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The People in Your Neighborhood: Social Interactions and Mutual Fund Portfolios

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ABSTRACT

We find that socially connected fund managers have more similar holdings and trades. The overlap of funds whose managers reside in the same neighborhood is considerably higher than that of funds whose managers live in the same city but in different neighborhoods. These effects are larger when managers share a similar ethnic background, and are not explained by preferences. Valuable information is transmitted through these peer networks: a long-short strategy composed of stocks purchased minus sold by neighboring managers delivers positive risk-adjusted returns. Unlike prior empirical work, our tests disentangle the effects of social interactions from community effects.

DESPITE THE IMPORTANT ROLE professional money managers play in financial markets, and decades of academic study, relatively little is known about how they generate investment ideas. Research shows that managers invest in companies headquartered nearby (Coval and Moskowitz (1999, 2001)), and in companies to which they are linked through school networks (Cohen, Frazzini, and Malloy (2009)). They also choose stocks based on their political ideology (Hong and Kostovetsky (2012)) and stocks with which they are merely familiar (Pool, Stoffman, and Yonker (2012)).

But, as Aristotle famously noted, humans are social animals, so perhaps fund managers also trade stocks that they learn about from other managers. While numerous papers examine the effects of social interaction on choices in other domains,[1] there is little empirical evidence on how word-of-mouth communica-

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[1] For example, Grinblatt, Keloharju, and Ikäheimo (2008) document a substantial influence of near-neighbors on automobile purchases. Bayer, Ross, and Topa (2008) show the importance of

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tions influence professional investors' decision to trade a stock. Hong, Kubik, and Stein (2005) take an important first step in answering this question by studying a broad sample of mutual funds. They show that the holdings and trades of fund managers who work in the same city are correlated.[2]

Although these results are consistent with the hypothesis that professional money managers transmit investment ideas socially,[3] the authors point to several alternative hypotheses that are difficult to rule out with their data. Specifically, the correlation in portfolios could be due to fund managers in the same city being exposed to the same local media outlets, being visited by the same corporate executives during investor-relations road shows, or herding with local managers, which could be induced by geographic segmentation of the job market combined with career concerns (Scharfstein and Stein (1990) and Chevalier and Ellison (1999)). These alternative "community effects" would imply that news travels through formal information channels, whereas the social hypothesis implies that information travels through informal person-to-person relationships. Of course, both channels can operate simultaneously. In this paper we implement a test that allows us to disentangle the two effects.

If we could observe whether any two managers know and communicate with each other, constructing an empirical test would be straightforward. In the absence of such data, however, we rely on a unique identification strategy to uncover person-to-person relationships. We argue that managers who live near one another ("neighbors") have a better chance of meeting—and subsequently becoming acquaintances or friends—than managers who live further apart.[4] For example, managers might meet at a neighborhood park or school, or while taking the train to work. The longer they live near each other, the more such opportunities to become friends will arise. Further, having become friends, neighbors may have more ongoing interactions due to a higher probability of random encounters or shared social connections through local schools or places of worship. Importantly, we are not suggesting that these random encounters are the main method for the transmission of ideas, but rather that they increase the probability of planned interactions.

social interaction in labor markets, while Sacerdote (2001) finds strong peer effects on educational outcomes among randomly assigned college roommates. Bertrand, Luttmer, and Mullainathan (2000) find similar effects on welfare participation rates, as do Glaeser, Sacerdote, and Scheinkman (1996) on crime rates.

[2] Ivković and Weisbenner (2007) find similar results for individual investors who live within 50 miles of each other. Feng and Seasholes (2004) show correlated trading among proximate individual investors in China by exploiting brokerage rules that investors must trade at their branch office. In earlier survey research, Shiller and Pound (1989) find that both individual and institutional investors report that their portfolio choices are driven in part by interpersonal communication.

[3] Throughout the paper, we use the phrase "social interactions" in the traditional sense of the social sciences literature. That is, any relation between two people, regardless of where the interaction takes place.

[4] In this respect, our approach is similar in spirit to that of Cohen, Frazzini, and Malloy (2008), who argue that board members and executives who attended the same college at the same time are likelier to know each other.

To determine which managers are neighbors, we collect the complete residential address history of fund managers in our sample from public records data, and calculate the pairwise distance between the homes of all managers. We classify managers as neighbors only if they live truly close to each other—for example, just a fraction of a mile in densely populated areas in Manhattan or Boston. (The distance cutoff varies by population density, as we explain later.) Using our distance measure to proxy for social interaction creates variation within a city that is independent of sharing a media market, road shows, career concerns–induced herding, or any other community effects.

Thus, while previous papers document correlated trading among professional and individual investors using far coarser definitions of neighbors, we are able to identify the effects of social contact by zeroing in on fund managers who are likely to know each other, rather than treating all fund managers based in, say, New York City as neighbors. Prior studies rely on coarse definitions of neighbors for two reasons. First, they do not have residential addresses for the investors in their samples. Second, their empirical design tests whether the trades and holdings of investors are more sensitive to those of nearby investors than to those of a distant cohort. To perform such portfolio-based tests it is necessary to have a sufficient number of nearby investors to form the nearby cohort for each investor in the sample. As the distance between investors constituting "nearby" gets smaller, fewer and fewer investors meet this criterion, making such a test impossible to implement.

We circumvent this problem by designing an empirical test that does not require every investor in our sample to have a neighbor. We construct measures of pairwise overlap in holdings and trades for all funds, and test whether the overlap is greater when fund pairs are managed by neighbors. This design allows us not only to shrink the distance in the definition of neighbor, but also to control for other common community effects that are difficult to separate from the effects of social interactions in other empirical setups.

Remarkably, the portfolio overlap of funds managed by neighbors is 12% higher in our baseline model than that of funds whose managers live in the same city but are not neighbors—even after controlling for investment styles and fund family memberships. This increases to 28% when we implement a cleaner test by focusing on funds with just one manager. We find similarly strong results for trades.

These results are economically large. The increase in portfolio overlap that comes with being neighbors is 2.5 times that of funds whose managers are in the same 50-mile radius media market, and five times as large as the effect of being in the same city. Moreover, abnormal overlap for neighbor funds is about half as large as it is for two funds whose managers comanage another fund together, and it is about one quarter as large as the overlap between funds that belong to the same fund family, which shares analysts and other stock-selection resources. Despite the size of the estimates, however, our results are likely to *understate* the true magnitude of the effects of social contact for two reasons. First, our neighbor proxy is clearly a noisy measure of whether managers socially interact. If, for example, only half of the managers whom we classify as neighbors actually know each other, the true effect would be twice the size of our estimates. Second, managers and other investors have many social connections that we do not capture with our proxy, and any of these connections could generate trade ideas.

To increase the precision of our estimates, we collect information on manager characteristics to better identify social connections within neighborhoods. Commonality in these characteristics is likely to increase the probability that two neighboring managers know each other. For example, neighbors who also work for the same fund family represent a subset of our sample for which the likelihood of knowing one another is especially high. Similarly, common ethnic backgrounds could increase the likelihood of managers meeting at a cultural or religious event, or of their children attending the same school or youth organization. We find that our results are even stronger for neighbors who work together, those who have lived near one another longer, and those who share the same ethnic background.

While shrinking the distance between managers allows us to identify social interaction, it simultaneously introduces another challenge. Since a person's choice of where to reside is not random, a manager's investment decisions may coincide with those of her neighbors not because of personal contact, but rather because of similarities in preferences that drive both housing and portfolio choices. Indeed, the economics literature has long recognized the endogeneity problems present when studying social influence (Evans, Oates, and Schwab (1992), Manski (1993)).

Given the importance of distinguishing this "preferences" alternative from our hypothesis, we conduct a number of additional tests. First, we exploit changes in the residences of managers. If an individual's preferences are generally stable over time and similarities in portfolio choices are driven by preferences, then we should see correlated trading between managers with similar preferences even before they become neighbors. This is not what we find. Rather, we show that the holdings and trades of managers who are not neighbors—but become neighbors later in our sample—are *not* correlated.

Second, we add a number of control variables to our main regressions to capture similarities in preferences between managers. These controls include additional manager characteristics such as age, proxies for wealth, and the cultural makeup of their neighborhoods. We find that these control variables are associated with higher portfolio and trade overlap—confirming that they do capture some elements of preferences, as designed—but they have almost no effect on the magnitude of our initial findings.

Third, we address the preferences alternative by instrumenting our neighbor variable with indirect neighbors using "intransitive triads" (Bramoullé, Djebbari, and Fortin (2009)). In particular, we identify two managers who are not neighbors with each other, but each is a neighbor of a different comanager of another fund. Since these indirect neighbors are not neighbors, the endogeneity concern due to neighborhood selection is mitigated, but they remain informationally linked through their network. The coefficient estimates in this specification are similar to those in our baseline results.

Finally, we perform portfolio tests to evaluate the performance of neighboring managers' common holdings and trades. If abnormal overlap were driven by preferences, and not an exchange of information, the shared portfolio choices of neighbors would not generate significantly positive risk-adjusted returns, but we find that they do. A long-short strategy based on neighboring trades yields a statistically significant positive abnormal return of 6% to 7% per year, which is similar in magnitude to the estimates reported by Cohen, Frazzini, and Malloy (2008) of the abnormal returns generated by information shared between executives and fund managers.[5] Of course, this abnormal performance only arises in a portion of neighboring managers' portfolios, so the effect of socially induced trades on overall portfolio performance is much smaller.

Our performance results are valuable not only because they provide evidence against the preferences alternative, but also because they offer an important contribution to the literature on social influence. In particular, Hong, Kubik, and Stein (2005) argue that understanding the effect of word-of-mouth among professional money managers is interesting primarily because of its potential effect on asset prices. Notwithstanding, they leave open the question of whether fund managers share value-relevant information by socially interacting with one another or simply propagate noise or biases. By investigating performance, we provide the first evidence suggesting that information is transmitted through word-of-mouth by professional investors.

What are the sources of the information that we find is being shared? Though some systematic source may play a role—such as access to information on local stocks—our results are not driven solely by nearby securities. Each manager could also have a unique set of information sources, including proprietary analyses of public documents or discussions with experts. The idiosyncratic nature of such sources across managers would make it difficult for researchers to identify the origin of these ideas, but the likelihood of finding such ideas would probably be higher among securities whose prices are less informationally efficient. Consistent with this view, we show that both neighbor trades and performance are strongest in hard-to-research stocks such as those with low levels of advertising and low analyst coverage.

Finally, the social transmission of value-relevant information is particularly interesting in our setting given that it occurs among potential competitors. There are several possible explanations for this phenomenon. First, since we do not observe trading behavior within the quarter, it is possible that managers who have bought a stock subsequently share their information in an effort to have information impounded in prices more quickly, thereby allowing them to profit. Second, the cost of sharing information is not likely to be high; the effect on relative performance of sharing a few stock picks is probably small. This may be especially true among managers whose funds are not in the same style category as direct performance comparisons between these funds are less relevant.

[5] Cohen, Frazzini, and Malloy (2008) show that information shared in these networks generates abnormal returns of 7.8% per year. Our focus is on information transmission in networks of peers, rather than between managers and informed insiders. While it is certainly important to know if fund managers exploit their links to inside information, the question of whether valuable information is shared between professional investors has remained unanswered.

Third, managers may have an expectation of quid pro quo, whereby sharing information now could help them in the future. Stein (2008) discusses some of these possibilities and provides a model of information exchange consistent with what we document here.

The rest of the paper is organized as follows. In Section I we describe the data used in our tests. We begin the main empirical analysis in Section II by examining whether social interactions are related to fund holdings and trades. Next, we ask whether valuable information is being shared by neighbor managers in Section III. We provide a number of robustness tests in Section IV, and Section V concludes.

I. Data and Sample Construction

We combine several data sources in this study. We obtain information on fund managers from Morningstar, which reports the name of each manager for a fund (including individuals on team-managed funds), their start and end dates with the fund, and information about the manager's educational background. We limit the sample to actively managed U.S. equity funds by filtering Morningstar style categories and manually screening fund names.

We obtain mutual fund holdings from the Thomson Financial CDA/Spectrum Mutual Fund database, which contains the quarter-end holdings reported by U.S.-based mutual funds in mandatory SEC filings. Thomson uses two date variables, RDATE and FDATE, which refer to the actual date for which the holdings are valid and the Thomson vintage date on which the data were cut, respectively. We follow standard practice and restrict the holdings to those observations for which the FDATE is equal to the RDATE to avoid the use of stale data in our analysis. We drop observations when the stock price, CUSIP, or number of shares held are missing. From this starting point, there are 4,685,084 quarterly fund-holding observations from the first quarter of 1996 to the fourth quarter of 2010. The sample includes 2,558 funds and 4,622 managers, with an average of 810 funds per quarter.

To focus on funds for which we can properly control for investment style, we restrict the sample to those funds with a Morningstar category in the 3-by-3 size/value grid (US Large Blend, US Large Growth, US Large Value, US Mid-Cap Blend, US Mid-Cap Growth, US Mid-Cap Value, US Small Blend, US Small Growth, or US Small Value). We also remove funds with fewer than 20 holdings, or more than 500. Funds with more than 500 holdings could be index funds that were missed in our first set of screens. Adding these screens reduces the sample to an average of 688 funds per quarter.

A. Pairwise Distances

Our goal is to identify pairs of managers who have homes close to each other. We follow the method of Yonker (2015) and Pool, Stoffman, and Yonker (2012) to identify fund managers in the LexisNexis Public Records database using name, age, and location searches.[6] These data include a history of all addresses associated with each person, including all owned or rented properties. The data are extensive, drawing on public records from county tax assessor records, state motor vehicle registrations, reports from credit agencies, court filings, and post office records, among other sources. We are able to identify 2,042 managers in the database of the 4,622 managers in the initial sample. Our final sample includes an average of 412 funds per quarter, or just over 68,000 fund-pairs per quarter. The sample coverage is very similar to that of Pool, Stoffman, and Yonker (2012), who show that their sample is representative of the broader sample of actively managed mutual funds, covering over 80% of actively managed mutual funds by assets under management.

We identify the addresses of each manager at the beginning of each quarter. As a preliminary step, we calculate distances between each pair of managers using the latitude and longitude of the centroid of each home's zip code.[7] We refer to this distance as *zipdistance*. To get more precise travel distances for nearby pairs, we next determine the driving distance between any two managers where *zipdistance* $\leq$ five miles. This includes all pairs of managers who live in the same zip code, as *zipdistance* $= 0$ for these pairs. We identify 281,605 such pairs, and for each we use an online driving directions tool to record the precise travel distance between the two addresses.[8] For managers with more than one home, we record the minimum pairwise distance as the unique distance for the pair.

We make one further refinement to our distance measure. Two managers who live one mile apart in Manhattan are clearly further from each other in terms of "social distance" than two managers who live one mile apart in the considerably less populated town of New Canaan, CT, where several managers live. With more than 30,000 people per square mile in Manhattan, the probability of two people knowing each other is much lower. We therefore calculate a normalized distance measure that accounts for the population density of the area where managers live. For each zip code, we calculate the "household density" by dividing the number of households by the land area of the zip code.[9] The

[6] See Pool, Stoffman, and Yonker (2012) for a detailed explanation of the matching procedure.

[7] The distance in miles between two points with latitude/longitude pair (ϕ_i, λ_i) is calculated using the Vincenty formula for distances on ellipsoids,

$$\text{distance} = 3963.19 \times \arctan\left(\frac{\sqrt{(\cos\phi_2 \sin(\lambda_2 - \lambda_1))^2 + (\cos\phi_1 \sin\phi_2 - \sin\phi_1 \cos\phi_2 \cos(\lambda_2 - \lambda_1))^2}}{\sin\phi_1 \sin\phi_2 + \cos\phi_1 \cos\phi_2 \cos(\lambda_2 - \lambda_1)}\right).$$

The formula is known to work well for both short and long distances.

[8] We only calculate driving distance for potentially close pairs because the precision of the distance measure matters more for short distances: since we classify managers as "neighbors" or not, it matters much more if two managers are really within one mile of each other than if they are within 100 or 101 miles of each other. It is also not feasible to use online tools to determine driving distances for millions of pairs. To avoid problems with one-way streets causing artificially long distances when calculated as driving distances, we collect walking distances for the five biggest cities in our sample, and confirm that our results remain qualitatively unchanged.

[9] Figures for population and land area are obtained for zip code tabulation areas from the 2000 census.

normalized driving distance between managers i and j is then calculated as

$$NDD_{i,j} = \text{Driving distance}_{i,j} \times \frac{\max(hhdens_i, hhdens_j)}{\text{median}(hhdens)},$$

where $hhdens_i$ denotes the household density of manager i's zip code. The median density, calculated across all observations in the sample, is 878.6 households per square mile. The calculation implies that one normalized mile is just 62 feet on the Upper East Side of Manhattan, while in New Canaan it is 3.0 miles. It is approximately one mile in the Boston suburb of Wellesley, or in Colorado Springs.

B. Neighbors

Our main variable of interest will be an indicator variable for whether two fund managers are "neighbors." Therefore, we must specify some threshold distance below which we classify managers as neighbors. We do this by first creating another dummy variable, *DistBtwnHms1Mile*, that takes a value of one when managers live within one (nonnormalized) mile of each other. As shown in Table I, this condition is true in 0.3% of our observations.

We next choose a threshold for the normalized distance measure, *NDD*, to give it the same mean. That is, we choose a distance cutoff so that *NDD* is true in the same number of observations as *DistBtwnHms1Mile*, albeit for a different set of managers; more pairs will be classified as neighbors in less densely populated areas, while fewer pairs in big cities will meet the criterion. This threshold is 2.6 normalized miles, so we set the *Neighbors* dummy to take a value of one if two managers live within 2.6 normalized miles of each other. As we show in the next section, it turns out that the *Neighbors* dummy has considerably more power to detect portfolio correlations than does the *DistBtwnHms1Mile* variable.

While only 0.3% of our pairwise observations meet this condition, 37% of funds in a given quarter have at least one neighbor, so our results are not driven by just a few funds. Continuing the example from earlier, for managers who live on the Upper East Side to be considered neighbors, they must live in the same or adjacent building (within 161 feet), but managers in New Canaan can live up to 7.8 miles apart.

The neighbor cutoff is of course somewhat arbitrary, but robustness tests confirm that our main results are not sensitive to this particular distance. In general, requiring neighbors to live even closer causes our coefficient estimates to increase, but the standard errors increase as fewer people are included in the neighbor category.

Figure 1 shows how close neighbors in our sample live to each other. In Panel A we plot the cumulative distribution of driving distances between neighbors. We then calculate, for each neighbor pair, how many households are expected to live within the same distance of a manager as his neighbor. That is, we calculate the expected number of households in the circle whose radius is the distance

Table I
Summary Statistics

The table reports means of the overlap measures, measures of social interactions, and control variables for the full sample of observations, observations for which managers live within 2.6 normalized miles of one another (*Neighbors*), observations for which managers live within 50 miles of one another (*SameMediaMkt*), and observations for which managers live more than 50 miles apart. Variable names appear in italics. The sample includes 4.1 million quarterly fund-pair observations from the first quarter of 1996 through the fourth quarter of 2010. Manager-specific control variables are constructed at the manager-pair level. The sample includes 8.8 million manager pairs from the first quarter of 1996 through the fourth quarter of 2010. Means in column (3) are compared to (2), and in column (4) are compared to column (3). Significance levels for tests of difference in these means are denoted by a, b, c, which correspond to the 1%, 5%, and 10% levels, respectively. Standard errors are two-way clustered by each fund in the pair for these tests. * denotes unconditional means.

	(1)	(2)	(3)	(4)	
		Mean when			
SameMediaMkt =	*	0	1	1	
Neighbors =	*	0	0	1	N (000's)
Dependent variables					
% of overlapping holdings	8.02	7.91	8.86[a]	11.06[a]	4,090
% of overlapping purchases	8.48	8.34	9.52[a]	11.68[a]	3,370
% of overlapping sales	6.67	6.57	7.52[a]	8.99[a]	3,370
Independent variables of interest					
Driving dist. between homes $\leq$ 1 mile (*DistBtwnHms1Mile*)	0.003				4,090
Norm. driving dist. between homes $\leq$ 2.6 miles (*Neighbors*)	0.003				4,090
Baseline control variables					
Zip code distance between homes $\leq$ 50 miles (*SameMediaMkt*)	0.10				4,090
Funds are located in the same city (*SameMFCity*)	0.07	0.04	0.32[a]	0.41[a]	4,090
Funds are in the same family (*SameFundFam*)	0.01	0.01	0.05[a]	0.11[a]	4,090
Mgrs. mng. at least 1 other fund together (*MngOtherFundTogether*)	0.00	0.00	0.01[a]	0.04[a]	4,090
Funds have at least 1 mgr. in common (*CommonMgr*)	0.00	0.00	0.01[a]	0.05[a]	4,090
Funds are both small-cap (*BothSmallCap*)	0.07	0.06	0.07	0.06	4,090
Funds are both mid-cap (*BothMidCap*)	0.06	0.06	0.05[c]	0.04	4,090
Funds are both large-cap (*BothLargeCap*)	0.41	0.41	0.43	0.47	4,090
Funds are both value (*BothValue*)	0.05	0.05	0.06[b]	0.06	4,090

(*Continued*)

Table I—*Continued*

	(1)	(2)	(3) Mean when	(4)	
SameMediaMkt =	*	0	1	1	
Neighbors =	*	0	0	1	*N* (000's)
Funds are both growth (*BothGrowth*)	0.23	0.24	0.21[a]	0.26[b]	4,090
Funds are both blend (*BothBlend*)	0.09	0.09	0.10	0.08[b]	4,090
Difference in TNA-based quintiles between fund pairs (*TNAQuinDiff*)	1.59	1.59	1.56[b]	1.53	4,090
Average TNA-based quintile of fund pair (*TNAQuinAvg*)	3.02	3.01	3.13[a]	3.23[c]	4,090
Manager-specific control variables					
Number of years managers have been neighbors (*NeighborTenure*)	6.84				15
Both mgrs. reside in religious areas (*BothReligiousAreas*)	0.27	0.23	0.69[a]	0.72	8,846
Both mgrs. reside in Jewish areas (*BothJewishAreas*)	0.09	0.07	0.37[a]	0.28[a]	8,846
Both mgrs. reside in Catholic areas (*BothCatholicAreas*)	0.09	0.07	0.37[a]	0.53[a]	8,846
Both mgrs. reside in high home value areas (*BothHighHomeValueAreas*)	0.07	0.06	0.24[a]	0.48[a]	8,846
Both mgrs. reside in low home value areas (*BothLowHomeValueAreas*)	0.06	0.06	0.02[a]	0.03[b]	8,846
Ratio of the current value of mgrs' prim. residences (*HomePriceRatio*)	0.44	0.44	0.50[a]	0.58[a]	6,615
Ratio of the lot size of mgrs' prim. residences (*LotSizeRatio*)	0.37	0.37	0.37	0.49[a]	4,666
Ratio of the home age of mgrs' prim. residences (*HomeAgeRatio*)	0.37	0.37	0.39[a]	0.39	5,127
Difference in mgrs' ages $\leq$ 10 years (*SimilarAge*)	0.52	0.51	0.53[a]	0.58[a]	8,846
Mgrs. of the same minority ethnicity (*SameEthnicity*)	0.01	0.01	0.01[b]	0.02	8,846
Mgrs. graduated from the same college (*SameCollege*)	0.02	0.01	0.04[a]	0.10[a]	6,114
Both mgrs are experienced (*BothExp*)	0.27	0.27	0.23[a]	0.34[a]	7,090
Both mgrs. are inexperienced (*BothInexp*)	0.25	0.25	0.29[a]	0.23[a]	7,090

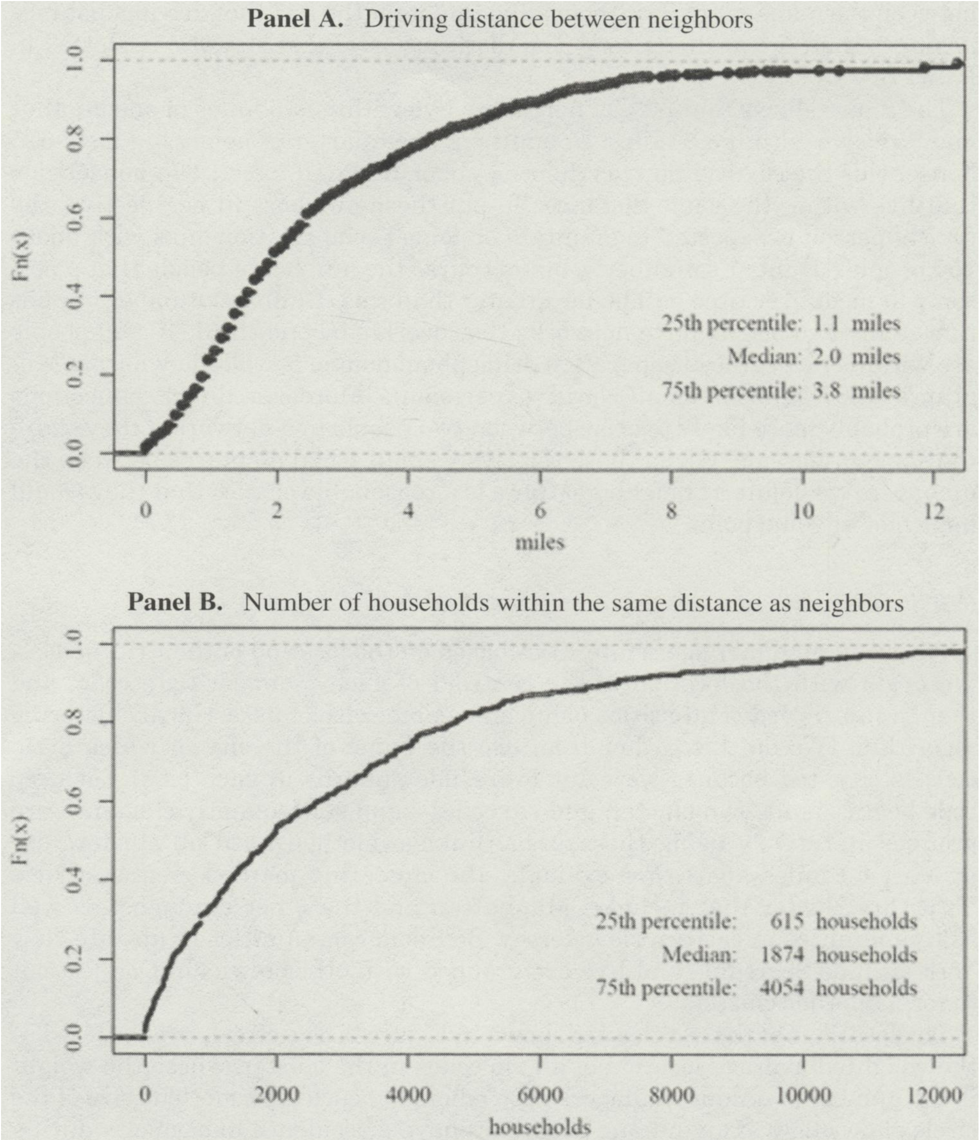


Figure 1. Cumulative distributions of neighbor proximity. The figure shows cumulative distributions of driving distances (Panel A) and number of households within the same linear distance (Panel B) for neighbor pairs. Managers in a pair are classified as neighbors if they live within 2.6 normalized miles of one another. The number of households within the same distance as manager pair (i, j) is calculated as $\pi \times \left(\frac{DD_{i,j}}{1.98}\right)^2 \times \max(HHD_i, HHD_j)$, where $DD_{i,j}$ is the driving distance between the residences of the managers, HHD_i is the household density in manger i's zip code, and 1.98 is the sample average of the ratio of the driving distance between homes to the linear distance between homes.

between the manager pair, with an adjustment for the fact that driving distance is longer than linear distance.[10] The distribution of this statistic is plotted in Panel B.

The figure shows that 50% of neighbors live within two miles of one another and 75% live within 3.8 miles. In addition, one-quarter of neighbors have 615 households that live as close as their neighbor, and half have 1,874 households that live within the same distance. To put these numbers in perspective, the typical person is expected to maintain ongoing social relationships with about 150 people ("Dunbar's number"), but of course the number of people that a person will meet over time will be far greater than this. Dunbar's number implies that if any two people have networks that overlap by one-third, 10,000 people are within one degree of separation. This phenomenon is well known from Milgram's (1967) famous "small-world" experiments. Moreover, direct connections are probably more likely to arise between two people in a network if they share the same profession. Given these relatively small social distances between the managers we define as neighbors, there is a reasonable chance that they would have met at some point.

C. City Clusters

We group fund managers into cities based on the location of their residences. We begin with the location of the centroid of each manager's zip code, and then group zip codes into cities using an agglomerative hierarchical clustering algorithm. (We do this rather than use the name of the city in which a zip code is situated because we want to include suburbs in each city.) Each zip code begins as its own cluster, and zip codes—and subsequently clusters—are grouped iteratively using the average linkage method until all clusters are at least 50 miles apart. For example, the clustering method creates a New York City cluster that includes Manhattan and the other boroughs, as well as communities in eastern New Jersey. Bedroom communities in upstate New York such as Scarsdale and Rye are grouped with other towns in Connecticut to form another cluster.

The locations of these cities are shown in Figure 2. The circles are centered at the weighted-average location of all zip codes in the cluster, where the weight is the number of unique managers with a home in each zip code. The size of the circle shows how many unique managers have a residence in each city during our sample period, and the proportion of these managers who are part of a neighbor pair at some point during the period is shown as a shaded pie slice. It is clear from the picture that managers are widely disbursed, and that, while pairs are more likely to be found in larger cities, they are also quite disbursed. Cities with only one fund manager do not have any neighbor pairs, but we

[10] In our sample, the ratio of driving distance to linear distance for any two neighbors is 1.98, so the adjusted distance is $\pi \times \left(\frac{DD_{i,j}}{1.98}\right)^2 \times \max(HHD_i, HHD_j)$, where $DD_{i,j}$ is the driving distance between the residences of the managers, and HHD_i is household density in manger i's zip code. Note that we are being conservative by taking the maximum of the household densities.

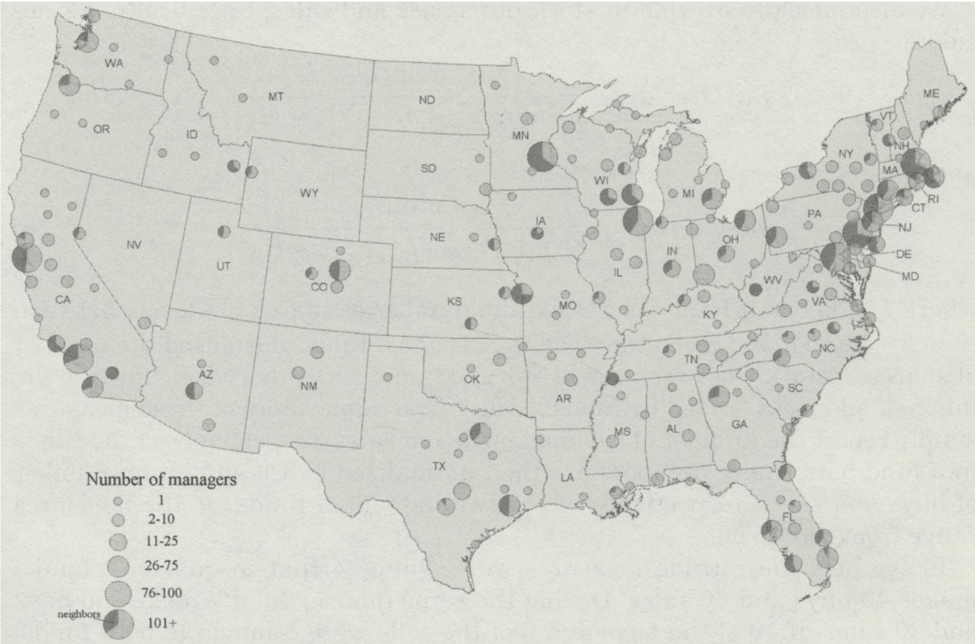


Figure 2. City clusters and neighbor pairs. The figure shows the geographic dispersion of fund managers. We use a clustering algorithm to group nearby zip codes into cities, as explained in the text. The algorithm stops joining clusters that are more than 50 miles apart. The size of the plotted circle is determined by the number of unique managers who have a residence in the city at some point during our sample period, while the shaded pie slice indicates the proportion of managers who are classified as neighbors with at least one other manager at some point during the sample period. We classify managers as neighbors if they live within 2.6 normalized miles of each other, where the normalization adjusts for population density (see text for details).

include these managers in our sample to provide information about typical holdings and trades.

D. Measures of Overlap

We measure the portfolio overlap in holdings between funds i and j during quarter t as

$$PortOverlap_{i,j,t} = \sum_{k \in \mathcal{H}_t} \min\{w_{i,k,t}, w_{j,k,t}\}, \tag{1}$$

where $w_{i,k,t}$ is fund i's portfolio weight in stock k during quarter t, and $\mathcal{H}_t$ is the set of all stocks held by funds i and j as reported at the end of quarter t. We aggregate the overlap measure to the fund level since conducting the analysis at the stock level would lead to billions of observations.

We also measure overlap in stock purchases and sales. Specifically, we calculate

$$BuyOverlap_{i,j,t} = \frac{\sum_{k\in\mathcal{T}_t} \min\{I^+_{i,k,t}, I^+_{j,k,t}\}}{\min\{\sum_{k\in\mathcal{T}_t} I^+_{i,k,t}, \sum_{k\in\mathcal{T}_t} I^+_{j,k,t}\}} \tag{2}$$

and

$$SaleOverlap_{i,j,t} = \frac{\sum_{k\in\mathcal{T}_t} \min\{I^-_{i,k,t}, I^-_{j,k,t}\}}{\min\{\sum_{k\in\mathcal{T}_t} I^-_{i,k,t}, \sum_{k\in\mathcal{T}_t} I^-_{j,k,t}\}}, \tag{3}$$

where $I^+_{i,k,t}$ is one if fund i increased the number of shares in stock k between time $t-1$ and t, and zero otherwise, $I^-_{i,k,t}$ is one if fund i decreased the number of shares in stock k between time $t-1$ and t, and zero otherwise, and $\mathcal{T}_t$ is the union of all stocks traded by funds i and j. The numerators of these measures simply report the number of common buys and sales, respectively, of the funds in a fund pair. These numbers are then normalized by the minimum number of buys and sales, respectively, of the two individual funds, so the measures range from zero to one.

To see how these trade measures work, suppose that in quarter t fund i makes 40 buys and 60 sales. During the same quarter fund j makes 30 buys and 20 sales. If 10 of the buys and 5 of the sells were common to both funds, then $BuyOverlap_{i,j,t} = 10/30 = 33\%$ and $SaleOverlap_{i,j,t} = 5/20 = 25\%$.

Since we conduct a pairwise analysis, our regressions in the next section use observations of $N-1$ overlaps for each fund in each quarter, so there is clearly a lack of independence. In each quarter, a fund's portfolio contributes to multiple observations, and between quarters, portfolios are persistent. While this correlation will not bias the coefficient estimates, it will lead standard errors to be underestimated. We therefore calculate standard errors that are clustered in two dimensions—one for each fund in the pair—so repeated observations of a fund can be correlated.

The average number of funds in the sample each quarter is 412 and our analysis includes 60 quarters, so we should have over 5.1 million quarterly fund-pair overlap observations. However, since mutual funds are constrained by their investment mandates, it is difficult for funds in different size categories to engage in overlapping trades. Specifically, large-cap funds are constrained from trading in small-cap stocks and vice versa. For this reason we exclude pairs where one fund is a large-cap fund and the other is a small-cap fund. This reduces the sample to approximately 4.1 million quarterly observations. Notably for the standard errors, however, the number of funds used in the clustering calculation is 1,677.

II. Do Social Interactions Influence Fund Holdings and Trades?

A. Summary Statistics

Table I reports the means of the variables used in our analysis. We separately report means for (1) the full sample of fund pairs, (2) funds whose managers live at least 50 miles apart, (3) funds whose managers live within 50 miles ("same media market") but are not neighbors, and (4) funds whose managers are neighbors ("neighbor funds"). The average portfolio overlap between any two funds in the sample is 8.02%. The overlap in holdings, purchases, and sales of neighbor funds are unconditionally higher than that of fund pairs with managers who live in the same media market. The average overlap in holdings of neighbor funds is 11.06%, while it is 8.86% for funds with nonneighbor managers in the same media market. This 2.2% difference in overlap is significant at better than the 1% level, and is the first hint of our main result—that neighbor funds have more overlap than nonneighbors located in the same media market.

Across all fund pairs, 0.3% have at least one manager pair that are neighbors, which appears to be a small number of neighbors, but about 37% of funds in each quarter have at least one manager who is a neighbor of a manager at another fund, so the results that follow are not driven by just a few funds. The table also shows that 10% of fund pairs have at least one pair of managers who live in the same media market, 7% of fund pairs are located in the same city, and about 1% of fund pairs are from the same family.

Table I reports that neighbor funds are significantly more likely to be headquartered in the same city, be from the same fund family, share common managers, or have managers that manage at least one other fund together. Neighbors are more similar along a number of dimensions than are managers who live in the same media market but are not neighbors. For example, managers who are neighbors have more similar home values and lot sizes, and are closer in age. These managers are also more likely to have substantial portfolio management experience and to have graduated from the same college. This evidence highlights the importance of examining commonality in preferences as an alternative hypothesis to social interactions as an explanation for the common patterns in fund holdings and trades.

B. Social Interactions and Holdings

We begin our main analysis by estimating the regression

$$\begin{aligned} PortOverlap_{i,j,t} = \alpha \; & + \beta Neighbors_{i,j,t} + \delta SameMFCity_{i,j,t} \\ & + \gamma SameMediaMkt_{i,j,t} + \Gamma' \mathbf{Controls}_{i,j,t} + \epsilon_{i,j,t}, \qquad (4) \end{aligned}$$

where $PortOverlap$ is defined in (1), $Neighbors_{i,j,t}$ is a dummy variable that is one if at least one manager from fund i is a neighbor of a manager from fund j during quarter t (as defined in Section I.B), $SameMFCity_{i,j,t}$ is a dummy variable that is one if funds i and j are headquartered in the same city (defined as in Section I.C but using mutual fund company addresses), $SameMediaMkt_{i,j,t}$ is a dummy variable that is one if at least one pair of managers from the management teams of funds i and j live within 50 miles of one another, and $\mathbf{Controls}_{i,s,t}$ is a vector of relevant control variables.

As controls, we include a set of dummy variables that are one if funds i and j match on (i) Morningstar size or value/growth categories (*BothSmallCap*, *BothMidCap*, *BothLargeCap*, *BothValue*, *BothGrowth*, *BothBlend*); (ii) the absolute value of the difference between the total net asset (TNA)-based quintiles of funds i and j (*TNAQuinDiff*); or (iii) the average TNA-based quintiles of funds i and j (*TNAQuinAvg*). We also include dummy variables that indicate if funds i and j are from the same mutual fund family (*SameFundFam*), at least one pair of managers from funds i and j manage at least one other fund together (*MngOtherFundTogether*), or if funds i and j have at least one manager in common (*CommonMgr*). In most of the subsequent analyses we exclude observations in which funds have a common manager, but it is interesting to include these observations in our initial regressions so we can compare the magnitude of the effect of being neighbors to that of working for interlocking funds.

If social interactions influence mutual fund managers' portfolio choices, then β in equation (4) should be positive. The estimate of β is the incremental increase in portfolio overlap from managers being neighbors beyond the influence of local media and what Hong, Kubik, and Stein (2005) call "local investor relations." The effect of managers being exposed to the same local media is captured by *SameMediaMkt*, and any influence of the relative proximity of the mutual funds themselves, such as local investor relations, is captured by *SameMFCity*.

Table II displays the coefficient estimates and standard errors for various forms of equation (4). Standard errors are two-way clustered at the fund level for each fund in the pair. In column 1 we include only *SameMFCity* and the control variables. This regression gives us an estimate of the magnitude of the Hong, Kubik, and Stein (2005) results in our empirical framework. The coefficient on *SameMFCity* is 36 basis points (bps) and is significantly greater than zero, confirming that the results of Hong, Kubik, and Stein (2005) hold in our setting.

The estimates on the control variables are also of interest. Not surprisingly, matching on fund size and value/growth categories is extremely important for explaining the commonality in holdings of two mutual funds, with large-cap fund pairs having the greatest portfolio overlap. Among the other fund linkages included in the controls, having a manager in common has the largest effect. The portfolio overlap increases by 9.16% in this case, which, given the average overlap of 8.02%, implies that the portfolio overlap is over 110% higher if a fund pair has a manager in common. When two funds have managers who manage a third fund together, the portfolio overlap is also higher: the estimate on *MngOtherFundTogether* is 179 bps, implying that the overlap between these funds is about 22% greater. Finally, funds that belong to the same fund family also have higher overlap. This abnormal commonality in holdings is 364 bps and may be due to shared analysts and other stock-selection resources.

In column 2 we add a measure that captures whether the fund managers are neighbors, but based on raw driving distance rather than our population density–adjusted measure. For a given fund pair, *DistBtwnHms1Mile* is a dummy variable that takes the value of one if at least one of the managers of

Table II
Social Interactions and Mutual Fund Holdings

The table reports the coefficient estimates and standard errors from the OLS estimation of various forms of the regression equation

$$Overlap_{i,j,t} = \alpha + \beta Neighbors_{i,j,t} + \delta SameMFCity_{i,j,t} + \gamma SameMediaMkt_{i,j,t} + \Gamma' \mathbf{Controls}_{i,j,t} + \epsilon_{i,j,t},$$

where $Overlap_{i,j,t}$ is the overlap in holdings of fund i and fund j during quarter t and is defined as in equation (1). The variable of interest is $Neighbors_{i,j,t}$, a dummy variable that is one if at least one manager from fund i lives within 2.6 normalized miles of at least one manager from fund j during quarter t. $SameMFCity_{i,j,t}$ is a dummy variable that is one if funds i and j are headquartered in the same city, $SameMediaMkt_{i,j,t}$ is a dummy variable that is one if at least one pair of managers for a given fund pair live within 50 miles of one another, and $\mathbf{Controls}_{i,s,t}$ is a vector of relevant control variables. In column 2 the variable of interest is a dummy variable that is one if the raw driving distance between the residences of at least one pair of managers for a given fund pair is one mile or less (*DistBtwnHms1mile*). Controls include a dummy variable that is one if funds i and j are headquartered in the same city (*SameMFCity*), a dummy variable that is one if at least one pair of managers for a given fund pair live within 50 miles of one another (*SameMediaMkt*), dummy variables that are one if funds i and j match on Morningstar size or value/growth categories (*BothSmallCap*, *BothMidCap*, *BothLargeCap*, *BothValue*, *BothGrowth*, *BothBlend*), an indicator variable that is one if funds i and j are from the same mutual fund family (*SameFundFam*), an indicator variable that is one if at least one pair of managers from funds i and j manage at least one other fund together (*MngOtherFundTogether*), an indicator variable that is one if funds i and j have at least one manager in common (*CommonMgr*), the absolute value of the difference between the total net asset (TNA)-based quintiles of the funds in the pair (*TNAQuinDiff*), and the average TNA quintiles of the funds in the pair (*TNAQuinAvg*), where TNA quintiles are computed quarterly. The sample includes 4.1 million quarterly fund-pair observations from the first quarter of 1996 to the fourth quarter of 2010. In column 5, the sample is limited to fund pairs with no managers in common during quarter t. Column 6 excludes fund pairs within the same fund family. In column 7 the results are presented for the sample of funds where both funds in the pair are managed by a single manager. Standard errors, two-way clustered by each fund in the pair, are in parentheses. Significance levels are denoted by a, b, c, which correspond to the 1%, 5%, and 10% levels, respectively.

Sample:	Full (1)	Full (2)	Full (3)	Full (4)	No Mgrs. Common (5)	Different Families (6)	Single Mgr. Pairs (7)
Neighbors			1.29[a]	0.99[a]	0.99[a]	0.88[a]	2.24[b]
			(0.31)	(0.30)	(0.29)	(0.28)	(0.95)
DistBtwnHms1Mile		0.84[b]					
		(0.36)					
SameMFCity	0.36[b]	0.35[b]	0.34[b]	0.20	0.19	0.20	0.11
	(0.16)	(0.16)	(0.16)	(0.15)	(0.15)	(0.15)	(0.34)
SameMediaMkt				0.39[a]	0.39[a]	0.33[b]	0.13
				(0.14)	(0.14)	(0.14)	(0.28)
SameFundFam	3.64[a]	3.64[a]	3.63[a]	3.63[a]	3.76[a]		6.16[a]
	(0.38)	(0.38)	(0.37)	(0.37)	(0.38)		(0.82)
MngOtherFundTogether	1.79[a]	1.78[a]	1.79[a]	1.77[a]	1.55[a]		0.88
	(0.46)	(0.46)	(0.46)	(0.46)	(0.46)		(1.14)

(*Continued*)

Table II—*Continued*

Sample:	Full (1)	Full (2)	Full (3)	Full (4)	No Mgrs. Common (5)	Different Families (6)	Single Mgr. Pairs (7)
CommonMgr	9.16[a]	9.12[a]	9.07[a]	8.99[a]			
	(0.79)	(0.79)	(0.79)	(0.79)			
BothSmallCap	1.21[a]	1.21[a]	1.21[a]	1.21[a]	1.19[a]	1.18[a]	1.15[a]
	(0.16)	(0.16)	(0.16)	(0.16)	(0.16)	(0.16)	(0.28)
BothMidCap	1.78[a]	1.78[a]	1.78[a]	1.79[a]	1.77[a]	1.76[a]	1.21[a]
	(0.20)	(0.20)	(0.20)	(0.20)	(0.20)	(0.20)	(0.30)
BothLargeCap	12.06[a]	12.06[a]	12.06[a]	12.06[a]	12.05[a]	12.02[a]	13.04[a]
	(0.36)	(0.36)	(0.36)	(0.36)	(0.36)	(0.36)	(0.64)
BothValue	2.84[a]	2.84[a]	2.84[a]	2.84[a]	2.84[a]	2.85[a]	2.17[b]
	(0.42)	(0.42)	(0.42)	(0.42)	(0.42)	(0.42)	(0.88)
BothGrowth	3.44[a]	3.44[a]	3.44[a]	3.44[a]	3.43[a]	3.43[a]	3.31[a]
	(0.24)	(0.24)	(0.24)	(0.24)	(0.24)	(0.24)	(0.40)
BothBlend	2.35[a]	2.35[a]	2.35[a]	2.35[a]	2.34[a]	2.34[a]	1.76[a]
	(0.33)	(0.33)	(0.33)	(0.33)	(0.33)	(0.33)	(0.45)
TNAQuinDiff	−0.18[a]	−0.18[a]	−0.18[a]	−0.18[a]	−0.18[a]	−0.17[a]	−0.25[a]
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.06)
TNAQuinAvg	0.66[a]	0.66[a]	0.66[a]	0.66[a]	0.66[a]	0.65[a]	0.73[a]
	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.19)
Constant	−0.11	−0.11	−0.11	−0.13	−0.12	−0.08	−0.19
	(0.30)	(0.30)	(0.30)	(0.30)	(0.30)	(0.30)	(0.56)
$Adj\,R^2$	0.41	0.41	0.41	0.41	0.41	0.41	0.42
N (thousands)	4,090	4,090	4,090	4,090	4,084	4,051	472

a fund lives within a one-mile driving distance from one of the managers of the other fund, and is zero otherwise. The estimate on *DistBtwnHms1Mile* is 84 bps and statistically significant. This indicates that, when we compare two fund pairs that are located in the same city—one with managers who live within a mile of one another and the other with managers who do not—the fund pair whose managers also share a neighborhood will have 84 bps higher overlap. Since the magnitude of *SameMFCity* is 35 bps in this specification, if a pair of funds have managers who live within a mile of one another and the funds are located in the same city, then their portfolio overlap will be $35 + 84 = 119$ bps greater than that of fund pairs from different cities.

As discussed earlier, one problem with using raw driving distance to measure social interactions is that distance alone does not capture the probability that two individuals actually know one another. As we describe in Section I, we therefore scale driving distances between managers to adjust for the household density of the managers' residential areas. This increases the "social distance" in more densely populated areas and shrinks it in less densely populated regions. We then choose a cutoff for our variable of interest, *Neighbors*, so that it is true for the same number of managers as those that live one mile apart.

In column 3 we replace *DistBtwnHms1Mile* with our *Neighbors* dummy. It is apparent that we are choosing neighbors much more appropriately using the normalized driving distance measure. The coefficient estimate on *Neighbors* is 129 bps, 1.5 times that on *DistBtwnHms1Mile*, and the standard error of this estimate is lower than that of the raw distance measure. The fact that this adjustment matters is our first indication that social interactions, and not similarity in preferences, drive the result of higher overlap between neighbors. If living close together just captured preferences, it should not matter if two managers live nearby in an area that has high or low population density. The density adjustment would just affect the probability of having a chance to meet.

In column 4 we add *SameMediaMkt* to control for managers living within 50 miles of one another. Even if funds are located in different cities, it is possible that managers live close to each other. They could vacation in the same area or they may manage their funds remotely, spending most of their time away from the fund headquarters. *SameMediaMkt* takes these possibilities into account and should also capture broad local effects, such as managers being exposed to common media sources. The estimate on *SameMediaMkt* is 39 bps and is statistically significant. Its inclusion in the regression reduces the magnitudes of the coefficients on both *SameMFCity* and *Neighbors*, but the coefficient on *Neighbors* remains statistically positive at 99 bps while the coefficient on *SameMFCity* is no longer statistically different from zero.

Next, we come to our "baseline" regression in column 5, where we exclude fund pairs with common managers. In this specification the estimates on *Neighbors*, *SameMFCity*, and *SameMediaMkt* are 99 bps, 19 bps, and 39 bps, respectively. This implies that two funds that are located in the same city with at least one pair of managers who are neighbors will have 157 bps greater overlap than the typical fund pair. This is equivalent to over $157/802 = 20\%$ greater commonality in holdings. The coefficient estimates decompose this effect into social interactions and broad local effects, the former representing about two-thirds of the total magnitude.

To provide perspective on the influence of having fund managers who are neighbors, we compare the coefficient estimates on *Neighbors* to those on some of the other control variables. For instance, the effect on fund holdings from social interactions is about two-thirds of the effect of two funds with managers who manage another fund together, and more than 10% of the effect of having a manager in common. Of course, we cannot be sure that all neighbors in our sample actually know one another, so our estimates are likely to understate the true magnitude of the effect. If only half the managers who are identified as neighbors actually know one another, and we were instead able to estimate the regression with only these manager pairs, then we would expect the coefficient estimate to be twice as large, which would be quite similar to the overlap in funds whose managers manage another fund together. We return to this issue in Section II.D.

The regression in column 6 excludes all fund pairs within the same fund family. Not surprisingly, the coefficient estimates on *Neighbors* and the two local effects measures decrease, but they remain significantly positively estimated and economically relevant. The results confirm that, even across families, fund managers share information about their holdings.

Finally, in column 7 we estimate equation (4) for the sample of fund pairs that only include funds managed by just one manager ("single"). Since both funds in these fund pairs have managers who are sole decision makers, we would expect that, when these managers are neighbors, social interactions will be more important in determining portfolio composition. We find that this is the case. The coefficient estimate on *Neighbors* is 224 bps, statistically significant at better than the 5% level. The estimate suggests that the effect of social contact on portfolio overlap is remarkably large; it leads to 28% greater overlap than that of the average fund pair in the sample.

C. Social Interactions and Trades

We next investigate whether fund pairs managed by neighbors are more likely to make similar trades than those that are managed by nonneighbors. In Table III we test whether social interactions affect the trading behavior of mutual fund managers by estimating regression (4) using the purchase and sale overlap measures defined in equations (2) and (3), respectively, as the dependent variables. We estimate the regressions using three different specifications for both purchases and sales: the sample excluding fund pairs with common managers, the sample excluding fund pairs within the same fund family, and fund pairs managed by a single manager.

The results are similar to those of Table II—fund pairs that have managers who are neighbors have significantly more overlap in trades than those that do not. For common buy trades, the baseline model (column 1) indicates that fund pairs with managers who are neighbors have 116 bps more purchase overlap than those that live in the same media market. Given that the average purchase

Table III

Social Interactions and Mutual Fund Trades

The table reports the coefficient estimates and standard errors from OLS estimation of purchase and sale overlap on *Neighbors* and the control variables defined in Table II. The dependent variable for the regression results displayed in columns 1 through 3 (4 through 6) is the percentage of overlapping stock purchases (sales) between a given pair of funds during the quarter. These dependent variables are defined in equations (2) and (3), respectively. The sample includes 3.4 million quarterly fund-pair observations from 1996 to 2010. In columns 1 and 4, the sample is limited to fund pairs with no managers in common during quarter t. Columns 2 and 5 exclude fund pairs within the same fund family. In columns 3 and 6 the results are presented for the sample of funds where both funds in the pair are managed by a single manager. Standard errors, two-way clustered by each fund in the pair, are in parentheses. Significance levels are denoted by a, b, c, which correspond to the 1%, 5%, and 10% levels, respectively.

Dependent Variable:	% of Overlapping Buys			% of Overlapping Sales		
Sample:	No Mgrs. Common (1)	Different Families (2)	Single Mgr. Pairs (3)	No Mgrs. Common (4)	Different Families (5)	Single Mgr. Pairs (6)
Neighbors	1.16[a]	0.83[b]	4.51[a]	0.57[c]	0.48	0.73
	(0.38)	(0.36)	(1.25)	(0.30)	(0.30)	(0.82)
SameMFCity	−0.17	−0.14	0.21	0.22	0.23[c]	0.49[c]
	(0.14)	(0.14)	(0.31)	(0.13)	(0.14)	(0.28)
SameMediaMkt	0.80[a]	0.73[a]	0.54[b]	0.54[a]	0.49[a]	0.14
	(0.12)	(0.12)	(0.24)	(0.12)	(0.12)	(0.26)
SameFundFam	3.82[a]		6.26[a]	3.88[a]		5.99[a]
	(0.36)		(0.64)	(0.34)		(0.61)
MngOtherFundTogether	1.91[a]		−0.50	1.43[a]		−0.54
	(0.45)		(1.00)	(0.51)		(0.96)
BothSmallCap	0.99[a]	0.98[a]	0.79[a]	0.68[a]	0.67[a]	0.53[b]
	(0.19)	(0.19)	(0.28)	(0.18)	(0.18)	(0.26)
BothMidCap	1.56[a]	1.55[a]	1.07[a]	1.14[a]	1.13[a]	0.68[a]
	(0.21)	(0.21)	(0.31)	(0.18)	(0.18)	(0.25)
BothLargeCap	8.68[a]	8.65[a]	8.93[a]	6.82[a]	6.79[a]	7.67[a]
	(0.27)	(0.27)	(0.46)	(0.24)	(0.24)	(0.43)
BothValue	2.42[a]	2.43[a]	1.77[a]	0.78[a]	0.78[a]	1.03[c]
	(0.34)	(0.34)	(0.66)	(0.28)	(0.28)	(0.62)
BothGrowth	2.77[a]	2.76[a]	3.05[a]	2.43[a]	2.42[a]	2.42[a]
	(0.22)	(0.22)	(0.38)	(0.21)	(0.21)	(0.34)
BothBlend	1.84[a]	1.85[a]	1.14[a]	1.59[a]	1.58[a]	0.94[a]
	(0.24)	(0.24)	(0.31)	(0.23)	(0.23)	(0.30)
TNAQuinDiff	−0.03	−0.03	−0.06	−0.07[b]	−0.07[b]	−0.14[b]
	(0.03)	(0.03)	(0.05)	(0.03)	(0.03)	(0.06)
TNAQuinAvg	0.59[a]	0.58[a]	0.58[a]	0.40[a]	0.40[a]	0.46[a]
	(0.09)	(0.09)	(0.17)	(0.09)	(0.09)	(0.14)
Constant	1.48[a]	1.52[a]	1.39[b]	1.41[a]	1.44[a]	1.27[a]
	(0.32)	(0.32)	(0.57)	(0.29)	(0.29)	(0.47)
Adj R^2	0.14	0.14	0.16	0.10	0.10	0.12
N (thousands)	3,364	3,335	381	3,364	3,335	381

overlap is 848 bps between fund pairs, this suggests that funds with managers who are neighbors have 14% greater commonality in trades than funds whose managers live in the same media market. The most striking results are for the sample of single-manager pairs. The coefficient estimate on *Neighbors* is 451 bps, suggesting that the impact of social interactions on single-managed funds is huge—fund pairs with single managers who are neighbors have over 50% higher overlap in their buys than the average mutual fund pair in the sample.

The magnitude of the impact of social interactions for stock sales is smaller, although the *Neighbors* estimate of 57 bps in the baseline model (column 4) is statistically significant at the 10% level. This asymmetry is not surprising because mutual funds face short sale constraints so overlap in sales is conditional on both funds in a pair already owning the stock. Moreover, these short sale constraints lead to an asymmetry in how managers respond to information. For example, if a manager receives a positive signal about a stock, she can respond either by expanding the position if she already owns the stock (an "intensive margin buy") or by establishing a new position if she does not (an "extensive margin buy"). Both actions can be arbitrarily scaled—subject to regulatory limits—and reveal an equally strong conviction. But, if the signal is negative, the strongest response is to liquidate the position ("extensive margin sell"). Therefore, on the sell side, we would expect that social interactions are more likely to induce extensive margin transactions, but on the buy side we would expect no such asymmetry.

To examine this conjecture, we decompose purchases and sales into extensive and intensive margins. We use an approach analogous to that in equations (2) and (3) to calculate these alternative overlap measures. By construction, for each fund pair and for both buys and sales, the sum of the intensive margin overlap and the extensive margin overlap that requires at least one of the funds to trade on the extensive margin is equal to our original overlap value. The results are summarized in Table IV. The table confirms that, for buys, both the extensive margin and the intensive margin overlap measures are statistically significant, while for sales the *Neighbors* coefficient is significantly positive only for extensive margins.

D. Manager Characteristics

Up to this point, we measure the likelihood of social contact using the distance between two managers' homes. However, the likelihood can be further refined *within* the group of neighbors: social interactions may be more likely or frequent between those neighbors who share common characteristics or among neighbors who have known each other longer. The idea that people gravitate toward those who are similar to themselves, called homophily, is well established in the sociology literature (Lazarsfeld and Merton (1954)). Common characteristics between neighbors could proxy for the quality of their network (Bertrand, Luttmer, and Mullainathan (2000)), so we expect that, when network quality is higher, the manager pair has a greater commonality in their

Table IV

Extensive and Intensive Margin Overlap

The table reports the coefficient estimates and standard errors from OLS estimation of various measures of portfolio overlap on *Neighbors*, *SameMediaMkt*, and *SameMFCity*. Each row reports results for a separate regression, which include all control variables from Table II. Also reported is the average overlap in the sample for the dependent variable. Overlap in extensive purchases is the purchase overlap of newly initiated purchases. In row 1 (2) the dependent variable includes purchase overlap in stocks that were not held at the end of the previous quarter by either (at least one) fund in the pair. Intensive purchase overlap is the purchase overlap in stocks that were held by both funds during the previous quarter. Overlap in extensive sales is the sale overlap in stocks that were completely sold off during the quarter. In row 4 (5) the dependent variable includes sale overlap in stocks that are not held at the end of the current quarter by either (at least one) fund in the pair. Intensive sales overlap is the sale overlap in stocks that are held by both funds at the end of the current quarter. Note that the sum of extensive sales (buys) for at least one fund overlap and intensive sale (buy) overlap is equal to the sale (buy) overlap for each fund. The analysis in the table uses quarterly fund-pair observations for the sample of 3.4 million purchase and sale overlap observations from 1996 to 2010. The samples are limited to fund pairs that have no managers in common during quarter t. Standard errors, two-way clustered by each fund in the pair, are in parentheses. Significance levels are denoted by a, b, c, which correspond to the 1%, 5%, and 10% levels, respectively.

Overlap in:	*Neighbors*	*Same MFCity*	*Same MediaMkt*	Avg. Overlap
Extensive buys (for both funds)	0.21[b]	0.10[a]	0.15[a]	1.77
	(0.09)	(0.04)	(0.03)	
Extensive buys (for at least one fund)	0.47[b]	0.04	0.50[a]	5.01
	(0.19)	(0.07)	(0.06)	
Intensive buys	0.68[a]	−0.21[a]	0.31[a]	3.47
	(0.21)	(0.07)	(0.07)	
Extensive sales (for both funds)	0.16[b]	0.01	0.01	1.08
	(0.07)	(0.03)	(0.02)	
Extensive sales (for at least one fund)	0.40[a]	0.06	0.15[a]	3.12
	(0.14)	(0.05)	(0.05)	
Intensive sales	0.18	0.16	0.39[a]	3.55
	(0.19)	(0.09)	(0.08)	

trades and holdings. We test this conjecture by interacting *Neighbors* with various manager-specific characteristics. The characteristics that we investigate include working at the same fund family, age, ethnicity, portfolio management experience, and attending the same college.

Specifically, *SimilarAge* is a dummy variable that is one if the difference in the managers' ages is less than the median in the sample (10 years) and *SameEthnicity* is a dummy that is one if the managers are of the same minority ethnicity.[11] *BothExp* (*BothInexp*) is a dummy variable that is one

[11] We use a classification algorithm developed by Ambekar et al. (2009) to group names into one of 13 categories, including Indian, Jewish, Muslim, East Asian, and others. The classifier applies a Hidden Markov Model trained with names gathered from Wikipedia. We classify a fund manager as belonging to an ethnic group if the predicted probability assigned by the algorithm is above 85%. Visual inspection of the classification results confirms that the algorithm appears to perform well. The classifier may be accessed at http://www.textmap.com/ethnicity/.

if both managers have more (less) portfolio management experience than the median manager in the sample. *SameCollege* is a dummy variable that is one if both managers attended the same college or university. In this analysis, we follow the specification described in regression (4), but we use manager-pair observations instead of fund-pair observations because characteristics are identified at the manager level. As a result, we now have multiple observations for fund pairs with more than one manager. This means that the observations are not independent, so we continue to two-way cluster the standard errors by each fund in the pair. As we show later, our estimates and standard errors using the manager-pair specification are nearly identical to those in the collapsed fund-pair specification.

The results of the tests are summarized in Table V. We focus on the results for holding and purchase overlaps since sales overlap is less informative, because commonality in sales is conditional upon both funds holdings the stock. In the first column we report the interaction of the *Neighbors* dummy with *SameFundFamily*, which indicates whether the two funds are managed by the same fund family. Since neighbors who also work for the same family are very likely to know each other, this specification can tell us how much our main specification underestimates the word-of-mouth effect. If *Neighbors* perfectly captured the existence of a social connection, the coefficient on *Neighbors* $\times$ *SameFundFamily* would be zero; therefore, a nonzero coefficient reveals the extent to which it is an imperfect measure. The estimates indicate that funds with neighbors who work together have an abnormal portfolio (purchase) overlap of 380 (632) bps, compared to an abnormal overlap of 88 (71) bps for all other neighbors. Assuming that neighbors who work together definitely know each other, these estimates imply that about $71/632 = 11\%$ to $88/380 = 23\%$ of the neighbors that we identify in the sample have social contact. This in turn suggests that the word-of-mouth effect may be similar in magnitude to that of managing another fund together.

The analysis in the remaining columns of the table shows that funds managed by neighbors who are more likely to interact socially because of homophily have greater commonality in their holdings and trades. Fund pairs with managers who are neighbors and are of a similar age have greater overlap in purchases though the coefficient is not statistically significant. Funds with managers who are neighbors and are of the same ethnicity have a remarkably high overlap in holdings and purchases. Their commonality in holdings is 296 bps more than that of funds that are managed by neighbors who do not have the same minority ethnicity.

The specifications that include interactions with *BothExp* and *BothInexp* give us the first indication of what type of information is spread through social interaction. The table shows that at least one manager in each pair must be experienced in order for there to be investment-related information flow between neighbors, suggesting that the information is value-relevant and not merely noise. The greatest overlap occurs in funds where both managers are experienced. In these cases the abnormal holding overlap is 90 bps more than when one manager is experienced and the other is not. For purchases, the

Table V

Social Interactions and Manager Characteristics

The table reports the coefficient estimates and standard errors for OLS estimation of measures of portfolio overlap on *Neighbors* and interactions of *Neighbors* with various manager characteristics. The dependent variable for the regression results displayed in columns 1 through 5 is the percentage of overlapping stock holdings between the funds managed by a given pair of managers, and for columns 6 through 10 the dependent variable is the percentage of overlapping stock purchases between the funds managed by a given pair of managers during the quarter. *SameFundFam* is a dummy variable that is one if the two funds in the fund pair belong to the same fund family. *SimilarAge* is a dummy variable that is one if the difference in the managers' ages is less than the median in the sample (10 years). *SameEthnicity* is a dummy that is one if the surnames of the managers are of the same minority ethnicity. *SameCollege* is a dummy variable that is one if both managers attended the same college or university. College attendance data are from Morningstar. *BothExp* (*BothInexp*) is a dummy variable that is one if both managers have more (less) portfolio management experience than the median manager in the sample. All regressions include the control variables described in Table II. The samples are limited to manager pairs whose funds have no managers in common during quarter t. Standard errors, two-way clustered by each fund in the pair, are in parentheses. Significance levels are denoted by a, b, c, which correspond to the 1%, 5%, and 10% levels, respectively.

	% of Overlapping Holdings					% of Overlapping Buys				
Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Neighbors	0.88^{a}	1.11^{a}	0.99^{a}	0.81^{b}	0.61	0.71^{b}	0.84^{c}	1.03^{a}	0.59^{c}	−0.02
	(0.28)	(0.37)	(0.29)	(0.34)	(0.40)	(0.35)	(0.50)	(0.37)	(0.35)	(0.41)
Neighbors × *SameFundFam*	2.92^{b}					5.61^{a}				
	(1.34)					(1.64)				
Neighbors × *SimilarAge*		−0.10					0.43			
		(0.46)					(0.61)			
SimilarAge		0.40^{a}					0.29^{a}			
		(0.08)					(0.07)			
Neighbors × *SameEthnicity*			3.87^{a}					2.81^{c}		
			(1.14)					(1.53)		
SameEthnicity			-0.91^{a}					0.11		
			(0.35)					(0.28)		
Neighbors × *SameCollege*				0.84					0.32	
				(0.79)					(1.09)	
SameCollege				−0.19					−0.13	
				(0.18)					(0.17)	
Neighbors × *BothExp*					1.16^{b}					3.33^{a}
					(0.52)					(0.73)

(Continued)

Table V—*Continued*

Dependent Variable:	% of Overlapping Holdings (1)	(2)	(3)	(4)	(5)	% of Overlapping Buys (6)	(7)	(8)	(9)	(10)
Neighbors × *BothInexp*					−0.24					−0.57
					(0.61)					(0.52)
BothExp					−0.26[b]					−0.30[b]
					(0.11)					(0.12)
BothInexp					0.38[a]					0.43[a]
					(0.13)					(0.12)
SameFundFam	3.11[a]	3.13[a]	3.15[a]	2.57[a]	3.00[a]	3.23[a]	3.29[a]	3.32[a]	2.98[a]	3.24[a]
	(0.41)	(0.40)	(0.40)	(0.40)	(0.40)	(0.40)	(0.40)	(0.40)	(0.40)	(0.37)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$Adj R^2$	0.40	0.40	0.40	0.40	0.39	0.14	0.14	0.14	0.13	0.13
N (thousands)	8,823	8,823	8,823	6,099	7,075	7,353	7,353	7,353	5,045	5,873

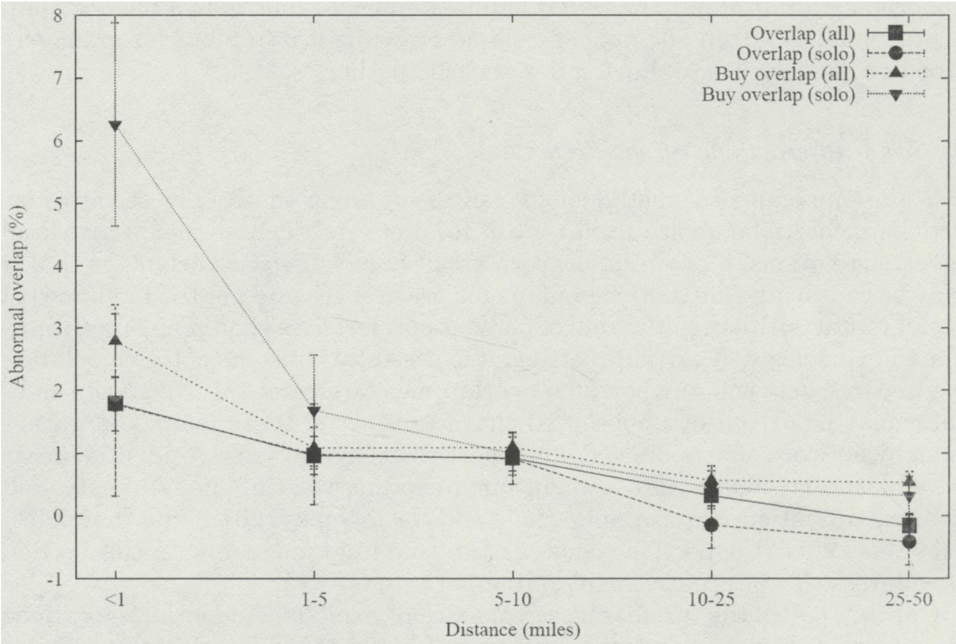


Figure 3. Portfolio overlap and manager distance. The figure plots abnormal portfolio overlap in holdings and purchases for all fund pairs and for fund pairs with single managers as a function of the distance between the residences of the funds' managers. For distances of less than five miles, we use the population density–adjusted driving distance, as explained in the text; larger distances are not adjusted and are computed using the distance between the geographic centers of zip codes. Abnormal overlap is the remaining portfolio overlap after controlling for funds matching on Morningstar size or value/growth categories (*BothSmallCap*, *BothMidCap*, *BothLargeCap*, *BothValue*, *BothGrowth*, *BothBlend*), funds being from the same mutual fund family (*SameFundFam*), and funds having at least one pair of managers that manage at least one other fund together (*MngOtherFundTogether*). It is estimated using all fund-pair observations, excluding those fund pairs with at least one manager in common, as in columns 5 and 7 of Table II and columns 1 and 3 in Table III. Standard errors are shown as error bars.

abnormal overlap among neighbors only occurs when both neighbors are experienced portfolio managers. These results suggest that information is likely being shared; we confirm this with further analysis in Section III.

Finally, our neighbor cutoff is somewhat arbitrary, and using a shorter distance threshold could further increase the probability that two managers are socially connected. Therefore, to provide additional insight on how much our baseline results underestimate the word-of-mouth effect, we reestimate regression (4), but add dummy variables that indicate if funds have managers who live within one normalized mile, one to five normalized miles, 5 to 10 miles, 10 to 25 miles, and 25 to 50 miles. The results are displayed in Figure 3. The figure shows that overlap in holdings and trades is much higher for funds that have managers who live within one normalized mile of one another than for those with managers who live between one and five miles of one another. The abnormal overlap in purchases for single-managed funds is 626 bps for managers who live within one mile of each other, while it is 168 bps for managers who live between one and five miles of each another.

E. Social Interactions versus Preferences

While our empirical methodology can disentangle the role of social interactions in portfolio choice from that of local effects, such as media and local investor relations, a potential concern about our *Neighbors* variable is that it may be capturing similarities in manager preferences and not the likelihood of social contact. Although the role of personal preferences should be largely mitigated in a delegated portfolio setting, it is possible that some of the correlation we uncover between our portfolio overlap measures and the *Neighbors* indicator may be driven by unobserved characteristics of these managers, rather than by network effects. Indeed, managers choosing the same residential areas may be similar along many dimensions including wealth, age, ethnicity, and political affiliation. For example, Hong and Kostovetsky (2012) find that political values affect the portfolio choices of mutual fund managers. In this section we address this alternative explanation.

Our first test of the "preferences explanation" exploits residential relocations of managers. If managers with similar preferences tend to select into similar neighborhoods and these common preferences are driving the commonality in trades and holdings, then these managers should exhibit similar trading behavior *prior* to becoming neighbors. This test relies on the traditional economic assumption that preferences over investment behavior and housing choice are stable over time, which is supported by recent empirical evidence.[12] To conduct the test, we create two additional variables: $FutureNeighbors_{k,l,t}$ is a dummy variable that is one if managers k and l are neighbors for some $s > t$ but are not neighbors for $s \leq t$ and $PastNeighbors_{k,l,t}$ is a dummy variable that is one if managers k and l are neighbors for some $s < t$ but are not neighbors for $s \geq t$.

In Table VI we report estimates of

$$\begin{aligned} Overlap_{i,j,t} &= \alpha + \beta_f FutureNeighbors_{k,l,t} + \beta Neighbors_{k,l,t} \\ &+ \beta_p PastNeighbors_{k,l,t} + \delta SameMFCity_{i,j,t} \\ &+ \gamma SameMediaMkt_{k,l,t} + \Gamma' \mathbf{Controls}_{i,j,t} + \epsilon_{i,j,t}, \end{aligned} \tag{5}$$

where $Overlap_{i,j,t}$ is the holdings or trade overlap between funds i and j at time t, manager k is a manager of fund i, while manager l is a manager of fund j, and the control variables are described in Table II. Note that *Neighbors* is manager specific in this specification; therefore, we estimate the regression using manager-pair observations as in Table V.

[12] Cronqvist, Münkel, and Siegel (2011) find that about 40% of home location choice is determined by genetics and over 30% is driven by childhood environment. Barnea, Cronqvist, and Siegel (2010) show that about one-third of investment behavior is determined by genetics alone. Cesarini et al. (2009, 2010) find similar results for risk-taking. See also Andersen et al. (2008).

Table VI

Social Interactions versus Preferences: Neighbor Relocations

The table reports the coefficient estimates and standard errors from OLS estimation of holdings, purchase, and sale overlap on *Neighbors*, *FutureNeighbors*, and *PastNeighbors*. The analysis in the table uses quarterly manager-pair observations for the sample of approximately 8.8 million quarterly manager-pair holdings overlap observations and 7.4 million quarterly manager-pair purchase and sale overlap observations from 1996 through 2010. *Neighbors* is a dummy variable that is one if the pair of managers live within 2.6 normalized miles of one another during the quarter. *FutureNeighbors* is a dummy variable that is one if two managers are neighbors in the future, but are not currently neighbors ($Neighbors_{i,j,s} = 1$ for some $s > t$). *PastNeighbors* is a dummy variable that is one if the managers in the pair were neighbors in the past, but are not currently neighbors ($Neighbors_{i,j,s} = 1$ for some $s < t$). All regressions include the control variables described in Table II. The sample is limited to manager pairs whose funds have no managers in common during quarter t. Standard errors, two-way clustered by each fund in the pair, are in parentheses. Significance levels are denoted by a, b, c, which correspond to the 1%, 5%, and 10% levels, respectively.

	% of Overlapping		
Dependent Variable:	Holdings (1)	Buys (2)	Sales (3)
FutureNeighbors	−0.38	−0.04	0.04
	(0.59)	(0.56)	(0.66)
Neighbors	1.08[a]	1.10[a]	0.54[c]
	(0.29)	(0.37)	(0.29)
PastNeighbors	1.18[b]	0.66	0.37
	(0.49)	(0.47)	(0.43)
Controls	Yes	Yes	Yes
$Adj R^2$	0.40	0.14	0.09
N (thousands)	8,823	7,353	7,353

If overlapping preferences drive the commonality in trades and holdings between neighbor managers, then the estimate of β_f should be positive. Table VI shows that this is not the case. The coefficient estimates on *FutureNeighbors* for overlap in holdings, purchases, and sales are not statistically different from zero. The coefficient estimates on *Neighbors* are significantly estimated at 108 bps, 110 bps, and 54 bps for overlap in holdings, buys, and sales, respectively. As might be expected, there is some evidence that past neighbors may remain in contact and continue to discuss investment ideas. The coefficient estimate on *PastNeighbors* is positive and significantly estimated for overlap in holdings, and is positive but not statistically different from zero for overlap in both buys and sales.

In Figure 4 we extend this analysis by asking how the length of time that managers have been neighbors influences their information sharing. We plot the relationship between "neighbor tenure" and abnormal overlap, where neighbor tenure is the number of years two managers have been neighbors (possibly starting before becoming managers). Under the word-of-mouth hypothesis we would expect no abnormal overlap prior to becoming neighbors

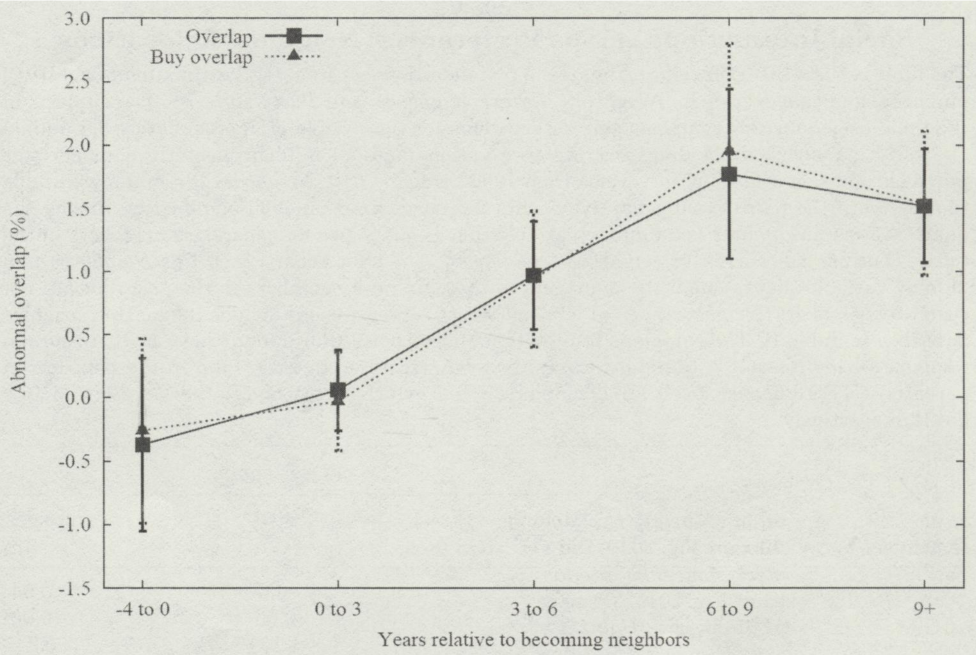


Figure 4. Portfolio overlap and neighbor tenure. The figure plots abnormal portfolio overlap in holdings and purchases as a function of time relative to a manager pair becoming neighbors. A pair is classified as becoming neighbors in the first quarter that they live within 2.6 normalized miles of each other (regardless of whether they were fund managers at the time). Specifically, we estimate

$$\begin{aligned}
&Overlap_{i,j,t} = \alpha + \beta_f FutureNeighbors_{i,j,t} \times I\{-4 \geq NeighborsTenure_{i,j,t} < 0\} \\
&+ \beta_1 Neighbors_{i,j,t} \times I\{0 \leq NeighborTenure_{i,j,t} < 3\} \\
&+ \beta_2 Neighbors_{i,j,t} \times I\{3 \leq NeighborTenure < 6\} \\
&+ \beta_3 Neighbors_{i,j,t} \times I\{6 \leq NeighborTenure < 9\} + \beta_4 Neighbors_{i,j,t} \times I\{9 \leq NeighborTenure\} \\
&+ \delta SameMFCity_{i,j,t} + \gamma SameMediaMkt_{i,j,t} + \Gamma' \mathbf{Controls}_{i,j,t} + \epsilon_{i,j,t},
\end{aligned}$$

where $NeighborTenure$ is the number of years that two managers have been neighbors and $I\{\cdot\}$ is an indicator function. The model is estimated using all manager-pair observations, excluding those manager pairs with at least one manager in common, as in columns 5 and 8 of Table II and columns 1 and 4 in Table III. The figure plots the coefficient estimates of β_f, β_1, β_2, β_3, and β_4. Standard errors are shown as error bars.

and that it may take some time once becoming neighbors to meet or build enough trust to be willing to share investment ideas.

This is precisely what we find. Overlap is increasing in the time two managers are neighbors. Additionally, it takes at least three years, on average, before neighbor managers show significant overlap in their holdings or trades. Not only is this result intuitive if the *Neighbors* dummy is capturing the likelihood of social interactions, but it casts additional doubt on the preferences alternative since it is harder to argue that similarities in preferences that drive housing choice should require an incubation period of three years before taking effect on portfolio decisions.

As a second test of the preferences story, we include additional demographic control variables in our quarterly manager-pair specification. If managers select certain neighborhoods that match their preferences, then controlling for the characteristics of those neighborhoods should eliminate or at least reduce the effect of preferences on investment behavior. Unlike our distance measures, these demographic control variables allow us to test whether managers living in similar environments in different parts of the country exhibit similar investment behavior. For instance, the demographic control variables pick up whether a manager who lives in a highly religious area of Ohio is likely to invest similarly to a manager who lives in a highly religious area of California.

Specifically, we first control for the effects of common religious beliefs and similar wealth by including five additional variables in our regression specifications. The variables *BothReligiousAreas*, *BothJewishAreas*, and *BothCatholicAreas* are dummies equal to one if the percentage of the population that are religious adherents, adherents to Judaism, or adherents to Catholicism in both managers' counties of residence are greater than the 75th percentile in the sample, respectively, and *BothHighHmValueAreas* (*BothLowHmValueAreas*) is a dummy variable equal to one if the median home price in both managers' zip code of residence is greater than the 75th percentile (less than the 25th percentile) in the sample.[13]

For each measure of overlap, Panel A of Table VII reports the results from the baseline regression and the regression including the additional demographic control variables using our quarterly manager-pair specification. While the religious control variables are highly significant in all specifications, they do not significantly alter the coefficient estimates on *Neighbors*. Including these control variables, however, does substantially reduce the coefficient estimates of *SameMediaMkt* for all three overlap measures. For instance, when investigating the overlap in holdings between funds, the estimate on *SameMediaMkt* falls from 54 bps to 11 bps after including the demographic control variables, and for purchase overlap the coefficient estimate falls from 78 bps to 33 bps.

Since there is large variation in housing within counties and zip codes, we further extend the analysis summarized in Panel A by controlling for additional manager-specific attributes in order to successfully capture commonality between two people. Ideally, we should control for all factors that affect both neighborhood and investment choices. For example, age, wealth, marital status, and number of children are likely determinants of neighborhood selection and could also influence risk-taking behavior. Age is available for a large proportion of the managers in our sample. In addition, while data are not readily available on wealth, marital status, or number of children, we use

[13] County-level religious data are from the U.S. Church Membership Data from the Association of Religious Data Archives (http://www.thearda.com). Zip code–level median home prices are from the 2000 Census.

Table VII
Social Interactions versus Preferences: Additional Controls

The table reports the coefficient estimates and standard errors from OLS estimation of holdings, purchase, and sale overlap on *Neighbors* and demographic and manager control variables in Panels A and B, respectively. The analysis in the table uses the quarterly manager-pair observations outlined in Table IV. *BothReligiousAreas*, *BothJewishAreas*, and *BothCatholicAreas* are dummy variables that are one if the percentage of the population that are religious adherents, adherents to Judaism, or adherents to Catholicism in both managers' counties of residence are greater than the 75th percentile in the sample, respectively. *BothHighHmValueAreas* (*BothLowHmValueAreas*) is a dummy variable that is one if the median home price in both managers' zip code of residence is greater than the 75th percentile (less than the 25th percentile) in the sample. County-level religious data are from the U.S. Church Membership Data from the Association of Religious Data Archives, and zipcode–level median home prices are from the 2000 U.S. Census. *SimilarAge* is a dummy variable that is one if the difference in the managers' ages is less than the median in the sample (10 years). $HomePriceRatio = \min\{HP_1, HP_2\} \max\{HP_1, HP_2\}$, where HP_i is the estimate of the current value of manager i's primary residence. $LotSizeRatio = \min\{LS_1, LS_2\} \max\{LS_1, LS_2\}$, where LS_i is the lot size of manager i's primary residence. $HomeAgeRatio = \min\{HA_1, HA_2\} \max\{HA_1, HA_2\}$, where HA_i is the age of manager i's primary residence. Home characteristic data are from zillow.com. The estimates of the home prices are as of February 2013. All regressions include the control variables described in Table II. The samples are limited to manager pairs whose funds have no managers in common during quarter t. Standard errors, two-way clustered by each fund in the pair, are in parentheses. Significance levels are denoted by a, b, c, which correspond to the 1%, 5%, and 10% levels, respectively.

Panel A: Controlling for Demographic Characteristics

	% of Overlapping					
	Holdings		Buys		Sales	
Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)
Neighbors	1.07[a]	1.07[a]	1.10[a]	1.02[a]	0.53[c]	0.63[b]
	(0.29)	(0.29)	(0.37)	(0.35)	(0.28)	(0.28)
SameMFCity	0.26[c]	0.22	−0.06	−0.11	0.30[b]	0.28[b]
	(0.15)	(0.14)	(0.14)	(0.13)	(0.13)	(0.13)
SameMediaMkt	0.54[a]	0.11	0.78[a]	0.33[a]	0.56[a]	0.27[b]
	(0.15)	(0.11)	(0.13)	(0.11)	(0.13)	(0.10)
BothReligiousAreas		0.86[a]		0.59[a]		0.81[a]
		(0.22)		(0.17)		(0.17)
BothJewishAreas		0.12		0.30[c]		0.08
		(0.21)		(0.18)		(0.19)
BothCatholicAreas		0.19		0.23		−0.04
		(0.25)		(0.20)		(0.19)
BothHighHomeValueAreas		−0.21		0.14		−0.47[a]
		(0.22)		(0.19)		(0.17)
BothLowHomeValueAreas		0.11		−0.24		−0.07
		(0.22)		(0.18)		(0.18)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
$Adj R^2$	0.40	0.40	0.14	0.14	0.09	0.09
N (thousands)	8,823	8,823	7,353	7,353	7,353	7,353

(Continued)

Table VII—*Continued*

Panel B: Controlling for Manager Characteristics and Manager Home Characteristics						
	% of Overlapping					
	Holdings		Buys		Sales	
Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)
Neighbors	1.10[a]	1.00[a]	1.39[a]	1.35[a]	0.68[b]	0.61[c]
	(0.34)	(0.34)	(0.44)	(0.44)	(0.33)	(0.33)
SameMFCity	0.36[b]	0.35[b]	0.02	0.01	0.23[c]	0.22
	(0.15)	(0.15)	(0.14)	(0.14)	(0.14)	(0.13)
SameMediaMkt	0.71[a]	0.68[a]	0.81[a]	0.78[a]	0.69[a]	0.66[a]
	(0.17)	(0.17)	(0.15)	(0.14)	(0.16)	(0.16)
SimilarAge		0.52[a]		0.40[a]		0.49[a]
		(0.09)		(0.08)		(0.08)
HomePriceRatio		0.42[c]		0.35[c]		0.37[b]
		(0.24)		(0.21)		(0.18)
LotSizeRatio		0.30		0.03		0.09
		(0.19)		(0.16)		(0.16)
HomeAgeRatio		−0.04		−0.16		−0.07
		(0.14)		(0.14)		(0.13)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
$Adj R^2$	0.40	0.40	0.14	0.14	0.09	0.09
N (thousands)	3,851	3,851	3,211	3,211	3,211	3,211

information on various features of the managers' homes to proxy for these personal characteristics.

We collect manager home characteristics from Zillow, a website that compiles data from county assessor records on home size, home characteristics, and lot size, and uses a proprietary method to calculate an estimate of the home's value. To control for similarity in wealth, we include *HomePriceRatio*, a variable that captures the similarity in the managers' home values. To control for marital status and having school age children, we include similarity in two variables, manager age and lot size, *SimilarAge* and *LotSizeRatio*, respectively. Lot size is included since married individuals with children may be more likely to prefer quieter suburbs. In addition, this variable will likely capture the urban versus rural housing choice of the manager. We also add a variable that measures the similarity in the age of the homes of the two managers, *HomeAgeRatio*, hypothesizing that certain individuals prefer older, while others prefer newer, homes.

Panel B of Table VII reports the regression results. Managers who are close in age and who have similarly valued homes have greater overlap in their holdings and trades regardless of whether they live close to one another. Importantly, while these proxies for similarity in wealth and risk-aversion matter for overlap, they do not significantly alter the coefficient estimates on *Neighbors*. The inclusion of these additional controls reduces the estimate on *Neighbors* from 110 bps to 100 bps, 139 to 135 bps, and 68 to 61 bps for overlap in holdings, purchases, and sales, respectively. The analysis suggests that personal attributes exist that drive both the housing choice and the investment choice of mutual fund managers, but these common attributes cannot explain the abnormal overlap between neighbor managers.

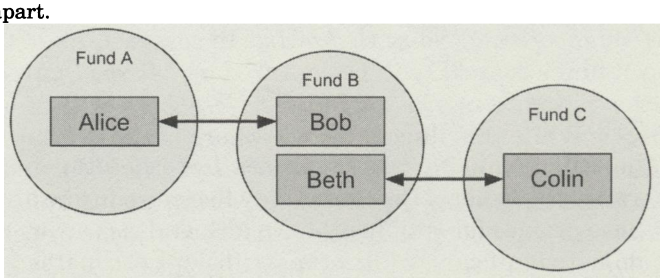
Finally, our third test against the preferences story is summarized in Table VIII. Our empirical strategy is to identify indirect neighbors using "intransitive triads" (Bramoullé, Djebbari, and Fortin (2009))—that is, managers who are not neighbors but are connected through team-managed funds. By looking only at these connected managers who are not neighbors, we significantly mitigate any endogeneity concerns that arise from common characteristics that could cause managers to select into the same neighborhood.

We classify two funds as indirect neighbors if they each have a manager with a *different* neighbor and those neighbors comanage another fund. Importantly, we also require that indirect-neighbor funds are not neighbors, and that the minimum distance between their managers' homes is five normalized miles. (In a second specification, the minimum distance is 50 miles.) For example, in the diagram in Table VIII, fund A and fund C are indirect neighbors because (1) Alice and Bob are neighbors, (2) Beth and Colin are neighbors, and (3) Bob and Beth comanage fund B but are not neighbors. In other words, Alice and Colin are indirect neighbors, linked to each other through Bob and Beth.

Table VIII shows that we see abnormal overlap in holdings and trades for both indirect neighbors (separated by at least five normalized miles) and distant indirect neighbors (separated by at least 50 miles). The magnitude of the coefficients on *IndirectNeighbors* and *DistantIndirectNeighbors* is similar to

Table VIII
Social Interactions versus Preferences: Indirect Neighbors

The table reports the coefficient estimates and standard errors from OLS estimation of holdings, purchase, and sale overlap on *Neighbors*, *IndirectNeighbors*, and *DistantIndirectNeighbors*. The analysis in the table uses quarterly fund-pair observations outlined in Table II. *IndirectNeighbors* is a dummy variable that is one if the funds in the pair are indirect neighbors and zero otherwise. Two funds are classified as indirect neighbors if they each have a manger who is a neighbor of a different manager of a common fund. Indirect neighbors cannot also be neighbor funds, and the minimum distance between their managers' homes is five normalized miles. For example, in the diagram below, fund A and fund C are indirect neighbors because Alice and Bob are neighbors and Beth and Colin are neighbors, and Bob and Beth comanage fund B. If, however, Alice and Colin were both neighbors of Bob, then funds A and C would not be indirect neighbors. *DistanctIndirectNeighbors* is a dummy that adds the requirement that indirect neighbors be at least 50 miles apart.



All regressions include the control variables described in Table II. The samples are limited to manager pairs whose funds have no managers in common during quarter t. Standard errors, two-way clustered by each fund in the pair, are in parentheses. Significance levels are denoted by a, b, c, which correspond to the 1%, 5%, and 10% levels, respectively.

	% of Overlapping					
	Holdings		Buys		Sales	
Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)
IndirectNeighbors	0.91[b]		0.80[b]		0.73[c]	
	(0.36)		(0.39)		(0.37)	
DistantIndirectNeighbors		1.05[c]		1.36[b]		1.18[b]
		(0.55)		(0.55)		(0.52)
Neighbors	0.99[a]	0.99[a]	1.16[a]	1.16[a]	0.58[c]	0.57[c]
	(0.29)	(0.29)	(0.38)	(0.38)	(0.30)	(0.30)
SameMFCity	0.19	0.19	−0.17	−0.17	0.22	0.22
	(0.15)	(0.15)	(0.14)	(0.14)	(0.13)	(0.13)
SameMediaMkt	0.39[a]	0.39[a]	0.80[a]	0.80[a]	0.54[a]	0.54[a]
	(0.14)	(0.14)	(0.12)	(0.12)	(0.12)	(0.12)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
$AdjR^2$	0.41	0.41	0.14	0.14	0.10	0.10
N (thousands)	4,084	4,084	3,364	3,364	3,364	3,364
Mean of dependent variable when:						
Neighbors = 1		11.06		11.68		8.99
IndirectNeighbors = 1		9.65		9.96		7.94
DistantIndirectNeighbors = 1		9.09		9.74		7.72

that on *Neighbors*. While it first appears surprising that these indirect coefficients are larger than the *Neighbors* coefficient in some specifications, this result is driven by the fact that the *Neighbors* coefficient only measures overlap beyond that explained by community effects, while this is not necessarily true for indirect neighbors. For example, only 65% of indirect neighbor pairs live in the same media market and none of the distant indirect pairs do.

To illustrate using the diagram in Table VIII, suppose Alice manages a fund in Boston, as does her neighbor Bob. Alice shares two stock picks with Bob. Bob's comanager, Beth, vacations on Cape Cod near Colin, who manages a fund in New York. If all three funds trade on Alice's information, the abnormal overlap between funds A and B will be captured by *Neighbors*, *SameMediaMkt*, and *SameMFCity*, while the abnormal overlap between funds B and C will be captured only by *Neighbors* and *SameMediaMkt*. In contrast, all of the abnormal overlap between funds A and C will be captured by *DistantIndirectNeighbors*. As a result, the estimate of *DistantIndirectNeighbors* will be greater than that of *Neighbors*. While this illustrates why more of the overlap is attributed to the word-of-mouth channel in the *DistantIndirectNeighbors* coefficient, the last three rows of the table show that average overlap among indirect neighbors is lower than among neighbors, indicating that fewer ideas are shared among indirect neighbors.

While this approach provides a clean test to address the selection problem that arises from managers' nonrandom neighborhood choice, one could argue that managers who comanage a fund may also have similar preferences to successfully work together. If neighborhood and job choices reveal an overlapping set of personal attributes, by transitivity, indirect neighbors—though not selecting into the same neighborhoods or jobs—may still be somewhat similar. To rule out this explanation we control for common preferences in the indirect neighbor regressions by including the housing, demographic, and manager-level characteristics used previously in Table VII. The results are displayed in the Internet Appendix.[14] Controlling for similar preferences has almost no effect on the *DistantIndirectNeighbors* coefficients for all three measures of overlap. In fact, the coefficient estimates slightly increase. This suggests that it is not likely that common preferences are driving the abnormal overlap in holdings and trades of indirect neighbors.

III. Are Neighbor Trades Informed?

A. Portfolio Tests

Our results thus far show that interpersonal communication with peers plays an important role in mutual fund managers' portfolio decisions. In this section, we directly test whether the word-of-mouth influence among these investors represents the transmission of value-relevant information, or whether managers are merely sharing personal sentiments and biases with each other. Not

[14] The Internet Appendix may be found in the online version of this article.

only do these analyses provide the first evidence on the nature of the word-of-mouth effect, but they also cast further doubt on the preferences alternative.

A priori, it is difficult to make a prediction. Since mutual fund managers are professional investors in a highly transparent industry and are subject to career concerns (Chevalier and Ellison (1999)), it would be puzzling if peers could systematically bias each other toward the wrong stock. On the other hand, one may find the transmission of information among peers perhaps just as surprising. After all, why would managers willingly share their costly knowledge with one another? Indeed, in many models of informed trading, it is in the best interest of speculators to conceal their information advantage so that others cannot profit from it.

Several papers suggest, however, that informed traders may benefit from coordinating with each other (Froot, Scharfstein, and Stein (1992), Brunnermeir and Abreu (2002)), although this phenomenon may be more relevant to short-term speculators such as hedge funds. Among mutual funds, the cost of sharing information is not likely to be high, as the effect on relative performance of sharing a few stock picks with peers is probably small, especially in light of regulatory limits on the size of mutual fund positions or among managers who manage funds of different styles. Managers may also have an expectation of quid pro quo, so in this case sharing information could benefit them in the future. Stein (2008) provides a model of information exchange and discusses some of these possibilities.

To test whether the abnormal overlap in the investment decisions of neighbor managers reflects information sharing or, alternatively, a value-irrelevant exchange, we examine the performance of their common holdings and trades. If neighbors share information with one another concerning stock fundamentals, then the subsequent returns of the stocks in which neighbors make overlapping portfolio decisions should reflect this information.

For our holdings-based portfolio tests, at the beginning of each quarter we sort stocks in each fund's portfolio into two groups based on whether any of the fund's neighbors also hold the stock in the previous quarter. If at least one neighbor also owns the stock in the previous quarter, we place the stock in the "neighbor" portfolio; otherwise, the stock is allocated to the nonneighbor ("other") portfolio. We rebalance our neighbor and nonneighbor portfolios each quarter.

To compare the performance of neighbor holdings to those of nonneighbor holdings, we calculate neighbor and nonneighbor monthly portfolio returns for each fund using holdings reported at the end of the previous quarter:

$$R_{i,t}^{N} = \sum_{k \in \mathcal{N}} \left(\frac{w_{i,k,t}}{\sum_{k \in \mathcal{N}} w_{i,k,t}} \right) r_{k,t+1} \tag{6}$$

and

$$R_{i,t}^{O} = \sum_{k \in \mathcal{O}} \left(\frac{w_{i,k,t}}{\sum_{k \in \mathcal{O}} w_{i,k,t}} \right) r_{k,t+1}, \tag{7}$$

where $\mathcal{N}$ is the set of stocks held by at least one of fund i's neighbors and $\mathcal{O}$ is the set of all other stocks in fund i's portfolio not held by any of fund i's neighbors in quarter t. We average these monthly neighbor and nonneighbor returns for each fund in each quarter and then aggregate the quarterly neighbor and nonneighbor portfolio returns by calculating the weighted average of the returns in equations (6) and (7) across funds in quarter t, weighting each fund's return by its assets under management (TNA) (see, for instance, Coval and Moskowitz (2001)).

We use the average monthly benchmark-adjusted excess return as in Daniel et al. (1997, hereafter DGTW) and Wermers (2005)[15] to assess the performance of the neighbor and nonneighbor portfolios. We calculate the DGTW benchmark returns in two ways: first, we include the CRSP universe of common stocks in the calculation; second, we limit our sample to stocks with prices above \$5, as funds often face restrictions on investments in low-priced stocks.

The average returns for the neighbor (R^N) and nonneighbor (R^O) holdings portfolios over the 60 quarters and the difference between these averages are presented in Table IX. The table shows that, when we allocate stocks into portfolios based on common holdings, the neighbor portfolios perform no better or worse than the nonneighbor portfolios in our sample. While these results suggest that managers do not have an information advantage in shared portfolio holdings, many studies argue that holdings are a very noisy measure of managerial information: trades reflect a stronger conviction than does passively holding a stock (Cohen, Coval, and Pástor (2005)). Therefore, we next turn to common trades and examine whether these are informative.

In these trade-based portfolio tests, we form four distinct portfolios at the beginning of each quarter based on (1) whether the fund bought or sold a stock during the previous quarter and (2) whether the trade overlapped with a neighboring manager or not. Stocks that are bought are aggregated into the "buy" portfolio, while those that are sold are placed in the "sell" portfolio. We then create two subgroups within the buy and sell portfolios: "neighbor" and "nonneighbor" trades. We rebalance our portfolios every quarter based on the direction of the fund's trade, and those of its neighbors. For each fund-quarter, each stock's return in the portfolios is weighted by the new money it receives during the previous quarter, although equal-weighting produces qualitatively similar results.[16] Finally, we aggregate the neighbor and nonneighbor buy and sell portfolio returns in each quarter by averaging across funds, using the funds' TNA as weights.

Our trade-based portfolio results are summarized in Table X. For brevity, we only report DGTW benchmark returns using the CRSP universe of common stocks, but our alternative DGTW benchmark results are very similar. Additionally, we obtain qualitatively similar results using the Fama-French-Carhart four-factor model instead (see the Internet Appendix). We first

[15] DGTW data are available at http://www.smith.umd.edu/faculty/rwermers/ftpsite/Dgtw/coverpage.htm.

[16] All robustness results mentioned in this section are tabulated in the Internet Appendix.

Table IX

The Performance Effect of Social Interactions: Holdings

The table reports returns for "neighbor" (N) and "nonneighbor" (O) holdings of fund managers. The neighbor portfolio of fund i contains those stocks in the fund's portfolio that are also held by at least one other fund managed by a manager who lives in the neighborhood of at least one of the managers of fund i. Holdings that do not meet this criterion comprise the nonneighbor portfolio. We average the monthly performance measures

$$R_{i,t}^{N} = \sum_{k\in\mathcal{N}}\left(\frac{w_{i,k,t}}{\sum_{k\in\mathcal{N}} w_{i,k,t}}\right) r_{k,t+1}$$

and

$$R_{i,t}^{O} = \sum_{k\in\mathcal{O}}\left(\frac{w_{i,k,t}}{\sum_{k\in\mathcal{O}} w_{i,k,t}}\right) r_{k,t+1}$$

to calculate each fund's neighbor and nonneighbor returns in each quarter. We then average across funds in the quarter, using the dollar assets (TNA) of each fund in the previous quarter as weights, producing value-weighted average monthly returns of the neighbor and nonneighbor holdings portfolios. ($\mathcal{N}$ denotes the set of neighbor stocks, $\mathcal{O}$ the set of nonneighbor stocks, and $w_{i,k,t}$ the actual portfolio weight of fund i in stock k during quarter t.) Columns 2 and 3 report the average raw and risk-adjusted quarterly returns for the neighbor and nonneighbor portfolios for our sample period. Risk adjustment is based on DGTW benchmark returns. Column 4 reports the difference in returns between these portfolios. Standard errors are reported in parentheses. The sample is limited to fund pairs that have no managers in common during quarter t. Significance levels for tests of difference in means are denoted by a, b, c, which correspond to the 1%, 5%, and 10% levels, respectively.

	R^N	R^O	$R^N - R^O$
Excess return	0.62	0.55	0.07
	(0.48)	(0.46)	(0.66)
DGTW (entire CRSP universe)	0.02	0.01	0.02
	(0.07)	(0.08)	(0.10)
DGTW (CRSP with price>$5)	0.03	−0.02	0.05
	(0.07)	(0.08)	(0.10)

summarize the findings using the full sample of fund transactions. Column 1 of the table shows that the neighbor buy portfolio outperforms its size, book-to-market, and momentum benchmark portfolio by an average of 23 bps per month (2.8% per year). On the sell side, column 2 reveals that the neighbor portfolio underperforms its characteristics-adjusted benchmark by 24 bps per month (2.9% per year).

In column 3, we report the returns on the long-short strategy of buying the portfolio of stocks that mutual funds and their neighbors buy together and simultaneously selling those that they sell together. The strategy delivers 47 bps per month. The risk-adjusted performance measure is statistically significantly positive at the 10% level and implies an annualized above-benchmark return of 5.6%. Interestingly, our long-short results are driven, to some extent, by the strong performance of the sell side of the strategy. When we exclude the financial crisis from our sample period, however, the neighbor buy portfolio plays a stronger role. In this subsample, neighbor buys deliver an abnormal return of 33 bps per month (4.0% per year), resulting in long-short returns of 58 bps per month (7.0% per year).

Table X

The Performance Effect of Social Interactions: Trades

This table reports the performance of portfolios based on trades made by neighboring and nonneighboring mutual fund managers. For each fund, we form four distinct portfolios at the beginning of each quarter based on (1) whether the fund bought or sold a stock during the previous quarter and (2) whether the trade overlapped with a neighboring manager or not. Stocks that are bought are aggregated into the "buy" portfolio, while those that are sold are placed in the "sell" portfolio. We create two subgroups within the buy and sell portfolios: "neighbor" and "nonneighbor" trades. We calculate the average monthly returns of these portfolios for each fund in each quarter weighting each stock's return in the portfolios by the new money it receives during the previous quarter. We rebalance at the end of the quarter. We then average the returns of each subportfolio across the funds in our sample using the dollar assets (TNA) of each fund in the previous quarter as weights, producing value-weighted average monthly returns of the neighbor and nonneighbor buy and sell portfolios for each quarter. Columns 1 and 2 report the risk-adjusted average quarterly returns of the neighbor buy and sell portfolios, respectively. Risk adjustment is based on DGTW benchmark returns. Columns 4 and 5 report the corresponding results for the nonneighbor portfolios. Finally, columns 3 and 6 describe the difference of the returns of the buy and sell portfolios for the neighbor and nonneighbor stocks, respectively, and column 7 provides the difference-in-difference estimate. Standard errors are reported in parentheses. Portfolio returns are reported for the entire sample period and the sample period that excludes the financial crisis, as well as for (1) all fund transactions, (2) extensive margin transactions, and (3) intensive margin transactions. The sample is limited to fund pairs that have no managers in common during quarter t. Significance levels for tests of difference in means are denoted by a, b, c, which correspond to the 1%, 5%, and 10% levels, respectively.

	DGTW-Adjusted Monthly Returns						
	Neighbor Portfolio			Nonneighbor Portfolio			
	Buys	Sells	Diff.	Buys	Sells	Diff.	Diff.-Diff.
All trades							
Full sample	0.23	−0.24	0.47[c]	0.05	0.05	0.00	0.48[c]
	(0.18)	(0.19)	(0.27)	(0.12)	(0.09)	(0.12)	(0.27)
Excluding the financial crisis	0.33[c]	−0.25	0.58[c]	0.11	0.07	0.04	0.54[c]
	(0.19)	(0.21)	(0.30)	(0.13)	(0.10)	(0.13)	(0.30)
Extensive margin trades							
Full sample	0.25	−0.27	0.53[c]	0.19	0.26	−0.08	0.60[c]
	(0.23)	(0.24)	(0.29)	(0.16)	(0.18)	(0.21)	(0.35)
Excluding the financial crisis	0.30	−0.50[b]	0.81[a]	0.06	0.32	−0.26	1.07[a]
	(0.25)	(0.24)	(0.29)	(0.12)	(0.21)	(0.23)	(0.34)
Intensive margin trades							
Full sample	0.32[c]	−0.18	0.50[c]	−0.04	0.04	−0.07	0.58[b]
	(0.18)	(0.22)	(0.27)	(0.14)	(0.12)	(0.12)	(0.29)
Excluding the financial crisis	0.45[b]	−0.12	0.57[c]	0.06	0.07	−0.01	0.58[c]
	(0.19)	(0.24)	(0.31)	(0.15)	(0.13)	(0.13)	(0.32)

Columns 4 to 6 report the corresponding results for the nonneighbor buy and sell portfolios. In contrast to our neighbor portfolios, both nonneighbor buy and sell portfolio returns are positive and very close to zero on a benchmark-adjusted basis, falling between 4 and 11 bps per month (0.5% to 1.32% per year) in the full sample and the subsample that excludes the financial crisis.

Moreover, the long-short strategy that uses nonneighbor trades delivers no abnormal return. Finally, column 7 of the table shows the difference-in-difference estimates, which also control for managerial skill. Our difference-in-difference estimates are equal to approximately 48 bps per month (5.8% per year) and are statistically significant. Table X also reports results for extensive and intensive margin transactions separately. As discussed in Section II, social interactions appear to result in both intensive and extensive margin buys; on the sell side, however, they tend to only induce extensive margin trades. Our results in columns 1 to 3 are very consistent with this previous finding: the strong subsequent negative performance of the neighbor sell portfolio is entirely driven by extensive margin sales, while on the buy side this is not the case.

Our findings are essentially unaltered when we exclude fund pairs that belong to the same fund family or local stocks (those that are headquartered within 50 miles of the fund). This suggests that the performance of neighbor trades is not driven simply by systematic sources, such as shared family resources or access to private information in nearby securities, which is consistent with the overlap results reported in Hong, Kubik, and Stein (2005). Moreover, as we would expect, the outperformance is concentrated among neighbor funds with abnormal overlap, as shown in the Internet Appendix.

To rule out the possibility that other confounding effects drive our results, we conduct an alternative performance test using a pairwise regression framework in which we control for the variables that are included in our analysis of fund overlap in Section II.B. In this test, the difference in the coefficient estimates on *Neighbors* between the overlapping buy and sell portfolios is 43 bps and statistically different from zero at better than the 5% level. The estimates in this framework are consistent with those we obtain using the portfolio approach reported in Table X.

Finally, it is important to note that these results do not imply that funds with neighbors outperform funds without neighbors. Rather, the estimates in the table suggest that the investment ideas that managers generate through peer interactions deliver abnormal risk-adjusted returns and are also significantly better than the rest of their trades.

Taken together, the results in this section suggest that the word-of-mouth influence among mutual fund managers likely represents the transmission of value-relevant information, rather than a mere propagation of personal sentiments and biases. The fact that valuable information appears to be shared casts additional doubt on the alternative explanation that similarities in preferences drive the commonalities in mutual fund investments.

B. Stock Characteristics

Having shown that word-of-mouth trades likely reflect information, we next ask whether neighbors establish common positions more often in certain types of stocks. In particular, we focus on various characteristics related to the information efficiency of the stock's price to provide further evidence on the source of shared ideas. Stocks with lower information efficiency provide more opportunities for acquiring an information advantage. Therefore, if neighbors transmit value-relevant ideas among each other, neighbor trades should be more concentrated in these stocks.

To assess how the frequency of initiating new positions is related to stock characteristics, we compute the following probabilities. First, for each quarter t in our sample we calculate the probability that, if fund i acquires a new position in stock k during the quarter, a neighbor fund that does not hold the stock in the previous quarter also acquires a new position in stock k in quarter t or $t+1$. Second, we calculate the corresponding probability using nonneighbor funds. We then average these probabilities across different types of stocks, for example, across those followed by many analysts and those that are not.

The probability of observing a coordinated new position between nonneighbor funds serves as a benchmark in our analysis. This is important because the likelihood that any two managers enter a contemporaneous trade in a widely known stock is unconditionally higher than that for a more opaque security.

Table XI reports the time-series average of the frequency of common new positions by stock type. Column 1 shows the probability that an extensive margin buy in our sample is accompanied by a simultaneous new position by a nonneighbor fund. Column 2 restricts each fund's nonneighbor funds to those located within 50 miles. Column 3 provides the corresponding probabilities for neighbors. The difference in probabilities between columns 3 and 1 is reported in column 4, while the difference between columns 3 and 2 is shown in column 6. Finally, columns 5 and 7 summarize the percent difference in neighbor and nonneighbor probabilities so that the reported averages can be interpreted on a relative basis.

The first row of the table indicates that neighbors are significantly more likely to establish simultaneous new positions across all stocks. The difference in probabilities is statistically significant at the better than 1% level. Perhaps more informative is the percent difference in probabilities reported in columns 5 and 7. When considering all fund pairs, neighbors are 52% more likely to initiate new positions together, while for pairs within 50 miles of each other, the probability is approximately 20% higher.

In the remaining rows of the table we split stocks based on various measures of visibility. High (low) sales, advertising, and analyst coverage stocks are those whose corresponding characteristic is greater (less) than the sample median in a given year. Since these individual characteristics are often missing for many stocks in our sample, we also combine them and classify a stock more generally as "hard-to-research" if it has at least one of the characteristics of low sales, low advertising, or low analyst coverage. Finally, the last three lines of the table describe shared extensive margin buys for stocks that are included and those that are not included in the S&P 500 index, respectively.

Consistent with our expectations, both neighbor and nonneighbor funds are less likely to initiate common new positions in more opaque stocks. For instance, among nonneighbor fund pairs, the frequency of observing simultane-

Table XI

Social Interactions, New Positions, and Stock Characteristics

The table describes correlated extensive margin buys among the fund pairs in our sample by various stock types. High (low) sales, advertising, and analyst coverage stocks are those whose corresponding characteristic is greater (less) than the median of the sample in a given year. Hard-to-research stocks are stocks that have at least one of the the following criteria: low sales, low advertising, or low analyst coverage. Finally, the last few lines of the table describe shared extensive margin buys for stocks that are included and those that are not included in the S&P 500 index, respectively. Column 1 shows the mean probability that, if fund i acquires a new position in stock k in quarter t, a nonneighbor fund that does not hold the stock in quarter $t-1$ also acquires a new position in stock k in quarter t or $t+1$. Column 2 restricts nonneighbor funds to those located within the same media market as fund i. Column 3 provides the corresponding average probabilities for neighbors. The difference in probabilities between columns 3 and 1 is reported in column 4, while the difference between columns 3 and 2 is shown in column 6. Finally, columns 5 and 7 summarize the percent difference in neighbor and nonneighbor probabilities. The average probabilities reported in the table represent time-series averages of the corresponding quarterly frequencies. The sample is limited to fund pairs that have no managers in common during quarter t. Significance levels for tests of difference in means are denoted by a, b, c, which correspond to the 1%, 5%, and 10% levels, respectively.

	Prob. of Establishing New Positions Together When						
SameMediaMkt =	0	1	1		Avg. % Diff. in		Avg. % Diff. in
Neighbors =	0	0	1	Diff.	Prob.	Diff.	Prob.
	(1)	(2)	(3)	(3)–(1)		(3)–(2)	
All stocks	3.59	4.45	5.36	1.77[a]	52.26[a]	0.91[a]	19.71[a]
	(0.13)	(0.14)	(0.22)	(0.17)	(5.50)	(0.13)	(2.75)
High sales stocks	3.23	4.00	4.68	1.45[a]	44.46[a]	0.68[a]	15.49[a]
	(0.11)	(0.12)	(0.21)	(0.14)	(4.24)	(0.12)	(2.96)
Low sales stocks	1.42	1.97	2.78	1.36[a]	106.92[a]	0.81[a]	45.70[a]
	(0.07)	(0.13)	(0.24)	(0.23)	(20.67)	(0.19)	(14.16)
Difference					−62.47[a]		−30.21[b]
					(20.56)		(14.92)
High advertising stocks	3.49	4.26	4.80	1.31[a]	37.10[a]	0.54[a]	11.68[a]
	(0.14)	(0.16)	(0.25)	(0.16)	(4.66)	(0.16)	(3.67)
Low advertising stocks	1.86	2.49	3.48	1.62[a]	87.37[a]	0.99[a]	31.40[a]
	(0.08)	(0.14)	(0.34)	(0.33)	(19.45)	(0.29)	(11.28)
Difference					−50.27[b]		−19.72[c]
					(19.69)		(10.93)

(Continued)

Table XII—*Continued*

	Prob. of Establishing New Positions Together When						
SameMediaMkt =	0	1	1		Avg. %		Avg. %
Neighbors =	0	0	1	Diff.	Diff. in Prob.	Diff.	Diff. in Prob.
	(1)	(2)	(3)	(3)–(1)		(3)–(2)	
High analyst coverage stocks	3.21	3.99	4.67	1.47[a]	45.68[a]	0.69[a]	15.80[a]
	(0.11)	(0.12)	(0.21)	(0.14)	(4.15)	(0.12)	(2.78)
Low analyst coverage stocks	1.68	2.15	3.02	1.34[a]	96.66[a]	0.87[a]	41.86[a]
	(0.21)	(0.24)	(0.42)	(0.31)	(22.40)	(0.28)	(14.16)
Difference					−50.98[b]		−26.06[c]
					(21.56)		(13.84)
Easy-to-research stocks	3.32	4.33	5.22	1.91[a]	60.63[a]	0.90[a]	20.37[a]
	(0.11)	(0.13)	(0.21)	(0.17)	(5.75)	(0.13)	(3.02)
Hard-to-research stocks	1.91	2.59	3.66	1.76[a]	105.00[a]	1.07[a]	43.41[a]
	(0.15)	(0.18)	(0.27)	(0.21)	(12.9)	(0.18)	(7.44)
Difference					−44.40[a]		−23.00[a]
					(13.92)		(7.57)
S&P 500 stocks	4.11	5.09	5.73	1.62[a]	39.23[a]	0.65[a]	11.62[a]
	(0.15)	(0.18)	(0.30)	(0.22)	(5.60)	(0.19)	(3.97)
Non-S&P 500 stocks	2.25	2.91	3.33	1.08[a]	50.44[a]	0.42[a]	14.10[a]
	(0.09)	(−0.11)	(0.20)	(0.16)	(7.58)	(0.15)	(4.70)
Difference					−11.20		−2.48
					(8.75)		(5.65)

Table XII

The Performance Effect of Social Interactions: Trades in Hard-to-Research Stocks

This table reports the performance of portfolios based on trades made in hard-to-research and easy-to-research stocks by neighbor and nonneighbor mutual fund managers. Stocks are categorized as "hard-to-research" if they satisfy at least one of the following criteria: low sales, low advertising expense, or low analyst coverage, where these variables follow the definitions described in Table XI. For both hard- and easy-to-research stocks we form four distinct portfolios for each fund-quarter by following the methodology outlined in Table X: a neighbor buy, neighbor sell, nonneighbor buy, and nonneighbor sell portfolio. We then average the returns of each subportfolio in each quarter across the funds in our sample using the dollar assets (TNA) of each fund in the previous quarter as weights, producing value-weighted monthly average returns for each quarter and each subportfolio. Columns 1 and 2 report the risk-adjusted average returns of the hard-to-research buy and sell neighbor and nonneighbor portfolios for our entire sample period, respectively. Risk adjustment is based on DGTW benchmark returns. Columns 4 and 5 report the corresponding results for the easy-to-research portfolios. Columns 3 and 6 describe the difference of the returns of the buy and sell portfolios within the neighbor and nonneighbor portfolios for hard-to-research and easy-to-research stocks, respectively, and column 7 provides the difference-in-difference estimate between the hard-to-research stocks and easy-to-research stocks within the neighbor and nonneighbor portfolios. The third row of the table shows the difference-in-difference between the neighbor and nonneighbor portfolios within hard-to-research and easy-to-research stocks. The sample is limited to fund pairs that have no managers in common during quarter t. Standard errors are reported in parentheses. Significance levels for tests of difference in means are denoted by a, b, c, which correspond to the 1%, 5%, and 10% levels, respectively.

	DGTW-Adjusted Monthly Returns						
	Hard-to-Research Stocks			Easy-to-Research Stocks			
	Buys	Sells	Diff.	Buys	Sells	Diff.	Diff.-Diff.
Neighbor portfolio	0.35	−1.64[b]	1.99[b]	0.24	−0.15	0.39	1.60[c]
	(0.58)	(0.82)	(0.90)	(0.18)	(0.17)	(0.26)	(0.85)
Nonneighbor portfolio	−0.61[c]	−0.25	−0.36	0.09	0.10	−0.01	−0.35
	(0.38)	(0.40)	(0.35)	(0.12)	(0.09)	(0.12)	(0.35)
Diff.−Diff.			2.35[b]			0.40	1.95[b]
			(0.90)			(0.26)	(0.86)

ous extensive margin buys is 3.21% for stocks with high analyst coverage compared to 1.68% for those with low coverage. Therefore, benchmarking neighbors against shared nonneighbor trades is important. Columns 5 and 7 show that neighbors are significantly more likely to trade low visibility stocks together. For example, compared to nonneighbors, neighbors are 61% more likely to initiate simultaneous positions in easy-to-research stocks but more than twice as likely to establish similar positions in hard-to-research stocks. The difference in percentage differences is 44%, significant at better than the 1% level.

Since these findings suggest that neighbor funds are more likely to coordinate trades in opaque stocks, our performance results in Table X should also be stronger for these securities. Table XII summarizes the trade-based performance of neighbor and nonneighbor portfolios for hard- and easy-to-research stocks separately. We find that the long-short strategy of buying hard-to-research stocks that neighbors buy together and selling those that they sell together significantly outperforms its characteristics-based benchmark, delivering a statistically significant abnormal return of 199 bps per month. In contrast, the return on a corresponding strategy in easy-to-research stocks is 39 bps per month and not statistically significant. It is also interesting to note that funds perform significantly better in only those hard-to-research stocks that they trade with their neighbors: their other trades in less visible securities do not deliver superior returns. The corresponding results for each individual stock characteristic are reported in the Internet Appendix, where we find consistent but statistically weaker results.

Finally, while the hard-to-research stock returns of neighbors are remarkably large, it is important to emphasize that these stocks represent a small fraction of the funds' portfolios. Therefore, their overall effect on total fund performance is considerably smaller.

C. Neighbor Trading and Past Performance

Our portfolio performance tests suggest that social interactions represent a channel through which valuable information is transmitted among mutual fund managers. This finding is important in and of itself, but it also casts further doubt on the argument that similarities in preferences are driving the overlap results. In this section, we provide a final test to support our word-of-mouth hypothesis against the preferences alternative.

In particular, we argue that, if similarities in portfolio choices arise from social interactions, neighbor managers who make successful similar trades in the past will be more likely to discuss ideas again in the future, and, as a result, enter into coordinated trades. In contrast, the preferences story has no implications for the probability of trading together conditional on past performance.

To conduct our test, we first create a dummy variable, *PosPastDGTWPerf*, that takes the value of one if the DGTW-adjusted return of the portfolio created from lagged common purchases minus sales of the fund pair is positive, and zero otherwise. We then reestimate the regression described in Table II by adding *PosPastDGTWPerf* and an interaction term between *Neighbors* and *PosPastDGTWPerf* as additional explanatory variables. As in Table II, we use quarterly fund-pair observations in the analysis; however, the sample is restricted to those fund pairs that had a positive trade overlap in the past quarter.

Table XIII reports the coefficient estimates and standard errors from the OLS estimation of measures of portfolio overlap on *Neighbors*, *PosPastDGTWPerf*, and the interaction of the two. The table shows that, consistent with social interactions, past good performance significantly increases both holdings and buy overlap between neighbors.

Table XIII

Neighbor Trading and Past Performance

The table reports the coefficient estimates and standard errors from the OLS estimation of holdings, purchase, and sale overlap on *Neighbors*, *PosPastDGTWPerf*, and the interaction of the two. *PosPastDGTWPerf* is a dummy variable that is one if the DGTW-adjusted return of the portfolio created from lagged overlapping purchases minus sales of the fund pair is greater than zero, and zero otherwise. The analysis uses quarterly fund-pair observations for the sample of fund pairs that had positive overlap in the past. The regressions include the control variables described in Table II. The sample is limited to fund pairs whose funds have no managers in common during quarter *t*. Standard errors, two-way clustered by each fund in the pair, are in parentheses. Significance levels are denoted by a, b, c, which correspond to the 1%, 5%, and 10% levels, respectively.

	% of Overlapping	
Dependent Variable:	Holdings (1)	Buys (2)
Neighbors	0.97[a]	1.23[a]
	(0.37)	(0.46)
Neighbors × *PositivePastDGTWPerf*	0.45[b]	0.54[c]
	(0.21)	(0.31)
PositivePastDGTWPerf	0.09[b]	−0.07[c]
	(0.04)	(0.04)
Controls	Yes	Yes
$AdjR^2$	0.40	0.14
N (thousands)	1,699	1,671

IV. Robustness

A. Alternative Overlap Measures

We perform a number of robustness tests to confirm that our results are not sensitive to a particular measure of overlap. We estimate our baseline models from columns 2 and 6 of Table III and column 5 of Table II using a number of different overlap measures.

First, as an alternative to the holdings measure used to estimate the regressions in Table II, we create a measure using the percentage of overlapping holdings analogous to those used for purchase and sale overlap. Second, we replace our trades-based measures described in equations (2) and (3) with two new measures that incorporate the magnitudes of the changes in portfolio weights rather than just the direction of the trades. In one case, we use the minimum change in weights across the two funds in absolute value conditional on the trades being in the same direction, and zero otherwise. In the second case, we adjust the weight changes to account for capital appreciation before choosing the smaller of the two weight changes, as before. Third, the sale and purchase overlaps based on extensive and intensive margins in Table IV can also be viewed as overlap alternatives. Our main results are qualitatively unchanged regardless of which measure of overlap we use, as shown in the Internet Appendix. Finally, in the Internet Appendix we also examine round-trip trade overlaps that involve entering and subsequently exiting a common position.

We show that neighbors are significantly more likely to liquidate a common position in the same quarter if they also established the position at the same time.

B. Alternative Standard Errors

Despite the fact that we two-way cluster standard errors by each fund, a potential concern is that, due to the large sample size, any result will appear significant, regardless of its practical relevance. As we argued previously, the magnitude of our results is also economically large, which mitigates this concern, but we provide additional analyses in this section for robustness.

Our first approach is a bootstrap procedure. We rerun the regression in column 5 of Table II, but impose the null of no neighbor effect by randomizing neighbors. In particular, if a manager-pair observation is in the same media market ($SameMediaMkt = 1$), we randomly assign the pair to be neighbors with probability 3.15%, which gives the same proportion of neighbors as in the sample. This allows us to randomly treat the manager pairs only with respect to their neighbor status and leave all other characteristics unaltered. We conduct 5,000 such simulations.

Figure 5 plots the distribution of the *Neighbors* coefficient based on the 5,000 simulations. The figure shows that our point estimate of 99 bps (column 5 of Table II) lies well to the right of the entire mass of the distribution under the null, and is more than six standard deviations above the mean.[17] Moreover, the bootstrap distribution has a standard deviation of 14.5 bps, which is about half the size of the standard error reported on *Neighbors* in column 5 of Table II. The results thus indicate that the standard errors on the initial regression estimates are conservative.

In our second approach, we assess the standard error of the *Neighbors* coefficient under alternative model specifications. In particular, we reestimate our regression using the Fama and MacBeth (1973) procedure. The coefficient estimate on *Neighbors* is 101 bps under the Fama-MacBeth method with a standard error of 11 bps, which is only about 38% of the standard error reported in column 5 of Table II.

C. Subsample Analysis

Additionally, we perform subsample analyses to test the robustness of our results. The results are tabulated in the Internet Appendix. When we exclude the largest mutual fund cities of New York and Boston, our main results are unchanged for all three measures of overlap. When we limit the sample to fund pairs that are located in the same city, again our results are not altered and, if anything, are stronger. Note that this reduces the number of observations for

[17] The mean is slightly positive, perhaps because some managers who really do know each other are classified as neighbors under the null. If we knew for certain which managers knew one another we could do a better job of imposing the null.

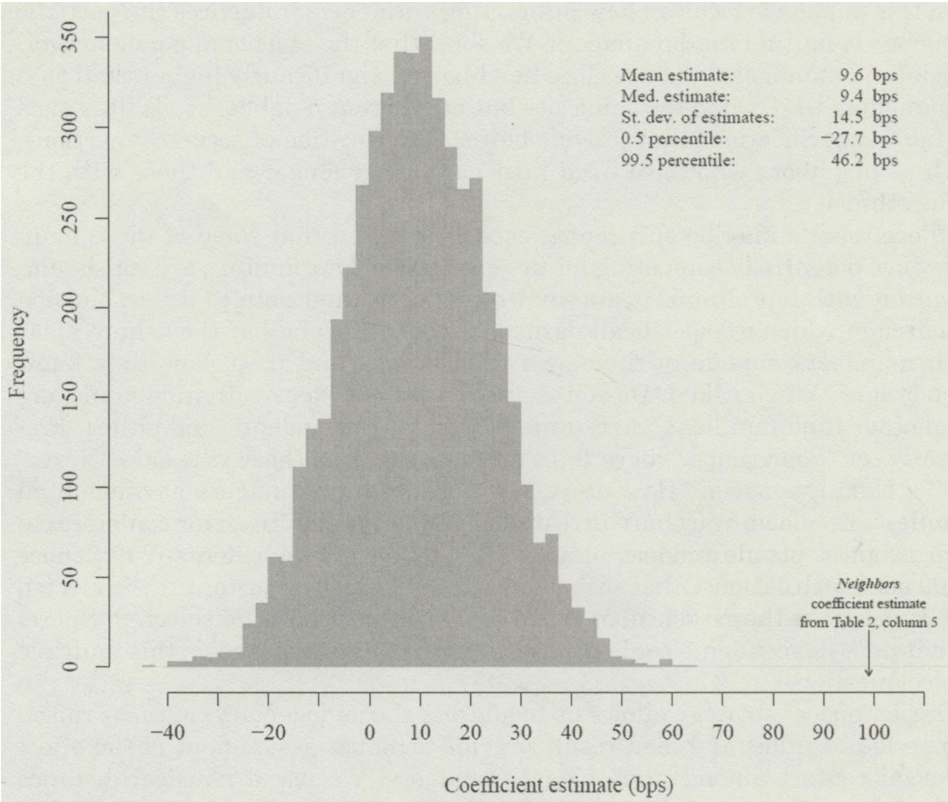


Figure 5. Bootstrap analysis. The figure shows the distribution of *Neighbors* coefficient estimates from 5,000 bootstrap simulations of the regression equation in Table II, column 5. We impose the null of no neighbor effect by randomizing who is a neighbor. If a manager-pair observation is in the same media market (*SameMediaMkt* = 1), we randomly assign the pair to be neighbors with probability 3.15%, which gives the same overall proportion of neighbors as in the sample.

holdings overlap from 4.1 million to 0.3 million. If we constrain the sample to managers who live within 50 miles of one another and whose funds are located in the same city, the results are even stronger than those reported in the paper despite the fact that the number of observations falls to 133,000.

V. Conclusion

A large literature in economics investigates the influence of social interactions on various economic outcomes. A small number of these studies focus on whether word-of-mouth communications affect investment behavior. Establishing causality is particularly challenging as it is difficult to disentangle the impact of social interactions from those of unobservable community effects and similarities in preferences.

In this paper, we focus on how interacting with peers influences the portfolio decisions of mutual fund managers. We show that the abnormal portfolio overlap of fund managers who are close neighbors is significantly higher than that of managers who live in the same city but in different neighborhoods. It is even higher in specifications that provide better identification of social connections, such as neighbors who have lived near each other longer and those with the same ethnicity.

These results may be surprising, especially given that some of these managers are potentially competing for investor funds. Our findings are consistent, however, with a preliminary survey we conducted and plan to use in ongoing research in which we specifically asked respondents whether they share ideas with managers outside of their own fund family, and if so, how they know each other.[18] When asked "Do you share investment ideas with fund managers from other fund families?" Sixty-nine percent of respondents said either "Frequently" or "Sometimes," more than twice as many as those who said "Never" (31%). Next, we asked "How do you know the fund managers at other fund families with whom you share investment ideas?" While "Investor conferences" was the most popular choice, almost one-quarter of respondents (24%) chose "Live near each other." Other social activities were also important: 15% selected "Children attend same school or youth organization," and 11% selected "Sports activities." (Respondents could choose more than one response on this multiple choice question.)

Our empirical strategy allows us to address the endogeneity concerns raised in previous studies and as a result provide a robust assessment of the effect of social contact among professional managers. We use a physical distance measure that creates variation within cities to dismiss the concern that similarities between the investment behavior of neighbor managers arise solely from the common community effects of living in the same city. This approach raises the concern, however, that the closeness of managers' homes proxies for the closeness of their preferences, which in turn drives the overlap in their portfolio choices. We therefore provide a battery of tests to address this issue. These tests show that: (1)in the period immediately prior to two managers becoming neighbors, their holdings and trades are not similar; (2)overlap is increasing in the length of time that two managers have been neighbors, and it takes a few years for significant overlap to exist; (3)controlling for home and neighborhood characteristics that are likely to be related to preferences does not significantly alter the *Neighbors* coefficient estimate; (4)indirect neighbors also have abnormal overlap in holdings and trades, despite not selecting into the same neighborhood; (5)neighbor portfolios earn positive returns, especially in hard-to-research securities; (6)neighbors are more likely to simultaneously initiate new positions than are nonneighbors, especially in hard-to-research

[18] Using an industry connection, we invited 1,196 mutual fund managers and analysts of U.S.-based actively managed equity funds to answer an online survey. We received 98 responses, for a response rate of 8.2%, which is on par with typical surveys in social sciences. We thank Kenneth Weakley for enabling us to conduct the survey.

securities; (7)neighbors trade more in the future if they experienced positive performance on shared trades in the past; and (8)scaling distance by population density increases our estimate of the effect of social interactions by over 50%.

Therefore, while we cannot exclude the possibility that our neighbor proxy captures some elements of preferences, it would be difficult for a preferences-based alternative to explain all of these findings.

Our analysis of the performance of trades common to neighbors also provides an important contribution to the literature. Hong, Kubik, and Stein (2005) argue that understanding the word-of-mouth effect among professional money managers is interesting primarily because of its potential effect on asset prices. Recent theoretical work in asset pricing finds that market efficiency is generally improved by information sharing among investors.[19] It is therefore important to know whether social networks facilitate the transmission of information or just propagate noise or biases. We provide the first direct evidence that the word-of-mouth influence among mutual fund managers likely represents the transmission of value-relevant information.

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[19] See, for example, Malinova and Smith (2006), Colla and Mele (2010), Ozsoylev and Walden (2011), and Han and Yang (2013).

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Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's website:

Appendix S1: Internet Appendix.