Does Word of Mouth Communication Affect Trading?*

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This draft: November, 2013
First draft: August, 2013

* For helpful comments, we thank James Choi, Daniel Ferreira, and Daniel Paravisini. Special thanks goes to Dong Lou and Byoung-Hyoun Hwang. All remaining errors are our own. Huang is from the Department of Finance, London School of Economics and Political Science, Houghton Street, London WC2A 2AE. Email addresses: s.huang5@lse.ac.uk.

ABSTRACT

This paper explores the causal effect of word-of-mouth communication on investors' trading decisions. Our innovation is to use stock-financed acquisitions as a source of exogenous variation in households' portfolios. We find that in the year after a stock-backed acquisition, both target investors and their neighbours substantially increase their trading intensity in the acquirer industry (excluding the acquirer firm); yet no such change is observed after cash-financed acquisitions. Moreover, the spillover effect on neighbours is stronger when the neighbour and target investor belong to the same social group and are from a more sociable state. Finally, target investors and their neighbours do not earn superior returns from increased trading in the acquirer industry. Together, these results suggest a causal impact of social interactions on investment decisions.

JEL Classification: G11, G12, G14, G20.

Key words: Word of Mouth, Information Diffusion, Excessive Trading, Stock-Financed Acquisitions, Cash-Financed Acquisitions.

1. Introduction

The question of how information is transmitted in the marketplace is at the heart of asset pricing. One such channel that appears often in informal accounts of stock market behaviour is that investors spread information (or noise) to one another *directly* through word-of-mouth communication. For instance, as Ellison and Fudenberg (1995) note, "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 book *Irrational Exuberance*, argues that word-of-mouth transmission of ideas can be an important source of short-term fluctuations in the stock market.

Potentially consistent with the hypothesis that word-of-mouth communication affects investor behaviour, a number of recent studies document strong positive correlations in trading decisions among investors that are more likely to be in direct contact with one another. Hong, Kubik and Stein (2005), for instance, find that mutual fund managers' increase their purchases of a stock when other managers from different fund families in the same city increase their purchases of the same stock. Relatedly, Ivkovich and Weisbenner (2007) find that when retail investors purchase a stock from a certain industry, other retail investors in the neighbourhood increase purchases of stocks from that same industry.

Establishing a causal link from direct investor communication to trading decisions is challenging, however, and the aforementioned results are subject to alternative interpretations. For example, investors in the same local area may receive correlated signals from local media, local analysts, or firm announcements, and submit correlated trades accordingly. Alternatively, investors that live close to each other may have similar risk preferences or be affected by common economic shocks and in turn have similar hedging motifs. The key challenges to establishing a causal relation between one investor's portfolio choices and another investor's trading decisions are that a) we as econometricians do not observe communication among investors, and b) investors' holding and trading decisions are endogenously determined and can be affected by unobserved factors. To show that word-of-mouth communication has a causal impact on investors' trading behaviour, we thus need to identify exogenous shocks to some investors' portfolios.

Our innovation in this paper is to use cross-industry stock-financed acquisitions as a source of exogenous variation in investors' portfolios; in particular, we exploit the fact that investors in the target firm will, at the completion of a stock-backed acquisition, receive some shares of the acquiring firm from a different industry. Our empirical strategy rests on two simple premises. First, once endowed with some shares of the acquiring firm, target investors start to gather information about the acquirer industry, and spread that information to other investors residing in the nearby neighbourhood, as geographic proximity facilitates exchanges of ideas by word of mouth. Second, investors' decisions to hold target shares are not motivated by their desire to get hold of acquirer firm shares through these merger transactions, as investors can always purchase acquirer shares directly in the open market. In further analyses, we show that our main results are unchanged if we define target investors as those holding target firms one year before merger announcements, at which point retail investors are unlikely to anticipate future merger activities.

To test these predictions, we collect data on all cross-industry merger and acquisition deals from the CRSP database for the period of 1991-1996, which is then matched to detailed trading records of about 78,000 US households from a discount brokerage firm. This trading dataset is used by Barber and Odean (2000) and others. This M&A dataset includes all deals where the target firms have been delisted after they were acquired by other firms. We categorize these 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 acquisition, we then track the trading behaviour of target investors, as well as neighbours of target investors (within three miles), in the acquirer industry, excluding the acquirer firm itself to eliminate any mechanical effect. We further require that investors in our sample do not have any positions in the acquirer industry in the year prior to the merger announcement to alleviate concerns that part of the post-merger trading in the acquirer industry is due to hedging or rebalancing considerations.

The data strongly supports a causal impact of social interactions on investors' trading decisions. In the one year after the completion of a stock-financed acquisition, target investors increase their trading frequency in the acquirer industry (based on

the Fama-French 49 industry classification (Fama and French (1997)) and excluding the acquirer firm itself), as a fraction of total trading across all industries, by 2.30% compared to other investors in the sample. The benefit of this comparison is to control the market wide variables which may have impacts on all investors' trading decision, such as profitability of acquirer firm's industry. Similarly, neighbours of these target investors that live within a three-mile radius increase their trading frequency in the acquirer industry by 23bp in the same period. For comparison, the unconditional trading frequency in any industry is given by $1/49 = 2.04\%$. Consistent with a social interaction interpretation, the spillover effect in trading decisions diminishes quickly as we expand our neighbour definition or the time horizon: The effect becomes statistically insignificant for neighbours that reside more than 15 miles away from any target investor, or when we extend our analysis to years two and three after merger completion.

An alternative interpretation of our results is that acquisitions can have a direct impact on investor trading. For example, consider the case where both target investors and their neighbours are employees of the target firm. After the takeover, these investors become employees of the acquirer firm. Believing, perhaps erroneously, that they now have better information about the acquirer industry, target investors and their neighbours start to trade more frequently in the acquirer industry after merger completion. To address this alternative explanation, we repeat the same set of analyses around cash-financed acquisitions. If our results are indeed driven by mergers directly impacting investor beliefs and preferences, we expect a similar pattern in trading behaviour around cash-financed acquisitions. Alternatively, if our results are driven by stock ownership inducing target investors to gather information about the acquirer industry, who in turn transmit that information to other investors in nearby areas through casual communication, we expect the effect of cash-financed acquisitions on investors' trading behaviour to be insignificant. Consistent with the word-of-mouth hypothesis, we observe virtually no change in trading patterns, for both target investors and their neighbours, around cash-financed acquisitions.

We explore the mechanism of word-of-mouth effects in greater depth by exploiting variations in the likelihood of social interactions within neighbourhoods. We argue that the investor and his neighbour are more likely to be in direct contact with each other if they are from the same social group and/or are from a more

sociable state. Both predictions are borne out in the data. Specifically, we categorize investors into groups based on their age and income, as reported by the same brokerage database, drawing on the notion that investors with similar age or income are more likely to share common experiences, and thus interact with one another. While the increase in trading frequency for neighbours from the same social group as the target investors is both economically and statistically significant, that for neighbours from different social groups is indistinguishable from zero. We also find that the spillover effect is substantially stronger for residents in more sociable states, measured by seminar and club meeting attendance and community project participation.

Moreover, we identity a non-linear effect of population density on neighbours' trading behaviour. More specifically, neighbours are affected by shocks to target investors' portfolios only in populated areas (i.e., areas with valid MSA codes); however, within those zip codes with valid MSA codes, the larger the population, the weaker the spillover effect on neighbours' trading behaviour. This non-linear pattern is consistent with the idea that word-of-mouth effects only occur when there are sufficient people living in the same area; however, direct communication in a community tends to decrease as the area gets too crowded (consider, for example, residents in Manhattan that barely know their neighbours).

Given that both target investors and target neighbours significantly increase their trading activities in the acquirer industry, a natural question is whether these investors are trading on superior information or responding to noise. The answer to this question also has implications for whether social interactions among investors are price (de-)stabilizing. To examine this issue, we construct a buy and a sell portfolio across all target investors and their neighbours in the acquirer industry in the year following each acquisition. The results are consistent across various portfolio weighting schemes (e.g., weight by shares traded, value traded, or portfolio weight changes): the buy portfolio underperforms (statistically insignificant) the sell portfolio in both the month and year following portfolio formation, indicating that these investors are not trading on superior information. Put differently, our results suggest that retail investors, to a large extent, exchange noise rather than useful ideas through word-of-mouth communication.

The paper proceeds as follows. Section 2 lays out the background for our study. Section 3 presents our data collection procedures and summary statistics. Section 4 provides our main results on the causal impact of social interactions on investment decisions. Section 5 examines the mechanism in more detail, while Section 6 conducts more robustness checks. Section 7 concludes.

2. Background

The question of how investment ideas are spread among market participants has motivated a large body of literature. The primary transmission channel considered by prior studies is that between investors and firms (Cohen, Frazzini and Malloy (2008)), and investors and financial analysts (Cohen, Frazzini and Malloy (2010)), and banks and firms (Engelberg, Gao and Parsons (2010)).

But investors do not operate in isolation and likely communicate with each other. Their communication may have influence on the agents' actions. Two strands of literature examine implications of such presumed investor communication in stock market. The first strand of literature is the peer effects and stock market participation. Hong, Kubik and Stein (2004) propose a theory that investors' participation decision in the stock market is influenced by social interaction. They use Health and Retirement Study data to find that households, who interact more with their neighbours or attend church frequently, are more likely to participate the stock market. Brown, Ivkovic, Smith and Weisbenner (2008) finds there is positive relation between an individual's decision to own stocks and the average stock market participation rate of his or her community. Kaustia and Knupfer (2012) finds neighbour's recent stock returns will affect an individual's stock market entry decision. The second strand of literature is the peer effects and portfolio choice. For the peer effects and portfolio choice, Hong, Kubik and Stein (2005), for instance, find that mutual fund managers' increase their purchases of a stock when other managers from different fund families in the same city increase their purchases of the same stock. Relatedly, Ivkovich and Weisbenner (2007) find that when retail investors purchase a stock from a certain industry, other retail investors in the neighbourhood increase purchases of stocks from that same industry. Grinblatt and Keloharju (2001) find that households' stock trading will be significantly influenced by households' distance from public firms' headquarters, public firms' communication language and

culture origins. Furthermore, Duflo and Saez (2002, 2003) even find that employee's retirement plan choice will be influenced by their colleagues in the same department.

Our paper also shed some lights on investors' excess trading and its consequence. Odean (1998) proposes that investors will trade too much due to their overconfidence. Odean (1999) finds trades of retail investors can not cover the transaction costs and the securities they buy even underperform those they sell. In additional to test peer effects and stock market investment, this paper also documents that communication among peers generates excess trading and this excess trading also is hazardous to investors' trading profit.

3. Data

We mainly use two sources of data in this study. First, we obtain detailed holding and trading records for a subsample of US households for the period of 1991 to 1996 from a discount brokerage firm. The dataset covers investments made by these households in common stocks, mutual funds, and various other securities for the six-year period; we only focus on common stock investments in this paper. We use three files in this database. We extract information on investor trading, such as the quantity and price of each trade, from the transaction file. We also obtain their end-of-month holdings from the position file. Finally, we get various household/investor characteristics, such as age, income, and location, from the information base file. These three files can then be linked by a unique household ID and brokerage account number. Note that one household could have multiple accounts at the brokerage firm in our sample. For further details of this database, we refer the reader to Barber and Odean (2000).

We then match the trading records of these US households to all mergers and acquisitions that take place in the same six-year period, provided by the CRSP database. We require that the acquirer and target firms in each merger deal be from 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, does not change the main results of the paper. We also exclude all mergers and acquisitions where we cannot identify the

acquirer's or target's industry classification. We further categorize these merger and acquisition deals into stock-financed ones and cash-financed ones; the former are the ones that are at least partially financed by stock payments, while the latter are 100% financed by cash.

After applying these data selection and screening procedures, we end up with 460 mergers and acquisitions in the period of 1991 to 1996, out of which 317 are stock-financed and 143 are cash-financed transactions. Panel A of Table 1 reports the summary statistics of 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 acquisitions, the median acquirer market capitalization is $1561 million, and the median target market capitalization is $93 million, both of which are slightly larger than their stock-financed counterparts. Not surprisingly, the size distributions of both acquirer and target firms are heavily right skewed.

When matching household trading records to each merger and acquisition transaction, we require each investor in the sample to have at least one trade in any stock in the one year before and one year after the M&A transaction. We further require these investors to have no existing positions in the acquirer industry prior to the merger 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 full sample). Panel B of Table 1 provides summary characteristics associated with these accounts. The median and mean portfolio size is $13,141 and $41,030, respectively. On average, an investor holds 3.88 stocks in his portfolio and makes 0.47 trades each month, with the average value of trades in each month being $5,679. The distributions of these variables are all heavily right-skewed, suggesting that there are few wealthy, active investors in the sample that account for a considerable portion of all the holding and trading activities. Finally, the average investor age in our sample is 42 and the average annual household income is $69,500.

Finally, we augment our sample with geographic information from the US Census Bureaus' zip code database, which includes the population, household income,

the value of house, etc. for each zip code in the US. Given the home zip codes of any two investors in the brokerage database, we then compute the distance between the two investors using the longitude and latitude pair associated with each zip code, with curvature adjustments:

$$\begin{aligned}\text{distance(a, b)} = \arccos(&\cos(a_1)\cos(a_2)\cos(b_1)\cos(b_2) \\ &+ \cos(a_1)\sin(a_2)\cos(b_1)\sin(b_2) + \sin(a_1)\sin(b_1)) \times 3963,\end{aligned}$$

where a_1 and b_1 (a_2 and b_2) are the latitudes (longitudes) of the two zip codes and 3963 miles are the radius of the Earth.

In some further analyses, we classify US zip codes into two halves 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 in the period of 1975 to 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 for our purpose: 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.

4. Main Results

The main purpose of this paper is to explore the causal impact of social interactions on investors' trading behaviour. Our main innovation is to use stock-financed acquisitions as a source of exogenous variation in investors' portfolios. Specifically, we exploit the fact that investors in the target firm, at merger completion, receive some shares of the acquiring firm. Our empirical approach rests on two simple premises. First, investors' decisions to invest in the target firm, before the merger announcement, are not driven by their desire to invest in the acquirer firm through the merger, as investors who are interested in the acquiring firm can purchase acquirer shares directly in the secondary market, without having to worry about the deal falling apart. Second, upon owning some acquirer shares after deal completion, target investors start to gather information on the acquirer industry in order, for example, to find a good exit time, and further spread that information to other investors in the same community through word of mouth.

4.1. Target Investors

Our first set of analyses examines the latter assumption of our empirical approach 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 collection, we instead focus on their trading decisions, which ultimately are dictated by their information set. We exclude the acquirer firm itself 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 merger announcement to avoid trading in the acquirer industry in the post-merger period due to hedging or rebalancing concerns.

More specifically, we conduct the following linear regression:

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

where $Trading\_Freq_{i,m,Acq}$ is the trading by investor i in the acquirer industry (excluding the acquirer firm) as a fraction of his total trading across all industries after stock-financed merger m. Trading in each period is measured by both the number of trades and value of trades. Since the completion date is missing for many merger and acquisition 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 merger to complete. The main independent variable in the regression is $Target\_Investor_{i,m}$, which is a indicator variable that take the value of one if investor i holds shares in the target firm in the month before the merger announcement. In robustness checks, we define target investors based on their holdings one year before the merger announcement (at which point retail investors are unlikely to be able to forecast future merger 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 geographic 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 house value, number of household members, and household income. We also include a set of merger dummies in the regression to absorb any merger-specific effects. The standard errors are clustered at the time and zip code levels.

The regression results are reported in Panel A of Table 2. The dependent variable in the first four columns is trading frequency in the acquirer industry based on the number of trades, and that in the next four columns is based on the value of trades. As shown in column one, in a univariate setting, target investors increase their trading intensity in the acquirer industry, excluding the acquirer firm itself, by 2.48% more than other investors in months 6-18 after merger completion. For comparison, the unconditional trading frequency in any industry is given by $1/49 = 2.04\%$. 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 geographic characteristics and merger-fixed effects has virtually no impact on our results. For example, the coefficient on *Target_Invsetor* is 2.30% with a t-statistic of 4.89 in the full specification. Regression coefficients reported in columns 5-8, which are based on an alternative 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 after merger completion.

While these results are consistent with stock ownership inducing investors to collect information on related stocks and ultimately trading these stocks, there are alternative interpretations. In particular, our results can stem from the fact that mergers and acquisitions can directly impact investor decisions. Consider simple scenarios where these target investors are also affiliated with the target firm through other economic activities; for instance, these target investors may also be employees of the target firm, or work for suppliers or customers of the target firm. After the merger, these target investors become affiliated with the acquirer firm. Emboldened by such affiliation, target investors feel they now understand better the acquirer's business, and start trading more comfortably and frequently in other firms in the acquirer industry.

To address this alternative interpretation, we repeat the same set analyses around cash-financed acquisitions. If our results are truly driven by mergers and acquisitions directly impacting investor beliefs and preferences, we expect a similar change in trading intensity around cash-financed acquisitions. In contrast, if our results are due to stock ownership inducing investors to collect more information, we should observe no effect of cash-financed acquisitions on investor trading behavior. The results are reported in Panel B, where we focus on a subset of takeovers that are 100% financed by cash. The coefficients are only one fourth of those reported in Panel A in terms of economic magnitudes, and are far from having any statistical significance. Taken together, the results shown in this section confirms our notion that when endowed with some shares of a firm, investors start to gather information on the firm's underlying business and increase their trading in related firms in the same industry.

4.2. Target Neighbours

We next turn to the neighbors of target investors, who are the focus of this paper. Unlike prior studies on 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 in distance. We then conduct a similar regression analysis as in 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 himself. If an investor lives within three miles of more than one target investor, we only count him 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 any stocks in the acquirer industry before the merger announcement. Finally, we exclude target investors in the estimation of Equation (2) to ensure that the coefficients are not contaminated by the effect shown in Table 2.

Panel A of Table 3 reports target neighbours' trading behavior around stock-financed acquisitions. Similar to Table 2, the dependent variable in the first four columns of Panel A is trading intensity in the acquirer industry, excluding the acquirer firm, based on the number of trades, while that 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 one, in a univariate setting, neighbours who live within three miles of target investors increase their trading intensity in the acquirer industry by 39bp in months 6-18 after merger announcements, compared to other investors in the sample. Controlling for investor and geographic characteristics and merger-fixed effects only mildly reduces the coefficients. In the full specification, the coefficient on *Target_Neighbor* remains as high as 23bp with a t-statistic of 3.29. Put differently, target neighbours increase their trading intensity by over ten percentage points of the unconditional trading intensity in any given industry (1/49 = 2.03%). 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 on *Target_Neighbor* in the full specification (columns eight) is 22bp 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 a stock-financed acquisition 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 magnitudes is consistent with prior studies on word-of-mouth effects. 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, Ivkoiv and Weisbenner (2007) report that "a ten percentage point increase in neighbours' 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 acquisitions. If neighbours of target investors increase their trading in the acquirer industry because

the acquisition directly impacts neighbours' beliefs or preferences (through economic affiliations, for example), we expect to observe a similar pattern in trading around cash-financed acquisitions. In contrast, if neighbours of target investors increase their trading activities because of word-of-mouth communication with target investors, who only increase their trading in the acquirer industry after stock-backed acquisitions, we expect cash-financed acquisitions to have no impact on neighbours' trading decisions. The regression results, shown in Panel B of Table 3, are consistent with the latter explanation. The coefficient on *Target_Neighbor* in the full specification (columns 4 and 8) is almost zero, with a t-statistic below 0.3. Overall, the results shown in Table 3 support the hypothesis that social interactions can have a causal impact on investment decisions.

4.3. Alternative Specifications

If social interactions play a major role in generating the results in Tables 2 and 3, we expect the documented pattern to vary substantially with our definition of neighbours and with the time horizon during which we analyze the trades. Specifically, in Panel A of Table 4, we vary the distance over which we define neighbours. The dependent variable in columns (1), (3), (5), and (7) is trading intensity based on the number of trades, while that in columns (2), (4), (6), and (8) is trading intensity based on the value of trades. Columns (1) and (2) report the baseline regression results, where neighbours are identified within a radius of three miles. In columns (3) and (4), we increase the distance to 3 to 7 miles, and exclude both target investors and target neighbours within three miles from the sample. The coefficient on *Target_Neighbor* drops by about 20% to 18bp. In columns (5) and (6), we further increase the distance to 7 to 15 miles. The coefficient on *Target_Neighbor* drops by another 20% to 14bp. Finally, as shown in columns (7) and (8), stock-financed acquisitions have virtually no impact on neighbours that reside in between 15 and 30 miles of any target investor, and the results are similar for neighbours that are more than 30 miles away. This rapid decreasing pattern in coefficients is consistent with the idea that word-of-mouth effects decay quickly in distance.

We also vary the time period during which we measure investors' trading intensity. Specifically, instead of focusing on year one after merger completion (i.e., months 6-18 after merger announcements), we turn our focus to years two and three. Given the average holding period of one to two years of individual investors in our database, we predict that the effect of stock ownership in the acquirer firm on target investors, and in turn their neighbours, should decay in years two and three. Panel B of Table 4 reports regression results of trading behaviour in months 18-30 and months 30-42 after merger announcements. Similar to Panel A, the dependent variable in columns (1), (3), (5), and (7) is the trading intensity measure based on the number of trades, while that in columns (2), (4), (6), and (8) is the trading intensity measure based on the value of trades.

As shown in columns (1) to (4), target investors gradually reduce their trading intensity in the acquirer industry as compared to 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 merger announcements. This figure drops to 1.78% in months 18-30, and further to 1.23% in months 30-42. The drop in trading propensity for target neighbours is even more pronounced. There is no discernible difference in trading intensity between target neighbours and other investors beyond month 18: The coefficient on *Target_Neighbor* is 5bp and 1bp in months 18-30 and months 30-42, respectively, and both are statistically insignificant.

5. Mechanisms

In this section, we explore the mechanism of word-of-mouth effects in more detail. In particular, we exploit variations in the likelihood of direct communication between target investors and their neighbours. We argue that social interactions are more likely to take place if two investors are from the same social group (so that they are more likely to meet with each other in various social occasions), or are from areas where residents on average are more sociable (so that they are more likely to communicate with each other when they meet).

5.1. Social Groups

We start by examining the effect of social groups on word-of-mouth communication. In particular, we use investors' age and income, two of the most salient investor characteristics, to classify investors into different groups. Our basic premise is that investors that are born in nearby years have similar life experiences, and are thus more likely to interact with each other in various social meetings. By the same token, people with similar income tend to live in the same neighbourhood (within a zip code), to shop in the same stores, and to consume the same products, and are thus more likely to come into direct contact with one another.

More specifically, we categorize two investors to the same age group if their age difference is within ten years, in either direction. Similarly, we label two investors to be from the same income group if the difference in their income code, which ranges from one to nine as reported by the discount brokerage data, is smaller than two in either direction.[1] We then conduct separate regressions for target neighbours that are from the same social group as the target investors vs. those from different social groups. Moreover, if an investor is a neighbour of multiple target investors in an acquisition, we label him as from the same social group as long as he and one of these target investors are from the same age or income group.

The results of these separate regressions are reported in Table 5. Panel A examines the effect of investor age and life experiences on investment decisions. The main independent variable in the first two columns is an indicator variable that takes the value of one if the target neighbour is also from the same age group as the target investor, while that in the next two columns is an indicator variable that takes the value of one if the target neighbour is from a different age group. Similar to all prior tests, we use two definitions of trading intensity, one based on the number of trades (columns (1) and (3)), and the other based on the value of trades (columns (2) and (4)).

As can be seen from the first two columns, target neighbours that are also from the same age group as the target investors substantially increase their trading intensity in the acquirer industry in the post-merger period, as compared to other investors. The regression coefficient, based on the number of trades, of 40bps (t-statistic = 4.01) is almost twice as large as that in the full sample analysis. In contrast, as shown in columns (3) and (4), target neighbours that are from different

[1] The discount brokerage data categorizes household income into nine groups, corresponding to 0-15, 15-20, 20-30, 30-40, 40-50, 50-75, 75-100,100-125, and 125+ thousand dollars per annum, respectively.

age groups as target investors do not increase their trading intensity in the acquirer industry relative to other investors. The coefficient, based on the number of trades, is 11bp with a t-statistic of 1.38. The results are virtually identical if we focus on the value of trades.

5.2. Sociability

In our next set of analyses, we exploit variations in sociability (e.g., club membership, community project participation) across different states. To capture sociability, we use state-level values of the Social Capital Index, as collected by Putnam (2000). We rank all 50 states in the US into sociable and non-sociable ones, where the breakpoint is the sociability measure of the median state. As can be seen from the first two columns in Table 6, target neighbours that are in the more sociable states substantially increase their trading intensity in the acquirer industry in the post-merger period, as compared to other investors. The regression coefficient, based on the number of trades, of 30bps to 40 bps (t-statistics are above 3) is almost twice as large as that in the full sample analysis. In contrast, as shown in columns (3) and (4), target neighbours that are in less sociable states do no significantly increase their trading intensity in the acquirer industry relative to other investors. The coefficient, based on the number of trades, is -8 bps to 5bp with a t-statistics below 0.5. The results are virtually identical if we focus on the value of trades.

5.3. Population Density

While it is not a direct measure of sociability, population density in an area can play a vital role in social interaction among residents. On the one hand, if an area is too sparsely populated, each resident can only communicate with a handful of other residents in the nearby location; standard network theories predict that information diffusion, in such a setting, tends to be slow and ineffective. On the other hand, if an area is too densely populated, residents tend to adopt a closed-door policy and not to interact with their neighbours as much. In other words, as we increase the population in an area, while the number of nodes in the network increases, the likelihood of any two nodes having an interaction decreases. Consequently, there may exists a non-linear relation between population density and word-of-mouth communication.

To test this non-linearity, we first categorize all US zip codes into metropolitan areas (i.e., those with a valid Metropolitan Statistical Area (MSA) code) and other areas (i.e., those without a MSA code). Metropolitan Statistical Areas are defined by the Census Bureau as one or more counties that contain a city of 50,000 or more residents. Next, within the subset of zip codes with valid MSA codes, we sort them into two groups based on the population in each zip code. We choose the upper quartile in the zip code population distribution as our cutoff as it gives us similar numbers of observations in the less-populated and more-populated subsamples. Our results are qualitatively the same if we use the lower quartile or the sample median as our cutoff point.

Panel A of Table 7 reports the trading behaviour of target neighbours in metropolitan vs. other areas. The dependent variable in columns (1) and (3) is the trading intensity in the acquirer industry, excluding the acquirer firm, based on the number of trades in months 6-18 after merger announcements, while that in columns (2) and (4) is the trading intensity in the acquirer industry based on the value of trades. As can be seen from columns (1) and (2), neighbours of target investors in metropolitan areas substantially increase their trading in the acquirer industry in the post merger period, relative to other investors in the sample. The coefficient on the *Target_Neighbor* dummy of 21bp is highly statistically significant. In contrast, as shown in columns (3) and (4), neighbours of target investors in non-metropolitan areas do not exhibit an increase in their trading intensity in the acquirer industry; the coefficient of 7bp is far from being statistically significant.

In Panel B, we sort all zip codes from Metropolitan Statistical Areas into two groups based on the population in each zip code. As shown in columns (1) and (2), target neighbours from zip codes in the bottom three quartiles on the population distribution exhibit a substantial increase in their trading intensity in the acquirer industry, with a coefficient on the *Target_Neighbor* dummy of 21bp (t-statistic = 2.63). In contrast, neighbours from zip codes in the top quartile on the population distribution (column (3) and (4)) do not increase their trading in the acquirer industry in the post-merger period, with a coefficient estimate of 11bp (t-statistic = 1.08). Overall, the results presented in Table 7 support our prediction that population density has two opposing effects on word-of-mouth communication.

5.4. Returns to Increased Trading

The above results show that both target investors and target neighbours significantly increase their trading activities in the acquirer industry, it is natural to ask such a question as whether these investors trade on superior information or responding to noise. The answer to this question has implications on whether peer effects will generate excess trading has price destabilizing effect. Regarding to this issue, we construct a buy and a sell portfolio across all target investors and their neighbours in the acquirer industry in the year following each acquisition. Table 8 shows the returns to target investors/neighbours' trading. The results are consistent across various portfolio weighting schemes (e.g., weight by shares traded, value traded, or portfolio weight changes): the buy portfolio underperforms (statistically insignificant) the sell portfolio in both the month and year following portfolio formation. Taken as an example, the Panel A reports the return to buy-sell portfolio weighted by trading shares of target investors or neighbours. Four-factor alpha of this portfolio is -13 bps with T-statics -0.29 indicating that these investors are not trading on superior information. Put differently, our results suggest that retail investors, to a large extent, exchange noise rather than useful ideas through word-of-mouth communication.

6. Robustness

In this section, we will provide robust checks about our main results. In particular, we test alternative interpretations of our results. Firstly, we will test whether our results are caused by investors' anticipation of stock-financed M&A events and strategically choose to hold the target firms' stocks before the events. Secondly, we will provide placebo tests which randomly choose industries for target investors and check whether target neighbours also have significantly increased their trading in these industries around the M&A events which target investors or target neighbours are also target investors of other target firms and it is possible that these target investors or neighbours will mechanically to have positions of acquirer firms of latter target firms. Furthermore, we test whether our results are driven by possibility that where both target investors and their neighbours are employees of the target firms or acquire firms.

6.1. Lagged One Year Holdings

An alternative interpretation of our results is that there are good news about stock-financed M&As and investors hold the targets' firms in anticipation of M&A events. To address this concern, we define the target investors as those who have position of the target firms one year before the M&A events. This indicates that the main independent variable $Target\_Investor_{i,m}$ in the regression (1), which is a indicator variable that take the value of one if investor i holds shares in the target firm one year before the merger announcement. We repeat the analysis on the regression (1) with full specifications in Panel A of Table 9, while repeating the analysis on the regression (2) with full specifications in Panel B of Table 9. Although the coefficients in these regressions drop around 40%, they are also strongly significant in stock-financed M&A events and they are insignificant in cash-financed M&A events. As shown in column (1) of Panel A, target investors increase their trading intensity in the acquirer industry, excluding the acquirer firm itself, by 1.42% (with T-statics 4) more than other investors in months 6-18 after merger completion for stock-financed M&A events. For comparison, the unconditional trading frequency in any industry is given by 1/49 = 2.04%. In other words, ownership of acquirer stocks induces target investors to trade more than one and half than their normal trading activities in the acquirer industry. In contrast, column (3) of Panel A shows that targets investors do no significantly increase their trading intensity in the acquirer industry and only increase by 13 bps (with T-statics 0.33) for cash-financed M&A events. Furthermore, As shown in column (1) of Panel B, target neighbours also increase their trading intensity in the acquirer industry, excluding the acquirer firm itself, by 14 bps (with T-stats 2.33). For the cash-financed M&A events, there is no difference between target neighbours and other investors. The analysis in Table 9 confirms that our results could not be driven by investors' forecasting of future M&A events and their strategic behaviour.

6.2. Placebo Tests

Another concern about our study is that our result is due to the randomness. To address this concern, we carry out two bunches of placebo tests about column (4), column (8) in Table 2 and Table 3. In the first bunch of placebo tests, for each M&A events, we randomly choose one stock except the target firm in the target firm's industry and redefine the target investors as those who have position of this

randomly-choose stock at the end of one month before this M&A. We also redefine the target neighbours as before. The results from these placebo tests are reported in Panel A of Table 10. The results show that all coefficients are close to be zero and are with T-statics below 0.33.

In the second bunch of placebo tests, for each M&A event, we randomly choose one industry except the target firm's industry and acquirer firm's industry and calculate trading intensity of target investors/neighbours in this industry. The results from these placebo tests are reported in Panel B of Table 10. The results show that all coefficients are close to be zero and are with T-statics below 0.5. All of these results confirm that our results could not be generated randomly.

6.3. More Robustness Checks

We address two other concerns about the results in our study. The first concern is that our results are driven by mechanical effects in which target investors or target neighbours are also target investors of other target firms and it is possible that these target investors or neighbours will mechanically to have positions of acquirer firms of latter target firms. To address this concern, we exclude the sample in which the target investors or target neighbours are also the target investors of other target firms. After excluding the subsample, we repeat the analysis in Table 2 and Table 3 with full specification. The Panel A of Table 11 reports that the coefficients are almost the same in Table 2 and Table 3 after excluding the subsample. For example, column (1) of Panel A shows target investors increase their trading intensity in the acquirer industry, excluding the acquirer firm itself, by 2.30% (with T-statics above 4.5) more than other investors in months 6-18 after merger completion for stock-financed M&A events. Column (5) of Panel A shows that shows target investors increase their trading intensity in the acquirer industry, excluding the acquirer firm itself, by 22 bps (with T-statics above 3) more than other investors in months 6-18 after merger completion for stock-financed M&A events.

Furthermore, we test whether our results are driven by possibility that where both target investors and their neighbours are employees of the target firms or acquire firms. To address this concern, we exclude the sample where investors that

are within 100 miles of either target firms or acquire firms. After excluding the subsample, we repeat the analysis in Table 2 and Table 3 with full specification. The Panel B of Table 11 reports that the coefficients are almost the same in Table 2 and Table 3 after excluding the subsample. For example, column (1) of Panel A shows target investors increase their trading intensity in the acquirer industry, excluding the acquirer firm itself, by 2.38% (with T-statics above 4.5) more than other investors in months 6-18 after merger completion for stock-financed M&A events. Column (5) of Panel B shows that shows target investors increase their trading intensity in the acquirer industry, excluding the acquirer firm itself, by 17 bps (with T-statics 2.4) more than other investors in months 6-18 after merger completion for stock-financed M&A events.

7. Conclusion

Our paper provides new evidence for identifying the causal link between direct investor communication and their trading behavior. Using cross-industry stock-financed acquisitions as an exogenous source of variation to investors' portfolios, we find that in the year after a stock-financed acquisition, both target investors and their neighbours substantially increase their trading intensity in the acquirer industry (excluding the acquirer firm); yet no such change is observed after cash-financed acquisitions. Moreover, the spillover effect on neighbours is stronger when the neighbour and target investor belong to the same social group and are from a more sociable state. Meanwhile, we identity a non-linear effect of population density on neighbours' trading behaviour: neighbours are affected by shocks to target investors' portfolios only in populated areas (i.e., areas with valid MSA codes); however, within those zip codes with valid MSA codes, the larger the population, the weaker the spillover effect on neighbours' trading behaviour. This non-linear pattern is consistent with the idea that word-of-mouth effects only occur when there are sufficient people living in the same area; however, direct communication in a community tends to decrease as the area gets too crowded

Furthermore, target investors and their neighbours do not earn superior returns from increased trading in the acquirer industry. This evidence indicates that world-of-mouth communication will generate excess trading and transmit noise instead of information. This has some asset pricing implication of social interaction. Particularly, our results are not driven by other interpretation such as mechanical reason or investors' strategic behaviours. Together, these results suggest a causal impact of social interactions on investment decisions.

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Table 1. Summary Statistics

This table reports summary statistics of the sample analyzed in the paper for the period of 1991 to 1996. Panel A shows details of the M&A sample obtained from the SDC database. Stock-financed M&As are defined as acquisitions that are at least partially financed by stocks; cash-financed M&As are defined as acquisitions that are 100% financed by cash. Firm size is calculated as the number of shares outstanding multiplied by share price, in millions of dollars. Panel B shows investor and portfolio characteristics of the retail broker database used in Baber and Odean (2001). 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. Portfolio size is the total dollar value of all stock holdings in the portfolio. Number and value of trades are the total number and total dollar value of all buy and sell trades in each month. Panel C shows demographic information of each zip code from US Census 2000. The three sociability indices, Class or Seminar Attendance, Club Meeting Attendance, and Community Project Participation are measured at the state level each year for the period of 1991 to 1996.

Panel A: M&A Sample Characteristics						
	No. Obs.	25%	Median	75%	Mean	Std. Dev.
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-year 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 regressions of investors trading in the acquirer industry on the target investor dummy. The dependent variable in the first four columns 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 the next four columns 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. We skip 6 months because it takes on average 6 months for an acquisition to complete after the announcement. The main independent variable is a dummy variable that takes the value of one if the investor holds the target stock at the end of the month before the acquisition announcement. 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 of 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 based on stock-financed M&As, which are defined as acquisitions that are at least partially financed by stocks. Panel B reports 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 90%, 95%, and 99% 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
No. Observations	7,598,715	7,598,715	7,598,715	7,598,715	7,598,715	7,598,715	7,598,715	7,598,715
Adjusted R^2	0.00%	0.01%	1.65%	1.66%	0.00%	0.01%	1.59%	1.59%

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
No. Observations	3,489,281	3,489,281	3,489,281	3,489,281	3,489,281	3,489,281	3,489,281	3,489,281
Adjusted R^2	0.00%	0.01%	2.36%	2.37%	0.00%	0.01%	2.25%	2.26%

Table 3. Target Neighbors' Trading in the Acquirer Industry

This table reports regressions of investors trading in the acquirer industry on the target neighbor dummy. The dependent variable in the first four columns 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 the next four columns 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. We skip 6 months because it takes on average 6 months for an acquisition to complete after the announcement. 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 of 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 based on stock-financed M&As, which are defined as acquisitions that are at least partially financed by stocks. Panel B reports 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 90%, 95%, and 99% 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
No. Observations	7,596,415	7,596,415	7,596,415	7,596,415	7,596,415	7,596,415	7,596,415	7,596,415
Adjusted R^2	0.00%	0.01%	1.65%	1.66%	0.00%	0.01%	1.59%	1.59%

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
No. Observations	3,488,558	3,488,558	3,488,558	3,488,558	3,488,558	3,488,558	3,488,558	3,488,558
Adjusted R^2	0.00%	0.01%	2.36%	2.37%	0.00%	0.01%	2.25%	2.26%

Table 4. Different Definitions of Neighbors and Various Time Horizons

This table reports regressions of investors trading in the acquirer industry on the target investor and target neighbor dummies. The dependent variable in columns (1), (3), (5), and (7) 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, 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 neighbor 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 of 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 neighbors 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. Standard errors, shown in brackets, are clustered at zip code and time levels. *, **, *** denote significance at the 90%, 95%, and 99% level, respectively.

Panel A: Neighbors 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 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. Observations	7,596,415	7,596,415	7,558,105	7,558,105	7,485,049	7,485,049	7,336,619	7,336,619
Adjusted 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 Neighbors			
	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. Observations	5,814,983	5,814,983	3,696,168	3,696,168	5,812,950	5,812,950	3,694,682	3,694,682
Adjusted R^2	1.47%	1.39%	1.28%	1.21%	1.47%	1.39%	1.28%	1.21%

Table 5. The Effect of Social Groups

This table reports regressions of investors trading in the acquirer industry on the target neighbor 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 of 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 neighbors 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 neighbors that are in the same income group as the target investor 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 90%, 95%, and 99% level, respectively.

Panel A: Investor Age				
	Same Age Group		Different Age Groups	
	(1)	(2)	(3)	(4)
Target Neighbor	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. Observations	7,596,415	7,596,415	7,581,187	7,581,187
Adjusted 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 Neighbor	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. Observations	7,596,415	7,596,415	7,566,666	7,566,666
Adjusted R^2	1.66%	1.59%	1.66%	1.59%

Table 6. The Effect of Sociability Indices

This table reports regressions of investors trading in the acquirer industry on the target neighbor 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 of 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 Panels A, B, and C, we divide all states into two groups based on the average class or seminar attendance, the average seminar attendance, and the average community project participation, respectively. 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 90%, 95%, and 99% level, respectively.

Panel A: Seminar or Class Attendance				
	>= Median State		< Median State	
	(1)	(2)	(3)	(4)
Target Neighbor	0.0028***	0.0027***	-0.0008	-0.0008
	[0.0009]	[0.0009]	[0.0011]	[0.0011]
Investor Controls	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES
Event-Fixed Effects	YES	YES	YES	YES
No. Observations	3,334,639	3,334,639	1,718,047	1,718,047
Adjusted R^2	1.80%	1.73%	1.52%	1.46%

Panel B: Club Meeting Attendance				
	(1)	(2)	(3)	(4)
Target Neighbor	0.0039***	0.0039***	0.0003	0.0002
	[0.0011]	[0.0011]	[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. Observations	2,847,664	2,847,664	2,205,022	2,205,022
Adjusted R^2	1.72%	1.65%	1.67%	1.61%

Panel C: Community Project Participation				
	(1)	(2)	(3)	(4)
Target Neighbor	0.0030***	0.0030***	0.0005	0.0004
	[0.0009]	[0.0009]	[0.0010]	0.0010
Investor Controls	YES	YES	YES	YES
Zip Code Controls	YES	YES	YES	YES
Event-Fixed Effects	YES	YES	YES	YES
No. Observations	2,817,378	2,817,378	2,235,308	2,235,308
Adjusted R^2	1.76%	1.70%	1.62%	1.56%

Table 7. The Effect of Population Density

This table reports regressions of investors trading in the acquirer industry on the target neighbor 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 of 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 divide all zip codes into those with valid MSA codes and those without MSA codes. In Panel B, within all zip codes with MSA codes, we divide them into two groups based on the 75th percentile of the population distribution. 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 90%, 95%, and 99% level, respectively.

Panel A: Metropolitan vs. Other Areas				
	Metropolitan Areas		Other 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
No. Observations	3,020,577	3,020,577	2,105,810	2,105,810
Adjusted R^2	1.85%	1.77%	1.51%	1.45%

Panel B: Population Density within Metropolitan Areas				
	< Top Quartile		>= Top Quartile	
	(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
No. Observations	1,510,209	1,510,209	1,436,074	1,436,074
Adjusted R^2	1.73%	1.64%	1.99%	1.94%

Table 8. Returns to Target Investor/Neighbor Trading

This table reports monthly returns to the hedge portfolio that goes long in stocks bought by and goes short in stocks sold by target investors and target 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 in the previous 12 months, and are held for one month. In Panel B, the long and short portfolios are weighted by the dollar value traded by each investor in the previous 12 months, and are held for one month. Panel C and D use information from the holding file in the retail broker database. In Panel C, the long and short portfolios are weighted by the portfolio weight change of each investor in the previous month, and are held for one month. In Panel D, the long and short portfolios are weighted by the portfolio weight change of each investor in the previous month, and are held for 12 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 12 lags. *, **, *** denote significance at the 90%, 95%, and 99% level, respectively.

Panel A: (12, 1) Returns to Portfolios Weighted by Shares Traded				
	Excess Return	CAPM Alpha	Three-Factor Alpha	Four-Factor Alpha
Buy-Sell	-0.35%	-0.24%	-0.15%	-0.13%
	(-1.01)	(-0.53)	(-0.42)	(-0.29)
No. 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)
No. 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)
No. 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)
No. Months	61	61	61	61

Table 9. 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 neighbor 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 neighbor 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 of 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 90%, 95%, and 99% 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. Observations	6,943,336	6,943,336	3,220,313	3,220,313
Adjusted R^2	1.50%	1.44%	2.35%	2.24%

Panel B: Target Neighbors				
	(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
No. Observations	6,941,105	6,941,105	3,219,641	3,219,641
Adjusted R^2	1.50%	1.45%	2.35%	2.24%

Table 10. Placebo Tests

This table reports regressions of investors trading in the acquirer industry on the target investor and target neighbor dummies. The dependent variable in columns (1), (3), (5), and (7) 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), (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 neighbor 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 of 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 replace target investors with investors holding other stocks in the target industry. In Panel B, we replace the actual acquirer industry with a pseudo acquirer industry. 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 90%, 95%, and 99% level, respectively.

Panel A: Investors Holding Other Stocks in the Target Industry

	Target Investors				Target Neighbors			
	Stock M&As		Cash M&As		Stock M&As		Cash M&As	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Target	0.0006	-0.0006	-0.0009	-0.0003	-0.0003	-0.0003	0.0005	0.0004
	[0.0018]	[0.0019]	[0.0028]	[0.0030]	[0.0006]	[0.0006]	[0.0008]	[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
No. Observations	7,558,105	7,558,105	3,476,999	3,476,999	7,555,604	7,555,604	3,475,477	3,475,477
Adjusted R^2	1.66%	1.59%	2.36%	2.25%	1.66%	1.59%	2.36%	2.25%

Panel B: Target Investors'/Neighbors' Trading in the Pseudo Acquirer Industry

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Target	-0.0013	-0.0014	-0.0023	-0.0009	0.0005	0.0005	0.0007	0.0002
	[0.0025]	[0.0027]	[0.0032]	[0.0036]	[0.0007]	[0.0007]	[0.0013]	[0.0014]
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. Observations	7,598,715	7,598,715	3,489,281	3,489,281	7,596,415	7,596,415	3,488,558	3,488,558
Adjusted R^2	0.21%	0.20%	0.32%	0.31%	0.21%	0.20%	0.32%	0.31%

Table 11. Robust Checks

This table reports regressions of investors trading in the acquirer industry on the target investor and target neighbor dummies. The dependent variable in columns (1), (3), (5), and (7) 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), (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 neighbor 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 of 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. 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 90%, 95%, and 99% level, respectively.

Panel A: Exclude Investors Holding Other Target Stocks in the Sample Period

	Target Investors				Target Neighbors			
	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. Observations	7,576,448	7,576,448	3,479,807	3,479,807	7,574,164	7,574,164	3,479,091	3,479,091
Adjusted 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. Observations	7,497,533	7,497,533	3,449,347	3,449,347	7,495,339	7,495,339	3,448,646	3,448,646
Adjusted R^2	1.65%	1.59%	2.38%	2.27%	1.65%	1.59%	2.38%	2.27%