UNIVERSITY OF ABERDEEN

Business School

Department of Economics

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Discussion Paper in Economics No 15-1

February 2015

ISSN 0143-4543

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Abstract

Stock market investment decisions of individuals are positively correlated with those 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 are not associated with higher 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.

*This work has received financial support from the Norwegian Research Council under grant number 178944 and the National Centre of Competence in Research "Financial Valuation and Risk Management" (NCCR FINRISK). We thank Verdipapirsentralen (The Norwegian Central Securities Depository) for providing the data, Øystein Hungerholt for data assistance, and Bernt Arne Ødegaard for providing factor data for Norway. Finally, we are grateful to an anonymous referee, James Banks, Trond Døskeland, Rachel Griffith, Romano Gruber, Michel Habib, René Hegglin, Kasper Meisner Nielsen, Kjell Nyborg, Kristian Rydqvist, Andrei Simonov (discussant), Gaute Torsvik and seminar participants at University of Bergen, Gothenburg University, University of Helsinki, University of Lugano, University of Manchester, Michigan State University, Norwegian School of Economics, SUNY Binghamton, the HKUST Symposium on Household Finance, the 2013 SFI-FINRISK research day in Gerzensee, the 2014 Four Nations Cup in Lausanne, and the 2013 European Finance Association meetings in Cambridge for valuable comments.

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1 Introduction

The social psychology literature emphasizes the strength of face-to-face communication between individuals that frequently interact in producing and altering beliefs.[1] In the finance literature, Hong, Kubik and Stein (2004), show that social interaction leads to greater stock market participation. In this paper we examine the role of social interaction in the workplace on the investment decisions of individual investors. Conversations at work occasionally center on the stock market and, we conjecture, 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. Conversations with colleagues about stocks can also raise awareness of, or trust in, equity markets and make trading more likely (Guiso and Japelli, 2005, Guiso et al., 2008).[2]

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. 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 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.

The results suggest strong social interaction effects. For example, a one standard

[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. In a case study with a randomized trial design, Duflo and Saez (2003) document workplace social influence in the decision to enroll in a tax deferred account retirement plan.

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

deviation increase in the fraction of co-workers that make a purchase in a given month is associated with a 41 percent increase in the 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 195 percent increase in the fraction of that month's purchases 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 for 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 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 the results, we would expect the correlation with future co-workers to be of comparable magnitude to

[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.

the correlation with *current* co-workers.

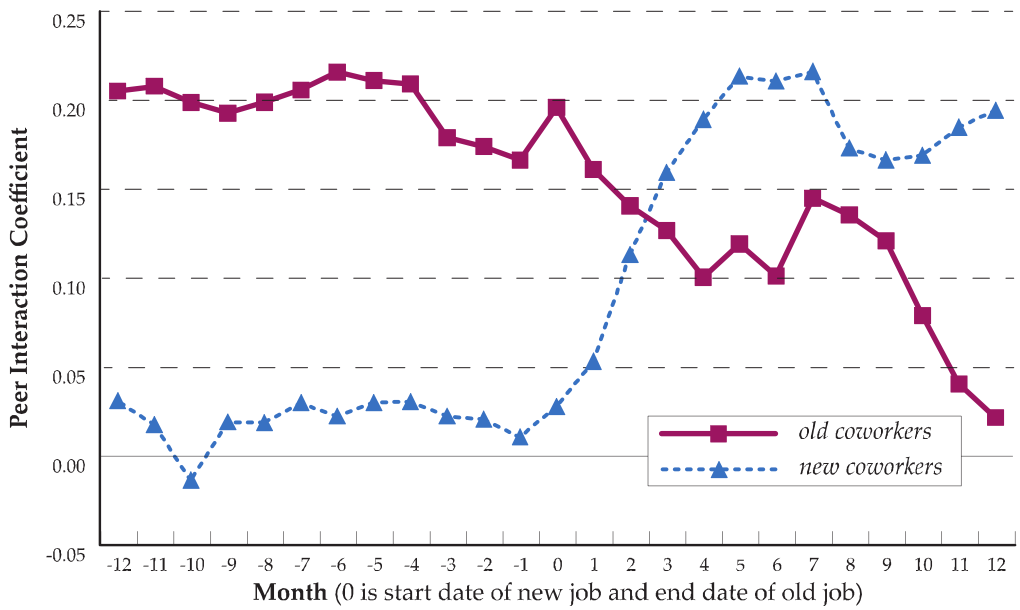

Figure 1. See the appendix for a detailed caption.

In Figure 1, the blue dashed line illustrates how the correlation in purchasing behavior with individuals that become co-workers after Month 0 evolves over time. Up to three months before the move, the correlation in purchasing activity with these future peers is close to zero. Thus endogenous group membership seems to be of minor concern. The red solid line illustrates how the correlation with individuals that are co-workers prior to the move evolves over time. Prior to Month 0 the correlation is significantly higher than the correlation with future co-workers. We discuss Figure 1 further in Section 3.2.

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 rumors, or involve imitation of unsound practices (March, 1991).[6] 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 are higher when co-workers purchase that stock more intensely. We find that purchases made under strong 'purchase pressure' do not outperform purchases made under weak purchase pressure. Hence the social interaction effects we document do not seem rooted in diffusion of 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. 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). 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. Moreover, within-industry stock purchases made under stronger peer pressure are not associated with significantly higher investment returns. Taken together these results suggest that investment mistakes can be propagated by social interaction.

Overall, the 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. 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 work on social interaction among individual investors (Hong et al., 2004, Ivković and Weissbenner, 2007, Brown et al., 2008, Kaustia and Knüpfer, 2012) is based on analysis of large groups, such as regions or neighborhoods, where identification of social effects is difficult (e.g.,

[6] 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."

Moffitt, 2001).[7] We construct peer groups at a much more local level, the workplace, 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. It also contrasts with Beshears et al. (2011) which finds negative co-worker peer effects ("boomerang effects") in the adoption of a simplified 401(k) plan.

Second, we provide empirical evidence on whether information obtained through social interaction is useful 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 root of social interaction in our setting seems to at best be 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, income or genetics (e.g., Campbell, 2006, Cohen, 2008, Cronqvist and Siegel, 2013), we emphasize the role of social interaction.[8]

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

[7] The same point can be made about much of the literature on social interaction in economics (e.g., Bertrand et al., 2000, and Moretti, 2011). Whilst our focus is social interaction in a naturally occurring group, a related literature considers social interaction effects under randomized group formation (e.g., Bursztyn et al., 2013, Dahl et al., 2012).

[8] The economics literature emphasizes positive spillover effects, e.g., Mas and Moretti (2009) on worker productivity. Our findings have an interesting parallel in the medical literature; Christakis and Fowler (2007) provide evidence consistent with obesity in the U.S. spreading through social interaction.

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 transaction date, 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. 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 sociodemographic characteristics of investors. The data comes from government registries assembled for tax-collection purposes, and is highly reliable.[9]

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.[10] For individuals that change firms during the sample period, the Statistics Norway data 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.

2.1 Sample Selection

The 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% of the population). We omit individual-years where the individual is employed part-time, or

[9] 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.

[10] 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.

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 the 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 (the results are slightly stronger if we keep these industries). These restrictions define a 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 zip code peer groups, individuals employed in the financial sector). The family peer group contains the spouse, children, parents, grandparents, grandchildren, siblings, uncles, aunts, cousins, nieces and nephews of the individual. The geographic peer group contains all individuals that live in the same zip code as the individual. 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 the purchase decision analysis of Section 3, we keep individuals 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. The purchasing activity of co-workers is our main explanatory variable, and we impose b) and c) in order to control for the purchasing activity of zip code and family members (these controls would not be defined otherwise). We also require that the sociodemographic variables are non-missing (this requirement only affects a small fraction of individual years). This leaves us with $97,264$ unique individuals over the entire period. In Panel A of Table $A2$ of the Appendix we provide sociodemographic descriptive statistics of the sample individuals (a random year for each individual has been selected). In the year 2000, the sample individuals are spread over about $2,600$ zip codes and roughly $18,000$ plants.

The stock selection analysis of Section 4 conditions on a purchase having been made (by definition) and therefore implies different sample selection criteria. For an individual-month to be included in the sample we require that the individual, at least one co-worker and one person in the zip code make a purchase in that month. Panel C of Table $A2$

of the Appendix provides descriptive statistics of the 118,432 unique individuals present in the stock selection analysis. The sociodemographic characteristics are similar to the sample used when examining the decision to purchase a stock (covered in Section 3). The sample is somewhat larger than in Section 3 because we exclude the family peer group. Restricting the analysis to individuals that have family members that purchase stocks in the same month would leave us with only 2,800 unique individuals. In unreported regressions we have verified that the results are very similar for this subsample, even after controlling for family members stock selection.

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 from the data. 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 the database due to this restriction. 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).[11] 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 14,284 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 variables, including plant and zip code size, the movers are on average very similar to the overall sample.

[11] 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.

3 The Purchase Decision

In this section we relate the decision of an individual to purchase common stocks in a given month with purchasing activity of co-workers. The motivation is simple; more trading by co-workers is expected to create more “buzz” about the stock market, and make the individual more likely to also trade. As our analysis includes a very large number of fixed effects, we use a linear probability model as our benchmark.

3.1 Basic Results

We estimate the following relation at the individual-month level,

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

$buy_{i,t}$ is a dummy that equals one if individual i purchases a stock in month t, and zero if not. $Buy_{i,t}^{plant}$ is the fraction of co-workers that purchase a stock that month (not including i himself) at the plant where individual i works. Table 1 presents descriptive statistics of our main dependent and independent variables. In equation (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, labor income, sex and the number of years of education, and various powers thereof (see the caption to Table 2 for specifics). For income and wealth we use the values reported in last year's tax return. As additional controls, we include $Buy_{i,t}^{fam}$ and $Buy_{i,t}^{zip}$. These variables control for correlation in timing of purchases within the zip code and inside the family, and are defined in the same manner as $Buy_{i,t}^{plant}$. Importantly, all of these variables exclude the individual since otherwise there would be a mechanical relation between the individual and the peer group. 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. For the same reason we include zip code fixed effects. We report t-statistics based on robust standard errors clustered (two-way) around time and plant. 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).

Panel A of 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 column (3) a one standard deviation increase in co-worker trading activity ($Buy_{i,t}^{plant}$) results in an increase in trading activity of 40.90% relative to the unconditional mean.[12] 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 of β is similar to that reported in column (3). In addition, we have considered specifications without fixed effects. In this case, the point estimates and economic magnitudes are larger than for (3).

We can note that the zip code level correlation in trading behavior is significantly reduced when workplace peer effects are introduced; the introduction of co-workers reduces the impact of neighbors by roughly 20% (comparing column (2) to (3)). In contrast, the impact of workplace peers is much less affected by the introduction of neighbors (3% reduction, when going from column (1) to (3)). This is what we would expect if the positive correlation at zip code level is partially driven by co-workers that live close to each other. 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.

The results could be driven by events in the industry or in the region, such as writings in industry journals or in local newspapers. In (5), 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. We account for time variant local events by including a fixed effect for each municipality-month combination in our dataset (there are 459 municipalities in Norway). The results, reported in column (6), are similar to those reported in column (3).[13]

[12] The estimated impact of family and neighbors is lower; a one standard deviation increase is associated with an increase in trading activity of 23% and 19% respectively.

[13] We have also considered the influence on non-coworkers on the individual as a placebo test. We constructed an analogue to Buy^{plant} for all non-coworkers ($Buy^{non-plant}$), capturing the purchasing intensity of non-coworkers. Since each of our plants represent a small fraction of the economy there is very limited variation across plants in $Buy^{non-plant}$ and therefore collinearity with our time fixed effects. When we exclude time fixed effects we find that $Buy^{non-plant}$ is postively related to the individual's

In the stock market participation model of Hong et al. (2004) some investors are susceptible 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.

The extant literature has documented that individual investors have a preference for local stocks (e.g., Coval and Moskowitz, 1999 and Huberman, 2001) and stocks from their industry of employment (Døskeland and Hvide, 2011). In unreported analysis we create new dummy variables that indicate whether the purchase made was local (the firm's headquarter is not more than 100 kilometers away from the zip code where the individual lives) or within the industry of emplyoment (the stock two digit NACE code matches that of the plant). Using these dummy variables we verify that co-worker peer effects are present across local, non-local, same-industry and different-industry stocks. In Section 4, where we study stock selection we also verify that those results are not restricted to local and same-industry stocks (see Table 9).[14]

We have also examined whether the peer effects that we document are restricted to particular industries. In Table $A3$ we estimate a separate co-worker peer effect for each of 36 industries that represent a significant proportion of the sample (no single industry accounts for more than 12% of the investor observations). These results strongly indicate that correlation in trading behavior among co-workers is universal across industries.

Social interaction can also affect sell decisions. Sells are restricted to stocks already owned by the individual (very few individual investors go short), and we therefore expect co-workers to have a positive effect on individual sells, but a weaker effect than on purchases (for a related argument, see Barber and Odean, 2008). To investigate the role of social interaction in sells, we define $sell_{i,t}$, a dummy variable that takes the value 1 if the individual makes a sale in month t and otherwise is 0. Our main explanatory variable

purchase decision. This is not surprising since $Buy^{non-plant}$ captures economy-wide sentiment. However, as expected, the economic impact of non-coworkers is limited when compared to actual co-workers (less than one fifteenth).

[14] In unreported regressions we find that co-worker same-industry purchases have a greater effect on individual same-industry purchases than on co-worker non same-industry purchases. This is not surprising if the co-worker and the individual actually purchase the same stock. Since we actually examine whether the individual purchases the same stocks as his co-workers in Section 4 we omit these results for brevity.

is $Sell_{i,t}^{plant}$ which is the fraction of the individual's co-workers making a sale in month t. In Panel B of Table 2 we re-estimate equation (1) using sale analogues. We consider the same specifications as in Panel A. Our results do suggest a positive peer effect on sells, but significantly smaller than on purchases; in column (3) a one standard deviation increase in $Sell_{i,t}^{plant}$ is associated with a 14.33% increase (relative to the unconditional mean) in the likelihood that the individual makes sale in that month. The vector $\mathbf{\Gamma}$ contains the same controls as in Panel A.

A drawback of the linear probability model is that it may imply predicted probabilities outside the unit interval. In Panel C we re-estimate equation (1) using the conditional fixed effects logit estimator.[15] We report odds ratios of our main variables and z-statistics based on standard errors clustered around plant.[16] Consistent with the results in Panel A, we find that the effect of $Buy_{i,t}^{plant}$ is statistically significant at the one percent level. In specification (3), the co-worker peer effect has an odds-ratio of 10.24. This implies that if $Buy_{i,t}^{plant}$ goes from zero to one the odds increases roughly ten-fold. Overall, the conditional fixed effects analysis complements our LPM results and it is reassuring that the results are qualitatively similar.

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 to a monthly level.[17] Workers that move between plants allow for a placebo test: we analyze how individual

[15] As pointed out by Chamberlain (1980) this estimator avoids the incidental parameter problem (Neyman and Scott, 1948) that would arise if using a probit or logit model where the fixed effects are estimated. This is particularly useful in our context since we have $164,713$ postcode-plant categories implying relatively few observations per fixed effect.

[16] When using the conditional fixed effects logit it is natural to report odds ratios. The reason is that all of the independent variables affect marginal effects in the logit setting and the fixed effects (postcode-plant in our context) are not estimated. Greene (2012) p. 763 notes "Because the fixed effects are not estimated, it is not possible to compute probabilities or marginal effects with these estimated coefficients, and it is a bit ambiguous what one can do with the results of the computations." We only cluster in the plant dimension since the conditional fixed effects estimator cannot accomodate standard errors clustered in multiple dimensions (see, Cameron and Miller, 2010).

[17] Bodnaruk (2009) uses investor moves to show that individuals shift their portfolios towards stocks that become local.

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 the results, we would expect the correlation with future co-workers to be of comparable magnitude to the correlation with *current* co-workers.

Considering 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, 1976, Moreland, 1985, Ashfort and Saks, 1996). 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.'

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 indicate whether that month is prior to leaving (joining) the old (new) plant or not (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 to each other). The variable $Buy_{i,t}^{old\ before}$ 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 $Buy_{i,t}^{old\ after}$ 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, $Buy_{i,t}^{new\ before}$ and $Buy_{i,t}^{new\ after}$, 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. 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. We estimate,

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

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 a major concern. In column (3) we include both current and future co-workers. The placebo coefficient from (2) is reduced by more than a fourth while the coefficient on $Buy_{i,t}^{old\ before}$ is not significantly affected. 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.

Column (7) shows that the correlation with old co-workers significantly drops after the individual leaves the old plant (the before-after difference is significantly different from zero at the one 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 similar to that reported in column (5).

In column (9) we include all sample months and consider the full specification 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 $Buy_{i,t}^{plant\ old}$ and $Buy_{i,t}^{plant\ new}$ and estimate the following regression,

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

The vector $\mathbf{\Gamma}$ contains the same controls as used when we estimated (2) and are described in the caption to Table 2.[18] 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

[18] 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 plant and the new plant, in fact we estimate

$$buy_{i,t} = \sum_{i=-13}^{13} \beta_t Buy_t^{plant\ old} \times \mathbf{1}_{old,t} + \sum_{i=-13}^{13} \beta_t Buy_t^{plant\ new} \times \mathbf{1}_{new,t} + \mathbf{b\Gamma} + \varepsilon_{i,t} \tag{4}$$

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.

regression are exhibited in Figure 1.

In Figure 1, the blue dashed 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 solid 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.

These findings 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 proximity to those co-workers.

3.3 Can shocks at the plant-month level drive the results?

The results could be driven by events at the plant-month level, such as visits from equity brokers or from investment advisors. We deal with this issue in two ways. First, if plant-month shocks are behind the results, we would expect a similar correlation in trading behavior between pairs of individuals at small and large plants. On the other hand, if social interaction drives our results we would expect stronger correlation between individuals at a small plant than at a large plant, simply because two individuals are more likely to engage at a small plant. To test this hypothesis, for each month we rank all plants into ten size deciles, based on number of employees. We then sample two individuals from each plant-month and estimate the within-plant correlation in purchasing activity across size deciles. For each of the plant size deciles we estimate the following regression,

$$buy_{i,t} = \beta buy_{j,t} + \mathbf{b\Gamma} + \varepsilon_{i,t} \tag{5}$$

where $buy_{j,t}$ is a dummy variable that takes the value 1 if the co-worker j made a purchase in that month and otherwise it is 0.[19] As before, we include $Buy_{i,t}^{fam}$, $Buy_{i,t}^{zip}$ as well as

[19] Bayer et al. (2008) use a similar regression strategy to study the role of informal networks in job hiring.

sociodemographic controls, month and plant fixed effects.

Panel A of Table 4 presents our point estimates of β for all size deciles. We also tabulate the mean number of employees for each size decile. In smaller plants the effect of co-workers is much larger than in larger plants. It is striking that as the number of workers increases from 4.74 (decile 1) to 21.91 (decile 4) the peer effect is reduced threefold.

In order to benchmark the peer effect we assign each individual a 'placebo co-worker' and examine the influence that they have on the individual's trading decision. Using the selected individuals we randomly assign them a co-worker from a different plant within their size decile and then we re-estimate equation (5) using the buying intensity of the placebo co-worker. Panel B of Table 4 tabulates the associated point estimates of the effect of placebo peers on the purchase decision of our individuals. As expected, the impact of placebo peers is economically marginal and it is only statistically significant for one size decile.

Figure 2 plots the effect (β from estimating (5)) of co-workers (solid red line) and placebo co-workers (dashed blue line) for different plant sizes. There is a sharp decrease in the effect of co-workers (y-axis) as we go from decile one to decile five. Additionally, in deciles one through four the effect co-workers is significantly greater than the effect of placebo peers.

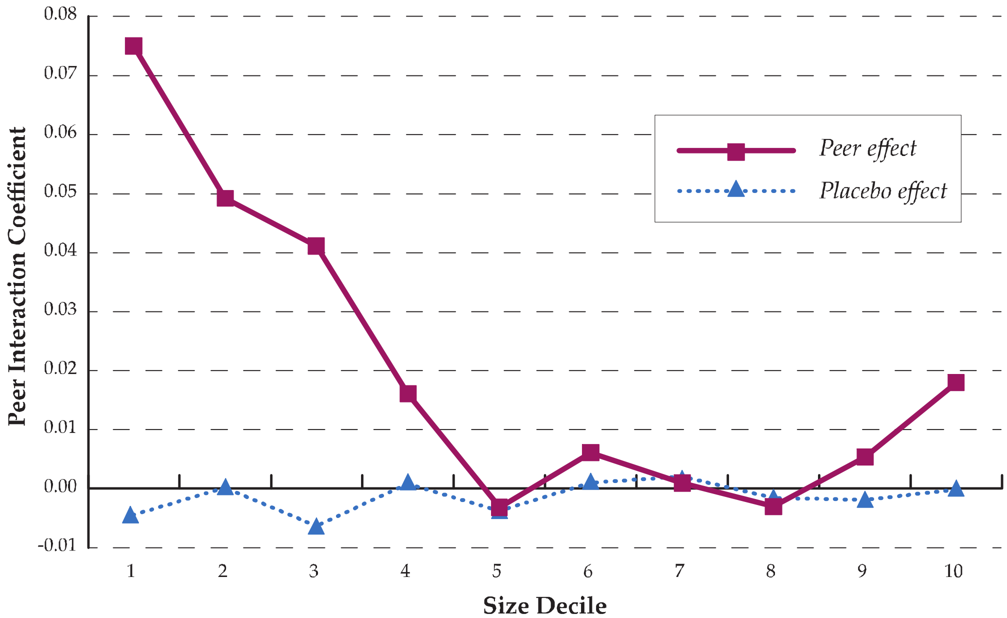

Figure 2. See the appendix for a detailed caption.

Another way to deal with the possibility of shocks at the plant-month level is to exploit that some subset of workers are more likely engage. Peer groups are likely to form along sociodemographic patterns. For example, females may talk more with females than with males, and individuals in the same age group may be more likely to talk. In Table *A*4 of the Appendix we follow Duflo and Saez (2002) and regress individual purchases on purchases made for each subgroup separately on the purchase decision. Similar to in Duflo and Saez (2002), the estimated peer group coefficients are more often than not larger within subgroups than between.

4 Stock Selection

In this section we consider the relation between an individual's stock selection and the stock selection decisions made by her co-workers. The motivation is simple; co-workers are likely to discuss their stock selection decisions and thereby attract attention to the stocks selected. The regression methodology is similar to the one applied by Ivković and Weisbenner (2007) in the study of industry selection.

We 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 we restrict our attention to only those months in which the individual makes at least a purchase (to study stock selection). An advantage of considering the stock selection decision is that it is less influenced by liquidity shocks than the purchase decision. The dependent variable, $f_{i,t,s}$, is defined for all stocks present in that month, and $\sum_s f_{i,t,s} = 1$ by construction. As main explanatory variable we construct an analogous variable, $F_{i,t,s}^{plant}$, which is the fraction of purchases made by individual i's co-workers (excluding the individual's purchases) that is invested in stock s. Again this variable is only defined if at least one co-worker makes a purchase in month t (if we did not condition on a purchase, the variable would confound stock selection with the decsion to be active). Table 5 provides descriptive statistics. 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 the sample period.

4.1 Basic Results

To relate individual stock selection to that of her co-workers, we estimate the following regression:

$$f_{i,t,s} = \beta F_{i,t,s}^{plant} + \mathbf{b}\mathbf{\Gamma} + \varepsilon_{i,t,s} \tag{6}$$

The coefficient β captures the extent to which stock selection of an individual is correlated with that of co-workers. To capture that a particular plant has a preference for a particular stock we include fixed effects for each stock-plant combination in $\mathbf{\Gamma}$. These control, for example, for the possibility that a plant has a business relationship with a particular listed company. 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, $F_{i,t,s}^{zip}$, which is defined in the same manner as $F_{i,t,s}^{plant}$. Both

$F_{i,t,s}^{plant}$ and $F_{i,t,s}^{zip}$ sum to 1 across stocks in a given month.

The results are presented in Table 6. In the regressions we have $87,812,052$ stock selection decisions, which corresponds to roughly $440,000$ purchase months or 3.7 purchase months per investor in the sample (the individual trading patterns reported here are similar to those found in Døskeland and Hvide, 2011). Column (3) is the main specification; the estimated β coefficient is positive and highly significant. In terms of economic magnitude, in column (3) a one standard deviation increase in the fraction of co-workers that purchase the same stock results in a 195% increase relative to the unconditional mean.[20] In columns (4) to (8) we consider alternative fixed effects. As evidenced by column (4), the introduction of plant-year-stock and postcode-year-stock fixed effects does not qualitatively alter the results. Neither does introducing municipality-stock fixed effects (column (6)).[21,22]

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 $F_{i,t,s}^{old\ before}$ 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. Similarly, the variable $F_{i,t,s}^{old\ after}$ is the fraction invested by co-workers at the old plant in stock s after the individual has left the plant. Before

[20] We can note from column (2) and (3) that the correlation with geographical neighbors drops 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 partly proxying for social interaction in the workplace.

[21] 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 $F_{i,t,s}^{plant}$ is very similar for this subsample also after controlling for $F_{i,t,s}^{fam}$.

[22] Similar to in the purchase analysis we examine the effect of non-coworkers on stock selection. We calculate $F_{i,t,s}^{non-plant}$ as the fraction of non-coworker purchases invested in stock s in month t. Unfortunately, there is little variation across individuals in $F_{i,t,s}^{non-plant}$ in a particular month which implies that there is collinearity between $F_{i,t,s}^{non-plant}$ and our $month \times stock$ fixed effects. When we exlcude our $month \times stock$ fixed effects we find that both $F_{i,t,s}^{non-plant}$ and $F_{i,t,s}^{plant}$ are statistically and economically significant.

that the variable takes the value of 0. The variables describing the stock selection of new plant co-workers, $F_{i,t,s}^{new\ before}$ and $F_{i,t,s}^{new\ after}$ 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 the new plant. The above mentioned selection criteria leaves us with $6,458$ 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. We estimate,

$$f_{i,t,s} = \beta_1 F_{i,t,s}^{old\ before} + \beta_2 F_{i,t,s}^{old\ after} + \beta_3 F_{i,t,s}^{new\ before} + \beta_4 F_{i,t,s}^{new\ after} + \mathbf{b\Gamma} + \varepsilon_{i,t,s}. \tag{7}$$

To control for geographical differences in preferences for certain stocks we include zip code-stock fixed effects in $\mathbf{\Gamma}$. As additional controls, we include zip code level stock selection, $F_{i,t,s}^{zip}$. The sample size is not sufficient to include plant-stock fixed effects, which 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 equation (7).

The results are presented in Table 7. 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) we relate individual purchases to that of future co-workers. The coefficient is noticeably smaller than the coefficient measuring the effect of current co-workers found in column (1). The magnitude and statistical significance of the coefficient is likely to be related to the omission of plant-stock fixed effects. In column (3) we include as regressors both the stock selection of current and future co-workers. Neither of the coefficients from (1) or (2) are much affected. In columns (4)-(6) we repeat the exercise of the previous three columns, but we now focus on the 12 months after the individual has joined the new plant. Notably, the coefficient on the stock selection new co-workers is not affected by including a control for previous co-workers, as seen from column (6).

In columns (7)-(9) we combine the period before the move with the period after joining the new plant. Column (7) shows that the correlation with new co-workers significantly increases after the individual joins the new plant (the before-after difference is significantly different from zero at the one 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 consider the full specification described in Equation (7). 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.

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

$$f_{i,t,s} = \sum_{t=-12}^{12} (\beta_{old,t} F_{i,t,s}^{plant\ old} + \beta_{new,t} F_{i,t,s}^{plant\ new}) \mathbf{1}_t + \mathbf{b}\mathbf{\Gamma} + \varepsilon_{i,t,s} \tag{8}$$

The vector $\mathbf{\Gamma}$ contains the same controls as equation (7). 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 3.

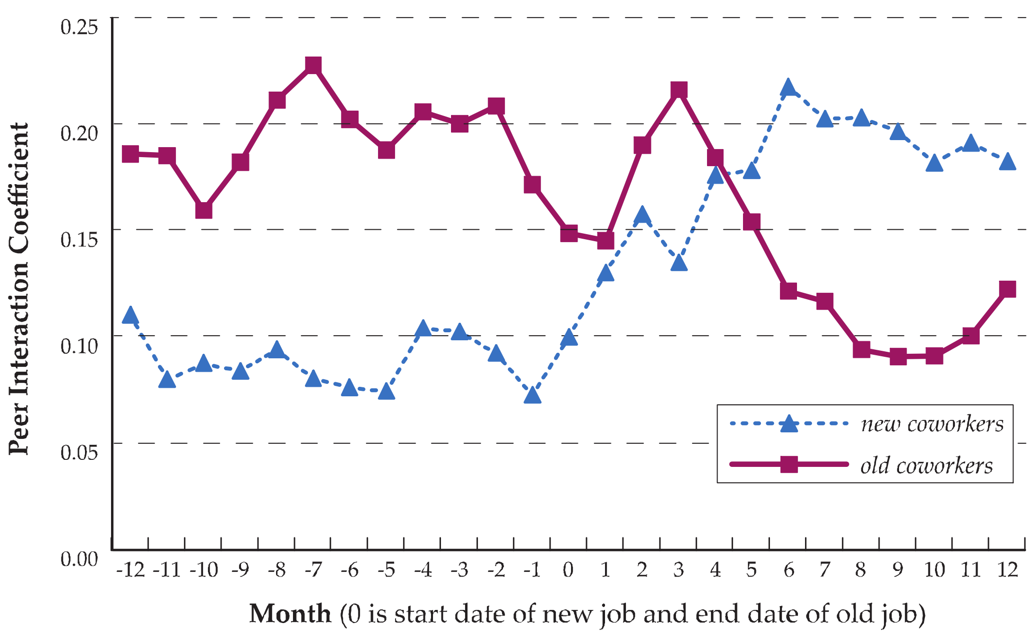

Figure 3. See the appendix for a detailed caption.

In Figure 3 we plot the interacted peer coefficient against the number of months before the move. Figure 3 looks similar to Figure 1.[23] Prior to the move the effect of old co-workers is greater than the effect of new co-workers. However, following the move the effect of new co-workers surpasses that of old co-workers. This indicates that the investment decisions of individuals is most affected by those peers that they interact the most with.

We also verify that the estimated peer effect gets smaller as the plant size increases (the stock selection analogue to section 3.3). From each plant and month we keep the stock selection decision of one individual and one of his co-workers. We divide all of our plants into ten size deciles and for each decile we estimate the following regression:

$$f_{i,t,s} = \beta f_{j,t,s} + \mathbf{b\Gamma} + \varepsilon_{i,t,s} \tag{9}$$

where $f_{j,t,s}$ is the allocation of co-worker j to stock s in month t. We control for stock selection at the postcode level by including $F_{i,t,s}^{zip}$ in $\mathbf{\Gamma}$. We also include plant-stock and month-stock fixed effects. As before our standard errors are clustered both at the time and plant level. Panel A of Table 8 presents our point estimates of β for all size deciles. The point estimate for size decile one is more than five times as large as the point estimate for decile ten. The mean number of employees in size decile one is 7.22 versus $1,464.67$ for decile ten.

We benchmark the peer effect by re-estimating equation (9) when the individual is paired with a random non-coworker in the same size decile. Panel B of Table 8 tabulates the associated point estimates of the effect of placebo peers on the stock selection decision of our individuals. As expected, they are markedly lower than those reported in Panel A.

[23] Recall that we do not include plant-stock fixed effects, so that the level of the estimates will be affected by plant-specific preferences for particular stocks. Thus we are mainly interested in the difference between points on the red versus points on the blue line.

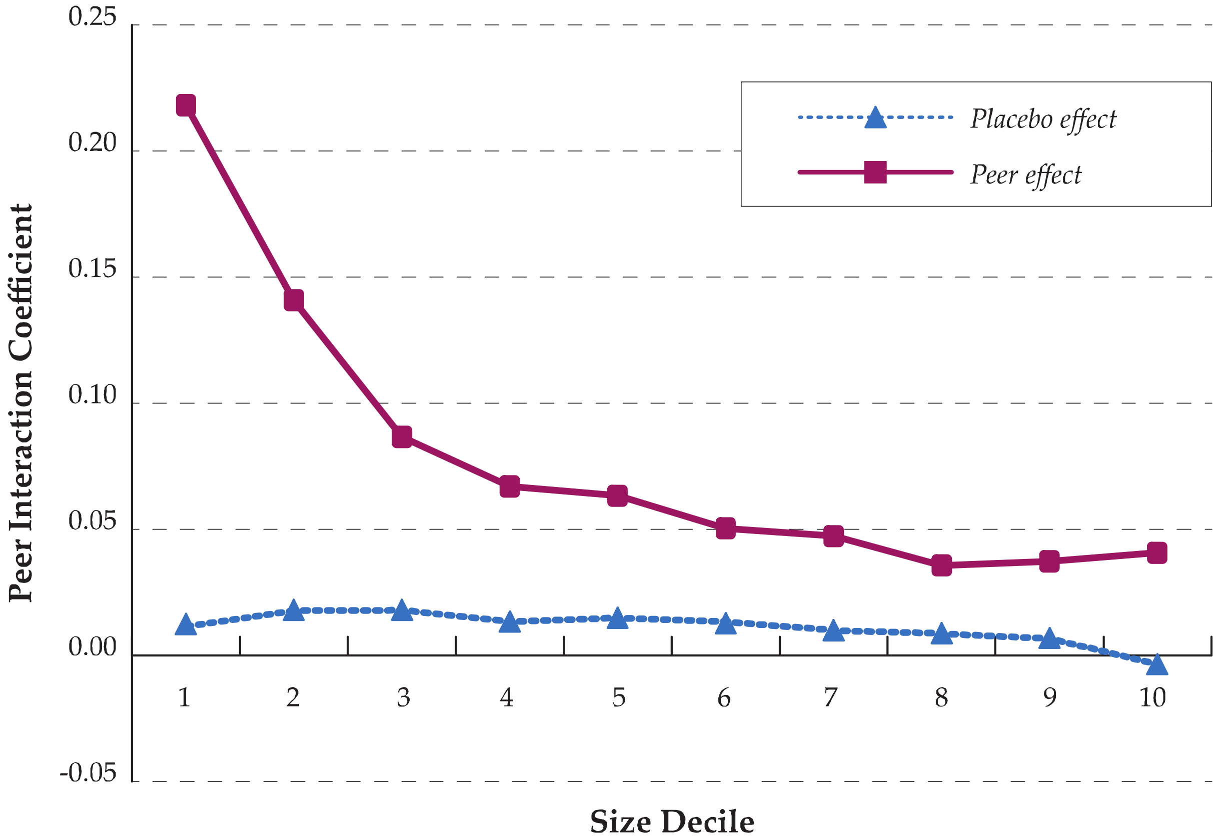

Figure 4. For a detailed caption see the appendix.

Figure 4 plots the effect (β from Eq. (9)) of co-workers (solid red line) and placebo peers (dashed blue line) for different plant sizes. There is a rapid decrease in the peer effect when going from decile one to four (the mean plant in size decile four has 61.87 employees). From decile four to ten there seems to be a more gradual decrease. However, the effect of co-workers is greater than that of placebo peers for all size deciles.

4.3 Same-Industry and Local Purchases

In the previous sections we found a close relation between the investment decisions of the individual and his / her co-workers. However, it is still an open question whether this is beneficial. In this section we take a first step (see also Section 5) in considering the effect of social interaction on the quality of investment decisions by considering whether social interaction contributes to the purchase of stocks that most likely are poor hedges of income risk (same-industry stocks, Døskeland and Hvide, 2011) and exposure to local economic conditions (local stocks, e.g., Coval and Moskowitz, 1999).[24,25] Thus, purchases of same-industry and local stocks are likely to be less than ideal from a diversification perspective.[26]

On the other hand, it is likely that local and same-industry stocks are salient objects of workplace conversations and thus peer group effects might be stronger for local and same-industry stocks than for non-local and different-industry stocks. To this end, in what follows we examine co-worker peer effects for these different types of stocks.

For each individual employed in the private sector, the 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 same-industry stock as a stock where the worker two-digit NACE code matches the NACE code of the stock. We classify all stocks as being local to the individual if the distance from the place of residence of the individual to the stock headquarters is less than 100 km. We create four new dependent variables $f_{i,t,s}^{same-industry}$, $f_{i,t,s}^{diff.-industry}$, $f_{i,t,s}^{local}$ and $f_{i,t,s}^{non-local}$ to capture the individual's selection of same-industry, different-industry, local and non-local stocks. For example, $f_{i,t,s}^{same-industry}$ is the fraction of total same-industry purchases made in month t invested in stock s. The other variables are defined analogously. Table $A6$ of the Appendix presents descriptive statistics of the dependent variables.

In Table 9 we re-estimate equation (6) using the new dependent variables. In column (1) to column (4) we consider local , non-local, same-industry and different-industry stocks, respectively. We include month-stock, plant-stock, and zip code-stock fixed effects

[24] The extant literature has documented that the return of within-industry stocks are correlated with labor income (see Baxter and Jermann, 1997, and Eiling, 2013).

[25] In the same vein, Massa and Simonov (2006) show that investors invest in stocks that have a high correlation with their non-financial income suggesting that investors do not use the stock market for hedging.

[26] Purchases of same-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 does not seem likely. Purchases of local stocks could be a hedge against shocks in the local price level.

in all specifications. The point estimate of $F_{i,t,s}^{plant}$ is always positive and statistically significant indicating that our previous results are not driven by individual investor preferences for local or same-industry stocks. As expected we find evidence that same-industry and local stocks are salient objects of conversation in the workplace. Although peer effects affect the selection of all four types of stock, the economic impact of social interaction on stock selection is larger for same-industry and local stocks than for their counterparts. A one standard deviation increase in the allocation of co-workers to a particular stock increases the individuals allocation to that stock by 211% for same-industry stocks and 134% for different-industry stocks. The corresponding numbers for local and non-local stocks are 183% and 157%, respectively. Thus, the results confirm that the economic impact is largest for those stocks that we expect to be discussed most frequently at the workplace. Additionally, these results suggest that one possible cause of local bias and within-industry bias is that social interaction centers around these kind of stocks. Finally, from a diversification perspective both local and same-industry stocks are arguably less than ideal and therefore this highlights that social interaction might lead to a sub-optimal portfolio allocation.

5 Should You Listen To Your 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. In this section we evaluate how peer pressure affects the performance of stock purchases using calendar time methodology (see, Odean, 1999, and Seasholes and Zhu, 2010). We measure peer pressure as the fraction of co-worker purchases allocated to stock s in excess of the economy-wide allocation to stock s. Hence, we rank all purchases made according to:

$$F_{i,t,s}^{plant} - F_{i,t,s}^{non-plant} \tag{10}$$

where $F_{i,t,s}^{non-plant}$ is the fraction of non-coworkers purchases allocated to stock s in month t.[27] Effectively, $F_{i,t,s}^{non-plant}$ controls for economywide trends. This implies that we consider instances when co-workers are more enthusiastic about a stock than the economy as a whole. This is important since some stocks are more popular choices by individual investors and therefore they will experience greater peer pressure mechanically.

Purchases made under above (below) median peer pressure are sorted into the high (low) peer pressure portfolio. This implies that each purchase is given a unique entry into either the high or low peer pressure portfolio. For purchases made in month t we consider the difference in return between the high peer pressure and the low peer pressure purchases $(HP-LP)_t$ in subsequent months.[28] To evaluate investor performance we run the following regression,

$$(HP-LP)_t = \alpha + \beta_1 MKT_t + \beta_2 SMB_t + \beta_3 HML_t + \beta_4 MOM_t + \varepsilon_t \tag{11}$$

where the risk factors MKT, HML, SMB and MOM, are all calculated for Norway by Ødegaard (2013).

In Panel A of Table 10 we examine the returns to peer purchases over different holding periods. We report monthly percentage alphas. For all holding periods, 1, 3, 6, 9 and 12 months, the abnormal return to the high minus low peer pressure portfolio is not statistically or economically significant. To verify that this result is not driven by the median cutoff in terms of peer pressure, in unreported results we also divide all purchases into quintiles on the basis of their peer pressure and examine the relative performance of extreme quintiles. Again, for all holding periods we do not find that peer pressure is associated with abnormal performance. Each column in Panel B of Table 10 examines the return to the peer pressure portfolio for a particular month after purchase. We consider the performance of the $(HP-LP)_t$ portfolio in months $t+2$, $t+3$, $t+4$, $t+5$ and $t+6$.

[27] We have also ranked purchases in terms of $Buy_{i,t}^{plant} - Buy_{i,t}^{non-plant}$ which captures more general stock market enthusiasm since it does not condition on the investor purchasing the same stock as his co-workers. The results suggest that there is no information in the enthusiasm of co-workers.

[28] Since we consider the performance of purchases in month $t+1$ and onwards we abstract from performance from the purchase day until the end of month t. Barber and Odean (2000) account for the performance within the purchase month and concludes that this does not qualitatively affect investor performance.

For all of these months we find that abnormal returns are insignificant.

The existing literature has documented evidence in favour of individual investors having superior performance when making local investments (Ivković and Weisbenner, 2005, and Massa and Simonov, 2006). Thus, it could well be that peer pressure is associated with abnormal performance when local or same-industry stocks are being discussed in the workplace. To that end, we separately consider all purchases of same-industry (same 2 digit NACE code) and local (headquartered within 100 km) stocks and then sort these purchases in terms of peer pressure. As before, purchases with above (below) median peer pressure are classified as having high (low) peer pressure. Panels C, D, E and F of Table 10 display the results from estimating (11) using as depedendent variable the difference in return between high and low peer pressure purchases of same-industry, different-industry, local and non-local stocks, respectively. In general, all of the panels confirm the previous findings that peer pressure is not associated with abnormal returns. Interestingly, high pressure same-industry purchases outperform low pressure same-industry purchases by 0.42% per month (Panel C). Although economically significant, the difference is measured with significant error and not statistically significant. Over a three month period the positive returns turn negative and when considering a horizon of 9 to 12 months the high peer pressure portfolio underperforms the low pressure portfolio, but again the performance difference is not statistically significant. For different-industry purchases the difference between high and low pressure purchases are universally economically and statistically insignificant. Taken together, the evidence does not suggest that listening to co-workers adds value to purchases.

In contrast, for local purchases there is weak evidence that high peer pressure purchases actually underperfom low peer pressure purchases. Over all horizons the difference is negative. When considering a three month horizon the monthly underperfomance is -0.083% and statistically significant at the 10% level.

Taken together these results suggest that it is unlikely that information about stock fundamentals is transmitted among co-workers. Overall, the results of this section combined with the findings of the previous sections of the paper lead us to conclude that individual investors follow the advice of their co-workers even though the advice does not contain value pertinent information.

6 Conclusion

This paper addresses whether co-workers influence investment choices. We employ comprehensive data from Norway that covers a large number of individual investors over 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 the results, as evidenced by the trading behavior of individuals that move between plants. As one would expect if the correlations are driven by social interaction (and not shocks at the plant level), the results are considerably stronger for small than for large plants.

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 the analysis is to focus on peer effects at a much more local level, the workplace, and to show that the measured social interaction effects are large even after accounting for correlated unobservables, endogenous group membership, and reflection.

We also analyze whether social interaction leads to better economic outcomes for the individuals that are affected. First, our evidence suggests that social interaction does not result in a superior portfolio allocation. Social interaction results in the purchase of same-industry and local stocks, both less than ideal from a diversification perspective. Second, we examine the performance of purchases made under greater peer pressure and we do not find evidence suggesting abnormal returns.

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

A recent literature addresses the co-movement of aggregate individual investor trading and asset returns (e.g., Kumar and Lee, 2006, Barber and Odean, 2008). One of the 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; we demonstrate that the workplace is a plausible candidate. Finally, our results have implications for theory. We find that purchases that are made under greater purchase activity by peers are not associated abnormally low or high 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

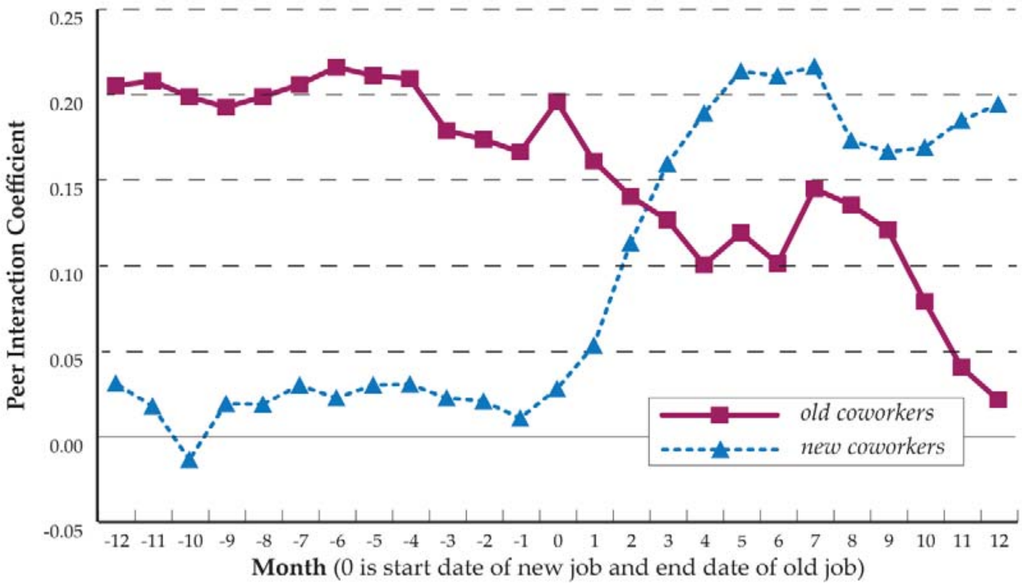

Figure 1: The above figure plots the slope coefficients from regression equation (3). The dependent variable is the dummy variable $buy_{i,t}$ that takes the value 1 if the investor makes a purchase in month t and 0 otherwise. Our main independent variables are the fraction of old (new) co-workers that make a purchase in month t interacted with 27 dummy variables, one for each of the 13 months prior leaving the old plant to 13 months after joining the new plant. We average three consecutive coefficients and in Figure 1 we plot the estimated coefficients from 12 months prior to leaving the old plant to 12 months after joining the new plant. We exclude investors that leave their job in December and join the new plant in January.

Figure 2: Plant size and peer effects

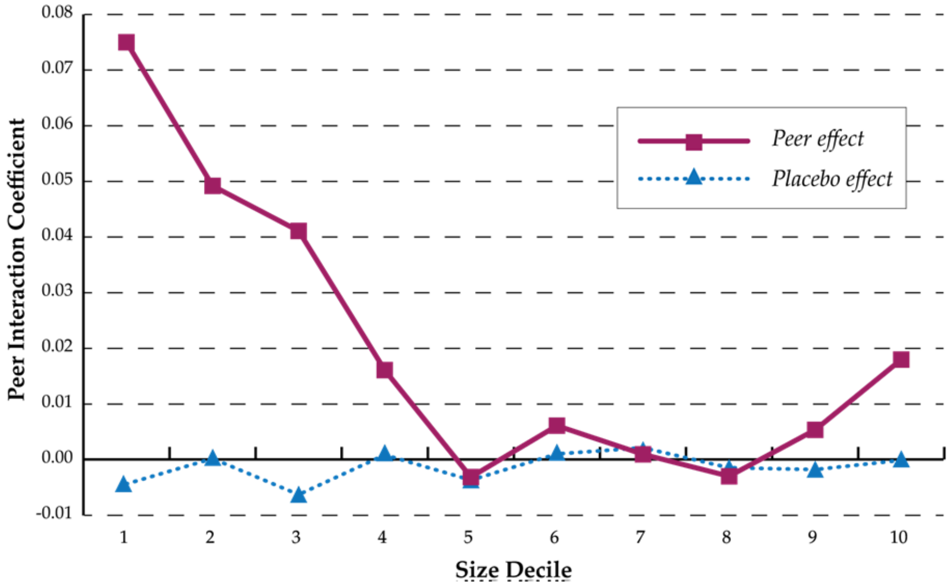

Figure 2: The above figure plots the slope coefficients from estimating equation (5). In each month we divide all of our plants into size deciles in terms of the number of employees. From each plant we sample two individuals (investor i and co-worker j). We relate the purchase decision of i ($buy_{i,t}$) to that of j ($buy_{i,t}$) by estimating the regression described in equation (5) for each size decile. The red solid line plots the estimated slope coefficients from the regressions. The blue dashed line labeled "placebo effect" plots the corresponding slope coefficients after matching investor i with a randomly chosen worker from a *different* plant in the same size decile.

Figure 3: Stock selection new and former co-workers

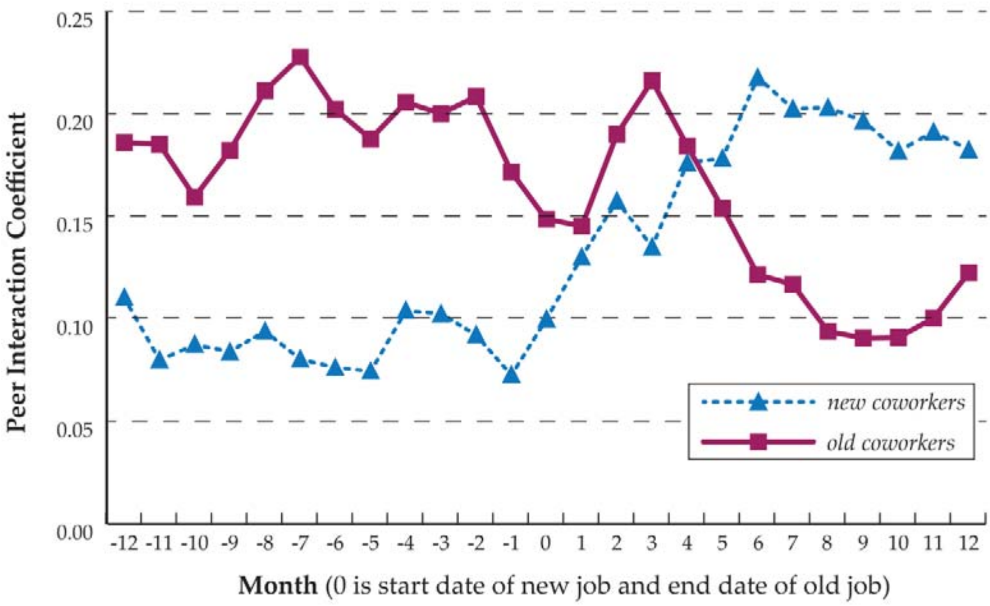

Figure 3: The above figure plots the slope coefficients from regression equation (8). The dependent variable is, $f_{i,t,s}$, the fraction invested by investor i in stock s in month t. Our main independent variables are the fraction invested in stock s in month t of old (new) co-workers interacted with 27 dummy variables, one for each of the 13 months prior leaving the old plant to 13 months after joining the new plant. We average three consecutive coefficients and in Figure 3 we plot the estimated coefficients from 12 months prior to leaving the old plant to 12 months after joining the new plant. We exclude investors that leave their job in December and join the new plant in January.

Figure 4: Stock selection in plants of different size

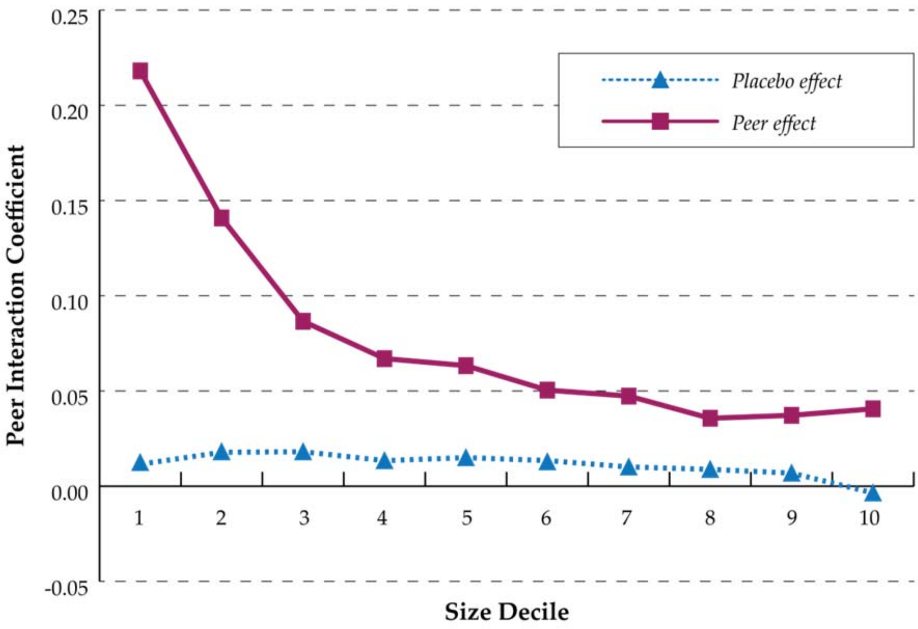

Figure 4: **In each month we divide all of our plants into size deciles in terms of the number of employees. From each plant we sample two individuals (investor i and co-worker j). We relate the stock selection decision of i ($f_{i,t,s}$) to that of j ($f_{j,t,s}$) by estimating the regression described in equation (9) for each size decile. The red solid line plots the estimated slope coefficients from the regressions. The blue dashed line labeled "placebo effect" plots the corresponding slope coefficients after matching investor i with a randomly chosen worker from a *different* plant in the same size decile.**

Table 1: Trading of individuals and peers

We present descriptive statistics on trading of individuals and their peers. In Panel A we consider stock purchases. $buy_{i,t}$ is a dummy variable that takes the value 1 if individual i makes a purchase in month t, otherwise it is 0. $Buy_{i,t}^{plant}$, $Buy_{i,t}^{family}$ and $Buy_{i,t}^{zip}$ are the fraction of plant, family and zip code peers that make a stock purchase in month t. In Panel B we consider stock sales. $sell_{i,t}$ is a dummy variable that takes the value 1 if individual i makes a stock sale in month t, otherwise it is 0. $Sell_{i,t}^{plant}$, $Sell_{i,t}^{family}$ and $Sell_{i,t}^{zip}$ are the fraction of plant, family and zip code peers that make a stock sale in month t. Variables are described in Table A1 of the Appendix.

Panel A: Investor Purchases

Variable	Mean	Median	Std. Dev.	Min	Max	N
Individual trading						
buy	0.0519	0	0.2219	0	1	6,025,608
Peer trading						
Buy^{plant}	0.0469	0	0.1160	0	1	6,025,608
Buy^{zip}	0.0453	0.0388	0.0369	0	1	6,025,608
Buy^{family}	0.0267	0	0.1361	0	1	6,025,608

Panel B: Investor Sales

Variable	Mean	Median	Std. Dev.	Min	Max	N
Individual trading						
sell	0.0444	0	0.2059	0	1	6,025,608
Peer trading						
$Sell^{plant}$	0.0396	0	0.1025	0	1	6,025,608
$Sell^{zip}$	0.0226	0	0.1249	0	1	6,025,608
$Sell^{family}$	0.0382	0.0339	0.0292	0	1	6,025,608

Table 2: Peer and Investor Trading

In Panel A (B) the dependent variable is the dummy variable *buy* (*sell*) that takes the value 1 if the investor makes a purchase (sale) in that month and 0 otherwise. In Panel A the main explanatory variables are the fraction of plant, family and zip code peers that make a stock purchase in month t ($Buy_{i,t}^{plant}$, $Buy_{i,t}^{family}$ and $Buy_{i,t}^{zip}$ respectively). In Panel B we consider the analogous explanatory variables for sales ($Sell_{i,t}^{plant}$, $Sell_{i,t}^{family}$ and $Sell_{i,t}^{zip}$). 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), 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. t-statistics (in parentheses) are based on robust two-way (plant and time) clustered standard errors. Specifications in Panel C are identical to those in Panel A except that we use the conditional logit as estimation method. We condition on our zip×plant categories and introduce month dummies. We report odds ratios and z-statistics (in parentheses). Our z-statistics are based on robust standard errors clustered around plant. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Variables are described in Table A1 of the Appendix.

Panel A: Investor purchases

	(1)	(2)	(3)	(4)	(5)	(6)
Buy^{plant}	0.189***		0.183***	0.180***	0.162***	0.179***
	(8.66)		(9.11)	(8.09)	(14.37)	(11.67)
Buy^{zip}		0.339***	0.272***	0.431***	0.209***	-0.052***
		(4.59)	(4.76)	(8.56)	(12.68)	(-3.89)
Buy^{family}	0.090***	0.099***	0.088***	0.087***	0.098***	0.095***
	(14.12)	(15.11)	(13.87)	(12.21)	(18.44)	(18.19)
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
Plant×Year FE	No	No	No	Yes	No	No
Zip×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	6,025,608	6,025,608	6,025,608	6,025,608	6,025,608	6,025,608
R^2	0.253	0.248	0.254	0.361	0.052	0.055

Panel B: Investor sales

	(1)	(2)	(3)	(4)	(5)	(6)
$Sell^{plant}$	0.063***		0.062***	0.044***	0.091***	0.092***
	(9.38)		(9.30)	(5.64)	(17.21)	(15.11)
$Sell^{zip}$		0.173***	0.160***	0.329***	0.196***	-0.072***
		(6.26)	(6.14)	(12.02)	(11.48)	(-6.41)
$Sell^{family}$	0.061***	0.063***	0.060***	0.055***	0.078***	0.075***
	(16.38)	(17.30)	(16.76)	(15.08)	(23.26)	(22.87)
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
Plant×Year FE	No	No	No	Yes	No	No
Zip×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	6,025,608	6,025,608	6,025,608	6,025,608	6,025,608	6,025,608
R^2	0.246	0.246	0.247	0.353	0.035	0.041

Panel C: Conditional logit estimations

	(1)	(2)	(3)
Buy^{plant}	11.775***		10.24***
	(15.98)		(16.54)
Buy^{zip}		206.618***	45.242***
		(9.35)	(10.42)
Buy^{family}	3.441***	3.943***	3.293***
	(39.86)	(52.86)	(39.39)
Sociodemographic	Yes	Yes	Yes
Time dummies	Yes	Yes	Yes
N	3,697,776	3,697,776	3,697,776
Pseudo R^2	0.080	0.067	0.083

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 $Buy_{i,t}^{plant}$ (of the old and new plant) to generate the independent variables $Buy_{i,t}^{old\ before}$, $Buy_{i,t}^{old\ after}$, $Buy_{i,t}^{new\ before}$ and $Buy_{i,t}^{new\ after}$. We estimate the following regression:

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

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 the old plant (*time prior leaving*), and dummies for the number of months prior to joining the new plant (*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). *t*-statistics (in parentheses) are based on robust two-way (plant and time) clustered standard errors. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Variables are described in Table A1 of the Appendix.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$Buy^{old\ before}$	0.193***		0.192***				0.189***		0.193***	0.196***
	(5.56)		(5.60)				(5.77)		(5.89)	(5.53)
$Buy^{old\ after}$				0.149***		0.128***	0.150***		0.125***	0.161***
				(3.98)		(4.29)	(4.11)		(4.33)	(4.26)
$Buy^{new\ before}$		0.0365*	0.0257					0.0454***	0.0315**	0.0429**
		(1.87)	(1.52)					(2.45)	(1.99)	(2.52)
$Buy^{new\ after}$					0.189***	0.180***		0.186***	0.180***	0.168***
					(5.20)	(5.50)		(5.23)	(5.59)	(4.44)
Buy^{zip}	0.0745	0.0978**	0.0730	0.109**	0.0977**	0.0848*	0.0911**	0.0941***	0.0765*	0.0958**
	(1.46)	(1.84)	(1.44)	(2.21)	(2.14)	(1.89)	(2.18)	(2.78)	(1.95)	(1.98)
Buy^{family}	0.0729***	0.0742***	0.0727***	0.0664***	0.0662***	0.0646***	0.0685***	0.0690***	0.0675***	0.0652***
	(6.05)	(6.14)	(6.03)	(5.40)	(5.33)	(5.27)	(7.35)	(7.30)	(7.21)	(6.42)
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	76,788	76,788	76,788	90,595	90,595	90,595	167,383	167,383	167,383	118,044
Adj. R^2	0.346	0.342	0.346	0.305	0.309	0.310	0.316	0.316	0.319	0.336

Table 4: Interaction in peer groups of different size

We examine the impact of co-workers in peer groups of different size. We sample two individuals from each plant and month (the individual and one co-worker). In each month we rank all remaining plants according to plant size (the number of workers employed at the plant) into ten deciles. We estimate the following regression for each size decile:

$$buy_{i.t} = \alpha_t + \beta_1 buy_{i.t} + \beta_2 Buy_{i.t}^{family} + \beta_3 Buy_{i.t}^{zip} + \boldsymbol{b\Gamma} + \varepsilon_{i.t} \quad (5)$$

where $buy_{j,t}$ takes the value of 1 if the co-worker makes a purchase in month t and otherwise takes the value 0. The vector $\boldsymbol{\Gamma}$ includes the sociodemographic variables listed in the caption to Table 2 as well as time and plant fixed effects. In Panel A we present point estimates of β_1 with corresponding t-statistics (standard errors are clustered around plant and time), the number of individual month observations used in the regression and the mean number of employees in the size decile. In Panel B we present our 'placebo analysis'. We now randomly pair each individual in the remaining sample to a co-worker of a different plant in the same size decile. We re-estimate (5) while replacing $buy_{j,t}$ with the corresponding buying intensity of the placebo co-worker. In Panel B we present the estimates of β_1 with t-statistics. The number of observations and the mean number of employees are identical to those in Panel A by construction. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Variables are described in Table A1 of the Appendix.

Panel A:

	Size Decile									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$buy_{i.t}$	0.0750***	0.0492***	0.0411***	0.0161*	-0.00312	0.00611	0.000974	-0.00297	0.00537	0.0180***
	(9.26)	(6.82)	(4.47)	(2.43)	(-0.52)	(0.95)	(0.18)	(-0.51)	(0.98)	(2.99)
N	97,452	97,381	97,417	97,379	97,403	97,404	97,404	97,392	97,404	97,344
Mean # Employees	4.74	9.63	15.11	21.91	30.74	41.93	57.72	82.73	131.18	464.52

Panel B:

	Size Decile									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$buy_{i.t}$	-0.0044	0.0002	-0.0063*	0.0009	-0.0037	0.0010	0.00215	-0.00144	-0.0018	-0.0001
	(-1.32)	(0.07)	(-2.05)	(0.26)	(-1.24)	(0.32)	(0.62)	(-0.42)	(-0.48)	(-0.03)

N and Mean # Employees are identical to those in Panel A by construction.

Table 5: Descriptive statistics on investor and peer stock selection

We present descriptive statistics on the stock selection decision of individuals and peers. $f_{i,t,s}$ is the fraction invested by investor i in stock s in month t. $F_{i,t,s}^{plant}$ and $F_{i,t,s}^{zip}$ is the average fraction invested in stock s in month t by plant and zip code peers, respectively. Variables are described in Table A1 of the Appendix.

Individual and peer stock selection

Variable	Mean	Median	Std. Dev.	Min	Max	N
Individual stock selection						
f	0.0049	0	0.0620	0	1	87,812,052
Peer stock selection						
F^{plant}	0.0049	0	0.0486	0	1	87,812,052
F^{zip}	0.0049	0	0.0331	0	1	87,812,052

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 fraction invested in that stock by the investor's peers. The dependent variable $f_{i,t,s}$ is the fraction of purchases invested in stock s in month t by the investor. $F_{i,t,s}^{plant}$ and $F_{i,t,s}^{zip}$ is the fraction of purchases invested in stock s in month t by plant 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. *t*-statistics (in parentheses) are based on robust two-way (plant and time) clustered standard errors. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Variables are described in Table A1 of the Appendix.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	f	f	f	f	f	f	f	f
F^{plant}	0.201***		0.196***	0.266***	0.309***	0.323***	0.288***	0.243***
	(11.15)		(11.37)	(10.44)	(16.90)	(14.72)	(17.42)	(15.25)
F^{zip}		0.111***	0.0835***	0.211***	0.116***	0.0984***	0.107***	0.0682***
		(6.53)	(6.65)	(5.19)	(11.74)	(11.28)	(12.17)	(6.48)
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	87,812,052	87,812,052	87,812,052	87,812,052	87,812,052	87,812,052	87,812,052	87,812,052
R^2	0.457	0.446	0.458	0.497	0.214	0.211	0.222	0.346

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 $F_{i,t,s}^{plant}$ (of the old and new plant) to generate the independent variables $F_{i,t,s}^{old\ before}$, $F_{i,t,s}^{old\ after}$, $F_{i,t,s}^{new\ before}$ and $F_{i,t,s}^{new\ after}$. We estimate:

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

where $f_{i.t.s}$ is the fraction of month t purchases invested in stock s by investor i. $F_{i,t,s}^{zip}$ is the average fraction invested in stock s in month t by zip code peers. We include month×stock and NACE2×stock fixed effects. In specification (10) we only consider those individuals who do not change the municipality of employment or residence surrounding the shift in plant. *t*-statistics (in parentheses) are based on robust two-way (plant and time) clustered standard errors. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Variables are described in Table A1 of the Appendix.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	f	f	f	f	f	f	f	f	f	f
$F^{old\ before}$	0.176***		0.170***					0.189***	0.189***	0.186***
	(7.02)		(7.24)					(8.11)	(8.37)	(7.33)
$F^{old\ after}$					0.209***	0.167***		0.208***	0.163***	0.162***
					(7.57)	(6.88)		(7.07)	(7.18)	(6.36)
$F^{new\ before}$		0.108***	0.0917***				0.137***		0.115***	0.124***
		(4.19)	(4.32)				(5.98)		(6.15)	(4.77)
$F^{new\ after}$				0.200***		0.193***	0.208***		0.193***	0.207***
				(10.96)		(8.05)	(8.58)		(9.25)	(7.71)
F^{zip}	0.0564***	0.0605***	0.0544***	0.0619***	0.0646***	0.0619***	0.0587***	0.0629***	0.0534***	0.0487***
	(3.53)	(3.80)	(3.44)	(4.64)	(5.06)	(4.91)	(5.75)	(5.68)	(5.34)	(4.02)
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	448,622	448,622	448,622	529,507	529,507	529,507	900,111	900,111	900,111	640,872
R^2	0.272	0.266	0.274	0.315	0.309	0.321	0.254	0.253	0.262	0.294

Table 8: Interaction in peer groups of different size

We examine the impact of co-workers in peer groups of different size. We sample two individuals from each plant and month (the individual and one co-worker). In each month we rank all plants according to plant size (the number of workers employed at the plant) into ten deciles. We estimate the following regression for each size decile:

$$f_{i.t.s} = \beta_1 f_{j.t.s} + \beta_2 F^{zip}_{i.t.s} + \boldsymbol{b\Gamma} + \varepsilon_{i,t,s} \qquad (9)$$

where $f_{j,t,s}$ is the fraction of co-worker j's purchases allocated to stock s in month t. We control for the stock selection of neighbors by including $F^{zip}_{i,t,s}$. The vector $\boldsymbol{\Gamma}$ includes plant×stock and month×stock fixed effects. In Panel A we present point estimates of β_1 with corresponding t-statistics (standard errors are clustered at the plant and time level), the number of individual month observations used in the regression and the mean number of employees in the size decile. In Panel B we present our 'placebo analysis'. We now randomly pair each individual in the remaining sample to a co-worker of a different plant in the same size decile. We re-estimate (9) while replacing $f_{j,t,s}$ with the corresponding stock allocation of the placebo co-worker. In Panel B we present the estimates of β_1 with t-statistics. The number of observations and the mean number of employees are identical to those in Panel A by construction. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Variables are described in Table A1 of the Appendix.

Panel A:

	Size Decile									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$f_{j.t.s}$	0.218***	0.141***	0.087***	0.067***	0.063***	0.050***	0.047***	0.036***	0.037***	0.041***
	(15.54)	(12.43)	(9.65)	(7.77)	(6.55)	(5.82)	(5.60)	(4.08)	(4.86)	(4.96)
N	2,155,146	2,155,076	2,155,097	2,155,066	2,155,082	2,155,096	2,155,085	2,155,074	2,155,109	2,155,132
Mean # Employees	7.22	20.73	39.02	61.87	91.56	130.49	184.68	270.23	433.77	1,464.67

Panel B:

	Size Decile									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$f_{j.t.s}$	0.012**	0.018**	0.018**	0.013*	0.015*	0.013	0.010	0.009	0.007	-0.004
	(1.97)	(2.40)	(2.33)	(1.86)	(1.94)	(1.59)	(1.27)	(1.10)	(0.90)	(-0.57)

N and Mean # Employees are identical to those in Panel A by construction.

Table 9: Stock Selection of Local and Same-Industry Stocks

We investigate the relation between the stock selection of peers and the stock selection of investors in local and same-industry stocks. The dependent variable $f_{i,t,s}$ is the fraction of total purchases invested in stock s in month t by investor i. $F_{i,t,s}^{plant}$ and $F_{i,t,s}^{zip}$ is the average fraction invested in stock s in month t by plant 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 stock s. In specification (2), we only consider non-local stocks. Specification (3) considers only same-industry stocks (defined as in Døskeland and Hvide, 2011), while specification (4) considers different-industry stocks. We include month×stock, plant×stock, zip×stock and zip×plant×stock fixed effects. *t*-statistics (in parentheses) are based on robust two-way (plant and time) clustered standard errors. *, ** and *** indicate statistical significance at the 10%, 5% and 1% level, respectively. Variables are described in Table A1 of the Appendix.

	(1) Local	(2) Non-Local	(3) Same-Industry	(4) Diff.-Industry
F^{plant}	0.218***	0.151***	0.381***	0.136***
	(9.50)	(9.75)	(12.07)	(9.85)
F^{zip}	0.0666***	0.0810***	0.106***	0.0703***
	(6.19)	(4.96)	(5.92)	(5.13)
Time×Stock FE	Yes	Yes	Yes	Yes
Plant×Stock FE	Yes	Yes	Yes	Yes
Zip×Stock FE	Yes	Yes	Yes	Yes
N	24,714,288	53,266,715	2,442,980	75,710,195
Adj. R^2	0.513	0.444	0.741	0.434

Table 10: Peer pressure and returns

We present regression results relating peer pressure to returns. In each month we rank all purchases in terms of their peer pressure, $F_{i,t,s}^{plant} - F_{i,t,s}^{non-plant}$, formally, the allocation of co-workers to stock s in excess of the economy average ($F_{i,t,s}^{non-plant}$) allocation to stock s. Purchases with above (below) median peer pressure are placed in the High Pressure (Low Pressure) portfolio. Purchases are kept in the portfolio from the last day of the purchase month until the end of the holding period (up to one year later). As dependent variable we use the time-series of monthly differences between the mean return of the High Pressure portfolio and the mean return of the Low Pressure portfolio (HP-LP). We estimate the following regression,

$$HP - LP_t = \alpha + \beta_1 MKT_t + \beta_2 SMB_t + \beta_3 HML_t + \beta_4 MOM_t + \varepsilon_t, \quad (11)$$

where MKT_t, SMB_t, HML_t, and MOM_t, are the Fama-French and the Carhart (1997) factors calculated for Norway by Ødegaard (2013). In Panel A, we present regression results for holding periods 1, 3, 6, 9 and 12 months. Panel B presents our return results for the individual months t+2 to t+6. We report monthly percentage alphas. In Panel C, we consider whether peer pressure affects the performance of same-industry purchases. To do this, in each month we rank all same-industry purchases in terms of peer pressure and as before above (below) median purchases are placed in the High (Low) Pressure portfolio. Thus, we re-estimate (11) with our dependent variable $HP - LP_t$ based on only same-industry purchases. Panel C reports point estimates of α over several holding periods. Panel D, E and F are identical to Panel C except that we only consider different-industry, local and non-local purchases, respectively. We report Newey and West (1987) standard errors with 3 lags. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Holding periods and returns

Holding period	1 month		3 months		6 months		9 months		12 months	
	alpha	std error	alpha	std error	alpha	std error	alpha	std error	alpha	std error
High pressure	0.319	(1.130)	0.142	(1.107)	0.125	(1.080)	0.088	(1.066)	0.014	(1.051)
Low pressure	0.333	(1.133)	0.148	(1.111)	0.122	(1.083)	0.076	(1.068)	0.010	(1.054)
High-Low	-0.014	(0.023)	-0.006	(0.016)	0.003	(0.011)	0.011	(0.010)	0.005	(0.011)

Panel B: Monthly returns to peer trading

Month	t+2		t+3		t+4		t+5		t+6	
	alpha	std error	alpha	std error	alpha	std error	alpha	std error	alpha	std error
High pressure	0.026	(1.150)	0.025	(1.110)	-0.151	(1.104)	0.486	(1.113)	0.006	(1.154)
Low pressure	0.018	(1.150)	0.068	(1.119)	-0.186	(1.104)	0.476	(1.118)	0.035	(1.154)
High-Low	0.008	(0.030)	-0.043	(0.0316)	0.034	(0.0239)	0.010	(0.018)	-0.029	(0.024)

Panel C: Peer pressure and same-industry investments

Holding period	1 month		3 months		6 months		9 months		12 months	
	alpha	std error	alpha	std error	alpha	std error	alpha	std error	alpha	std error
High pressure	0.790	(1.293)	-0.042	(1.199)	-0.088	(1.144)	0.026	(1.124)	-0.114	(1.087)
Low pressure	0.374	(1.249)	-0.105	(1.179)	-0.056	(1.169)	0.150	(1.165)	0.074	(1.128)
High-Low	0.416	(0.259)	0.063	(0.233)	-0.032	(0.161)	-0.124	(0.174)	-0.188	(0.164)

Panel D: Peer pressure and different-industry investments

Holding period	1 month		3 months		6 months		9 months		12 months	
	alpha	std error	alpha	std error	alpha	std error	alpha	std error	alpha	std error
High pressure	0.328	(1.129)	0.164	(1.111)	0.137	(1.079)	0.084	(1.063)	0.007	(1.049)
Low pressure	0.338	(1.131)	0.164	(1.113)	0.134	(1.083)	0.075	(1.067)	0.001	(1.054)
High-Low	-0.010	(0.023)	0.001	(0.017)	0.003	(0.011)	0.009	(0.010)	0.006	(0.012)

Panel E: Peer pressure and local investments

Holding period	1 month		3 months		6 months		9 months		12 months	
	alpha	std error	alpha	std error	alpha	std error	alpha	std error	alpha	std error
High pressure	0.089	(1.151)	-0.069	(1.133)	0.027	(1.118)	0.058	(1.105)	-0.001	(1.087)
Low pressure	0.170	(1.154)	0.014	(1.135)	0.049	(1.116)	0.075	(1.106)	0.017	(1.088)
High-Low	-0.081	(0.064)	-0.083***	(0.031)	-0.022	(0.025)	-0.017	(0.028)	-0.018	(0.028)

Panel F: Peer pressure and non-local investments

Holding period	1 month		3 months		6 months		9 months		12 months	
	alpha	std error	alpha	std error	alpha	std error	alpha	std error	alpha	std error
High pressure	0.430	(1.145)	0.250	(1.115)	0.179	(1.079)	0.107	(1.062)	0.019	(1.047)
Low pressure	0.453	(1.142)	0.258	(1.118)	0.164	(1.081)	0.083	(1.064)	0.008	(1.051)
High-Low	-0.023	(0.038)	-0.008	(0.029)	0.015	(0.019)	0.025*	(0.015)	0.011	(0.016)

Appendix

Table A1: Definitions of Regression Variables

Variable	Description of Variable
Trade Variables (monthly)	
$buy_{i,t}$	Takes the value 1 if investor i makes a stock purchase in month t otherwise 0.
$sell_{i,t}$	Takes the value 1 if investor i makes a stock sale in month t otherwise 0.
$Buy_{i,t}^{plant}$	The fraction of co-workers that make a purchase in month t.
$Buy_{i,t}^{family}$	The fraction of family members that make a purchase in month t.
$Buy_{i,t}^{zip}$	The fraction of neighbors living in the same zip code that make a purchase in month t.
$Sell_{i,t}^{plant}$	The fraction of co-workers that make a sale in month t.
$Sell_{i,t}^{family}$	The fraction of family members that make a sale in month t.
$Sell_{i,t}^{zip}$	The fraction of neighbors that make a sale in month t.
$Buy_{i,t}^{old\ before}$	In months before the individual leaves the old plant, this is the fraction of co-workers at the old plant making a stock purchase in month t. After the move this variable takes the value 0.
$Buy_{i,t}^{old\ after}$	In months after the individual leaves the old plant, this is the fraction of co-workers at the old plant making a stock purchase in month t. Before the move this variable takes the value 0.
$Buy_{i,t}^{new\ before}$	In months before the individual joins the new plant, this is the fraction of co-workers at the new plant making a stock purchase in month t. After joining the new plant this variable takes the value 0.
$Buy_{i,t}^{new\ after}$	In months after the individual joins the new plant, this is the fraction of co-workers at the new plant making a stock purchase in month t. Before joining the new plant this variable takes the value 0.
$Buy_{i,t}^{non-plant}$	The fraction of non-coworkers that make a purchase in month t.
Stock Selection Variables (monthly)	
$f_{i,t,s}$	The fraction of total investor purchases by investor i invested in stock s in month t.
$F_{i,t,s}^{plant}$	The fraction of total co-worker purchases invested in stock s in month t.
$F_{i,t,s}^{family}$	The fraction of total family purchases invested in stock s in month t.
$F_{i,t,s}^{zip}$	The fraction of total neighbor purchases invested in stock s in month t.
$F_{i,t,s}^{old\ before}$	In months before the individual leaves the old plant, this is the fraction of total purchases of co-workers at the old plant invested in stock s in month t. After the move this variable takes the value 0.
$F_{i,t,s}^{old\ after}$	In months after the individual leaves the old plant, this is the fraction of total purchases of co-workers at the old plant invested in stock s in month t. Before the move this variable takes the value 0.
$F_{i,t,s}^{new\ before}$	In months before the individual joins the new plant, this is the fraction of total purchases of co-workers at the new plant invested in stock s in month t. After the move this variable takes the value 0.
$F_{i,t,s}^{new\ after}$	In months after the individual joins the new plant, this is the fraction of total purchases of co-workers at the new plant invested in stock s in month t. Before the move this variable takes the value 0.
$F_{i,t,s}^{non-plant}$	The fraction of total non-coworker purchases invested in stock s in month t.
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.
Same-industry 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 income reported by the individual in the previous year's tax return. Reported in Norwegian Kroner.
Wealth	The total wealth reported in the individual's tax return for the previous 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, Zip size and Family size present descriptive statistics on the size of the individual's plant, zip code and family respectively (excluding the individual). The rows Plant investors, Zip investors, and Family 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 analysis. 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	391.74	78	884.76	1	7,845	97,264
Zip size	3,715.22	2,390	4,360.69	5	44,195	97,264
Family size	6.60	5	5.46	1	122	97,264
Plant investors	122.78	13	333.74	1	2,428	97,264
Zip investors	224.00	145	277.43	1	3,213	97,264
Family investors	1.92	1	1.33	1	18	97,264
Wealth (NOK)	802,271.58	322,028	10,451,753.75	0	2,127,096,064	97,264
Income (NOK)	381,028.56	336,231	227,873.71	0	9,773,526	97,264
Age	37.30	36	8.95	21	69	97,264
Male	0.76	1	0.43	0	1	97,264
Education	13.08	13	3.39	0	21	97,264
Number of trades	4.83	1	9.11	1	129	97,264

Panel B:

Variable	Mean	Median	Std. Dev.	Min	Max	N
Plant size	325.05	59	826.28	1	7,845	14,284
Zip size	3,912.70	2,393	4,758.11	13	44,195	14,284
Family size	6.23	5	5.17	1	79	14,284
Plant investors	74.42	11	221.07	1	2,428	14,284
Zip investors	239.77	150	308.49	1	3,213	14,284
Family investors	1.88	1	1.28	1	17	14,284
Wealth (NOK)	653,248.91	323,650	4,501,711.67	0	414,171,968	14,284
Income (NOK)	397,649.71	342,800	246,819.21	900	9,773,526	14,284
Age	36.73	36	7.87	21	65	14,284
Male	0.80	1	0.40	0	1	14,284
Education	13.36	13	3.48	0	21	14,284
Number of trades	5.83	2	10.21	1	129	14,284

Panel C:

Variable	Mean	Median	Std. Dev.	Min	Max	N
Plant size	501.86	156	956.53	1	7,845	118,432
Zip size	3,613.26	2,424	3944.25	13	44,195	118,432
Plant investors	158.17	37	315.16	1	2,731	118,432
Zip investors	389.59	253	463.23	1	5,400	118,432
Wealth (NOK)	1,035,361.47	468,494	8,698,890.60	0	2,127,096,064	118,432
Income (NOK)	450,049.26	391,600	279,455.05	0	13,387,700	118,432
Age	42.52	42	11.22	20	70	118,432
Male	0.80	1	0.40	0	1	118,432
Education	12.87	12	3.64	0	21	118,432
Number of trades	6.36	2	10.68	1	129	118,432

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

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

where I_j is a dummy variable that takes a value of 1 if the investor works in industry j and 0 otherwise. Column 4 reports our point estimates of the peer effect for our 36 industries. The vector $\boldsymbol{\Gamma}$ of control variables includes the socio-demographic control variables listed in the caption to Table 2. In addition to time (month), plant and zip fixed effects; we include zip-plant interaction fixed effects. *t*-statistics (in column 5) are based on robust two-way (plant and time) clustered standard errors. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Industry (NACE code)	Investors	OSE Firms	Coefficient	t-stat
Fishing, fish farming (5)	484	2	0.107**	(4.00)
Oil and gas extraction. Oil and gas services (11)	5,199	19	0.503***	(5.24)
Food products and beverages (15)	2,277	4	0.275***	(4.37)
Wood and wood products (20)	592	2	0.106***	(2.73)
Publishing, printing, reproduction (22)	1,837	5	0.215***	(3.91)
Chemicals and chemical products (24)	1,889	2	0.910**	(19.12)
Other non-metallic mineral products (26)	516	2	0.111***	(2.14)
Basic metals (27)	1,885	2	0.801***	(8.34)
Fabricated metal products (28)	859	1	0.098***	(3.39)
Machinery and equipment (29)	1,822	7	0.161**	(4.41)
Electrical machinery and apparatus (31)	737	4	0.280***	(2.44)
Radio, TV, communication equip (32)	838	7	0.538***	(5.24)
Instruments, watches and clocks (33)	753	4	0.200***	(2.58)
Motor vehicles, trailers, semi-tr.(34)	618	2	0.821***	(5.91)
Other transport equipment (35)	2,990	2	0.240**	(2.85)
Furniture, manufacturing (36)	865	4	0.451***	(2.50)
Electricity, gas and water supply (40)	1,395	3	0.148***	(3.56)
Construction (45)	5,807	2	0.057***	(4.58)
Motor vehicle services (50)	1,671	0	0.063***	(3.79)
Wholesale trade, commission trade (51)	8,041	8	0.152***	(3.40)
Retail trade, repair personal goods (52)	3,362	6	0.079***	(6.96)
Hotels and restaurants (55)	1,359	2	0.045	(1.45)
Land transport, pipeline transport (60)	1,460	2	0.070**	(2.12)
Water transport (61)	1,952	42	0.422***	(4.66)
Air transport (62)	760	2	0.100**	(2.03)
Services for transport and travel agencies (63)	1,558	0	0.086***	(4.07)
Post and telecommunications (64)	2,976	5	0.631***	(4.94)
Real estate activities (70)	1,477	8	0.208***	(4.96)
Computers and related activities (72)	4,703	20	0.272***	(5.14)
Research and development (73)	1,275	3	0.445***	(3.40)
Other business activities (74)	11,628	8	0.116***	(6.92)
Public administration, defense and social security (75)	8,357	0	0.051***	(2.80)
Education (80)	5,730	0	0.324**	(1.96)
Health and social services (85)	6,258	0	0.008	(0.53)
Interest groups (91)	723	0	0.049***	(3.31)
Cultural and sporting activities (92)	1,141	2	0.077***	(3.68)
Total	95,794	182		

Appendix

Table A4: Subsample analysis

This table examines whether peer effects are stronger among co-workers that are more likely to interact. We classify co-workers along the dimensions of sex, tenure, age, education and wealth. We sort co-workers in each year and plant into two groups depending on sex (Panel A), median tenure (Panel B), median age (Panel C), median education (Panel D), and median wealth (Panel E). For each group of co-workers we calculate the fraction of individuals that make a purchase in that month ($Buy_{i,t}^{Group\ 1\ plant}$ and $Buy_{i,t}^{Group\ 2\ plant}$). For both groups (for example, males are Group 1 and females are Group 2) we estimate the following regression:

$$buy_{i,t}^{Group\ j} = \alpha_t + \beta_1 Buy_{i,t}^{Group\ 1\ plant} + \beta_2 Buy_{i,t}^{Group\ 2\ plant} + \beta_3 Buy_{i,t}^{family} + \beta_4 Buy_{i,t}^{zip} + \boldsymbol{b\Gamma} + \varepsilon_{i,t.}$$

The column Group 1 reports point estimates of β_1 and β_2 when the individual belongs to Group 1. We also report the p-value of the F-test of a difference between β_1 and β_2. Column 2 presents the corresponding results when the individual belongs to Group 2. 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. *t*-statistics (in parentheses) are based on robust two-way (plant and time) clustered standard errors. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Group 1	Group 2
A Group 1: male and group 2: female		
$Buy_{i,t}^{male\ plant}$	0.260***	0.244***
	(11.05)	(13.32)
$Buy_{i,t}^{female\ plant}$	0.276***	0.400***
	(11.33)	(15.31)
P-value of test coeff. diff.	0.2896	0.0000
N	2,499,192	923,820
B Group 1: below median tenure and group 2: above median tenure		
$Buy_{i,t}^{low\ tenure\ plant}$	0.287***	0.198***
	(9.66)	(8.25)
$Buy_{i,t}^{high\ tenure\ plant}$	0.171***	0.226***
	(10.06)	(8.10)
P-value of test coeff. diff.	0.0000	0.0012
N	1,803,516	1,559,964
C: Group 1: below median age and group 2: above median age		
$Buy_{i,t}^{low\ age}$	0.230***	0.217***
	(10.09)	(10.22)
$Buy_{i,t}^{high\ age}$	0.196***	0.237***
	(10.73)	(9.23)
P-value of test coeff. diff.	0.0007	0.0345
N	2,117,232	1,893,984
D: Group 1 below median education and group 2: above median education		
$Buy_{i,t}^{low\ education}$	0.290***	0.217***
	(10.71)	(9.88)
$Buy_{i,t}^{high\ education}$	0.184***	0.228***
	(11.00)	(9.30)
P-value of test coeff. diff.	0.0000	0.22223
N	1,892,928	1,852,272
E: Group 1: below median wealth and group 2: above median wealth		
$Buy_{i,t}^{low\ wealth}$	0.248***	0.200***
	(10.23)	(9.29)
$Buy_{i,t}^{high\ wealth}$	0.188***	0.264***
	(10.54)	(9.67)
P-value of test coeff. diff.	0.0000	0.0000
N	1,668,420	2,180,760

Appendix

Table A5: Descriptive statistics on investor and peer stock selection of local and same-industry stocks

We examine individual stock selection of same-industry, different-industry, local and non-local stocks (examined in Table 9). A local stock is headquartered less than 100km from the residence of the individual. Same-industry stocks have the same two digit NACE code as the employer of the individual.

Stock selection of local and same-industry stocks

Variable	Mean	Median	Std. Dev.	Min	Max	N
Individual stock selection						
f^{local}	0.0070	0	0.075	0	1	24,714,288
$f^{non\text{-}local}$	0.0044	0	0.059	0	1	53,266,715
$f^{same\text{-}industry}$	0.0203	0	0.134	0	1	2,442,980
$f^{diff.\text{-}industry}$	0.0047	0	0.061	0	1	75,710,195