The People in Your Neighborhood: Social Interactions and Mutual Fund Portfolio Choice*

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October 6, 2012

Abstract

We find that socially connected fund managers have more similar holdings and trades. The portfolio 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 are neighbors longer or are of 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 social interaction from community effects.

*We thank Kenneth Ahern, Matt Billett, Martijn Cremers and Ryan Israelsen as well as seminar participants at the Early Career Women in Finance Conference, the IU-Notre Dame-Purdue Summer Symposium, the University of Toledo, and brown bags at The Ohio State University and Indiana University for helpful comments.

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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 has found 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 humans are, as Aristotle famously noted, social animals, so perhaps fund managers also trade stocks that they learn about from other managers. Exploring this channel is important, as a large theoretical literature suggests that the social transmission of information by peers can play a role in determining asset prices.[1]

While numerous papers examine the effects of social interaction on choices in other domains,[2] there is little empirical evidence on how word-of-mouth communications 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.[3] Although these results are consistent with the hypothesis that professional money managers transmit investment ideas socially, the authors point out 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 common local media outlets, being visited by the same corporate executives during investor-relations road shows, or by geographic segmentation of the job market combined with career concerns (Scharfstein and Stein, 1990; 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.

[1] Recent theoretical papers include Han and Hirshleifer (2012), Han and Yang (2011), and Colla and Mele (2010). See also the empirical work by Shive (2010), or Hirshleifer and Teoh (2009) for a literature survey.

[2] 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 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.

[3] 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) found that both individual and institutional investors reported that their portfolio choices were partially driven by interpersonal communication.

Of course, both channels can operate simultaneously, and in this paper we implement a test that allows us to disentangle the two effects. To identify potential person-to-person relationships, we use public records data to collect the complete residential address history of fund managers in our sample, which enables us to determine the pairwise distance between the homes of all managers. We argue that managers who live near one another ("neighbors") are likely to come into direct contact. Managers could meet, for example, at a neighborhood park or school, or while taking the train to work. 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 have documented 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. In order 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 fund holdings and trades, 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 the 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 13% higher in our baseline model than that of funds whose managers live in the same city but are not neighbors—even after controlling for fund families and investment styles. This increases to 35% when we implement a cleaner test by focusing on funds with just one manager. We find similarly strong results for trades.

To better identify social networks within neighborhoods, we also collect information on manager characteristics. Commonality in these characteristics is likely to increase the probability that two managers interact socially. For example, common ethnic backgrounds could increase the likelihood of meeting at a church or cultural event, or that their children attend the same school or youth organization. Our main results are even stronger when managers have the same ethnic background or have lived near one another longer.

Our results are economically large. The abnormal overlap for neighbors is about the same as it is for funds that are part of the same fund family, which share analysts. Moreover, the neighbor effects are about twice as large as the effect of being in the same city (Hong et al., 2005). But despite the size of the estimates, they are likely to *understate* the true magnitude of the effects of social interaction for two reasons. First, while our neighbor measure is a good proxy for whether managers know one another, it is clearly noisy. If, for example, only half of the managers who we classify as neighbors actually interact socially, the true effect would be twice the size of our estimates. Second, managers and other investors have many social networks that we do not capture in the paper.

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

We conduct several tests to reject this "preferences" alternative. First, our main identification strategy exploits changes in the residences of managers. If an individual's preferences are generally stable through 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.

In another test of the preference story we rely on the observation that managers are limited in their home choices by the supply of real estate; people can only buy homes that are for sale. People with overlapping preferences may choose to live in the same residential area, but over small distances, housing market frictions induce random variation in fund managers' residential locations. The distance between managers' homes is therefore unlikely to decline monotonically with the closeness of their preferences. In contrast, the social interaction argument suggests that the closer two people live to one another in a given area, the more likely it is that they will meet. Our results show that distance matters even within very small areas: overlaps in portfolio holdings and trades are much higher when managers live within one (population-adjusted) mile than when they live between one and five miles apart.

In contrast to Hong et al. (2005), we also perform a direct test of whether social influence among professional investors represents the transmission of valuable information, or if managers are merely sharing personal sentiments and biases with each other. We address this question by investigating the performance of overlapping stocks held and traded by neighbors.

We divide each fund's portfolio into holdings that overlap with those of a neighbor and the remainder. We then examine whether subsequent performance is better in holdings that are shared with neighbors. Similarly, we evaluate the performance of a strategy of buying stocks that represent simultaneous buy trades of the managers and their neighbors and selling short the portfolio of stocks that neighbor managers sell together. These tests indicate that social interactions create valuable information transfers between fund managers. In particular, while the sub-portfolio of holdings that are shared across the neighbors does not outperform the other holdings of the fund, the long-short strategy of neighbor trades yields a statistically significantly positive abnormal return of approximately 6% per year. When we exclude the financial crisis, our estimates are around 7% per year. Our results are similar in magnitude to the estimates reported by Cohen et al. (2008) of the abnormal returns generated from information shared between executives and fund managers.[4] The finding that valuable information appears to be shared casts additional doubt on the preferences alternative.

[4]Cohen et al. (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.

The social transmission of value-relevant information is particularly interesting in our setting given that the sharing is among potential competitors. There are several possible explanations for this phenomenon. First, since we don't 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. Similarly, managers may also openly announce their information to induce coordinated trading among their peers in order to mitigate synchronization risk (Brunnermeir and Abreu, 2002). 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. Third, managers may have an expectation of *quid pro quo*, so 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.

1 Data and sample construction

We combine several data sources in this study. First, we draw 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 where 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 the 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 2558 funds and 4622 managers, with an average of 810 funds per quarter.

To focus on funds for which we can properly control for fund 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.

1.1 Pairwise distances

Our goal is to identify pairs of managers who have homes close to each other. We follow the method of Yonker (2010) and Pool et al. (2012) to identify fund managers in the LexisNexis Public Records database using name, age, and location searches.[5] 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 2042 managers in the database of the 4622 managers in the initial sample. Our final sample includes 422 funds, or an average of just over 70,000 fund-pairs per quarter. The sample coverage is very similar to Pool et al. (2012), who show that their sample is representative of the broader sample of actively-managed mutual funds, covering over 80 percent of actively managed mutual funds by assets under management.

Whenever possible, we identify the addresses (including vacation homes) of each manager at the beginning of each quarter. We then calculate distances between each pair of managers using the latitude and longitude of the centroid of each home's zipcode.[6] 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* $\leq$ 5 miles. This includes all pairs of managers

[5] See Pool et al. (2012) for a detailed explanation of the matching procedure.

[6] 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,

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

where $\Delta\lambda = \lambda_2 - \lambda_1$. This formula is known to work well for both short and long distances.

who live in the same zip code, as $zipdistance = 0$ for these pairs. We identify 281,605 such pairs, and for each one we use an online driving directions tool to record the precise travel distance between the two addresses.[7] 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 clearly 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.[8] The normalized driving distance between manager 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 903.8 households per square mile. The calculation implies that one normalized mile is just 60 feet on the Upper East Side of Manhattan, while in New Canaan it is 2.9 miles. It is approximately one mile in the Boston suburb of Wellesley, or in Colorado Springs, CO.

1.2 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 will classify managers as neighbors. We do this by first creating another dummy variable, $DistBtwnHms1Mile$,

[7]We only calculate driving distance for potentially close pairs because the precision of the distance measure matters more for short distances: since we will 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 tens of 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.

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

that takes a value of one when managers live within one (non-normalized) mile of each other. As shown in Table 1, this condition is true in 0.37% of our observations.

We then choose a threshold for the normalized distance measure, NDD, that has exactly the same mean. This threshold turns out to be 2.9 normalized miles, so we set the $Neighbors$ dummy to take a value of one if two managers live within 2.9 normalized miles of each other. 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 175 feet), but managers in New Canaan can live up to 8.4 miles apart.

The neighbor cutoff is of course somewhat arbitrary, but robustness tests confirm that our 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. While the same number of managers are classified as neighbors by the $DistBtwnHms1Mile$ and $Neighbor$ dummies, different managers are classified as neighbors under each measure—the $Neighbor$ dummy includes more managers from less-populated areas, where the probability of knowing each other is higher. As we show in the next section, it turns out that the $Neighbor$ dummy has considerably more power to detect portfolio correlations than the $DistBtwnHms1Mile$ variable.

1.3 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.[9] Each zip code begins as its own cluster, and zip codes—and then 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.

[9]We do this rather than using the name of the city in which a zip code is situated because we want to include suburbs in each city grouping.

The locations of these cities are shown in Figure 1. 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. 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 include these managers in our sample to provide information about typical holdings and trades.

2 Do social interactions influence fund holdings and trades?

We begin our empirical analysis by testing whether social interactions influence the holdings and trades of mutual funds. We do so by creating portfolio overlap measures in holdings and in trades between fund pairs and testing whether these measures are greater for fund pairs with managers who are neighbors. This pairwise methodology creates a large number of observations since the number of unique pairs of N funds is $N(N-1)/2$. Given the pairwise structure of the data, we are careful to calculate standard errors in a way that allows repeated observations of a firm to be correlated. In particular, our observations are not independent since we calculate $N-1$ overlaps for each fund in each quarter. A common component in each of these observations is the portfolio of that fund. Moreover, fund portfolios are persistent over time. While this correlation does not bias the coefficient estimates, it will underestimate the standard errors. We therefore report standard errors that have been two-way clustered by each fund in the fund pair.

The average number of funds in the sample each quarter is 422 and our analysis includes 60 quarters, so we should have over 5.3 million quarterly fund-pair 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 member is a large-cap fund and the other is a small-cap fund. This reduces the sample to approximately 4.2

million quarterly observations. Notably for the standard errors, however, the number of funds used in the clustering calculation is 1699.

2.1 Social interactions and holdings

We first investigate whether the overlap in holdings is greater for funds whose managers are neighbors. 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 during t. We aggregate the overlap measure to the fund level since conducting the analysis at the stock level would lead to well over a billion observations.

Using this measure of overlap, we estimate the regression

$$\begin{aligned} PortOverlap_{i,j,t} = {} & \beta Neighbors_{i,j,t} + \delta SameMFCity_{i,j,t} \\ & + \gamma SameMediaMkt_{i,j,t} + \mathbf{\Gamma}'\mathbf{Controls}_{i,j,t} + \epsilon_{i,j,t}, \end{aligned} \tag{2}$$

where $Neighbors_{i,j,t}$ is a dummy variable that is one if at least one manager from fund i is a neighbor (as defined in Section 1.1.2) of a 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 (defined as in Section 1.1.3 but using mutual fund company addresses), $SameMediaMkt_{i,j,t}$ 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.

We include the following controls: dummy variables that are one if funds i and j match on Morningstar size or value/growth categories ($BothSmallCap$, $BothMidCap$, $BothLargeCap$, $BothValue$, $BothGrowth$, $BothBlend$), the absolute value of the difference between the total net asset-based quintiles of funds i and j ($TNAQuinDiff$), the average TNA-based quintiles of funds i and j ($TNAQuinAvg$), and 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 ($MngOtherFundTogether$), and funds i and j have at least one manager in common ($CommonMgr$). In most of the subsequent analyses we exclude observations where 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 having a manager in common.

If social interactions influence mutual fund managers' portfolio choices, then β in equation (2) should be should be positive. The estimate of β is the incremental increase in portfolio overlap from managers being residential neighbors beyond the influence of local media and what Hong et al. (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 1 reports the means and standard deviations of the variables used in the analysis. The table shows that the average portfolio overlap between two funds in the sample is 8.03% and that 0.37% of fund pairs have at least one manager pair that are neighbors. While this may appear to be a small number of neighbors, about 44% of funds in each quarter have at least one manager who is a neighbor of a manager at another fund, so the results are not driven by just a few funds. The table also shows that 10.46% of fund pairs have at least one manager pair that lives within 50 miles of one another, 7.12% of fund pairs are located in the same city, and 2.62% of fund pairs are from the same family. There is also a great deal of variation in our dependent variable, the standard deviation of $PortOverlap$ is 9.43%.

Table 2 displays the coefficient estimates and standard errors for various forms of equation (2). Standard errors are two-way clustered at each fund's level in the fund pair. In column 1 we include only $SameMFCity$ and the control variables. This regression gives us an estimate of the magnitude of the Hong et al. (2005) results in our empirical framework. The coefficient on $SameMFCity$ is 65 basis points (bps) and is significantly greater than zero. Given the average overlap in holdings in the sample of 8.03%, this implies that the portfolio overlap of funds located in the same city is about 8% greater than funds located elsewhere.

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 10.42% in this case, implying that for the average fund pair the portfolio overlap is over 125% higher if they have 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 211 bps, implying that the overlap between these funds is about 26% greater. Finally, funds that belong to the same fund family also have higher overlaps. This abnormal commonality in holdings is 103 bps and may be due to common research departments or work-related interactions.

In column 2 we add a measure that captures whether the fund managers are neighbors based on raw driving distance. For a given fund pair, the variable $DistBtwnHms1Mile$ is a dummy variable that takes the value of one if at least one of the managers of 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 88 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 88 bps higher overlap. Since the magnitude of $SameMFCity$ is 64 bps, 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 $64 + 88 = 152$ 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 1, 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 $Neighbors$. It is apparent that we are choosing neighbors much more appropriately using the normalized driving distance measure. The coefficient estimate on $Neighbors$ is 134 bps, 1.5 times that on $DistBtwnHms1Mile$.

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 38 bps and is statistically significant. Its inclusion in the regression reduces the magnitudes of both $SameMFCity$ and $Neighbors$, but of the three, the magnitude of $Neighbors$ is still the largest at 105 bps.

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 108 bps, 49 bps, and 38 bps, respectively. This implies that two funds that are located within the same city with at least one pair of managers who are neighbors will have 195 bps greater overlap than the typical fund pair. This is equivalent to over $195/803 = 24\%$ greater commonality in holdings. The coefficient estimates decompose this effect into social interactions and broad local effects, the former representing over half of the total magnitude.

To put the influence of having fund managers who are neighbors in perspective, we can compare the coefficient estimates on $Neighbors$ to that on some of the other control variables. For instance, two funds from different families located in the same city with managers who are neighbors will have a comparable overlap in holdings to two funds in the same fund family. Additionally, the effect on fund holdings from social interactions is 57% of the effect of two funds having managers who manage another fund together. Of course, we cannot be sure that all neighbors in our sample actually know one another. If only half the managers that are identified as neighbors actually know one another, and we were instead able to estimate the regression with only the managers who do know one another, 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.

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 are willing to share information about their holdings.

Finally in column 7, we estimate equation (2) 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. We find that this is the case. The coefficient estimate on $Neighbors$ is 280 bps, statistically significant at the better than 1% level. The estimate suggests that the effect of social contact on portfolio overlap is remarkably large; it leads to 35% greater overlap than that of the the average fund pair in the sample.

2.2 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 non-neighbors. In order to do so we construct measures of overlap in both purchases and sales. We measure overlap in stock purchases and sales as:

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

$$SaleOverlap_{i,j,t} = \frac{\sum_{k\in\mathcal{T}_t} \min\{I^-_{i,k,t}, I^-_{j,k,t}\}}{\min\{\sum_{k\in\mathcal{T}_t} I^-_{i,k,t}, \sum_{k\in\mathcal{T}_t} I^-_{j,k,t}\}}, \tag{4}$$

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 is 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 is 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 measures work, suppose that fund i makes 100 trades during quarter t and that 40 trades are buys and 60 are sales. During the same quarter fund j executes 50 trades, 30 of which are buys and 20 are sales. Assume further that funds i and j buy 10 stocks in common and sell 5 stocks in common. In this case $BuyOverlap_{i,j,t} = 10/30 = 33\%$ and $SaleOverlap_{i,j,t} = 5/20 = 25\%$.

In Table 3 we test whether social interactions affect the trading behavior of mutual fund managers by estimating regression equation (2) using the purchase and sale overlap measures defined in equations (3) and (4), respectively, as the dependent variables. We estimate the regressions using four different specifications for both purchases and sales: the full sample of fund pairs, the sample excluding fund pairs with common managers, the sample excluding fund pairs within the same fund family, and fund pairs managed by single managers. The results are similar to those of Table 2; 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 2) indicates that fund pairs with managers who are neighbors have 119 bps more purchase overlaps than those that live within 50 miles of one another. Given that the average purchase overlap is 851 bps between fund pairs, this suggests that funds with managers who are neighbors have 14% greater commonality in trades than do funds whose managers live within 50 miles of one another. The most striking results are for the sample of single manager pairs. The coefficient estimate on $Neighbors$ is 481 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 63 bps in the baseline model (column 6) is statistically significant at the 5% 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 by either 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, we decompose purchases and sales into extensive and intensive margins. We use an approach analogous to that in equations (3) and (4) 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 extensive margin is equal to our original overlap value. The results are summarized in Table 4. The table confirms that for buys, both extensive margin and the intensive margin overlap measures are statistically significant, but for sales the $Neighbors$ coefficient is significantly positive only for extensive margins.

2.3 Social interactions vs. preferences

Although 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 measuring similarities in manager preferences and not the likelihood of social contact. This means that 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 on many dimensions including wealth, age, political affiliation, and ethnicity. There is empirical evidence that political values, for example, affect the portfolio choices of mutual fund managers (Hong and Kostovetsky, 2012). In this section we address this alternative explanation.

Our first test of the "preference 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.[10] To test this, we create two additional variables, $FutureNeighbors$ and $PastNeighbors$. $FutureNeighbors_{k,l,t}$ is a dummy variable that is one if managers k and l are neighbors for some $s > t$ but are not neighbors for

[10] Our test relies on the traditional economic assumption that preferences over investment behavior are stable through time. Recent empirical evidence supports this view: Barnea, Cronqvist, and Siegel (2010) show that about one-third of investment behavior is determined by genetics alone. Cesarini, Dawes, Johannesson, Lichtenstein, and Wallace (2009) and Cesarini, Johannesson, Lichtenstein, Sandewall, and Wallace (2010) find similar results for risk-taking. See also Andersen, Harrison, Lau, and Rutström (2008).

$s \leq t$. $PastNeighbors_{k,l,t}$ is a dummy variable that is one if managers k and l are neighbors for some $s < t$, but are not neighbors for $s \geq t$.

We add these variables to the regression in equation (2) with one change. Instead of using fund-pair observations we use manager-pair observations. We do this because relocations are unique to managers and not funds. We also use this specification later when investigating how the demographics of neighborhoods and characteristics of managers affect our variables of interest. 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 will show later, our estimates and standard errors using the manager -pair specification are nearly identical to those in the collapsed fund-pair specification.

In Table 5 we report estimates of

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

where $Overlap_{i,j,t}$ is the holding or trade overlap between funds i and j at time t, and manager k is a manager of fund i, while manager l is a manager of fund j. Note that the $Neighbors$ variable is also manager-specific in this specification.

If overlapping preferences drive the commonality in trades and holdings between neighboring managers, then the estimate of β_f should be positive and significant. The table 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 113 bps, 111 bps, and 56 bps for overlap in holdings, purchases, and sales, respectively. Interestingly, 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 purchases and sales.

As a second test of the preference 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 will pick up whether a manager who lives in a highly religious area of Ohio is likely to invest similarly to a peer who lives in highly religious area of California.

Specifically, we control for the effects of common religious beliefs and similar wealth by including five additional variables in our regression specifications. $BothReligiousAreas$, $BothJewishAreas$, 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.[11]

For each measure of overlap, Table 6 reports the results from the baseline regression and the regression including the additional demographic control variables using our quarterly manager-pairs 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 52 bps to 11 bps after including the demographic control variables and for purchase overlap the coefficient estimate falls from 76 bps to 33 bps.

Our final test of the preference story is based on the idea that managers are not perfectly mobile in their home choices. Preferences may drive a manager to live in a certain suburb or area of a city. The manager may even find the perfect home that matches his preferences, but if the home is not for sale he must choose another home. Therefore, people with the most similar preferences will live within a small area, but within that area they will be allocated based on the supply of real estate at the time they moved to the area. The social interaction story however suggests that the closer two

[11]County-level religious data are from the U.S. Church Membership Data from the Association of Religious Data Archives (`www.thearda.com`). Zip code-level median home prices are from the 2000 census.

people live to one another, the more likely it is that they will have social contact, and this is true even within very small areas.

We therefore estimate our baseline regression, but add dummy variables that indicate if funds have managers who live within one normalized mile, one to five normalized miles, five to 10 miles, 10 to 25 miles, and 25 to 50 miles. The results are displayed in Figure 2. 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. In fact the abnormal overlap in purchases for the sample of single managers is 788 bps for managers who live within one mile, while it is 220 bps for managers who live between one and five miles of each another. Thus in order to favor the preference story over the social interaction story one must believe that the preferences of managers who live within one mile of each other are over 3.5 times more similar than managers living between one and five miles of one another. Given the frictions in the real estate market and that mutual fund managers are certainly a more homogeneous group than the general population, this would be very surprising.

2.4 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 et al., 2000), so we expect that when the network quality is higher, the manager pair has a greater commonality in their trades and holdings. We test this by interacting $Neighbor$ with various manager-specific characteristics. The characteristics that we investigate include neighbor tenure, age, ethnicity, portfolio management experience, and college attendance.

Specifically, $NeighborsTenure$ is the number of years two managers have been neighbors, $SimilarAge$ 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.[12] *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. *SameCollege* is a dummy variable that is one if both managers attended the same college or university. For the remainder of the paper we only display the results for holding and purchase overlaps since sale overlaps convey a similar picture and are less informative because commonality in sales is conditional upon both funds holdings the stock.

The results of the tests are summarized in Table 7. The results in the first column show that managers who are neighbors longer have greater overlap in holdings and purchases. For each year that managers are neighbors, their portfolio (purchase) overlap increases by 19 (25) bps. Not only is this result intuitive if the *Neighbors* dummy is capturing the likelihood of social interactions, but it also casts additional doubt on the preference story since the length of time two managers live near each other is unlikely to increase the similarity in their preferences.

The analysis in the remaining columns of the table show 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 a higher overlap in both holdings and purchases though the coefficients are 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 373 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 in the paper as to what type of information is spread through social interaction. Is value-relevant information spread through social contact or is it merely irrationally exuberant information? 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. The greatest overlap occurs in funds where both managers are experienced. In these cases the abnormal holding overlap is

[12] We use a classification algorithm developed by Ambekar, Ward, Mohammed, Male, and Skiena (2009) to classify names into one of thirteen 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 a 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.

112 bps more than when one manager is experienced and the other is not. For purchases, the abnormal overlap among neighbors only occurs when both neighbors are experienced portfolio managers. We return to this issue in the next section.

2.5 Stock characteristics

Having established that social interactions affect the holdings and trades of mutual fund managers, it is interesting to test whether there are distinct patterns in the types of stocks in which the influence of social interactions are greatest. Thus we create measures of overlap in holdings and purchases for subsets of different types of stocks. These overlap measures are created analogously to the overlap measures in equations (1) and (3). For example, the holdings overlap for S&P 500 stocks is computed as in (1) with the additional condition that $k \in S$, where S is the set of stocks that are included in the S&P 500 index. Similarly, the holding overlap for non-S&P 500 stocks is subject to the constraint that $k \notin S$. The sum of these overlap measures is then equal to the total overlap in holdings between funds in the sample. Notice that in Table 8 the average overlap of S&P 500 stocks is 6.75%, while the overlap in non-S&P 500 stocks is 1.22%, the sum of these is roughly equal to the total holding overlap in the sample of 8.03%.[13]

In Table 8 we report coefficient estimates and standard errors for $Neighbors$, $SameMediaMkt$, and $SameMFCity$ from the baseline version of equation (2) for overlaps in purchases and holdings of different types of stocks. We also report the average overlap in the sample for each overlap measure so that the coefficient estimates can be interpreted on a relative basis. In the first two rows we report the estimates for local and non-local stocks. Coval and Moskowitz (2001) find that mutual fund trades in local stocks are informed. Ivković and Weisbenner (2007) find that individual investors are much more sensitive to the trades in local stocks of peers within 50 miles. Since there is evidence that local investors are informed investors, they interpret this as evidence that information diffusion effects are informed. We find similar results for mutual fund managers. Mutual fund pairs managed by managers who live within 50 miles of one another have 25% higher overlap in local stocks than the average fund pair in the sample (22 bps/88 bps). However, that effect is likely driven by the influence of the common media sources or local investor relations and not social contact.

[13]For several of these characteristic overlap measures the sum of the two subsets is smaller than 8.03%. This is due to data availability of the characteristics.

Fund pairs whose managers are neighbors have lower than normal overlaps in local stocks. In fact, the abnormal overlap in local stocks for fund pairs that are neighbors is essentially zero (−23 bps +22 bps).

We next investigate whether the portfolio overlap is particularly high in politically sensitive stocks. Hong and Kostovetsky (2012) find that mutual fund managers who make campaign contributions to Democratic candidates tend to be "closet socially responsible investors". If certain neighborhoods tend to be more "Republican" or more "Democratic" then we might find that the abnormal overlap tends to manifest itself in either politically sensitive stocks or non-politically sensitive stocks. The results show that social interactions only influence stocks that are not politically sensitive, however the politically sensitive stocks are so few in number that the effect of social interactions may be difficult to detect.

In the next several rows of the table we split stocks based on various measures of visibility and/or levels of asymmetric information. Finding that social interactions are stronger among "harder to research stocks" may hint at value-relevant information flowing between managers through this channel. The relative overlap in S&P 500 stocks and non-S&P 500 stocks is very similar at around 13% for S&P 500 stocks and 16% of non-S&P 500 stocks (for both holding and purchase overlaps). For stocks split by analyst coverage, the low analyst coverage stocks have much higher relative abnormal overlap, but again there is so little overlap in low analysts stocks that it is difficult to say that much of the overall commonality in trades is due to stocks with low analyst coverage.

If irrationally exuberant ideas are spread through social interactions, then we might find that overlap is particularly high in lottery-type stocks. We investigate overlap in lottery stocks and find that although the proportional overweighting is much higher in lottery stocks, there is so little overlap in these stocks that it is difficult to draw broad conclusions from this finding.

3 Is value-relevant information spread through social interactions?

Our results thus far show that interpersonal communication with peers plays an important role in mutual fund managers' portfolio decisions. In this section, we examine whether the word-of-mouth influence among these investors represents the transmission of value-relevant information or managers are merely sharing personal sentiments and biases with each other.

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 investors may benefit from coordinating with each other. For example, Froot et al. (1992) show that when some speculators have short-horizons, they may find it optimal if others traded on their information as well. Brunnermeir and Abreu (2002) argue that arbitrageurs face synchronization risk that arises because often a critical mass of traders is required to correct mispricings. In order to minimize this risk, arbitrageurs may openly announce their information in order to induce coordinated trading among their peers.

Additionally, since the cost of sharing information is not likely to be high; the effect on relative performance of letting neighbors in on a few stock picks is probably small. Managers may also have an expectation of *quid pro quo*, so sharing information now 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, we first tabulate the overlap between the holdings of each fund i and all other funds in our sample during the quarter. We define funds i and j as neighbor funds if j is managed by a manager who lives in the same neighborhood as at least one of the managers of fund i. Using this definition, for each stock in fund i's portfolio, we determine whether any of the fund's neighbors also hold the stock. If at least one neighbor fund also owns the stock during the quarter, we place the stock in the "neighbor" portfolio, otherwise, the stock is allocated to the "non-neighbor" ("other") portfolio. We rebalance our neighbor and non-neighbor portfolios each quarter.

To compare the performance of neighbor holdings to those of non-neighbor holdings separately, we calculate neighbor and non-neighbor monthly portfolio returns between holdings disclosures for each fund:

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

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 that are held by at least one of fund i's neighbor funds 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 then aggregate the neighbor and non-neighbor portfolio returns by calculating the weighted average of the returns in equations (6)–(7) across funds at time t, weighting each fund's return by its assets under management (TNA).

In our trading-based portfolio tests, we form portfolios at the beginning of each quarter based on whether fund i buys or sells a given stock, respectively, in the previous quarter. Stocks that are bought form the "buy" portfolio, while those that are sold are placed in the "sell" portfolio. We create two additional subgroups within the buy and sell portfolios. For example, for buy transactions, the first group includes stocks that at least one neighbor of fund i also bought in the previous quarter ("neighbor buy" portfolio), and the second contains all other stocks ("non-neighbor buy" portfolio). We rebalance our portfolios every quarter based on the direction of the fund's trade, and those of its neighbors. For each fund, each quarter, stocks in the portfolios are weighted by the new money they receive during the quarter; however, equal-weighting produces qualitatively identical results. Finally we aggregate the neighbor and non-neighbor buy and sell portfolio returns in each quarter by averaging across funds, using the funds' TNA as weights.

For both holdings and trades, we use the average monthly benchmark-adjusted excess return as in Daniel, Grinblatt, Titman, and Wermers (1997, "DGTW") and Wermers (2005)[14] to assess

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

the performance of the neighbor and non-neighbor 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.[15]

The average returns for the neighbor (R^N) and non-neighbor (R^O) holdings portfolios over the 60 quarters and the difference between these averages are presented in Table 9. The table shows that when we allocate stocks into portfolios based on common holdings, the neighbor portfolios perform no better or worse than the non-neighbor portfolios in our sample. While these results suggest that managers do not appear to 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 Pastor, 2005). Therefore, we next turn to common trades and examine whether these are informative.

Our trade-based portfolio results are summarized in Table 10. For brevity, we only report DGTW benchmark returns using the CRSP universe of common stocks, but our alternative DGTW benchmarks produce very similar results. We first summarize results 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 21 bps per month (2.5% per year). On the sell side, column 2 reveals that the neighbor portfolio underperforms its characteristics-adjusted benchmark by 27 bps per month (3.2% 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 48 bps per month. The risk-adjusted performance measure is statistically significantly positive at the 5% level and implies an annualized above benchmark return of 5.8%. Interestingly, our long-short results are, to some extent, driven 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 32 bps per month (3.8% per year), resulting in long-short returns of 62 bps per month (7.4% per year).

[15] We obtain qualitatively identical results when we assess the performance of the neighbor and non-neighbor holdings and trades portfolios using the Carhart (1997) four-factor model instead.

Columns 4–6 report the corresponding results for the non-neighbor buy and sell portfolios. In contrast to our neighbor portfolios, both non-neighbor buy and sell portfolio returns are positive and very close to zero on a benchmark-adjusted basis, falling between 4 and 16 bps per month (0.5–1.9% per year) in the full sample and the subsample that excludes the financial crisis. Moreover, the long-short strategy that uses non-neighbor 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 and are statistically significant.

Table 10 also reports results for extensive and intensive margin transactions separately. For a mutual trade to be classified as an extensive margin transaction, we require that both funds trade on the extensive margin. As discussed in Section 2, 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 1–3 are very consistent with this previous finding. In particular, the strong subsequent negative performance of the neighbor sell portfolio is entirely driven by extensive margin sales, while on the buy side, we find a symmetrical result.

Taken together, our performance results imply 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 investment.

4 Robustness

4.1 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 3 and column 5 of Table 2 using a number of different overlap measures.

First, as an alternative to the holdings measure used to estimate the regressions in Table 2, 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 (3) and (4) 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. Finally, the sale and purchase overlaps based on extensive and intensive margins in Table 4 can also be viewed as overlap alternatives. Our main results are qualitatively unchanged regardless of which measure of overlap we use.

4.2 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 the 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 2, 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 3.5%. (This gives the same overall proportion of neighbors as in the sample, and is calculated by dividing the mean of $Neighbors$, 0.0037, by the mean of $SameMediaMkt$, 0.1046. See Table 1.) 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 3 plots the distribution of the $Neighbors$ coefficient based on the 5,000 simulations. The figure shows that our point estimate of 108 bps (column 5 of Table 2) lies well to the right of the entire mass of the distribution under the null, and is more than seven standard deviations above the mean.[16] Moreover, the bootstrap distribution has a standard deviation of 13.7 bps, which is about

[16]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.

half the size of the standard error reported on $Neighbors$ in column 5 of Table 2, so the results 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 re-estimate our regression using the Fama and MacBeth (1973) procedure. The coefficient estimate on $Neighbors$ is 105 bps under the Fama-MacBeth method with a standard error of 18 bps, which is only about 70% of the standard error reported in column 5 of Table 2.

4.3 Work-related interactions

It is possible that our results stem from work-related—not social—interactions. If two mutual fund companies are located in the same area of a city, then perhaps managers choose to live in the same suburb for commuting reasons. These managers may never socialize at home, but they may know one another because their offices are near one another. While this would still be consistent with our story that information is shared by managers who know one another, the channel of information flow would be different. We therefore test this by including a dummy variable in our baseline regressions that is one if the offices of the mutual funds are within one mile of one another and the mutual funds are not in the same family. If work-related interactions are driving the main results then the coefficient estimate on this work neighbor variable should be significantly positively estimated. In untabulated results, we find that this is not the case; it does not appear that we are capturing work-related interactions with our measure of social interactions.

4.4 Subsample analysis

Additionally, we perform subsample analysis to test the robustness of our results (untabulated). When we exclude the largest mutual funds 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 holdings overlap from 4.2 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, again the results are even stronger than those reported in the paper despite the fact that the number of observations falls to 137,000.

4.5 Performance excluding local stocks

Although we showed in Table 8 that stock picks transmitted through social interactions do not tend to be local stocks, when investigating performance in the holdings and trades of mutual funds it is important to control for the effect of local stocks, since it has been shown that mutual fund managers make informed investments in these securities (Coval and Moskowitz, 2001). In untabulated results we confirm that the results of Tables 9 and 10 are unaltered by excluding local stocks (those that are headquartered within 50 miles of the fund) from the "Neighbors" portfolio.

5 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 alters the portfolio decisions of mutual fund managers. We use managers' residential addresses to establish driving distances between manager pairs. We show that the portfolio overlap of fund managers who are close neighbors is 13% higher than that of managers who live in the same city but in different neighborhoods—even after controlling for fund families and investment styles—and considerably higher in other specifications that provide cleaner tests.

Using a physical distance measure that creates variation within cities allows us to dismiss the concern that similarities between the investment behavior of neighboring managers arise solely from the common community effects of living in the same city. We decompose the same-city effect documented in previous studies into the effect of social contact, of sharing management company locations, and of sharing local media outlets or other city level information. We show that while the latter two effects are important, their influence is less than half of that of social interactions.

We also address the concern that the closeness of managers' homes proxies for the closeness of their preferences, which, in turn, drives the overlap in their portfolio choices. In particular, we show that "future neighbors"—managers who are not currently neighbors but become neighbors later during the sample period—do not have excessively overlapping holdings or trades. Since neighbors are likely to share many similarities in their tastes and these would have existed before the pair became neighbors, our finding suggests that preferences that determine housing choices do not also drive commonalities in investment behavior.

While our empirical framework allows us to estimate the magnitude of the effect of social interactions on portfolio choice, the true effect of social interactions is likely to be much greater than our estimates suggest. As Seasholes (2010, p. 19) points out, "[t]he difficulty in measuring information diffusion arises because investors' information sets are unobservable." This is certainly true in our empirical framework; there is of course no way for us to confirm that managers who we classify as neighbors have social contact, or even whether they know each other. Thus, the coefficient on our main variable of interest, $Neighbors$, underestimates the true effect of social interactions on portfolio management decisions.

This is evident from the results presented previously in Figure 2, which show that as the distance between managers shrinks, the portfolio overlap between two funds increases. Shrinking this distance can be thought of as increasing our confidence that two managers know one another socially. So, while on average our baseline regressions estimate that social interactions lead to an increase in portfolio overlap of about 13% (108/803), as we become more confident about the information sets of managers by reducing our social distance to one normalized mile the effect increases to over 26% (208/803). When we consider only single managed funds to remove the distorting effects of multiple managers, our estimates suggest that social interactions account for an additional 380 bps in overlap or 47% (380/803) more than that of the average fund pair. These effects are even larger when the managers are neighbors longer or have the same ethnic background, further increasing our confidence that two managers interact socially.

Our results suggest that social interactions with peers have a substantial influence on professional managers' portfolio choices. Several recent theoretical asset pricing papers model information sharing across investors.[17] These models predict that social communications between investors will improve market efficiency.[18] This prediction comes with the caveat that actual information, and not noise, is spread through social interactions. We provide the first direct evidence 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 or biases.

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

[18] This is true as long as information is exogenous to the model. Han and Yang (2011) allow information acquisition to be endogenously determined and show that when the cost of acquiring information is high, increased social communications can lead to less efficient markets.

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Table 1

Summary statistics

The table reports summary statistics for the various overlap measures, the measures of social interactions, and control variables used in the analysis. Variable names appear in italics. The sample includes 5.6 million quarterly fund-pair observations from the first quarter of 1996 through the fourth quarter of 2010.

	Mean	St. Dev.	N (000's)
Dependent variables			
% of overlapping holdings	8.03	9.43	4,221
% of overlapping purchases	8.51	11.65	3,478
% of overlapping sales	6.64	11.09	3,478
Independent variables of interest			
Driving dist. between homes $\leq$ 1 mile (*DistBtwnHms1Mile*)	0.0037	0.0581	4,221
Norm. driving dist. between homes $\leq$ 2.9 miles (*Neighbors*)	0.0037	0.0607	4,221
Norm. driving dist. between homes $\leq$ 1 mile	0.0013	0.0360	4,221
Norm. driving dist. between homes $\leq$ 5 miles	0.0054	0.0733	4,221
Control variables			
Zip code distance between homes $\leq$ 10 miles	0.0444	0.2059	4,221
Zip code distance between homes $\leq$ 25 miles	0.0800	0.2712	4,221
Zip code distance between homes $\leq$ 50 miles (*ShareMediaMkt*)	0.1046	0.3061	4,221
Funds are located in the same city (*ShareMFCity*)	0.0712	0.2571	4,221
Funds are both small-cap (*BothSmallCap*)	0.0653	0.2470	4,221
Funds are both mid-cap (*BothMidCap*)	0.0642	0.2451	4,221
Funds are both large-cap (*BothLargeCap*)	0.4133	0.4924	4,221
Funds are both value (*BothValue*)	0.0492	0.2163	4,221
Funds are both growth (*BothGrowth*)	0.2325	0.4224	4,221
Funds are both blend (*BothBlend*)	0.0881	0.2834	4,221
Funds are in the same family (*ShareFam*)	0.0262	0.1598	4,221
Mgrs. mng. at least 1 other fund together (*MngOtherFundTogether*)	0.0028	0.0528	4,221
Funds have at least 1 mgr. in common (*CommonMgr*)	0.0016	0.0402	4,221
Difference in TNA-based quintiles between fund pairs (*TNAQuinDiff*)	1.5898	1.1954	4,221
Average TNA-based quintile of fund pair (*TNAQuinAvg*)	3.0239	0.9993	4,221

Table 2

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

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

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) of the text. The variable of interest is $Neighbors_{i,j,t}$, which is a dummy variable that is one if at least one manager from fund i lives within 2.9 normalized miles of at least one manager from fund j during quarter t, $SameMFCity$ is a dummy variable that is one if funds i and j are headquartered in the same city, $SameMediaMkt$ 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 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 4.2 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 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)
$Neighbors$			1.34^a	1.05^a	1.08^a	0.82^a	2.80^a
			(0.27)	(0.26)	(0.26)	(0.25)	(0.80)
$DistBtwnHms1Mile$		0.88^a					
		(0.32)					
$SameMFCity$	0.65^a	0.64^a	0.63^a	0.49^a	0.49^a	0.21^c	0.98^a
	(0.14)	(0.14)	(0.14)	(0.13)	(0.13)	(0.13)	(0.31)
$SameMediaMkt$				0.38^a	0.38^a	0.31^b	0.27
				(0.12)	(0.13)	(0.13)	(0.26)
$SameFundFam$	1.03^a	1.03^a	1.02^a	1.01^a	0.99^a		2.36^a
	(0.24)	(0.24)	(0.24)	(0.24)	(0.24)		(0.49)
$MngOtherFundTogether$	2.11^a	2.10^a	2.09^a	2.08^a	1.87^a		1.23
	(0.43)	(0.43)	(0.43)	(0.43)	(0.44)		(1.01)
$CommonMgr$	10.42^a	10.38^a	10.32^a	10.24^a			
	(0.66)	(0.66)	(0.65)	(0.66)			

Table 2 continues on the following page.

Table 2 continued from the previous page.

sample:	full (1)	full (2)	full (3)	full (4)	No mgrs. common (5)	Different families (6)	Single mgr. pairs (7)
$BothSmallCap$	1.22^a	1.22^a	1.22^a	1.22^a	1.20^a	1.20^a	1.11^a
	(0.15)	(0.15)	(0.15)	(0.15)	(0.14)	(0.14)	(0.25)
$BothMidCap$	1.80^a	1.80^a	1.80^a	1.80^a	1.79^a	1.78^a	1.21^a
	(0.18)	(0.18)	(0.18)	(0.18)	(0.18)	(0.18)	(0.26)
$BothLargeCap$	12.08^a	12.08^a	12.08^a	12.07^a	12.06^a	12.05^a	13.06^a
	(0.31)	(0.31)	(0.31)	(0.31)	(0.31)	(0.31)	(0.56)
$BothValue$	2.91^a	2.91^a	2.91^a	2.91^a	2.91^a	2.93^a	2.27^a
	(0.36)	(0.36)	(0.36)	(0.36)	(0.36)	(0.36)	(0.74)
$BothGrowth$	3.45^a	3.45^a	3.45^a	3.45^a	3.44^a	3.44^a	3.34^a
	(0.21)	(0.21)	(0.21)	(0.21)	(0.21)	(0.21)	(0.35)
$BothBlend$	2.35^a	2.35^a	2.35^a	2.35^a	2.34^a	2.34^a	1.70^a
	(0.29)	(0.29)	(0.29)	(0.29)	(0.29)	(0.29)	(0.41)
$TNAQuinDiff$	-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	0.64^a	0.75^a
	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.16)
$Constant$	-0.13	-0.13	-0.13	-0.15	-0.14	-0.06	-0.32
	(0.27)	(0.27)	(0.27)	(0.27)	(0.27)	(0.27)	(0.49)
$AdjR^2$	0.41	0.41	0.41	0.41	0.41	0.41	0.42
N (thousands)	4,221	4,221	4,221	4,221	4,214	4,109	484

Table 3

Social interactions and mutual fund trades

The table reports the coefficient estimates and standard errors from the OLS estimation of purchase and sale overlap on *Neighbors* and control variables described in Table 2. The dependent variable for the regression results displayed in columns 1 through 4 (5 through 8) is the percentage of overlapping stock purchases (sales) between a given pair of funds during the quarter. These dependent variables are defined in equations (3) and (4) in the text, respectively. The sample includes 3.5 million quarterly fund pair observations from the first quarter of 1997 to the fourth quarter of 2010. In columns 2 and 6, the sample is limited to fund pairs with no managers in common during quarter t. Columns 3 and 7 exclude fund pairs within the same fund family. In columns 4 and 8 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 1%, 5%, and 10% levels, respectively.

Dependent variable:	% of overlapping buys				% of overlapping sales			
sample:	full (1)	No mgrs. common (2)	Different families (3)	Single mgr pairs (4)	full (5)	No mgrs. common (6)	Different families (7)	Single mgr pairs (8)
Neighbors	1.03^a	1.19^a	0.67^b	4.81^a	0.50^c	0.63^b	0.40	1.20
	(0.34)	(0.34)	(0.30)	(1.13)	(0.26)	(0.26)	(0.25)	(0.76)
SameMFCity	0.07	0.07	-0.12	0.99^a	0.55^a	0.54^a	0.25^b	1.44^a
	(0.13)	(0.13)	(0.13)	(0.29)	(0.12)	(0.12)	(0.12)	(0.29)
SameMediaMkt	0.78^a	0.79^a	0.71^a	0.61^a	0.51^a	0.52^a	0.46^a	0.22
	(0.11)	(0.11)	(0.11)	(0.22)	(0.11)	(0.11)	(0.11)	(0.24)
SameFundFam	1.78^a	1.83^a		3.25^a	1.18^a	1.19^a		2.47^a
	(0.24)	(0.24)		(0.46)	(0.21)	(0.21)		(0.46)
MngOtherFundTogether	2.25^a	2.42^a		-0.24	1.52^a	1.64^a		-0.51
	(0.46)	(0.43)		(0.93)	(0.49)	(0.47)		(0.84)
CommonMgr	13.47^a				11.49^a			
	(0.97)				(0.88)			
BothSmallCap	1.06^a	1.02^a	1.02^a	0.76^a	0.72^a	0.69^a	0.69^a	0.48^b
	(0.17)	(0.17)	(0.17)	(0.25)	(0.16)	(0.16)	(0.16)	(0.24)
BothMidCap	1.59^a	1.58^a	1.57^a	1.10^a	1.15^a	1.14^a	1.13^a	0.69^a
	(0.19)	(0.19)	(0.19)	(0.28)	(0.16)	(0.16)	(0.16)	(0.22)
BothLargeCap	8.72^a	8.71^a	8.67^a	9.01^a	6.81^a	6.81^a	6.79^a	7.72^a
	(0.24)	(0.24)	(0.24)	(0.40)	(0.22)	(0.22)	(0.22)	(0.38)
BothValue	2.52^a	2.53^a	2.53^a	1.83^a	0.74^a	0.75^a	0.77^a	1.04^c
	(0.30)	(0.30)	(0.30)	(0.58)	(0.25)	(0.25)	(0.25)	(0.56)
BothGrowth	2.79^a	2.79^a	2.77^a	3.12^a	2.42^a	2.41^a	2.41^a	2.49^a
	(0.20)	(0.20)	(0.20)	(0.35)	(0.19)	(0.19)	(0.19)	(0.30)
BothBlend	1.82^a	1.81^a	1.82^a	1.06^a	1.59^a	1.59^a	1.59^a	0.88^a
	(0.22)	(0.22)	(0.22)	(0.28)	(0.20)	(0.20)	(0.20)	(0.28)
TNAQuinDiff	-0.03	-0.03	-0.03	-0.07	-0.08^a	-0.08^a	-0.07^a	-0.16^a
	(0.03)	(0.03)	(0.03)	(0.05)	(0.03)	(0.03)	(0.03)	(0.05)
TNAQuinAvg	0.61^a	0.61^a	0.59^a	0.60^a	0.41^a	0.41^a	0.40^a	0.52^a
	(0.08)	(0.08)	(0.08)	(0.15)	(0.08)	(0.08)	(0.08)	(0.13)
Constant	1.41^a	1.43^a	1.51^a	1.25^b	1.37^a	1.37^a	1.43^a	1.04^b
	(0.29)	(0.29)	(0.29)	(0.51)	(0.27)	(0.27)	(0.27)	(0.42)
$AdjR^2$	0.15	0.14	0.14	0.16	0.10	0.10	0.10	0.12
N (thousands)	3,478	3,471	3,390	390	3,478	3,471	3,390	390

Table 4

Extensive and intensive margin overlap

The table reports the coefficient estimates and standard errors from the OLS estimation of various measures of portfolio overlap on $Neighbors$, $SameMediaMkt$, and $SameMFCity$. Each row reports results for a separate regression. Included in the regressions are the control variables from Table 2, but their coefficient estimates are not reported. 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 sale 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 4.2 million holdings overlap observations and 3.5 million purchase and sale overlap observations from 1996 through 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 1%, 5%, and 10% levels, respectively.

Overlap in:	$Neighbors$	$Same$ $MediaMkt$	$Same$ $MFCity$	Avg. overlap
Extensive buys (for both funds)	0.19^{b}	0.16^{a}	0.16^{a}	1.80
	(0.08)	(0.04)	(0.03)	
Extensive buys (for at least one fund)	0.46^{b}	0.14	0.50^{a}	5.07
	(0.18)	(0.08)	(0.07)	
Intensive buys	0.73^{a}	-0.06	0.29^{a}	3.45
	(0.20)	(0.07)	(0.07)	
Extensive sales (for both funds)	0.16^{a}	0.08^{a}	0.01	1.07
	(0.06)	(0.03)	(0.02)	
Extensive sales (for at least one fund)	0.37^{a}	0.23^{a}	0.15^{a}	3.09
	(0.13)	(0.06)	(0.05)	
Intensive sales	0.26	0.31^{a}	0.38^{a}	3.56
	(0.18)	(0.09)	(0.08)	

Table 5

Social interactions vs. preferences: neighbor relocations

The table reports the coefficient estimates and standard errors from the 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 9.1 million quarterly manager-pair holdings overlap observations and 7.6 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 2.9 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 manager 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 2. 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 1%, 5%, and 10% levels, respectively.

Dependent variable:	% of overlapping		
	holdings	buys	sales
	(1)	(2)	(3)
$FutureNeighbors$	-0.39	-0.19	-0.23
	(0.50)	(0.49)	(0.60)
$Neighbors$	1.13^a	1.11^a	0.56^b
	(0.25)	(0.33)	(0.25)
$PastNeighbors$	1.09^a	0.48	0.30
	(0.40)	(0.48)	(0.38)
Controls	Yes	Yes	Yes
$AdjR^2$	0.40	0.14	0.09
N (thousands)	9,122	7,605	7,605

Table 6

Social interactions vs. preferences: demographic controls

The table reports the coefficient estimates and standard errors from the OLS estimation of holdings, purchase, and sale overlap on $Neighbors$ and demographic control variables. The analysis in the table uses quarterly manager-pair observations outlined in Table 5. $Neighbors$ is a dummy variable that is one if the pair of managers live within 2.9 normalized miles of one another during the quarter. $BothReligiousAreas$, $BothJewishAreas$, 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. All regressions include the control variables described in Table 2. 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 1%, 5%, and 10% levels, respectively.

Dependent variable:	% of overlapping					
	holdings		buys		sales	
	(1)	(2)	(3)	(4)	(5)	(6)
$Neighbors$	1.13^a	1.13^a	1.10^a	1.04^a	0.56^b	0.65^a
	(0.25)	(0.25)	(0.33)	(0.31)	(0.25)	(0.25)
$SameMFCity$	0.48^a	0.44^a	0.12	0.07	0.55^a	0.52^a
	(0.13)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)
$SameMediaMkt$	0.52^a	0.11	0.76^a	0.33^a	0.53^a	0.25^a
	(0.13)	(0.10)	(0.11)	(0.09)	(0.11)	(0.09)
$BothReligiousAreas$		0.84^a		0.55^a		0.77^a
		(0.19)		(0.16)		(0.15)
$BothJewishAreas$		0.11		0.33^b		0.06
		(0.19)		(0.17)		(0.17)
$BothCatholicAreas$		0.19		0.22		-0.03
		(0.22)		(0.18)		(0.17)
$BothHighHomeValueAreas$		-0.22		0.11		-0.47^a
		(0.20)		(0.18)		(0.16)
$BothLowHomeValueAreas$		0.09		-0.28^c		-0.08
		(0.19)		(0.17)		(0.17)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
$AdjR^2$	0.40	0.40	0.14	0.14	0.09	0.09
N (thousands)	9,122	9,122	7,605	7,605	7,605	7,605

Table 7

Social interactions and manager characteristics

The table reports the coefficient estimates and standard errors for the 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 4 is the percentage of overlapping stock holdings between of the funds managed by a given pair of managers and for columns 5 through 8 the dependent variable is the percentage of overlapping stock purchases between the funds managed by a given pair of managers during the quarter. *NeighborTenure* is the length of time in years that two managers have lived within 2.9 normalized miles of one another. *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. See the appendix for a description of the algorithm used to determine ethnicities of surnames. *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 2 as well as the levels of all included manager characteristics (not reported). 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 1%, 5%, and 10% levels, respectively.

Dependent variable:	% of overlapping holdings					% of overlapping buys				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Neighbors	0.79^{a}	0.97^{a}	1.06^{a}	0.81^{a}	0.80^{b}	0.63^{c}	0.67	1.04^{a}	0.49	0.09
	(0.28)	(0.32)	(0.25)	(0.29)	(0.35)	(0.37)	(0.43)	(0.33)	(0.33)	(0.35)
Neighbors × *NeighborTenure*	0.19^{c}					0.25^{c}				
	(0.11)					(0.15)				
Neighbors × *SimilarAge*		0.24					0.75			
		(0.36)					(0.48)			
Neighbors × *SameEthnicity*			3.73^{a}					2.61^{c}		
			(1.13)					(1.55)		
Neighbors × *SameCollege*				1.03^{c}					1.09	
				(0.57)					(1.00)	
Neighbors × *BothExp*					1.12^{b}					3.16^{a}
					(0.46)					(0.64)
Neighbors × *BothInexp*					-0.51					-0.62
					(0.49)					(0.44)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
$AdjR^2$	0.40	0.40	0.40	0.40	0.39	0.14	0.14	0.14	0.13	0.13
N (thousands)	9,122	9,122	9,122	6,296	7,307	7,605	7,605	7,605	5,212	6,069

Table 8

Social interactions and stock characteristics

The table reports the coefficient estimates and standard errors from the OLS estimation of various measures of portfolio overlap in holdings (columns 1 through 4) and in purchases (columns 5 through 8) split by various stock-level characteristics on $Neighbors$, $SameMediaMkt$, and $SameMFCity$. Included in the regressions, are the control variables from Table 2, but their coefficient estimates are not reported. Also, reported is the average overlap in the sample for the dependent variable. A stock is categorized as local if its headquarters is within 50 miles of the headquarters of either fund in the fund pair. All other stocks are non-local. Following Hong and Kostovetsky (2012), politically sensitive stocks are those in the the Tobacco, Guns and Defense, or Natural Resources industries; all others are categorized as non-politically sensitive. High (low) analyst coverage stocks are those whose analyst coverage is greater (less) than the median in the sample each year. High (low) advertising expense stocks are those whose advertising expense is greater (less) than the median in the sample each year. Lottery stocks are defined as in Kumar (2009). All stocks that do not meet the definition are categorized as non-lottery stocks. The analysis in the table uses quarterly fund-pair observations for the sample of 4.2 million holdings overlap observations and 3.5 million purchase overlap observations from 1996 through 2010. Stocks with missing characteristic information are omitted from the analysis. The samples are limited to fund pairs with 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 1%, 5%, and 10% levels, respectively.

	% overlapping holdings				% overlapping buys			
	$Neighbors$	$Same$ $MediaMkt$	$Same$ $MFCity$	Avg. overlap	$Neighbors$	$Same$ $MediaMkt$	$Same$ $MFCity$	Avg. overlap
local stocks	-0.23^a	0.22^a	0.06	0.88	-0.21^a	0.19^a	-0.0	0.78
	(0.08)	(0.04)	(0.06)		(0.06)	(0.04)	(0.05)	
non-local stocks	0.95^a	0.06	0.22^b	5.77	0.90^a	0.39^a	-0.10	6.15
	(0.20)	(0.09)	(0.10)		(0.24)	(0.08)	(0.10)	
politically sensitive stocks	-0.00	0.02^a	0.02^a	0.11	0.01	0.01	0.03^a	0.12
	(0.01)	(0.01)	(0.01)		(0.02)	(0.01)	(0.01)	
non-politically sensitive stocks	0.96^a	0.35^a	0.37^a	7.67	1.00^a	0.74^a	-0.05	7.74
	(0.25)	(0.12)	(0.12)		(0.32)	(0.10)	(0.12)	
S&P 500 stocks	0.86^a	0.24^c	0.33^a	6.75	0.81^a	0.52^a	-0.04	6.24
	(0.25)	(0.12)	(0.13)		(0.28)	(0.10)	(0.11)	
non-S&P 500 stocks	0.19^b	0.13^a	0.15^a	1.22	0.30^b	0.24^a	0.09^b	1.86
	(0.09)	(0.02)	(0.03)		(0.14)	(0.03)	(0.04)	
high analyst cov. stocks	1.02^a	0.35^a	0.48^a	7.91	1.08^a	0.74^a	0.03	7.98
	(0.25)	(0.12)	(0.13)		(0.33)	(0.11)	(0.12)	
low analyst cov. stocks	0.03^a	0.01^a	0.01^b	0.07	0.03	0.02^a	0.02^a	0.13
	(0.01)	(0.00)	(0.00)		(0.02)	(0.01)	(0.01)	
high advert. exp. stocks	0.38^a	0.06	0.31^a	3.70	0.37^b	0.22^a	0.09	3.56
	(0.14)	(0.07)	(0.07)		(0.17)	(0.06)	(0.06)	
low advert. exp. stocks	0.00	0.01^a	0.01^a	0.09	0.03	0.02^a	0.01	0.14
	(0.01)	(0.00)	(0.00)		(0.02)	(0.00)	(0.01)	
lottery stocks	0.01^b	0.01^a	0.01^a	0.04	0.04^b	0.02^a	0.03^a	0.09
	(0.01)	(0.00)	(0.00)		(0.02)	(0.00)	(0.00)	
non-lottery stocks	1.04^a	0.36^a	0.47^a	7.94	1.06^a	0.74^a	0.03	8.01
	(0.25)	(0.12)	(0.13)		(0.33)	(0.11)	(0.12)	

Table 9

The performance effect of social interactions: Holdings

The table reports returns for "neighbor" (N) and "non-neighbor" (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 non-neighbor portfolio. We use the 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 the neighbor and non-neighbor returns, respectively, each quarter by averaging across funds in quarter t, weighting each fund's return by its total net asset value. ($\mathcal{N}$ denotes the set of neighbor stocks, $\mathcal{O}$ is the set of non-neighbor stocks, and $w_{i,k,t}$ is the actual portfolio weight of fund i in stock k during quarter t.) Columns 2 and 3 report the raw and risk-adjusted returns for the neighbor and non-neighbor portfolios. Risk adjustment is based on DGTW. Column 4 reports the difference in returns between these portfolios, and the results of the paired t-test testing whether the difference is significantly different from zero. Standard errors are reported in parentheses. Significance levels are denoted by a, b, c, which correspond to 1%, 5%, and 10% levels, respectively.

	R^N	R^O	$R^N - R^O$
Excess return	0.61	0.55	0.06
	(0.49)	(0.47)	(0.68)
DGTW (entire CRSP universe)	0.10	0.06	0.04
	(0.08)	(0.08)	(0.11)
DGTW (CRSP with price>$5)	0.02	0.00	0.02
	(0.08)	(0.09)	(0.12)

Table 10

The performance effect of social interactions: Trades

This table reports the investment performance of mutual fund trades. We form portfolios at the beginning of each quarter based on whether the fund bought or sold a stock, respectively. Stocks that are bought are aggregated into the "buy" portfolio, while those that are sold during the quarter are placed in the "sell" portfolio. We create two additional subgroups within the buy and sell portfolios: "neighbor" (N) and "non-neighbor" (O) trades. To calculate the value-weighted quarterly returns of the neighbor and non-neighbor buy and sell portfolios, 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. Columns 1 and 2 report the risk-adjusted average returns of the neighbor buy and sell portfolios, respectively. Risk adjustment is based on DGTW. Columns 4 and 5 report the corresponding results for the non-neighbor portfolios. Finally, columns 3 and 6 describe the difference of the returns of the buy and sell portfolios for the neighbor and non-neighbor 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 a sample period that excludes the financial crisis; as well as for 1) all fund transactions, 2) extensive margins, and 3) intensive margin transactions. Significance levels are denoted by a, b, c, which correspond to 1%, 5%, and 10% levels, respectively.

	DGTW-adjusted monthly returns						
	Neighbor portfolio			Non-neighbor portfolio			
	Buys	Sells	Diff.	Buys	Sells	Diff.	Diff.-Diff.
All trades							
Full sample	0.21	-0.27	0.48^{b}	0.10	0.10	0.01	0.48^{b}
	(0.16)	(0.17)	(0.23)	(0.11)	(0.10)	(0.15)	(0.24)
Excluding the financial crisis	0.32^{c}	-0.30	0.62^{b}	0.16	0.04	0.12	0.49^{c}
	(0.19)	(0.20)	(0.28)	(0.13)	(0.11)	(0.17)	(0.28)
Extensive margin trades							
Full sample	0.25	-0.73^{b}	0.99^{b}	0.22	0.34	-0.12	1.11^{b}
	(0.23)	(0.32)	(0.40)	(0.17)	(0.21)	(0.27)	(0.42)
Excluding the financial crisis	0.37	-0.92^{b}	1.30^{a}	0.21	0.34	-0.13	1.43^{a}
	(0.26)	(0.39)	(0.47)	(0.21)	(0.23)	(0.31)	(0.48)
Intensive margin trades							
Full sample	0.27^{c}	-0.13	0.40^{c}	0.03	0.06	-0.03	0.43^{c}
	(0.16)	(0.18)	(0.24)	(0.13)	(0.12)	(0.18)	(0.25)
Excluding the financial crisis	0.37^{b}	-0.11	0.48^{c}	0.09	0.02	0.07	0.41
	(0.18)	(0.21)	(0.28)	(0.15)	(0.13)	(0.20)	(0.29)

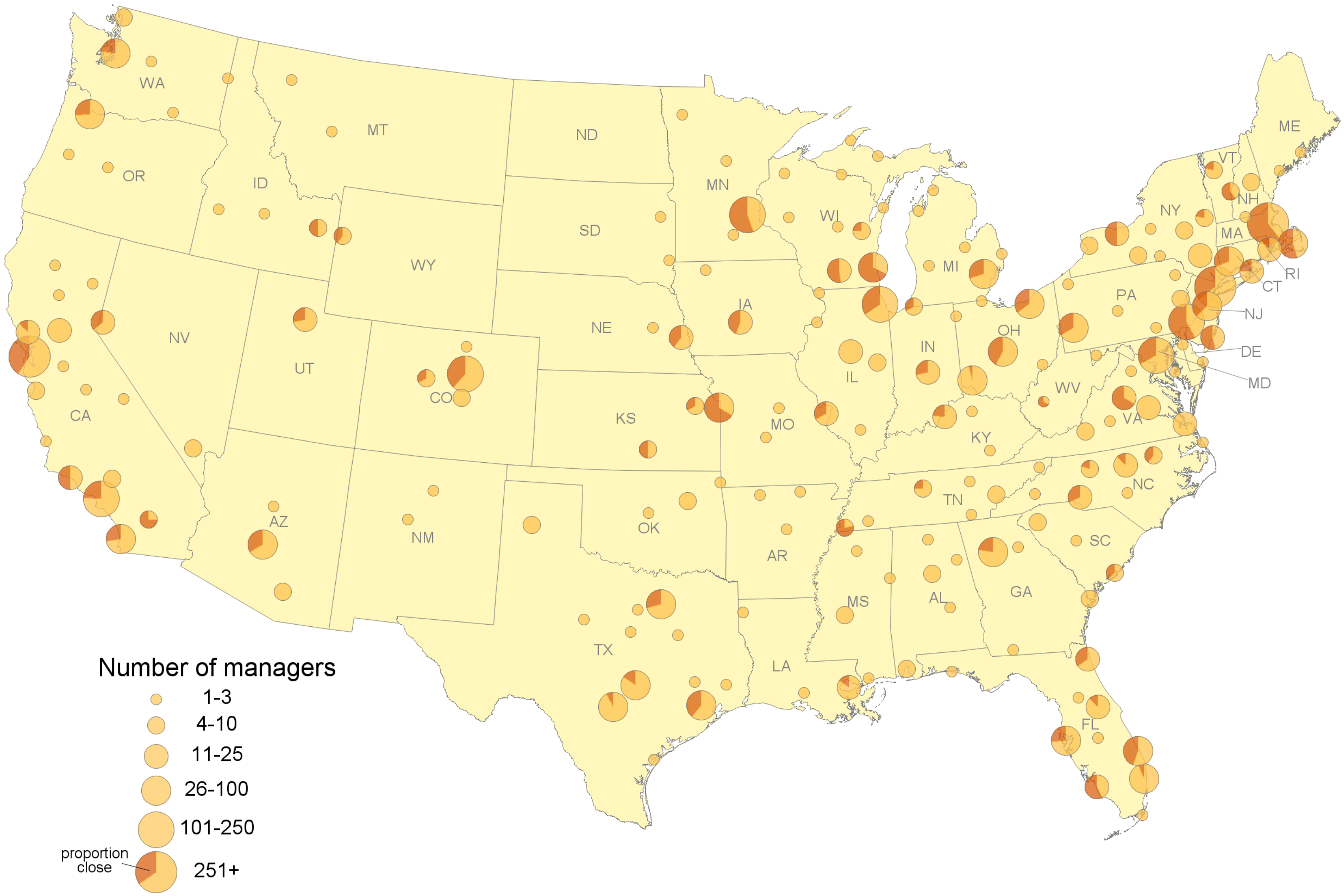

Figure 1: City clusters and close manager pairs

The figure shows the geographic dispersion of fund managers. Residences are identified by zip codes, and we use a clustering algorithm to group nearby zip codes into cities, as explained in the main text. The algorithm stops joining clusters that are more than 50 miles apart. The size of the 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 close to at least one other manager at some point during the sample period. Managers are classified as close if they live within 2.9 normalized miles of each other, where the normalization adjusts for population density (see text for details).

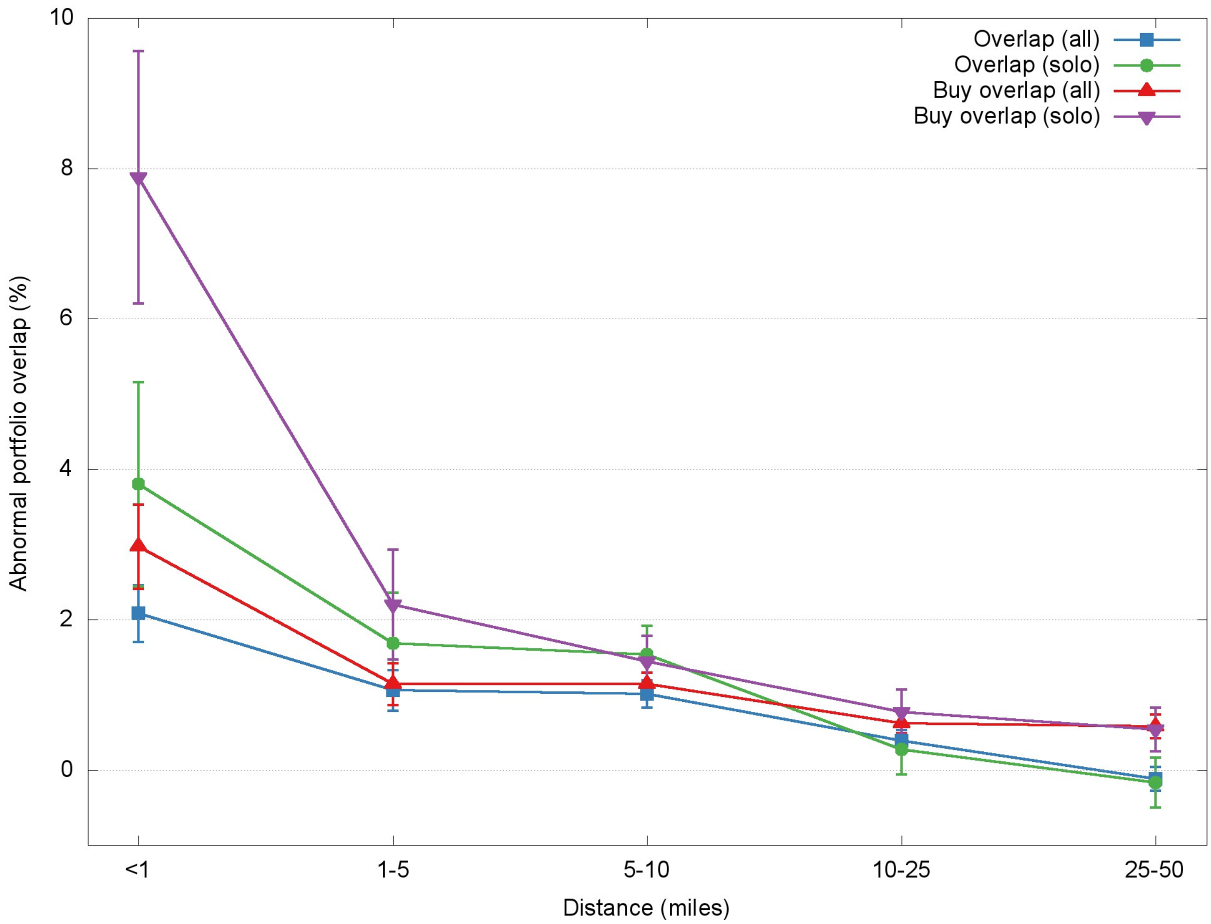

Figure 2: Manager distance and portfolio overlap

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 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 8 of Table 2 and columns 2 and 4 in Table 3. Standard errors are shown as error bars.

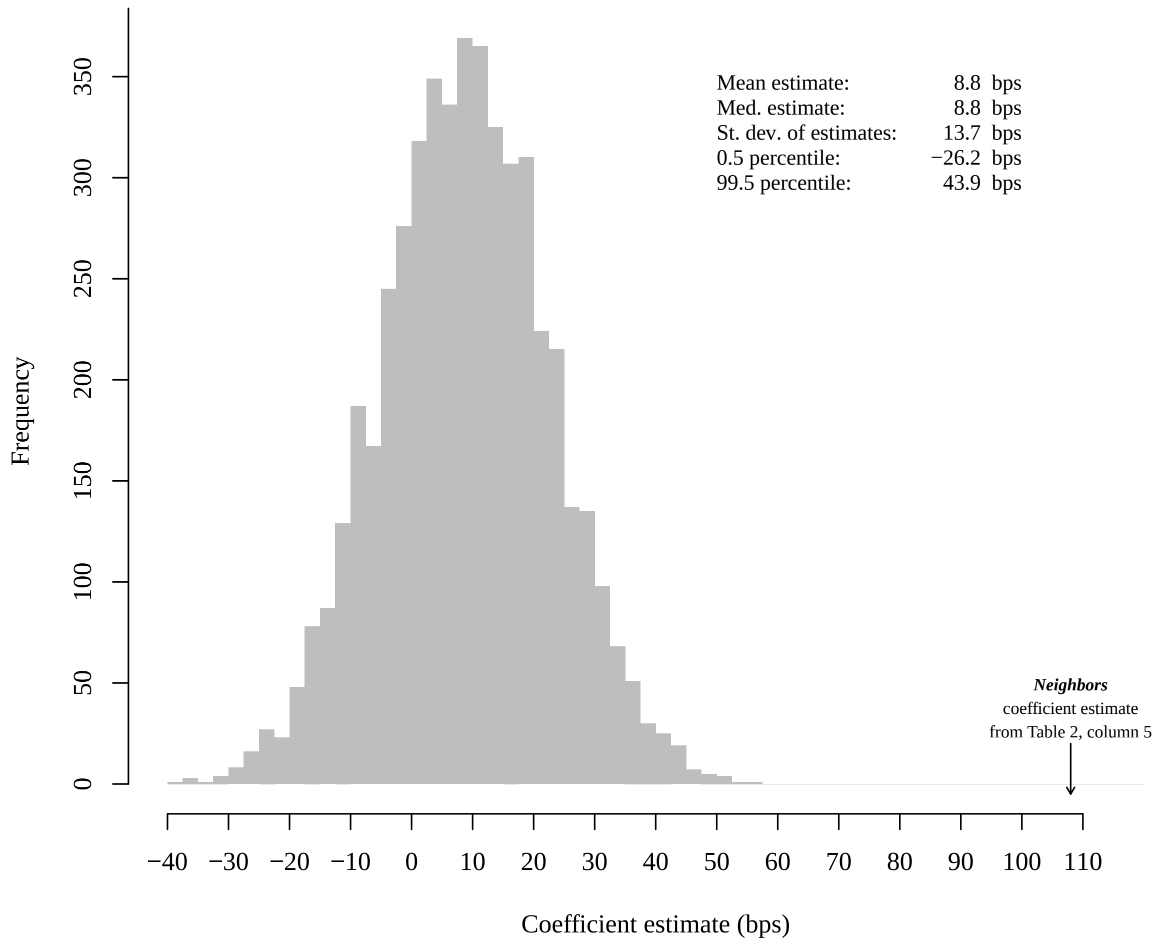

Figure 3: Bootstrap analysis

The figure shows the distribution of $Neighbors$ coefficient estimates from 5,000 bootstrap simulations of the regression equation in Table 2, 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 3.5%, which gives the same overall proportion of neighbors as in the sample. The sample coefficient estimate is approximately 7 standard deviations greater than zero.