Stock Investments at Work*

Hans K. Hvide and Per Östberg†

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Abstract

Stock market investment decisions of individuals are positively correlated with that of co-workers. Sorting of unobservably similar individuals to the same workplaces is unlikely to explain our results, as evidenced by the investment behavior of individuals that move between plants. Purchases made under stronger co-worker purchase activity is not associated with higher investment returns. Moreover, social interaction appears to drive the purchase of within-industry stocks; an investment mistake. Overall, our results suggest a strong influence of co-workers on investment choices, but not an influence that improves the quality of investment decisions.

Keywords: Individual investors, peer effects, social interaction, investment decisions, stock selection. JEL codes: G02, G11.

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†Hvide is affiliated with University of Bergen, CEPR, and the University of Aberdeen. Email: hans.hvide@econ.uib.no. Östberg is affiliated with the University of Zurich, Department of Banking and Finance and the Swiss Finance Institute. E-mail: per.oestberg@bf.uzh.ch.

1 Introduction

Investment decisions made by individual investors 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, Shiller and Pound, 1989, Shiller, 2000, Hong et al., 2004), lack of data has made it difficult to test for this mechanism. In this paper we use unique data from Norway to examine whether individual investors are affected by their co-workers. We also analyze whether co-worker influence appears to improve the quality of investment decisions.

The social psychology literature emphasizes the strength of face-to-face communication between individuals that frequently interact in producing and altering beliefs.[1] Individuals spend a considerable fraction of their time at the workplace, and even the most efficient firms create opportunities for face-to-face communication. In fact, social interaction is often encouraged by company management. It is plausible that conversations at work occasionally center on the stock market, and that these conversations can influence behavior. For example, investors pick among a dizzying number of individual stocks when evaluating which stock to purchase, and may obtain information from discussions with their colleagues, or make inferences based on hearing about their choices. Or, conversations with colleagues about stocks can raise awareness of or trust in equity markets and make trading more likely (Guiso and Japelli, 2005, Guiso et al., 2008). Duflo and Saez (2003) document strong evidence of workplace interaction in a related context: using a randomized trial they find that social interaction influences the decision to enroll in a tax deferred account retirement plan.[2]

In order to address the influence of co-workers on investment choices, we combine two data sources. The matched employer–employee data (which covers the full population

[1] In a classic study by Asch (1955) individuals alone and in groups compared the lengths of line segments. The lengths were sufficiently different that when responding alone 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.

[2] For suggestive evidence, Shiller (1984) cites surveys from the 1950s and 1960s where the answers to the questions 'Do you own any stocks' and 'Do you have any friends or colleagues who own any stocks' were practically identical.

of Norway) identifies co-workers at plant level (i.e., the same business address) over a ten-year period. We combine the employer-employee dataset with a complete record of common stock transactions made by individual investors at the Oslo Stock Exchange over the same period. We focus on individuals that make at least one purchase of common stocks over the sample period.[3] We omit individual-years where the individual is employed by a listed company or a subsidiary of a listed company to avoid capturing mechanic effects of company stock plans.

Our results suggest strong social interaction effects. For example a one standard deviation increase in the fraction of co-workers that make a purchase in a given month is associated with a more than 34 percent increase in probability of making a purchase. Moreover, conditional on making a purchase, a one standard deviation increase in the fraction of co-workers that purchase a particular stock is associated with a striking 176 percent increase in the fraction invested in the same stock.

Stock purchases could be correlated inside plants for other reasons than social interaction (e.g., Manski, 1993). The literature highlights correlated unobservables, endogenous group membership, and reflection as obstacles for estimation of causal effects.[4] We control for fixed effects in order to address correlated unobservables. For example, plant fixed effects control for unobservables such as company culture, composition of the workforce, and industry affiliation.[5] Other fixed effects control for geographical differences in investment behavior (a preference for local stocks, for example) and of individuals following simple decision rules such as picking stocks based on their recent performance record. On top of this, we control for socio-demographic variables at the individual-year level.

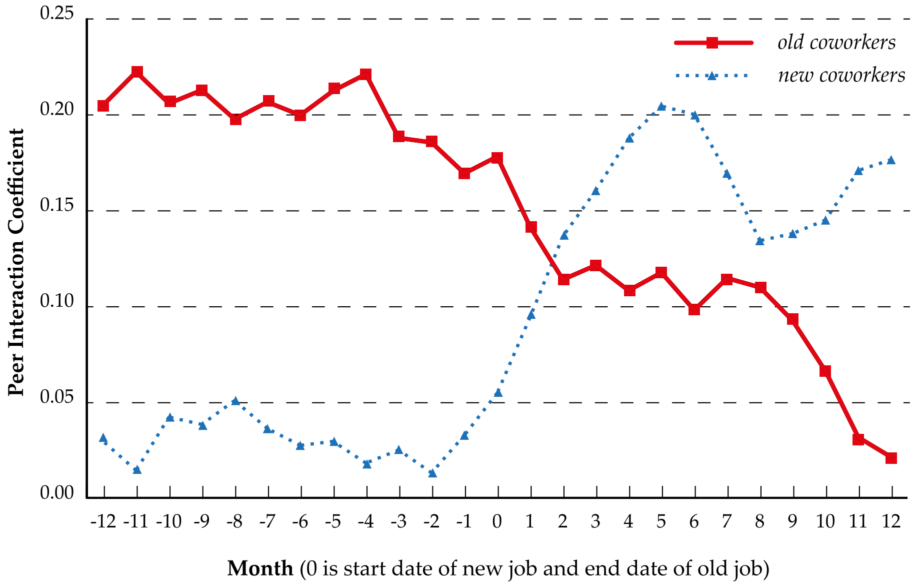
Workers with similar unobserved characteristics, such as risk preferences, access to

[3] In a draft version of the paper we also studied stock market participation and obtained similar results.

[4] These concepts can be illustrated with an example. Suppose that purchases are correlated across individuals in the same plant. The correlation could be due to receiving the same news (correlated unobservables), because they have similar investment style (endogenous group membership) or because of social interaction. Under social interaction, the group affects the individual and the individual affects the group, in which case it is not straightforward to back out the structural parameters of social influence from the estimated correlations. This is the 'reflection problem' of Manski (1993), referred to as the 'simultaneity problem' in Moffit (2001).

[5] These are 'contextual and ecological effects' in the terminology of Manski (1993), which should be contrasted to the endogenous social effects. Lee (2007) and Lee et al. (2010) analyze how fixed effects alleviate the problem of correlated unobservables in identification of endogenous social effects. Blume et al. (2010) surveys the literature.

information, or investment style, could self-select to plants in a pattern not captured by the controls. To address endogenous group membership, we analyze the investment behavior of individuals that move between plants (the data allow us to identify whether these individuals also move from their zip code). The idea is that *future* co-workers are unlikely to influence via social interaction but may still exhibit correlated behavior due to similarity along unobservables. Thus if unobserved similarities drive our results, we expect the correlation with future co-workers to be of comparable magnitude to the correlation with *current* co-workers. The blue line in Figure 1 illustrates how individual purchases relate to purchases made by future co-workers.



In Figure 1, Month 0 is the month when the individual joins the new plant.[6] Up to three months before joining the new plant, the correlation in purchasing activity with future peers is close to zero. In contrast, the correlation with current co-workers, illustrated by the red line, is substantially higher.[7] We discuss Figure 1 further in Section 3.2.

[6]Each point in Figure 1 is an estimated regression coefficient from a pooled regression, as described in Section 3.1. The coefficients can be interpreted as the percentage point increase in the probability to purchase a stock that month given a 100% increase in the purchasing propensity of co-workers for that month.

[7]The numbers on the y-axis should be interpreted as the increase in the predicted probability of making a purchase in that month conditional on 100% of the investor co-workers purchasing a stock that month. See Section 3 for a further discussion of economic magnitudes.

Does social interaction improve the quality of investment decisions? The literature on information cascades (Bikhchandani et al., 1992; Banerjee, 1992; Ellison and Fudenberg, 1993) posits that imitating co-workers can make investment decisions better informed and improve investment returns. Or, individuals can learn investment principles such as diversification and hedging from co-workers. On the other hand, information transmitted at the workplace could be noise or even false rumours, or involve imitation of unsound practices (March, 1991).[8] The welfare implications are obviously quite different.

We address whether social interaction improves investment quality in two ways. Using the calendar time portfolio approach (e.g., Odean, 1999, Seasholes and Zhu, 2010), we analyze whether risk-adjusted investments returns vary with the purchasing 'pressure' of co-workers. We do not find a positive relation; purchases made under strong purchase pressure do not outperform (and sometimes underperform) purchases made under weak purchase pressure. Hence the social interaction effects we document do not seem rooted in value-relevant asymmetric information. Second, the empirical literature has shown that individual investors miss out on opportunities to reduce risk (see Benartzi and Thaler, 2007, and Campbell, 2006, for overviews). One investment mistake that has been abundantly documented is the tendency to hedge poorly against fluctuations in future labor income by holding own-company or own-industry stocks.[9] We analyze whether the impact of co-workers is larger for the purchase of within-industry stocks than for other stocks, and find strong affirmative evidence. This suggests, interestingly, that investment mistakes can be propagated by social interaction.[10]

Overall, our findings suggest that individuals are strongly influenced by their co-workers, but this influence does not improve, and sometimes reduces, the quality of their

[8] An anecdote relayed by Benartzi and Thaler (2007, p.94) in the context of 401(k) pension plan choices by employees in a supermarket chain in Texas provides a nice illustration of this point: "The plan provider noticed that participants' behavior in each supermarket was remarkably homogeneous, but the behavior across supermarkets was fairly heterogeneous. It turns out that most of the supermarket employees considered the store butcher to be the investment maven and would turn to him for advice. Depending on the investment philosophy of the butcher at each individual location, employees ended up being heavily invested in stocks or heavily invested in bonds."

[9] As a stark example, employees of Pfizer, Inc., invest almost 90% of the value of their defined contribution plan in Pfizer common stock, see Cohen (2008).

[10] As shown in Section 5, we do not find evidence that within-industry stock purchases made under stronger peer pressure are associated with higher investment returns.

investment choices. At the normative level, we offer advice to individual investors themselves: listening to co-workers is unlikely to improve the quality of investments.

The paper connects to several ongoing debates. First, much of the existing evidence is based on similarity in trading inside large groups, such as regions or neighborhoods (Hong et al., 2004, Ivković and Weissbenner, 2007, Brown et al., 2008, Kaustia and Knüpfer, 2012).[11] We measure peer groups at a much more local level, and find evidence of strong social interaction effects even after accounting for correlated unobservables, endogenous group membership, and reflection. Our evidence contrasts with Feng and Seasholes (2004), who in a small-group environment (trading rooms in China) do not find evidence of social interaction effects among stock market investors. It also contrasts with Beshears et al. (2011), who find negative co-worker peer effects ("boomerang effects") in the adoption of a simplified 401(k) plan. In contrast, our findings from the analysis of tens of thousands of plants, suggest that individuals do tend to be influenced by their co-workers.

Second, we contribute to our understanding of whether information obtained via informal channels is useful for investors themselves or not. The theoretical literature on information cascades (Bikhchandani et al., 1992; Banerjee, 1992; Ellison and Fudenberg, 1993) posits that information cascades in social groups are (at least on average) rooted in value-relevant information. We fail to find affirmative evidence for this hypothesis; the social interaction effects we document seem rooted in information that is at best noise. We also contribute to the discussion on what explains investment mistakes. While the extant literature attempts to explain investment mistakes with individual characteristics such as IQ, wealth, or income (e.g., Campbell, 2006, Cohen, 2008), we emphasize the role of social interaction in explaining poor hedging; investments in within-industry stocks.[12] Finally, a recent literature addresses the co-movement of aggregate individual investor trading and asset returns (e.g., Kumar and Lee, 2006, Barber et al., 2008). One of the

[11] The same point can be made about much of the literature on social interaction in economics (e.g., Bertrand et al., 2000, Hong et al., 2005, Moretti, 2011). While our focus is social interaction in a naturally occurring group, a large literature in economics considers social interaction effects under randomized group formation (see Dahl et al., 2012, for references). A recent interesting example on investment decisions is Bursztyny et al., (2012).

[12] In the medical literature, Christakis and Fowler (2007) provide evidence consistent with obesity in the U.S. spreading through social interaction. The economics literature emphasizes positive spillover effects; for example, the field study Mas and Moretti (2009) find evidence of positive spillover effects in productivity among workers at the same plant.

ideas of this literature is that individual investors can affect asset prices if their trades are sufficiently correlated due to 'social movements' (Shiller, 1984). A social movement needs to start somewhere; our paper demonstrates that the workplace is a plausible candidate.

The remainder of this paper is organized as follows. Section 2 presents the data. Section 3 presents results on the timing of purchases and Section 4 presents results on stock selection. Section 5 analyzes whether purchases that are highly correlated with co-workers are associated with abnormal returns. Section 6 concludes.

2 Data

The dataset is proprietary and has been collected from three sources. First, a record of all common stock trades made between January 1994 and December 2005 on the Oslo Stock Exchange (OSE) by Norwegian residents 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. To preserve anonymity, the trade records of the 20 most active investors are not contained in the data. Second, we obtained from the OSE daily ticker prices and other company information such as market capitalization and company ID numbers. Where needed, we supplemented this information with data from Borsprosjektet (the OSE-project) at the Norwegian School of Economics. Third, from the government statistical agency, Statistics Norway, we obtained register data on the socio-demographic characteristics of investors. For each individual-year, the data includes the ID of the plant at which the individual is employed (the plant ID stays fixed through ownership changes), the ID of the individual's spouse and children and the zip code in which the individual lives. We also identify other family members: Parents, grandparents, grandchildren, siblings, uncles, aunts, cousins, nieces and nephews. The socioeconomic variables include income and wealth, age, gender, education, and employer variables such as industry (five-digit NACE code) and an unique employer ID number.[13] For individuals that change firms during our sample period, the Statistics Norway data

[13]NACE stands for Nomenclature Generale des Activites Economiques dans I'Union Europeenne and is a European industry standard classification system equivalent to the SIC system in the US.

contain the end date of employment at the old firm and the start date of employment at the new firm. Huttunen et al. (2011), contains a further description of the job start and job end variables. Since the data is collected from government registries, it is highly reliable.[14]

2.1 Sample Selection

Our starting point for the sample selection is individuals that are employed full-time for at least one year between 1994 and 2005, and moreover purchase common stocks on the Oslo Stock Exchange at least once during the same period (about 12%). We omit individual-years where the individual is employed part-time, or employed by a listed company or a subsidiary of a listed company. This exclusion is done to ensure that employee stock ownership plans, which would imply a near-mechanic correlation in purchasing behavior at the plant level, are not driving our results (in Norway, purchases up to NOK 1500 in own-company stock are subject to a tax break). We also exclude individual-years of employment in Financial Services (NACE codes 65, 66, and 67) as a simple way to eliminate professional investors from the sample.[15] These restrictions define our sample of about $170,000$ individuals. The co-worker peer group of these individuals is defined somewhat more broadly; we include part-time employees (and, for family and postcode peer groups, individuals employed in the financial sector). We refer to an individual in the sample or in one of the peer groups that makes at least one purchase of stock during the period 1994 to 2005 as an 'investor'.

For each individual-year in the purchase decision analysis of Section 3, we create three peer groups; co-workers, family members, and individuals living in the same zip code. We keep individual-years where a) at least one co-worker is an investor (i.e., purchases stocks at least once between 1994 and 2005), b) at least one person in the same zip code is an investor, and c) at least one other family member is an investor.[16] The investment activity of co-workers is our main explanatory variable and the latter two groups will act

[14]The data is described in more detail in Døskeland and Hvide (2011), which also discuss the Norwegian institutional environment, including questions about representativity and the Norwegian pension system.

[15]Our results are slightly stronger if we keep these industries.

[16]We also require that the sociodemographic variables are non-missing. This affects only a small fraction of individual-years.

as controls (these controls would not be defined without criteria b and c). This leaves us with approximately $80,942$ unique individuals over the entire period. In Panel A of Appendix $A2$ we randomly select a year in which the individual is present and provide descriptive statistics. In the year 2000, the sample individuals are spread over about $2,600$ zip codes and about $18,000$ plants.

For the stock selection analysis of Section 4, we consider purchases by the sample individuals where at least one co-worker and one person in the zip code purchases a stock in the same month. Panel C of Table $A2$ of the Appendix provides descriptive statistics of the roughly $120,000$ unique individuals present in the stock selection analysis (again a random year for each individual has been selected). The sociodemographic characteristics are similar to that in the sample of Section 3. The sample is somewhat larger than in Section 3 because we exclude the family peer group.[17]

In Section 3.2 and Section 4.2 we consider individuals that move between plants. For a move to be included in the analysis we require that the termination date of the old job and the start date of the new job are both non-missing.[18] At the time of the move, we require that the individual did not change plant in the preceding year nor in the following year (in order to focus on jobs that are not of a temporary nature).[19] We focus on stock market activity during the twelve months prior to leaving the old plant, and twelve months after moving to the new plant, which means that we consider moves that occur between January 1995 and December 2004. These criteria leave us with approximately $13,000$ unique individuals in the purchase decision analysis of Section 3.2. Panel B of Table $A2$ contains descriptive statistics on these individuals for a random year. For all the sociodemographic variables, including age, income, wealth, and all the peer group

[17] Restricting the analysis to individuals that have family members that purchase stocks in the same month would leave us with a small number of unique individuals (2800). In unreported regressions we have verified that the results are very similar for this subsample, even after controling for family members stock selection.

[18] This means, for example, that individuals that start a job fresh from education or move from abroad are excluded. We lose about half of the moves in our database due to this restriction.

[19] Additionally, we require that the investor moves at most four times between 1993 and 2005, that the start date at the new plant is later than the stop date at the previous plant, and that the unemployment spell (if any) lasts less than 6 months. These three criteria exclude only a very small fraction of moves. For some individuals plant information is missing at the end of year $t-2$. For these individuals we require them to have worked at the old plant for at least 18 months.

variables, including plant and zip code size, the movers are very similar to the overall sample.

3 The Purchase Decision

In this section we relate the decision of an individual to purchase common stocks in a given month with the same decision made by his or her co-workers.[20]

We create a dummy variable $buy_{i,t}$ that takes the value 1 if individual i makes a purchase in month t, and 0 otherwise. We relate $buy_{i,t}$ to the fraction of investor co-workers that purchase that month, denoted by Buy_t^{plant} (peer group variables are capitalized). For example, if an individual has four co-workers that are investors and one of them makes a purchase in month t, then $Buy_t^{plant} = 1/4$. A different way to operationalize co-worker purchasing activity is to change the denominator in Buy_t^{plant} to the total number of co-workers (i.e., including non-investors). All of our results are robust to this alteration.[21] Panel A of Table 1 provides descriptive statistics of the dependent and main independent variables. A stock purchase is made in 4.77% of individual-months. The mean fraction of co-workers that make a purchase in a given month is 4.35%.

3.1 Basic Results

We examine the effect of peers on the decision to trade by estimating the following linear regression at the individual-month level,

$$buy_{i,t} = \beta Buy_t^{plant} + \mathbf{b\Gamma} + \varepsilon_{i,t}. \tag{1}$$

The estimated β will capture the extent to which the individual's purchasing activity is correlated with that of his co-workers. $\mathbf{\Gamma}$ is a column vector of control variables and $\mathbf{b}$ is a row vector of coefficients. The sociodemographic controls include: Age, wealth,

[20] Since very few individual investors short stocks, considering sell transactions implies conditioning on the investor already owning stocks. In unreported analysis we consider the decision to sell as dependent variable. The results are qualitatively the same.

[21] In unreported regressions we find that the point estimates are very similar to those reported in Table 2, and highly significant, but the economic magnitudes are somewhat lower.

labor income, sex and the number of years of education, and various powers thereof (see the caption to Table 2 for specifics.[22] As additional controls, we include Buy_t^{fam} and Buy_t^{zip}. These variables control for correlation in timing of purchases within the zip code and inside the family, and are defined in the manner as Buy_t^{plant}. We include a set of month dummies (132 in total) that controls for time-varying aggregate patterns in trading behavior. To control for contextual effects, we include plant fixed effects and fixed effects for (two-digit) industry of the plant.[23] For the same reason we include zip code fixed effects. We estimate (1) using ordinary least squares and cluster the standard errors at the individual level to control for serial correlation in errors.[24] Similar regression models that link individual behavior to mean group behavior have been used by e.g., Bertrand et al. (2000), Duflo and Saez (2002), Ivković and Weissbenner (2007).

Table 2 presents the empirical results. Column (3) is the main specification. The estimated β is positive and highly significant. In terms of economic magnitude, in (3) a one standard deviation increase in co-worker trading activity (Buy_t^{plant}) results in an increase in trading activity of 34% relative to the unconditional mean.[25] In column (4) we account for time-variant changes at the plant or zip code level by including yearly plant and zip code fixed effects. The point estimate is similar to the coefficient reported in column (3).[26]

We can note that the zip code level correlation in trading behavior is significantly reduced when workplace and family peer effects are introduced; the introduction of co-workers reduces the impact of neighbors by roughly 16% (comparing (2) and (3)). In contrast, the impact of workplace peers is much less affected by the introduction of neighbors (7% reduction). This is what we would expect if the positive correlation at zip code

[22]For income and wealth we use one-year lagged values.

[23]Of course, the industry dummies will be redundant unless the plant changes industry sometime during the time frame.

[24]In unreported results we have also clustered standard errors at the monthly level. The resulting t-statistics are lower, but still highly significant.

[25]The estimated impact of family and neighbors is lower; a one standard deviation increase is associated with an increase in trading activity of 24% and 16% respectively.

[26]We re-estimated (1) using a probit model (including only month and industry fixed effects). The estimated marginal effects, evaluated at the mean, are larger than those reported for specification (3). In addition, we have considered specifications without fixed effects. In this case, the point estimates and economic magnitudes are larger.

level is partially driven by co-workers that live close to each other.[27]

The results could be driven by events in the industry or in the region, such as writings in industry journals or in local newspapers. We account for time-variant industry-specific events by including monthly industry-level fixed effects. This affects the estimated co-worker peer effect only to a minor extent, as reported in column (5). We account for time variant local events by including monthly municipality fixed effects (in Norway, there are 459 municipalities). The results, reported in column (6), are similar to those reported in column (3). The results in Table 2 could be driven by a particular industry. In Appendix 3, we estimate a separate co-worker peer effect for each of 37 industries that represent a significant proportion of our sample (no single industry accounts for more than 11.87% of our investor observations). These results strongly indicate that correlation in trading behavior among co-workers is universal across industries.[28]

As an alternative regression strategy to (1), we estimated the correlation in purchasing activity between two randomly chosen individuals at each plant-month, including the same controls as in (1). The correlation is positive and significant. In contrast, the 'placebo treatment', i.e., the correlation between two randomly chosen individuals at *different* plants, has zero correlation. We also estimated how the pairwise correlation depends on plant size and found that the strongest pairwise correlation occurs for plants that have less than thirty employees.

Peer groups are likely to be smaller than plants, and to form along sociodemographic patterns. For example, females may talk more with females than with men, and individuals in the same age group may be more likely to talk. In unreported analysis we followed Duflo and Saez (2002) and regress individual purchases on purchases made for each subgroup separately on the purchase decision. Similar to Duflo and Saez (2002), the estimated peer group coefficients are consistently larger within subgroups than between. In the stock market participation model of Hong et al. (2005) some investors are sus-

[27] Additionally, in unreported analysis, we find that the introduction of sociodemographic controls reduces impact of the neighborhood peers while the impact of co-workers is less affected.

[28] The regression model (1) postulates that the purchase probability of an individual depends linearly on the fraction of peers that purchase in a given period. It is likely, however, that the fraction of co-worker purchasing has a different effects for plants of different size. In unreported regressions, we find that the effect of co-workers is statistically significant (at the 1% level) for all deciles of plant size.

ceptible to social influence and some are not. In unreported analysis we analyze whether sociodemographic characteristics are related to the strength of co-worker peer effects. We find that co-workers exert a greater influence on males. We find no relation to age or the level of education.

3.2 Changes in Place of Work

Workers with similar unobserved characteristics, such as risk preferences or investment style, could self-select to the same plants in a pattern not captured by the control variables. The data allow us to track individuals that move between plants down at a monthly level. Workers that move between plants allow for a placebo test: we analyze how individual purchases relate to the purchase activity of *future* co-workers. The idea is that future co-workers are unlikely to influence via social interaction but may still exhibit correlated behavior due to similarity along unobservables. Thus if unobserved similarities drive our results, we would expect the correlation with future co-workers to be of comparable magnitude to the correlation with *current* co-workers.

Workers that move between plants also provides us with an intuitive way to deal with the reflection problem, i.e., that the estimated coefficients in Table 2 reflect both the influence of the group on the individual and the influence of the individual on the group. One can argue that recently arrived individuals are much less likely to influence the incumbent group at the new plant than vice versa (at least for some time), and that identification of peer effects is in that case quite sharp. Of course one can think of exceptions to this rule, such as an academic department hiring a new star scientist, or a firm hiring a new manager. The much more common experience, according to the social psychology and sociology literature, is that listening and adaptation is the prevalent mode in a new job at least for a few months (e.g., van Maanen, 1975, Moreland, 1985, Ashfort and Saks, 1996).[29]

To analyze the impact of new and former co-workers, we interact the fraction of old and new co-workers that make a purchase in a given month with dummy variables that

[29] For example, Ashfort and Saks (1996, p.149) state that 'Individuals are particularly susceptible to influence during role transitions, such as organizational entry, because of the great uncertainty regarding role requirements.'

indicate whether that month is prior to leaving (joining) the old (new) plant or not.[30] The variable $Buy_t^{old\ before}$ is the fraction of old co-workers that make a purchase prior to the individual leaving the old plant (this variable takes the value 0 after leaving the old plant), and the variable $Buy_t^{old\ after}$ is the fraction of old co-workers that make a purchase after the individual has left the old plant (this variable takes the value 0 before leaving the old plant). The variables describing the purchase activity of new plant co-workers, $Buy_t^{new\ before}$ and $Buy_t^{new\ after}$, are defined in the same manner. Additionally, we restrict the sample to 12 months before the individual leaves the old plant and 12 months after joining new plant.[31] We estimate,

$$buy_{i,t} = \beta_1 Buy_t^{old\ before} + \beta_2 Buy_t^{old\ after} + \beta_3 Buy_t^{new\ before} + \beta_4 Buy_t^{new\ after} + \mathbf{b}\mathbf{\Gamma} + \varepsilon_{i,t}. \quad (2)$$

Estimating equation (2) allows us to track how the correlation in behavior with different co-workers evolves over time. It is conceivable that trading frequency changes in connection with a move (for example due to severance packages or time constraints). The vector $\mathbf{\Gamma}$ therefore includes, in addition to the same sociodemographic control variables and fixed effects as in column (3) of Table 2, dummies for the number of months before leaving from the old plant, and dummies for the number of months prior to joining the new plant.

We start out by focusing attention to the months prior to leaving the old plant. Column (1) of Table 3 considers the effect of co-workers at the old plant before leaving. The estimated coefficient is similar to the estimated coefficient on co-workers for the overall sample, in column (3) of Table 2.

In column (2) of Table 3 we perform the placebo test, by relating individual purchases to that of future co-workers. The coefficient on future co-workers is positive, but small and barely significant. This suggests that endogenous group membership is not substantial. In column (3) we include both current and future co-workers. The placebo coefficient from

[30] For more than 80 percent of moves, the individual moves straight from the old plant to the new plant, without gap months. In this case, these two dummy variables are just the complements of each other.

[31] We exclude the month in which the individual leaves the old plant and the month when he joins the new plant because they cannot be clearly assigned to either before or after the move. Later on, we take the analysis one step further and estimate separate effects for each month.

(2) is reduced by a third while the coefficient on $Buy_t^{old\ before}$ is not significantly affected.[32]

Column (7) shows that the correlation with old co-workers significantly drops after the individual leaves the old plant (for both regressions, the before-after difference is significantly different at the 1 percent level). In (8) we address the reflection problem by considering the correlation with new co-workers the year after the individual has joined the plant. As argued above, β_4 in (8) is likely to be mainly driven by influence from the incumbent group of workers on the individual. The estimated β_4 is smaller than in (5), but of comparable magnitude.

In column (9) we include all sample months and consider the full specification in described in equation (2). All the coefficients are similar to those reported in (1)-(8). Finally, in column (10) we restrict the sample to those individuals that do not change the municipality where they live or the municipality where they work in conjunction with the change in plant. The results are similar.

In order to consider how the relation with the two peer groups evolves over time in more detail, we move to the monthly level. Let t denote event time in months; for example $t = -12$ denotes 12 months prior to leaving the old plant and $t = 12$ denotes 12 months after joining the new plant. Furthermore, define 25 dummy variables $\{\mathbf{1}_t\}_{t=-12,12}$. Each dummy equals 1 for month t and 0 otherwise (e.g., $\mathbf{1}_3 = 1$ if $t = 3$ and 0 otherwise). We interact $\{\mathbf{1}_t\}$ with $Buy_t^{plant\ old}$ and $Buy_t^{plant\ new}$ and estimate the following regression

$$buy_{i,t} = \sum_{t=-12}^{12} (\beta_{old,t} Buy_t^{plant\ old} + \beta_{new,t} Buy_t^{plant\ new})\mathbf{1}_t + \mathbf{b\Gamma} + \varepsilon_{i,t} \tag{3}$$

The vector $\mathbf{\Gamma}$ contains the same controls as used when we estimated (2) and are described in the caption to Table 2.[33] The coefficients $\beta_{old,t}$ and $\beta_{old,t}$ capture the correlation with old and new co-workers in month t, after controlling for fixed effects. The results of this regression are exhibited in Figure 1.

[32] In column (4)-(6) we perform the same exercise on months after joining the new plant. The coefficient on new co-workers in column (5) is again very similar to the overall sample, column (3) of Table 2. We can note that the coefficient on new co-workers is not substantially affected by including a control for previous co-workers, as seen from column (6). Note also that the correlation with previous co-workers cannot be used as a placebo test, because the individual is likely to stay in touch with his old co-workers.

[33] The regression specification in (3) is a slight simplification of the actual regression specification. First, in order to capture the less than 20 percent of the sample that moves with a gap month between the old

In Figure 1, the blue line depicts how newly employed individuals are influenced by their peers. The sharp ascent of the blue line around Month 0 reveals that the correlation with new co-workers is initially low but becomes substantial after a very short period in the new job. This is consistent with the individual gradually becoming socialized and adopting the investment behavior of his peers. The red line in Figure 1 illustrates how the correlation with past co-workers evolves over time (Month 0 is the month when the individual leaves the old plant). The correlation with old co-workers decreases significantly when the individual leaves the old plant.

We can note from Figure 1 that the red graph does not reach the same level after Month 0 as the blue graph has prior to Month 0. This is consistent with the red graph before Month 0 reflecting both the effect of the group on the individual and the effect of the individual on the group, while the blue graph after Month 0 captures only the effect of the group on the individual. In other words, the difference in the two graphs is consistent with the blue graph after Month 0 indeed capturing social effects unpolluted by the reflection problem.

Overall, these findings to our minds give strong support to the notion that social interaction in the workplace influences individuals' decision to purchase stocks; we find it striking how the correlation with different sets of peers evolves in a pattern that reflects time-specific proximity.

3.3 Within-Industry and Local Purchases

Within-industry stocks are well-known to be poor hedges (Baxter and Jermann, 1997, Goldstein, 2007, Eiling, 2013). Døskeland and Hvide (2011) document that individual

plant and the new plant, in fact we estimate

$$buy_{i,t} = \sum_{i=-12}^{12} \beta_t Buy_t^{plant\ old} \times \mathbf{1}_{old,t} + \sum_{i=-12}^{12} \beta_t Buy_t^{plant\ new} \times \mathbf{1}_{new,t} + \mathbf{b\Gamma} + \varepsilon_{i,t} \tag{4}$$

where $\{\mathbf{1}_{old,t}\}$ and $\{\mathbf{1}_{new,t}\}$ are dummies that are complements only for moves with no gap. For the less than 20 percent of moves where the number of gap months g exceeds zero, we keep the gap months but drop the g months on each extremity of the time window. For example, if an individual has $g = 1$ then we drop the twelfth month before leaving the old plant and the twelfth month after joining the new plant. Second, in order to calculate the moving average of coefficients, presented in Figure 1, we include interaction effects for month 13 prior to leaving the old plant and month 13 after joining the new plant.

investors have a bias towards purchasing such 'expertise' stocks and that they are not associated with positive abnormal return (in some specifications associated with a negative abnormal return). Thus, the purchase of within-industry stocks is likely an investment mistake.[34] In this section, we consider the impact of peers on within-industry and non within-industry 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 (for 1995 we impute the NACE codes from 1996). Following Døskeland and Hvide (2011), we define an expertise stock as a stock where the worker two-digit NACE code matches the NACE code of the stock. In order to examine within-industry purchases we create two dummy variables, $buy_{expertise}$ and $buy_{non-expertise}$ that take the value 1 if the stock being purchased is expertise or non-expertise respectively. We estimate (1) using these variables as dependent variables.

There is substantial evidence in the literature that investors show a preference for local stocks (for example Coval and Moskowitz, 1999 and Huberman, 2001). We expect local stocks to be salient objects of workplace conversations and thus peer group effects to be stronger for local than for non-local stock. 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. In order to examine local purchases we create two dummy variables, buy_{local} and $buy_{non-local}$ that take the value 1 if the stock being purchased is local or non-local respectively. We estimate (1) using these variables as dependent variables.

Specification (1) of Table 4 considers only local buys (i.e., uses buy_{local} as the dependent variable), while column (2) considers non-local buys. Specifications (3) and (4) consider expertise and non-expertise buys respectively. We include the same controls as in Table 2.

In all four specifications, the fraction of co-workers that make a purchase is statistically significantly related to the decision to make a stock purchase at the 1% level. The impact of co-workers is somewhat larger for local (34% relative to the unconditional mean) than

[34]Purchases of within-industry stocks could be a hedge against negative shocks to own-firm performance. As the stock price of firms in the same industry tend to be strongly correlated, this interpretation does not seem plausible.

for non-local stocks (30%) but the difference is not statistically significant. 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 0.68% (0.70%), which represents a 149% (26%) change compared to the unconditional mean. The difference in impact is statistically significant. We conclude that social interaction seems to play an important role in explaining the purchase of within-industry stocks.

4 Stock Selection

In this section we consider the relation between an individual's stock selection and the stock selection decisions made by co-workers.

We consider only individual-months where the individual purchases a stock and create a variable $f_{i,t,s}$, which equals the fraction of total purchases by investor i in month t that is made in stock s. Note that $f_{i,t,s}$ is defined for all stocks present in that month, and that $\sum_s f_{i,t,s} = 1$ by construction. As main explanatory variable we use the fraction of total purchases invested in stock s in month t by co-workers, denoted by $F_{t,s}^{plant}$. Note that $F_{t,s}^{plant}$ is only defined if at least one co-worker and makes a purchase that month.[35] Table 5 provides descriptive statistic. The mean fraction of total purchases invested in a stock is 0.49% which makes intuitive sense since there are roughly 200 stocks on the Oslo Stock Exchange over our sample period.

4.1 Basic results

To relate individual stock selection to that of his co-workers, we estimate the following equation for the purchases in our database,

$$f_{i,t,s} = \beta F_{t,s}^{plant} + \mathbf{b\Gamma} + \varepsilon_{i,t,s} \qquad (5)$$

[35] If we did not condition on making purchases, the β's in (5) would combine the effects of being active and stock selection.

The coefficient β captures the extent to which stock selection of an individual is correlated with that of co-workers. To control for differences across plants in preference for certain stocks, due to e.g., having a business relationship with a listed company, we include plant-stock fixed effects in $\mathbf{\Gamma}$. To account for local bias and other geographical effects, we include zip code-stock fixed effects. We also include monthly stock dummies (one for each stock) to control for time-varying aggregate patterns in the demand for individual stocks (such as individual investors pursuing 'glitter stocks' as in Barber and Odean, 2008, or stocks with strong prior performance, as in Benartzi, 2001). As additional controls, we include zip code level stock selection, $F_{t,s}^{zip}$, which is defined in the same manner as $F_{t,s}^{plant}$. Both $F_{t,s}^{plant}$ and $F_{t,s}^{zip}$ sum to 1 across stocks in a given period.

The results are presented in Table 6, column (3) is our main specification; the estimated β coefficient positive and highly significant. In terms of economic magnitude, in (3) a one standard deviation increase in the fraction of co-workers that purchase the same stock results in a 176% increase relative to the unconditional mean.[36] In (7) we drop the plant fixed effects and introduce yearly-stock-industry fixed effects. The point estimate of the co-worker peer effect increases to 0.247 from 0.179. Thus, it appears that plant fixed effects captures a substantial amount of variation in the stock selection decision.[37]

4.2 Changes In Place of Work

To analyze the impact of new and former co-workers on stock selection, we interact the fraction of old and new co-workers that make a purchase in a particular stock in a given month with dummy variables that indicate whether that month is prior to leaving (joining) the old (new) plant or not. For example, the variable $F_{t,s}^{old\ before}$ is the fraction invested in stock s by old co-workers prior to the investor leaving the old plant. After the departure date the variable takes the value 0. The variable describing the stock selection of new

[36]We can note from column (2) and (3) that the correlation with geographical neighbors drops significantly when co-worker stock selection is included. The converse is not the case; the correlation with co-workers is hardly affected by introducing neighbors, as seen by contrasting (1) and (3). This is consistent with correlation at the zip code being predominantly driven by social interaction in the workplace.

[37]Recall that we do not control for family group stock selection, as this would leave us with a very small sample size. We have verified that the estimated coefficient on $F_{t,s}^{plant}$ is very similar for this subsample also after controlling for $F_{t,s}^{fam}$.

plant co-workers, $F_{t,s}^{new\ before}$ and $F_{t,s}^{new\ after}$ are defined in the same manner. As in the purchase decision analysis, we restrict the sample to 12 months before the individual leaves the old plant and 12 months after joining new plant.[38] We estimate,

$$f_{i,t,s} = \beta_1 F_{t,s}^{old\ before} + \beta_2 F_{t,s}^{old\ after} + \beta_3 F_{t,s}^{new\ before} + \beta_4 F_{t,s}^{new\ after} + \mathbf{b\Gamma} + \varepsilon_{i,t,s}. \quad (6)$$

To control for geographical differences in preference for certain stocks we include zip code-stock fixed effects in $\mathbf{\Gamma}$. As additional controls, we include zip code level stock selection, $F_{t,s}^{zip}$. The sample size is not sufficiently large to include plant-stock fixed effects. This means that the level of the coefficients estimated in the present section will be contaminated by plant-specific preferences for particular stocks, and the analysis mainly has interest in illustrating differences between the estimated coefficients in (6).

We start out by confining attention to the months prior to leaving the old plant. In column (1) we consider the effect of co-workers at the old plant before leaving. In column (2) of Table 7 we relate individual purchases to that of future co-workers. The coefficient is significantly smaller than the coefficient on current co-workers. The coefficient is positive and significant which is likely due to the omission of plant-stock fixed effects. In column (3) we include both current and future co-workers. Neither of the coefficients from (1) or (2) are much affected.[39]

In columns (7)-(9) we consider how the correlation with new and old co-workers change surrounding the change in place of work. Column (8) shows that the correlation with new co-workers significantly increases after the individual joins the new plant (the before-after difference is significantly different at the 1 percent level). As in Section 3.2, the difference between these two coefficients is likely to be largely driven by influence from the incumbent group of workers on the individual. In column (9) we include all sample months and consider the full specification in (2). All the coefficients are similar to those reported in (1)-(8). Finally, in column (10) we restrict the sample to those individuals

[38] The analysis of this section covers about 6,300 individuals. The sociodemographic characteristics of these individuals with respect to age, income, wealth etc. are very similar to that covered in the other parts of the paper.

[39] In column (4)-(6) we perform the same exercise on months after joining the new plant. We can note that the coefficient on new co-workers is not affected by including a control for previous co-workers, as seen from column (6).

that do not change the municipality where they live or the municipality where they work in conjunction with the change in plant.

Similar to in Section 3.2, we now examine the evolution of the relation between co-worker stock selection and investor stocks selection. We estimate the following regression

$$f_{i,t,s} = \sum_{t=-12}^{12} (\beta_{old,t} F_{t,s}^{plant\ old} + \beta_{new,t} F_{t,s}^{plant\ new}) \mathbf{1}_t + \mathbf{b\Gamma} + \varepsilon_{i,t,s} \tag{7}$$

The vector $\mathbf{\Gamma}$ contains the same controls as in (6). The coefficients $\beta_{old,t}$ and $\beta_{new,t}$ capture the correlation with old and new co-workers in month t, after controlling for fixed effects. The results of this regression are exhibited in Figure 2.

In Figure 2 we have plotted the interacted peer coefficient against the number of months before the move. Figure 2 looks similar to Figure 1. Prior to the move the effect of old co-workers is greater than the effect of new co-workers, but following the move the effect of new co-workers surpasses that of old co-workers, indicating that the investment decisions of individuals is most affected by those peers that they interact the most with.

4.3 Within-Industry and Local Purchases

In this section we consider how the stock selection of our peer groups relates to the stock selection of the investor when the stock is either in the individual's industry of employment or a local stock. To do this we use the classification of expertise and local stocks introduced in section 3.3. Table $A4$ of the Appendix presents descriptive statistics of the fractions invested in expertise and local stocks by the individuals in our sample.

In Table 8 we estimate (5) for local, non-local, expertise and non-expertise stocks in columns (1) to (4). We include month-stock, plant-stock, and zip code-stock fixed effects in all columns. The estimated co-worker coefficient is positive and significant in all specifications. There is no statistically significant difference between the impact (one standard deviation increase) of co-workers on local versus non-local stocks. The impact on expertise stocks (142% relative to the unconditional mean) is higher than for non-expertise stocks (126%). Note also that the adjusted R^2 of column (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, similar to in the analysis of the timing of purchases.

5 Should You Listen To Your Co-workers?

We have shown that social interaction appears to influence stock investments. We now ask whether purchases made under greater purchase activity by co-workers makes an abnormal return compared to purchases made under less intense purchase activity by co-workers. The literature on information cascades (Bikhchandani et al., 1992; Banerjee, 1992; Ellison and Fudenberg, 1993) posits that imitating co-workers can make investment decisions better informed and improve investment returns.

Even though the dataset has a large set of individuals, these individuals construct portfolios by combining a limited number of stocks, which creates cross-sectional correlation in returns across investors (e.g., Lyon et al., 1999).[40] To deal with this problem, we follow Odean (1999) and Seasholes and Zhu (2010) and apply the calendar time methodology. This methodology eliminates cross-sectional correlation by collapsing each time period into a single observation.

For each calendar month t, we calculate the excess return on a portfolio with one position in each purchase during the six-month portfolio formation period (the results using three- and 12-month formation periods are similar, and are reported in Appendix $A5$). A stock may have been purchased on several occasions during the formation period. If so, each purchase generates a separate position in the portfolio. Each purchase is weighed equally. We estimate the following regression,

$$r_{b,t} - r_f = \alpha + \beta Buy_t^{plant} + \mathbf{b}\boldsymbol{\Gamma} + \varepsilon_{b,t} \tag{8}$$

where $r_{b,t} - r_f$ is the excess return (over the 1 month Norwegian Interbank Offered Rate) of the buy portfolio in month t. Our vector of control variables $\boldsymbol{\Gamma}$ includes the risk factors $MRKT$, HML, SMB and the Carhart (1997) momentum factor, all of them calculated

[40] We have roughly $500,000$ purchases which are distributed over roughly 200 stocks.

for Norway, in addition to Buy^{zip} and Buy^{family}.[41] The coefficient β reflects whether purchases made under stronger co-worker purchasing activity is associated with higher risk-adjusted returns. In other words, we are comparing the returns of purchases made under weak or no 'purchase pressure' against the returns of purchases made under strong purchase preesure. For the regression implementation, we use the generalised calendar time methodology developed by Hoechle et al. (2009) which allows for the introduction of continuous investor characteristics. We report Dirscoll and Kraay (1998) standard errors with three lags.[42]

Panel A of Table 9 presents our regression results. In column (1), the point estimate of β is -0.00584 and statistically significant at the 1% level, indicating that purchases made under stronger purchase pressure perform *worse*. In terms of economic magnitude, a one standard deviation increase in co-worker purchasing activity results in a reduction in monthly returns of 12 basis points. Column (3) mimics column (1) but only consider expertise trades. Again the estimated β is significantly negative. Column (5) mimics column (1) but only considers buys in local stocks. Here the estimated β is not significantly different from zero. Purchases made at the same time as neighbors are associated with negative abnormal performance (the corresponding economic impact is -0.44%). As Buy^{zip} can be seen as a proxy for economy-wide buy pressure by individual investors, this finding is similar to that reported by Barber and Odean (2008).

In order to understand better what drives the significantly negative β coefficient in columns (1) and (3), we divide Buy_t^{plant} into five quintiles for each month of our sample period. We are particularly interested in whether a strong purchase pressure (Buy_t^{plant} in the fifth quintile) is associated with a negative excess returns relative to the returns under weak purchase pressure. In column (2) we include in the regression dummies for quintiles 2 to 5. In column (2) there does not seem to be a clear negative pattern between abnormal returns and purchase pressure. Column (4) mimics column (2) but only considers expertise purchases. The results from column (4) indicates that expertise purchases that are made when peer effects are particularly strong (quintile 5) are associated with significantly lower

[41] We use the factors calculated in Ødegaard (2009).

[42] Hoechle et al. show that Driscoll and Kraay standard errors is equivalent to Newey and West (1987) standard errors in the generalised calendar time context.

abnormal returns. In Panel B of Table 9 we consider stock selection which means that we estimate (8), but replace the co-worker purchase variable Buy_t^{plant} with the stock selection equivalent, $F_{t,s}^{plant}$ (we include $F_{t,s}^{zip}$ as a control).

Based on Table 9, we reject the notion that purchasing when purchase pressure is stronger is associated with positive abnormal returns; the β coefficient is never positive and significant. For some specifications in Table 9, in particular for expertise stocks, we find a negative relation between abnormal returns and purchase pressure. Thus purchases of within-industry stocks in conjunction with purchase pressure is not only a poor hedge, but also yields poor investment returns.

6 Conclusion

This paper addresses whether co-workers influence investment choices. Employing comprehensive data from Norway that covers a ten-year period, we find that the stock market behavior of individual investors is highly correlated with the stock market behavior of their co-workers. Sorting of unobservably similar individuals to the same workplaces is unlikely to drive our results, as evidenced by the trading behavior of individuals that move between plants. We also analyze whether social interaction leads to better economic outcomes for the individuals that are affected, by linking the measures of social interaction with investor returns, and do not find evidence suggesting abnormal returns. Moreover, we find that social interaction is intimately linked with investments in own-industry stocks.

Overall, our findings suggest that individuals are strongly influenced by their co-workers, but this influence does not improve, and sometimes reduces, the quality of their investment choices. Listening to co-workers is unlikely to improve the quality of investments.

The results point to social interaction as an important element in the investment behavior of individuals. Existing evidence in favor of social interaction comes from relatively large peer groups, such as regions or neighborhoods. However, these findings are subject to several interpretations (e.g., Moffit, 2001). One contribution of our analysis is to focus on peer effects at a much more local level, and to show that the measured social interaction effects are large even after accounting for correlated unobservables, endogenous group membership, and reflection. A social movement needs to start somewhere, and our paper demonstrates that the workplace is a plausible candidate.

Our results have implications for theory. We find that purchases that are made under greater purchase activity by peers are not associated with abnormally high returns, and in some cases are even associated with negative abnormal returns. The latter stands in contrast to standard models of information cascades where agents are rational and ex-ante beliefs are homogenous (see Edmond, 2008, for an exception), and suggests the relevance of behavioral theories of information cascades.

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Figure 1: New and former co-workers

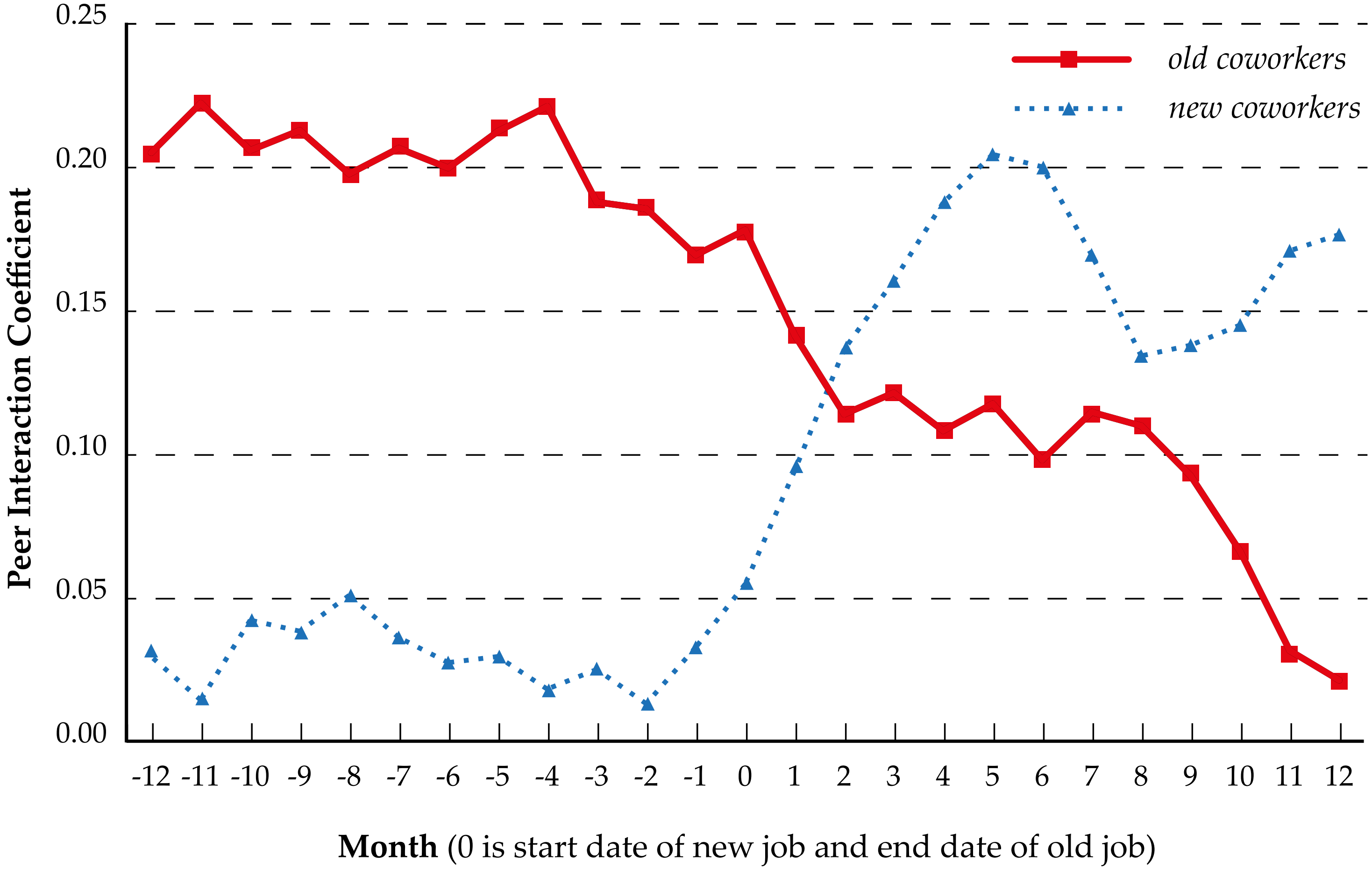

Figure 1: The above figure plots the slope coefficients from regression equation (3). We run a regression where the dependent variable is the dummy variable Buy that takes the value 1 if the investor makes a purchase in that month and 0 otherwise. Our main independent variables is the fraction of old (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 three 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

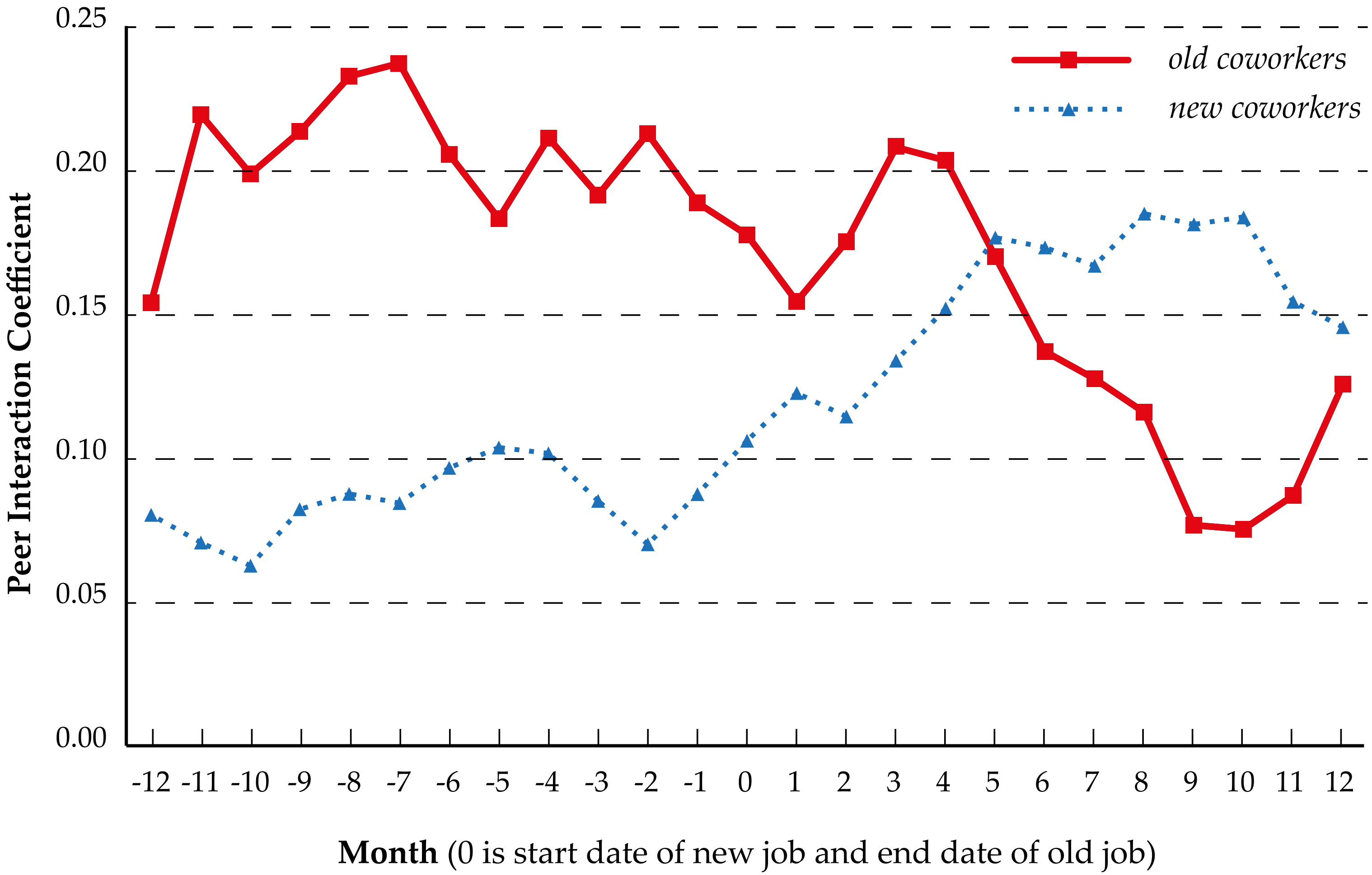

Figure 2: The above figure plots the slope coefficients from regression equation (7). We run a regression where the dependent variable is, $f_{t,s}$, the fraction invested by the investor in stock s in month t. Our main independent variables is the fraction invested in stock s in month t of old (new) 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 three consecutive coefficients and similar to Figure 1 we exclude investors that leave their job in December and join the new plant in January.

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 4). $buy^{expertise}$, $buy^{non\text{-}expertise}$, 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.

Panel A: Trading of individuals and peers

Variable	Mean	Median	Std. Dev.	Min	Max	N
Individual trading						
buy	0.0477	0.000	0.2131	0	1	6,979,992
Peer trading						
Buy^{plant}	0.0435	0.000	0.1095	0	1	6,979,992
Buy^{zip}	0.0411	0.035	0.0338	0	1	6,979,992
Buy^{family}	0.0212	0.000	0.1197	0	1	6,979,992

Panel B: Individual trading in local and expertise stocks

Variable	Mean	Median	Std. Dev.	Min	Max	N
Individual trading						
$buy^{expertise}$	0.0046	0.000	0.0676	0	1	6,739,228
$buy^{non\text{-}expertise}$	0.0272	0.000	0.1626	0	1	6,739,228
buy^{local}	0.0219	0.000	0.1464	0	1	6,846,089
$buy^{non\text{-}local}$	0.0250	0.000	0.1560	0	1	6,846,089

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 sociodemographic 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 some specifications we include time (month), two digit NACE code (of investor plant), plant and zip, and zip-plant interaction fixed effects. In specification (4) we include plant×year, zip×year, and zip×plant×year fixed effects. In specification (5) we include month×industry (NACE 2) fixed effects and in specification (6) we include month×municipality fixed effects. To accommodate the large number of fixed effects in specifications (5) and (6) we use the STATA routine reg2hdfe developed by Guimarães and Portugal (2012). 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. Variables are described in Appendix.

	(1)	(2)	(3)	(4)	(5)	(6)
Buy^{plant}	0.152***		0.149***	0.132***	0.121***	0.139***
	(57.48)		(56.66)	(48.64)	(50.26)	(53.25)
Buy^{zip}		0.269***	0.225***	0.209***	0.189***	-0.0979***
		(33.86)	(30.05)	(26.94)	(28.34)	(-13.00)
Buy^{family}	0.0962***	0.104***	0.0946***	0.0911***	0.0950***	0.0917***
	(48.70)	(52.51)	(48.06)	(47.46)	(48.73)	(46.92)
Sociodemographic	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	No	No	No
Plant FE	Yes	Yes	Yes	No	No	No
Zip FE	Yes	Yes	Yes	No	No	No
Industry FE	Yes	Yes	Yes	Yes	No	No
Plant×Year FE	No	No	No	Yes	No	No
Postcode×Year FE	No	No	No	Yes	No	No
Time×Industry FE	No	No	No	No	Yes	No
Time×Municipality FE	No	No	No	No	No	Yes
N	6,979,992	6,979,992	6,979,992	6,979,992	6,979,992	6,979,992
R^2	0.251	0.248	0.252	0.360	0.257	0.259

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:

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

where $\boldsymbol{\Gamma}$ includes the sociodemographic 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). In specification (10), we only consider those individuals that do not change the municipality where they live or work in conjunction with the plant move (i.e., they shift plant within the municipality). 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)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$Buy^{old\ before}$	0.192***		0.192***				0.190***		0.194***	0.188***
	(10.39)		(10.38)				(10.41)		(10.70)	(9.39)
$Buy^{old\ after}$				0.123***		0.108***	0.124***		0.105***	0.130***
				(7.95)		(7.39)	(8.13)		(7.33)	(7.07)
$Buy^{new\ before}$		0.0313**	0.0213*					0.0430***	0.0298**	0.0415***
		(2.39)	(1.71)					(3.33)	(2.44)	(2.90)
$Buy^{new\ after}$					0.170***	0.164***		0.165***	0.162***	0.149***
					(11.42)	(11.26)		(11.26)	(11.31)	(8.38)
Buy^{zip}	0.139***	0.166***	0.137***	0.140***	0.128***	0.111**	0.138***	0.143***	0.121***	0.122***
	(2.85)	(3.34)	(2.80)	(3.00)	(2.83)	(2.48)	(4.05)	(4.26)	(3.64)	(3.26)
Buy^{family}	0.0803***	0.0822***	0.0801***	0.0813***	0.0810***	0.0799***	0.0809***	0.0815***	0.0801***	0.0767***
	(6.69)	(6.82)	(6.68)	(7.84)	(7.83)	(7.74)	(10.12)	(10.20)	(10.04)	(8.05)
Sociodemographic	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Plant FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zip FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time prior leaving FE	Yes	Yes	Yes	No	No	No	Yes	Yes	Yes	Yes
Time prior joining FE	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	93,219	93,219	93,219	108,463	108,463	108,463	201,682	201,682	201,682	142,724
Adj. R^2	0.336	0.332	0.337	0.297	0.300	0.302	0.308	0.308	0.311	0.327

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. Variables are described in Appendix.

	(1) Local	(2) Non-local	(3) Expertise	(4) Non-expertise
Buy^{plant}	0.0677***	0.0707***	0.0635***	0.0651***
	(43.52)	(35.10)	(44.33)	(33.23)
Buy^{family}	0.0425***	0.0514***	0.00454***	0.0539***
	(34.69)	(34.91)	(7.56)	(34.92)
Buy^{zip}	0.115***	0.0982***	0.0693***	0.0748***
	(25.87)	(14.94)	(12.73)	(16.83)
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	6,846,089	6,846,089	6,739,228	6,739,228
Adj. R^2	0.211	0.190	0.119	0.225

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.

Individual and peer stock selection

Variable	Mean	Median	Std. Dev.	Min	Max	N
Individual stock selection						
f	0.0049	0.0000	0.0618	0	1	90,220,510
Peer stock selection						
F^{plant}	0.0049	0.0000	0.0479	0	1	90,220,510
F^{zip}	0.0049	0.0000	0.0327	0	1	90,220,510

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 purchases invested in stock s in month t by the investor. F^{plant}, F^{family} and F^{zip} is the fraction of purchases 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}	0.182***		0.179***	0.253***	0.270***	0.289***	0.247***	0.206***
	(103.73)		(103.04)	(116.64)	(166.87)	(161.78)	(161.52)	(135.36)
F^{zip}		0.0902***	0.0688***	0.220***	0.106***	0.0908***	0.0988***	0.0585***
		(52.07)	(45.20)	(101.70)	(79.60)	(71.37)	(76.20)	(45.15)
Time×Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Plant×Stock FE	Yes	Yes	Yes	No	No	No	No	No
Zip×Stock FE	Yes	Yes	Yes	No	No	No	No	Yes
Plant×Year×Stock FE	No	No	No	Yes	No	No	No	No
Zip×Year×Stock FE	No	No	No	Yes	No	No	No	No
Nace2×Stock FE	No	No	No	No	Yes	No	No	Yes
Municipality×Stock FE	No	No	No	No	No	Yes	No	No
Nace2×Year×Stock FE	No	No	No	No	No	No	Yes	No
N	90,220,510	90,220,510	90,220,510	90,220,510	90,220,510	90,220,510	90,220,510	90,220,510
R^2	0.435	0.426	0.436	0.471	0.193	0.332	0.202	0.332

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 and NACE2×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)	(9)	(10)
	f	f	f	f	f	f	f	f	f	f
$F^{old\ before}$	0.195***		0.188***				0.199***		0.198***	0.185***
	(12.14)		(11.90)				(13.32)		(13.21)	(10.68)
$F^{old\ after}$				0.188***		0.161***	0.198***		0.161***	0.157***
				(11.81)		(11.04)	(13.13)		(11.25)	(8.50)
$F^{new\ before}$		0.110***	0.0918***					0.134***	0.110***	0.114***
		(7.48)	(6.74)					(9.68)	(8.32)	(7.24)
$F^{new\ after}$					0.185***	0.170***		0.182***	0.171***	0.174***
					(12.83)	(12.16)		(13.31)	(12.62)	(10.23)
F^{zip}	0.0719***	0.0783***	0.0687***	0.0823***	0.0756***	0.0702***	0.0728***	0.0726***	0.0641***	0.0501***
	(4.98)	(5.29)	(4.83)	(5.88)	(5.48)	(5.26)	(6.97)	(6.89)	(6.35)	(4.21)
Time×Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Nace2×Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	468,412	468,412	468,412	525,938	525,938	525,938	925,841	925,841	925,841	648,394
R^2	0.276	0.267	0.278	0.307	0.312	0.317	0.258	0.256	0.265	0.298

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)	(2)	(3)	(4)
	Local	Non-Local	Expertise	Non-Expertise
F^{plant}	0.171***	0.139***	0.231***	0.130***
	(71.68)	(68.94)	(39.13)	(79.32)
F^{zip}	0.0386***	0.0649***	0.0934***	0.0565***
	(18.66)	(32.73)	(15.14)	(38.10)
Time×Stock FE	Yes	Yes	Yes	Yes
Plant×Stock FE	Yes	Yes	Yes	Yes
Zip×Stock FE	Yes	Yes	Yes	Yes
N	25,533,110	54,293,131	2,626,431	77,383,390
Adj. R^2	0.500	0.422	0.709	0.423

Table 9: Returns to Peer Trading

We present regression results relating peer buying pressure and stock selection to returns. We use the Generalised Calendar Time (GCT) regression methodology developed by Hoechle, Schmid and Zimmerman (2009) that allows for the introduction of continuous investor characteristics. Our dependent variable is the monthly excess return of stock s over the one month Norwegian Interbank Offered Rate (NIBOR). In Panel A, we consider our trade variables buy_t^{plant}, buy_t^{family}, buy_t^{zip} as main independent variables while in Panel B we consider stocks selection (variables F_t^{plant} and F_t^{zip}). We consider a 6 month portfolio formation period. In both panels, columns (1) and (2) consider all trades, (3) and (4) consider only expertise trades and (5) and (6) consider only local trades. In columns (2), (4) and (6) we introduce dummy variables for the quintile of the observation in terms of $buy_t^{plant}$and thereby examine whether a particularly strong peer effects are associated with abnormal returns. All specifications include the factors MRKT, HML, SMB and the Carhart (1997) momentum factor (all of them calculated for Norway). The estimation is performed with Weighted Least Squares (WLS) with each observation receiving a weight $1/N_t$, where N_t is the number of observations in month t. 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.

Panel A: Trading of Peers and Returns

	(1) All	(2) All	(3) Expertise	(4) Expertise	(5) Local	(6) Local
Buy^{plant}	-0.00584***		-0.0264***		-0.00264	
	(-3.44)		(-3.13)		(-0.54)	
Quintile2		0.000204		-0.00753*		-0.0003
		(0.30)		(-1.68)		(-0.18)
Quintile3		-0.000007		-0.00317		-0.00144
		(-0.13)		(-0.48)		(-0.96)
Quintile4		0.000459		0.00211		-0.00084
		(0.51)		(0.29)		(-0.43)
Quintile5		-0.00061		-0.0118*		0.000316
		(-0.50)		(-1.75)		(0.10)
Expertise Stock	0.00462	0.00464			-0.00158	-0.00159
	(1.35)	(1.35)			(-0.47)	(-0.47)
Local Stock	-0.00014	-0.000097	-0.0104**	-0.00910*		
	(-0.04)	(-0.03)	(-1.97)	(-1.74)		
Buy^{zip}	-0.123***	-0.126***	-0.172***	-0.190***	-0.159**	-0.160**
	(-2.80)	(-2.85)	(-2.69)	(-2.99)	(-2.11)	(-2.12)
Buy^{family}	-0.00083	-0.00169	-0.00282	-0.00498	-0.0036	-0.0042
	(-0.42)	(-0.85)	(-0.62)	(-1.04)	(-1.37)	(-1.56)
Risk Factors	Yes	Yes	Yes	Yes	Yes	Yes
N	2,294,705	2,294,705	139,748	139,748	841,410	841,410
R^2	0.223	0.223	0.261	0.273	0.231	0.232

Panel B: Peer Stock Selection and Returns

	(1) All	(2) All	(3) Expertise	(4) Expertise	(5) Local	(6) Local
F^{plant}	0.00695		0.0022		0.00969	
	(0.74)		(0.20)		(0.80)	
Quintile2		-0.000661**		0.000678		-0.00026
		(-2.20)		(0.32)		(-0.62)
Quintile3		-0.00046		-0.00334**		6.83E-05
		(-1.09)		(-2.00)		(0.12)
Quintile4		-0.00124**		0.00364		-0.00191**
		(-2.39)		(1.51)		(-2.42)
Quintile5		-0.000023		0.0102*		0.000943
		(-0.01)		(1.69)		-0.19
Expertise Stock	0.00458	0.00466			-0.00239	-0.00217
	(1.41)	(1.49)			(-0.75)	(-0.68)
Local Stock	0.00038	0.000422	-0.0109**	-0.0105*		
	(0.12)	(0.13)	(-2.02)	(-1.91)		
F^{zip}	0.00562	0.0074	0.0192	0.00837	0.0176	0.0191
	(0.14)	(0.18)	(0.37)	(0.17)	(0.42)	(0.46)
Risk Factors	Yes	Yes	Yes	Yes	Yes	Yes
N	2,294,705	2,294,705	139,748	139,748	841,410	841,410
R^2	0.224	0.226	0.262	0.269	0.232	0.233

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

This table presents descriptive statistics on our sample individuals. 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 (excluding the individual). The rows Plant investors, Family investors and Zip investors presents descriptive statistics on the number of investors (i.e., individuals that trade at least once over the period 1994 to 2005 and are therefore included in the individual's peer group) in the individuals respective groups. Additionally, we provide descriptive statistics on the socio-demographic variables wealth, income, age, male and education. The USD NOK exchange rate was 8.77 in December 2000. Number of trades is the number of months in our sample that the individual makes at least one trade. Panel A samples a random year of each individual that is present at one time in our trade. Analogously, Panel B samples a random year of each individual present in our mover analysis (see section 3.2). In Panel C, we consider a random year of all individuals present in our stock selection analysis (Section 4).

Panel A:

Variable	Mean	Median	Std. Dev.	Min	Max	N
Plant size	340.99	59	858.71	1	7,845	80,942
Zip size	3,701.03	2372	4,313.99	7	44,195	80,942
Family size	6.44	5	5.34	1	122	80,942
Plant investors	80.89	10	223.88	1	2,449	80,942
Zip investors	210.61	136	265.29	1	3,095	80,942
Family investors	1.97	2	1.37	1	20	80,942
Wealth (NOK)	885,181.00	347,975	10,331,738.00	0	2,127,096,064	80,942
Income (NOK)	384,411.00	335,300	246,474.00	0	13,387,692	80,942
Age	37.68	37	8.92	21	65	80,942
Male	0.77	1	0.42	0	1	80,942
Education	13.15	13	3.40	0	21	80,942
Number of trades	5.94	2	10.19	1	132	80,942

Panel B:

Variable	Mean	Median	Std. Dev.	Min	Max	N
Plant size	307.71	56	809.44	1	7,845	13,303
Zip size	3,876.62	2,396	4,724.21	9	44,195	13,303
Family size	6.17	5	5.08	1	79	13,303
Plant investors	64.68	10	181.59	1	2,449	13,303
Zip investors	228.21	141	301.69	1	3,095	13,303
Family investors	1.92	2	1.32	1	20	13,303
Wealth (NOK)	708,556.00	344,064	4,871,698.23	0	414,171,968	13,303
Income (NOK)	401,949.00	346,600	240,034.27	900	7,300,798	13,303
Age	37.03	36	7.93	21	63	13,303
Male	0.81	1	0.39	0	1	13,303
Education	13.39	13	3.48	0	21	13,303
Number of trades	6.41	2	10.64	1	129	13,303

Panel C:

Variable	Mean	Median	Std. Dev.	Min	Max	N
Plant size	469.85	129	961.83	1	7,845	119,617
Zip size	3645.89	2,432	3,974.60	7	44,195	119,617
Plant investors	124.24	30	266.43	1	2,449	119,617
Zip investors	410.40	268	487.36	1	5,638	119,617
Wealth (NOK)	1,081,317.00	482,989	10,045,965.27	0	2,127,096,064	119,617
Income (NOK)	449,336.00	388,112	307,861.63	0	27,922,100	119,617
Age	42.82	42	11.12	20	65	119,617
Male	0.80	1	0.40	0	1	119,617
Education	12.94	13	3.60	0	21	119,617
Number of trades	6.62	2	10.98	1	132	119,617

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 0.4% of investor observations (i.e., the industry has at least roughly 423 investors). This restriction implies a loss of less than 3% of the complete sample. To decompose the co-worker peer effect across industries we estimate the following regression

$$buy_{i,t} = \alpha_t + \sum_{j=1}^{37} \beta_j \, Buy_t^{plant} \times I_j + \beta_{38} Buy_t^{family} + \beta_{39} Buy_t^{zip} + \boldsymbol{b\Gamma} + \varepsilon_{i,t}$$

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 37 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.

Industry (NACE code)	Investors	OSE Firms	Coefficient	t-stat
Fishing, fish farming, incl. services (5)	489	2	0.0866***	(3.96)
Oil and gas extraction. Oil and gas services (11)	5,210	19	0.255***	(11.18)
Food products and beverages (15)	2,426	4	0.224***	(12.60)
Wood and wood products (20)	754	2	0.0803***	(3.80)
Publishing, printing, reproduction (22)	1,811	5	0.141***	(7.46)
Chemicals and chemical products (24)	1,653	2	0.920***	(46.44)
Rubber and plastic products (25)	427	0	0.0870*	(1.81)
Other non-metallic mineral products (26)	609	2	0.104***	(4.45)
Basic metals (27)	468	2	0.193***	(4.03)
Fabricated metal products (28)	1,042	1	0.0647***	(3.82)
Machinery and equipment (29)	1,956	7	0.107***	(7.13)
Electrical machinery and apparatus (31)	808	4	0.281***	(8.02)
Radio, TV, communication equip (32)	605	7	0.708***	(18.98)
Instruments, watches and clocks (33)	621	4	0.102***	(4.34)
Motor vehicles, trailers, semi-tr.(34)	574	2	0.798***	(21.27)
Other transport equipment (35)	2,986	2	0.346***	(16.33)
Furniture, manufacturing (36)	711	4	0.0610***	(2.67)
Electricity, gas and water supply (40)	1,665	3	0.0716***	(4.92)
Construction (45)	6,641	2	0.0486***	(7.34)
Motor vehicle services (50)	1,971	0	0.0532***	(4.84)
Wholesale trade, commission trade (51)	9,407	8	0.148***	(16.89)
Retail trade, repair personal goods (52)	3,710	6	0.0726***	(8.26)
Hotels and restaurants (55)	1,432	2	0.0338	(1.53)
Land transport, pipeline transport (60)	1,667	2	0.0785***	(3.84)
Water transport (61)	2,077	42	0.384***	(14.99)
Air transport (62)	1,050	2	0.105***	(3.68)
Services for transport and travel agencies (63)	1,837	0	0.0682***	(6.50)
Post and telecommunications (64)	3,341	5	0.610***	(30.38)
Real estate activities (70)	1,593	8	0.186***	(11.66)
Computers and related activities (72)	5,255	20	0.222***	(20.77)
Research and development (73)	1,203	3	0.289***	(11.07)
Other business activities (74)	12,275	8	0.102***	(16.56)
Public administration, defense and social security (75)	9,729	0	0.0479***	(8.08)
Education (80)	7,543	0	0.0291***	(4.95)
Health and social services (85)	7,946	0	-0.00506	(-0.62)
Interest groups (91)	924	0	0.0321***	(2.59)
Cultural and sporting activities (92)	1,333	2	0.0513***	(3.55)
Total	105,749	182		

Appendix

Table A4: Descriptive statistics on investor and peer stock selection of local and expertise stocks

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.

Stock selection of local and expertise stocks

Variable	Mean	Median	Std. Dev.	Min	Max	N
Individual stock selection						
$f^{expertise}$	0.0130	0.0000	0.1042	0	1	177,827
$f^{non\text{-}expertise}$	0.0046	0.0000	0.0592	0	1	6,344,989
f^{local}	0.0068	0.0000	0.0729	0	1	2,102,794
$f^{non\text{-}local}$	0.0039	0.0000	0.0543	0	1	4,420,022

Appendix

Table A5: Returns to Peer Trading – 3 and 12 month formation periods

We use the Generalised Calendar Time (GCT) regression methodology developed by Hoechle, Schmid and Zimmerman (2009) that allows for the introduction of continuous investor characteristics. Our dependent variable is the monthly excess return of stock s over the one month Norwegian Interbank Offered Rate (NIBOR). In Panel A (C), we consider our trade variables Buy_t^{plant}, Buy_t^{family}, Buy_t^{zip} as main independent variables while in Panel B (D) we consider stocks selection (variables F_t^{plant} and F_t^{zip}). In Panels A and B (C and D) we consider a 3 (12) month portfolio formation period. In all panels, columns (1) and (2) consider all trades, (3) and (4) consider only expertise trades and (5) and (6) consider only local trades. In columns (2), (4) and (6) we introduce dummy variables for the quintile of the observation in terms of Buy_t^{plant} and thereby examine whether a particularly strong peer effects are associated with abnormal returns. All specifications include the factors MRKT, HML, SMB and the Carhart (1997) momentum factor (all of them calculated for Norway). The estimation is performed using Weighted Least Squares (WLS) with each observation receiving a weight $1/N_t$, where N_t is the number of observations in month t. 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.

Panel A: Trading of Peers and Returns – 3 month formation period

	(1) All	(2) All	(3) Expertise	(4) Expertise	(5) Local	(6) Local
Buy^{plant}	-0.00513**		-0.0395***		0.00164	
	(-2.08)		(-3.77)		(0.25)	
Quintile2		0.00124		-0.00883		0.00103
		(1.63)		(-1.35)		(0.65)
Quintile3		0.00116		-0.00365		0.000117
		(0.90)		(-0.45)		(0.10)
Quintile4		0.000563		-0.007		-0.00091
		(0.57)		(-0.76)		(-0.38)
Quintile5		0.000482		-0.0182**		0.00302
		(0.34)		(-2.44)		(0.96)
Expertise Stock	0.00472	0.00475			-0.00243	-0.00254
	(1.62)	(1.64)			(-0.77)	(-0.81)
Local Stock	0.00114	0.00118	-0.00999*	-0.00875		
	(0.34)	(0.35)	(-1.68)	(-1.43)		
Buy^{zip}	-0.164***	-0.167***	-0.268***	-0.276***	-0.240**	-0.239**
	(-2.85)	(-2.85)	(-3.11)	(-3.45)	(-2.31)	(-2.31)
Buy^{family}	-0.002	-0.00291	0.00195	-0.00178	-0.00343	-0.0036
	(-0.99)	(-1.36)	-0.27	(-0.25)	(-0.98)	(-1.00)
Risk Factors	Yes	Yes	Yes	Yes	Yes	Yes
N	1,188,366	1,188,366	72,251	72,251	433,334	433,334
R^2	0.202	0.202	0.243	0.255	0.218	0.218

Panel B: Peer Stock Selection and Returns – 3 month formation period

	(1) All	(2) All	(3) Expertise	(4) Expertise	(5) Local	(6) Local
F^{plant}	0.009		0.00232		0.0156	
	(0.80)		(0.25)		(1.02)	
Quintile2		-0.0007		-0.0039		-0.00082
		(-1.20)		(-1.56)		(-0.89)
Quintile3		-0.0000		-0.00498**		0.000264
		(-0.13)		(-2.07)		(0.35)
Quintile4		-0.00144**		0.00159		-0.00193**
		(-2.03)		(0.45)		(-2.04)
Quintile5		-0.00144		0.00539		-0.0003
		(-0.26)		(0.76)		(-0.04)
Expertise Stock	0.00479*	0.00507*			-0.00359	-0.00309
	(1.69)	(1.85)			(-1.17)	(-1.00)
Local Stock	0.00191	0.00189	-0.0110*	-0.0106*		
	(0.56)	(0.56)	(-1.76)	(-1.67)		
F^{zip}	0.00713	0.0109	0.0379	0.0307	0.0263	0.0306
	(0.13)	(0.20)	(0.53)	(0.46)	(0.52)	(0.60)
Risk Factors	Yes	Yes	Yes	Yes	Yes	Yes
N	1,188,366	1,188,366	72,251	72,251	433,334	433,334
R^2	0.203	0.206	0.243	0.25	0.219	0.22

Panel C: Trading of Peers and Returns – 12 month formation period

	(1) All	(2) All	(3) Expertise	(4) Expertise	(5) Local	(6) Local
Buy^{plant}	-0.00122		-0.0168*		0.00201	
	(-0.59)		(-1.75)		(0.47)	
Quintile2		0.000464		-0.00642*		0.000296
		(0.68)		(-1.72)		(0.26)
Quintile3		0.000069		0.00436		-0.00125
		(0.10)		(0.87)		(-1.22)
Quintile4		0.000976		0.00864		-0.0000
		(1.02)		(1.37)		(-0.03)
Quintile5		0.0000		-0.00545		0.00142
		(0.00)		(-0.82)		(0.59)
Expertise Stock	0.0055	0.00547			0.00136	0.00134
	(1.64)	(1.64)			(0.38)	(0.38)
Local Stock	0.00166	0.00169	-0.0042	-0.00258		
	(0.53)	(0.54)	(-0.95)	(-0.56)		
Buy^{zip}	-0.0038	-0.00509	-0.0375	-0.0567	-0.00356	-0.00263
	(-0.13)	(-0.16)	(-0.92)	(-1.38)	(-0.07)	(-0.05)
Buy^{family}	0.000286	0.000162	-0.00027	-0.002	-0.00193	-0.00183
	(0.17)	(0.09)	(-0.08)	(-0.54)	(-1.04)	(-0.90)
Risk Factors	Yes	Yes	Yes	Yes	Yes	Yes
N	4,314,375	4,314,375	262,910	262,910	1,599,905	1,599,905
R^2	0.234	0.234	0.265	0.273	0.252	0.252

Panel D: Peer Stock Selection and Returns – 12 month formation period

	(1) All	(2) All	(3) Expertise	(4) Expertise	(5) Local	(6) Local
F^{plant}	0.00498		-0.00218		0.00849	
	(0.59)		(-0.22)		(0.83)	
Quintile2		-0.000780***		-0.00064		-0.00035
		(-3.08)		(-0.49)		(-1.05)
Quintile3		-0.00012		-0.00061		0.000162
		(-0.42)		(-0.40)		(0.45)
Quintile4		-0.00101***		0.0027		-0.00134***
		(-2.64)		(1.13)		(-3.08)
Quintile5		0.00129		0.00866		0.0023
		-0.32		(1.60)		(0.47)
Expertise Stock	0.00540*	0.00535*			0.000939	0.00101
	(1.70)	(1.74)			(0.27)	(0.30)
Local Stock	0.00162	0.00165	-0.00476	-0.00445		
	(0.53)	(0.53)	(-1.07)	(-0.99)		
F_t^{zip}	0.00438	0.00485	0.0046	-0.00356	0.0106	0.0115
	(0.14)	(0.16)	(0.13)	(-0.10)	(0.33)	(0.36)
Risk Factors	Yes	Yes	Yes	Yes	Yes	Yes
N	4,314,375	4,314,375	262,910	262,910	1,599,905	1,599,905
R^2	0.234	0.235	0.264	0.268	0.254	0.254