The Speed of Communication[*]

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The Speed of Communication

Abstract

Drawing from research on disease contagion, we estimate a transmission matrix to *quantify* how the speed at which information (or noise) travels through the investor population varies with distances in social characteristics (such as age, income, and gender). We utilize cross-industry stock-financed mergers and acquisitions as a source of plausibly exogenous variation in investors' information-gathering activity. In particular, we conjecture that, once "infected" with shares of an acquiring firm, target investors are more likely to study and trade in an acquirer industry; target investors also spread any newly acquired industry views to their neighbors. Tracing the path of contagion via investors' trading behavior, we estimate that, regarding any relevant investor pair, a ten-year difference in age, a one-step difference in income, and being of different genders lowers the effective communication rate between the investor pair by 12%, 2%, and 17%, respectively. In addition, the effective communication rate from older, wealthier, female investors to their younger, poorer, male counterparts is noticeably higher than the effective communication rate that runs in the reverse direction.

JEL Classification: G11, G12, G14, G20

Key words: Word-of-Mouth, Speed of Communication, Household Investment

1. Introduction

The question how information, or noise, travels through the marketplace is at the heart of asset pricing, and economics in general. One channel that has long been thought to play a primary role in disseminating information is word-of-mouth communication. For instance, Ellison and Fudenberg (1995) note that "economic agents must often make decisions without knowing the costs and benefits of the possible choices" and thus "rely on whatever information they have obtained via casual word-of-mouth communication." Shiller (2000), in his *New York Times* bestseller, argues that word-of-mouth transmission of ideas can be an important source of short-term fluctuations in the stock market.

Corroborating these views, several recent studies document a positive correlation in stock trading activity across investors who are likely to be in direct contact with one another. Using physical proximity to measure the likelihood of contact and communication, Hong, Kubik, and Stein (2005), for instance, find that mutual fund managers increase their stock purchases (sales) when other managers from other fund families in the same city increase their purchases (sales) of the same stock.[1]

While these findings are consistent with the view that word-of-mouth communication affects investor behavior, they are silent on an important, and perhaps more interesting, follow-up question: if information spreads via word of mouth, how *quickly* does it spread and what factors, and to what degree, determine the speed of communication? To draw an analogy to epidemiology, if we think of the initial person with a disease as the infected and those around him/her as the susceptible, it is important to know whether the disease is contagious or not.[2] It is perhaps even more important to know the exact contagion rate and what factors, and to what degree, determine that rate.

From a policy/practical perspective, knowledge of the speed of communication and its determinants can help organizations design strategies for effectively disseminating information to their target audiences (e.g., information about the availability of small business loans or government-

[1] Relatedly, Ivkovich and Weisbenner (2007) find that when retail investors purchase (sell) a stock from a certain industry, other retail investors in the neighborhood increase their purchases (sales) of stocks in the same industry.

[2] Needless to say, there are important differences between disease transmission and the spread of information: in the former case people have a desire to avoid contact with the infected, while in the latter case people have an incentive to learn about the information. Such differences will be reflected in the speed of communication and its determinants.

sponsored healthcare programs). From a theoretical perspective, such knowledge can help researchers calibrate models that examine the effect of communication between economic agents on aggregate economic outcomes (e.g., prices, growth, and investment).

To quantify the rate at which economic agents communicate with one another, we need to identify the source of information so as to map out the path of "contagion." This is similar to identifying "patient zero" in studies of disease transmission. The ideal experiment would be to randomly pick an investor ("patient zero") and have that investor research and trade stock S. The rate at which other investors (with various social characteristics) in the nearby neighborhood start trading stock S, or related firms, would then inform the researcher about the speed and social determinants of communication between investors.

Motivated by this hypothetical ideal design, we exploit *cross-industry stock-financed* mergers and acquisitions (M&As) as a source of plausibly exogenous variation in investors' information-gathering activity. In particular, we exploit the fact that, at the completion of a cross-industry stock-financed M&A, investors of the target firm (from industry X) receive shares of the acquiring firm (from industry Y). We conjecture that, once endowed with shares of the acquiring firm, target investors more carefully study the acquirer industry, which leads to an increase in target investors' trading activity in the acquirer industry (outside of the acquiring firm). This may occur because investors become curious about the acquirer industry once having received shares of the acquirer firm. Perhaps more importantly, there is a fixed cost involved in learning about an industry and, prior to the acquisition, target investors may have perceived that the incremental benefit from trading firms in the acquirer industry falls below that fixed cost. Since target investors now are forced to think carefully about when to sell their holdings in the acquirer firm, the fixed cost no longer represents a barrier to trading firms in the acquirer industry.

If target investors more carefully study the acquirer industry and if target investors communicate their views to other investors in the same neighborhood via word-of-mouth, we may observe increased trading activity in the acquirer industry not only by target investors but also by their neighbors and their neighbors' neighbors. Tracing the path of the "contagion" of trading in the acquirer industry then enables us to quantify the speed of communication between investors and to estimate how that speed varies with distances in social characteristics between investors.

To implement our empirical tests, we collect data on all cross-industry M&A deals from 1991 through 1996 and we match these deals to detailed trading records of 78,000 US households from a discount brokerage.[3] We separate cross-industry M&A transactions into those that are stock-financed and those that are cash-financed: the former are defined as deals that are at least 50% equity-financed; the latter are 100% cash-financed. The *cash-financed* M&As serve as our counterfactual. In cash-financed M&As, target investors receive cash as opposed to shares in the acquirer firm and, as such, are less incentivized to study the acquirer industry. After each cross-industry *stock-financed* M&A, we track the trading behavior of target investors in the acquirer industry, excluding the acquirer firm itself to eliminate any mechanical effect (target investors are bound to sell their holdings in the acquirer firm sooner or later). We repeat this exercise for non-target investors who live within three miles of the target investors.

Our first set of tests provides initial evidence within a simple static setting. Our empirical analysis reveals that in the year after the completion of a cross-industry stock-financed M&A, target investors, compared with other investors, more than double their trading intensity in the acquirer industry. This spike in trading intensity extends to neighbors of target investors who increase their trading in the acquirer industry by more than 11%. Consistent with the hypothesis that face-to-face social interaction plays an important role in trading, the neighbor effect becomes statistically and economically weaker as we expand the physical distance between target investor and target neighbor.

In a series of placebo tests to rule out alternative interpretations, we find that our documented effect completely disappears when we examine investors' trading around cash-financed M&As. Our results also disappear when we examine investors of pseudo-target firms, which we define as industry peers of target firms that are of similar size and book-to-market ratio as the target firms. Together, these findings provide fairly clean evidence of the presence of a word-of-mouth effect.

Our second and main set of tests draws from research on disease transmission. (To keep the presentation focused, we defer a full discussion of the methodology to the main body of the text.)

[3] We discuss in detail in Section 2 the various advantages and disadvantages of focusing on this particular sample of investors in our analysis.

Essentially, we estimate an N by N transmission matrix, *B*, from one period to the next (where N is the number of investors). The (*i*,*j*)th element in this matrix captures the impact of investor *i*'s behavior on investor *j* over one period, and vice versa for the (*j*,*i*)th element. To examine the effect of communication over *P* periods, we simply raise the matrix B to the power of *P*. The dynamic approach provides an important advantage over the static method because it explicitly accounts for indirect links between investors. That is, the dynamic approach accounts for the possibility that investor *i* transmits his/her view to investor *j* through a common third tie *m*, without being in direct contact with investor *j*.

To estimate the transmission matrix, we trace investors' trading activity, following an M&A, in the acquirer industry. Each period represents one quarter and we study the four quarters following each M&A. For tractability, we impose a linear structure on all the elements in the transmission matrix. That is, we assume that $B_{i,j}$ is a linear function of physical and social distances between households *i* and *j*. The residual term captures the unobserved determinants of the effective communication rates. We consider the following three social characteristics: income, age, and gender. Like a parallel matrix in epidemiology, the transmission matrix *B* reflects the product of the likelihood of communication between investors (the "contact rate") and the probability that investors act on the advice of their neighbors (the "transmission rate" given contact). Without differentiating between these two components, we focus our analysis on the *effective* communication rate.

Our results suggest that the contagion rate in our setting is far below the rates of some of the most intensely studied diseases such as HIV/AIDS, SARS, and Ebola. In other words, unlike infectious diseases, which require counter-measures such as isolation or vaccinations to contain an epidemic, industry information gathered by "patient zero" dies out on its own over the course of one year. We observe strong regional differences in the effective communication rate, however, and we find that our effective communication rate, estimated by reference to investors' trading behavior, strongly positively correlates with responses from surveys on regional differences in how frequently one takes advice from a friend.

Additional tests reveal that for any pair of investors a ten-year difference in age, a one-step difference in income (as defined by the brokerage firm that supplied the data), and being of different genders lowers the effective communication rate by 12%, 2%, and 17%, respectively. To the best of our knowledge, we are the first to *quantify* the decreasing effects that stem from differences in age, income, and gender on the effective communication rate and it appears interesting that differences in age and gender represent much greater barriers to communication than differences in socio-economic background (as we detail in the main body of the text, a one-step difference in income represents an economically meaningful gap).

We further uncover some interesting asymmetries. In particular, our results indicate that the effective communication rate of information from older, wealthier, female investors to their younger, less wealthy, male neighbors is noticeably higher than the effective communication rate that runs in the reverse direction. For instance, we find evidence that less wealthy investors are 20%–30% more likely to act on the opinions of wealthier investors than the other way around. An important practical implication of these asymmetries is that organizations wishing to transmit a message in an efficient or cost-effective manner should consider targeting older, wealthier, female members of the community who are the most likely to spread messages to other members of the community via word of mouth.

In our final test, we examine whether investors, through casual conversation, transmit value-relevant information that has not been factored into prices or simply spread noise. We examine this issue by constructing long–short portfolios tracking target investors' and target neighbors' trading decisions. We find that, across a number of specifications, following an M&A, stocks in the acquirer industry bought by target investors and their neighbors subsequently underperform stocks sold by these investors. These results suggest that in our setting retail investors do not spread value-relevant information via word-of-mouth communication.

2. Data

2.1. Data Sources and Descriptive Statistics

We use two primary data sources in this study. First, we obtain detailed trading and holdings records for a subsample of US households for 1991–1996 from a discount brokerage firm. Our dataset comprises three files. We extract information on investor trading in common stocks from the "transaction file." We obtain their daily holdings from the "position file." Finally, we obtain various household/investor characteristics, such as age, income, gender (of the household head), and location (zip code) from the "information base file." These three files can be linked via a unique household identifier and a brokerage account number. Note that one household can have multiple accounts at the brokerage firm. Our analyses are conducted at the household level; that is, we aggregate all accounts held by the same household into one observation. Going forward, we use the terms "households" and "investors" interchangeably. For further details on this database, we refer the reader to Barber and Odean (2000).

We match the trading and holdings 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 Security Price (CRSP) delisting file. We require that the acquirer firm and the target firm reside in separate industries, whereby industries are defined based on the Fama-French 49-industry classification. Using alternative industry classifications, such as the Fama-French 38- and 30-industry classifications and the GICS industry classification, 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: the former are defined as deals that are at least 50% equity-financed; the latter are 100% cash-financed.

Our final sample contains 460 M&As from 1991 through 1996, of which 317 are stock-financed and 143 are cash-financed. Panel A of Table 1 reports 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.

When matching household trading records to M&A transactions, 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 after the M&A. We exclude households in states where the target or acquirer firm has any business operation—identified using both headquarters and factory locations.[4] We further require that these households have no existing positions in the acquirer industry prior to the M&A announcement. We do so to avoid counting "mechanical" trading in the subsequent period due to hedging or rebalancing reasons; in

[4] We thank Alok Kumar for sharing his data on firms' headquarters and factory locations.

particular, target investors with prior holdings in the acquirer industry may "mechanically" sell such holdings upon receiving acquirer shares to reduce their exposure to the acquirer industry.

We settle on a sample of about 70,000 investor accounts (culled from around 150,000 in the original sample). Panel B of Table 1 provides 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 his/her portfolio and places 0.47 trades a month, with the average monthly trade value being $5,679. The average investor age in our sample is 42 and the average annual household income is $69,500.

The brokerage database contains the zip code of the investor's home, which allows us to compute the distance between any two investors using the longitude and latitude associated with each zip code adjusted for curvature.[5] We augment this dataset with geographic information from the US Census Bureau's zip code database, which includes the population and the average household income for each zip code.

We also categorize US zip codes based on various measures of sociability. Like those used by Ivkovich and Weisbenner (2007), our sociability indices are taken from the DDB lifestyle survey for the years 1975 through 1998. The DBB survey has been used in a number of sociology studies (e.g., Putnam (2000)). Since DBB conducts each iteration of the survey at the state level (i.e., there is an aggregate score for each state), we assign the same score to all zip codes within a state.

2.2. Discussion

The backbone of our analysis is a detailed dataset on individual investor trading activity in the early 1990s. This setting is appealing for a number of reasons: First, the median retail investor in this sample holds three stocks. As such, substituting any one position with another stock from an entirely separate industry is likely to have a significant impact on investors' information-gathering activity.

Second, our empirical design requires a simple environment in which we can cleanly measure the "distance" between any two investors. With the emergence of the Internet, physical distances no

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

longer represent a meaningful barrier to communication. Our sample, which predates the Internet age, does not suffer from this problem. Relatedly, our dataset contains detailed information on the physical locations and social characteristics of the investors, both of which we require to set up our tests.

Our empirical setting is, however, also subject to several caveats. First, the landscape of the U.S. equity market has changed dramatically over the past three decades. The fraction of shares held directly by retail investors has steadily decreased from nearly 50% in 1990 to less than 20% today. This raises questions as to whether we can extrapolate our retail investor based results to today's marketplace.

Second, 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 is likely to comprise households that are better connected to one another than U.S. households distributed over multiple brokers. This introduces an *upward bias* in our estimate of the effective communication rate.

Third, the average rate of communication is constantly evolving. As noted above, with advancements in information technology and social media, more and more daily social interactions have shifted online. As a result, the speed of communication likely is at least an order of magnitude higher today than it was during our sample period. Despite the upward-bias noted above, we therefore believe that whatever estimate we arrive at in this study is an overall downward-biased estimate of today's effective communication rate.

The above considerations pertain primarily to the estimation of the baseline effective communication rate. Much of our analysis focuses on how the effective communication rate varies with social distances (e.g., what is the average percentage drop in the effective communication rate when the age gap between two investors increases by ten years?). To the extent that there are inherent, persistent components in social structures and norms (e.g., the tendency to interact with people near one's age), the above factors are less concerning and the determinants of the effective communication rate that we estimate in this study are likely to generalize to multiple investor groups as well as across time.

3. A Static Setting

We begin our empirical analysis by providing some baseline evidence for the effects of social interactions on investor trading behavior. Our innovation is to use cross-industry stock-financed M&As as a source of plausibly exogenous variation in investors' information-gathering activity. Section 3.1 examines changes in target investors' trading behavior in the acquirer industry around an M&A and Section 3.2 extends the analysis to their neighbors. Sections 3.3 through 3.5 then conduct additional analyses and consider alternative interpretations.

3.1 Target Investors

We argue that target investors start collecting information about the acquirer industry once they become owners of an acquirer firm. Since we do not directly observe investors' information-gathering activity, we focus instead on their trading decisions, which are driven by their information sets. We estimate the following regression equation:

$$Trading_{i,m,Acq} = a_0 + a_1\, Target\_Investor_{i,m} + CONTROL * \gamma + \varepsilon_{i,j,t}, \quad (1)$$

where $Trading_{i,m,Acq}$ is trading by investor i in the acquirer industry as a fraction of his/her total trading across all industries after stock-financed M&A m. Trading is measured by both the number of trades and the dollar value of trades. Since the exact completion date is missing for many M&A deals, we examine trading behavior in months seven through eighteen after the M&A announcement day as, on average, it takes six months for an M&A to complete (Giglio and Shue, 2014).

As discussed in Section 2.1, we exclude acquirer firms from our calculations of trading to avoid any mechanical effect (again, target investors are bound to sell their holdings in an acquirer firm sooner or later). We further require that target investors in our sample do not hold stocks in the acquirer industry before an M&A announcement to preclude trading in the acquirer industry in the post-M&A period for hedging or rebalancing. We also exclude investors from states where the target or acquirer firms have headquarters or factories.

The main independent variable in the regression is $Target\_Investor_{i,m}$, which is an indicator that takes the value of one if investor i holds shares in the target firm in the month prior to the M&A

announcement. The control variables fall into one of two groups: investor/household characteristics and demographics at the zip code level. The former includes household income, number of children, and number of family members as well as the investor's age, gender, and marital status. The latter includes the zip code population, the fraction of male residents, the average home value, the average number of household members, and average household income. We include M&A fixed effects to absorb any M&A-specific effects. The standard errors are clustered at the zip-code and year-month levels.

We report the regression results in Panel A of Table 2. The dependent variable shown in the first four columns is the trading intensity in the acquirer industry based on the *number* of trades; the dependent variable shown in the next four columns is based on the *dollar value* of trades. As shown in Column (1), target investors increase their trading intensity in the acquirer industry by an incremental 2.48% compared with other investors (*t*-statistic = 5.40). To put this number in perspective, the unconditional trading in any industry is 2.04% (=100%/49). That is, ownership of acquirer stocks induces target investors to more than double their normal trading activities in the acquirer industry. Furthermore, as can be seen in Columns (2)–(4), neither controlling for investor characteristics and demographics nor including M&A-fixed effects has any significant impact on our results. The regression coefficients reported in Columns (5)–(8), which are based on the dollar-weighted measure of trading intensity, are almost identical to those reported in Columns (1)–(4).

As a placebo test, we repeat our analyses for cash-financed M&As. If our results are driven by the direct impact of M&As on investor beliefs and preferences (e.g., a deal causes media outlets to jointly discuss the acquirer and target industries), we should observe a similar change in trading intensity around cash-financed M&As. In contrast, if stock ownership induces investors to collect more information about the acquirer industry, we should observe no effect associated with cash-financed M&As.

The results are reported in Panel B. The coefficients are only one-fourth of those reported in Panel A and they are far from being statistically significant. Taken together, the results reported in this section support our prediction that, once endowed with shares of an acquiring firm, target investors more carefully study the acquirer industry, which leads to an increase in target investors' trading activity in the acquirer industry (outside of the acquiring firm).

3.2 Target Neighbors

We now turn our attention to the neighbors of the target investors. Unlike prior studies that examine the relationship between local investors and firms, we use a rather narrow definition of neighbors—investors who live within a three-mile (as opposed to 60-mile) radius of a target investor. We do this because the likelihood that two individuals come into direct contact with each other diminishes rapidly with distance. We impose the same data requirements as in the previous subsection and we estimate a regression equation that is almost identical to equation (1):

$$Trading_{i,m,Acq} = a_0 + a_1\, Target\_Neighbor_{i,m} + CONTROL * \gamma + \varepsilon_{i,j,t}, \quad (2)$$

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 him-/herself. If an investor lives within three miles of more than one target investor, we count that investor only once. In unreported analyses, we assign greater weight to neighbors of multiple target investors and the results are by and large unchanged.

Panel A of Table 3 reports target neighbors' trading behavior related to stock-financed M&As. As in Table 2, the dependent variable shown in the first four columns of Panel A is trading intensity in the acquirer industry based on the number of trades, while the dependent variable shown in the next four columns is trading intensity in the acquirer industry based on the dollar value of trades. As can be seen from Column (1), neighbors who live within three miles of a target investor disproportionately increase their trading intensity in the acquirer industry by 39bps after the M&A (*t*-statistic = 4.88). When controlling for investor characteristics and demographics as well as M&A fixed effects, the coefficient estimate for *Target_Neighbor* turns to 23bps (*t*-statistic of 3.29). That is, target neighbors increase their trading intensity by over eleven percent of the unconditional trading intensity in a given industry, which is 2.04%. The results based on the dollar value of trades, shown in the next four columns, are identical to those reported in the first four columns. For example, the coefficient estimate for *Target_Neighbor* in the full specification is now 22bps (*t*-statistic of 3.14).

Comparing the results shown in Panel A of Table 2 with those shown in Panel A of Table 3, we observe that the effect of stock-financed M&As on target investors' trading intensity is about ten times as large as that on target neighbors' trading intensity (2.30% vs. 23bp). This difference in magnitude is consistent with findings reported in prior word-of-mouth studies. Hong, Kubik, and Stein (2004), for instance, find that "*a given 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 purchase of the same stock by 1 percentage point*." Similarly, Ivkovic and Weisbenner (2007) report that "*a ten percentage point increase in neighbors' purchases of stocks from an industry is associated with a two percentage point increase in households' own purchases of stocks from that industry*," and they attribute "*approximately one-quarter to one-half of the correlation between households' stock purchases and stock purchases made by their neighbors to word-of-mouth communication*."

To better understand the difference in trading behavior between target investors and target neighbors, we decompose the unconditional trading intensity into a) the probability that an investor trades in an acquirer industry after merger completion (i.e., the probability the investor becomes "infected") and b) the trading intensity once "infected." We estimate similar regression equations to the ones shown in Tables 2 and 3, but the dependent variable is now an indicator that equals one if an investor places any trades in an acquirer's industry in months seven through eighteen after an M&A is announced, and zero otherwise.

As can be seen in Table 4, irrespective of whether we estimate logit regressions or OLS regressions, our results indicate that the probability of reporting a trade in an acquirer industry in the post-merger-completion period (i.e., the probability of becoming infected) is six to nine times higher for target investors than for target neighbors. The similarity to the previously observed ten-to-one ratio in trading intensity between target investors and target neighbors indicates that, once "infected," target neighbors exhibit a trading intensity that is similar to that of target investors.

As a placebo test, we again repeat the whole set of analyses for cash-financed M&As. If neighbors of target investors increase their trading in an acquirer industry because the M&A directly influences neighbors' beliefs or preferences (through, for instance, joint media coverage of the acquirer and target industries), we should observe a similar pattern in trading in cash-financed M&As. In contrast, if neighbors of target investors increase their trading because of word-of-mouth effects, we expect cash-financed M&As to have no impact on neighbors' trading decisions. The regression results shown in Table 4 are consistent with the latter prediction. The coefficient estimate for *Target_Neighbor* in the full specification (Columns (4) and (8)) is almost zero, with a *t*-statistic lower than 0.3.

We also consider investors who, at the time of an M&A, hold shares in the target industry, but not the target firm itself. In particular, for each M&A event, we identify the industry peer with the closest market capitalization and book-to-market ratio to the actual target firm (≡ "pseudo target firm"). We then examine whether current shareholders of the pseudo target firm and their neighbors change their trading in the acquirer industry. The results are reported in Table 5. As we found in the placebo test based on cash-financed M&As, we observe no increase in trading activity in the acquirer industry for shareholders of the pseudo target firms and their neighbors.

3.3 Additional Tests

3.3.1. Population density

We conjecture that the members of an investor pair living within a three-mile radius in a less populated area, such as certain areas in Upstate New York, are more likely to interact with each other than the members of an investor pair living within a three-mile radius in a more populated area, such as New York City. In Appendix Table A1, we divide all zip codes into two groups: the top quartile in population and the bottom three quartiles. We use a quartile cutoff to ensure that we have similar numbers of investors in both groups; naturally, there are many more investors in high-population areas than in low-population areas. We re-estimate our main regression equation separately for each of the two groups. Consistent with our conjecture, we find that the coefficient estimate for *Target_Neighbor* is more than twice as large for the less-populated areas than for the more-populated areas (0.0026 versus 0.0011).

3.3.2. Target firm announcement day returns

Theories explaining investor communication (e.g., Han and Hirshleifer, 2015) prescribe a positive correlation between past, realized investment returns and willingness to communicate. We test this assertion by sorting all merger events into two groups based on target firms' announcement-day returns. As shown in Appendix Table A2, the coefficient estimate for *Target_Neighbor* in merger events with above-median target announcement-day returns is nearly twice as large as that in merger events with below-median target announcement-day returns (0.0032 versus 0.0017).

3.3.3. # Stocks in the portfolio

An important ingredient in our argument is that retail investors hold small numbers of stocks in their portfolios. A change of one stock position therefore has a material impact on a retail investor's information-gathering activity. There is, however, substantial variation in portfolio size across retail investors. In Appendix Table A3, we sort investors into two groups based on the number of stocks they hold in their portfolios. We find that the coefficient estimate for *Target_Neighbor* is twice as large for investors carrying a below-median portfolio size than for investors carrying an above-median portfolio size (0.0032 versus 0.0016). For reference, the median portfolio size in our sample is three stocks.

3.4 Alternative Specifications

If social interactions play a major role, then we should expect the documented pattern to vary substantially with our definition of neighbors and with the time horizon over which we analyze the trades. All our analyses discussed in this subsection are tabulated in Appendix Table A4.

In our first set of tests, we vary the distance over which we define neighbors. When we focus on neighbors who live between three and seven miles from a target investor, the coefficient estimate for *Target_Neighbor* in the full regression specification using the dollar-weighted measure of trading intensity drops to 18bp (from 22bp). As we further increase the distance to between seven and fifteen miles, the coefficient estimate for *Target_Neighbor* drops to 14bp; if we increase the distance yet again, to between fifteen and thirty miles, the coefficient estimate drops to 2bp. We make almost identical observations when switching the dependent variable to trading intensity based on the number of trades. This rapid decrease in the coefficient estimates is consistent with the idea that word-of-mouth effects decay with distance.

We also experiment with the time period over which we measure investors' trading intensity. Specifically, instead of focusing on the one-year period after M&A completion (i.e., months seven through eighteen after an M&A announcement), we expand our window to years two and three.

Irrespective of the dependent variable, we find that target investors gradually reduce their trading intensity in an acquirer industry compared with other investors. In particular, in our baseline regression, which runs from months seven through eighteen after the M&A announcement, target investors disproportionately increase their trading in an acquirer industry by 2.30% (Table 2, Panel A, Column (4)). This figure drops to 1.78% in months eighteen through thirty, only to drop further to 1.23% in months thirty through forty-two. The drop in trading propensity for target neighbors is even more pronounced. The coefficient estimate for *Target_Neighbor* drops from 39bp in months seven through eighteen to 5bp in months eighteen through thirty and to 1bp in months thirty through forty-two.

We also test what happens during months one through six after an M&A announcement. An attention-based explanation of our findings predicts that our patterns should be stronger near the M&A *announcement date*, not the completion date. In contrast, Appendix Table 4 reveals that target investors and target neighbors trade in the acquirer firm's industry more frequently from months seven through eighteen (when most target investors have received shares of the acquiring firm) than from months one through six, which suggests that our results are unlikely to be driven by an attention effect or common information story.

3.5 Alternative Interpretations

One potential concern with our interpretation of the data is that our documented communication pattern might be driven by media coverage of a merger event, which affects target investors and target neighbors without their directly communicating with each other. While we cannot rule out this channel completely, it is unlikely that media coverage is the main driver of our results. First, for the media channel to work, we would need media coverage to be substantially higher in areas with target investors than in areas without. There are two possible reasons media coverage could vary in this particular way: (a) financial media optimally choose to concentrate in areas with target investors; (b) target investors live in areas that generally have more concentrated media coverage (e.g., metropolitan areas). The former is unlikely to hold as our analysis focuses exclusively on small retail investors whose collective holdings in the target firm are negligible (on top of that, we exclude firms' headquarters and factory locations from the analysis). The latter is at odds with the finding that the neighbor effect disappears once we focus on the most populated metropolitan areas. Moreover, we do not find a significant communication pattern when we look instead at cash-financed mergers, so the media explanation would make sense only if the geographic pattern of media coverage is substantially different for cash-financed deals compared with stock-financed ones. We are unaware of any evidence on mergers and acquisitions that would indicate that such a pattern exists.

Another potential concern with our interpretation of the data is that "target investors" anticipate a merger event and buy shares of the target firm immediately prior to the merger announcement to purchase the acquirer firm's shares at a discount. This can partially explain target investors' increased trading in the acquirer industry after merger completion. To address this concern, we instrument target investors using lagged holdings information. In Appendix Table A5, the target investor dummy takes the value of one if the investor holds the target stock one year *prior to* the acquisition announcement. It is implausible that retail investors are able to forecast merger events one year in advance. Yet, we find that all of our main results still go through under this alternative specification.

4. A Dynamic Setting

Having provided some baseline evidence on the effects of social interactions on investor trading, we now attempt to quantify the speed of communication and the determinants of such speed within a 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* following an M &A 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

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

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

$$\dots$$

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

Compared with the static OLS setting, we now explicitly and dynamically account for the cumulative effect of being a neighbor of patient zero, being a neighbor of a neighbor of patient zero, etc. Put differently, we now explicitly account for the fraction of "primary-case infected", the fraction of "secondary-case infected" in period *t*+2, the fraction of "tertiary-case infected" in period *t*+3, etc. Put yet another way, we now explicitly allow for the possibility that investor *i* transmits his/her view to investor *j* through a third party (or a chain of third parties), without being in direct contact with investor *j*.

Compounding the transmission matrix 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 merger completion date and p is the number of periods after merger completion. If the set of X_{t+p} satisfied the exogeneity condition, we could simply estimate a vector autoregression based on $X_{t+p} = B * X_{t+p-1}$ by stacking observations across both merger events and event quarters. Given that the exogeneity condition is unlikely to hold, however, we instrument the independent variable in each of these equations by the initial portfolio shock induced by the merger event. 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 merger completion. The technical difficulty in estimating this set of equations is that we need to raise an unknown 70,000 x 70,000 matrix to the power of 2, 3, . . ., p as we have roughly 70,000 investors in our sample. To get around this technical complexity, we instead employ a three-stage approach.

In the first stage, we instrument the set of X_{t+p}'s using portfolio shocks experienced by target investors at the merger 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 is a dummy variable that equals one if investor i holds target firm shares at the time of the M&A announcement. Trading activity in the acquirer industry is defined as the total number of trades (the total dollar value of trades) in the acquirer industry (excluding the acquirer firm) divided by the total number of trades (the total dollar value of trades) across all industries by the investor.

In the 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. We define each period p as one quarter, as retail investors in our sample, on average, trade once every quarter. We study the four quarters after each M&A as our findings in the previous section suggest that merger events no longer have a discernible effect on target neighbors' trading activity in years two and three after merger completion.

$$
\begin{aligned}
X_{t+1} &= B * \widehat{X_t} + e_{t+1} = B * \widehat{X_t} + e_{t+1} \\
X_{t+2} &= B^2 * \widehat{X_t} + e_{t+2} = B * \widehat{X_{t+1}} + e_{t+2} \qquad (5) \\
&\cdots \\
X_{t+4} &= B^4 * \widehat{X_t} + e_{t+4} = B * \widehat{X_{t+3}} + e_{t+4}.
\end{aligned}
$$

If we were to stop here, our estimates for the B matrix would be unbiased (to the extent that our instruments are truly exogenous). However, we would lose efficiency as we do not impose the following condition in the first stage estimation:

$$\widehat{X_{t+p}} = B * \widehat{X_{t+p-1}} = \cdots = B^p * \widehat{X_t}.$$

In the third stage, we improve the efficiency of our estimates for the B matrix using a recursive method. Specifically, in each round, 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 iteration with the B matrix estimated from the second stage, and stop the iteration 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, $\beta_{i,j}$.[6] In particular, we conjecture that the effective communication rate between any two investors is a function of (1) $Dist_{ij}$, the physical distance between investors i and j; (2) $|Income_{ij}|$, the "income gap" between investors i and j; (3) $|Age_{ij}|$, the age gap between investors i and j;[7] and (4) $|Gender_{ij}|$, the gender gap between investors i and j. The "income gap" is defined as follows: Our data vendor sorts households into one of the following nine *bins*:

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.

Income gap is the absolute difference in *bins*. For instance, if household *i*'s income is \$60,000 (*bin* = 6) and household *j*'s income is \$90,000 (*bin* = 7), the income gap would be 1.

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

$$\beta_{i,j} = b_0 + b_1 * Dist_{i,j} + b_2 * |IncomeDiff_{i,j}|$$
$$+b_3 * |AgeDiff_{i,j}| + b_4 * |GenderDiff_{i,j}| + \varepsilon_{i,j} \quad (i \neq j),$$

[6] We assume that each of the diagonal terms, $\beta_{i,i}$, which capture persistence in investor trading behavior, is a constant across all investors.

[7] "Heads" of households are those who are registered as the primary brokerage account holders.

where $\varepsilon_{i,j}$ captures the unobserved determinants of $\beta_{i,j}$ and b_0 reflects the baseline communication rate (with all social distances set at zero). Scaling our estimates for b_2, b_3, and b_4 by our estimate for b_0 therefore yields the proportional change in the effective communication rate as a function of social distances.

The results are reported in Table 6. The estimate for the baseline communication rate, b_0, equals 0.489 in Column (1) and 0.505 in Column (2) depending on whether trading activity is based on the number of trades or the dollar value of trades, respectively. As noted in Section 2.2, we believe these two estimates are upward biased (relative to the average U.S. household). Despite this upward bias, our estimates are far below one and below the reproduction ratios of some of the most extensively studied diseases such as HIV/AIDS, SARS, and Ebola. In other words, unlike infectious diseases, which require counter-measures such as isolation or vaccinations to contain an epidemic, our baseline communication rate implies that industry information gathered by "patient zero" is unlikely to trigger an epidemic. Instead, the communication effect dies out on its own by the end of Q4, a conclusion that is already hinted at in the previous section, which shows that merger events no longer have a discernible effect on target neighbors' trading activity starting in the second year after merger completion.

The coefficient estimates for the age, income, and gender differences suggest that the effective communication rate varies significantly with social closeness/distance. In particular, when scaling our estimates for b_2, b_3, and b_4 by our estimate for b_0, our results suggest that a ten-year difference in age, a one-step difference in income, and being of different genders lowers the effective communication rate by 12%, 2%, and 17%, respectively. As noted above, a one-step difference in household income represents an economically meaningful difference (e.g., household income of \$60,000 versus \$90,000). Yet, our estimate implies that such a meaningful difference has only a marginal impact on the effective communication rate. Comparatively speaking, age and gender have a much stronger impact on the effective communication rate. To the best of our knowledge, we are the first to make this observation.

We next entertain the possibility that the rate of communication between investors varies with social characteristics *to varying degrees* around the zero point. That is, instead of estimating one slope for the entire range of pairwise distances, we now estimate two slopes, one slope for investor pairs where the difference (i minus j) is positive and another slope for investor pairs where the difference (i minus j) is negative. To illustrate, we now create two age-difference variables, $|AgeDiff_{i,j}|^{+}$ and $|AgeDiff_{i,j}|^{-}$. The former is the absolute age difference if the difference is greater than zero, and zero otherwise; the latter is the absolute age difference if the difference is less than zero, and zero otherwise. With this modification, we now have a linear equation of $\beta_{i,j}$ with seven elements.

The results, presented in Table 7, show some asymmetry. For instance, the coefficient estimate for $|AgeDiff_{i,j}|^{-}$ in Column (1) is -0.006; in comparison, the coefficient estimate for $|AgeDiff_{i,j}|^{+}$ is only -0.003. To interpret these estimates, consider investor i who is forty years old. The effective communication rate is maximized if investor *j* is also forty years old ("base rate"). Our estimates suggest that if investor i is younger than investor *j*, the effective communication rate from j to i declines by 0.003 per one-year age gap compared with the base rate. If investor *i* is older than investor *j*, the effective communication rate from j to i declines by 0.006. That is, when investor i is younger than investor *j*, the effective communication rate from *j* to *i* declines by less than when investor i is older than investor *j*. So, on average, younger investors are more likely to act on older investors' views than the other way around.

By the same token, we find that the coefficient estimate for $|GenderDiff_{i,j}|^{+}$ is substantially more negative than that for $|GenderDiff_{i,j}|^{-}$, suggesting that the average effective communication rate from female to male is higher than that from male to female. The coefficient estimate for $|IncomeDiff_{i,j}|^{-}$ is also more negative than that for $|IncomeDiff_{i,j}|^{+}$, suggesting that lower-income investors are more likely to act on higher-income investors' views than vice versa.

In the final experiment we describe in this section, we include state dummies in the linear function of $\beta_{i,j}$ to estimate regional differences in the effective communication rate after controlling for observable social characteristics. Figures 1 and 2, which are heat maps of effective communication rates by state, reveal strong regional differences in the speed of communication. Some of the highest effective communication rates are in the Southeast (e.g., North and South Carolina, Georgia, Florida); some of the lowest effective communication rates are in the central West/Midwest (e.g., Montana, Wyoming, Kansas).

When comparing our heat map, estimated from investors' trading behavior, with a heat map of communication rates based on survey responses that pertain to how frequently one takes advice from a friend (Figure 3), we find that there is much overlap. We take the survey data from Putnam (2000); details on the survey methodology can be found in Appendix I of Putnam and on the following website: http://bowlingalone.com. The Pearson correlation coefficient between our effective communication rate (aggregated to the state level) and the state-level survey responses is 0.43 or 0.44, depending on whether the communication speed is measured by the number of trades or the dollar value of trades.

5. Value-Relevant News Transmission or Spreading of Noise?

We conclude this study by assessing whether investors in our setting transmit value-relevant news or simply spread noise. The answer to this question has important implications concerning whether social interactions between investors in our setting help improve market efficiency or destabilize prices.

We examine this issue by constructing long–short portfolios. At the end of each month t following an M&A, we look at all stocks in the acquirer industry that were bought or sold by target investors and target neighbors during month t (we exclude the acquirer firm itself). We experiment with three portfolio construction schemes:

1) For each stock and each month, we compute the total number of shares bought by target investors and target neighbors minus the total number of shares sold. The long portfolio contains stocks for which target investors and their neighbors are net buyers; the short portfolio contains stocks for which they are net sellers. The long and short portfolios are weighted by the net total number of shares bought (sold) across target investors and target neighbors, and are held for from one month up to one year.

2) For each stock in the acquirer industry, we compute the total dollar value of shares bought minus the total dollar value of shares sold. We form long–short portfolios as above.

3) For each stock and each month, we compute the equal-weighted average change in that stock's weight in target investors' and target neighbors' portfolios. We long the stocks that experience

an increase and short the stocks that experience a decrease. The long and short portfolios are then weighted by the relevant stock's portfolio weight change and we hold portfolios for one month up to one year.

We report the results in Table 7. Irrespective of the portfolio formation scheme, we find that the long portfolio subsequently underperforms the short portfolio, albeit not to a statistically significant degree. These results do not support the notion that the newly acquired views and opinions about firms in the acquirer industry reflect value-relevant news.

6. Conclusion

We study word-of-mouth effects in financial markets. Our first innovation is that we study word-of-mouth effects associated with cross-industry stock-financed M&As, which produces results that are perhaps easier to interpret than results based on mere correlations in trading activity. Our second innovation is that we introduce a method that allows us to estimate the speed of communication and how that speed varies with distances in social characteristics. When we implement that method, we find that word-of-mouth effects exist in financial markets and that, among other factors, differences in age and gender represent much greater barriers to communication than differences in socio-economic background. We also point to interesting asymmetries in the communication rate.

Our baseline estimation also suggests that the effective "information contagion rate" is far below the contagion rates of the most commonly studied diseases. Does this mean that word-of-mouth can never trigger an epidemic in financial markets? The effective communication rate likely is a function of how exciting the underlying story is and how frequently the story is repeated or updated. In our setting, target investors are "infected" only once with acquirer firm shares. Moreover, general industry news is perhaps not the most exciting news. It is therefore unclear to what extent our findings extend to other types of events and stories (e.g., a story reporting that Tesla Motors is close to a major breakthrough or collapse). We hope that much more research on this topic will be forthcoming.

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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 partially financed by stocks; cash-financed M&As are 100% cash-financed. Firm size is the number of shares outstanding multiplied by the share price as of the month prior to the M&A [$millions]. All observations are at the M&A event level. Panel B shows investor- and portfolio characteristics for the retail investor sample used in Barber and Odean (2001). We only include retail brokerage accounts that perform at least one trade in the two-year window surrounding the M&A; we further require that investors do not trade or hold any stocks from the acquirer industry in the year prior to the M&A. 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 a male and zero for a female. All observations are at the account/year-month level. Panel C shows demographic information for each zip code included in our sample. All observations under "Basic Characteristics" are at the zip-code/year-month level. The sociability index "*Seeking Advice from Friends*" captures how often people seek advice from friends and is from Putnam (2000).

	N	25%	Median	75%	Mean	Std. Dev.
Panel A: M&A Sample 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 ($)	70,608	5,513	13,141	31,818	41,030	216,539
Number of Stocks Held	70,608	1	2	5	3.88	5.03
Number of Trades Each Month	70,608	0	0	0	0.47	1.76
Value of Trades Each Month ($)	70,608	0	0	0	5,679	76,056
Investor Age	70,608	36	46	56	42.02	21.44
Investor Income ($)	70,608	45,000	62,500	87,500	69,500	30,064
Investor Gender	70,608	1	1	1	0.90	0.30
Panel C: Zip Code Characteristics						
Basic Characteristics						
Population	42,057	785	2,777	11,960	8,965	13,134
No. Household Members	42,057	2.40	2.56	2.73	2.59	0.35
House Value ($)	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 Index (measured at the state level)						
Seeking Advice from Friends	294	2.90	3.07	3.21	3.06	0.31

Table 2. Target Investors' Trading in the Acquirer Industry

This table reports coefficient estimates from regressions of investors trading in the acquirer industry on a target investor dummy. The observations are at the M&A event/brokerage account/year-month level. The dependent variable in Columns (1)-(4) 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 seven through eighteen after the M&A is announced. The dependent variable in Columns (5)-(8) 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 seven through eighteen after the M&A is announced. We skip the initial six months as it takes an average of six months for the M&A to complete after its initial announcement. The main independent variable, *Target Investor*, is an indicator, which equals one if the account holder possesses shares of the target stock at the end of the month prior to the M&A announcement. Investor-level controls include the account holder's income, age, number of children, number of family members, gender, and marital status. Zip-code-level controls include the zip-code population, fraction of male residents, average home value, number of household members, and household income. We only consider account holders who perform at least one trade in the two year window surrounding the M&A; we further require that investors do not trade or hold any stocks from the acquirer industry in the year prior to the M&A. Panel A reports regression results for stock-financed M&As. Panel B reports regression results for the counterfactual, cash-financed M&As. Standard errors, shown in brackets, are clustered at the zip-code- and time levels. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Stock-Financed M&As

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Target Investor	0.0248***	0.0248***	0.0230***	0.0230***	0.0220***	0.0219***	0.0204***	0.0203***
	[0.0046]	[0.0046]	[0.0047]	[0.0047]	[0.0045]	[0.0045]	[0.0047]	[0.0047]
Investor Controls	NO	YES	NO	YES	NO	YES	NO	YES
Zip Code Controls	NO	YES	NO	YES	NO	YES	NO	YES
Event-Fixed Effects	NO	NO	YES	YES	NO	NO	YES	YES
Adj. R^2	0.00%	0.01%	1.65%	1.66%	0.00%	0.01%	1.59%	1.59%
No. Obs.	7,598,715	7,598,715	7,598,715	7,598,715	7,598,715	7,598,715	7,598,715	7,598,715

Panel B: Cash-Financed M&As

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Target Investor	0.0046	0.0046	0.0044	0.0043	0.0061	0.0061	0.0059	0.0059
	[0.0037]	[0.0037]	[0.0035]	[0.0035]	[0.0042]	[0.0042]	[0.0040]	[0.0040]
Investor Controls	NO	YES	NO	YES	NO	YES	NO	YES
Zip Code Controls	NO	YES	NO	YES	NO	YES	NO	YES
Event-Fixed Effects	NO	NO	YES	YES	NO	NO	YES	YES
Adj. R^2	0.00%	0.01%	2.36%	2.37%	0.00%	0.01%	2.25%	2.26%
No. Obs.	3,489,281	3,489,281	3,489,281	3,489,281	3,489,281	3,489,281	3,489,281	3,489,281

Table 3. Target Neighbors' Trading in the Acquirer Industry

This table reports coefficient estimates from regressions of investors trading in the acquirer industry on a target neighbor dummy. The observations are at the M&A event/brokerage account/year-month level. The dependent variable in Columns (1)-(4) 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 seven through eighteen after the M&A is announced. The dependent variable in Columns (5)-(8) 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 seven through eighteen after the M&A is announced. We skip the initial six months as it takes an average of six months for the M&A to complete after its initial announcement. The main independent variable, *Target Neighbor*, is an indicator, which equals one if the account holder lives within three miles of a target investor and is not a target investor him-/herself. Investor-level controls include the account holder's income, age, number of children, number of family members, gender, and marital status. Zip-code-level controls include the zip-code population, fraction of male residents, average home value, number of household members, and household income. We only consider account holders who perform at least one trade in the two year window surrounding the M&A; we further require that investors do not trade or hold any stocks from the acquirer industry in the year prior to the M&A. Panel A reports regression results for stock-financed M&As. Panel B reports regression results for the counterfactual, cash-financed M&As. Standard errors, shown in brackets, are clustered at the zip-code- and time levels. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Stock-Financed M&As

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Target Neighbor	0.0039***	0.0044***	0.0021***	0.0023***	0.0037***	0.0041***	0.0019***	0.0022***
	[0.0008]	[0.0007]	[0.0007]	[0.0007]	[0.0008]	[0.0008]	[0.0007]	[0.0007]
Investor Controls	NO	YES	NO	YES	NO	YES	NO	YES
Zip Code Controls	NO	YES	NO	YES	NO	YES	NO	YES
Event-Fixed Effects	NO	NO	YES	YES	NO	NO	YES	YES
Adj. R^2	0.00%	0.01%	1.65%	1.66%	0.00%	0.01%	1.59%	1.59%
No. Obs.	7,596,415	7,596,415	7,596,415	7,596,415	7,596,415	7,596,415	7,596,415	7,596,415

Panel B: Cash-Financed M&As

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Target Neighbor	0.0010	0.0015	-0.0001	0.0003	0.0009	0.0014	-0.0002	0.0002
	[0.0012]	[0.0011]	[0.0010]	[0.0010]	[0.0012]	[0.0012]	[0.0010]	[0.0010]
Investor Controls	NO	YES	NO	YES	NO	YES	NO	YES
Zip Code Controls	NO	YES	NO	YES	NO	YES	NO	YES
Event-Fixed Effects	NO	NO	YES	YES	NO	NO	YES	YES
Adj. R^2	0.00%	0.01%	2.36%	2.37%	0.00%	0.01%	2.25%	2.26%
No. Obs.	3,488,558	3,488,558	3,488,558	3,488,558	3,488,558	3,488,558	3,488,558	3,488,558

Table 4. Likelihood of Trading in the Acquirer Industry

This table reports coefficient estimates from regressions of trading-in-the-acquirer-industry indicators on a target investor dummy or a target neighbor dummy. We focus on stock-financed M&As and the observations are at the M&A event/brokerage account/year-month level. The dependent variable equals one if there is any trading in the acquirer's industry in months seven through eighteen after the M&A is announced, and zero otherwise. We skip the initial six months as it takes an average of six months for the M&A to complete after its initial announcement. The main independent variable in columns (1)-(3), *Target Investor*, is an indicator, which equals one if the account holder possesses shares of the target stock at the end of the month prior to the M&A announcement. The main independent variable in columns (4)-(6), *Target Neighbor*, is an indicator, which equals one if the account holder lives within three miles of a target investor and is not a target investor him-/herself. Investor-level controls include the account holder's income, age, number of children, number of family members, gender, and marital status. Zip-code-level controls include the zip-code population, fraction of male residents, average home value, number of household members, and household income. We only consider account holders who perform at least one trade in the two year window surrounding the M&A; we further require that investors do not trade or hold any stocks from the acquirer industry in the year prior to the M&A. We estimate both logit models (Columns (1) and (4)) and OLS regressions (Column (2)-(3) and (5)-(6)). For the logit models, the coefficient estimates are converted into marginal probabilities. Standard errors, shown in brackets, are clustered at the zip-code- and time levels. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

	Target Investors			Target Neighbors		
	Logit	OLS	OLS	Logit	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
Target Dummy	0.0653***	0.0977***	0.0967***	0.0100***	0.0103***	0.0075***
	[0.0058]	[0.0121]	[0.0118]	[0.0019]	[0.00021]	[0.0003]
Investor Controls	YES	YES	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES	YES	YES
Event-Fixed Effects	NO	NO	YES	NO	NO	YES
Adj. R^2	0.11%	0.06%	1.66%	0.10%	0.05%	2.53%
No. Obs.	7,598,715	7,598,715	7,598,715	7,596,415	7,596,415	7,596,415

Table 5. Pseudo-Target Firms

This table repeats the analyses reported in Tables 2 and 3, but now replaces target investors with pseudo-target investors who, at the time of the M&A, hold shares in a pseudo-target firm, but not the target firm itself. The pseudo-target firm is the industry peer with the closest market capitalization and the closest book-to-market ratio to the actual target firm. Pseudo-target neighbors are account holders who live within three miles of a pseudo-target investor and are not pseudo-target investors themselves.
The dependent variable in Columns (1), (3), (5), and (7) 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 seven through eighteen after the M&A is announced. The dependent variable in Columns (2), (4), (6), and (8) 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 seven through eighteen after the M&A is announced. We skip the initial six months as it takes an average of six months for the M&A to complete after its initial announcement. Columns (1)-(2) and (5)-(6) report regression results for stock-financed M&As. Columns (3)-(4) and (7)-(8) report regression results for cash-financed M&As. Investor-level controls include the account holder's income, age, number of children, number of family members, gender, and marital status. Zip-code-level controls include the zip-code population, fraction of male residents, average home value, number of household members, and household income. We only consider account holders who perform at least one trade in the two year window surrounding the M&A; we further require that investors do not trade or hold any stocks from the acquirer industry in the year prior to the M&A. Standard errors, shown in brackets, are clustered at the zip-code- and time levels. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

	Pseudo Target Investors				Pseudo Target Neighbors			
	Stock M&As		Cash M&As		Stock M&As		Cash M&As	
	# Trades	$Trades	# Trades	$Trades	# Trades	$Trades	# Trades	$Trades
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Target Dummy	0.0006 [0.0018]	-0.0006 [0.0019]	-0.0009 [0.0028]	-0.0003 [0.0030]	-0.0003 [0.0006]	-0.0003 [0.0006]	0.0005 [0.0008]	0.0004 [0.0008]
Investor Controls	YES	YES	YES	YES	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES	YES	YES	YES	YES
Event-Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Adj. R^2	1.66%	1.59%	2.36%	2.25%	1.66%	1.59%	2.36%	2.25%
No. Obs.	7,558,105	7,558,105	3,476,999	3,476,999	7,555,604	7,555,604	3,475,477	3,475,477

Table 6. Dynamic Estimation of Communication Speed (Symmetric)

This table reports the results of the three-stage estimation of the transmission matrix. The detailed estimation procedure is described in Section 4. To facilitate the computation of the transmission matrix, we impose a linear structure on all the off-diagonal elements. (For all the diagonal terms, which capture the persistence in investor trading behavior, we assume it is a constant across all investors.) In particular, we conjecture that the effective communicate rate between any two investors is a function of (1) $Dist_{ij}$, the geographic distance between investors i and j, (2) $|Income_i\text{-}Income_j|$, the income gap between investors i and j, (3) $|Age_i\text{-}Age_j|$, the age gap between investors i and j, and (4) $|Gender_i\text{-}Gender_j|$, the gender gap between investors i and j. In column 1, we examine the number of trades in the acquirer industry as a fraction of the total number of trades across all industries in each of the four quarters after a merger completion. In column 2, we focus on the dollar value of trades in the acquirer industry as a fraction of the total dollar value of trades in each of the four quarters. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

Estimation of the Transmission Matrix		
	#Trades	$Trades
	(1)	(2)
$\widehat{Trade}_{i,t}$	0.599***	0.580***
	[0.022]	[0.022]
$\widehat{Trade}_{j,t}$	0.489***	0.505***
	[0.028]	[0.028]
$\widehat{Trade}_{j,t}$ $*Dist_{i,j}$	-0.0194***	-0.020***
	[0.005]	[0.005]
$\widehat{Trade}_{j,t}$ $*\|Age_i\text{-}Age_j\|$	-0.006***	-0.006***
	[0.001]	[0.001]
$\widehat{Trade}_{j,t}$ $*\|Income_i\text{-}Income_j\|$	-0.009**	-0.007**
	[0.003]	[0.003]
$\widehat{Trade}_{j,t}$ $*\|Gender_i\text{-}Gender_j\|$	-0.084***	-0.087***
	[0.015]	[0.015]
Adj. R^2	0.016	0.016
No. Obs.	1,609,602	1,609,602

Table 7. Dynamic Estimation of Communication Speed (Asymmetric)

This table reports the results of the three-stage estimation of the transmission matrix. The detailed estimation procedure is described in Section 4. To facilitate the computation of the transmission matrix, we impose a linear structure on all the off-diagonal elements. (For all the diagonal terms, which capture the persistence in investor trading behavior, we assume it is a constant across all investors.) In particular, we conjecture that the effective communicate rate between any two investors is a function of (1) $Dist_{ij}$, the geographic distance between investors i and j, (2) $|Income_i\text{-}Income_j|$, the income gap between investors i and j, (3) $|Age_i\text{-}Age_j|$, the age gap between investors i and j, and (4) $|Gender_i\text{-}Gender_j|$, the gender gap between investors i and j. For each of the social-characteristic-distance variable, we further separate it into a "+" component and a "--" component to capture the asymmetry between positive vs. negative differences (e.g., male communicating to female vs. female communicating to male). Specifically, the + component takes the value of the absolute difference if the difference is greater than zero, and is set to 0 otherwise. Similarly, the - component takes the value of the absolute difference if the difference is smaller than zero, and is set to 0 otherwise. In column 1, we examine the number of trades in the acquirer industry as a fraction of the total number of trades across all industries in each of the four quarters after a merger completion. In column 2, we focus on the dollar value of trades in the acquirer industry as a fraction of the total dollar value of trades in each of the four quarters. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

Estimation of the Transmission Matrix		
	#Trades	$Trades
	(1)	(2)
$\widehat{Trade}_{i,t}$	0.595***	0.575***
	[0.022]	[0.022]
$\widehat{Trade}_{j,t}$	0.474***	0.490***
	[0.027]	[0.028]
$\widehat{Trade}_{j,t}$ *$Dist_{i,j}$	-0.023***	-0.023***
	[0.005]	[0.005]
$\widehat{Trade}_{j,t}$ *$\|Age_j\text{-}Age_i\|^{+}$	-0.003***	-0.006***
	[0.001]	[0.001]
$\widehat{Trade}_{j,t}$ *$\|Age_j\text{-}Age_i\|^{--}$	-0.006***	-0.007***
	[0.001]	[0.001]
$\widehat{Trade}_{j,t}$ *$\|Income_j\text{-}Income_i\|^{+}$	-0.010**	-0.009**
	[0.003]	[0.003]
$\widehat{Trade}_{j,t}$ *$\|Income_j\text{-}Income_i\|^{--}$	-0.013**	-0.011**
	[0.005]	[0.005]
$\widehat{Trade}_{j,t}$ *$\|Gender_j\text{-}Gender_i\|^{+}$	-0.135***	-0.140***
	[0.045]	[0.048]
$\widehat{Trade}_{j,t}$ *$\|Gender_j\text{-}Gender_i\|^{--}$	-0.041	-0.042
	[0.052]	[0.053]
Adj. R^2	0.016	0.016
No. Obs.	1,609,602	1,609,602

Table 8. Returns to Target Investors' and Target Neighbors' Trading

This table reports monthly returns of hedge portfolios that go long stocks bought by and short stocks sold by target investors and their neighbors. Panels A and B use information from the trade file in the retail broker database. In Panel A, the long- and short portfolios are weighted by the number of shares traded by each investor over the previous twelve months, and portfolios are held for one month. In Panel B, the long- and short portfolios are weighted by the dollar value of shares traded by each investor over the previous twelve months, and portfolios are held for one month. Panels C and D use information from the holdings file in the retail broker database. In Panel C, the long- and short portfolios are weighted by the portfolio-weight change of each investor over the previous one month, and portfolios are held for one month. In Panel D, the long- and short portfolios are weighted by the portfolio-weight change of each investor over the previous month, and portfolios are held for twelve months. We deal with overlapping portfolios in each holding month by taking the equal-weighted average return across portfolios formed in different months. *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	CAPM Alpha	Three-Factor Alpha	Four-Factor Alpha
Panel A: (12, 1) Returns to Portfolios Weighted by Shares Traded				
Buy-Sell	-0.35%	-0.24%	-0.15%	-0.13%
	(-1.01)	(-0.53)	(-0.42)	(-0.29)
N (of Months)	61	61	61	61
Panel B: (12, 1) Returns to Portfolios Weighted by Trading Value				
Buy-Sell	-0.36%	-0.13%	-0.16%	-0.02%
	(-0.73)	(-0.23)	(-0.28)	(-0.04)
N (of Months)	61	61	61	61
Panel C: (1, 1) Returns to Portfolios Weighted by Portfolio Weight Changes				
Buy-Sell	-1.14%	-1.29%	-0.69%	-0.33%
	(-0.90)	(-1.01)	(-0.69)	(-0.29)
N (of Months)	61	61	61	61
Panel D: (1, 12) Returns to Portfolios Weighted by Portfolio Weight Changes				
Buy-Sell	-0.32%	-0.26%	-0.24%	-0.17%
	(-1.24)	(-0.99)	(-0.98)	(-0.71)
N (of Months)	61	61	61	61

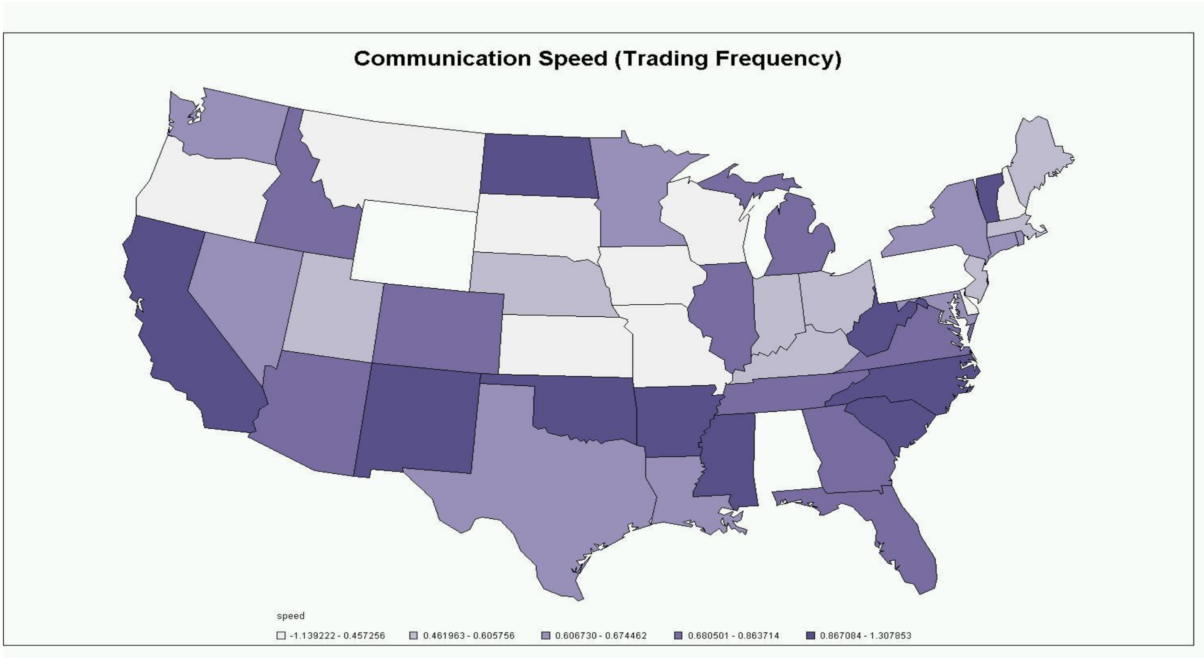

Figure 1. U.S. Heat Map of Communication Speed (#Trades)

This figure shows the "U.S heat map" for our estimates of state-level communication speed using the three-stage procedure discussed in Section 4. Communication speed is inferred from the number of trades in the acquirer industry as a fraction of the total number of trades across all industries in each of the four quarters after a merger completion. To facilitate the computation of the transmission matrix, we impose a linear structure on all the off-diagonal elements. (For all the diagonal terms, which capture the persistence in investor trading behavior, we assume it is a constant across all investors.) In particular, we conjecture that the effective communicate rate between any two investors is a function of (1) $Dist_{ij}$, the geographic distance between investors i and j, (2) $|Income_i\text{-}Income_j|$, the income gap between investors i and j, (3) $|Age_i\text{-}Age_j|$, the age gap between investors i and j, and (4) $|Gender_i\text{-}Gender_j|$, the gender gap between investors i and j. We further allow the coefficient estimate for $Trade_j$ (shown in Table 6) to vary across states.

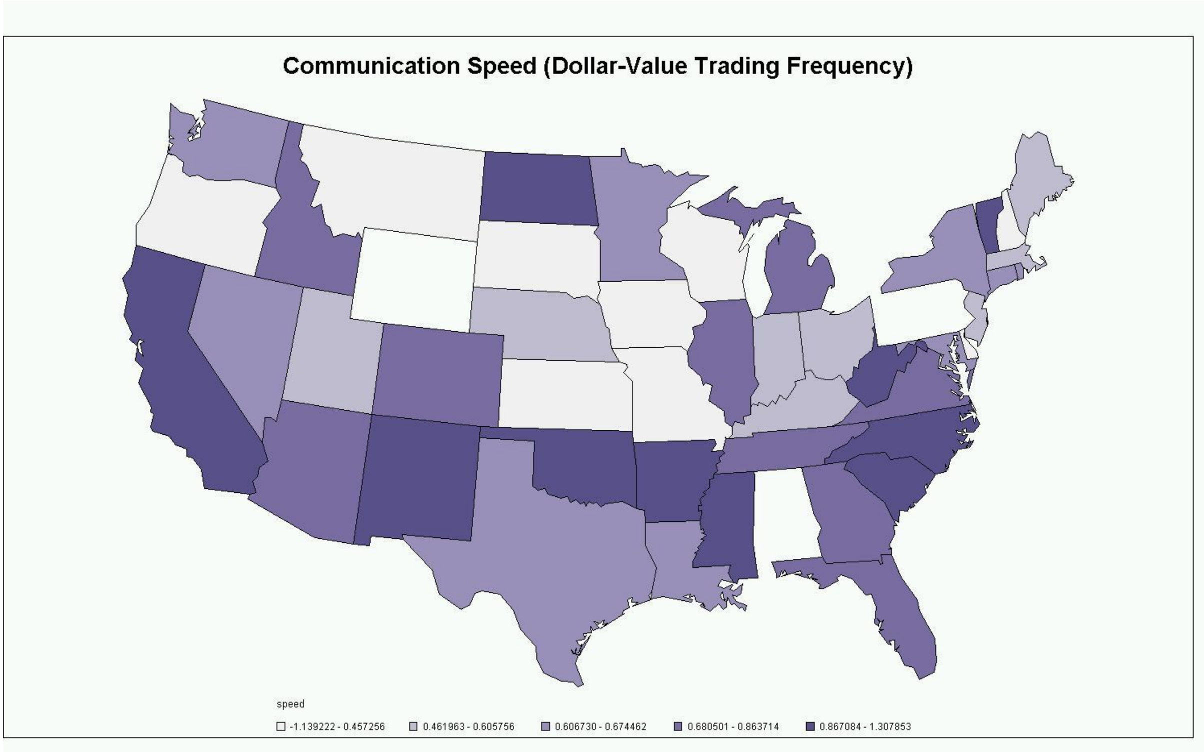

Figure 2. U.S. Heat map of Communication Speed ($Trades)

This figure shows the "U.S heat map" for our estimates of state-level communication speed using the three-stage procedure discussed in Section 4. Communication speed 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 in each of the four quarters after a merger completion. To facilitate the computation of the transmission matrix, we impose a linear structure on all the off-diagonal elements. (For all the diagonal terms, which capture the persistence in investor trading behavior, we assume it is a constant across all investors.) In particular, we conjecture that the effective communicate rate between any two investors is a function of (1) $Dist_{ij}$, the geographic distance between investors i and j, (2) $|Income_i\text{-}Income_j|$, the income gap between investors i and j, (3) $|Age_i\text{-}Age_j|$, the age gap between investors i and j, and (4) $|Gender_{i}\text{-}Gender_j|$, the gender gap between investors i and j. We further allow the coefficient estimate for $Trade_j$ (shown in Table 6) to vary across states.

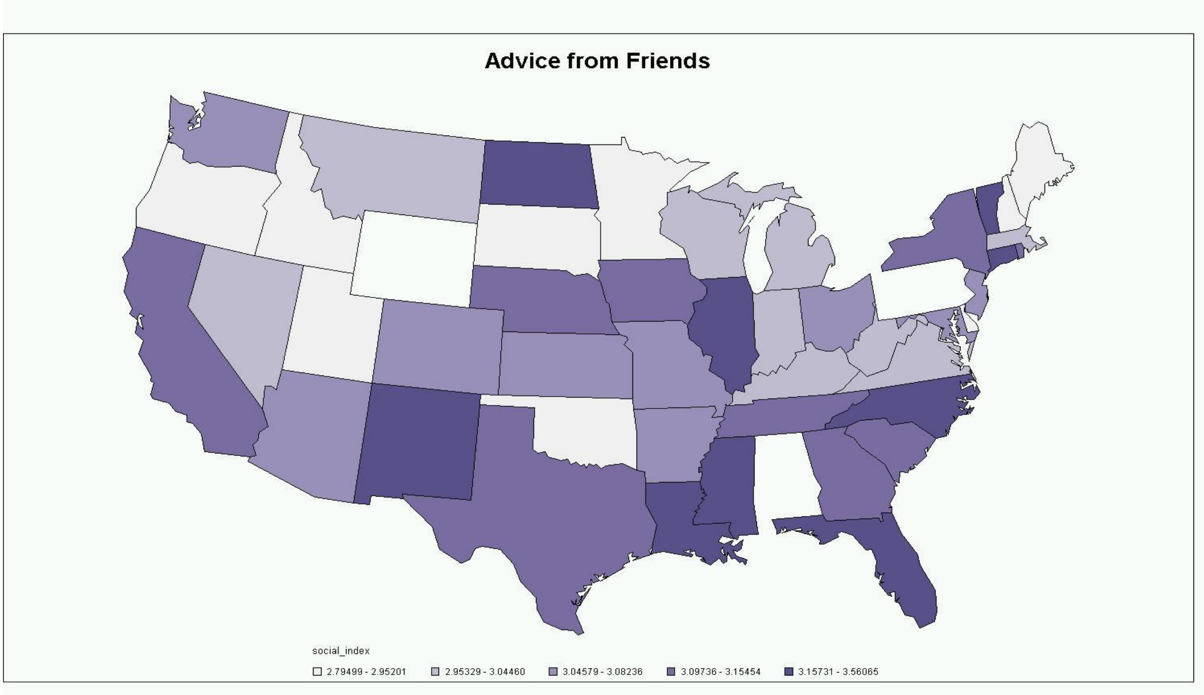

Figure 3. U.S. Heat Map of Social Capital Index

This figure shows the “U.S. heat map” for the state-average response to the question of whether one takes advice from a friend, as compiled by Putnam (2000). The correlation between our estimate of communication speed based on the number and dollar value of trades and this Social Capital Index is 0.43 and 0.45, respectively. Both are statistically significant.

Internet Appendix to
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Table A1. The Effect of Population Density

This table reports results from regressions of investor trading in the acquirer industry on a target neighbor dummy. We focus on stock-financed M&As and the observations are at the M&A event/brokerage account/year-month level. The dependent variable in Columns (1) and (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 seven through eighteen after the M&A announcement. The dependent variable in Columns (2) and (4) 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 seven through eighteen after the M&A announcement. The main independent variable, *Target Neighbor*, is an indicator, which equals one if the account holder lives within three miles of a target investor and is not a target investor him-/herself. Investor-level controls include the account holder's income, age, number of children, number of family members, gender, and marital status. Zip-code-level controls include the zip-code population, fraction of male residents, average home value, number of household members, and household income. We only consider account holders who perform at least one trade in the two year window surrounding the M&A; we further require that investors do not trade or hold any stocks from the acquirer industry in the year prior to the M&A. In Panel A, we divide all zip codes into those that are part of a metropolitan area (Metropolitan Area) and those that are not (Rural Area). In Panel B, within all zip codes that are part of a metropolitan area, we separate zip codes based on the 75th percentile of the population distribution. Standard errors, shown in brackets, are clustered at the zip-code- and time levels. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Metropolitan vs. Rural Areas

	Metropolitan Areas		Rural Areas	
	(1)	(2)	(3)	(4)
Target Neighbor	0.0021***	0.0019**	0.0007	0.0011
	[0.0008]	[0.0008]	0.0015	0.0015
Investor Controls	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES
Event-Fixed Effects	YES	YES	YES	YES
Adj. R^2	1.85%	1.77%	1.51%	1.45%
No. Obs.	3,020,577	3,020,577	2,105,810	2,105,810

Panel B: Population Density within Metropolitan Areas

	< 75th Percentile		>= 75th Percentile	
	(1)	(2)	(3)	(4)
Target Neighbor	0.0026**	0.0025**	0.0011	0.0011
	[0.0012]	[0.00012]	0.0010	0.0010
Investor Controls	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES
Event-Fixed Effects	YES	YES	YES	YES
Adj. R^2	1.73%	1.64%	1.99%	1.94%
No. Obs.	1,510,209	1,510,209	1,436,074	1,436,074

Table A2. Effect of Announcement Day Returns

This table reports results from regressions of investors trading in the acquirer industry on a target neighbor dummy. We focus on stock-financed M&As and the observations are at the M&A event/brokerage account/year-month level. The dependent variable in Columns (1) and (3) is the number of trades in the acquirer industry (excluding the acquirer firm) as a fraction of total number of trades across all industries in months seven through eighteen after the M&A announcement. The dependent variable in Columns (2) and (4) is the dollar value of trades in the acquirer industry (excluding the acquirer firm) as a fraction of total dollar value of trades in months seven through eighteen after the M&A announcement. The main independent variable, *Target Neighbor*, is a dummy variable that takes the value of one if the investor lives within three miles of any target investor and is not a target investor him-/herself. Investor-level controls include the investor's income, age, number of children, number of family member, gender, and marital status. Zip code level controls include the zip code population, fraction of male residents, average house value, number household members, and household income. We only consider accounts that perform at least one trade in the two-year window surrounding an acquisition; we further require that investors do not trade or hold any stocks from the acquirer industry in that two-year window. Panel A, Columns (1) and (2) represent events with higher-than-median announcement days's CAR(-1,1), while Columns (3) and (4) represent events with lower-than-median announcement days's CAR(-1,1). Panel B, Columns (1) and (2) represent events with higher-than-median market returns in the month of the announcement days.; Columns (3) and (4) represent events with lower-than-median market returns in the month of the announcement days'. Standard errors, shown in brackets, are clustered at zip code and time levels. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: CAR(-1,1) Around Announcement Day

	High		Low	
	(1)	(2)	(3)	(4)
Target Neighbor	0.0032***	0.0031***	0.0017*	0.0016
	[0.0010]	[0.0010]	[0.0010]	[0.0010]
Investor Controls	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES
Event-Fixed Effects	YES	YES	YES	YES
Adj. R^2	1.83%	1.63%	1.63%	1.56%
No. Obs.	3,360,116	3,360,116	4,238,599	4,238,599

Panel B: Market Return

	High		Low	
	(1)	(2)	(3)	(4)
Target Neighbor	0.0022**	0.0020**	0.0027***	0.0026**
	[0.0010]	[0.0010]	[0.0010]	[0.0011]
Investor Controls	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES
Event-Fixed Effects	YES	YES	YES	YES
Adj. R^2	2.10%	2.00%	1.31%	1.26%
No. Obs.	3,573,951	3,573,951	4,024,764	4,024,764

Table A3. Subsample Analyses Based on Portfolio Size

This table reports results from regressions of investors trading in the acquirer industry on target investor and target neighbor dummies. The dependent variable in Columns (1) and (3) is the number of trades in the acquirer industry (excluding the acquirer firm) as a fraction of total number of trades across all industries in months seven through eighteen after the M&A announcement. The dependent variable in Columns (2) and (4) is the dollar value of trades in the acquirer industry (excluding the acquirer firm) as a fraction of total dollar value of trades in months seven through eighteen after the M&A announcement. The main independent variable in Panel A, *Target Investor*, takes the value of one if the investor holds the target stock prior to the acquisition announcement. The main independent variable in Panel B, *Target Neighbor*, takes the value of one if the investor lives within three miles of a target investor and is not a target investor him-/herself. Investor-level controls include the investor's income, age, number of children, number of family member, gender, and marital status. Zip code level controls include the zip code population, fraction of male residents, average house value, number household members, and household income. We only consider accounts that perform at least one trade in the two-year window surrounding an acquisition; we further require that investors do not trade or hold any stocks from the acquirer industry in that two-year window. Columns (1) and (2) report regression results based on target investors who hold more than the median number of different stocks; Columns (3) and (4) report regression results based on target investors who hold fewer than the median number of different stocks. Standard errors, shown in brackets, are clustered at zip code and time levels. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Target Investors

	High Number of Stocks		Low Number of Stocks	
	(1)	(2)	(3)	(4)
Target Investor	0.0070**	0.0074**	0.0393***	0.0324***
	[0.0033]	[0.0033]	[0.0094]	[0.0085]
Investor Controls	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES
Event-Fixed Effects	YES	YES	YES	YES
Adj. R^2	1.73%	1.65%	1.73%	1.66%
No. Obs.	6,681,986	6,681,986	6,693,735	6,693,735

Panel B: Target Neighbors

	High Number of Stocks		Low Number of Stocks	
	(1)	(2)	(3)	(4)
Target Neighbor	0.0017*	0.0016*	0.0032***	0.0029***
	[0.0010]	[0.0009]	[0.0011]	[0.0010]
Investor Controls	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES
Event-Fixed Effects	YES	YES	YES	YES
Adj. R^2	1.73%	1.66%	1.73%	1.66%
No. Obs.	6,698,311	6,698,311	6,696,256	6,696,256

Table A4. Alternative Definitions of "Target Neighbors" and Time Horizons

This table reports coefficient estimates from regressions of investor trading in the acquirer industry on target investor- or a target neighbor dummy. We focus on stock-financed M&As and the observations are at the M&A event/brokerage account/year-month level. The dependent variable in Columns (1), (3), (5), and (7) is the number of trades in the acquirer industry (excluding the acquirer firm) as a fraction of total number of trades across all industries. The dependent variable in Columns (2), (4), (6), and (8) is the dollar value of trades in the acquirer industry (excluding the acquirer firm) as a fraction of total dollar value of trades. The main independent variable in Panel A, *Target Neighbor*, takes the value of one if the investor lives within *N* miles of a target investor (where *N* varies from 3 to 30 miles) and is not a target investor him-/herself. The main independent variables in Panels B and C, *Target*, are target investor and target neighbor dummies: the former takes the value of one if the investor holds the target stock at the end of the month before the M&A announcement; the latter takes the value of one if the investor lives within three miles of a target investor and is not a target investor him-/herself. Panel C reports the difference between investors' trading frequency in months one through six and months seven through twelve. Investor-level controls include the account holder's income, age, number of children, number of family members, gender, and marital status. Zip-code-level controls include the zip-code population, fraction of male residents, average home value, number of household members, and household income. We only consider account holders who perform at least one trade in the two year window surrounding the M&A; we further require that investors do not trade or hold any stocks from the acquirer industry in the year prior to the M&A. Standard errors, shown in brackets, are clustered at zip code and time levels. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Neighbors of Different Distances from Target Investors								
	0 to 3 Miles		3 to 7 Miles		7 to 15 Miles		15 to 30 Miles	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Target Neighbor	0.0023***	0.0022***	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
Event-Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
No. Obs.	7,596,415	7,596,415	7,558,105	7,558,105	7,485,049	7,485,049	7,336,619	7,336,619
Adj. R^2	1.66%	1.59%	1.66%	1.59%	1.65%	1.59%	1.65%	1.58%

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	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Target	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
Event-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%
No. 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: Months 1-6 and Months 7-18								
	Target Investors				Target Neighbors			
	Stock-Financed M&As		Cash-Financed M&As		Stock-Financed M&As		Cash-Financed M&As	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Target	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
Event-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%
No. 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

Table A5. Target Investors Defined Based on Lagged One Year Holdings

This table reports results from regressions of investors trading in the acquirer industry on target investor- and target neighbor dummies. The dependent variable in Columns (1) and (3) is the number of trades in the acquirer industry (excluding the acquirer firm) as a fraction of total number of trades across all industries in months seven through eighteen after the M&A announcement. The dependent variable in Columns (2) and (4) is the dollar value of trades in the acquirer industry (excluding the acquirer firm) as a fraction of total dollar value of trades in months seven through eighteen after the M&A announcement. The main independent variable in Panel A, *Target Investor*, takes the value of one if the investor holds the target stock a year prior to the acquisition announcement. The main independent variable in Panel B, *Target Neighbor*, takes the value of one if the investor lives within three miles of a target investor and is not a target investor him-/herself. Investor-level controls include the investor's income, age, number of children, number of family member, gender, and marital status. Zip code level controls include the zip code population, fraction of male residents, average house value, number household members, and household income. We only consider accounts that perform at least one trade in the two-year window surrounding an acquisition; we further require that investors do not trade or hold any stocks from the acquirer industry in that two-year window. Columns (1) and (2) report regression results based on stock-financed M&As; Columns (3) and (4) report regression results based on cash-financed M&As. Standard errors, shown in brackets, are clustered at zip code and time levels. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Target Investors

	Stock-Financed M&As		Cash-Financed M&As	
	(1)	(2)	(3)	(4)
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
Event-Fixed Effects	YES	YES	YES	YES
Adj. R^2	1.50%	1.44%	2.35%	2.24%
No. Obs.	6,943,336	6,943,336	3,220,313	3,220,313

Panel B: Target Neighbors

	Stock-Financed M&As		Cash-Financed M&As	
	(1)	(2)	(3)	(4)
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
Event-Fixed Effects	YES	YES	YES	YES
Adj. R^2	1.50%	1.45%	2.35%	2.24%
No. Obs.	6,941,105	6,941,105	3,219,641	3,219,641