Effect of Target Neighbors' Past Portfolio Performance				
Target Neighbor	0.0037***	0.0036***	0.0001	0.0001
	[0.0010]	[0.0010]	[0.0008]	[0.0008]
Investor control	YES	YES	YES	YES
Zip Code control	YES	YES	YES	YES
M&A Fixed Effects	YES	YES	YES	YES
Adj. R^2	1.65%	1.58%	1.65%	1.58%
# Obs.	7,559,069	7,559,069	7,560,058	7,560,058

Table A8. Social Characteristics and the Size of Contagion

This table reports coefficient estimates from regressions of investor trading in the acquirer industry on a target neighbor dummy. The regressions are identical to those in Panel B of Table 2, but we now estimate the regressions separately for various subsamples. In Panel A, we consider three indices of sociability from Putnam (2000): (1) seminar or class attendance; (2) club meeting attendance; (3) community project participation. We sort target neighbors based on whether the corresponding target investor and target neighbors reside in a state with above-median sociability, or below-median sociability. In Panel B, we sort target neighbors based on whether the corresponding target investor's length of residency at his/her current address is above five years ("High"), or below ("Low"). We choose the five-year cutoff to ensure that we have roughly the same number of observations in each group. In Panel C, we focus on target neighbors residing in metropolitan areas and we sort target neighbors based on whether the corresponding target investor's metropolitan area has a population size that sits above the 75th percentile of its distribution ("High"), or below ("Low"). We choose the 75th-percentile cutoff to ensure that we have roughly the same number of observations in each group. Standard errors, shown in brackets, are clustered at the zip-code- and the year-month-of-an-M&A-announcement level. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

	>= Median State		< Median State	
	# Trades (1)	$ Trades (2)	# Trades (3)	$ Trades (4)
Panel A1: Seminar or Class Attendance				
Target Neighbor	0.0028***	0.0027***	-0.0008	-0.0008
	[0.0009]	[0.0009]	[0.0011]	[0.0011]
Panel A2: Club Meeting Attendance				
Target Neighbor	0.0039***	0.0039***	0.0003	0.0002
	[0.0011]	[0.0011]	[0.0009]	[0.0009]
Panel A3: Community Project Participation				
Target Neighbor	0.0030***	0.0030***	0.0005	0.0004
	[0.0009]	[0.0009]	[0.0010]	0.0010
Investor Controls	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES
Event-Fixed Effects	YES	YES	YES	YES

Table A8. Continued.

	High		Low	
	# Trades (1)	$ Trades (2)	# Trades (3)	$ Trades (4)
Panel B: Target Investors' Length at Current Residence				
Target Neighbor	0.0026*** [0.0008]	0.0027*** [0.0008]	0.0010 [0.0020]	0.0006 [0.0021]
Investor control	YES	YES	YES	YES
Zip Code control	YES	YES	YES	YES
M&A Fixed Effects	YES	YES	YES	YES
Adj. R^2	1.73%	1.66%	1.73%	1.56%
# Obs.	6,711,168	6,711,168	6,689,865	6,689,865
Panel C: Population Density				
Target Neighbor	0.0011 0.0010	0.0011 0.0010	0.0026** [0.0012]	0.0025** [0.00012]
Investor control	YES	YES	YES	YES
Zip Code control	YES	YES	YES	YES
M&A Fixed Effects	YES	YES	YES	YES
Adj. R^2	1.99%	1.94%	1.73%	1.64%
# Obs.	1,436,074	1,436,074	1,510,209	1,510,209

Table A9. Income Group Definition

Our data vendor groups households into one of the following nine *bins* based on their income:

bin = 1: household income < $15,000;
bin = 2: $15,000 ≤ household income ≤ $19,999;
bin = 3: $20,000 ≤ household income ≤ $29,999;
bin = 4: $30,000 ≤ household income ≤ $39,999;
bin = 5: $40,000 ≤ household income ≤ $49,999;
bin = 6: $50,000 ≤ household income ≤ $74,999;
bin = 7: $75,000 ≤ household income ≤ $99,999;
bin = 8: $100,000 ≤ household income ≤ $124,999;
bin = 9: $125,000 ≤ household income.

Table A10. Alternative Definitions of *Target Neighbor* and Alternative Time Horizons

This table reports coefficient estimates from regressions of investor trading in the acquirer industry on a target investor- or a target neighbor dummy. We experiment with alternate definitions of what constitutes a target neighbor and we examine trading over alternative time horizons. We also consider differences in trading in months 7 through 18 versus trading in months 1 through 6 after the M&A is announced. Standard errors, shown in brackets, are clustered at the zip-code- and the year-month-of-an-M&A-announcement level. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

	# Trades (1)	$ Trades (2)	# Trades (3)	$ Trades (4)	# Trades (5)	$ Trades (6)	# Trades (7)	$ Trades (8)
Panel A: Neighbors of Different Distances to Target Investors								
	0 to 3 Miles		3 to 7 Miles		7 to 15 Miles		15 to 30 Miles	
Target Neighbor	0.0022***	0.0021***	0.0018***	0.0018***	0.0014***	0.0015***	0.0002	0.0002
	[0.0007]	[0.0007]	[0.0005]	[0.0005]	[0.0003]	[0.0003]	[0.0003]	[0.0003]
Investor Controls	YES	YES	YES	YES	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES	YES	YES	YES	YES
M&A Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Adj. R^2	1.66%	1.59%	1.66%	1.59%	1.65%	1.59%	1.65%	1.58%
# Obs.	7,578,642	7,578,642	7,558,105	7,558,105	7,485,049	7,485,049	7,336,619	7,336,619
Panel B: Alternative Time Horizons								
	Target Investors				Target Neighbors			
	Months 19 to 30		Months 31 to 42		Months 19 to 30		Months 31 to 42	
Target Investor/ Target Neighbor	0.0178***	0.0130***	0.0123***	0.0107***	0.0005	0.0008	0.0001	0.0005
	[0.0030]	[0.0026]	[0.0035]	[0.0032]	[0.0006]	[0.0006]	[0.0007]	[0.0007]
Investor Controls	YES	YES	YES	YES	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES	YES	YES	YES	YES
M&A Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Adj. R^2	1.47%	1.39%	1.28%	1.21%	1.47%	1.39%	1.28%	1.21%
# Obs.	5,814,983	5,814,983	3,696,168	3,696,168	5,812,950	5,812,950	3,694,682	3,694,682
Panel C: Trading in Months 7-18 minus Trading in Months 1-6								
	Target Investors				Target Neighbors			
	Stock-Financed M&As		Cash-Financed M&As		Stock-Financed M&As		Cash-Financed M&As	
Target Investor/ Target Neighbor	0.0122***	0.0118***	0.0089*	0.0091*	0.0025***	0.0026 **	0.0008	0.0006
	[0.0038]	[0.0038]	[0.0051]	[0.0051]	[0.0007]	[0.0007]	[0.0011]	[0.0011]
Investor Controls	YES	YES	YES	YES	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES	YES	YES	YES	YES
M&A Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Adj. R^2	1.42%	1.38%	2.06%	1.99%	1.41%	1.37%	2.06%	1.98%
# Obs.	4,892,588	4,892,588	2,283,907	2,283,907	4,890,872	4,890,872	2,283,329	2,283,329