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Social interaction at work[☆]

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ABSTRACT

Stock market investment decisions of individuals are positively correlated with those of coworkers. Sorting of unobservably similar individuals to the same workplaces is unlikely to explain this pattern, as evidenced by the investment behavior of individuals who move between plants. Purchases made under stronger coworker purchase activity are not associated with higher returns. Moreover, social interaction appears to drive the purchase of within-industry stocks. Overall, we find a strong influence of coworkers on investment choices, but not an influence that improves the quality of investment decisions.

1. Introduction

Although the literature has long acknowledged the existence of social interaction effects among individual investors (e.g., Shiller, 1984; Shiller and Pound, 1989), most work explaining individual investment decisions focuses on other factors such as risk and time preferences, wealth, or overconfidence (Campbell, 2006). One exception is Hong, Kubik, and Stein (2004), who hypothesize that social interaction leads to greater stock market participation and find that those who interact with neighbors or attend church are more likely to invest in stocks. Using extremely

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detailed data from Norway, we show that social interactions with colleagues at work appear to strongly affect individual investors' trading intensity and their stock selection. We also analyze whether coworker influence appears to improve the quality of investment decisions.

The social psychology literature emphasizes the strength of face-to-face communication between individuals who frequently interact in producing and altering beliefs.[2] Conversations at the workplace occasionally center on the stock market and, we conjecture, can influence investment behavior. For example, investors pick among a dizzying number of individual stocks when picking stocks, and they can 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 Jappelli, 2005; Guiso, Sapienza, and Zingales, 2008).[3]

To examine whether individual investors are affected by their coworkers, we combine two data sources from Norway. The matched employer–employee data, which cover the whole population of workers over a ten year period from 1995 to 2005, identify coworkers at the plant level. We combine these data with a complete record of common stock transactions made by individual investors at the Oslo Stock Exchange (OSE) over the same period. We focus on individuals who make at least one purchase of common stocks over the sample period.[4] We omit individual-years in which 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 1 standard deviation increase in the fraction of coworkers who make a stock purchase in a given month is associated with a 41% increase in the probability of making a purchase. Moreover, conditional on making a purchase, a 1 standard deviation increase in coworkers' purchase of a particular stock is associated with a striking 194% increase in the fraction of that month's purchases invested in that stock by the individual.

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.[5] We control for fixed effects to address correlated unobservables. For example, plant fixed effects control for unobservables such as company culture, composition of the workforce, and industry affiliation.[6] 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 such endogenous group membership, we analyze the investment behavior of individuals who move between plants. The idea is that future coworkers are unlikely to influence via social interaction but can still exhibit correlated behavior due to similarity along unobservables. Thus, if unobserved similarities drive the results, we would expect the correlation with future coworkers to be of comparable magnitude to the correlation with current coworkers.

In Fig. 1, time is on the horizontal axis and the correlation in purchasing behavior is on the vertical axis. Month 0 is the starting month in the new job and end month in the old job. The blue dashed line illustrates how the correlation in purchasing behavior with individuals who become coworkers after the move 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 who are coworkers prior to the move evolves over time. Prior to Month 0, the correlation is significantly higher than the correlation with future coworkers and then fades out after the move.

Our results could be driven by events at the plant-month level, such as visits from equity brokers. If so, we would expect a similar correlation in trading behavior between pairs of individuals at small and large plants. If social interaction drives our results, in contrast, we would expect stronger correlation between individuals at small plants than at large plants, simply because two individuals are more likely to engage at a small plant. 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. In support of the

[2] In a classic study by Asch (1955), individuals alone and in groups compare the lengths of line segments. The lengths are sufficiently different that when responding alone very few wrong answers are given. Yet when placed in a group in which all other members are instructed to give the same wrong answers, individuals frequently give wrong answers.

[3] For suggestive evidence, Shiller (1984) cites surveys from the 1950s and 1960s in which the answers to the questions “Do you own any stocks?” and “Do you have any friends or colleagues who own any stocks?” are practically identical. In a case study with a randomized trial design, Duflo and Saez (2003) show workplace social influence in the decision to enroll in a tax-deferred account retirement plan.

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

[5] 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 Moffitt (2001).

[6] These are contextual and ecological effects in the terminology of Manski (1993), which should be contrasted with the endogenous social effects. Lee (2007) and Lee, Liu, and Lin (2010) analyze how fixed effects alleviate the problem of correlated unobservables in the identification of endogenous social effects. Blume, Brock, Durlauf, and Ioannides (2010) survey the literature.

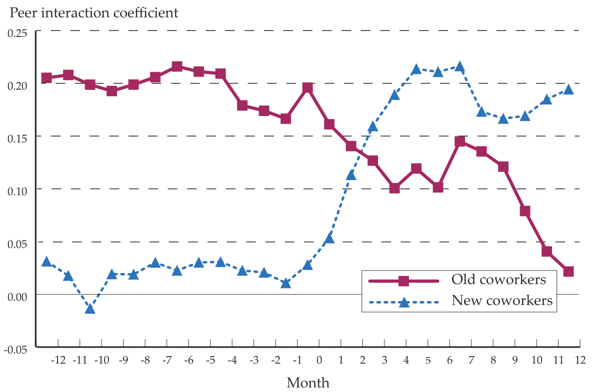

Fig. 1. This figure plots the peer interaction coefficient, which is the increase in the likelihood that an individual makes a purchase if all of his coworkers make a purchase in that month. We consider the effect of old coworkers and new coworkers from 12 months before the individual leaves the old plant to 12 months after the individual joins the new plant. 0 is the start date of the new job and end date of the old job. See Section 3.2 for estimation details.

social interaction mechanism, we find that the estimated correlation is considerably larger for small plants than for large plants.

Does social interaction improve the quality of investment decisions? The logic behind information cascade models (Bikhchandani, Hirshleifer, and Welch, 1992; Banerjee, 1992; Ellison and Fudenberg, 1993) suggests that imitating coworkers can make investment decisions more enlightened through diffusion of return-relevant information or, perhaps, through learning sound investment principles such as diversification and hedging. Alternatively, information transmitted at the workplace could be false rumors, or it could involve imitation of unsound practices such as technical analysis (March, 1991).[7] The welfare implications are substantially different.

We address whether social interaction improves investment quality in two ways. First, using the calendar time portfolio approach (e.g., Odean, 1999; Seasholes and Zhu, 2010), we analyze whether risk-adjusted investment returns are higher when coworkers purchase a particular 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 find do not seem rooted in diffusion of value-relevant asymmetric information. Second, an investment mistake that has been abundantly recorded 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 coworkers is larger for the purchase of within-industry stocks than for other stocks, and we 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 social interaction, instead of improving investor welfare, could propagate investment mistakes.

Overall, the findings suggest that individuals are strongly influenced by their coworkers, 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 coworkers 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, Kubik, and Stein, 2004; Ivković and Weisbenner, 2007; Brown, Ivković, Smith, and Weisbenner, 2008; Kaustia and Knüpfer, 2012) is based on analysis of large groups, such as regions or neighborhoods, in which identification of social effects is difficult (e.g., Moffitt, 2001).[8] 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, Choi, Laibson, Madrian, and Milkman (2011), who find negative coworker 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, Hirshleifer, and Welch, 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 insights transmitted through social interaction between coworkers seem to be noise at best. Our results stand in contrast to the economics literature, which emphasizes positive spillover effects [e.g., Mas and Moretti (2009) on productivity spillover effects for workers].

Third, we contribute to the debate on what explains investment mistakes. A large empirical literature has shown that individual investors tend to make systematic investment mistakes [see, e.g., Odean (1999) or Benartzi and Thaler (2007) and Campbell (2006) for overviews]. While the extant literature attempts to explain these

[7] 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."

[8] The same point can be made about much of the literature on social interaction in economics (e.g., Bertrand, Luttmer, and Mullainathan, 2000; Moretti, 2011). Whilst our focus is on social interaction in a naturally occurring group, a related literature considers social interaction effects under randomized group formation (e.g., Bursztyn, Ederer, Ferman, and Yuchtman, 2013; Dahl, Løken, and Mogstad, 2012).

mistakes with individual characteristics such as gender, wealth, income, genetics, or intelligence quotient (IQ) (e.g., Campbell, 2006; Cohen, 2008; Cronqvist and Siegel, 2013), we demonstrate the role of social interaction. Our findings have an interesting parallel in the medical literature. Christakis and Fowler (2007) provide evidence consistent with obesity in the US spreading through social interaction.

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

2. Data

The data set is proprietary and has been collected from three sources. First, a record of all common stock trades made between January 1994 and December 2005 by Norwegian residents on the Oslo Stock Exchange (OSE) come from Verdipapirsentralen (the Norwegian Central Securities Depository). For each transaction made by an individual, the data contain the (anonymized) identification (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, company information such as ticker, prices, market capitalization, and company ID numbers come from the OSE. When needed, additional information is collected from Borsprosjektet (the OSE project) at the Norwegian School of Economics. Third, the government statistical agency, Statistics Norway, provided register data on the socio-demographic characteristics of investors. The data come from government registries assembled for tax-collection purposes and are highly reliable.[9]

For each individual-year, the data include the ID of the plant at which the individual is employed (the plant ID stays fixed through ownership changes), the IDs of the individual's spouse and children, and the zip code in which the individual lives. If two individuals work at the same plant, it means that they share the same business address. We also identify other family members: parents, grandparents, grandchildren, siblings, uncles, aunts, cousins, nieces and nephews. The socioeconomic variables are 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, Møen, and Salvanes (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 who are employed full time for at least one year between 1994 and 2005 and who purchase common stocks on the Oslo Stock Exchange at least once during the same period (about 12% of the population). We omit individual-years in which the individual is employed part time or is 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 Norwegian Kroner (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). We are left with a sample of about 170,000 individuals. The coworker peer group of the 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 those who live in the same zip code as the individual. We refer to an individual in the sample or in one of the peer groups who 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 who have (a) at least one coworker who is an investor (i.e., purchases stocks at least once between 1994 and 2005), (b) at least one person in the same zip code who is an investor, and (c) at least one family member who is an investor. The purchasing activity of coworkers is our main explanatory variable, and we impose (b) and (c) to control for the purchasing activity of zip code and family members (these controls would not be defined otherwise). We also require that the socio-demographic variables are non-missing (this requirement affects only a small fraction of observations). This leaves 97,264 unique individuals over the entire period. In Panel A of Table A2 we provide socio-demographic 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 26 hundred zip codes and roughly 18 thousand 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 coworker, and one person in the zip code make a purchase in that month. Panel C of Table A2 provides descriptive statistics of the 118,432 unique individuals present in the stock selection analysis. The socio-demographic characteristics of this sample are

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

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

similar to the purchase decision sample (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 who have family members purchasing stocks in the same month would leave only 28 hundred unique individuals. In unreported regressions, we verify that the results are very similar for this subsample, even after controlling for the stock selection of family members.

In Section 3.2 and Section 4.2, we consider individuals who 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 nonmissing from the data. This implies that individuals who start a job fresh from school or move from abroad are excluded. We lose about half of the moves in the database due to this restriction. We require that, at the time of the move, the individual did not change plants in the preceding year or in the following year (to focus on jobs that are not of a temporary nature).[11] We focus on stock market activity during the 12 months prior to leaving the old plant and 12 months after moving to the new plant, which means that we consider moves that occur between January 1995 and December 2004. These criteria leave 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 socio-demographic variables (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.

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 coworkers. The motivation is simple: More trading by coworkers is expected to create more buzz about the stock market and make the individual more likely to also trade.

3.1. Basic results

Our main dependent variable is $buy_{i,t}$ a dummy variable that takes the value one if individual i makes a purchase in month t and that is zero otherwise.

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

$$buy_{i,t} = \beta_1 Buy_{i,t}^{plant} + \mathbf{b}\mathbf{\Gamma} + \varepsilon_{i,t}. \tag{1}$$

$Buy_{i,t}^{plant}$ is the fraction of coworkers who 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 Eq. (1), the estimated β_1 captures the extent to which the individual's purchasing activity is correlated with that of his coworkers. $\mathbf{\Gamma}$ is a column vector of control variables, and $\mathbf{b}$ is a row vector of coefficients. The socio-demographic controls are age, wealth, labor income, sex, 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. We also include $Buy_{i,t}^{fam}$ and $Buy_{i,t}^{zip}$, which control for correlation in timing of purchases inside the family, and within the zip code, respectively and are defined in the same manner as $Buy_{i,t}^{plant}$. Importantly, all of these variables exclude the individual because otherwise a mechanical relation would exist between the individual and the peer group. We include a set of month dummies (132 in total) that control 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, Luttmer, and Mullainathan (2000), Duflo and Saez (2002), and Ivković and Weisbenner (2007).

To estimate Eq. (1), we use the linear probability model as our benchmark and verify robustness using the conditional fixed effects logit estimator. In settings such as Eq. (1), where many fixed effects need to be estimated, it is not obvious whether linear or nonlinear regression models are more appropriate. The disadvantage with the linear probability model is that predicted probabilities could lie below zero or above one. However, the nonlinear alternatives also have disadvantages. First, introducing fixed effects can imply inconsistent estimates due to the incidental parameters problem (see Neyman and Scott, 1948; Lancaster, 2000). The degree of inconsistency depends on the number of observations per fixed effect (Hsiao, 1986; Heckman, 1979), which means that including many controls for unobserved heterogeneity makes the incidental parameter problem more severe (omitting fixed effects is particularly problematic in probit and logit estimations because the estimates will be inconsistent even if the fixed effect is uncorrelated with other explanatory variables; see Yatchew and Griliches, 1985). Taken together, this implies that including an extensive amount of fixed effects leads to inconsistent estimates in the nonlinear setting. The same holds if we relevant fixed effects are excluded. Arguably, the best nonlinear model in our setting is the conditional fixed effects logit model of Chamberlain (1980), which bypasses the incidental parameters problem by not estimating the fixed effects. However, a disadvantage of the conditional fixed effects logit model is that, because the fixed effects are not estimated, this implies that the marginal effects are of limited use (see Greene, 2012, p. 763, footnote 32).

Panel A of Table 2 presents the empirical results. Column 3 is the main specification. The estimated β_1 is positive and highly significant. In terms of economic magnitude, in Column (3), a 1 standard deviation increase in coworker trading activity ($Buy_{i,t}^{plant}$) results in an increase in trading activity of 40.90% relative to the unconditional mean [0.183 (point estimate from Model 3) times 0.116 (standard deviation of

[11] In addition, 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 six 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.

Buy^{plant})] divided by 0.0519 (the mean of *buy*).[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_1 is similar to that reported in Column 3. In addition, we consider specifications without fixed effects. In this case, the point estimates and economic magnitudes are larger than for Column 3.

The zip code-level correlation in trading behavior is significantly reduced when workplace peer effects are introduced. The introduction of coworkers reduces the impact of neighbors by roughly 20% (comparing Column 2 with 3). The difference is statistically significant at the 1% level. 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 would be expected if the positive correlation at the zip code level is partially driven by coworkers who live close to each other. In addition, in unreported analysis, we find that the introduction of socio-demographic controls reduces the impact of neighborhood peers, while the impact of coworkers 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 Specification 5, we account for time-variant industry-specific events by including monthly industry-level fixed effects. This affects the estimated coworker peer effect only to a minor extent. We account for time-variant local events by including a fixed effect for each municipality-year combination in our data set (there are 459 municipalities in Norway). The results, reported in Column 6, are similar to those reported in Column 3.[13]

In the stock market participation model of Hong, Kubik, and Stein (2004), some investors are susceptible to social influence and some are not. In an unreported analysis, we consider whether socio-demographic characteristics are related to the strength of coworker peer effects. We find that coworkers exert a greater influence on males. We find no relation to age or the level of education.

The extant literature shows that individual investors have a preference for local stocks (e.g., Coval and Moskowitz, 1999; 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 headquarters is not more than one hundred kilometers away from the zip code where the individual lives) or within the industry of employment (the stock two-digit NACE code matches that of the plant). Using these dummy variables, we verify that coworker peer effects are present across local, nonlocal, same-industry, and different-industry stocks. In Section 4, which covers stock selection, we also verify that those results are not restricted to local and same-industry stocks (see Table 9).[14]

We also examine whether the peer effects that we find are restricted to particular industries. Table A3 estimates a separate coworker 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 coworkers is universal across industries.

A drawback of the linear probability model (LPM) is that it can imply predicted probabilities outside the unit interval. In Panel C, we reestimate Eq. (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] The lower number of observations in this panel is due to the conditional fixed effect estimator requiring variation in the dependent variable within each group ($plant \times postcode$). So plants with very few individuals (or relatively few individuals and a short time series) are dropped from the estimation. Consistent with the results in Panel A, we find that the effect of $Buy^{plant}_{i,t}$ is statistically significant at the 1% level. In Specification 3, the coworker peer effect has an odds ratio of 10.24. So, if $Buy^{plant}_{i,t}$ goes from zero to one, the odds increase roughly tenfold.

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 coworkers 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 one if the individual makes a sale in month t and that otherwise is zero. Our main explanatory variable is $Sell^{plant}_{i,t}$, which is the fraction of the individual's coworkers making a sale in month t. In Panel B of Table 2, we reestimate Eq. (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 1 standard

[12] The estimated impact of family and neighbors is lower. A 1 standard deviation increase is associated with an increase in trading activity of 23% [(0.1361 × 0.88)/0.0519] and 19% [(0.0369 × 0.272)/0.0519], respectively.

[13] We also consider the influence of non-coworkers on the individual as a placebo test. We construct an analogue to Buy^{plant} for all non-coworkers ($Buy^{non\text{-}plant}$), capturing the purchasing intensity of non-coworkers. Because each of our plants represent a small fraction of the economy, there is very limited variation across plants in $Buy^{non\text{-}plant}$ and, therefore, collinearity with our time fixed effects. When we exclude time fixed effects, we find that $Buy^{non\text{-}plant}$ is positively related to the individual's purchase decision. This is not surprising because $Buy^{non\text{-}plant}$ captures economy-wide sentiment. However, as expected, the economic impact of non-coworkers is limited when compared with actual coworkers (less than one-fifteenth).

[14] In unreported regressions, we find that coworker same-industry purchases have a greater effect on individual same-industry purchases than on coworker non-same-industry purchases. This is not surprising if the coworker and the individual purchase the same stock. Because we examine whether the individual purchases the same stocks as his coworkers (see Section 4), we omit these results for brevity.

[15] Chamberlain (1980) shows that this estimator avoids the incidental parameter problem (Neyman and Scott, 1948) that arises in a probit or logit model when fixed effects are estimated. This is particularly useful in this context because there are 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:

Table 1
Trading of individuals and peers.

This table contains descriptive statistics on trading of individuals and their peers. Panel A considers stock purchases. $buy_{i,t}$ is a dummy variable that takes the value one if individual *i* makes a purchase in month *t*, otherwise it is zero. $Buy_{i,t}^{plant}$, $Buy_{i,t}^{family}$, and $Buy_{i,t}^{zip}$ are the fraction of plant, family, and zip code peers that make a stock purchase in month *t*. Panel B, we consider stock sales. $sell_{i,t}$ is a dummy variable that takes the value one if individual *i* makes a stock sale in month *t*, otherwise it is zero. $Sell_{i,t}^{plant}$, $Sell_{i,t}^{family}$ and $Sell_{i,t}^{zip}$ are the fraction of plant, family, and zip code peers that make a stock sale in month *t*. The sample period is January 1995 to December 2005.

Variable	Mean	Median	Standard deviation	Minimum	Maximum	*N*
Panel A: Purchases						
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: Sales						
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

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 a sale in that month. A *t*-test of the difference between the point estimate of $Buy_{i,t}^{plant}$ and $Sell_{i,t}^{plant}$ is statistically significant at the 1% level, confirming that peer effects are larger for purchases than sales.[17] The vector $\mathbf{\Gamma}$ contains the same controls as in Panel A.

In Panel D, we revisit the sales analysis using conditional fixed effects logit estimation. Qualitatively, the results are similar to those found in Panel B. For all three peer groups, an increase in the fraction of peers that undertake a sale results in higher selling activity of the individual. In Specification 3, the odds ratio associated with $Sell_{i,t}^{plant}$ is 2.484 which is substantially lower than for purchases (10.240, Specification 3 of Panel C). 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 who move between plants, down to a monthly level.[18] Workers who move between plants allow for a placebo test. We analyze how individual purchases relate to the purchase activity of future coworkers. The idea is that future coworkers are unlikely to influence via social interaction but can still exhibit correlated behavior due to similarity along unobservables. Thus, if unobserved similarities drive the results, we would expect the correlation with future coworkers to be of comparable magnitude to the correlation with current coworkers.

Considering workers who move between plants also provides 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. 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: "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 coworkers, we interact the fraction of new and former coworkers who

(*footnote continued*)
"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 cluster only in the plant dimension because the conditional fixed effects estimator cannot accommodate standard errors clustered in multiple dimensions (see Cameron and Miller, 2010).

[17] Corresponding differences for the family and zip code peer groups are statistically significant at the 1% and 10% level, respectively.

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

Table 2
Peer and investor trading.

The following equation is estimated:

$$buy_{i,t} = \beta_1 Buy_{i,t}^{plant} + \beta_2 Buy_{i,t}^{family} + \beta_2 Buy_{i,t}^{zip} + \mathbf{b}\boldsymbol{\Gamma} + \varepsilon_{i,t}.$$

In Panel A (B), the dependent variable is the dummy variable *buy* (*sell*) that takes the value one if the investor makes a purchase (sale) in that month and zero 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 $Sell_{i,t}^{zip}$). $\boldsymbol{\Gamma}$ includes the following socio-demographic variables: *Age*, Age^2, *LogIncome*, $LogIncome^2$, $LogIncome^3$, *LogWealth*, $LogWealth^2$, $LogWealth^3$, $LogIncome \times LogWealth$, *Male*, and *Education*. In some specifications we include time (month), plant, 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 [Nomenclature Generale des Activites Economiques dans I'Union Europeenne (NACE) 2] fixed effects and in Specification 6, we include year × municipality fixed effects. t-statistics (in parentheses) are based on robust two-way (plant and time) clustered standard errors. The specifications in Panel C (D) are identical to those in Panel A (B) except that we use the conditional logit as estimation method. We condition on the zip × plant categories and include month dummies in the regressions. We report odds ratios and z-statistics (in parentheses). The z-statistics are based on robust standard errors clustered at the plant level. *** indicates statistical significance at the 1% level. The sample period is January 1995 to December 2005.

	1	2	3	4	5	6
Panel A: Investor purchases						
Buy^{plant}	0.189***		0.183***	0.172***	0.162***	0.189***
	(8.66)		(9.11)	(7.38)	(14.37)	(11.42)
Buy^{zip}		0.339***	0.272***	0.266***	0.209***	0.212***
		(4.59)	(4.76)	(3.83)	(12.68)	(3.98)
Buy^{family}	0.090***	0.099***	0.088***	0.085***	0.098***	0.098***
	(14.12)	(15.11)	(13.87)	(12.46)	(18.44)	(18.81)
Socio-demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Monthly Fixed effects	Yes	Yes	Yes	Yes	No	Yes
Plant Fixed effects	Yes	Yes	Yes	No	No	No
Zip Fixed effects	Yes	Yes	Yes	No	No	No
Plant × zip Fixed effects	Yes	Yes	Yes	No	No	No
Plant × year Fixed effects	No	No	No	Yes	No	No
Zip × year Fixed effects	No	No	No	Yes	No	No
Plant × zip × year Fixed effects	No	No	No	Yes	No	No
Time × industry Fixed effects	No	No	No	No	Yes	No
Year × municipality Fixed effects	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						
$Sell^{plant}$	0.063***		0.062***	0.035***	0.091***	0.102***
	(9.38)		(9.30)	(4.47)	(17.21)	(16.85)
$Sell^{zip}$		0.173***	0.160***	0.129***	0.196***	0.243***
		(6.26)	(6.14)	(4.26)	(11.48)	(11.38)
$Sell^{family}$	0.061***	0.063***	0.060***	0.053***	0.078***	0.078***
	(16.38)	(17.30)	(16.76)	(14.34)	(23.26)	(23.44)
R^2	0.246	0.246	0.247	0.354	0.035	0.041
Socio-demographic controls, N and fixed effects are identical to Panel A						
Panel C: Conditional logit estimations, purchases						
Buy^{plant}	11.775***		10.240***			
	(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)			
Socio-demographic controls	Yes	Yes	Yes			
Monthly dummies	Yes	Yes	Yes			
N	3,697,776	3,697,776	3,697,776			
Pseudo R^2	0.080	0.067	0.083			
Panel D: Conditional logit estimations, sales						
$Sell^{plant}$	2.597***		2.484***			
	(13.28)		(13.06)			
$Sell^{zip}$		37.019***	26.819***			
		(11.01)	(10.89)			
$Sell^{family}$	2.576***	2.728***	2.522***			
	(32.61)	(36.10)	(32.09)			
N	3,163,212	3,163,212	3,163,212			
Pseudo R^2	0.053	0.052	0.054			
Socio-demographic controls and fixed effects are identical to Panel C						

make a purchase in a given month with dummy variables that indicate whether that month is prior to leaving (joining) the former (new) plant or not. For more than 80% 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 the complements to each other. The variable $Buy_{i,t}^{old\ before}$ is the fraction of former coworkers who make a purchase prior to the individual leaving the old plant (this variable takes the value zero after leaving the old plant), and the variable $Buy_{i,t}^{old\ after}$ is the fraction of former coworkers who make a purchase after the individual has left the old plant (this variable takes the value zero before leaving the old plant). The variables describing the purchase activity of new plant coworkers, $Buy_{i,t}^{new\ before}$ and $Buy_{i,t}^{new\ after}$, are defined in the same manner. In addition, we restrict the sample to 12 months before the individual leaves the old plant and 12 months after joining the 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

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

Estimating Eq. (2) allows us to track how the correlation in behavior with different coworkers 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 socio-demographic 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 coworkers at the old plant before leaving. The estimated coefficient is similar to the estimated coefficient on coworkers 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 coworkers. The coefficient on future coworkers is positive, but small and barely significant. Thus, endogenous group membership does not appear to be a major concern. In Column 3, we include both current and future coworkers. The placebo coefficient from Column 2 is reduced by more than a fourth, while the coefficient on $Buy_{i,t}^{old\ before}$ is not significantly affected. In Columns 4–6, we perform the same exercise on months after joining the new plant. The coefficient on new coworkers in Column 5 is again very similar to the overall sample, Column 3 of Table 2. The coefficient on new coworkers is not substantially affected by including a control for previous coworkers, as seen from Column 6. Also, the correlation with previous coworkers cannot be used as a placebo test, because the individual is likely to stay in touch with former coworkers.

Column 7 shows that the correlation with former coworkers significantly drops after the individual leaves the old plant (the before–after difference is significantly different from zero at the 1% level). In Column 8, we address the reflection problem by considering the correlation with new coworkers the year after the individual has joined the plant. As argued above, β_4 in Column 8 is likely to be mainly driven by influence from the incumbent group of workers on the individual. The estimated β_4 is similar to that reported in Column 5.

In Column 9, we include all sample months and consider the full specification described in Eq. (2). All the coefficients are similar to those reported in Columns 1–8. Finally, in Column 10, we restrict the sample to those individuals who 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.

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, we define 25 dummy variables $\{\mathbf{1}_t\}_{t=-12,12}$. Each dummy equals one for month t and zero otherwise (e.g., $\mathbf{1}_3 = 1$ if $t=3$ and zero 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 Eq. (2) and are described in the caption to Table 2.[19] The coefficients $\beta_{old,t}$ and $\beta_{new,t}$ capture the correlation with former and new coworkers in month t, after controlling for fixed effects. A large fraction of individuals leave their jobs in December and join their new job in January. To illustrate that the results are robust to these individuals, we exclude them when estimating Eq. (3), but they are kept in Table 3 (the previous table). The results of this regression are exhibited in Fig. 1. Month 0 in Fig. 1 is the month when the individual leaves the old plant.

[19] The regression specification in Eq. (3) is a slight simplification of the actual regression specification. First, to capture the less than 20% of the sample that moves with a gap month between the old plant and the new plant, we estimate

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

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% of moves in which 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 12th month before leaving the old plant and the 12th month after joining the new plant. Second, to calculate the moving average of coefficients in Fig. 1, we include interaction effects for month 13 prior to leaving the old plant and month 13 after joining the new plant.

Table 3
New and former coworkers.

We examine the relative impact of new and former coworkers before and after the investor leaves (joins) the old (new) plant. To do so, we create two dummy variables that take the value of one for months before (after) the investor leaves the old plant and zero otherwise. Similarly, we create two dummy variables that take the value of one for months before (after) the investor joins the new plant and zero 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 equation:

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

where $\mathbf{\Gamma}$ includes the socio-demographic variables listed in the caption to Table 2. In addition to month, plant, and zip code fixed effects we include zip × plant fixed effects. We also include dummies for the number of months before leaving 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 Column 10, we consider only those individuals who do not change the municipality where they live or work in conjunction with the plant move. *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. The sample period is January 1995 to December 2005.

	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					0.045***	0.032**	0.043**
		(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}	0.075	0.098**	0.073	0.109**	0.098**	0.085*	0.091**	0.094***	0.077*	0.096**
	(1.46)	(1.84)	(1.44)	(2.21)	(2.14)	(1.89)	(2.18)	(2.78)	(1.95)	(1.98)
Buy^{family}	0.073***	0.074***	0.073***	0.067***	0.066***	0.065***	0.069***	0.069***	0.068***	0.065***
	(6.05)	(6.14)	(6.03)	(5.40)	(5.33)	(5.27)	(7.35)	(7.30)	(7.21)	(6.42)
Socio-demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Monthly Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Plant Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Zip Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Plant × Zip Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time prior leaving Fixed effects	Yes	Yes	Yes	No	No	No	Yes	Yes	Yes	Yes
Time prior joining Fixed effects	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

In Fig. 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 coworkers 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 Fig. 1 illustrates how the correlation with past coworkers evolves over time. The correlation with former coworkers 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 coworkers evolves in a pattern that reflects proximity to those coworkers.

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 advisers. 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. 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:

$$buy_{i,t} = \beta_1 buy_{j,t} + \mathbf{b\Gamma} + \varepsilon_{i,t}, \tag{5}$$

where $buy_{j,t}$ is a dummy variable that takes the value one if coworker j makes a purchase in month t and otherwise it is zero.[20] As before, we include $Buy_{i,t}^{fam}$ and $Buy_{i,t}^{zip}$, as well as socio-demographic controls and month and plant fixed effects.

Panel A of Table 4 presents our point estimates of β_1 for all size deciles. We also tabulate the mean number of employees for each size decile. In smaller plants, the effect

[20] Bayer, Ross, and Topa (2008) use a similar regression strategy to study the role of informal networks in job hiring.

of coworkers 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.

We benchmark the peer effect by estimating a placebo effect. First, we match each selected individual with a randomly chosen individual from a different plant within his size decile (the placebo coworker). We then reestimate Eq. (5) using the buying intensity of the placebo coworker. Panel B of Table 4 tabulates the associated point estimates of the effect of the placebo coworkers. As expected, the estimated effect is economically marginal, and it is statistically significant only for one size decile.

Fig. 2 plots the effect [β_1 from estimating Eq. (5)] of coworkers (red solid line) and placebo coworkers (blue dashed line) for different plant sizes. There is a sharp decrease in the effect of coworkers (y-axis) going from Decile 1 to Decile 5. In addition, in Deciles 1 through 4, the effect of coworkers is significantly greater than the effect of placebo peers.

Following Duflo and Saez (2002), a third way to deal with the possibility of shocks at the plant-month level is to exploit that peer groups are likely to form among individuals with similar socio-demographic characteristics. For example, females could talk more with females than with males, and individuals in the same age group could be more likely to talk with each other. In Table A4, we regress individual purchases on purchases made for each subgroup separately. The estimated peer group coefficients are more often than not larger within subgroups than between subgroups.

4. Stock selection

In this section, we consider the relation between an individual's stock selection and the stock selection decisions made by his coworkers. The motivation is simple: Coworkers 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. We restrict our attention to only those months in which the individual makes at least one purchase. 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 a main explanatory variable, we construct an analogous variable, $F^{plant}_{i,t,s}$, which is the fraction of purchases made by individual i's coworkers (excluding the individual's purchases) that is invested in stock s. This variable is defined only if at least one coworker makes a purchase in month t (if we did not condition on a purchase, the variable would confound stock selection with the decision 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 because there are roughly two hundred stocks on the OSE over the sample period.

4.1. Basic results

To relate individual stock selection to that of his coworkers, we estimate the following regression:

$$f_{i,t,s} = \beta_1 F^{plant}_{i,t,s} + \mathbf{b\Gamma} + \varepsilon_{i,t,s}. \quad (6)$$

The coefficient β_1 captures the extent to which stock selection of an individual is correlated with that of coworkers. To accommodate 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 relation 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, $F^{zip}_{i,t,s}$, which is defined in the same manner as $F^{plant}_{i,t,s}$. Both $F^{plant}_{i,t,s}$ and $F^{zip}_{i,t,s}$ sum to one across stocks in a given month.

The results are presented in Table 6. In the regressions, there are 87,812,052 observations, which correspond 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_1 coefficient is positive and highly significant. In terms of economic magnitude, in Column 3, a 1 standard deviation increase in the fraction of coworkers' purchases allocated to a particular stock results in a 194% increase in the fraction of that month's purchases allocated to that stock by the individual.[21] 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).[22]

[21] The economic impact is calculated as 0.196 [point estimate from Specification (3)] $\times$ 0.0486 (standard deviation of F^{plant}) / 0.0049 (the mean of f). From Column 2 and 3: the correlation with geographical neighbors drops when coworker stock selection is included. The converse is not the case. The correlation with coworkers is hardly affected by introducing neighbors, as seen by contrasting Columns 1 and 3. This is consistent with correlation at the zip code partly proxying for social interaction in the workplace.

[22] We do not control for family group stock selection, as this would leave us with a very small sample size. We verify that the estimated coefficient on $F^{plant}_{i,t,s}$ is very similar for this subsample also after controlling for $F^{fam}_{i,t,s}$. We also examine the effect of noncoworkers on stock selection. We calculate $F^{non-plant}_{i,t,s}$ as the fraction of non-coworker purchases invested in stock s in month t. Unfortunately, little variation emerges across individuals in $F^{non-plant}_{i,t,s}$ in a particular month, which implies that collinearity exists between $F^{non-plant}_{i,t,s}$ and the *month* $\times$ *stock* fixed effects. When we exclude *month* $\times$ *stock* fixed effects, both $F^{non-plant}_{i,t,s}$ and $F^{plant}_{i,t,s}$ are statistically and economically significant.

Table 4
Interaction in peer groups of different size.

We examine whether the impact of coworkers depends on plant size. For each month, we sample two individuals from each plant, denoted by worker i and worker j, and rank plants into ten size deciles, according to the number of workers in total at the plant. We estimate the following equation for each size decile:

$$buy_{i,t} = \alpha_t + \beta_1 buy_{j,t} + \beta_2 Buy_{i,t}^{family} + \beta_3 Buy_{i,t}^{zip} + \mathbf{b\Gamma} + \varepsilon_{i,t},$$

where $buy_{i,t}$ takes the value one if worker i makes a purchase in month t and zero otherwise, and $buy_{j,t}$ is the corresponding dummy variable for worker j. The vector $\mathbf{\Gamma}$ includes the socio-demographic 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. Panel B contains a placebo analysis where we estimate the above equation after randomly pairing two individuals from different plants in the same size decile. 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%, and 1% level, respectively. The sample period is January 1995 to December 2005.

	Size decile									
	1 (small)	2	3	4	5	6	7	8	9	10 (large)
Panel A: Peer effects in plants of different size										
$buy_{j,t}$	0.0750***	0.0492***	0.0411***	0.0161*	−0.0031	0.0061	0.0010	−0.0030	0.0054	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 number of employees	4.74	9.63	15.11	21.91	30.74	41.93	57.72	82.73	131.18	464.52
Panel B: Placebo peers										
$buy_{j,t}$	−0.0044	0.0002	−0.0063*	0.0009	−0.0037	0.0010	0.0022	−0.0014	−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 number of employees are identical to those in Panel A by construction										

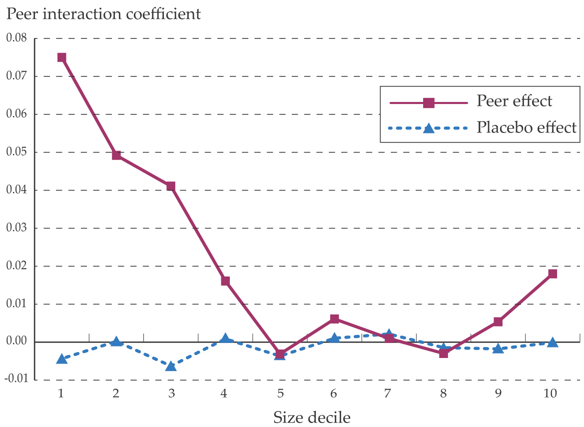

Fig. 2. Slope coefficients from estimating Eq. (5). We divide all of our plants into size deciles in terms of the number of employees in each month. From each plant, we sample two individuals (investor i and coworker j). We relate the purchase decision of i ($buy_{i,t}$) to that of j ($buy_{j,t}$) by estimating the regression described in Eq. (5) for each size decile. The red solid line plots the estimated slope coefficients from the regressions. The blue dashed line, the 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.

4.2. Changes in place of work

To analyze the impact of new and former coworkers on stock selection, we interact the fraction of former and new coworkers who 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 former coworkers prior to the investor leaving the old plant. After the departure date, the variable takes the value zero. Similarly, the variable $F_{i,t,s}^{old\ after}$ is the fraction invested by coworkers at the old plant in stock s after the individual has left the plant. Before that, the variable takes the value of zero. The variables describing the stock selection of new plant coworkers, $F_{i,t,s}^{new\ before}$ 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 6,458 individuals. The socio-demographic 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

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

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 here are contaminated by plant-specific preferences for particular stocks, and the analysis mainly has interest in illustrating differences between the estimated coefficients in Eq. (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 coworkers at the old plant before leaving that plant. In Column 2, we relate individual purchases to that of future coworkers.

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. $F^{plant}_{i,t,s}$ and $F^{zip}_{i,t,s}$ are the average fraction invested in stock s in month t by plant and zip code peers, respectively. The sample period is January 1995 to December 2005.

	Mean	Median	Standard deviation	Minimum	Maximum	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

The coefficient is noticeably smaller than the coefficient measuring the effect of current coworkers 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 the stock selection of both current and future coworkers. Neither of the coefficients from Column 1 or Column 2 is 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 of new coworkers is not affected by including a control for previous coworkers, as seen from Column 6.

Columns 7–9 combine the period before the move with the period after joining the new plant. Column 7 shows that the correlation with new coworkers significantly increases after the individual joins the new plant (the before–after difference is significantly different from zero at the 1% 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 Eq. (7). All the coefficients are similar to those reported in Columns 1–8. Finally, in Column 10, we restrict the sample to those individuals who do not change the municipality where they live or the municipality where they work in conjunction with the change in plant.

To consider how the relation with the peer group evolves over time in more detail, we move to the monthly level. Let t denote event time in months. We estimate the following regression:

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

The vector $\mathbf{\Gamma}$ contains the same controls as Eq. (7). The coefficients $\beta_{old,t}$ and $\beta_{new,t}$ capture the correlation with former and new coworkers in month t, after controlling for fixed effects. The results of this regression are exhibited in Fig. 3.

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

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. If social interaction drives our results, we would expect stronger correlation between individuals at a small plant than at a large plant, because two individuals are more likely to engage at a small plant. To test this hypothesis, from each plant-month we keep the stock selection decision of one individual and one of his coworkers. We divide all of our plants into ten size deciles and for each decile we estimate the following regression (the stock selection analogue to Section 3.3):

$$f_{i,t,s} = \beta_1 f_{j,t,s} + \mathbf{b\Gamma} + \varepsilon_{i,t,s}, \tag{9}$$

where $f_{j,t,s}$ is the allocation of coworker j to stock s in month t. We control for stock selection at the postcode level by including $F^{zip}_{i,t,s}$ 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_1 for all size deciles. The point estimate for Decile 1 is more than five times as large as the point estimate for Decile 10. The mean number of employees in Decile 1 is 7.22 versus 1,464.67 for Decile 10.

We benchmark the peer effect by reestimating Eq. (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.

Fig. 4 plots the effect [β_1 from Eq. (9)] of coworkers (red solid line) and placebo peers (blue dashed line) for different plant sizes. There is a rapid decrease in the peer effect when going from Decile 1 to 4 (the mean plant in Decile 4 has 61.87 employees). From Decile 4 to 10, there seems to be a more gradual decrease. However, the effect

[23] We do not include plant-stock fixed effects, so that the level of the estimates is affected by plant-specific preferences for particular stocks.

(*footnote continued*)
Thus, we are mainly interested in the difference between points on the red line and points on the blue line.

Table 6
Peer effects and stock selection.

This table relates 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 following equation is estimated:

$$f_{i,t,s} = \beta_1 F^{plant}_{i,t,s} + \beta_2 F^{zip}_{i,t,s} + \mathbf{b}\boldsymbol{\Gamma} + \varepsilon_{i,t,s},$$

where the dependent variable $f_{i,t,s}$ is the fraction of purchases invested in stock s in month t by the investor. $F^{plant}_{i,t,s}$ and $F^{zip}_{i,t,s}$ are 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 Column 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. *** indicates statistical significance at the 1% level. The sample period is January 1995 to December 2005.

	1	2	3	4	5	6	7	8
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 Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Plant × stock Fixed effects	Yes	Yes	Yes	No	No	No	No	No
Zip × stock Fixed effects	Yes	Yes	Yes	No	No	No	No	Yes
Plant × year × stock Fixed effects	No	No	No	Yes	No	No	No	No
Zip × year × stock Fixed effects	No	No	No	Yes	No	No	No	No
NACE2 × stock Fixed effects	No	No	No	No	Yes	No	No	Yes
Municipality × stock Fixed effects	No	No	No	No	No	Yes	No	No
NACE2 × year × stock Fixed effects	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 coworkers.

We examine the relative impact of new and former coworkers 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 one for months before (after) the investor leaves the old plant and zero otherwise. Similarly, we create two dummy variables that take the value of one for months before (after) the investor joins the new plant and zero otherwise. We interact these four dummy variables with the variable $F^{plant}_{i,t,s}$ (of the old and new plant) to generate the independent variables $F^{old\ before}_{i,t,s}$, $F^{old\ after}_{i,t,s}$, $F^{new\ before}_{i,t,s}$, and $F^{new\ after}_{i,t,s}$. We estimate

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

where $f_{i.t.s}$ is the fraction of month t purchases invested in stock s by investor i. $F^{zip}_{i,t,s}$ 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 Column 10, we consider only 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. *** indicates statistical significance at the 1% level. The sample period is January 1995 to December 2005.

	1	2	3	4	5	6	7	8	9	10
$F^{old\ before}$	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 Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
NACE2 × stock Fixed effects	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

of coworkers is greater than that of placebo peers for all size deciles.

4.3. Same-industry and local stock purchases

In the previous subsections, we find a close relation between the investment decisions of the individual and his coworkers. However, it is still an open question whether this is beneficial. In this subsection, we take a first step (see also Section 5) in considering the effect of social interaction on the quality of investment decisions by determining whether social interaction contributes to the purchase of stocks that most likely are poor hedges of income risk (same-industry stocks; Døskeland and Hvide,

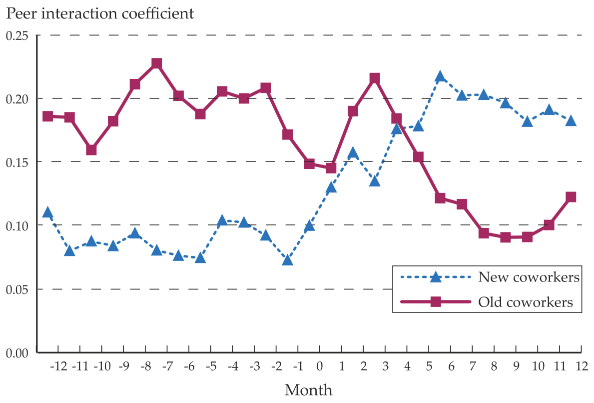

Fig. 3. Slope coefficients from regression equation (8). The dependent variable $f_{i,t,s}$, is the fraction invested by investor i in stock s in month t. Our main independent variables are the fraction invested in stock s in month t of old (new) coworkers interacted with 27 dummy variables, one for each of the 13 months prior to leaving the old plant to 13 months after joining the new plant. We average three consecutive coefficients, and we plot the estimated coefficients from 12 months prior to leaving the old plant to 12 months after joining the new plant. 0 is the start date of new job and end date of old job. We exclude investors who leave their job in December and join the new plant in January.

2011) and exposure to local economic conditions (local stocks; e.g., Coval and Moskowitz, 1999).[24] Thus, purchases of same-industry and local stocks are likely to be less than ideal from a diversification perspective.[25]

Meanwhile, local and same-industry stocks likely are salient objects of workplace conversations and, thus, social interaction effects could be stronger for local and same-industry stocks than for nonlocal and different-industry stocks. To this end, in what follows we examine coworker peer effects for these different types of stocks.

For each individual employed in the private sector, the data set 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 a same-industry stock as a stock in which the worker two-digit NACE code matches the NACE code of the stock. We classify a stock as being local to the individual if the distance from the place of residence of the individual to the stock headquarters is less than one hundred kilometers (km). We create four new dependent variables—$f^{same-industry}_{i,t,s}$, $f^{diff.-industry}_{i,t,s}$, $f^{local}_{i,t,s}$, and $f^{non-local}_{i,t,s}$— to capture the individual's selection of same-industry, different-industry, local, and nonlocal stocks. For example, $f^{same-industry}_{i,t,s}$ is the fraction of total same-industry purchases made in month t invested in stock s. The other variables are defined analogously. Table A5 presents descriptive statistics of the dependent variables.

In Table 9, we reestimate Eq. (6) using the new dependent variables. In Column 1 to Column 4 we consider local, nonlocal, same-industry, and different-industry stocks, respectively. We include month-stock, plant-stock, and zip code–stock fixed effects in all specifications. The point estimate of $F^{plant}_{i,t,s}$ 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 1 standard deviation increase in the allocation of coworkers 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 nonlocal 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. In addition, 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 could lead to a suboptimal portfolio allocation.

5. Should people listen to their coworkers?

The literature on information cascades (Bikhchandani, Hirshleifer, and Welch, 1992; Banerjee, 1992; Ellison and Fudenberg, 1993) posits that imitating coworkers 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; Seasholes and Zhu, 2010). We measure peer pressure as the fraction of coworker purchases allocated to stock s in excess of the economy-wide allocation to stock s. Hence, we rank all purchases made according to

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

where $F^{non-plant}_{i,t,s}$ is the fraction of non-coworkers purchases allocated to stock s in month t.[26] Effectively, $F^{non-plant}_{i,t,s}$ controls for economy-wide trends. So we consider instances in which coworkers are more enthusiastic about a stock than the economy as a whole.

[24] The extant literature shows that the returns of within-industry stocks are correlated with labor income (see Baxter and Jermann, 1997; Eiling, 2013). In the same vein, Massa and Simonov (2006) show that investors invest in stocks that have a high correlation with their nonfinancial income, suggesting that investors do not use the stock market for hedging.

[25] Purchases of same-industry stocks could be a hedge against negative shocks to own-firm performance. As the stock prices 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 at the local price level.

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

Table 8
Interaction in peer groups of different size.

We examine the impact of coworkers in peer groups of different size. For each month, we sample two individuals from each plant, denoted by worker i and worker j, and rank plants into ten size deciles, according to the number of workers in total at the plant. We estimate the following equation for each size decile:

$$f_{i,t,s} = \beta_1 f_{j,t,s} + \beta_2 F^{zip}_{i,t,s} + \mathbf{b\Gamma} + \varepsilon_{i,t,s}$$

where $f_{j,t,s}$ is the fraction of coworker 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 $\mathbf{\Gamma}$ 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 the placebo analysis where we randomly pair two individuals from different plants in the same size decile. We reestimate the above equation while replacing $f_{j,t,s}$ with the corresponding stock allocation of the placebo coworker. 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. The sample period is January 1995 to December 2005.

	Size decile									
	1 (small)	2	3	4	5	6	7	8	9	10 (large)
Panel A: Peer effects in plants of different size										
$f_{j,t,s}$	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 number of employees	7.22	20.73	39.02	61.87	91.56	130.49	184.68	270.23	433.77	1,464.67
Panel B: Placebo peers										
$f_{j,t,s}$	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 number of employees are identical to those in Panel A by construction										

Purchases made under above (below) median peer pressure are sorted into the high (low) peer pressure portfolio. Each purchase is given a unique entry into either the high or the 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.[27] To evaluate investor performance, we run the following regression:

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

where the risk factors MKT (market), HML (high minus low), SMB (small minus big), and MOM (momentum), 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 and standard errors of the long, the short, and the long-short portfolio. For all considered holding periods—one, three, six, nine, and 12 months—the abnormal return to the high-minus-low peer pressure portfolio is not statistically or economically significant. It is important to note that the calendar time methodology implies that we have only one observation for each month over the period 1994 to 2005, suggesting that the power of our test is limited by the length of our time series and that the standard errors of the alphas of the long and the short portfolio are large (around one). This implies that, when examining the long and the short portfolio separately, we have low power. Fortunately, the standard errors of the alphas of the long-short portfolio are much smaller. This suggests that the four-factor model for Norway does not explain returns very well.

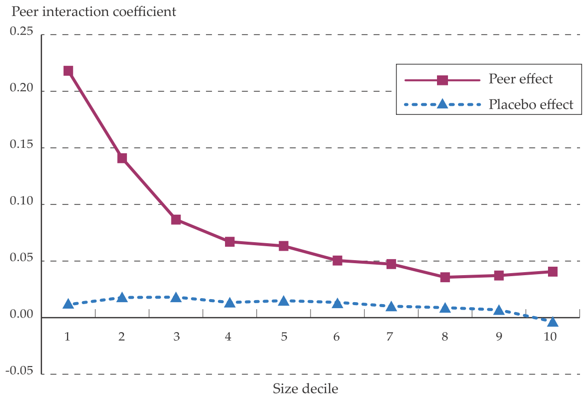

Fig. 4. Slope coefficients from estimating Eq. (9). In each month, we divide all of our plants into size deciles by the number of employees. From each plant, we sample two individuals (investor i and coworker j). We relate the stock selection decision of i ($f_{i,t,s}$) to that of j ($f_{j,t,s}$) by estimating the regression described in Eq. (9) for each size decile. The red solid line plots the estimated slope coefficients from the regressions. The blue dashed line, 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.

[27] Because we consider the performance of purchases in month $t+1$ and onward 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 conclude that this does not qualitatively affect investor performance.

Table 9
Stock selection of local and same-industry stocks.

This table investigates the relation between the stock selection of peers and the stock selection of investors in local and same-industry stocks. The following equation is estimated:

$$f_{i,t,s} = \beta_1 F_{i,t,s}^{plant} + \beta_2 F_{i,t,s}^{zip} + \mathbf{b\Gamma} + \varepsilon_{i,t,s}$$

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 $F_{i,t,s}^{zip}$ are the average fraction invested in stock s in month t by plant and zip code peers, respectively. Column 1 considers only local stocks (stocks headquartered closer than one hundred kilometers from the investor). Thus, the dependent variable $f_{i,t,s}$ measures the fraction of local purchases invested by the individual in stock s. Column 2 considers nonlocal stocks only. Column 3 considers same-industry stocks (defined as in Døskeland and Hvide, 2011) only, and Column 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. *** indicates statistical significance at the 1% level. The sample period is January 1995 to December 2005.

	Local (1)	Nonlocal (2)	Same-industry (3)	Different-industry (4)
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 Fixed effects	Yes	Yes	Yes	Yes
Plant × stock Fixed effects	Yes	Yes	Yes	Yes
Zip × stock Fixed effects	Yes	Yes	Yes	Yes
Plant × zip × stock Fixed effects	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

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 $t+6$. For all of these months, we find that abnormal returns are insignificant.

The preference for local and same-industry stocks could be driven by individual investors profiting from local information (Ivković and Weisbenner, 2005; Massa and Simonov, 2006). Alternatively, investors could suffer from a familiarity bias (Huberman, 2001), in which case local and same-industry purchases should not be associated with any abnormal performance. In fact, the two possibilities are not mutually exclusive. Some investors could act on information while other investors buy what they are familiar with. To evaluate the overall impact of these two hypotheses, we separately consider all purchases of same-industry (same two digit NACE code) and local (headquartered within one hundred 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 Eq. (11) using as the dependent variable the difference in return between high and low peer pressure purchases of same-industry, different-industry, local, and nonlocal 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 is not statistically significant. Over a three-month period, the positive returns turn negative, and when considering a horizon of nine to 12 months, the high peer pressure portfolio under-performs 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 coworkers adds value to purchases.

In contrast, for local purchases, high peer pressure purchases actually under-perform low peer pressure purchases. Over all horizons, the difference is negative. When considering a three-month horizon, the monthly under-performance 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 coworkers. Overall, the results of this section combined with the findings of the previous sections off the paper lead us to conclude that individual investors follow the advice of their coworkers even though the advice does not contain value-pertinent information.

6. Conclusion

This paper addresses whether coworkers influence investment choices, and whether such influence is useful to the investor himself. We employ comprehensive data

Table 10
Peer pressure and returns (January 1994 – December 2005).

We present regression results relating peer pressure to returns. In each month we rank all purchases in terms of their peer pressure, $F^{plant}_{i,t,s} - F^{non-plant}_{i,t,s}$, formally, the allocation of coworkers to stock s in excess of the economy average ($F^{non-plant}_{i,t,s}$) allocation to stock s. Purchases with above (below) median peer pressure are placed in the high (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 the 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:

$$HP - LP_t = \alpha + \beta_1 MKT_t + \beta_2 SMB_t + \beta_3 HML_t + \beta_4 MOM_t + \varepsilon_t$$

where MKT_t, SMB_t, HML_t, MOM_t, are the Fama and French and the Carhart (1997) factors calculated for Norway by Ødegaard (2013). In Panel A, we present results for holding periods one, three, six, nine and 12 months. Panel B presents the 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 reestimate the above equation 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 consider only different-industry, local, and nonlocal purchases, respectively. We report Newey and West (1987) standard errors with three lags. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Holding periods and returns

	Holding period									
	One month		Three months		Six months		Nine months		12 months	
	Alpha	Standard error	Alpha	Standard error	Alpha	Standard error	Alpha	Standard error	Alpha	Standard 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	Standard error	Alpha	Standard error	Alpha	Standard error	Alpha	Standard error	Alpha	Standard 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									
	One month		Three months		Six months		Nine months		12 months	
	Alpha	Standard error	Alpha	Standard error	Alpha	Standard error	Alpha	Standard error	Alpha	Standard 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									
	One month		Three months		Six months		Nine months		12 months	
	Alpha	Standard error	Alpha	Standard error	Alpha	Standard error	Alpha	Standard error	Alpha	Standard 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									
	One month		Three months		Six months		Nine months		12 months	
	Alpha	Standard error	Alpha	Standard error	Alpha	Standard error	Alpha	Standard error	Alpha	Standard 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)

Table 10 (*continued*)

Panel F: Peer pressure and nonlocal investments

	Holding period									
	One month		Three months		Six months		Nine months		12 months	
	Alpha	Standard error	Alpha	Standard error	Alpha	Standard error	Alpha	Standard error	Alpha	Standard 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)

from Norway that cover 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 coworkers. Sorting of unobservably similar individuals to the same workplaces is unlikely to drive the results, as evidenced by the trading behavior of individuals who 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., Moffitt, 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 who are affected. Social interaction appears to increase 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 do not find evidence suggesting abnormal returns. The bottom line of the paper is that individuals are strongly influenced by their coworkers, but this influence does not improve investment quality.

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 idea explored by this literature is that individual investors can affect asset prices if their trading is sufficiently correlated due to social movements (Shiller, 1984). A social movement needs to start somewhere, and the workplace is a plausible candidate. An interesting avenue for future work is to test for social interaction between plants, using movers or geographical proximity as possible drivers of between-plant diffusion.

Appendix A

See Tables A1–A5.

Table A1
Variable definitions.

Variable	Definition
Trade variables (monthly)	
$buy_{i,t}$	Takes the value one if investor *i* makes a stock purchase in month *t* and zero otherwise
$sell_{i,t}$	Takes the value one if investor *i* makes a stock sale in month *t* and zero otherwise
$Buy_{i,t}^{plant}$	Fraction of coworkers who make a purchase in month *t*
$Buy_{i,t}^{family}$	Fraction of family members who make a purchase in month *t*
$Buy_{i,t}^{zip}$	Fraction of neighbors living in the same zip code who make a purchase in month *t*
$Sell_{i,t}^{plant}$	Fraction of coworkers who make a sale in month *t*
$Sell_{i,t}^{family}$	Fraction of family members who make a sale in month *t*
$Sell_{i,t}^{zip}$	Fraction of neighbors who 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 coworkers at the old plant making a stock purchase in month *t*. After the move, this variable takes the value zero
$Buy_{i,t}^{old\ after}$	In months after the individual leaves the old plant, this is the fraction of coworkers at the old plant making a stock purchase in month *t*. Before the move this variable takes the value zero
$Buy_{i,t}^{new\ before}$	In months before the individual joins the new plant, this is the fraction of coworkers at the new plant making a stock purchase in month *t*. After joining the new plant this variable takes the value zero
$Buy_{i,t}^{new\ after}$	In months after the individual joins the new plant, this is the fraction of coworkers at the new plant making a stock purchase in month *t*. Before joining the new plant this variable takes the value zero
$Buy_{i,t}^{non-plant}$	Fraction of non-coworkers who make a purchase in month *t*
Stock selection variables (monthly)	
$f_{i,t,s}$	Fraction of total investor purchases by investor *i* invested in stock *s* in month *t*
$F_{i,t,s}^{plant}$	Fraction of total coworker purchases invested in stock *s* in month *t*
$F_{i,t,s}^{family}$	Fraction of total family purchases invested in stock *s* in month *t*
$F_{i,t,s}^{zip}$	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 coworkers at the old plant invested in stock *s* in month *t*. After the move this variable takes the value zero
$F_{i,t,s}^{old\ after}$	In months after the individual leaves the old plant, this is the fraction of total purchases of coworkers at the old plant invested in stock *s* in month *t*. Before the move this variable takes the value zero
$F_{i,t,s}^{new\ before}$	In months before the individual joins the new plant, this is the fraction of total purchases of coworkers at the new plant invested in stock *s* in month *t*. After the move this variable takes the value zero
$F_{i,t,s}^{new\ after}$	In months after the individual joins the new plant, this is the fraction of total purchases of coworkers at the new plant invested in stock *s* in month *t*. Before the move this variable takes the value zero
$F_{i,t,s}^{non-plant}$	Fraction of total non-coworker purchases invested in stock *s* in month *t*
Individual-stock variables (yearly)	
Local stock	Dummy variable that takes the value one if the headquarters of the stock is located within one hundred kilometers of the place of residence of the investor and zero otherwise
Same-industry stock	Dummy variable that takes the value one if the investor's two-digit Nomenclature Generale des Activites Economiques dans I'Union Europeenne (NACE) code of employment matches the two-digit NACE code of the stock and zero otherwise
Socio-demographic control variables (yearly)	
Income	Income reported by the individual in the previous year's tax return. Reported in Norwegian kroner
Wealth	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	Dummy variable that takes the value one if the individual is male and zero otherwise
Education	Number of completed years of schooling

Table A2
Descriptive statistics of peer groups and socio-demographic variables.

This table presents descriptive statistics on the sample individuals: size of the individual's plant, zip code, and family (excluding the individual) and the number of investors (i.e., individuals who trade at least once over the period 1995 to 2005 and are therefore included in the individual's peer group) in the individuals' respective groups. In addition, we provide descriptive statistics on the socio-demographic variables wealth, income, age, male, and education. The US dollar Norwegian kroner (NOK) exchange rate was 8.77 in December 2000. Number of trades is the number of months in the sample that the individual makes at least one trade. Panel A samples a random year of each individual who 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). The sample period is January 1995 to December 2005.

Variable	Mean	Median	Standard deviation	Minimum	Maximum	*N*
Panel A: Descriptive statistics of purchase sample						
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: Descriptive statistics of mover sample						
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: Descriptive statistics of stock selection sample						
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

Table A3
Industry decomposition of investors, firms, and coworker peer effects.

This table presents descriptive statistics on the industries that our investors work in (Column 1) and the industries that are represented on the Oslo Stock Exchange (OSE) (Column 2). In addition, we decompose the coworker peer effect depending on the industry of employment of the investor. Financial firms [Nomenclature Generale des Activites Economiques dans l'Union Europeenne (NACE) codes 65, 66, and 67] have been excluded from the sample. For this table, we consider only industries that represent at least 0.4% of investor observations (i.e., the industry has at least roughly 420 investors). This restriction implies a loss of less than 3% of the complete sample. To decompose the coworker peer effect across industries, we estimate the following regression:

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

where I_j is a dummy variable that takes a value of one if the investor works in industry j and zero otherwise. Column 3 reports the point estimates of the peer effect for our 36 industries. The vector $\mathbf{\Gamma}$ 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 4) are based on robust two-way (plant and time) clustered standard errors. **, and *** indicate statistical significance at the 5%, and 1% level, respectively. The sample period is January 1995 to December 2005.

Industry (NACE code)	Investors (1)	OSE Firms (2)	Coefficient (3)	t-statistic (4)
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 equipment (32)	838	7	0.538***	(5.24)
Instruments, watches, and clocks (33)	753	4	0.200***	(2.58)
Motor vehicles, trailers, semi-trailers (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		

Table A4
Subsample analysis.

This table examines whether peer effects are stronger among coworkers who are more likely to interact. We classify coworkers along the dimensions of sex, tenure, age, education, and wealth. We sort coworkers 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 coworkers, we calculate the fraction of individuals who 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:

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

The column Group 1 reports point estimates of β_1 and β_2 when the individual belongs to Group 1 [see Duflo and Saez (2002) for an application of this methodology to pension plan enrollment]. We also report the p-value of the F-test of a difference between β_1 and β_2. The Column Group 2 presents the corresponding results when the individual belongs to Group 2. The vector $\mathbf{\Gamma}$ of control variables are 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. *** indicates statistical significance at the 1% level. The sample period is January 1995 to December 2005.

	Group 1	Group 2
Panel A: Male (Group 1) and female (Group 2)		
$Buy_{i,t}^{male\ plant}$	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 coefficient difference	0.29	0.00
N	2,499,192	923,820
Panel B: Low tenure (Group 1) and high tenure (Group 2)		
$Buy_{i,t}^{low\ tenure\ plant}$	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 coefficient difference	0.00	0.00
N	1,803,516	1,559,964
Panel C: Low age (Group 1) and high age (Group 2)		
$Buy_{i,t}^{low\ age}$	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 coefficient difference	0.00	0.03
N	2,117,232	1,893,984
Panel D: Low education (Group 1) and high education (Group 2)		
$Buy_{i,t}^{low\ education}$	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 coefficient difference	0.00	0.22
N	1,892,928	1,852,272
Panel E: Low wealth (Group 1) and high wealth (Group 2)		
$Buy_{i,t}^{low\ wealth}$	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 coefficient difference	0.00	0.00
N	1,668,420	2,180,760

Table A5
Investor stock selection of local and same-industry stocks.

We examine individual stock selection of same-industry, different-industry, local, and nonlocal stocks (examined in Table 9). A local stock is headquartered less than one hundred kilometers from the residence of the individual. Same-industry stocks have the same two digit Nomenclature Generale des Activites Economiques dans l'Union Europeenne (NACE) code as the employer of the individual. The sample period is January 1995 to December 2005.

Variable	Mean	Median	Standard deviation	Minimum	Maximum	*N*
Individual stock selection						
f^{local}	0.0070	0	0.075	0	1	24,714,288
$f^{non-local}$	0.0044	0	0.059	0	1	53,266,715
$f^{same-industry}$	0.0203	0	0.134	0	1	2,442,980
$f^{diff.-industry}$	0.0047	0	0.061	0	1	75,710,195

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