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Peer Effects at Work: The Common Stock Investments of Co-workers

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Peer Effects at Work: The Common Stock Investments of Co-workers*

Hans K. Hvide and Per Östberg†

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Abstract

Stock market behavior of individual investors is highly correlated with stock market behavior of their co-workers. For example, a ten percentage point increase in the fraction of co-workers that purchase stocks in a given month is associated with a two percentage point increase in the likelihood of individuals' making a purchase. The high correlation exists even after taking controlling for individual socio-demographic characteristics and for time, stock, zip code, and plant fixed effects. Using data on family relations and on residential zip code, we show that the high correlation is not driven by peer effects at the family or zip code level. Moreover, workplace peer effects appear to be strong relative to geographical peer effects.

Keywords: individual investors, peer effects, portfolio, social interaction, stock market, stock selection.

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A fundamental observation about human society is that people who communicate regularly with one another think similarly. There is at any place and in any time a Zeitgeist, a spirit of the times. . . .Word-of-mouth transmission of ideas appears to be an important contributor to day-to-day or hour-to-hour stock market fluctuations. Shiller (2000, p. 148, 155)

1 Introduction

Investment decisions made by individual investos may be influenced by the individuals that they interact with. If sufficiently correlated, trades based on social interaction might even affect asset prices. Although the literature has long acknowledged the existence of social interaction effects among individual investors (e.g., Shiller, 1984, Pound and Shiller, 1989, Hong et al., 2004), lack of data has made it difficult to test for this mechanism.[1]

In this paper we examine a novel channel for social interaction between individual stock market investors: the workplace. We assess how co-workers can affect the stock market behavior of individuals.[2] The social psychology literature emphasizes the strength of face-to-face communication between individuals that frequently interact in producing and altering beliefs.[3] Individuals spend a considerable fraction of their time at the workplace, and even the most efficient firms create opportunities for face-to-face communication; by the water-cooler, during lunch, or at company outings. In fact, such social interaction is often encouraged. It is plausible that conversations at work occasionally center on the

[1]Existing studies find mixed support. Using weekly data from China, Feng and Seasholes (2004) find that individual investors' purchase decisions are correlated, but driven by common reaction to locally available news rather than word-of-mouth effects. Using quarterly data from a discount brokerage house in the U.S., Ivkovic and Weissbenner (2007) find that individual investors' purchase decisions are correlated at the zip code level. The correlation is higher in states with a high level of sociability.

[2]The literature on informational cascades (Bikhchandani et al., 1992; Banerjee, 1992; Elison and Fudenberg, 1993) provide reasons why information (correct or not) obtained from co-workers may be an important factor in asset allocation. Hong, Kubik and Stein (2004) construct a theory in which stock market behavior may be influenced by social interaction.

[3]In a classic study byAsch (1955) individuals alone and in groups compared the lengths of line segments. The lengths were sufficiently different that, when responding alone, such that very few wrong answers were given. Yet when placed in a group in which all other members were instructed to give the same wrong answers, individuals frequently gave wrong answers. See Shiller (1995, 2000) for further references.

stock market, and that these conversations can affect individual behavior. For example, investors pick among a dizzying number of individual stocks when evaluating which to buy, and may obtain information from discussions with their colleagues, or make inferences based on observing their decisions. Or, conversations with colleagues can simply raise awareness of or trust in equity markets and make trading more likely (Guiso and Japelli, 2003, Guiso, Sapienza and Zingales, 2010). Duflo and Saez (2003) document strong evidence of workplace interaction in a related context. They use a randomized trial to document that social interaction influences the decision to enroll in a tax deferred account scheme.[4]

In order to examine the effects of co-workers on stock market behavior, we use a unique matched employer–employee panel data set from Norway. The dataset has annual observations on the entire labor market, which allows us to track individuals as they move between plants over time. Because we are able to match individuals to plants, we also know who each individual's colleagues are in every year.[5] We combine the matched employer-employee dataset with yearly socio-demographic information at individual level, including information about family relationships and residential zip code. This data is merged with a complete record of common stock transactions made by Norwegian individual investors in the period 1994-2005. To avoid capturing effects via employee stock programs, we exclude plants that are belong to publicly listed companies.

We study whether an individual makes a purchase in a given month, and link it to the fraction of their co-workers that make a purchase, controlling for individual socio-demographic characteristics. The effects are large: a ten percentage point increase in the fraction of co-workers that make a purchase in a given month is associated with an increase of two percentage points in the individual's propensity to make a purchase. An advantage of our data is that we can examine peer effects also at a much more detailed level; in the selection of individual stocks. We find that a ten percentage point increase in the fraction of co-workers that purchase a particular stock is associated with an increase of 1.7 percentage points in the individual's own purchase of that stock. This result, which

[4]In a different context Gompers et al. (2005, p. 612) argue that when working with colleagues who have been involved in startups, "employees learn from their coworkers about what it takes to start a new firm." One can easily see envisage similar mechanisms for stock market activity.

[5]There are 50 people employed in the median firm in our dataset.

continues to hold after including stock fixed effects, is quite striking given that there are hundreds of stocks listed at the Oslo Stock Exchange.

Stock market activity could be correlated at the plant level due to other reasons than conversations between colleagues. We apply panel data techniques in order to address concerns about common unobservables. Plant and zip code fixed effects account for time-invariant systematic differences. Including yearly socio-demographic variables as controls make it unlikely that our results are driven by individual fixed effects. Monthly (stock-level) fixed effects remove the influence of market-wide news releases, and of extrapolation from past returns. Industry-month fixed effects remove the influence of trade journals and other industry-specific events. Plant peer effects are both economically and statistically strong after taking into account fixed effects.

A possibility nevertheless remains that the workers of a particular plant experience common shocks (such as bonus payments) that are unobservable. In order to address this issue, we study individuals that change place of work. We consider how the relation between the trading decision of co-workers and the individual evolves over time. Figure 1 illustrates how the effect of old (new) co-workers decreases (increases) significantly when the individual leaves (joins) the new plant (see section 3.1 for details). We find that old peers affect individual choices after the investors has moved from the plant albeit at a lower rate, which is suggestive of word-of-mouth effects.[6]

A natural question is whether our results are driven by neighborhood effects. This could be the case, for example, if co-workers tend to live in the same area. Because we have matched socio-demographic information for the entire population of Norway we can identify both neighbors and family members, and control for peer effects along these dimensions. Our analysis suggests that the impact of zip code peers is significantly reduced when workplace and family peers are introduced. In contrast, the impact of workplace peers is much less affected by the introduction of the other peer groups and socioeconomic control variables.

[6] One can also argue that although the domain of common unobservable shocks that could affect the number of individuals that purchase a stock could be quite large, the domain of common unobservable shocks that leads to the purchase of the *same stock* is much less restricted.

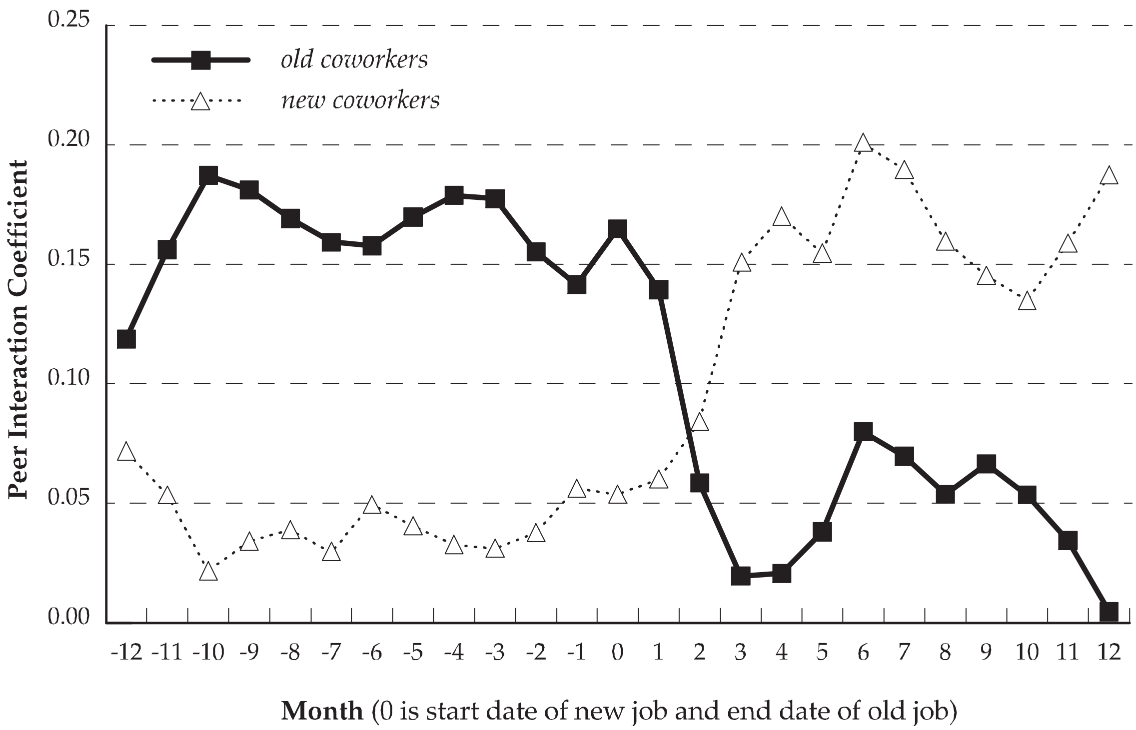

Figure 1. Co-worker and individual trading.

To assess workplace peer effects further, we can compare their magnitude to the magnitude of family and zip code peer effects. Our results suggest that workplace peer effects are comparable in magnitude to the peer effects inside the family, and are quite large compared to peer effects at the zip code level. For example, increasing the fraction of co-workers that make a stock purchase by one standard deviation (6.32 percentage points) increases the probability of making a purchase by 1.08 percentage points. In contrast, increasing the fraction of geographical peers that make a purchase by one standard deviation (1.63 percentage points) increases the predicted probability of making a purchase by 0.44 percentage points.

Further, we consider whether peer purchases yield abnormal returns. We find that co-worker peer purchases are neither associated with positive abnormal performance or negative abnormal performance. However, since we have abstracted from transaction costs, peer effects could be detrimental in so far that it might result in excessive amounts of trading. This suggest that investors respond to communication from peers even though this communication does not appear to contain valuable information.

To sum up, we analyze a new channel for social interaction between stock market investors: the workplace, and find strong evidence of peer effects both in the timing of purchases and in stock selection. The effects are robust to including fixed effects that control for unobserved heterogeneity at the plant, geographical and stock level.

Our paper connects to the literature in several ways. Most importantly, while the literature on institutional investors in the stock market finds strong evidence of social interaction and network effects (Hong et al., 2005, Cohen et al., 2007, Cohen et al., 2010), the evidence on social interaction among individual investors in the stock market (Feng and Seasholes, 2004, Ivkovic and Weissbenner, 2007) has been more mixed. We consider a novel channel for peer effects; the workplace, and contribute to the literature by showing that co-worker investment decisions can explain a substantial amount of heterogeneity in individual stock market behavior.

The previous literature has considered peer effects along one dimension. A methodological innovation of the paper is to accommodate *three* types of peer effects: at work, in the family, and in the neighborhood. This allows us to control for alternative peer effects in the analysis of workplace peer effects. It also allows us to perform a "horse race" between different kinds of peer effects. We find that neighborhood effects can be important, consistent with Ivkovic and Weissbenner (2007), but obtain more unequivocal affirmative results for the effects of social interaction in the workplace and, less surprisingly, in the family. As discussed in Glaeser, Sacerdote, and Scheinkman (2002), it is often important for policy purposes to separate individual and social multiplier effects, the reason being that the aggregate impact of intervention may be larger than the sum of its effects on each individual's decision. Consistent with the findings of Duflo and Saez (2003) in the context of retirement plans, our results suggest that the social multiplier in the workplace is large both in absolute and relative terms when it comes to stock market behavior.

The paper also connects to a large empirical literature that documents poor asset allocation in settings related to the workplace. For example, Benartzi (2001) finds that employees often invest voluntarily in company stock, in spite of the poor hedging properties of such investments. Although excessive extrapolation, as suggested by Benartzi (2001) can explain why individuals invest in stocks with a strong prior performance, this argument cannot fully explain why they invest in company stock - there are many stocks with a strong prior performance. Our findings suggests the possibility that voluntary investments in company stock may be partially driven by peer effects at the firm level: one obvious object of workplace conversations is the stock market performance of the employer's stock. Cohen (2008) documents that employees of stand-alone firms invest 10 percentage points more in company stock than conglomerate employees. While excessive extrapolation cannot explain this pattern, Cohen (2008) argues that it is consistent with greater loyalty among employees of stand-alone firms. The results of the present paper suggests a complementary explanation: that social interaction effects, affecting beliefs, could be stronger at stand-alone firms than at conglomerates.[7]

The remainder of this paper is organized as follows. Section 2 presents the data. Section 3 presents the main results on the purchase decision and Section 4 the main results on stock selection and in Section 5 we consider the performance of peer purchases. Section 6 concludes.

2 Data

The data are proprietary and have been collected from three sources. First, a record of all common stock trades made on the Oslo Stock Exchange (OSE) by Norwegian residents from January 1994 to December 2005 was collected from Verdipapirsentralen (the Norwegian Central Securities Depository). For each transaction made by an individual, the data contain the (anonymized) ID of the individual, the date of transaction, the ticker of the security and the number of shares bought or sold. Verdipapirsentralen (the Norwegian Central Securities Depository) is a centralized register where all common stock trades at the OSE are recorded. To preserve anonymity, the trade records of the 20 most active investors are not contained in the data. Second, from the OSE we obtained daily ticker

[7] Doskeland and Hvide (2011) show that individual investors tend to overweigh their holdings of own-industry stocks. Again we can relate this to social interaction; a natural object of workplace conversations is the stock market performance of within-industry stocks.

prices and other company information such as market capitalization and company ID number. Where needed, we supplemented this information with data from Borsprosjektet at the Norwegian School of Economics. Third, from the government statistical agency, Statistics Norway, we obtained register data on the socio-demographic characteristics of the investors per December 31 from 1986 to 2006. From this data we can identify a number of peer groups that the individual belongs to. For each individual, the data includes the plant at which the individual is employed, the ID of the individuals spouse and children and the zip code in which the individual lives. We also identify family members; our family peer group is comprised of parents, grandparents, children, grand children, siblings, uncles, aunts, cousins, nieces and nephews. The socioeconomic data allows us to control for a number of background variables including income and wealth, age, gender, education, and employer variables such as industry (five digit NACE code) and an unique employer ID number. Since the data are collected from government registries, their reliability are high.

2.1 Peer Groups

Our reference group is the about $460,000$ individuals that make at least one purchase of a common stock at the Oslo Stock Exchange between 1994 and 2005 (about 10% of the population of Norway). This is the relevant group when we define the peer variables. For each year, we construct the sample of individuals in the following way. (1) In order for the peer group variables to be defined, we keep individuals that has a) at least one family member that is also an investor sometime between 1994 and 2005, b) at least one co-worker that is also an investor between 1994 and 2005, and c) at least one person in the same zip code that is also an investor between 1994 and 2005. (2) We keep individual that have full-time employment and is not employed by a listed company or a subsidiary of a listed company. This is done to ensure that employee stock ownership plans or investments in company stock are not driving our results. We also exclude individuals employed in Financial Services (two digit NACE codes 65, 66, and 67) as a simple way to eliminate professional investors from the sample.

Criterion (1) and (2) leaves us with $227,790$ unique individuals in the year 2000

(376,597 over the entire period). From this we sample 20% of all individual months. Since most individuals appear repeatedly in the dataset this only implies a reduction to 170,499 unique individuals in the year 2000.[8,9] These individuals are spread over 2,798 zip codes and 42,675 plants. We provide descriptive statistics of the socio-demographic variables and the size of the peer groups of these individuals in Table $A2$ in the Appendix.

3 Trade

In this section we relate the decision to trade of peers to the trading decision of the investor. Since very few individual investors short stocks, considering sell transactions implies conditioning on the investor already owning stocks. Therefore, we consider only trades that are purchases.[10]

We create a dummy variable $buy_{i,t}$ that takes the value 1 if investor i has bought a stock in month t and 0 otherwise. For our three peer groups we calculate the fraction of peers that trade in that particular month. We denote these fractions $buy_{i,t}^{plant}$, $buy_{i,t}^{family}$ and $buy_{i,t}^{zip}$ respectively. Table 1 provides descriptive statistics of our dependent and main independent variables for the trade analysis. In any given month 2.12% of all individuals buy a stock. The mean fraction of plant, family and zip code peers that make a purchase is 1.97%, 0.93% and 1.83% respectively.

We examine the effect of peers on the decision to trade by estimating the following linear regression,

$$buy_{i,t} = \alpha + \beta_1 \times buy_t^{plant} + \beta_2 \times buy_t^{family} + \beta_3 \times buy_t^{zip} + \mathbf{b\Gamma} + \varepsilon_{i,t} \qquad (1)$$

where dependent and independent variables are defined as above. In the above specification, $\mathbf{\Gamma}$ is a column vector of socio-demographic control variables and b is a row vector of coefficients. Our socio-demographic variables include (and various powers of, see the caption to Table 2 for specifics): age, wealth, labour income, sex and the number of years

[8] The final sample contains 329,634 unique individuals over the entire sample period (1994 to 2005).

[9] In 2000, about 45,000 individuals are excluded by criterion (2).

[10] In unreported analysis we have also considered the decisions to trade and to sell as dependent variables. The results are qualitatively unchanged.

of education.[11] We include month dummies to control for economy-wide trends in trading behavior. It is possible that the similarity in trading behavior among co-workers is driven by other plant specific factors. To control for this we introduce plant fixed effects. Similarly, we introduce zip code and postcode-plant interaction fixed effects. It is reasonable to expect that some part of the trading decision is driven by the industry that the individual is employed in, perhaps through industry periodicals. To this end we introduce industry fixed effects based on the first two digits of the NACE code of the industry of employment of the individual. We cluster our standard errors at the individual level to control for serial correlation in errors.[12]

Table 2 presents our regression results. Specifications (1) to (3) considers the relation between one of our peer groups and the trading decision of the individual while specifications (4) to (6) considers pair-wise combinations of the peer groups and (7) includes all three peer groups. All three peer groups are significant at the 1% level in all specifications. In terms of economic magnitude, in specification (7) a one standard deviation increase in co-worker trading activity (buy_t^{plant}) results in an increase in trading activity of 50.98% relative to the unconditional mean. The impact of family and neighbors is lower, a one standard deviation increase is associated with an increase in trading activity of 27.71% and 20.52% respectively. Thus, co-workers have the largest impact on the trading decision. Additionally, the introduction of co-workers reduces the impact of neighbors by roughly 19% (comparing specifications (3) and (5)).[13]

A potential concern is that our results are driven by a particular industry. For example, it could be the case that the co-worker peer effect is particularly strong in the energy sector. To mitigate these concerns we (*i*) include industry fixed effects in all of our specifications in Table 2 and (*ii*) in Appendix 3 we estimate a separate co-worker peer effect for each of 36 industries that represent a significant proportion of our sample. The results overwhelmingly support the notion that co-worker peer effects are universal across

[11] Appendix 2 contains descriptive statistics of our sociodemographic control variables.

[12] In unreported results we have also clustered standard errors around month. The resulting t-statistics are lower, but still highly significant.

[13] We have also considered more parsimonious specifications without our fixed effects. In this case, the impact of all of our peer groups is significantly stronger. We also find that the introduction of sociodemographic control variables reduces the impact of neighborhood peers disproportionately, suggesting that the zip code proxies for co-worker interaction and other omitted variables.

industries. Additionally, we decompose our investor observations (for the entire sample) according to industry. No single industry accounts for more than 11.37% of our investor observations.[14]

It could also be the case that the similarity in trading that we observe is to some extent driven by common responses to media. In Norway there are 429 municipalites (kommuner) and even local media serves multiple municipalities and therefore introducing municipality$\times$month fixed effects controls for common media shocks. In unreported analysis we find that the introduction of these fixed effects does not qualitatively change the co-worker peer effect.[15]

3.1 New and Former Co-workers

In this section we consider the evolution of peer effects when the investor changes place of work. Our data contains the end date of employment at the old plant and the start date of employment at the new plant (see Huttunen, Møen and Salvanes, 2009, for a complete description of the data). This allows us to examine whether peer effects of old (new) peers is decreasing (increasing) over time following a move.

For an investor move to be included in our analysis we require that the termination and start date are both non-missing. At the time of the move, we require that the investor did not change place of employment in the preceding year or does not change place of employment in the next year. Additionally, we require that the investor moves at most four times between 1993 and 2005. Finally, we require that the start date at the new place of work is later than the stop date at the previous plant. Applying, the above criteria leaves us with roughly $45,000$ investor moves. Of these moves, roughly 40% are investors that just move once, another 40% move twice and the remaining investors move three times (almost no individuals move four times).

To test the relative impact of new and former co-workers we interact our explanatory

[14] We have also considered specifications with industry$\times$month fixed effects. This does not affect the co-worker peer effect qualitatively suggesting that industry trade journals and the like are not driving our results.

[15] Additionally, we have considered whether socio-demographic variables are related to the strength of co-worker peer effects. We find that co-workers exert a greater influence on males, but that there is no conclusive relation between age or the level of education and the size of the peer effect.

variables, the fraction of old (buy^{old}) and new (buy^{new}) co-workers that make a purchase, with dummy variables that takes the value 1 if the date is before / after the stop / start at the old / new plant and 0 otherwise. For example, the variable $buy_t^{old\ before}$ is the fraction of old co-workers that make a purchase prior to the investor leaving the plant and after the termination date the variable takes a value of 0. Our basic regression estimates

$$buy_{i,t} = \alpha + \beta_1 \times buy_t^{old\ before} + \beta_2 \times buy_t^{old\ after} + \beta_3 \times buy_t^{new\ before} + \beta_4 \times buy_t^{new\ after} + \mathbf{b\Gamma} + \varepsilon_{i,t}$$

where the vector of control variables $\mathbf{\Gamma}$ contains our family (buy_t^{family}) peers, neighbor (buy_t^{zip}) peers and socio-demographic control variables that we control for in Table 2. We include month, plant and zip code fixed effects. We also include postcode-plant interaction fixed effects.

Our results are presented in Table 3. In specification (1) we consider the change in the peer effect of old co-workers when the individual leaves the plant. For each individual, we restrict the sample to 12 months before the individual leaves the plant to 12 months after leaving. The purchases of former co-workers is positively related to the purchases before and after the move. However, as expected the effect of former co-workers is greater before the move than after the move (the point estimate drops from 0.270 to 0.159). The F test statistic for a difference between β_1 and β_2 is 16.71 and significant at the 1% level.

In specification (2) we consider the change in peer effect of new co-workers when the individual joins the new plant. We restrict the sample to 12 months before the individual joins the new plant to 12 months after the start date.[16] We find that the purchasing decision of new co-workers is positively related to the individual's trading decision. As expected, the impact of new co-workers increases substantially (the point estimate increases from 0.116 to 0.332) when the individual joins the new plant. The F statistic for a difference between β_3 and β_4 is 97.55 and statistically significant at the 1% level.

Finally, in specification (3) we simultaneously include the effect of old and new co-workers before and after the individual leaves (joins) the old (new) plant. For each

[16] Note that this sample is different from the sample in specification (1) since for most individuals there is some time inbetween leaving the old plant and joining the new plant.

investor, we restrict the sample to 12 months before the investor joins the new plant and to 12 months after the investor leaves the old plant.

The results in this specification mirrors those in the two previous specifications. Firstly, the effect of co-workers at the old plant is substantial, but significantly reduced following the move. Secondly, the effect of new co-workers is substantially increased once the individual has joined the new plant. Additionally, this specification allows us to compare the impact of individuals that previously have been co-workers to the effect of those individuals that will become co-workers. The point estimate of $buy_t^{old\ after}$ is 0.142 while the point estimate of $buy_t^{new\ before}$ is 0.0545. This suggests that the individual interacts with old co-workers even after the individual has left the plant, but that the interaction with future co-workers is limited.[17,18]

It is possible that our results could be driven by common liquidity shocks induced by severance packages. To control for this possibility we include dummies for the number of months before leaving from the old plant (in specifications 1 and 3), and dummies for the number of months prior to joining the new plant (specifications 2 and 3). Thus, these fixed effects will capture any extra buying intensity in the month after leaving the old plant due to the individual investing his severance package (or buying less due to job uncertainty).

Next, we examine the evolution of the effect of peers surrounding the move. To do this we introduce one dummy variable for each of 12 months before to 12 months after the investors leaves (joins) the old (new) plant. We interact our dummy variables with $buy_t^{plant\ old}$ to examine how the influence of peers evolve surrounding the move. We

[17]Interestingly, we find that future co-workers ($buy_t^{new\ before}$) affect the decision to make a purchase of the investor. This is likely driven by the investor interacting with his future colleagues and perhaps even acquiring his future job through these interactions.

[18]In unreported analysis, we follow Nanda and Sørensen (2010) and consider the impact of placebo peers on the decision to purchase stock. A placebo peer is someone that during the previous year moved away from the plant to which the investor moved to in this year. That is, the investor and the peer have both been exposed to plant specific effects, but they have never overlapped at the same plant. As expected, we find no relation between the purchases of placebo peers and the purchase decision of investors.

estimate the following regression

$$buy_{i,t} = \alpha + \sum_{i=-12}^{12} \beta_i \times buy_t^{plant\ old} \times \gamma_j + \mathbf{b\Gamma} + \varepsilon_{i,t} \tag{2}$$

The vector $\mathbf{\Gamma}$ contains family and zip peer control variables with coefficients. Additionally, we include plant fixed effects and time dummies. We run one separate regression for the old plant and the new plant.

In Figure 1 we have plotted the interacted peer coefficient against the number of months before the move. For the new plant, there is a striking increase in the effect of new co-workers following the move. Additionally, the effect of co-workers at the old plant drops significantly after the move. [19]

3.2 Local and Expertise Trades

There is substantial evidence in the literature that investors show a preference for local stocks (for example Coval and Moskowitz, 1999 and Huberman, 2001). Additionally, Døskeland and Hvide (2011) document that investors show a preference for investing in stocks in industries that they have expertise in. In this section, we verify that our results are not driven by investor preferences for local or expertise stocks. Additionally, we consider whether the impact of peers is different for local and expertise stocks.

We classify all stocks as being local to the individual if the distance from place of residence of the individual to the stock headquarters is less than 100 km. We draw on Døskeland and Hvide (2011) in defining expertise stocks. For each individual employed in the private sector, our dataset contains an employer two-digit NACE code at year-end. For each stock on the OSE, we have the primary NACE codes at year-end from 1996 to 2005. We define an expertise stock as a stock where the worker two-digit NACE code matches the NACE code of the stock.

[19] Examining the months that investors move shows that investors predominantly leave one job in December and join a new one in January. The results in Table 3 includes investors that leave in December and join in January while the results used to produce Figure 1 excludes these movers. Taken together these result indicate that our results are robust to the fact that investors pre-dominantly move at the turn of the year.

In order to examine local and expertise purchases we create four dummy variables, buy_{local}, $buy_{non-local}$, $buy_{expertise}$ and $buy_{non-expertise}$ that take the value 1 if the stock being purchased is local, non-local, expertise or non-expertise respectively. We estimate (1) using our dummy variables as dependent variables. Specification (1) of Table 4 considers only local buys (i.e., uses buy_{local} as the dependent variable), while specification (2) considers non-local buys. Specifications (3) and (4) consider expertise and non-expertise buys respectively. We include the same socio-demographic variables as in Table 2, month, plant and postcode fixed effects. We also include zip code plant interaction fixed effects.

In all four specifications, all three peer groups are statistically significantly related to the decision to make a stock purchase at the 1% level. It is noteworthy that the impact of co-workers is greater for expertise stocks than for non-expertise stocks. A one standard deviation increase in the fraction of co-workers that make a purchase results in an increase in the probability that the investor makes an expertise (non-expertise) purchase by 0.47% (0.43%), which represents a 248% (35%) change compared to the unconditional mean. This suggest that co-workers are particularly important for the decision to purchase of stock from the same industry as the investor works in.[20]

4 Stock Selection

The decision to purchase a particular stock can be seen as two consecutive decisions; first the decision to make a stock purchase and second the decision to purchase a particular stock. In this section we consider the relation between investor stock selection and the stock selection decisions of co-workers, family and neighbors.

To do this, we consider as dependent variable, $f_{i,t,s}$, which is the fraction of total purchases by investor i in month t invested in stock s. As main independent variables we use the fraction of total purchases invested in stock s in month t by co-workers ($F_{t,s}^{plant}$), family ($F_{t,s}^{family}$) and neighbors ($F_{t,s}^{zip}$). Descriptive statistics of our stock selection variables are presented in Table 5. The mean fraction of total purchases invested in a stock is 0.48% which makes intuitive sense since there are roughly 200 stocks on the Oslo Stock Exchange

[20] The impact of co-workers is similar for local (46% relative to the unconditional mean) and non-local stocks (47%).

over our sample period.

To relate investor stock selection to peer stock selection we estimate the following regression,

$$f_{i,t,s} = \alpha + \beta_1 F_{t,s}^{plant} + \beta_2 F_{t,s}^{family} + \beta_3 F_{t,s}^{zip} + Stock \times Month + \varepsilon_{i,t,s} \quad (3)$$

where $Stock \times Month$ is a stock month fixed effect to control for the economy-wide average fraction invested in stock s in month t. The coefficients β_1, β_2, and β_3 tells us what is the relation between purchases of the peer group and purchases of the individual investor when the peer group and the investor make purchases. Since, we are considering purchases by the individual and the peer group we are effectively conditioning on both the investor and the peer group being active.[21] If we did not condition on making purchases our coefficient β_1 would combine the effects of being active and stock selection. When estimating (3) we therefore require that both the investor makes a purchase in that month and that his peers make a purchase in the same month. In the year 2000, this leaves us with a sample of $2,824$ individuals. It turns out that the restrictive selection criteria is that at least one member of the family makes a purchase in that month.[22] Footnote 23 describes our results when considering a sample that abstracts from family peers (the sample is increased 14.5 times) to make a purchase.

Our regression results are presented in Table 6. Specifications (1) to (3) considers a single peer group, while specifications (4) to (6) considers two peer groups simultaneously. Specification (7) includes all three peer groups. In all specifications we include month×stock fixed effects to control for time-varying economy wide buying pressure.

It is noticeable that introducing all three peer groups reduces the impact of neighbors by 56.55% (comparing specification (3) to (7)), while the impact of the other two groups are less affected by the introduction of the other two groups. Considering specification (7) we can benchmark the relative impact of the three different peer groups on

[21] We consider buys rather than trade in a particular stock since very few individual investors go short in a stock and therefore the selling decision is limited to a very small subset of stocks that is already owned by the investor.

[22] The requirement that all peer groups have undertaken a trade implies that these individuals in general belong to larger peer groups than the individuals in the trade analysis, but we have compared the socio demographic variables of the two groups of individuals and in general they are similar.

stock selection. A one standard deviation increase in co-worker purchases of a particular stock increases the investor purchases by 0.96% (or 198% relative to the unconditional mean). The corresponding numbers for family peers are astounding, a one standard deviation increase in family purchases of a stock increases investor purchases by 1.97% or 404.41% relative to the unconditional mean. A one standard deviation increase in neighbor purchases of a stock results in the investor increases the fraction invested in the stock by 0.33% or 69.55% relative to the unconditional mean. Additionally, when considering specification (7) the *Adjusted* R^2 is 0.263 indicating that peer effects and stock month fixed effects explain a significant proportion of the stock selection decision. However, the difference in explanatory power between specification (4) and specification (7) is negligible (0.002) indicating that adding neighbors to co-workers and family negligibly increases the explanatory power. Therefore, it may be the case that peer effects documented in the existing literature using zip code are predominantly driven by social interaction with family and co-workers.

A potential explanation of our results is that workers at a particular plant have a preference for a particular stock for reasons other than social interaction. For example, this could be due to the plant using products or services of that particular company. In specification (8) we introduce plant×stock, zip×stock and plant×zip×stock dummies to control for stock preferences at the plant and the zip level. The introduction of these fixed effects captures a substantial amount of variation, the *Adjusted* R^2 has increased to 0.695 from 0.263. The point estimate of the co-worker peer effect is reduced to 0.0777, however the point estimate is still significant at the 1% level. Now the economic effect is reduced to 77% relative to the unconditional mean.[23]

Overall, the findings of Table 6 suggest that peer effects are important for stock selection and even though all of the three peer group are important it is evident that co-workers and family reduce the importance of neighbors. Additionally, the astounding economic impact of family suggests that it is important to control for the influence of family when considering peer effects.

[23] We re-estimated specification (5) on an expanded dataset that does not require that a family member has made a purchase (resulting in 92, 440, 849 observations). The co-worker coefficient ($F^{plant}_{t,s}$) is 0.309 and the neighbor coefficient ($F^{zip}_{t,s}$) is 0.121, both which are similar to what we found in the above analysis.

4.1 Changes in Place of work

In this section we consider the effect of former and new co-workers on the stock selection decision after the investor shifts plant. To examine movers in the trade analysis we required that the investor shifts plant and at least one member of all of his peer groups makes a purchases during the entire sample period. To consider all peer groups in the stock analysis we would require the investor to shift plant and that at least one investor in all of the peer groups trade in that particular month. This would result in a severely limited sample and therefore we exclude family peers from this section.

We consider the same investor moves as we did in the trade analysis. That is, we require (i) that the end and start date are both non-missing, (ii) the investor did not change place of employment in the preceding year or does not change place of employment in the next year, (iii) the investor moves at most 4 times between 1993 and 2005, and finally (iv) the start date at the new place of work is later than the stop date at the previous plant.

In this analysis our main explanatory variables are the fractions invested in stock s in month t by old ($F_{s,t}^{old}$) and new ($F_{s,t}^{new}$) co-workers, respectively. We interact these variables with four dummy variables that take the value 1 if the date is before / after the stop / start at the old / new plant and 0 otherwise. For example, the variable $F_{s,t}^{old\ before}$ is the fraction invested in stock s by old co-workers prior to the investor leaving the plant and after the departure date the variable takes a value of 0. Our basic regression estimates

$$f_{i,t,s} = \alpha + \beta_1 F_{s,t}^{old\ before} + \beta_2 \times F_{s,t}^{old\ after} + \beta_3 \times F_{s,t}^{new\ before} + \beta_4 \times F_{s.t}^{new\ after} + \beta_5 \times F_{t,s}^{zip} + \varepsilon_{i,t,s}$$

Our results are presented in Table 7. In specifications (1) we examine the impact of old co-workers before and after the change in workplace. The stock selection of former co-workers is positively related to the investor's stock selection before and after the move. In specification (2) we consider the impact of co-workers at the new plant before and after the investor has started to work at the plant. The impact of new co-workers is significantly greater (an F statistic of 40.15) after the investor has started at the plant, suggesting that increased interaction between individuals leads to similar stock selection decisions. In specification (3) we combine the effects past and new co-workers. The effect of current co-workers (at the old or the new plant) is roughly twice that of the effect of past and future co-workers (F tests of the difference in the coefficients are highly significant).

Similar to the trade section, we examine the evolution of the relation between co-worker stock selection and investor stocks selection. We estimate the following regression

$$f_{i,t,s} = \alpha + \sum_{j=-12}^{12} \beta_j \times F_{t,s}^{plant\ old} \times \boldsymbol{\gamma}_j + \beta_{25} \times F_{t,s}^{zip} \times + \varepsilon_{i,t,s} \tag{4}$$

where γ_j is a dummy variable that takes the value of 1 if it is j months until the investor leaves the plant and otherwise 0. Therefore interacting $F_{t,s}^{plant\ old}$ with γ_j implies that we can evaluate the impact of old (new) co-workers from 12 months before the move to 12 months after the move. We run one separate regression for the old plant and the new plant. Additionally, we include stock-time fixed effects and time dummies.

In Figure 2 we have plotted the interacted peer coefficient against the number of months before the move. The effect of old co-workers falls gradually following the departure from the old plant and the effect of new co-workers increases rapidly following the individual's start at the new plant.

4.2 Local and Expertise Trades

In this section we consider how stock selection by our peer groups relates to stock selection of the investor when the stock is either local to the investor or in expertise area of the investor. To do this we use the classification of local and expertise stocks introduced in section 3.2. Panel B of Table 5 presents descriptive statistics of the fractions invested in local and expertise stocks by the individuals in our sample. We find evidence of local and expertise bias. The mean fraction of purchases invested in expertise stocks 1.30% compared to 0.46% for non-expertise stocks. Similarly, the mean fraction invested in local stocks is 0.68% while the mean fraction invested in non-local stocks is 0.39%.

In Table 8 we estimate (3) for local, non-local, expertise and non-expertise stocks in specifications (1) to (4). We include month×stock, plant×stock, zip code×stock and zip code×plant×stock fixed effects in all specifications. It is reassuring to see that the impact of peer effects on stock selection is always statistically significant at the 1% level.

The economic impact of co-workers is significant in all specifications, a one standard deviation increase in co-worker purchases results in an increase in the individual's allocation to stock s by at least 58.57% relative to the unconditional mean. It is noteworthy that the fit of specification (3) (expertise stocks) is significantly larger than the other specifications suggesting that peer effects are particularly important for explaining the selection of stock in industries that the investor has expertise in. Overall our results suggest that it is not local bias or expertise bias that is driving our results.

5 Should you listen to your co-workers?

In this section we investigate whether peer purchases are associated with abnormal performance. We use the adapted calendar time methodology introduced by Hoechle, Schmid and Zimmerman (2009) that allows for the introduction of continuous investor characteristics. We consider a one month formation period, implying that we consider the return of all buys made over the previous month and consider the return of these purchases over the next month.[24] A stock may have been purchased several times during the portfolio formation period. If so, each purchase generates a separate position in the portfolio. Each position is weighed equally. We estimate the following regression,

$$r_{s,t} = \alpha + \beta_1 \times buy_{t-1}^{plant} + \beta_2 \times buy_{t-1}^{family} + \beta_3 \times buy_{t-1}^{zip} + \mathbf{b\Gamma} + \varepsilon_{i,t}$$

where $r_{s,t}$ is the excess return of stock s in month t over the three month Norwegian Interbank Offered Rate (NIBOR). Our explanatory variables buy_{t-1}^{plant} , buy_{t-1}^{family} and buy_{t-1}^{zip} are the fraction of co-workers, family and neighbors that make a purchase in the formation month. Our vector of control variables $\mathbf{\Gamma}$ includes the risk factors $MRKT$, HML, SMB and the Carhart (1997) momentum factor (all of them calculated for Norway).[25]

Our regression results are presented in Table 9. In all of our specifications, purchases by co-workers are not associated with abnormal investor performance. However, there seems to a negative relation between neighbor purchases and the performance of the

[24] We have also considered formation periods of 1 and 12 months, the results are qualitatively very similar.

[25] We are grateful to Bernt Arne Ødegaard for providing the factors. The factor data is described in Ødegaard (2009).

individual. This suggests that investing when local sentiment is positive is detrimental to performance. Specifications, (2), (3) and (4) verify that there is no significant abnormal performance for local or expertise purchases.

We have verified (in unreported analysis) that the lack of abnormal performance associated with peer purchases is not due to the inclusion of risk factors, sentiment purchases or the one month formation period.[26]

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[26]The results are qualittatively unchanged with four and 12 month formation periods.

6 Conclusion

Portfolio theory predicts that investors should invest in risky assets according to the weight that these assets represent in the market portfolio. Although portfolio theory is of considerable normative value, the literature has documented an abundant number of empirical deviations.[27] Motivated by the social psychology literature – which emphasizes the strength of face-to-face communication between individuals that frequently interact in affecting behavior, we ask whether co-workers can explain some of the unexplained portion of stock market behavior by individuals. To examine this question, we use an exceptionally detailed data set from Norway that combines matched employer-employee panel data with common stock transaction data over a 10-year period. Our results suggest large effects of co-workers both on the decision to purchase a stock and the decision which stock to purchase. The results are robust to accounting for unobserved plant, geographical, stock and time heterogeneity at the plant, geographical, and stock level. They are also robust to including measures of peer effects at the geographical and family level. In sum, our results provide strong evidence that individuals' stock purchase decisions are related to those made by their co-workers due to social interaction.

A methodological innovation of the paper is to examine three types of peer effects simultaneously: in the workplace, in the family, and in the neighborhood. This allows us to control for family and neighborhood peer effects in our analysis of workplace peer effects. It also allows us to compare the magnitude of the different types of peer effects and perform a "horse race" between them. We find that neighborhood effects can be important, consistent with Ivkovic and Weissbenner (2007), but obtain more unequivocal affirmative results for the effects of social interaction in the workplace and, less surprisingly, in the family.

Our results contribute to an ongoing debate on the role of social interaction in the stock market. While the literature on institutional investors finds strong evidence of social interaction and network effects among institutional investors, the evidence on social interaction among individual investors has been more mixed. We consider a novel channel

[27] For example, individual investors are biased in favour of domestic stocks (French and Poterba, 1991), local stocks (Coval and Moskowitz, 1999 and Huberman, 2001) and stocks from their industry of employment (Døskeland and Hvide, 2011).

for peer effects; the workplace, and contribute to the literature by showing that co-worker investment decisions can explain a substantial amount of heterogeneity in individual asset allocation decisions. As discussed in Glaeser, Sacerdote, and Scheinkman (2002), it is often important for policy purposes to separate individual and social multiplier effects, the reason being that the aggregate impact of intervention may be larger than the sum of its effects on each individual's decision. Consistent with the findings of Duflo and Saez (2003) in the context of retirement plans, our results suggest that the social multiplier in the workplace is large both in absolute and relative terms when it comes to stock market behavior.

Our findings suggests the possibility that puzzling voluntary investments in company stock (e.g., Benartzi, 2001) may be partially driven by peer effects at the firm level: one obvious object of workplace conversations is the stock market performance of the employer's stock. An interesting question for future work is whether investments in own-company stock relates to proxies for social interaction among employees, such as the amount of time employees spend together outside work, or the extent to which leisure time interests such as reading novels or going to the gym are shared.

Kedia and Rajgopal (2012) find evidence consistent with social interaction between firms in the adoption of stock option grants. Another extension of the current work would be to investigate whether social interaction effects between plants also exist for the decision to purchase stocks and the decision which stock to purchase.[28] For example, it would be interesting to see whether trading behavior is correlated between plants in the same industry and region. This research might be one step in the direction of better understanding contagion effects in stock markets.

[28] In future work, we would also be interested in examining whether intra-plant and inter-plant social interaction can explain patterns in stock market participation. The results of Brown et. al (2008) and Kaustia and Knüpfer (2012) suggest that this is a promising avenue for research.

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Figure 1: New and former co-workers

Figure 1: The above figure plots the slope coefficients from regression equation (2). We run a regression where the dependent variable is the dummy variable Buy that takes the value 1 if the investor makes a purchase in that month and 0 otherwise. Our main independent variables is the fraction of old (new) co-workers that make a purchase in month t interacted with 25 dummy variables, one for each of the 12 months prior to and after leaving (joining) the old (new) plant. We average two consecutive coefficients and we exclude investors that leave their job in December and join the new plant in January.

Figure 2: Stock selection new and former co-workers

Figure 2: The above figure plots the slope coefficients from regression equation (4). We run a regression where the dependent variable is, $f_{t,s}$, the fraction invested by the investor in stock s in month t. Our main independent variables is the fraction invested in stock s in month t of old (new) co-workers interacted with 25 dummy variables, one for each of the 12 months prior to and after leaving (joining) the old (new) plant. We average two consecutive coefficients and similar to Figure 1 we exclude investors that leave their job in December and join the new plant in January.

Appendix

Table A1: Definitions of Regression Variables

Variable	Description of Variable
Trade Variables (monthly)	
buy	Takes the value 1 if the investor makes a stock purchase otherwise 0.
buy^{plant}	The fraction of co-workers that make a purchase.
buy^{family}	The fraction of family members that make a purchase.
buy^{zip}	The fraction of neighbors living in the same zip code that makes a purchase.
Stock Selection Variables (monthly)	
f	The fraction of total investor purchases invested in stock s.
F^{plant}	The fraction of total co-worker purchases invested in stock s.
F^{family}	The fraction of total family purchases invested in stock s.
F^{zip}	The fraction of total neighbor purchases invested in stock s.
Individual-Stock Variables (yearly)	
Local stock	A dummy variable that takes the value 1 if the headquarters of the stock is located within 100km of the place of residence of the investor, otherwise 0.
Expertise stock	A dummy variable that takes the value 1 if the investor's two digit NACE code of employment matches the two digit NACE code of the stock, otherwise 0.
Socio-demographic Control Variables (yearly)	
Income	The yearly income as reported in the individual's tax return. Reported in Norwegian Kroner.
Wealth	The total wealth reported in the individual's tax return for the year. Reported in Norwegian Kroner.
Age	Investor age at the end of the year.
Male	A dummy variable that takes the value 1 if the individual is male and 0 otherwise.
Education	The number of completed years of schooling.

Appendix

Table A2: Descriptive Statistics of Peer groups and Socio-demographic Variables in 2000

This table presents descriptive statistics on the individuals that are in our sample in year 2000. The rows plant size, family size and zip size present descriptive statistics on the size of the individual's plant, family and zip code respectively. The rows Plant investors, Family investors and Zip investors presents descriptive statistics on the number of peers in the individuals respective peer groups (i.e., individuals that trade at least once over the period 1994 to 2005). Additionally, we provide descriptive statistics on the socio-demographic variables wealth, income, age, male and education.

Variable	Mean	Median	Std. Dev.	Min	Max	N
Plant size	356.15	50	911.95	2	5811	170,499
Family size	7.41	6	5.05	2	120	170,499
Zip size	3242.18	2428	2521.25	4	12671	170,499
Plant investors	153.58	17	414.12	1	2465	170,499
Family investors	2.57	2	1.95	1	42	170,499
Zip investors	454.56	325	400.54	1	2239	170,499
Wealth (NOK)	563,028.00	278,630	6,956,396.00	10,000	2,127,096,064	170,499
Income (NOK)	353,904.00	312,700	221,713.00	11,100	32,798,800	170,499
Age	38.56	38	8.50	21	65	170,499
Male	0.71	1	0.46	0	1	170,499
Education	12.81	12	3.02	0	21	170,499

Appendix

Table A3: Industry Decomposition of Investors, Firms and Co-worker Peer effects

This table presents descriptive statistics on the industries that our investors work in (column 2) and the industries that are represented on the Oslo Stock Exchange (column 3). Additionally, we decompose the co-worker peer effect depending on the industry of employment of the investor. Financial firms, NACE codes 65, 66 and 67 have been excluded from the sample. For this table, we only consider industries that represent at least 0.5% of investor observations (i.e., the industry has at least roughly 1,600 investors). This restriction implies a loss of less than 6% of the complete sample. To decompose the co-worker peer effect across industries we estimate the following regression

$$buy_{i,t} = \alpha + \sum_{j=1}^{36} \beta_j \, buy_t^{plant} \times I_j + \beta_{37} buy_t^{family} + \beta_{38} buy_t^{zip} + \boldsymbol{b\Gamma} + \varepsilon_{i,t}$$

where I_j is a dummy variable that takes a value of 1 if the investor works in industry j and 0 otherwise. Column 4 reports our point estimates of the peer effect for our 36 industries. The vector $\boldsymbol{\Gamma}$ of control variables includes the socio demographic control variables listed in the caption to Table 2. In addition to time (month), plant and zip fixed effects; we include zip plant interaction fixed effects. Standard errors are clustered at the individual level. T-values are reported in column 5. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. We use a 20% sample of all investor months for which peer groups are defined and socio demographic variables are non-missing.

Industry (NACE code)	Investors	OSE Firms	Coefficient	t-stat
Oil and gas extraction. Oil and gas services (11)	10,327	19	0.257***	(5.85)
Food products and beverages (15)	7,874	4	0.187***	(6.61)
Wood and wood products (20)	2,507	2	0.035	(1.13)
Publishing, printing, reproduction (22)	4,601	5	0.117***	(4.56)
Chemicals and chemical products (24)	3,123	2	0.984***	(36.27)
Rubber and plastic products (25)	1,214	0	0.139***	(2.71)
Other non-metallic mineral products (26)	1,870	2	0.110***	(2.67)
Basic metals (27)	1,380	2	0.286***	(3.16)
Fabricated metal products (28)	3,549	1	0.060**	(2.05)
Machinery and equipment (29)	5,107	7	0.126***	(5.59)
Electrical machinery and apparatus (31)	2,095	4	0.310***	(5.33)
Radio, TV, communication equip (32)	1,063	7	0.805***	(9.89)
Instruments, watches and clocks (33)	1,537	4	0.077**	(1.98)
Motor vehicles, trailers, semi-tr.(34)	1,366	2	0.896***	(21.86)
Other transport equipment (35)	7,656	2	0.434***	(11.23)
Furniture, manufacturing (36)	1,845	4	0.067*	(1.86)
Electricity, gas and water supply (40)	4,044	3	0.081***	(2.91)
Construction (45)	25,163	2	0.015	(1.64)
Motor vehicle services (50)	7,697	0	0.033**	(2.47)
Wholesale trade, commission trade (51)	23,878	8	0.173***	(14.64)
Retail trade, repair personal goods (52)	14,794	6	0.027**	(2.40)
Hotels and restaurants (55)	5,535	2	0.017	(0.58)
Land transport, pipeline transport (60)	6,612	2	0.043	(1.60)
Water transport (61)	5,211	42	0.474***	(10.81)
Air transport (62)	2,486	2	0.086*	(1.73)
Services for transport and travel agencies (63)	5,406	0	0.099***	(5.10)
Post and telecommunications (64)	7,566	5	0.680***	(24.84)
Real estate activities (70)	3,897	8	0.190***	(8.19)
Computers and related activities (72)	12,060	20	0.263***	(15.45)
Research and development (73)	2,960	3	0.369***	(7.37)
Other business activities (74)	30,674	8	0.101***	(11.18)
Public administration, defense and social security (75)	28,874	0	0.008	(0.67)
Education (80)	24,833	0	-0.011	(-1.13)
Health and social services (85)	35,356	0	-0.017*	(-1.65)
Interest groups (91)	2,857	0	-0.003	(-0.22)
Cultural and sporting activities (92)	3,966	2	0.035*	(1.81)
Total	310,983	180		

Table 1: Descriptive statistics on investor and peer trading

We present descriptive statistics on trading of individuals and their peers. In Panel A, buy is a dummy variable that takes the value 1 if the individual trades in month t, otherwise it is 0. buy^{plant}, buy^{family} and buy^{zip} are the fraction of plant, family and zip code peers that make a stock purchase in month t. In Panel B we consider the individual's trading of local and expertise stocks (used in Table 3). $buy^{expertise}$, $buy^{non\text{-}expertise}$, buy^{local} and $buy^{non\text{-}local}$ takes the value 1 if the individual purchases an expertise, non-expertise, local and non-local stock in month t, respectively. A local stock is headquartered closer than 100 km to the residence of the individual. An expertise stock is a stock that has the same two digit NACE code as the firm that employs the individual. We use a 20% sample of all investor months for which peer groups are defined and socio demographic variables are non-missing.

Panel A: Trading of individuals and peers

Variable	Mean	Median	Std. Dev.	Min	Max	N
Individual trading						
buy	0.0212	0.0000	0.1440	0	1	4,580,530
Peer trading						
buy^{plant}	0.0197	0.0000	0.0632	0	1	4,580,530
buy^{family}	0.0093	0.0000	0.0714	0	1	4,580,530
buy^{zip}	0.0183	0.0148	0.0163	0	1	4,580,530

Panel B: Individual trading in local and expertise stocks

Variable	Mean	Median	Std. Dev.	Min	Max	N
Individual trading						
$buy^{expertise}$	0.0019	0.0000	0.0436	0	1	4,506,903
$buy^{non\text{-}expertise}$	0.0120	0.0000	0.1092	0	1	4,506,903
buy^{local}	0.0095	0.0000	0.0968	0	1	4,537,550
$buy^{non\text{-}local}$	0.0112	0.0000	0.1052	0	1	4,537,550

Table 2: Peer and Investor Trading

We present results from pooled panel regressions relating investor buys to the fraction of peers that buy in month t. The dependent variable is the dummy variable *buy* that takes the value 1 if the investor makes a purchase in that month and 0 otherwise. buy^{plant} , buy^{family} and buy^{zip} is the fraction of plant, family and zip code peers that make a stock purchase in month t. The socio-demographic variables that we control for are: Age, Age^2, LogIncome, $LogIncome^2$, $LogIncome^3$, LogWealth, $LogWealth^2$, $LogWealth^3$, LogIncome×LogWealth, Male and Education. In addition to time (month), two digit NACE code (of investor plant), plant and zip code fixed effects, we include zip-plant interaction fixed effects. Standard errors are clustered at the individual level (t-values are reported in parentheses). *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. We use a 20% sample of all investor months for which peer groups are defined and socio demographic variables are non-missing. Variables are described in Appendix.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	buy	buy	buy	buy	buy	buy	buy
buy^{plant}	0.181***			0.176***	0.177***		0.171***
	(45.89)			(44.67)	(45.39)		(43.65)
buy^{family}		0.0892***		0.0820***		0.0877***	0.0823***
		(36.57)		(33.87)		(36.09)	(33.82)
buy^{zip}			0.346***		0.280***	0.329***	0.267***
			(23.97)		(20.52)	(22.92)	(19.60)
Constant	1.848***	1.789***	1.811***	1.833***	1.853***	1.794***	0.0006
	(3.77)	(3.64)	(3.68)	(3.74)	(3.77)	(3.64)	(1.12)
Socio demographic	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Plant FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zip FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	4,580,530	4,580,530	4,580,530	4,580,530	4,580,530	4,580,530	4,580,530
Adj. R^2	0.207	0.204	0.203	0.208	0.207	0.204	0.204

Table 3: New and Former Co-workers

We examine the relative impact of new and former co-workers before and after the investor leaves (joins) the old (new) plant. To do so, we create two dummy variables that take the value of 1 for months before (after) the investor leaves the old plant and 0 otherwise. Similarly, we create two dummy variables that take the value of 1 for months before (after) the investor joins the new plant and 0 otherwise. We interact these four dummy variables with buy_t^{plant} to generate the independent variables $buy_t^{old\ before}$, $buy_t^{old\ after}$, $buy_t^{new\ before}$ and $buy_t^{new\ after}$. We estimate the OLS regression:

$$buy_{i,t} = \alpha + \beta_1 buy_t^{old\ before} + \beta_2 buy_t^{old\ after} + \beta_3 buy_t^{new\ before} + \beta_4 buy_t^{new\ after} + \beta_5 buy_t^{family} + \beta_6 buy_t^{zip} + \boldsymbol{b\Gamma} + \varepsilon_{i,t}$$

where $\boldsymbol{\Gamma}$ includes the socio demographic variables listed in the caption to Table 2. In addition to month, plant and zip code fixed effects; we include zip×plant fixed effects. We also include dummies for the number of months before leaving from old job (*time prior leaving*), and dummies for the number of months prior to joining new job (*time prior joining*). There is one dummy variable for each month starting from 12 months before the investor leaves (joins) the old (new) plant to 12 months after (month 0 is omitted). Standard errors are clustered at the individual level (t-values in parentheses). *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)
	buy	buy	buy
$buy_t^{old\ before}$	0.270***		0.340***
	(14.40)		(10.37)
$buy_t^{old\ after}$	0.159***		0.142***
	(7.41)		(6.49)
$buy_t^{new\ before}$		0.116***	0.0545***
		(6.59)	(2.79)
$buy_t^{new\ after}$		0.332***	0.386***
		(20.89)	(15.38)
buy_t^{family}	0.0750***	0.0725***	0.0761***
	(11.37)	(10.28)	(8.15)
buy_t^{zip}	0.152***	0.126***	0.113***
	(5.79)	(4.84)	(3.04)
Constant	Yes	Yes	Yes
Socio demographic	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Plant FE	Yes	Yes	Yes
Zip FE	Yes	Yes	Yes
Time prior leaving FE	Yes	No	Yes
Time prior joining FE	No	Yes	Yes
N	623,384	623,707	330,443
Adj. R^2	0.296	0.291	0.300

Table 4: Trading of Local and Expertise Stocks

We investigate the relation between the fraction of peers making a purchase and investor purchases. We estimate the same regression as in Table 2, but consider different dependent variables. In (1), the dependent variable is a dummy variable that takes the value 1 if the investor buys a local stock (stocks headquartered closer than 100 km to the individual). In (2), the dependent dummy variable takes the value 1 if the investor purchases a stock that is not local. In (3), the dependent dummy variable takes the value 1 if the investor purchases a stock that he has expertise in (defined as in Døskeland and Hvide, 2011), while in (4) the dummy variable takes the value 1 if the individual purchases a stock that he does not have expertise in. The socio demographic variables that we control for are listed in the caption to Table 2. In addition to month, plant and zip fixed effects, we include zip×plant fixed effects. Standard errors are clustered at the individual level (t-values in parentheses). *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. We use a 20% sample of all individual months for which peer groups are defined and socio demographic variables are non-missing. Variables are described in Appendix.

	(1) Local	(2) Non-local	(3) Expertise	(4) Non-expertise
buy^{plant}	0.0825***	0.0775***	0.0760***	0.0688***
	(32.57)	(26.30)	(32.69)	(24.15)
buy^{family}	0.0381***	0.0411***	0.00389***	0.0432***
	(24.19)	(22.46)	(5.06)	(23.81)
buy^{zip}	0.156***	0.101***	0.0900***	0.0854***
	(20.12)	(8.76)	(10.16)	(10.36)
Constant	Yes	Yes	Yes	Yes
Socio demographic	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Plant FE	Yes	Yes	Yes	Yes
Zip FE	Yes	Yes	Yes	Yes
N	4,537,550	4,537,550	4,506,903	4,506,903
Adj. R^2	0.205	0.160	0.114	0.204

Table 5: Descriptive statistics on investor and peer stock selection

In Panel A we present descriptive statistics on the stock selection decision of individuals and peers. *f* is the fraction invested by investor i in stock s in month t. F^{plant} , F^{family} and F^{zip} is the average fraction invested in stock s in month t by plant, family and zip code peers respectively. In Panel B we examine individual stock selection of expertise, non-expertise, local and non-local stocks (examined in Table 8). A local stock is headquartered less than 100km from the residence of the individual. Expertise stocks have the same two digit NACE code as the employer of the individual.

Panel A: Individual and peer stock selection

Variable	Mean	Median	Std. Dev.	Min	Max	N
Individual stock selection						
f	0.0049	0	0.0609	0	1	6,522,816
Peer stock selection						
F^{plant}	0.0049	0	0.0479	0	1	6,522,816
F^{family}	0.0049	0	0.0596	0	1	6,522,816
F^{zip}	0.0049	0	0.0319	0	1	6,522,816

Panel B: Stock selection of local and expertise stocks

Variable	Mean	Median	Std. Dev.	Min	Max	N
Individual stock selection						
$f^{expertise}$	0.0130	0	0.1042	0	1	177,827
$f^{non\text{-}expertise}$	0.0046	0	0.0592	0	1	6,344,989
f^{local}	0.0068	0	0.0729	0	1	2,102,794
$f^{non\text{-}local}$	0.0039	0	0.0543	0	1	4,420,022

Table 6: Peer Effects and Stock Selection

We present the results of pooled panel regressions relating the fraction of purchases invested in a particular stock by the investor to the fractions invested in that stock by the investor's peers. The dependent variable f is the fraction of total purchases invested in stock s in month t by the investor. F^{plant} , F^{family} and F^{zip} is the average fraction invested in stock s in month t by plant, family and zip code peers respectively. We include month×stock fixed effects in all specifications. In specification (8) we also include, plant×stock, zip×stock, and zip×plant×stock fixed effects. Standard errors are clustered at the individual level (t-values in parentheses). *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Variables are described in Appendix.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	f	f	f	f	f	f	f	f
F^{plant}	0.294***			0.209***	0.279***		0.201***	0.0777***
	(57.77)			(46.91)	(56.42)		(45.59)	(9.65)
F^{family}		0.369***		0.335***		0.360***	0.330***	0.212***
		(83.79)		(78.81)		(82.75)	(77.73)	(24.44)
F^{zip}			0.244***		0.177***	0.145***	0.106***	0.0441***
			(37.48)		(34.29)	(29.96)	(24.76)	(5.92)
Constant	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time×Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Plant×Stock FE	No	No	No	No	No	No	No	Yes
Zip×Stock FE	No	No	No	No	No	No	No	Yes
N	6,522,816	6,522,816	6,522,816	6,522,816	6,522,816	6,522,816	6,522,816	6,344,989
Adj. R^2	0.174	0.239	0.141	0.261	0.18	0.243	0.263	0.695

Table 7: Stock Selection, New and Former Co-workers

We examine the relative impact of new and former co-workers before and after the investor leaves (joins) the old (new) plant (as in Table 3). To do so, we create two dummy variables that take the value of 1 for months before (after) the investor leaves the old plant and 0 otherwise. Similarly, we create two dummy variables that take the value of 1 for months before (after) the investor joins the new plant and 0 otherwise. We interact these four dummy variables with the variable $F_{t,s}^{plant}$ to generate the independent variables $F_{t,s}^{old\ before}$, $F_{t,s}^{old\ after}$, $F_{t,s}^{new\ before}$ and $F_{t,s}^{new\ after}$. We estimate:

$$f_{i,t,s} = \alpha + \beta_1 F_{t,s}^{old\ before} + \beta_2 F_{t,s}^{old\ after} + \beta_3 F_{t,s}^{new\ before} + \beta_4 F_{t,s}^{new\ after} + \beta_5 F_{t,s}^{zip} + \varepsilon_{i,t,s}$$

where $f_{i,t,s}$ is the fraction of month t purchases invested in stock s by investor i. F^{zip} is the average fraction invested in stock s in month t by zip code peers. We include month×stock fixed effects. Standard errors are clustered at the individual level (t-values in parentheses). *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Variables are described in Appendix.

	(1)	(2)	(3)
	f	f	f
$F_t^{old\ before}$	0.225***		0.227***
	(27.25)		(13.27)
$F_t^{old\ after}$	0.228***		0.129***
	(13.60)		(7.07)
$F_t^{new\ before}$		0.157***	0.0974***
		(14.38)	(6.98)
$F_t^{new\ after}$		0.230***	0.221***
		(28.89)	(14.40)
F_t^{zip}	0.0836***	0.0810***	0.0687***
	(11.68)	(12.20)	(6.59)
Constant	Yes	Yes	Yes
Time×Stock FE	Yes	Yes	Yes
N	2,117,599	2,630,630	969,292
Adj. R^2	0.173	0.173	0.235

Table 8: Stock Selection of Local and Expertise Stocks

We investigate the relation between the stock selection of peers and the stock selection of investors in local and expertise stocks. The dependent variable $f_{i,t,s}$ is the fraction of total purchases invested in stock s in month t by investor i. F^{plant} , F^{family} and F^{zip} is the average fraction invested in stock s in month t by plant, family and zip code peers respectively. In specification (1), we only consider stocks that are local to the investor (stocks headquartered closer than 100 km to the investor); thus our dependent variable $f_{i,t,s}$ measures the fraction of local purchases invested by the individual in stocks s. In specification (2), we only consider non-local stocks. Specification (3) considers only expertise stocks (defined as in Døskeland and Hvide, 2011), while specification (4) considers non-expertise stocks. We include month×stock, plant×stock, zip×stock, and zip×plant×stock fixed effects. Standard errors are clustered at the individual level (t-values in parentheses). *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Variables are described in Appendix.

	(1) Local	(2) Non-Local	(3) Expertise	(4) Non-Expertise
F_t^{plant}	0.0701***	0.0863***	0.109***	0.0777***
	(7.34)	(8.31)	(3.61)	(9.65)
F_t^{family}	0.211***	0.202***	0.151***	0.212***
	(17.39)	(21.27)	(6.22)	(24.44)
F_t^{zip}	0.0259***	0.0540***	0.0649**	0.0441***
	(2.62)	(5.39)	(1.98)	(5.92)
Constant	Yes	Yes	Yes	Yes
Time×Stock FE	Yes	Yes	Yes	Yes
Plant×Stock FE	Yes	Yes	Yes	Yes
Zip×Stock FE	Yes	Yes	Yes	Yes
N	2,102,794	4,420,022	177,827	6,344,989
Adj. R^2	0.737	0.685	0.860	0.695

Table 9: Returns to Peer Trading

We present regression results relating peer buying pressure to returns. We use calendar time portfolio methodology by applying the Hoechle, Schmid and Zimmerman (2009) implementation which allows for the introduction of continuous investor characteristics. Our dependent variable is the monthly excess return of stock s over the three month Norwegian Interbank Offered Rate (NIBOR). Our main independent variables are buy_{t-1}^{plant}, buy_{t-1}^{family}, buy_{t-1}^{zip}, the fraction of co-workers, family members and neighbors that make a purchase in month t-1, respectively. All specifications include the factors MRKT, HML, SMB and the Carhart (1997) momentum factor (all of them calculated for Norway). Local stock and Expertise stock are dummy variables that take the value 1 if the stock is local (headquarters within 100 km) to the investor or a stock that the investor has expertise (same two digit NACE code) in, respectively. T-values are based on Driscoll and Kraay (1998) standard errors and reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Variables are defined in the Appendix.

	(1) return	(2) return	(3) return	(4) return
$\mathrm{buy}_{t-1}^{\mathrm{plant}}$	-0.00372	-0.00309	-0.00726	-0.00124
	(-1.30)	(-1.07)	(-0.73)	(-0.38)
$\mathrm{buy}_{t-1}^{\mathrm{family}}$	-0.00174	-0.00197	-0.00771	-0.00157
	(-0.73)	(-0.83)	(-1.15)	(-0.43)
$\mathrm{buy}_{t-1}^{\mathrm{zip}}$	-0.450***	-0.445***	-0.473**	-0.366**
	(-2.66)	(-2.60)	(-2.39)	(-2.05)
Local stock		0.000987	-0.00452	
		(0.38)	(-0.95)	
Expertise stock		-0.00155		-0.00471
		(-0.45)		(-1.06)
Constant	0.00766	0.00761	0.00988	0.00687
	(1.14)	(1.15)	(1.12)	(1.02)
Risk factors	Yes	Yes	Yes	Yes
N	947,891	918,560	45,544	318,665
Adj. R^2	0.215	0.217	0.244	0.247

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