The Speed of Communication*

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Abstract

Drawing from prior research on disease contagion, we estimate a transmission matrix to quantify the communication rate across investors, as well as how it varies with distances in social characteristics (such as age, income, and gender). In particular, exploiting cross-industry stock-financed mergers and acquisitions as a source of plausibly exogenous shocks to some investors' portfolio composition and consequently, their information gathering activity, we trace out the path of "contagion" from these directly impacted investors ("patient zero") to their neighbors. Further, we link the speed of communication to various social characteristics; quantitatively, our estimates imply that a ten-year difference in age, a one-step difference in income, and having a different gender lower the communication rate by 12%, 14%, and 32%, respectively.

JEL Classification: G11, G12, G14, G20

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1. Introduction

The question of how information, or noise, is transmitted in the marketplace is at the heart of asset pricing and economics in general. One important channel through which information or noise travels is via 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) argues that word-of-mouth transmission of ideas can be an important source of short-term fluctuations in the stock market.

In an effort to examine the effect of word-of-mouth communication on economic agents' behavior, a number of recent studies document a positive correlation in stock trading activity across investors that are likely to be in direct contact with one another. Hong, Kubik and Stein (2005), for instance, find that mutual fund managers increase their stock purchases (sales) when other managers from different fund families in the same city increase their purchases (sales) of the same stock.[1]

While these prior findings are consistent with the notion that word-of-mouth communication can affect investor behavior, they are silent on an important aspect of social interaction: the speed at which information or noise travels in the population and becomes reflected in economic agents' behavior. To illustrate, let's draw an analogy with studies of disease transmission – think of the person with the information (or noise) as the infected, and those who do not possess the knowledge as the susceptible. In epidemiology, it is useful to know if a disease is infectious; it is

[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.

perhaps even more important to know the speed of contagion in the population, along with the determinants, so as to stop the epidemic.

This exact logic carries over to the setting of financial markets. From a policy/practical perspective, knowing the rate of communication and its determinants can help design strategies to most efficiently disseminate information (e.g., about the availability of small business loans or government sponsored health care programs) to its target audience; or in some situations, to prevent the dissemination of information among the unintended audience. From a theoretical perspective, knowing the rate of communication and the determinants can help calibrate models that study the effect of communication among economic agents on aggregate economic outcomes (e.g., prices, growth, and investment).

Empirically, to estimate the rate at which economic agents communicate with one another, we need to identify the source of the information or noise, 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 assign an investor (a.k.a., "patient zero") to start collecting information on some stock S. The rate at which other investors in the nearer vs. further away neighborhoods start trading stock S, or related firms, would then inform the researcher about the speed of communication among investors.

Motivated by this hypothetical, ideal setting, we exploit *cross-industry stock-financed* mergers and acquisitions (M&As) as a source of plausibly exogenous variation in some investors' portfolio composition and their subsequent information collection activity. In particular, we exploit the fact that, at the completion of a stock-backed cross-industry 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" then have a stronger incentive to gather information about the acquirer industry – at the minimum, target investors would now have to think carefully about when to sell their holdings in the acquirer firm. This elevated level of attention/information gathering in the acquirer firm can then lead to an increase in target investors' trading activity in the acquirer industry. More important for our purpose, if target investors also communicate their views and opinions to other investors in the same neighborhood ("target neighbors") via word-of-mouth, then shocks to target investors' portfolios can further lead to an increase in target neighbors' trading activity in the acquirer industry. Thus, by tracing out the path of "contagion" across target neighbors, we can then quantify the speed of communication among investors.

To implement our empirical tests, we collect data on all cross-industry M&A deals for the period 1991 to 1996, which are then matched to detailed trading records of 78,000 US households from a discount brokerage.[2] We categorize cross-industry M&A transactions into stock-financed and cash-financed ones: the former are at least partially financed through equity, while the latter are 100% financed by cash. After each 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. We repeat this exercise for neighbors of target investors, whereby a neighbor is defined as a non-target investor who lives within three miles of any target investor.

We perform our tests in two steps. First, in a simple static setting, we verify that target investors indeed are affected by exogenous shocks to their portfolio composition, and further communicate their views and opinions to their neighbors. More specifically, we compare target investors' and target neighbors' trading activity

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

in the acquirer industry in the year prior vs. subsequent to the M&A, relative to other investors in our sample.

The data support the notion that social interactions affect investors' trading decisions. In the year after the completion of a stock-financed cross-industry M&A, relative to other investors, target investors more than double their trading frequency in the acquirer industry (excluding the acquirer firm itself). More importantly, neighbors of target investors also increase their trading frequency in the acquirer industry by over 11% in the same period. Consistent with social interaction playing an important role, the neighbor effect becomes statistically and economically insignificant as we expand the geographical distance (e.g., for neighbors that reside more than 7 miles away from a target investor) and also when we extend our analysis to the second or third year after M&A completion.

In a series of placebo tests to help rule out alternative interpretations, we show that our documented effect completely disappears if we instead a) examine investors' trading behavior around cash-financed M&As (where target investors receive cash as opposed to shares in the acquirer firm), or b) examine investors of the pseudo target firm – i.e., the firm that is in the same industry as the target firm with the closest size and book-to-market ratio.

Our second and main set of tests draws from prior research on disease transmission. The fundamental idea is to estimate an N by N transmission matrix B from one period to the next (assuming N households in the system). To examine the effect of communication over P periods, we simply raise the matrix B to the power of P. The (i,j)th element in this matrix then captures the impact of household j's behavior on household i over one period, and vice versa for the (j,i)th element. It is worth noting that, just as in epidemiology, the matrix B reflects the joint effect of communication among investors (the contact rate), as well as the probability that an investor would act upon the advice from his neighbor (the transmission risk given contact). Without differentiating between the two components, we focus our analysis on the (effective) communication rate.

To estimate this transmission matrix, after each M&A event, we trace the monthly trading activity of each household (living within a certain distance from any target investor) in the acquirer industry (excluding the acquiring firm itself) over the next 12 months – i.e., a maximum P of 12. (Our results are similar if we instead use trading data from the next 6, 9, 15, or 18 months). We then instrument the initial shock to investors' portfolio composition and incentives to gather information by the target dummy – which equals one if the investor was holding target firm shares prior to the M&A announcement, and zero otherwise. Following this procedure, we end up with a set of 12 event-time equations, which need to be estimated jointly.

For simplicity, we impose a linear structure on the elements of the transmission matrix. That is, we explicitly assume that $B_{i,j}$ is a linear function of the physical and social distances between households i and j. The intercept in this linear specification reflects the base rate of communication (that is, with all distances equal to zero), and the residual term captures the unobserved determinants of communication rates.

In our baseline specification, we include three social characteristics in this linear equation: income, age, and gender, all of which are likely to impact the effective communication rate between two investors. Quantitatively, our estimates imply that a ten-year difference in age, a one-step difference in income (as defined by the brokerage firm that supplied the data), and having a different gender lower the communication rate by 12%, 14%, and 32%, respectively.

In additional tests, we also include state dummies in the equation of $B_{i,j}$ to estimate the residual state-fixed effects after controlling for observable social characteristics. We then correlate these state-fixed effects with existing proxies for sociability at the state level compiled by Putnam (2000). Corroborating our findings, there is generally a positive correlation between our estimated state-level communication rate (that is extracted from investors' trading behavior) and the various components of the sociability index compiled from survey data. For example, the correlation between our measure of the state-average communication speed and a key variable in Putnum's (2000) survey – whether one takes advice from a friend – is over 50%.

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. The answer to this question has implications for whether social interactions among investors, at least within our setting, are improving price efficiency. We examine this issue by constructing long-short portfolios tracking target investors' and their neighbors' trading decisions. The results are consistent across all specifications: stocks bought (in the acquirer industry) subsequently underperform stocks sold by these investors (albeit insignificantly), irrespective of the holding period and before transaction costs. These results suggest that retail investors, to a large extent, exchange noise rather than value-relevant information via word-of-mouth communication.

2. Data

2.1. Data Sources and Descriptive Statistics

We mainly use two data sources in this study. First, we obtain detailed trading and holdings records for a subsample of US households for the period from 1991 through 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 end-of-month holdings from the "position file". Finally, we get various household/investor characteristics, such as age, income, and location (zip code), from the "information base file". These three files can be linked by a unique household identifier and brokerage account number. Note that one household can have multiple accounts at the brokerage firm in our sample. Going forward, we use the terms "brokerage account holder" and "investor" 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 in the same six-year window. We require that the acquirer- and the target firm reside in two different industries, where industries are defined based on the Fama-French 49 industry classification. Using alternative industry classifications, such as the Fama-French 38 or 30 industry classifications or the GICS industry classification, does not change the main results of the paper. We exclude M&As for which we cannot identify the acquirer's or the target's industry category. We separate M&A deals into those that are stock-financed and those that are cash-financed; the former represent M&A deals that are at least 50% financed by stock payments, while the latter are 100% cash-financed.

Our final sample contains 460 M&As from 1991 through 1996, out of which 317 are stock-financed and 143 are cash-financed transactions. 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 each investor in the sample to 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 further require that these investors have no existing positions in the acquirer industry prior to the M&A announcement to avoid trading in the subsequent period due to hedging or rebalancing reasons; in particular, target investors that have prior holdings in the acquirer industry may mechanically sell their existing holdings upon receiving acquirer shares as a way to reduce their exposure to the acquirer industry.

We end up with a sample of about 70,000 investor accounts (down from around 150,000 in the original sample). Panel B of Table 1 provides summary characteristics for these accounts. The median and mean portfolio size is $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 distributions of these variables are all highly right-skewed, suggesting that there are a few wealthy, active investors that account for a considerable portion of holdings- and trading activities. The average investor age in our sample is 42 and the average annual household income is $69,500.

We augment our sample with geographic information from the US Census Bureaus' zip code database, which includes the population and the average household income for each zip code. Using the home zip codes information in the brokerage database, we compute the distance between any two investors using the longitude and latitude associated with each zip code adjusted for curvature.[3]

We categorize US zip codes based on various measures of sociability. Similar to Ivkovich and Weisbenner (2007), our sociability indices are from the DDB lifestyle survey data, which is conducted from 1975 through 1998, and is used in a number of sociology studies (e.g., Putnam (2000)). Out of the hundreds of questions asked in the survey, we use three indicators: class or seminar attendance, club meeting attendance, and community project participation. Since the survey is conducted 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 behavior in the early 1990s. We focus on this particular setting to carry out our empirical estimation of the communication rate and its determinants for a number of reasons.

First, the median retail investor in this sample holds three stocks. As such, substituting any one position (out of three) with another stock from an entirely different industry is likely to have a significant impact on the investor's attention and incentives to collect information. In contrast, institutional investors, on average, hold over 100 stocks in their portfolios that span a diverse set of industries. Thus, any switch in position is likely to have a much-diminished impact on institutional investors' information gathering activity.

[3] 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.

Second, our empirical design requires a simple environment, in which we can cleanly measure the "distance" between any two investors. With the emergence of internet and mobile communication, physical distances no longer represent a meaningful barrier to communication as a substantial portion of communication is now carried out through the internet. As a result, we choose a period that predates the internet age.

Third, our discount brokerage dataset provides detailed information not only on the trading- and holdings decisions of a large sample of retail investors, but also on their physical locations and social characteristics, both of which are required for our empirical analyses.

Given that we focus on a particular sample of households in the early 1990s, our empirical design is also subject to a few caveats. First, the landscape of the U.S. equity market has changed dramatically in the past three decades. The fraction of shares held directly by households has steadily decreased from nearly 50% in 1990 to less than 20% today. This raises questions of whether we can extrapolate our results to today's marketplace.

Second, the set of households in our sample is not randomly drawn – by construction, they are all clients of the same discount brokerage firm. To the extent that having a common broker is an indication of belonging to the same social network, our sample of households is likely to be better connected to one another than the average U.S. household. Our estimate of the baseline communication rate is therefore likely to be upward biased.

Third, the average rate of communication is also constantly changing. People used to rely on face-to-face communication to disseminate information or noise. With the advancement in information technology and social media, more and more of the daily social interaction has shifted from meeting in a coffee shop to remote, online communication. As a result, the speed of communication is orders of magnitudes faster than that two or three decades ago.

In light of all these caveats, the focus of this paper is not the base rate of communication (which is estimated from a non-representative sample of U.S. households and is unlikely to hold constant over time). Rather, the main objective is to estimate how the communication rate varies with geographic and social distances. For example, for a ten-year increase in age difference, the communication rate between two people drops by X%. To the extent that there are inherent components in social structures and norms, the determinants of the communication rate (i.e., the slope estimates) are more likely to generalize to different investor groups as well as across time.

3. A Static Setting

The main purpose of this paper is to explore the causal impact of social interactions on investor trading behavior and quantify the intensity of the communication effect. This section provides evidence on the impact of social interactions on investor trading behavior. The next section quantifies the intensity of the communication effect and examines what factors and to what degree each of the factors increase or decrease the likelihood of a word-of-mouth epidemic.

Our innovation is to use stock-financed M&As as a source of exogenous variation in investors' portfolios. Specifically, we exploit the fact that investors in the target firm, at M&A completion, receive some shares of the acquiring firm. Our empirical approach rests on two simple premises. First, investors' decision to invest in the target firm is not driven by their desire to invest in the acquirer firm through the M&A. Second, upon endowment of acquirer shares, target investors start to gather information on the acquirer industry (e.g., to find a good time to exit); further, they spread their newly acquired views and opinions to other investors in the same community through word-of-mouth. The first assumptions appears innocent as investors interested in the acquiring firm can purchase acquirer shares directly in the secondary market without having to worry that the M&A falls apart. The next subsection assesses the validity of the second assumption.

3.1 Target Investors

We assume that target investors start to collect information on the acquirer industry after becoming owners of the acquirer firm. Since we do not directly observe investors' information set, we instead focus on their trading decisions, which ultimately are dictated by their information. We exclude the acquirer firm from our calculation of trading frequency to avoid any mechanical effect, as target investors are bound to sell their holdings in the acquirer firm in the following period. We further require that investors in our sample do not hold any stocks in the acquirer industry before the M&A announcement to avoid trading in the acquirer industry in the post-M&A period due to hedging or rebalancing concerns.

More specifically, we estimate the following linear regression equation:

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

where $Trading\_Freq_{i,m,Acq}$ is the trading by investor i in the acquirer industry (excluding the acquirer firm) as a fraction of her total trading across all industries after stock-financed M&A m. Trading in each period is measured by both the number of trades and dollar value of trades. Since the completion date is missing for many M&A deals, we examine trading behavior in months 6 to 18 after the announcement day. We skip six months in our analysis because it takes, on average, six months for a M&A to complete. The main independent variable in the regression is $Target\_Investor_{i,m}$, which is a indicator variable that takes the value of one if investor i holds shares in the target firm in the month before the M&A announcement. In robustness checks, we define target investors based on their holdings one year prior to the M&A announcement (at which point retail investors are unlikely to have been able to forecast future M&A activities) and our main results still go through.

The set of control variables in the regression can be broadly categorized into two groups: investor/household characteristics and demographic characteristics. The former includes the household income, number of children, number of family member, the investor's age, gender, and marital status; the latter includes the zip code population, fraction of male residents, average home value, number of household members, and household income. We also include a set of M&A dummies in the regression to absorb any M&A-specific effects. The standard errors are clustered at the zip-code- and time levels.

The regression results are reported in Panel A of Table 2. The dependent variable in the first four columns is the trading frequency in the acquirer industry based on the number of trades, and that 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 additional 2.48% compared with other investors (t-statistic = 5.40). To put this number in perspective, the unconditional trading frequency in any industry is 2.04%. In other words, ownership of acquirer stocks induces target investors to more than double their normal trading activities in the acquirer industry. Further, as can be seen in Columns (2)-(4), controlling for investor and demographic characteristics and M&A-fixed effects has virtually no impact on our results.

Regression coefficients reported in Columns (5)-(8), which are based on a dollar-weighted measure of trading intensity, are almost identical to those in Columns (1)-(4). For instance, in the full specification, target investors increase their trading intensity in the acquirer industry by 2.03% more than other investors (t-statistic = 4.32).

While these results are consistent with the notion that stock ownership induces investors to collect information on related stocks and ultimately trade on these stocks, there are alternative interpretations. In particular, consider the possibility that target investors are economically linked to the target firm (e.g., target investors are employees of the target firm, or work for suppliers or customers of the target firm). After the M&A, these target investors become affiliated with the acquirer firm. Emboldened by such affiliation, target investors believe they now understand the acquirer's business better, and start trading more comfortably and frequently in other firms in the acquirer industry.

To address this alternative interpretation, we perform the same sets of tests around cash-financed M&As. If our results are truly driven by M&As directly impacting investor beliefs and preferences, we should observe a similar change in trading intensity around cash-financed M&As. In contrast, if our results are due to stock ownership inducing investors to collect more information, we should observe no effect for 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 shown in this section confirm our suspicion that when endowed with shares of a firm, investors start gathering information about the firm's underlying business and increase their trading in related firms in the same industry.

3.2 Target Neighbors

We now turn to neighbors of target investors. Unlike prior studies that examine the relation between local investors and firms, we use a rather narrow definition of neighbors – households that live within a three-mile radius (as opposed to 60 miles). This is because the likelihood of two individuals coming into direct contact with each other diminishes rapidly with distance.

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

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

where $Target\_Neighbor_{i,m}$ is an indicator variable that takes the value of one if investor i lives within a three-mile radius of any target investor and is not a target investor him-/herself. If an investor lives within three miles of more than one target investor, we only count that investor once. In unreported analyses, we assign more weights to neighbors of multiple target investors, and the results are by and large unchanged. We also require that investors in our sample do not hold stocks in the acquirer industry prior to the M&A announcement.

Panel A of Table 3 reports target neighbors' trading behavior around stock-financed M&As. Similar to Table 2, the dependent variable in the first four columns of Panel A is the trading intensity in the acquirer industry based on the number of trades, while the dependent variable in the next four columns is the 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 target investors disproportionately increase their trading intensity in the acquirer industry by 39bps after the M&A (*t*-statistic = 4.88). Controlling for investor- and demographic characteristics and M&A-fixed effects only mildly reduces the coefficient estimates. In the full specification, the coefficient on *Target_Neighbor* remains as high as 23bps with a *t*-statistic of 3.29. That is, target neighbors increase their trading intensity by over ten percent of the unconditional trading intensity in a given industry, which is 2.04%. The results based on dollar value of trades, shown in the next four columns, are virtually identical to those reported in the first four columns. The coefficient estimate on *Target_Neighbor* in the full specification is 22bps with a *t*-statistic of 3.14.

Comparing the results shown in Panel A of Table 2 with those 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 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*."

We again replicate the whole set of analyses for cash-financed M&As. If neighbors of target investors increase their trading in the acquirer industry because the M&A directly impacts neighbors' beliefs or preferences (through economic affiliations), we should observe a similar pattern in trading around cash-financed M&As. In contrast, if neighbors of target investors increase their trading because of word-of-mouth communication with target investors, we expect cash-financed M&As to have no impact on neighbors' trading decisions.

The regression results, shown in Panel B of Table 3, are consistent with the latter explanation. The coefficient estimate on *Target_Neighbor* in the full specification (Columns (4) and (8)) is almost zero, with a *t*-statistic below 0.3.

Another related concern is that cross-industry M&As cause acquirer- and target industry to be discussed jointly in the media, which, in turn, causes investors to trade in both industries. The non-result for cash-financed M&As does not corroborate this alternative view of the data. To further investigate this channel, we look at investors that, at the time of the M&A, hold shares in the target industry, but not the target firm itself. We then examine their subsequent trading behavior in the acquirer industry as well as that of their neighbors. The results are reported in Table 4. Similar to the placebo test based on cash-financed M&A, we observe no increase in trading activity in the acquirer industry. This applies to both "target" investors and their neighbors.

3.3 Alternative Specifications

If social interactions play a major role, we 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 A1.

In our first set of tests, we vary the distance over which we define neighbors. When we increase the distance from 3 to 7 miles and exclude both target investors and target neighbors within three miles from the sample, the coefficient estimate on *Target_Neighbor* in the full regression specification using the dollar-weighted measure of trading intensity drops by about 20% to 18bp (from 22bp). As we further increase the distance to 15 miles, the coefficient estimate on *Target_Neighbor* drops by another 20% to 14bp. Both estimates remain statistically significant at all conventional levels. Stock-financed M&As have virtually no impact on neighbors that reside between 15 and 30 miles of a target investor and neighbors that are more than 30 miles away. We make almost identical observations when switching the dependent variable to trading intensity based on the number of trades. This rapid decrease in coefficient estimates is consistent with the idea that word-of-mouth effects decay quickly with distance.

We also experiment with the time period over which we measure investors' trading intensity. Specifically, instead of focusing on the one-year period after M&A completion (i.e., months 6-18 after 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 the acquirer industry compared with other investors. In the baseline regression, target investors exhibit a trading propensity in the acquirer industry that is 2.30% higher than the rest of the investors in months 6-18 after the M&A announcement (Table 2, Panel A, Column (4)). This figure drops to 1.78% in months 18-30, and to 1.23% in months 30-42. The drop in trading propensity for target neighbors is even more pronounced. There is no discernible difference in trading intensity between target neighbors and other investors beyond month 18: The coefficient estimate on *Target_Neighbor* is 5bp and 1bp in months 18-30 and months 30-42, respectively, and both are statistically insignificant.

We also test what happens during months 1 to 5. In particular, an attention-based explanation of our findings is that shareholders of target firms and their neighbors trade similarly because they are exposed to common information due to local media and spillover effects. If so, our patterns should be stronger around the M&A announcement date, not the completion date. We find that target investors and target neighbors trade more frequently from month 7-18 in the acquirer firms' industry than during months 1-6, which suggests that our results are not driven by an attention effect or common information story.

4. A Dynamic Setting

Having provided some baseline evidence on the impact of social interactions on investor trading behavior, we now attempt to examine what factors affect the speed of communication.

In essence, we estimate a transmission matrix that quantifies how views and opinions percolate through households from one month to another:

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

where $X_{i,t}$ is the trading activity of household i in the acquirer industry in period t and $X_{i,t+1}$ is the trading activity of household i in the acquirer industry in period t+1. To express this transmission matrix in a vector form, we have

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

While in the previous regression equation, we estimate to what degree "patient zero" increases trading in the target industry for households that live within a three-mile radius, we now dynamically estimate the cumulative effect from being a neighbor of patient zero, being a neighbor of a neighbor of patient zero, etc. Put differently, we now estimate not only the fraction of "primary-case infected", but also the fraction of "secondary-case infected" in period t+1, the fraction of "tertiary-case infected" in period t+2 etc. Accumulating this over p periods, we get

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

We employ a two-stage approach to estimate the matrix B. In the first stage, we instrument the set of independent variables, X_t through X_{t+p-1}, using portfolio shocks experienced by target investors at the merger completion date. Specifically, we estimate regression equations of trading activity by household i in the acquirer industry on $Target_i$, which is a dummy variable that equals one if investor i was holding target shares at the time of the M&A announcement. Trading activity in the acquirer industry is defined as the total number of trades (or total dollar value of trades) in the acquirer industry divided by the total number of trades (total dollar value of trades) across all industries.

The results from the first-stage regressions are reported in Table 5. We find that trading activity in the acquirer industry increases with the presence of target investors at all horizons, from six months following the M&A to seventeen months following the M&A. This pattern emerges irrespective of whether we measure trading activity based on the number of trades or the dollar value of trades.

In the second stage, we estimate how trading activity in the acquirer industry in period $t+p$ relates to the *fitted* trading activity in the acquirer industry in period $t+p$-1 from the first-stage regression, where p ranges from 1 to 12:

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

$$X_{t+2} = B^2 * \widehat{X_t} + e_{t+2} = B * \widehat{X_{t+1}} + \epsilon_{t+2}$$

$$\ldots$$

$$X_{t+12} = B^{12} * \widehat{X_t} + e_{t+12} = B * \widehat{X_{t+11}} + \epsilon_{t+12}.$$

Our focus centers on the matrix of beta coefficients, which capture the speed at which the information spreads. We conjecture the speed of communication to be a function of (1) $Dist_{ij}$, which is the geographic distance between households i and j, (2) $|Income_{ij}|$, which is the income gap between households i and j, (3) $|Age_{ij}|$, which is the age gap between the "heads" of households i and j,[4] and (4) $|Gender_{ij}|$, which is the gender gap between the "heads" of households i and j. We further include state dummies in the equation to estimate the (residual) state-fixed effects (after controlling for the observables) in the communication rate, and correlate these state effects with existing proxies for sociability.

To facilitate the computation of the matrix of beta coefficients, we impose a linear structure on all of the elements in the transmission matrix:

$$\beta_{i,j} = b_0 + b_1 * Dist_{i,j} + b_2 * |IncomeDiff_{i,j}|$$

$$+b_3 * |AgeDiff_{i,j}| + b_4 * |GenderDiff_{i,j}| + \varepsilon_{i,j},$$

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

where $\varepsilon_{i,j}$ captures the unobserved determinants of $\beta_{i,j}$, and b_0 captures the baseline communication rate with zero distance in every social dimension. Scaling our estimates of b_1, b_2, b_3, and b_4 by our estimate of b_0 therefore yields the proportional change in the 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 0.334 in Column (1) and 0.332 in Column (3) depending on whether trading activity is based on the number of trades or the dollar value of trades, respectively. As alluded to in Section 2.2, we believe these two estimates to be upward biased (relative to the average U.S. households). Despite this upward bias, our estimates are far below one and the equivalent basic reproduction ratios of some of the most studied diseases such as HIV/AIDS, SARS and Ebola. In other words, unlike infectious diseases, which require counter-measures such as isolation or vaccine to contain the epidemic, our baseline communication rate with zero distance in all observable social dimensions implies that industry information gathered by "patient zero" is unlikely to trigger an epidemic. Instead, the communication effect can be expected to die out.

The coefficients on the age, income, and gender differences suggest that the communication rate varies significantly with social closeness/distance. In particular, when scaling our estimates of b_2, b_3, and b_4 by our estimate of b_0, our results suggest that a ten-year difference in age, a one-step difference in income, and having a different gender lower the communication rate by 12%, 14%, and 32%, respectively. That is, under certain conditions, the communication effect can be expected to die out rather quickly, whereas under alternate conditions, the communication effect can be expected to die out very slowly.

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

In our final test, we examine whether investors in our setting transmit value-relevant news or simply spread noise. The answer to this question has important implications about whether social interactions among investors, at least within our setting, are improving market efficiency or adding noise to the price.

We examine this issue via long-short portfolios across target investors and their neighbors. At the end of each month *t*, we look at all stocks in the acquirer industry excluding the acquirer firm itself that were bought and all stocks in the acquirer industry excluding the acquirer firm itself that were sold by target investors and their neighbors during month *t*. We experiment with three portfolio construction schemes:

1) For each stock in the acquirer industry (excluding the acquirer itself), we compute the total number of shares bought by target investors and their neighbors minus the total number of shares sold. We go long the stocks for which these investors are net buyers and go short the stocks for which they are net sellers. The long and short portfolios are then weighted by the net trading across all target investors and target neighbors, and are held for 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 in the acquirer industry, we compute the aggregate portfolio weight change across all target investors and their neighbors. We form long-short portfolios as in 1).

The results are reported in Table 7. Irrespective of the portfolio formation scheme, we find that the long portfolio subsequently underperforms the short portfolio, albeit not statistically significantly so. These results suggest that the newly acquired views and opinions about firms in the acquirer industry mostly reflect noise rather than value-relevant news.

6. Conclusion

Exploiting cross-industry stock-financed M&As as a source of plausibly exogenous shocks to investors' portfolio composition, we examine the intensity with which investors communicate with one another, as well as the determinants of the communication rate. Specifically, our empirical strategy rests upon the simple premise that once endowed with shares of the acquiring firm, target investors start paying attention to and gathering information about the acquirer industry; more importantly, target investors communicate their views and opinions to other investors residing in the same neighborhood, as geographic proximity facilitates the exchange of ideas via word-of-mouth.

We conduct two sets of tests to quantify the impact of word-of-mouth communication on investor behavior. First, in a static setting, we show that in the year after M&A completion, target investors double their trading activity in the acquirer industry (excluding the acquirer firm) relative to other investors in the sample. Moreover, neighbors of target investors that live within a three-mile radius increase their trading intensity in the acquirer industry by more than 11% during the same period. Consistent with the communication channel, we show in a series of placebo tests that our results disappear if we instead a) use cash-financed M&As (where target investors receive cash as opposed to shares of the acquirer firm), and b) focus on pseudo target firms – i.e., firms in the same industry as the actual target firm and with similar characteristics.

Our second set of tests draws from research on disease transmission. Using essentially a VAR approach, we estimate a transmission matrix to quantify how views and opinions percolate across investors from one period to the next. For simplicity, we assume that all elements in the transmission matrix are linear functions of distances in social characteristics, such as age, income, and gender. Quantitatively, our estimates imply that a ten-year difference in age, a one-step difference in income, and having a different gender lower the communication rate by 12%, 14%, and 32%, respectively. Moreover, at the state level, we find a large positive correlation between our estimated state-average communication rate that is extracted from investors' trading behavior and the various components of the sociability index compiled from survey data.

Finally, we examine whether target investors and neighbors are trading on superior value-relevant news or responding to noise. Our results, consistent aross all specifications, suggest that retail investors, to a large extent, exchange noise rather than useful value-relevant news via casual, word-of-mouth communication.

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Table 1. Summary Statistics

This table reports summary statistics of our sample. Panel A presents statistics for the M&A sample from the SDC database. Stock-financed M&As are defined as those at least partially financed by stocks; cash-financed M&As are 100% financed by cash. Firm size is calculated as the number of shares outstanding multiplied by the share price as of the month prior to the M&A [millions]. Panel B shows investor- and portfolio characteristics for the retail investor sample used in Baber and Odean (2001). We only include retail investors who have 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 two year window surrounding the M&A. Portfolio size is the dollar value of the stock holdings. Number and value of trades are the total number of trades and the total dollar value of trades. All observations are at the account/year-month level. Panel C shows demographic information for each zip code in our sample. All observations are at the zip-code/year-month level. The three sociability indices are: Class or Seminar Attendance, Club Meeting Attendance, and Community Project Participation all of which are measured at the state/year level.

	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 ($)	70,608	45,000	62,500	87,500	69,500	30,064
Panel C: Zip Code Characteristics						
Basic Characteristics						
Population	42,057	785	2,777	11,960	8,965	13,134
No. Household Members	42,057	2.40	2.56	2.73	2.59	0.35
House Value ($)	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 Indices (measured at the state level)						
Class or Seminar Attendance	294	1.88	2.03	2.23	2.07	0.31
Club Meeting Attendance	294	2.07	2.26	2.45	2.29	0.41
Community Project Participation	294	1.47	1.57	1.70	1.60	0.22

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 an 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 six to 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 six to eighteen after the M&A is announced. We skip six months because it takes an average of six months for the M&A to complete after its initial announcement. The main independent variable 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 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.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A: Stock-Financed M&As				
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				
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 Neighbours' Trading in the Acquirer Industry

This table reports coefficient estimates from regressions of investors trading in the acquirer industry on a target neighbour dummy. The observations are at an 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 six to 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 six to eighteen after the M&A is announced. We skip six months because it takes an average of six months for the M&A to complete after its initial announcement. The main independent variable 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 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.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A: Stock-Financed M&As				
Target Neighbour	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				
Target Neighbour	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. Placebo Test: Investors Holding Other Stocks in the Target Industry

This table repeats the analyses reported in Tables 2 and 3, but now replaces target investors with investors that, at the time of the M&A, hold shares in the target industry, but not the target firm itself. Columns (1), (2), (5), and (6) report regression results for stock-financed M&As, which are defined as acquisitions that are at least partially financed by stocks. Columns (3), (4), (7), and (8) report regression results for cash-financed M&As, which are defined as acquisitions that are 100% financed by cash. 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 Neighbours			
	Stock M&As		Cash M&As		Stock M&As		Cash M&As	
	(1) # Trades	(2) $Trades	(3) # Trades	(4) $Trades	(5) # Trades	(6) $Trades	(7) # Trades	(8) $Trades
"Target" Investor/Neighbour	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 5. Communication Speed: First-Stage

This table reports coefficient estimates from regressions of trading activity in the acquirer industry on a target investor dummy. We focus on stock-based M&As and regressions are estimated separately for each event month, from six months following the stock-based M&A to seventeen months following the stock-based M&A. The observations are at an event/zip-code/year-month level. In Panel A, the dependent variable is the total number of trades in the acquirer industry in the zip code as a fraction of the total number of trades across all industries in the zip code. In Panel B, the dependent variable is the total dollar value of trades in the acquirer industry in the zip code as a fraction of the total dollar value of trades across all industries in the zip code. We skip six months because it takes an average of six months for the M&A to complete after its initial announcement. The main independent variable is an indicator, which equals one if there is a target investor in the zip code in question. Standard errors, shown in brackets, are clustered at the event level. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

Panel A: Trading Frequency

	M6	M7	M8	M9	M10	M11	M12	M13	M14	M15	M16	M17
Intercept	0.018	0.019	0.018	0.017	0.016	0.017	0.017	0.016	0.017	0.017	0.017	0.017
Target Investor	0.021***	0.018***	0.015***	0.022***	0.018***	0.012***	0.019***	0.015***	0.016***	0.011***	0.021***	0.017***
	[0.003]	[0.004]	[0.003]	[0.003]	[0.005]	[0.002]	[0.003]	[0.004]	[0.004]	[0.003]	[0.003]	[0.003]

Panel B: Dollar-Based Trading Frequency

	M6	M7	M8	M9	M10	M11	M12	M13	M14	M15	M16	M17
Intercept	0.019	0.019	0.019	0.018	0.017	0.017	0.017	0.017	0.017	0.017	0.018	0.017
Target Investor	0.022***	0.019***	0.021***	0.022***	0.020***	0.012***	0.020***	0.016***	0.020***	0.008***	0.019***	0.017***
	[0.003]	[0.005]	[0.004]	[0.004]	[0.005]	[0.002]	[0.004]	[0.004]	[0.005]	[0.003]	[0.004]	[0.004]

Table 6. Communication Speed: Second Stage

This table reports coefficient estimates from second-stage regressions of total trading activity in a household in the acquirer industry on its lagged own trading, lagged average trading of the neighbours and its interactions with physical and social distances. The dependent variable in Columns (1) and (2) is the number of trades in the acquirer industry as a fraction of the total number of trades across all industries in months 7 to 18 after the stock-financed M&A is announced. The dependent variable in Columns (3) and (4) is the dollar value of trades in the acquirer industry as a fraction of the total dollar value of trades in months 7 to 18 after the M&A is announced. $\widehat{Trade}_{i,t}$ is the *projected* amount of trading by all neighbour residents based on the first-stage regressions reported in Table 5. Columns (1) and (3) only consider neighbours living within 3 miles, while Columns (2) and (4) consider neighbours living 3 to 7 miles aways. $Avg(\widehat{Trade}_{j,t})$ is the average *projected* amount of trading of their neighbours based on the first-stage regressions reported in Table 5. $Dist_{i,j}$ is the distance between zip codes i and j. Age_i is the average age of all residents in zip code i. $Income_i$ is the average income of all residents in zip code i; $Gender_i$ is the fraction of male in zip code i. Standard errors, shown in brackets, are clustered at event level. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

	DepVar=$Trade_{i,t+1}$			
	(1)	(2)	(3)	(4)
$\widehat{Trade}_{i,t}$	0.736***	0.753***	0.723***	0.741***
	[0.029]	[0.029]	[0.030]	[0.030]
$Avg(\widehat{Trade}_{j,t})$	0.334***	0.345***	0.332***	0.355***
	[0.027]	[0.052]	[0.027]	[0.053]
$Avg(\widehat{Trade}_{j,t} * Dist_{i,j})$	-0.020***	-0.028***	-0.021***	-0.029***
	[0.005]	[0.008]	[0.005]	[0.008]
$Avg(\widehat{Trade}_{j,t} * \lvert Age_i - Age_j \rvert)$	-0.004***	-0.005***	-0.004***	-0.005***
	[0.001]	[0.001]	[0.001]	[0.001]
$Avg(\widehat{Trade}_{j,t} * \lvert Income_i - Income_j \rvert)$	-0.006**	-0.012***	-0.006**	-0.012***
	[0.003]	[0.004]	[0.003]	[0.004]
$Avg(\widehat{Trade}_{j,t} * \lvert Gender_i - Gender_j \rvert)$	-0.176***	-0.168***	-0.178***	-0.170***
	[0.016]	[0.015]	[0.016]	[0.016]
Adj. R^2	0.002	0.002	0.002	0.002
No. Obs.	4,782,541	4,782,541	4,782,541	4,782,541

Table 7. Returns to Target Investor/Neighbour Trading

This table reports monthly returns of hedge portfolios that go long stocks bought by and short stocks sold by target investors and target neighbours. 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

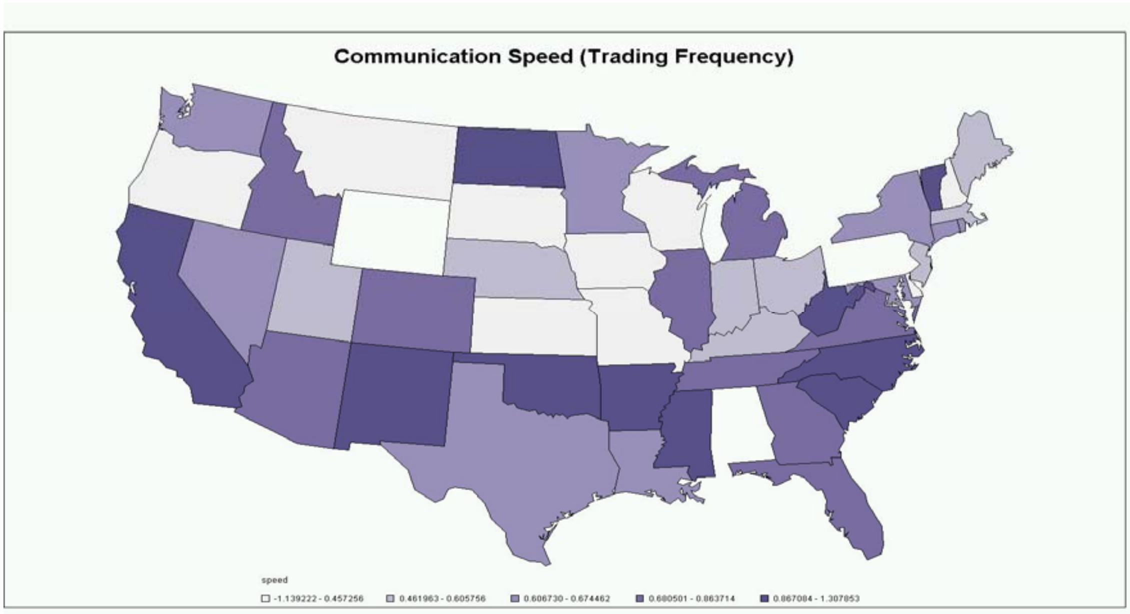

Figure 1. U.S. Heat map of Communication Speed (Trading Frequency)
This figure shows the U.S Heat map for our estimates of state-level communication speed calculated from regressions presented in Table 5. The dependent variable is the number of trades in the acquirer industry as a fraction of the total number of trades across all industries. In addition to the control variables included in Column (1) of Table 5, we allow the coefficient estimate on *Avg(Trade$_j$)* to vary across states. The correlation between our estimate of communication speed and the Social Index mapped in Figure 1 is 0.43 with a p-value below 0.01.

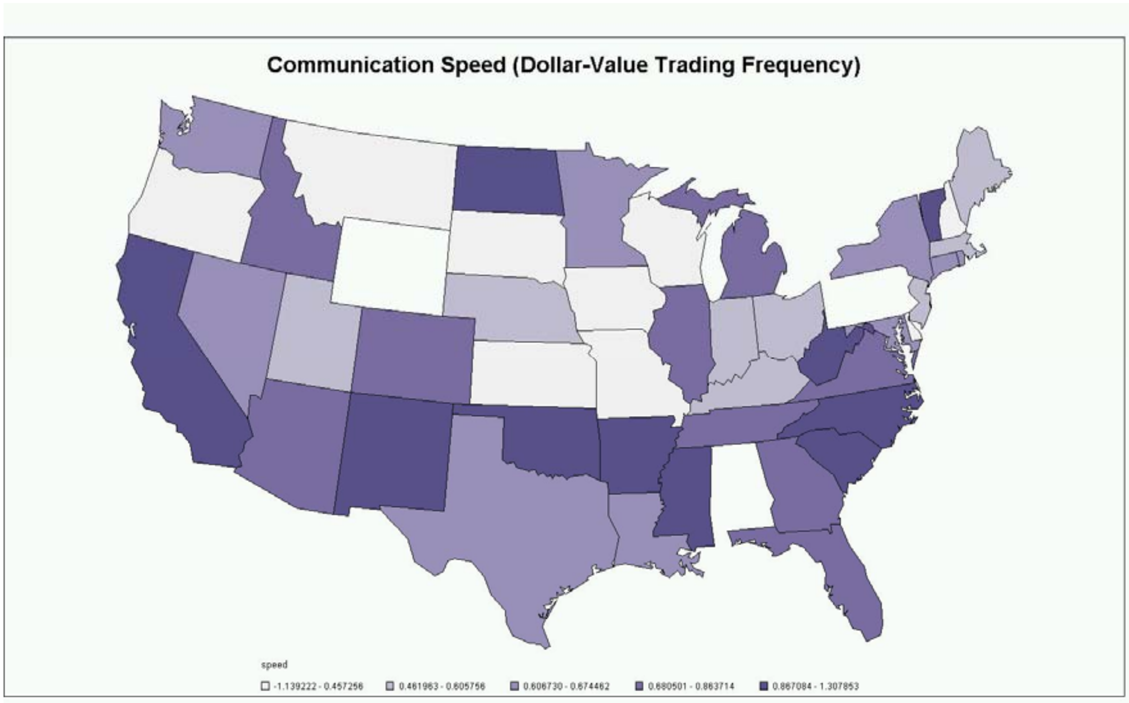

Figure 2. U.S. Heat map of Communication Speed (Dollar-Based Trading Frequency)
This figure shows the U.S Heat map for our estimates of state-level communication speed calculated from regressions presented in Table 5. The dependent variable is the dollar value of trades in the acquirer industry as a fraction of the total dollar value of trades across all industries. In addition to the control variables included in Column (3) of Table 5, we allow the coefficient estimate on $Avg(Trade_j)$ to vary across states. The correlation between our estimate of communication speed and the Social Index mapped in Figure 1 is 0.45 with a p-value below 0.01.

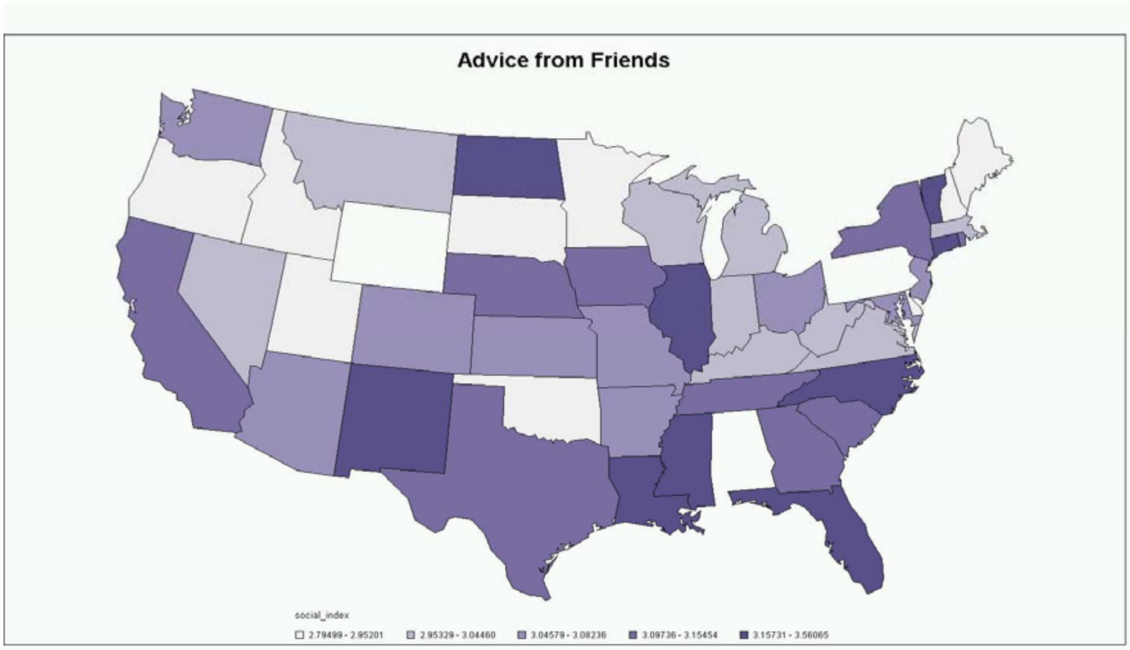

Figure 3. U.S. Heat map of Social Index
This figure shows the U.S. Heat map for the Social Index from Putnam (2000), which captures the frequency people seek advice from friends. We calculate the average state level indices from 1991-1996.

Internet Appendix to "The Speed of Communication"

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Table A1. Different Definitions of Neighbours and Various Time Horizons

This table reports regressions of investors trading in the acquirer industry on the target investor and target neighbour dummies. The dependent variable in columns (1), (3), (5), and (7) in both Panels A, B and C is the number of trades in the acquirer industry (excluding the acquirer firm) as a fraction of total number of trades across all industries, and that 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 variables are the target investor and target neighbour dummies; the former takes the value of one if the investor holds the target stock at the end of the month before the acquisition announcement and the latter takes the value of one if the investor lives within N miles of any target investor (where N varies from 3 to 30 miles) and is not a target investor himself. Investor-level controls include the investor's income, age, number of children, number of family member, gender, and martial 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 include in our sample retail investors that have at least one trade in the two year window surrounding an acquisition; we further require that these investors do not trade or hold any stocks from the acquirer industry in the year before the acquisition. Panel A reports regression results for target neighbours that are defined using various distances. Panel B reports regression results for trades that take place in various event windows. Only stock-financed M&As are considered in these regressions. Panel C reports the difference between investors trading frequency in Month 1-6 and Month 7-12. The columns (1), (2), (5) and (6) are for stock-financed M&As and we run regression for target-investors and target-neighbours respectively. The columns (3), (4), (7) and (8) are for cash-financed M&As and we run regression for target-investors and target-neighbours respectively. 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: Neighbours at Different Distances								
	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 Neighbour	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: Various Time Horizons								
	Target Investors				Target Neighbours			
	Months 18 to 30		Months 30 to 42		Months 18 to 30		Months 30 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
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
Adj. R^2	1.47%	1.39%	1.28%	1.21%	1.47%	1.39%	1.28%	1.21%

Panel C: Difference between Month 1-6 and Month 7-18								
	Target Investors				Target Neighbours			
	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
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
Adj. R^2	1.42%	1.38%	2.06%	1.99%	1.41%	1.37%	2.06%	1.98%

Table A2. The Effect of Social Groups

This table reports regressions of investors trading in the acquirer industry on the target neighbour dummy. The dependent variable in columns (1) and (3) in both Panels A and B 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 6 to 18 after an acquisition is announced, and that 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 6 to 18 after an acquisition is announced. The main independent variable is a dummy variable that takes the value of one if the investor lives within 3 miles of any target investor and is not a target investor himself. Investor-level controls include the investor's income, age, number of children, number of family member, gender, and martial 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 include in our sample retail investors that have at least one trade in the two year window surrounding an acquisition; we further require that these investors do not trade or hold any stocks from the acquirer industry in the year before the acquisition. In Panel A, we include in columns (1) and (2) all target neighbours that are in the same age group as the target investor and the rest in columns (3) and (4). In Panel B, we include in columns (1) and (2) all target neighbours that are in the same income group as the target investor and the rest in columns (3) and (4). In Panel C, we include in columns (1) and (2) all target neighbours that have lived in the current home for more than 5 years and the rest in columns (3) and (4). Only stock-financed M&As are considered in these regressions. 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: Investor Age				
	Same Age Group		Different Age Groups	
	(1)	(2)	(3)	(4)
Target Neighbour	0.0040***	0.0035***	0.0011	0.0012
	[0.0010]	[0.0010]	[0.0008]	[0.0008]
Investor Controls	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES
Event-Fixed Effects	YES	YES	YES	YES
No. Obs.	7,596,415	7,596,415	7,581,187	7,581,187
Adj. R^2	1.66%	1.59%	1.66%	1.59%

Panel B: Annual Income				
	Same Income Group		Different Income Groups	
	(1)	(2)	(3)	(4)
Target Neighbour	0.0026***	0.0026***	0.0011	0.0007
	[0.0007]	[0.0008]	[0.0014]	[0.0014]
Investor Controls	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES
Event-Fixed Effects	YES	YES	YES	YES
No. Obs.	7,596,415	7,596,415	7,566,666	7,566,666
Adj. R^2	1.66%	1.59%	1.66%	1.59%

Panel C: Years in Current Home				
	More Than 5 Years		Less Than 5 Years	
	(1)	(2)	(3)	(4)
Target Neighbour	0.0026***	0.0027***	0.0010	0.0001
	[0.0008]	[0.0008]	[0.0021]	[0.0021]
Investor Controls	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES
Event-Fixed Effects	YES	YES	YES	YES
No. Obs.	6,711,168	6,711,168	6,689,865	6,689,865
Adj. R^2	1.73%	1.66%	1.73%	1.65%

Table A3. The Effect of Population Density

This table reports coefficient estimates from regressions of investors trading in the acquirer industry on a target neighbour dummy. We focus on stock-financed M&As and the observations are at an 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 six to eighteen after the M&A is announced. 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 six to eighteen after the M&A is announced. We skip six months because it takes an average of six months for the M&A to complete after its initial announcement. The main independent variable 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 with MSA codes ("Metropolitan Areas") and those without MSA codes ("Rural Areas"). In Panel B, within all zip codes with MSA codes, 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 Neighbour	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 Neighbour	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 A4. Target Investors Based on Lagged One Year Holdings

This table reports regressions of investors trading in the acquirer industry on the target investor and target neighbour dummies. The dependent variable in columns (1) and (3) in both Panels A and B 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 6 to 18 after an acquisition is announced, and that 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 6 to 18 after an acquisition is announced. The main independent variable in Panel A is the target investor dummy that takes the value of one if the investor holds the target stock a *year* before the acquisition announcement, and that in Panel B is the target neighbour dummy that takes the value of one if the investor lives within 3 miles of any target investor and is not a target investor himself. Investor-level controls include the investor's income, age, number of children, number of family member, gender, and martial 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 include in our sample retail investors that have at least one trade in the two year window surrounding an acquisition; we further require that these investors do not trade or hold any stocks from the acquirer industry in the year before the acquisition. Columns (1) and (2) of both panels report regression results based on stock-financed M&As, which are defined as acquisitions that are at least partially financed by stocks; columns (3) and (4) report regression results based on cash-financed M&As, which are defined as acquisitions that are 100% financed by cash. 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
No. Obs.	6,943,336	6,943,336	3,220,313	3,220,313
Adj. R^2	1.50%	1.44%	2.35%	2.24%

Panel B: Target Neighbours

	(1)	(2)	(3)	(4)
Target Neighbour	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
No. Obs.	6,941,105	6,941,105	3,219,641	3,219,641
Adj. R^2	1.50%	1.45%	2.35%	2.24%

Table A5. Robustness Checks

This table reports regressions of investors trading in the acquirer industry on the target investor and target neighbour dummies. The dependent variable in columns (1), (3), (5), and (7) in both Panels A, B and C 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 6 to 18 after an acquisition is announced, and that 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 in months 6 to 18 after an acquisition is announced. The main independent variable in columns (1)-(4) in both panels is the target investor dummy that takes the value of one if the investor holds the target stock at the end of the month before the acquisition announcement, and that in columns (5)-(8) is the target neighbour dummy that takes the value of one if the investor lives within 3 miles of any target investor and is not a target investor himself. Investor-level controls include the investor's income, age, number of children, number of family member, gender, and martial 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 include in our sample retail investors that have at least one trade in the two year window surrounding an acquisition; we further require that these investors do not trade or hold any stocks from the acquirer industry in the year before the acquisition. In Panel A, we exclude investors that are also holding other target stocks in the sample period. In Panel B, we exclude investors that are within 100 miles of either the acquirer or target firms. I Panel C, we only consider the investors who hold stocks within the targets' industries before the M&A events or trade at least once within one year before the M&A events. Columns (1), (2), (5), and (6) report regression results based on stock-financed M&As, which are defined as acquisitions that are at least partially financed by stocks; columns (3), (4), (7), and (8) report regression results based on cash-financed M&As, which are defined as acquisitions that are 100% financed by cash. 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: Exclude Investors Holding Other Target Stocks in the Sample Period

	Target Investors				Target Neighbours			
	Stock M&As		Cash M&As		Stock M&As		Cash M&As	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Target	0.0230***	0.0204***	0.0045	0.0061	0.0022***	0.0021***	0.0001	0.0000
	[0.0048]	[0.0047]	[0.0035]	[0.0041]	[0.0007]	[0.0007]	[0.0010]	[0.0010]
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.	7,576,448	7,576,448	3,479,807	3,479,807	7,574,164	7,574,164	3,479,091	3,479,091
Adj. R^2	1.66%	1.60%	2.37%	2.25%	1.66%	1.60%	2.36%	2.25%

Panel B: Exclude Investors within 100 Miles of Either the Acquirer or Target

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Target	0.0238***	0.0212***	0.0042	0.0060	0.0017**	0.0015**	0.0003	0.0002
	[0.0049]	[0.0048]	[0.0036]	[0.0041]	[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
No. Obs.	7,497,533	7,497,533	3,449,347	3,449,347	7,495,339	7,495,339	3,448,646	3,448,646
Adj. R^2	1.65%	1.59%	2.38%	2.27%	1.65%	1.59%	2.38%	2.27%

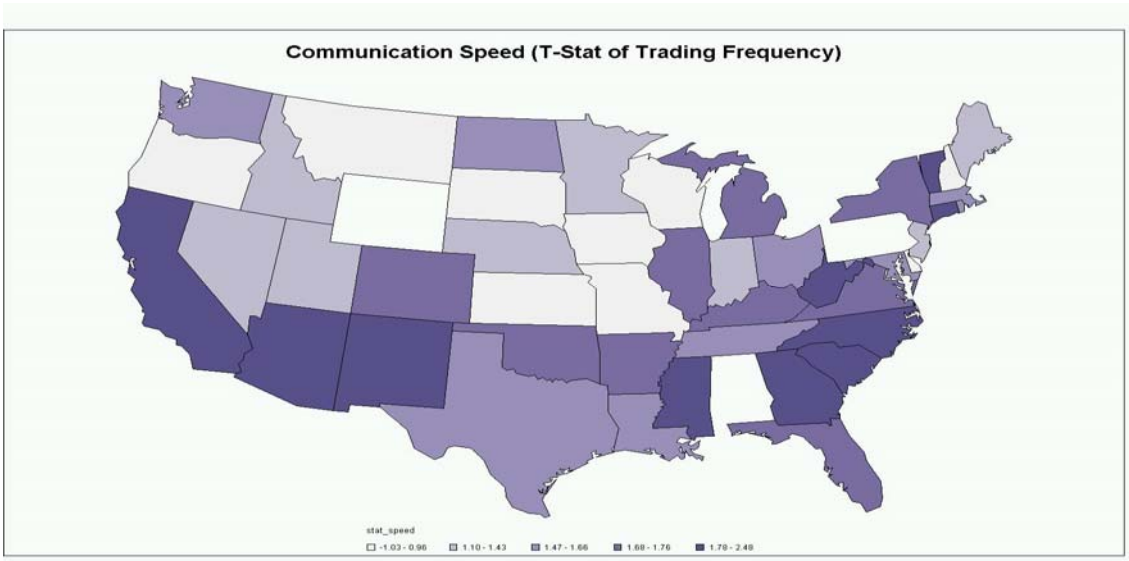

Figure A1 U.S Heat map of communication speed (Trading Frequency)
This figure shows the U.S heat map of state-level the communication speed calculated from the regression in the Table 5. The dependent variable is the number of trades in the acquirer industry as a fraction of total number of trades across all industries. In addition to the control variable in Column (1) of Table 5, we allow the coefficients of $Avg(Trade_j)$ to differ across states. Then we define t-stats of $Avg(Trade_j)$ as the communication speed for each state. The correlation between the communication speed and the social index in Figure 1 is 0.45 with p-value below 0.01.

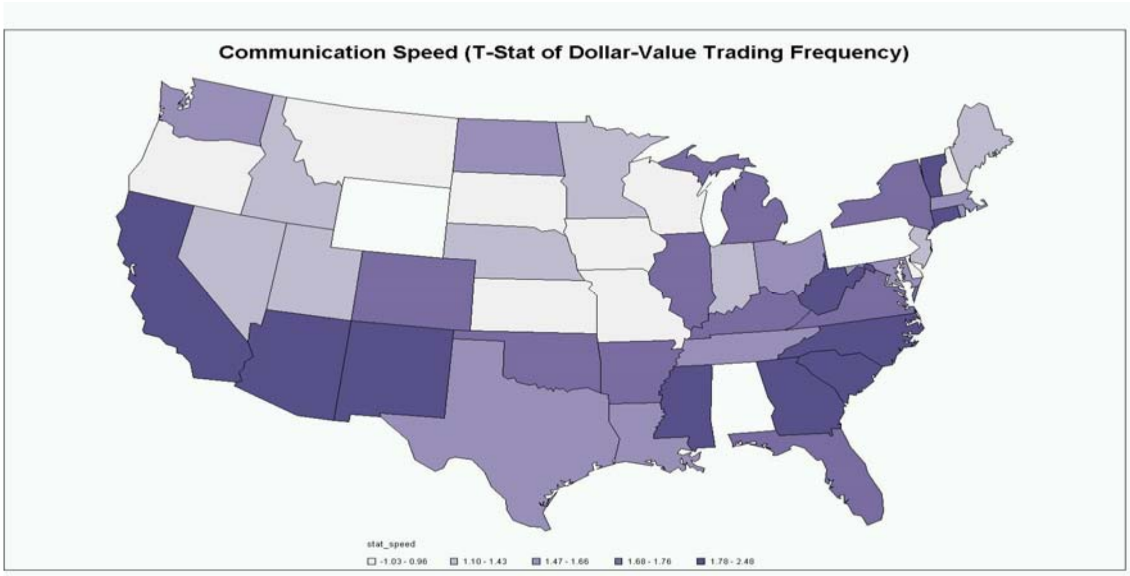

Figure A2 U.S Heat map of communication speed (Dollar-based Trading Frequency)

This figure shows the U.S heat map of state-level the communication speed calculated from the regression in the Table 5. The dependent variable is the dollar value of trades in the acquirer industry as a fraction of total dollar value of trades across all industries. In addition to the control variable in Column (1) of Table 5, we allow the coefficients of $Avg(Trade_j)$ to differ across states. Then we define t-stats of $Avg(Trade_j)$ as the communication speed for each state. The correlation between the communication speed and the social index in Figure 1 is 0.49 with p-value below 0.01.