Learning from Coworkers: Peer Effects on Individual Investment Decisions

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Abstract

We use unique data on employee decisions in the employee stock purchase plans (ESPPs) of U.S. public firms to measure the influence of networks on investment decisions. Comparing only employees within a firm during the same election window and controlling for a metro area fixed effect, we find that the local choices of coworkers to participate in the firm's ESPP exert a significant influence on employees' own decisions to participate. Local coworkers' trading patterns also disseminate to colleagues through the network. In the cross-section, we find that some employees (men, younger workers) are particularly susceptible to peer influence. Generally, we find that more similar employees exert greater influence on each other's decisions and, particularly, that high (low) information employees are most affected by other high (low) information employees. However, we also find that the presence of high information employees magnifies the effects of peer networks. We trace a value-increasing investment choice through employee networks. Thus, our analysis suggests the potential of networks and targeted investor education to improve financial decision-making.

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1. Introduction

How do people learn about investments? U.S. households hold roughly one third of their net worth in stocks and mutual fund shares.[1] Thus, personal financial decisions have important consequences for their wealth and welfare. Yet there is extensive evidence that individuals do not always make wise choices when managing their financial investments (Barber and Odean, 2013). We ask whether the choices made by their peers influence investors' individual financial decisions. Using data from the employee stock purchase plans (ESPPs) of U.S. corporations, we compare the trading and participation decisions of employees who work for a firm in the same core-based statistical area (CBSA) to the decisions among employees who simultaneously work for the same firm in different CBSAs. We find that employees are more likely to participate in ESPPs and to quickly sell acquired shares if their nearby colleagues are also doing so. We also find that high information employees facilitate the flow of information through peer networks, thereby identifying a set of employees that firms can target to broadly influence participation and trading behavior. Because ESPP shares are sold to employees at a discount to current market prices, the decisions induced by peer effects are likely to be profitable and to increase individual welfare.

There are many reasons to believe that people's decisions are influenced by those with whom they interact. Social network connections can serve as conduits for the flow of information between individuals (Ellison and Fudenberg, 1995; Banerjee, 1992; Bikhchandani, Hirshleifer, and Welch, 1992). Alternatively, individuals could have preferences that weight relative differences between their own consumption and the consumption of their peers, causing them to mimic external consumption patterns in order to "keep up with the Joneses" (Abel, 1990; Bernheim, 1994). Generally, models of both types predict heightened conformity of choices within peer groups compared to the general population.

Participation and trading decisions within employer sponsored plans – and ESPPs in particular – provide a fertile testing ground for the presence of peer influence on financial choices. The influence of peer decisions is likely to be maximized in an environment in

[1] Source: U.S. Census Bureau 2011 Survey of Income and Program Participation.

which the decisions faced by different individuals are the same – a similar decision setting both facilitates cross-individual comparisons and also maximizes the relevance of information that can be transferred between individuals. Within a single firm's ESPP, all employees simultaneously face the same choice to participate or not to participate given an identical set of plan properties (e.g., the discount from the current market price at which company stock is available for purchase).[2] Similarly, conditional on participating in an employer sponsored plan, all employees hold the same financial security (company stock) and thus their decisions of when to sell are affected by the same fundamental information. Moreover, ESPPs offer employees the opportunity to purchase stock at a discount from current market prices and typically do not place restrictions on the timing of sales. Thus, it is common to observe employees selling shares in the initial days following their election to purchase. These correlated trades, which are likely to be largely unrelated to fundamentals, make it easier to detect peer influence statistically.

We use data provided by an equity compensation administration services provider to test for the presence of peer influence on participation and trading decisions in employer sponsored plans in a sample of more than 500 U.S. firms. We consider a biannual choice for each worker whether to purchase company stock within her firm's ESPP as well as the subsequent decision of if and when to sell any acquired shares.[3] An immediate challenge for our analysis is to separate the influence of peer decisions on employee choices from the effects of selection and the exposure to common shocks (Manski, 1993). To begin to address these concerns, we exploit a unique feature of our data relative to other samples of investor trading decisions that have been used in prior research: its inclusion of worker-firm matches. Many prior studies define peer groups using the locations in which people reside, but as a result face the challenge of separating peer influence from the effects of local shocks. We instead define each individual's peer group (or network) by identifying sets of workers within a metropolitan area who work for the same firm. In all of our regressions, we include fixed effects for each metropolitan area-participation window pair. Thus, we remove the influence of shocks to the local economy

[2] The only difference between employees is the maximum size of participation allowed, which is typically set based on a fraction of pay. Most firms also have a hard ceiling on the maximum participation allowed, which will limit participation for high-income employees.
[3] The biannual time frame is an aggregation choice we make to facilitate consistent analysis across firms, but does not necessarily correspond to the frequency of plan purchases within a given firm's plan.

that might have correlated effects on the investment decisions of investors living in the metropolitan area. We instead exploit variation in the behavior of workers within the metropolitan area who work at different firms. Of course, coworkers in a firm could also be subject to common firm-level shocks. To address this concern, we also include a fixed effect for each firm-participation window pair. Thus, we compare each worker's decision to participate (or trade) to the simultaneous decisions of other workers in the same firm. The fixed effects capture shocks (such as shocks to the value of company stock) that are common to all investors in the plan, and our identification comes from differences in the behavior of groups of workers within the firm who are located in different metropolitan areas.

Including controls for worker characteristics (gender, age, income), we find a significant positive relation between the participation rate in the firm's ESPP in a firm-location and an individual worker's decision to participate. Economically, a ten percentage point increase in local participation is associated with a 1.5 percentage point increase in the likelihood a worker will choose to participate in a given participation window. We find that peer effects also matter for trading decisions. We find a positive association between the average number of days to first trade among employees in the worker's firm-location and the number of days until she makes her first trade, conditional on acquiring shares in the firm's ESPP. Moreover, the likelihood an employee sells the shares acquired in the ESPP within the first two weeks significantly increases with the frequency with which the employee's local co-workers make the same type of sale.

Though our baseline empirical strategy addresses the most obvious sources of confounding common shocks, it is possible that workers in a specific firm-location might be subject to different shocks from their colleagues at other locations in the firm. Firms could segregate different business activities in different geographic locations (e.g., finance versus production) and the workers who conduct those activities might be exposed to different shocks (in a way that is not reflected by differences in the observable demographics for which we control). Following a strategy similar to Duflo and Saez (2002) and Case and Katz (1991), we construct an additional instrumental variables test that exploits the predictive power of demographics for participation and trading decisions.[4] For example, we use the proportion of an employee's local co-workers who are in different five-year bins of the age distribution to instrument for the average participation rate in the firm's ESPP in a given location. The identifying assumption is that the proportion of workers of a certain age group in the office does not directly affect a worker's own decision to participate in the plan once we control for her own age. We confirm the findings of significant positive peer effects on both participation and trading decisions using this approach. It is important to note, however, that the identification strategy is not valid if there are positive exogenous peer effects. Though this mechanism does not appear to be compelling in our setting (e.g., ESPP choices are private and unobserved absent communication between peers), we nevertheless conduct additional tests to address the concern. We find evidence supportive of our identifying assumption.

As a first step toward identifying the economic mechanisms driving the peer effects, we test for cross-sectional differences in the influence of peers depending on observable employee characteristics. We consider interactions of the average participation rate (or propensity to trade shares in the first two weeks conditional on participation) with worker gender, age, and income. We find that women respond significantly less to the decisions of local co-workers than men. We also find that younger workers – particularly workers who are younger than 40 – respond more to the decisions of co-workers than employees of other age groups do. Peer influence is the strongest on workers in the middle portion of the income distribution. Though the gender differences could have many interpretations, the age and income patterns are broadly consistent with a larger peer influence on employees that are likely to have less information about effective trading in ESPPs.

Building on these findings, we turn to our main hypothesis: peer networks serve as conduits for the flow of information between colleagues. To test the hypothesis, we consider three proxies for employee information. First, we identify employees who work in occupations that are likely to be associated with high general knowledge of financial products (finance, accounting, and engineering) or with strong knowledge of the firm's specific compensation plans (human resources). Second, we consider employees who self-report "excellent" or "good" prior investment experience. Third, we consider employees in the highest reported income bin. Using each measure of "high information"

[4] Notably, we find an inverted U-shaped relation between age and ESPP participation and a lower propensity to trade among women and lower-income workers.

employees, we measure separately the average ESPP participation decision among high and low information employees. We then regress an indicator for each employee's decision to participate on the average decisions of high and low information peers as well as the interaction of the average decisions with an indicator for whether the employee herself is a high information employee. Consistent with prior research and the endogenous peer effects mechanism, we generally find the strongest influence of "like on like" (i.e., the decisions of high (low) information employees exert the greatest influence on the choices of other high (low) information employees); however, we also find evidence of significant cross-group influence. We find that the choices of high information employees affect the decisions of low information colleagues, consistent with the learning channel. Though we also find that the choices of low information workers affect the choices of their colleagues, we find that the influence is strongly and significantly muted when there are no high information employees present at the firm-location. Thus, peer interactions with low information colleagues appear to affect choices more strongly when it is more likely that information has diffused from high information colleagues through the network.

Our results identify ESPP participation as a setting in which peer influence serves to spread welfare-increasing practices among employees. As a final test of the endogenous peer effects mechanism, we assess whether peers exert more influence on investment choices among employees who work in CBSAs with higher population density. We find that peer effects are indeed weaker in CBSAs with lower density, suggesting that peer-to-peer learning is less effective when there is less personal contact between individuals. Overall, the results suggest significant potential externalities from educating small numbers of workers on making better financial choices within firm-offered plans.

Our results contribute to the finance literature that studies peer influences on investment decisions. A small subset of these papers uses field data to measure the relation between peers' choices (Brown, Ivkovic, Smith, and Weisbenner, 2008; Hong, Kubik, and Stein, 2004). These studies face a number of empirical challenges due to the limitations of available data. For example, they observe stock market participation, but cannot make more precise statements about how individuals invest. Moreover, they have no means to identify peer groups beyond exploiting geographic variation. One approach to sidestep these challenges, though at the potential expense of generalizability, is to conduct a field experiment. Bursztyn, Ederer, Ferman, and Yuchtman (2014) study the financial choices made by peer pairs who are clients of a Brazilian financial brokerage. They use randomly assigned treatment to identify peer influence, finding evidence consistent with both the information transfer and "keeping up with the Joneses" mechanisms. We instead introduce a richer set of field data. We study a setting in which we can clearly identify investors' asset choices and social network links within geographic partitions (co-worker relationships). The latter feature of the data in particular helps to mitigate the challenge of separating peer influence from exposure to common shocks.

A parallel literature studies how peers influence coworkers' choices in retirement plans. Duflo and Saez (2002) find evidence of endogenous peer influence on enrollment decisions in a Tax Deferred Account plan using field data from employees in a single university. Duflo and Saez (2003) use randomized treatment in a field experiment to confirm the presence of social effects on enrollment choices within a large university's Tax Deferred Account plan. Beshears, Choi, Laibson, Madrian, and Milkman (2015) also use field experiment methodology to study savings choices, finding evidence of a countervailing force: disseminating information about peer investments in the 401(k) retirement plan of a large manufacturing firm causes nonparticipants to decrease their savings, perhaps due to discouragement from unfavorable social comparisons. To our knowledge, ours is the first study to measure the effects of employee networks on investment choices in a large, multi-firm panel of field data. Though participation decisions in retirement plans may have some similarity to the choices employees make to participate in ESPPs, the scope of our data allows us to analyze cross-firm heterogeneity in peer influence and to analyze not only participation, but also repeated trading choices within plans.

Finally, our work contributes to the large literature studying how social influence through network ties affects investment choices. Consistent with an information channel, Cohen, Frazzini, and Malloy (2008) find that portfolio managers outperform when they invest in the stocks of firms that employ managers or directors with whom they share school ties. In a corporate context, Malmendier and Lerner (2013) exploit the random assignment of Harvard MBA students to core sections to show that exposure to more entrepreneurial colleagues as a student decreases the likelihood of engaging in unsuccessful entrepreneurship. Shue (2013) uses a similar empirical strategy to show evidence consistent with a mimicking channel: M&A decisions of CEOs who were classroom peers are more correlated with each other than with other CEOs, though the evidence does not suggest that these mergers are more efficient than typical M&A deals. Moreover, pay for luck also appears to propagate through executive peer networks.

2. Data

To measure peer influence on investors' choices, we use aggregated, non-identifiable data provided by an equity compensation administration services provider, hereafter referred to as "Company X". The data include information on participation and selling decisions in employee stock purchase plans (ESPPs). We observe information for over 500 publicly-traded firms between 2004 and 2013. We also observe ticker symbols for the firms included in the data allowing us to align employee equity ownership information with company accounting information from Compustat and stock price information from CRSP.

We construct several variables to measure how employees behave within their firms' ESPPs. Participation in ESPPs is at the employee's discretion. In a qualified ESPP plan, all full time employees are eligible to participate, meaning they have the right to purchase the firm's stock at a specified discount of up to 15% from the market price.[5] An employee who elects to participate must actively choose a portion of her compensation to be withheld in the plan during each pay period for the purchase of stock under the plan. The typical allowable range of contribution, conditional on participating, is 1% to 15% of compensation. There is typically also a cap on the total investment any employee can make into the plan. Purchases during a purchase period then occur on a single date for all employees inside the firm. In some cases, participants receive favorable tax treatment on long term capital gains (only) if they hold the stock for certain minimum holding periods. However, once stock is purchased, the employee can sell it at any time. Thus, we

[5] The discount may be calculated relative to the market price on the pre-determined purchase date or may be subject to a lookback provision, in which case it is calculated relative to the minimum of the price at the beginning and end of the purchase period.

consider variation both in employees' decisions to participate and in their holding periods conditional on participation. Given the discount at which these shares can be purchased, combined with the flexibility to immediately sell the stake, we interpret failure to participate to be an investment mistake, as in Babenko and Sen (2014).

We consider two main dependent variables in our analysis. First, we construct an indicator variable that takes the value of one if an employee chooses to participate in her firm's ESPP during a given election window. We analyze two participation decisions per firm-year.[6] Our data only includes employees who receive some form of equity-based compensation from their employer within a plan managed by Company X. These plans include stock options, restricted stock plans, and ESPPs. Though we know for certain whether employees elect to participate in their firms' ESPPs (we observe the associated share purchases), we do not necessarily observe all employees within the firm who were eligible, but declined to participate. In each six month window for each sample firm, we proxy for the set of eligible employees with the set of employees who participated in any of the plans managed by Company X and who received a grant during or before the window in question as well as during or after. The final restrictions maximize the likelihood that the employee remains with the firm in question. Because ESPPs are typically open to all employees, we can be confident that the set of employees we analyze in each decision window is eligible to participate. In our sample, we find a participation rate of 43%, which is higher than the 30% rate reported by Babenko and Sen (2014). Some of the difference likely arises from employees at firms in our sample who never receive a grant of any kind from the firm, though some of it could also arise from our more recent sample period. The 43% participation rate implies that we observe a substantial set of eligible employees who fail to participate. Employees we do not observe because they do not receive grants may be less financially knowledgeable and, therefore, more prone to peer influence on financial decisions. If so, our results could understate the importance of peer choices for participation decisions.

Our second outcome of interest is the timing of employees' decisions to sell ESPP shares conditional on participating in the plan. A benefit of studying trading decisions

[6] The frequency of purchase periods can vary across plans. We aggregate the data to two per firm-year to enforce consistency of the analysis across firms. Given the observed frequency of purchases in the data, two periods per year appears to be a reasonable level of aggregation.

within an ESPP compared to general stock trading decisions is that the features of the plan create focal periods within which we expect to see heightened trading that does not necessarily correlate with information about stock fundamentals. One such period is the time immediately after the initial purchase of ESPP shares. Because shares within an ESPP are purchased at a discount to the current market price, it is reasonable to expect some investors to sell the shares within the first few days of acquiring them to lock in the discount. In our data, we observe that more than 20% of ESPP participants sell acquired shares within the first two weeks following purchase. The high volume of trades during this window makes it statistically easier to identify potential peer effects than it might be if we instead were to focus on periods with lower trading base rates. The measurement problem would likely be particularly severe, for example, in an analysis of retail trade data. To exploit this feature of ESPPs, we define an indicator variable that takes the value one if the employee sells shares acquired in a given biannual ESPP grant window within two weeks of purchase. Though this is the main independent variable in our analysis of trading decisions, we also consider indicator variables for trades within different horizons (one week, one month, two months, three months, six months, and one year) as well as a continuous measure of time to first sale (in natural log form).

In Table 1, we present summary statistics of the data. In Panel A, we provide some demographic information on our sample. 28% of the workers in our sample are female and the average worker is 40 years old. Roughly 2.5% of the workers in the sample report income less than $25,000 annually. 8% report income between $25,000 and $50,000, 32% report income between $50,000 and $100,000, 40% report income between $100,000 and $200,000, and 17% report income greater than $200,000. Thus, our sample over-represents high income employees relative to the U.S. population. We also report summary statistics on employees' holdings of company stock options and restricted stock. We calculate these holdings monthly within our sample by adding new grants and then subtracting exercises or shares month by month. We begin the computation at the beginning of 2004, which is the first year for which we observe transactions in our data. As a result, employees who had grants prior to 2004 can have negative calculated holdings in our data. We set these negative values to 0 in our measure of holdings. To account for this censoring in our analysis, we include an indicator variable for employees with holdings of each type that are exactly equal to 0.

In Panel B, we present the distribution of the days to first trade for the subsample of employee biannual observations in which we observe ESPP participation. As noted above, it is relatively common for employees to sell their shares quickly. More than 20% of employees sell within 2 weeks and roughly half within the first year. However, there are employees who hold shares more than seven years without selling.

3. Peer Effects on ESPP Participation and Trading Decisions

We use the data on employee participation and trading within firms' ESPPs to test whether peers influence financial decision-making. Our setting is a natural one in which to test for network effects. For many employees, financial choices are difficult and outside their area of expertise. Moreover, the features of ESPPs, though relatively straightforward, are unlikely to be common knowledge to workers before they accept a job that grants them access to one. Thus, they are likely to value outside sources of information or guidance, including from their local peer groups.

3.1. Identification Strategy

The key challenge for our analysis is to separate the influence of employees' peers on choices from the effects of common shocks. We eliminate the influence of the most obvious common shocks that affect ESPP participation and trading choices by choosing appropriate treatment and control groups. For each employee, we define her peer group to be the set of employees who work at the same firm and who live in the same CBSA. Then we compare the employee's choices only to simultaneous choices by other employees of the same firm from different CBSAs. All employees who work at the same firm buy and sell the same company stock within the firm's ESPP. But by focusing on within-firm variation in peer groups, we eliminate the influence of shocks to firm fundamentals on employees' trading choices. It is also highly unlikely that employees select into different geographic locations inside the firm because of any factor having to do with the ESPP of the firm. Because we use within-firm geographic variation to identify peer groups, shocks to the local economy are another potential source of confounding variation. We eliminate the effect of these shocks on our inference by including fixed effects for each CBSA-month that we observe in our sample. The fixed effects capture any variation that is common to all employees who live in the same CBSA. Thus, our identification relies on the set of CBSAs in our data in which we observe workers from at least two sample firms. Given this discussion, our baseline linear probability model takes the following form:[7]

$$y_{iflt} = \alpha_{ft} + \gamma_{lt} + \boldsymbol{X}'_{\boldsymbol{iflt}}\boldsymbol{\beta} + \delta\bar{y}_{flt} + \varepsilon_{iflt}. \qquad (1)$$

The dependent variable y is either an indicator that equals one if the employee participates in the ESPP during period t or an indicator that equals one if the employee sells shares within two weeks of purchase conditional on acquiring them in the ESPP during period t. The time horizon is six months (i.e., the regressions include two observations per year for each employee in a sample firm) and t indexes the month in which the firms' ESPP purchases occur. α_{ft} is a firm-month fixed effect and γ_{lt} is a CBSA-month fixed effect. The month in which ESPP share purchases occur does not vary within-firm, but can vary across firms in any given six month window. Instead of biannual location fixed effects, our specification is more stringent, implying comparison within any six month window only across firms in which ESPP elections occur in the same month. This approach accommodates differences in location-specific conditions that might arise within six month windows. $\boldsymbol{X}'_{\boldsymbol{iflt}}$ is a vector of control variables that typically includes controls for employee demographics (age, gender, income) and holdings of company securities in other employer-sponsored plans (stock options, restricted shares). $\bar{y}_{flt}$ is the average choice made by workers in employee i's peer group – the other workers in her firm in the same CBSA in month t. We cluster standard errors at the firm-month level to account for the lack of independence of choices across employees in a given month, for example due to the same fundamentals of the underlying investment in company stock. Our null hypothesis is that δ = 0; that is, employees' participation and trading choices are unaffected by the choices of their peers.

A second challenge to identifying δ is the mechanical correlation between y_{iflt} and $\bar{y}_{flt}$ because the choice of employee i influences both quantities. If employee i

[7] The linear probability model is not only useful for avoiding the incidental parameters problem, given our inclusion of two different high dimensional fixed effects, but also for facilitating the interpretation of interaction terms that we include in the regressions later in the paper to assess cross-sectional differences in the influence of peers on employee choices.

participates, for example, there is tendency towards observing a positive value of δ because this choice to participate also increases the average participation rate in the firm-location. A potential way to address this problem is to calculate the average peer outcome by excluding the decision of employee *i* observation by observation. However, this approach biases the estimate of δ, particularly in a context with a binary outcome and a large difference in the frequencies of the two outcomes. To see this, consider the case in which *i* has an outcome of 1, which occurs with low frequency. By excluding *i*'s choice, we measure a lower value of the average for the observation corresponding to *i*. In the other observations in *i*'s peer group for which the outcome is 0, however, we measure a higher value of the average because *i*'s choice is included. To avoid this problem, we use a uniform distribution to randomly select half of the observations in each firm-location. We then measure the average outcome in the firm-location using half of the sample and estimate equation (1) in the other half. Thus, the average choice in a firm-location is the same for all workers in that location, but no employee in the estimation sample contributes directly to the measurement of that average.

The firm-month and CBSA-month fixed effects in Equation (1) address the concern that common shocks to firm fundamentals or the local economy generate similarities in choices that would otherwise be reflected in the estimate of δ. The remaining concern is that there are other sources of common shocks or similarities in unobservable characteristics that might lead to a rejection of the null hypothesis. This could occur, for example, if a firm locates its finance division in a different CBSA from its sales or production offices and those workers are subject to unique shocks. To address this concern, we follow a strategy similar to Duflo and Saez (2002) and Case and Katz (1991). Specifically, we exploit demographic patterns to identify δ. In Table 2, we demonstrate the strength of these patterns on ESPP participation.

In Column 1, we report the results of regressing the indicator variable for ESPP participation on control variables for holdings of restricted stock and stock options (see Section 2) and various employee demographic characteristics. We include indicator variables for five year increments of employee age (30≤ age <35; 35≤ age <40; 40≤ age <45; 45≤ age <50; 50≤ age <55; 55≤ age <60; age >60). The omitted category is workers younger than 30. We also include an indicator for female workers and indicator variables for four categories of reported annual income (\$25K < income ≤ \$50K; \$50K < income ≤ \$100K; \$100K < income ≤ \$200K; income > \$200K). The omitted category is workers who earn less than \$25K. We find a nonmonotonic pattern in worker age. Workers who are in their early thirties are significantly more likely to participate than younger workers. Starting at age 40, each older group of workers participates at significantly lower rates than the youngest workers and the magnitude of the differences increases monotonically as age increases. We also find that women are significantly less likely to participate than men. And, we see that workers with income levels in the middle two regions are significantly more likely to participate in an ESPP than the lowest and highest earning workers. All of these demographic differences are significant at the 1% level. In Column 2, we repeat the estimation, but add in firm-month and CBSA-month fixed effects. Thus, the effects are identified using only variation across employees in the same firm during the same participation window and who are observed in the same month in the same CBSA. We observe the same significant demographic patterns using the within variation as we observe in Column 1 in a pooled specification. A minor difference is that the heightened participation rates now exist for all workers in their thirties, compared to workers who are in their twenties. Finally, in Column 3, we reestimate the Column 2 specification, but using only the randomly chosen half of the sample in which we later identify peer effects. As expected given the random selection, we do not observe any notable differences from Column 2.

Given these patterns, our final identification strategy is to use differences in average demographics to instrument for average participation rates by firm-location-month. When we do so, we continue to control for individual demographics. Thus, identification of the peer effect comes from differences within a firm in the likelihood of participation (or trading) that depend only on differences in the average participation rate across firm-locations that are predicted by differences in average demographics across those locations. So, for example, consider a hypothetical firm with an office in Durham, NC in which the average employee age is 35 and a second office in College Park, MD in which the average employee age is 55. Given the age-pattern in participation from Table 2, we could identify a positive peer effect on ESPP participation using our IV strategy if a randomly selected employee of the firm in Durham is significantly more likely to participate in the ESPP than a randomly selected employee in College Park, controlling for the employees' own ages. Though this strategy allays remaining concerns about common shocks, it relies on the assumption that peer demographics affect individual choices only through their influence on peer choices. This assumption could fail in the presence of contextual peer effects. While it is difficult to construct a mechanism by which such effects would exist in our setting, we perform a number of supplementary analyses to assuage concerns about the instruments throughout our analysis. Moreover, even in this case, we would confirm that peers indeed matter for financial choices.

3.2. Baseline Peer Effects

Our first step is to test if peers – whether by providing information or merely an example – influence workers' ESPP participation and trading choices. To begin, we estimate Equation (1) using an indicator variable that equals one if an employee participates in her firm's ESPP as the dependent variable. We report results in Table 3.

In Column 1, we present the baseline estimates of Equation (1). Among the controls, the demographic variables exhibit the same patterns we observe in Table 2. Workers in their thirties are more likely to participate than younger workers, but after age 50, participation rates decline below those of younger workers. Women are also less likely to participate and workers with annual incomes between \$50K and \$200K are more likely to participate. We also find that workers with larger stock option or restricted stock holdings are more likely to participate. The indicators for having exactly zero holdings of restricted stock or options come in significant and with roughly equal magnitude, though opposite signs. This pattern likely reflects the high positive correlation of the two variables (if an employee had no holdings of restricted stock prior to 2004, they are likely to have no option holdings as well). Our results are insensitive to the choice to include or exclude these controls. The coefficient on the mean participation rate in the firm-location (δ) is positive and statistically significant at the 1% level. Economically, a ten percentage point increase in the mean participation rate among an employee's peers would increase her likelihood of participating in the ESPP by roughly 1.5 percentage points.

In the remaining columns of Table 3, we report estimates from three specifications of instrumental variables regressions using different combinations of average demographic characteristics at the firm-location to instrument for average participation rates. In

Column 2, we report the first stage regression using only the age distribution to construct the instruments. We include as instruments for average ESPP participation in a firm-location-month the average of each of the age category dummies at the firm-location-month. The instruments can also be interpreted as the fraction of employees in the firm-location-month that fall into each age category. To conserve space, we report the coefficient estimates for the instruments in the rows in which we report the estimates of the age dummies in Column 1. Though we do not report the estimates on the age dummies, we do include them in the regression (along with all the other control variables from Column 1). Consistent with the effects of employee age on individual participation rates, we observe that locations with more employees in their thirties have higher average participation rates compared to locations with more employees under the age of thirty and that locations with more employees over the age of sixty have lower participation rates. We also see heightened participation rates where the fraction of employees in their forties and early fifties is higher. This difference from the pattern in individual age comes mostly from not including other average demographics in the specification. Six of the seven instruments are statistically significant at the 10% level or higher (three at the 1% level) and the set of instruments as a whole is strongly statistically significant. The Hansen J test also fails to reject the exogeneity of the instruments. In Column 3, we report the second stage estimates, using only the variation in the instruments to identify the coefficient on the average participation rate in the firm-location-month (i.e., the peer effect). The coefficients estimates on all of the included controls are very similar to those we report in Column 1. Our estimate of δ remains positive and significant (now at the 5% level). Economically, the magnitude of the estimate is slightly larger: here the estimate implies that a ten percentage point increase in average participation in a firm-location would increase the likelihood that a randomly chosen individual employee at that location participates by roughly 2.5 percentage points.

In Columns 4 and 5, we repeat the IV estimation, but expanding the set of instruments to include the mean of the indicator for worker gender (female), or the fraction of women observed in the firm-location-month, as an additional instrument. The additional instrument has a significant negative effect on the endogenous variable (the average participation rate in the firm location), consistent with the effect of the gender control in

Column 1. We find almost no difference in our estimate of δ from expanding the set of instruments. Similarly, in Columns 6 and 7, we add the average of the income category dummies in the firm-location-month, or the fraction of employees in the location in each income category, as additional instruments. Here, we find larger discrepancies between the coefficients on the instruments and the estimates on the corresponding categories in Column 1, perhaps raising some concern as to the source of the variation that the income instruments capture in the participation sample. Nevertheless, we find little difference in our estimate of δ in the second stage, though it is now statistically significant at the 1% level. In all three IV specifications, we continue to find that peers' choices positively influence the decision to participate in an ESPP even when we isolate only the plausibly exogenous variation in coworkers' choices that is due to general tendencies to participate among their demographic groups. This source of variation is unlikely to be contaminated by any kind of unobserved location-specific common shock.

As discussed in Section 3.1, a potential threat to our identification strategy is the presence of contextual peer effects (i.e., that the mean characteristics of an employee's peers directly affect her choices, independently from peers' choices). One way to assess whether our IV strategy can separate endogenous peer effects from contextual ones is to construct a placebo test to see whether it fails to detect an endogenous peer effect in a context in which we know such an effect cannot exist. We do this by considering the effect of the average gender in a firm-location-month on an employee's gender. It could be the case that there are contextual peer effects in this setting; for example, women (or men) might choose to work in certain locations because of the presence of other women (or men) there. However, there is unlikely to be an endogenous peer effect (i.e., employees choose to be a woman because other employees in the office have chosen to be women). First, we confirm in a linear probability model that mirrors Column 1 that it is indeed the case that the likelihood a worker is a woman significantly increases with the fraction of women in a firm-location-month (i.e., it includes all controls and fixed effects, besides the gender dummy, from Column 1). We find a positive coefficient estimate of 0.0943 that is statistically significant at the 1% level. Next, we run an IV specification that mirrors our main specification in Columns 2 and 3, instrumenting for the fraction of female workers in the firm-location-month with the age category instruments. In the first stage, we find that the instruments are even stronger predictors of the fraction of women in the firm-location-month than they are of average participation in Column 2. All seven instruments are statistically significant at the 5% level or greater (six at the 1% level). The coefficient estimates are all negative, implying that locations with younger workers have significantly higher fractions of female workers. Yet, despite the strength of the first stage, we do not find a significant effect of the instrumented fraction of female workers in the firm-location-month on the likelihood of an employee being female in the second stage ($\delta = 0.016$; standard error = 0.101). Though not definitive, this evidence increases our confidence in the ability of our identification strategy to isolate the endogenous peer effects of interest.

Next we test whether the information or model provided by peers' choices also affect the way that employees trade stock conditional on participating in the firm's ESPP. Following the discussion in Section 2, we begin by analyzing the likelihood an employee sells ESPP shares within two weeks of purchase. We follow an approach that mirrors the analysis in Tables 2 and 3. To set the baseline, we estimate a pooled linear probability model on the full set of employees who participate in ESPPs, using an indicator variable that equals one if the employee sells shares within the first two weeks following purchase as the dependent variable. We include the full set of control variables for employee demographics and stock option and restricted stock holdings from Table 3. We report the results in Column 1 of Table 4. Generally, we find that younger workers are more likely to sell ESPP shares within two weeks than older workers. Beginning with workers with ages between 30 and 35, the likelihood of early sales declines monotonically in each successive age grouping. We also find that women are significantly less likely to sell within two weeks than men. Workers in the middle income groupings (annual income between \$25K and \$200K) are more likely to quickly sell ESPP shares than workers with the highest or lowest incomes. We also find significant effects of restricted stock and stock option holdings, however, the estimates change signs once we include firm-month and CBSA-month fixed effects in the remainder of the table. Generally, it appears that workers with larger holdings of restricted stock or stock options are less likely to promptly sell their ESPP shares. In Column 2, we restrict our analysis to ESPP participants within the random analysis subsample from Table 3 and include the full set of fixed effects from Equation (1). We also include the average of the indicator for ESPP share sales within two weeks in the firm-location-month as the explanatory variable of interest. We find similar effects of the control variables in the within specification. The exceptions are the already noted differences in the estimates on the restricted stock and option holdings controls and a weaker difference between the trading behavior of employees with incomes between $25K and $100K from the lowest income workers. As for the effect of interest, we find a positive and statistically significant estimate of δ – a randomly chosen worker in a firm-location-month is significantly more likely to sell acquired ESPP shares within two weeks if more of her local colleagues also do so, compared only to other workers in the same firm and adjusting for contemporaneous CBSA effects. Economically, a ten percentage point increase in the rate at which local employees sell shares in the first two weeks following purchase would increase the likelihood a random employee in that location would sell in the first two weeks by roughly 0.73 percentage points.

As in Table 3, we next use differences in average demographics across firm-locations to identify the peer effects. Here, we use the means of all of the demographic controls (age group indicators, female indicator, and income group indicators) as instruments. We report the coefficient estimates on the instruments in the first stage regression in Column 3 (all of the individual level controls from Column 2 are also included, but we omit the estimates from the table to increase readability). In this context, we do not find as much power to explain average trading rates from the age distribution in the firm-location as we did for participation choices (it is not possible to identify the second stage using only the age instruments). However, the gender and income group instruments are significant predictors of average trading rates, in directions consistent with the effects of gender and income in the baseline specification in Column 1. Jointly, the instruments are statistically significant, with an F-statistic of 54. In Column 4, we report the second stage estimates. We find that the instrumented rate at which peers sell within two weeks of the purchase of ESPP shares has a positive and significant effect on the likelihood an employee sells acquired ESPP shares within two weeks. A ten percentage point increase in the fraction of local colleagues who sell within two weeks is associated with a 2.5 percentage point increase in the likelihood an employee sells within two weeks. Interestingly, in this specification the economic magnitude of the peer effect is very similar to the magnitude of the instrumented peer effect in the participation regressions.

In Table 5, we extend our analysis of trading beyond the specific decision to sell ESPP shares within two weeks of purchase. We replicate the specification from Column 2 of Table 4, but with a series of alternative dependent variables. First, we consider five different specific horizons for the employee's first sale of ESPP shares: one week, one month, two months, three months, and one year. Second, we consider a continuous dependent variable: the natural logarithm of the number of days to the employee's first sale. In general, there is a tradeoff in defining the length of the trading window between choosing a narrow enough window that correlated trading across individuals is meaningful (at the extreme, if we considered a ten-year window, the fact that two employees both trade within the window would not indicate any meaningful commonality in their trading behavior) and choosing a wide enough window that we have enough power to conduct statistical tests. To a certain extent, the results in Table 5 reflect this tradeoff. Over all five alternative horizons, we observe a significant positive effect of local coworkers' tendency to trade in the given window on the likelihood an employee also trades in that window. The magnitude of the effect is slightly smaller at the one-week horizon than the effect we observe at the two week horizon in Table 4. Likewise, the magnitude of the effect monotonically declines as we go from two weeks to one month and beyond. Notably, the magnitude of the effect increases again slightly at the one-year horizon. This effect could reflect tax advantages that are sometimes available from holding shares up to a year, making one year another focal point for trading. In Column 6, we observe that the average of the log number of days to first trade among local coworkers is also a significant positive predictor of the log number of days to an individual employee's first trade. Thus, our basic conclusion holds even without identifying a specific horizon in which trades must occur. We generally find that our instruments have less power to identify the effects in these alternative specifications, with the most success in the specifications in which the peer effect in Table 5 is the most significant.

Overall, our evidence suggests that the investment decisions coworkers make in employer sponsored plans provide a significant guide to the decisions employees make regarding their own investments.

3.3. Cross-sectional Differences by Worker Demographics

Given a significant influence of coworkers' choices on investment decisions within ESPPs, a natural question is whether some types of workers are more influenced by their peers than others. To answer this question, we modify Equation (1) to allow for an interaction term between $\bar{y}_{flt}$, the mean outcome among an employee's peer group, and measures of the employee's characteristics. We consider differences by employee gender, age, and income levels. Though we do not generally invoke our instrumental variables strategy in the remainder of our analysis, it is more difficult than in our baseline setting to generate plausible concerns about common shocks that might drive the results. For example, it is unclear what kind of shock would affect men differently from women who simultaneously work in the same firm and same location. We view the econometric concerns stemming from low power of the instruments and the need to instrument for a binary interaction with the endogenous variable as potentially more severe than the remaining identification concerns.

We present the results in Table 6. In Columns 1 and 2, we report the effect of the interaction of employee gender with peer choices on ESPP participation decisions and the decision to sell shares within two weeks conditional on participating, respectively. To ease readability of the table, we report only the baseline coefficient on $\bar{y}_{flt}$ (δ) and the interaction term with the indicator variable that equals one if the worker is female. However, all controls from our prior analysis are included and have similar effects to those we report in Tables 3 and 4. We find that the investment choices of women are significantly less sensitive to the choices of their local peers than are the investment choices of men. We find that a ten percentage point increase in the local participation rate would have roughly 0.4 percentage points smaller of an effect on the decision of a female employee to participate than the decision of a male employee (Column 1). The difference is even more pronounced on the decision to sell shares within two weeks conditional on participating. We find a significant positive effect of local coworkers' tendency to do such sales on the decisions of men, but we do not observe an effect on the decisions of women (Column 2). The estimated interaction term of the female indicator with the local rate at which employees sell within two weeks fully offsets the baseline effect δ. On the one hand, larger peer effects among men might be surprising given that men on average are more prone to be overconfident in their own abilities (Lundeberg, Fox, and Punccohar, 1994). However, men also tend to have a greater affinity for competition than women (Niederle and Vesterlund, 2007), which could result in a greater awareness of the behavior of their peers.

In Columns 3 and 4, we consider instead employee age, interacting $\bar{y}_{flt}$ (average participation in the firm-location-month and the fraction of participators who sell within two weeks, respectively) with each of the age group dummies. Considering the interactions together with the baseline estimate of δ, we see that participation among local coworkers has a strong positive effect on the likelihood of participating among employees who are less than 30 years of age, between 30 and 35, and between 35 and 40 that is statistically indistinguishable across groups (Column 3). From the age of 40 forward, however, the effect of local coworkers' choices is significantly smaller in magnitude (though still positive) and declines monotonically from group to group to near zero among workers who are older than 60. In Column 4, the dependent variable is instead an indicator equal to one if the employee sells shares within two weeks, conditional on participating in the firm's ESPP. Because we have less power in this sample (it is roughly a third the size of the Column 3 sample), we aggregate the age dummies into larger categories before interacting them with $\bar{y}_{flt}$. Specifically, we consider the interactions of the rate at which local coworkers who participate in the ESPP sell within two weeks with (1) an indicator variable that takes the value of one for employees whose age is between 30 and 50 and (2) an indicator variable that takes the value of one for employees older than 50. We again observe a monotonically declining pattern in the (positive) influence of peers' choices on trading choices as age increases. Workers between 30 and 50 are significantly less influenced than their younger colleagues by the average choices of their local coworkers. The attenuation of the peer effect is even stronger for workers older than 50. In fact, the point estimate of the peer effect even turns slightly negative for this group. Overall, we observe that younger employees are more prone to mimic the decisions of their coworkers than their older colleagues. The effect could be consistent with greater expertise among older colleagues from a longer history of financial decision-making. Alternatively, it could reflect a stronger (false) confidence or lack of receptiveness to outside information among older employees.

Finally, in Columns 5 and 6, we test whether the effect of colleagues' choices on participation and trading decisions differs depending on the employee's reported annual income. We interact $\bar{y}_{flt}$ with our standard four income group categories (\$25K < income ≤ \$50K; \$50K < income ≤ \$100K; \$100K < income ≤ \$200K; income > \$200K). Unlike age, we do not find a simple monotonic effect of income on the degree to which peers' behavior affects individual investment decisions. In Column 5, we do not observe a significant difference between the effects of local coworkers' decisions on workers who earn less than \$25K or between \$25K and \$50K (both groups have a positive and significant estimate of δ). The participation decisions of workers who earn between \$50K and \$200K are more sensitive to the decisions of peers; however, the decisions of workers who earn greater than \$200K are significantly less sensitive to peers' choices. Such a pattern could arise if workers interact more with other workers in their own income groups and, as a result, workers in low income groups less often interact with colleagues who participate in the firm's ESPP. On the high end of the income spectrum, we may be more likely to observe workers in managerial positions whose decision are less likely to be informed by how their subordinates behave. In Column 6, we report the estimates in the context of peer trading. We do not observe significant differences in how peers influence the decision to sell ESPP shares within the first two weeks of the firm-wide purchase date (though the point estimates again suggest a heightened influence in the middle income ranges). We are cautious in interpreting these results, however, because they could simply reflect a lack of power to distinguish among the income categories.

Overall, we find significant differences in the degree to which word-of-mouth affects the investment choices of employees depending on observable characteristics. The gender differences admit a number of interpretations. For example, men may be more generally attentive to financial decisions and, thus, more likely to learn. Alternatively, women may seek information from sources other than peers. Consistent with peer learning, the age and income effects suggest that individuals who are likely to have less information about financial decisions learn the most from their peers.

3.4. Identifying the Peer Learning Mechanism

Our analysis thus far suggests that peer influence matters for financial choices. Moreover, the cross-sectional evidence hints that this influence could be particularly strong among individuals who have low information. Next, we present additional tests to tease out the mechanism through which peers matter. Some existing work finds evidence of peer effects driven, at least partially, by peer pressure or social norms (e.g., Mas and Moretti, 2009). However, in the case of ESPP participation and trading, decisions are confidential. Thus, it is unlikely that pressure to conform to group means is the primary driver of peer effects in this context.

Instead, our hypothesis is that information about value-maximizing investment rules spreads from "high information" employees to "low information" employees through direct communication between colleagues. Because participation in the ESPP is likely to be beneficial for employees, this channel suggests a "social multiplier." As one employee becomes more informed about the ESPP and then communicates the information to her colleagues, overall participation rates increase and there is a positive spillover effect.

The general peer effects channel predicts stronger peer influence among subpopulations of closer peers. For example, to the extent that individuals interact more with members of the same gender or age group, then peer effects should be stronger within those subpopulations (See, e.g., Duflo and Saez, 2000). Likewise, under our hypothesized learning mechanism we expect to see a pattern of "like influencing like" as information spreads among peers. However, our proposed channel also predicts that highly informed individuals or subgroups should influence less informed subgroups, providing the seeds of information that propagates through the network. Thus, to isolate our proposed peer learning channel, we focus on identifying the unique role of high information employees in transmitting information to less informed peers.

We consider three measures of "high information" employees. First, we use employees' reported occupations to identify individuals who work in occupations likely to be associated with relatively strong general knowledge of financial products (accounting, finance, and engineering) or strong knowledge of the firm's specific financial offerings (human resources).[8] For ease of exposition, we refer to these occupations as "HO," or high information occupations, and define an indicator that takes the value one for employees in the "HO" set and zero for all other non-executive employees for whom we observe non-missing occupation data. Second, we use employees' responses to questions about their prior investment experience to identify employees with "excellent" or "good" prior experience. We refer to these employees as "HE," or high experience employees. Data on prior investment experience is missing in most cases; in order to retain a sufficient sample size for our tests, we include employees with missing information in the reference group. To the extent that this induces measurement error, it should bias against finding a significant effect of high experience employees on their less experienced peers. Third, we use employees' reported income bins to proxy for high information. We define an indicator variable that takes the value one for employees in the highest reported income bin (income > $200K) and zero for all other non-executive employees for whom we observe income data. High income employees are likely to be influential not only because they are likely to have more experience with financial products than their colleagues, given their relative wealth, but also because they are likely to hold relatively senior positions inside the firm. We refer to the set of high income employees using the abbreviation "HI."

Before turning to regression tests using the high information measures, we present some summary statistics to provide preliminary validation of the measures. In Panel A of Table 7, we report the pairwise correlations between the three measures of "high information." We find that both the HO and HI measures are positively and significantly correlated with the measure of high self-reported prior investment experience (HE), providing some validation for our interpretation of the occupation and income groups. Interestingly, the HO and HI measures are negatively correlated with each other. Occupations that we associate with high information about financial products are not

[8] We drop employees who identify as executives from our tests because executives may face limitations in the information that they can directly share with employees. In our reported specifications, we also exclude finance workers who work in firms in the finance industry from the set of "high information" employees since it is less clear that they are more informed than their local colleagues; however, this restriction is not crucial for the results.

always the highest paying occupations.[9] Nevertheless, both occupation and income appear to capture aspects of investment experience, suggesting they have value as complementary measures. In Panel B, we present mean worker demographics and stock and option holdings across high and low information employee groups, using each of the three measures. Focusing first on the measure of self-reported investment experience, we find that men, older workers, and workers with higher holding of stock and options more frequently report high investment experience. The latter correlations are intuitive and suggest that individuals report realistic assessments of their experience. Across all three measures, we consistently find that high information employees have higher holdings of equity-linked securities, though the gender and age patterns are less consistent. It is important to note that we control for worker demographics and stock and option holdings in our analysis so that our information measures will not simply proxy for differences in these characteristics.

In Panel C of Table 7, we report mean ESPP participation, separately by occupation, income, and investment experience categories. Because failing to participate in an ESPP leaves money on the table (the discount between the market stock price and the purchase price within the plan), we can interpret it as an investment mistake. Thus, to the extent that we observe nonparticipation, it should occur among low information employees. We find that this is generally the case under each of our three information metrics. We observe a monotonically increasing participation rate as reported investment experience increases. Moreover, employees for whom we do not observe reported investment experience have the lowest participation rates, suggesting that including them in the "low information" group is appropriate. We also find a monotonically increasing relation between ESPP participation rates and income for the interior bins of the income distribution (groups two through five). One possible explanation for the relatively lower ESPP participation rate among workers in the highest income bin (group six) is that most firms place a ceiling on the total dollar amount that can be invested in the ESPP, a constraint most likely to be relevant for workers in this income category. In this case, the gains from participation are relatively less compared to total compensation than for

[9] The negative correlation is sensitive to exactly which occupations we include in the high information group. For example, if we exclude engineering or, alternatively, include employees in "research" occupations, the correlation with HI becomes positive and there is no qualitative change in our later results.

workers in the other income categories. The relatively high participation rate among workers in the lowest income bin (group 1) is surprising under the interpretation of income as a proxy for financial information; however, we find similar results if we exclude these workers from the analysis altogether. Finally, we observe substantial variation in ESPP participation rates across occupation categories. For two of the four subcategories of "high information" occupations (engineers and finance occupations), we verify the prediction of relatively high participation rates. Participation rates are higher than the overall sample mean (0.43) for all of the "high information" occupation groups, except HR. Overall, the summary statistics in Table 7, though noisy, provide some basic corroboration of our interpretation of the information measures.

Given our measures of high information (and low information) employees, we test for evidence of information flow not only within, but also between groups. That is, are high information employees key "influencers" within the peer network? First, we revisit the estimation of Equation (1), but replace $\bar{y}_{flt}$ with $\bar{\boldsymbol{y}}_{dflt}$, where d indicates whether the employee is a member of a high or low information partition and the vector includes group averages for each associated subgroup. We again measure group averages in a separate random sample of the data from the regression sample to eliminate mechanical correlations. We also allow for differences in the estimates of the subgroup means depending on the information of employee i by interacting $\bar{\boldsymbol{y}}_{dflt}$ with an indicator for whether employee i is a member of the high information group captured by d. We follow this approach for each of the three information partitions described above. For this analysis, the variable y is an indicator for whether the employee participates in the firm's ESPP at time t.

We do not consider the timing of sales conditional on participating in the ESPP. Our tests in this section partition the set of employees in the firm-location to measure the peer effect in smaller subgroups (high and low information). On average, roughly 40% of employees in a firm-location participate in an ESPP (Table 1). The average fraction of employees who trade within the first two weeks of stock purchase (our main measure of trading behavior) is an order of magnitude smaller. And, the high information subgroup is relatively small, by definition. Moreover, the outcome itself is binary. Thus, measurement error in group averages and a lack of power in the regression samples make it challenging to implement meaningful tests using trading measures as the choice variable.

We present the results in Table 8. In Column 1, we use occupation groups (HO) to identify high and low information employees. Consistent with heightened communication among more similar peers, we observe a pattern of “like influencing like” in our data. We find a positive and significant coefficient on the level effect of the participation rate among local low information employees, which measures the effect of low information peers on other low information employees. Likewise the sum of the coefficient estimates on the participation rate of high information peers and its interaction with the high information indicator, which measures the effect of high information peers on other high information employees, is positive and significant. We also find evidence of significant cross-group effects. Most relevant for our hypothesis, the level effect of the participation rate among local high information peers, which measures the peer effect on low information colleagues, is positive and statistically significant at the 1% level. We find broadly consistent evidence using the other two measures of high information. In Column 2, we report the results using high investment experience (HE) as the measure of information. We confirm the pattern of “like influencing like.” The point estimates also suggest an influence of high information employees on low information peers; however, the frequency of high experience employees is not sufficiently high to allow us to measure the effect with much statistical precision. Finally, in Column 3, we use high income (HI) to proxy for high information employees. Here the results closely mimic the results from Column 1: we find both significant evidence of “like influencing like” and of high information employees influencing low information peers. Notably, the difference between the effect of low information investors on high information peers and low information peers is also negative and statistically significant in this specification.

The results in Table 8 confirm that the choices of high information employees influence the choices of low information peers. However, we also see not only that low information employees respond to low information peers, but also that the magnitude of the influence of low information peers on their low information colleagues is generally larger than the influence of high information peers. It is important to note that these patterns are still consistent with our learning hypothesis. For example, one high information influencer in an office could share information with a small set of low information colleagues. These colleagues, in turn, could share the information with other low information employees through the peer network (in which low information employees are more likely to interact with other low information colleagues). In this case, it would appear as if the low information employees respond to the decisions of low information colleagues; however, the information that is spread would have originated from a high information colleague. And, such information could be particularly likely to propagate through the network.

To test for this type of diffuse information channel, we restrict our attention to the subsample of low information employees in the regression sample.[10] We define an indicator variable that takes the value one if there are no high information employees in employee *i*'s office location.[11] We then estimate Equation (1), including this indicator, the mean participation rate among low information employees in the firm-location, and the interaction of the two variables in addition to the baseline fixed effects and controls. The null hypothesis is that the coefficient estimate on the interaction term is 0; that is, the presence of a high information employee in the office does not change the effect of the decisions of low information employees on the decisions of their low information colleagues. We implement the test sequentially using each of our three measures of information. In Column 1 of Table 9, we report the results using occupations (HO) to identify high information employees. We strongly reject the null hypothesis. The effect of low information employees on their low information peers is significantly weaker in offices in which there is no high information employee present to seed the flow of information through the network. In Columns 2 and 3, we repeat the estimation using investment experience and income, respectively, to define the set of high information employees. The results are similar. In both cases we find negative estimates on the coefficient of the interaction term that are statistically significant at the 1% level.

In the remainder of Table 9, we report two robustness checks to address alternative interpretations of the evidence. First, a potential concern is that the result is driven by

[10] We also estimate the differential effects of the participation rates of high and low information colleagues only on the subsample of low information employees. We find results very much in line with the estimates of the level effects of the participation rates reported in Table 8.

[11] We define this indicator variable using the full set of employees in the office and not just the random half of the sample we use to measure average peer decisions.

very small offices. If the number of high information employees in an office is positively correlated with size, then the result could be driven by measurement error in the group average that attenuates the estimated peer effect (measurement error will be most severe in the smallest offices). To address this concern, we repeat the specifications from Columns 1 to 3, but exclude offices in the bottom ten percent of the size distribution. For symmetry, we also exclude the top decile of the distribution. We report the results in Columns 4 to 6. We continue to find that the presence of high information employees in the office increases the activity in the peer network of low information colleagues. Second, a potential concern is that the employees we identify as having "high information" are concentrated at or near headquarters where there is generally more information available to all employees. To address this concern, we repeat the specifications from Columns 1 to 3, but exclude firm-locations that are located in the state in which the firm has its headquarters. We report the results in Columns 7 to 9 and they are again similar to the baseline specifications.

Our evidence suggests that high information employees provide information on smart investment practices within the firm's ESPP to colleagues who then in turn spread the information to other employees through the peer network. If this mechanism is an important determinant of employees' investment decisions, then ESPP participation rates should be higher on firm-location-dates in which we observe high information employees, given that nonparticipation is an investment mistake. In Table 10, we report mean participation rates in firms' ESPPs across firm-location-dates, broken out based on the presence of a high information employee. We again consider all three of our measures of information. In all cases, we find higher participation rates among employees from offices in which there is a high information employee. Moreover, the minimum difference is roughly 25%, suggesting that the peer learning channel is indeed economically meaningful.

3.5. Differences in Peer Influence by Proxies for Intensity of Interaction

As a final step, we consider an additional robustness test to confirm that our estimates of peer effects are indeed likely to capture endogenous peer influence versus contextual factors. Specifically, we identify a source of variation in the likelihood that any two employees in a firm-location meaningfully interact. We then test whether the estimated peer effects are stronger among the subset of firm-locations in which we expect there to be a higher intensity of employee interaction.

Specifically, we consider differences in the neighborhoods in which workers are located. Workers who live and work in areas that are more densely populated may be more likely to have contact with each other and, therefore, to discuss or observe each other's decisions within the firm's ESPP. If so, then we should observe stronger estimates of peer influence among such employees. We retrieve data on population density at the CBSA-level for the years 2000 and 2010 from the U.S. Census Bureau.[12] There is substantial variation across metro areas in population density. A standard deviation of the 2000 sample is 1,709, measured in units of people per square mile. We define a set of indicator variables for quartiles of the distribution of population density within our sample. We then test whether the influence of peers on ESPP participation and trading choices differs depending on the density of the location in which the workers live by including the density indicators and their interaction with $\bar{y}_{flt}$ in Equation (1).[13] We report the results in Table 11 using population density as measured in the 2000 Census (the results using the 2010 Census are nearly identical). We generally find that peers exert a stronger influence on an individual worker's decision either to participate in the firm's ESPP or to sell acquired shares within two weeks of purchase conditional on participating (our standard trading dependent variable) among workers in CBSAs that have a greater population density. In Column 1, we find a monotonic increase in the coefficient estimates on the interaction terms of the population density indicators with average participation in the firm-location-month as population density increases. The estimate on the interaction of mean participation with the indicator for the highest quartile of CBSA population density is positive and statistically significant at the 1% level. Economically, a ten percentage point increase in average participation would increase an individual worker's likelihood of participation by roughly half a percentage point more if the worker's CBSA is in the fourth quartile by population density compared to the first quartile. In Column 2, we report the parallel estimates for trading decisions

[12] Data available from http://www.census.gov/population/metro/data/pop_data.html in the Chapter 3 spread-sheet. We use the population-weighted series (where the weights apply to Census tracts within each CBSA), though our results are robust to using the unweighted series.

[13] The level effects of the density indicators cannot be identified because they are collinear with the CBSA-month fixed effects.

conditional on participation in the firm's ESPP. Here, we do not find a monotonic pattern across the interactions of density quartiles with the fraction of participants in the firm-location-month who sell within two weeks of share grants. Workers in CBSAs in the second, third, and fourth quartiles all exhibit stronger sensitivity to peer trading than workers in CBSAs in the bottom quartile. The effects in the second and fourth quartiles are strongest statistically. Overall, our results suggest that increased contact between peers increases the degree to which their investment decisions affect each other. Though it is a clear prediction of the endogenous peer effects mechanism, this finding less obviously follows from the presence of contextual peer effects.

4. Conclusion

We use unique data from a company that administers employer-sponsored ownership plans in several hundred public, U.S. firms to study how individuals are influenced by the financial decisions of their peers. We focus on decisions inside ESPP plans because their features are particularly conducive to identifying peer effects. Generally all employees within a firm are eligible to participate in the plan and they are all simultaneously making the decision to buy (or sell) the same security (company stock) given the same prices.

We find evidence of significant diffusion of investment practices through employee networks. Comparing only workers at the same firm in the same election window and correcting for a general metro area effect, we find that average participation rates in a firm-location positively predict the likelihood an employee participates in the ESPP. Similarly, trading behavior is affected by the trading behavior of peers. We focus on one specific trading behavior that is common in ESPPs through most of our analysis: the decision to sell the shares purchased in a particular grant window within two weeks of the firm-wide grant date. We find that the fraction of local coworkers in the firm who exhibit this behavior also positively predicts the likelihood an employee herself chooses to make an early sale. In addition to our baseline fixed-effects identification strategy, we confirm the results using general demographic patterns as a source of exogenous variation. For example, women tend to be less likely to sell ESPPs. Thus, we use lower selling rates in a firm-location that are predicted solely by the presence of more women in the office to identify the effects of average selling decisions on peers. We also find differences in the extent to which individuals are susceptible to peer influence. Men, younger employees, and employees in the middle of the income distribution appear to be most prone to mirror the investment decisions of peers.

Our analysis centers on investment choices that are likely to improve employees' welfare, particularly the decisions to participate in their firms' ESPPs. Thus, the spread of this behavior among peers suggests that employee networks can be a mechanism for social learning. To test this channel more directly, we identify three sets of "high information" employees: employees in occupations that are likely to correlate with investment experience or expertise, employees who directly report good prior investment experience, and employees in the highest reported income bin. We find that the choices of such individuals exert a direct influence on their low information peers. Moreover, the choices of "low information" employees have a significantly weaker effect on the choices of their low information peers when there are no high information colleagues in their offices. Thus, the importance of peer networks is mitigated when there is less likely to be value-improving information to transmit through them.

Overall, our results can help us to understand how financial behavior disseminates through the population. This is particularly important given the abundance of evidence that individuals sometimes make suboptimal financial choices. Peer networks could form conduits through which bad behavior spreads through mimicry. However, they also hold promise as a mechanism to spread better decision-making to low information investors at lower cost than direct education. Providing information to key individuals within a network has strong positive externalities on the behavior of other investors. In our context, for example, educating a small number of influential employees on the benefits of ESPP participation could have an outsized impact on the rate at which workers in the firm participate in the ESPP. Since participation in ESPPs is nearly a guaranteed win for the worker, this is likely to improve overall employee satisfaction with the plan and to contribute to the success of the plan in improving employee morale, loyalty, and, ultimately, productivity.

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Table 1. Summary Statistics

Panel A. ESPP variables

	N	mean	SD	min	max
Participate	472,608	0.43	0.495	0	1
Age	472,608	40.45	9.428	18	108
Female	472,608	0.282	0.45	0	1
Option Holdings	472,608	4.185	4.433	0	17.53
Zero Option Holdings	472,608	0.507	0.5	0	1
Restricted Stock Holdings	472,608	1.011	2.231	0	13.51
Zero Restricted Stock Holdings	472,608	0.813	0.39	0	1
Income_25K	472,608	0.0249		0	1
Income_25K50K	472,608	0.0791		0	1
Income_50K100K	472,608	0.324		0	1
Income_100K200K	472,608	0.402		0	1
Income_200K	472,608	0.17		0	1

Panel B. Distribution of Days to First Trade (N = 334,192)

Percentile	Value
5	1
10	3
15	5
20	12
25	27
30	57
35	104
40	182
45	265
50	373
55	461
60	568
65	693
70	828
75	1004
80	1230
85	1550
90	1993
95	2678

Table 2. ESPP Participation

The full sample consists of one observation per biannual ESPP window for each eligible employee in a sample firm. The analysis sample is a subsample constructed by randomly choosing half of the observations in each firm-location-window. The dependent variable is an indicator variable that takes the value of one if the employee participated in the firm's ESPP. The count of observations is rounded to the nearest 1,000. Standard errors are clustered at the firm-month level. ***, **, * indicate statisticial significance as the 1%, 5%, and 10% level, respectively.

	(1)		(2)		(3)	
	Full Sample		Full Sample		Analysis Sample	
Age_3035	0.0136	***	0.0172	***	0.012	***
	(0.004)		(0.003)		(0.004)	
Age_3540	-0.0034		0.0133	***	0.0126	***
	(0.005)		(0.003)		(0.004)	
Age_4045	-0.0289	***	0.0035		0.0024	
	(0.006)		(0.003)		(0.004)	
Age_4550	-0.0452	***	-0.0012		-0.0008	
	(0.007)		(0.003)		(0.004)	
Age_5055	-0.0673	***	-0.0135	***	-0.014	***
	(0.008)		(0.004)		(0.005)	
Age_5560	-0.1037	***	-0.0398	***	-0.0372	***
	(0.009)		(0.004)		(0.006)	
Age_60	-0.1573	***	-0.0814	***	-0.0831	***
	(0.009)		(0.006)		(0.008)	
Female	-0.0185	***	-0.0238	***	-0.021	***
	(0.003)		(0.002)		(0.003)	
Option Holdings	0.0377	***	0.0238	***	0.0246	***
	(0.003)		(0.002)		(0.003)	
Restricted Stock Holdings	0.0126	***	0.0055	**	0.0057	**
	(0.004)		(0.002)		(0.003)	
Zero Option Holdings	0.2472	***	0.1251	***	0.1334	***
	(0.025)		(0.019)		(0.020)	
Zero Restricted Stock Holdings	-0.1564	***	-0.1563	***	-0.1582	***
	(0.022)		(0.012)		(0.014)	
Income_25K50K	-0.0225	**	0.0011		0.0047	
	(0.010)		(0.006)		(0.008)	
Income_50K100K	0.0347	***	0.0319	***	0.0374	***
	(0.010)		(0.006)		(0.008)	
Income_100K200K	0.0287	***	0.0217	***	0.0276	***
	(0.011)		(0.006)		(0.008)	
Income_200K	-0.0666	***	-0.0265	***	-0.0217	**
	(0.013)		(0.008)		(0.010)	
CBSA-Month Fixed Effect	No		Yes		Yes	
Firm-Month Fixed Effect	No		Yes		Yes	
R-Squared	0.052		0.278		0.309	
Observations	473,000		473,000		237,000	

Table 3. ESPP Participation: IV Regressions

The sample is the Analysis Sample (See Table 2). The dependent variable is an indicator variable that takes the value of one if the employee participated in the firm's ESPP. The reported F-statistic is the Cragg Donald F-statistic to test the joint significance of the instruments. The count of observations is rounded to the nearest 1,000. Standard errors are clustered at the firm-month level. ***, **, * indicate statisticial significance as the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	First Stage	Second Stage	First Stage	Second Stage	First Stage	Second Stage
Age_3035 (IV)	0.0127 ***	0.0726 ***	0.0129 ***	0.0654 ***	0.0132 ***	0.0576 ***	0.0127 ***
	(0.004)	(0.015)	(0.004)	(0.016)	(0.004)	(0.016)	(0.004)
Age_3540 (IV)	0.0125 ***	0.0286 **	0.0125 ***	0.0277 *	0.0133 ***	0.017	0.0133 ***
	(0.004)	(0.014)	(0.004)	(0.015)	(0.004)	(0.016)	(0.004)
Age_4045 (IV)	0.002	0.0599 ***	0.0022	0.0487 ***	0.0033	0.0374 **	0.0034
	(0.005)	(0.015)	(0.005)	(0.016)	(0.005)	(0.016)	(0.005)
Age_4550 (IV)	-0.0023	0.0354 **	-0.0021	0.0274	-0.0014	0.0143	-0.0015
	(0.005)	(0.016)	(0.005)	(0.017)	(0.005)	(0.018)	(0.005)
Age_5055 (IV)	-0.0142 ***	0.0352 *	-0.0141 ***	0.0357 *	-0.0124 **	0.0249	-0.0123 **
	(0.005)	(0.018)	(0.005)	(0.019)	(0.005)	(0.020)	(0.005)
Age_5560 (IV)	-0.0371 ***	-0.0177	-0.0368 ***	-0.027	-0.0363 ***	-0.07 ***	-0.0367 ***
	(0.006)	(0.022)	(0.006)	(0.023)	(0.006)	(0.024)	(0.006)
Age_60 (IV)	-0.0749 ***	-0.0872 ***	-0.073 ***	-0.0898 ***	-0.0721 ***	-0.1067 ***	-0.0719 ***
	(0.009)	(0.028)	(0.009)	(0.029)	(0.009)	(0.031)	(0.009)
Female (IV)	-0.0208 ***		-0.0209 ***	-0.207 **	-0.0212 ***	-0.0212 **	-0.0208 ***
	(0.003)		(0.003)	(0.009)	(0.003)	(0.009)	(0.003)
Option Holdings	0.0255 ***		0.0253 ***		0.0253 ***		0.0253 ***
	(0.003)		(0.003)		(0.003)		(0.003)
Restricted Stock Holdings	0.0067 **		0.0067 **		0.007 **		0.0069 **
	(0.003)		(0.003)		(0.003)		(0.003)
Zero Option Holdings	0.1424 ***		0.1405 ***		0.1406 ***		0.1403 ***
	(0.021)		(0.021)		(0.022)		(0.022)
Zero Restricted Stock Holdings	-0.1559 ***		-0.1559 ***		-0.1547 ***		-0.1558 ***
	(0.014)		(0.014)		(0.014)		(0.014)
Income_25K50K (IV)	0.0077		0.0081		0.008	-0.1048 ***	0.0086
	(0.008)		(0.008)		(0.008)	(0.009)	(0.008)
Income_50K100K (IV)	0.0437 ***		0.0437 ***		0.0438 ***	-0.0314	0.0448 ***
	(0.008)		(0.008)		(0.008)	(0.026)	(0.008)
Income_100K200K (IV)	0.0316 ***		0.0318 ***		0.0321 ***	-0.0327	0.0329 ***
	(0.008)		(0.008)		(0.008)	(0.025)	(0.008)
Income_200K (IV)	-0.0171 *		-0.0173 *		-0.017 *	-0.0505 *	-0.0157
	(0.010)		(0.010)		(0.010)	(0.027)	(0.010)
Mean_Participate	0.142 ***		0.2507 **		0.2519 **		0.2706 ***
	(0.009)		(0.112)		(0.120)		(0.094)
Second Stage Controls	Yes	Yes		Yes		Yes	
CBSA-Month Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Month Fixed Effect		Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.288	0.816	0.287	0.83	0.287	0.839	0.286
Observations	206,000	205,000	205,000	202,000	202,000	200,000	200,000
F-Statistic			126.083		106.024		124.698

Table 4. ESPP Sell Decisions

The sample consists of one observation per biannual ESPP window from the Full or Analysis Samples (see Table 2) in which an employee chose to participate. The dependent variable is an indicator variable that takes the value of one if the employee sold the shares acquired during the window within two weeks of purchase. The reported F-statistic is the Cragg Donald F-statistic to test the joint significance of the instruments. The count of observations is rounded to the nearest 1,000. Standard errors are clustered at the firm-month level. ***, **, * indicate statisticial significance as the 1%, 5%, and 10% level, respectively.

	(1)		(2)		(3)		(4)	
	Full Sample		Analysis Sample		First Stage		Second Stage	
Age_3035 (IV)	0.0126	***	0.0174	***	0.0063		0.0171	***
	(0.004)		(0.005)		(0.025)		(0.005)	
Age_3540 (IV)	0.0185	***	0.0199	***	0.0081		0.0201	***
	(0.005)		(0.006)		(0.024)		(0.006)	
Age_4045 (IV)	-0.0140	**	-0.0037		-0.0045		-0.0035	
	(0.006)		(0.006)		(0.025)		(0.006)	
Age_4550 (IV)	-0.0270	***	-0.0194	***	0.0116		-0.0188	***
	(0.007)		(0.007)		(0.025)		(0.007)	
Age_5055 (IV)	-0.0381	***	-0.0262	***	0.0536	*	-0.0258	***
	(0.008)		(0.008)		(0.031)		(0.008)	
Age_5560 (IV)	-0.0555	***	-0.0501	***	-0.0283		-0.0500	***
	(0.008)		(0.009)		(0.034)		(0.009)	
Age_60 (IV)	-0.0812	***	-0.0709	***	-0.1392	***	-0.0698	***
	(0.009)		(0.011)		(0.047)		(0.011)	
Female (IV)	-0.0681	***	-0.0565	***	-0.0371	***	-0.0561	***
	(0.003)		(0.003)		(0.012)		(0.003)	
Option Holdings	0.0136	***	-0.0112	***			-0.0109	***
	(0.002)		(0.002)				(0.002)	
Restricted Stock Holdings	-0.015	***	-0.0239	***			-0.0239	***
	(0.003)		(0.002)				(0.002)	
Zero Option Holdings	0.1550	***	-0.1155	***			-0.1144	***
	(0.023)		(0.017)				(0.017)	
Zero Restricted Stock Holdings	0.0021		0.0518	***			0.0514	***
	(0.013)		(0.013)				(0.013)	
Income_25K50K (IV)	0.0297	***	0.0062		0.1245	***	0.0056	
	(0.010)		(0.011)		(0.040)		(0.011)	
Income_50K100K (IV)	0.0319	***	0.0084		0.1117	***	0.0078	
	(0.010)		(0.009)		0.036		(0.009)	
Income_100K200K (IV)	0.0426	***	0.0218	**	0.1001	***	0.0213	**
	(0.010)		(0.009)		(0.036)		(0.009)	
Income_200K (IV)	-0.0122		0.0198	*	0.0464		0.0196	*
	(0.011)		(0.010)		(0.038)		(0.010)	
Mean_Sell_2Weeks			0.0731	***			0.2562	*
			(0.014)				(0.147)	
Second Stage Controls					Yes			
CBSA-Month Fixed Effect	No		Yes		Yes		Yes	
Firm-Month Fixed Effect	No		Yes		Yes		Yes	
R-Squared	0.024		0.248		0.816		0.245	
Observations	201,000		85,000		84,000		84,000	
F-statistic							53.861	

Table 5. ESPP Sell Decisions: Different Horizons

The sample consists of one observation per biannual ESPP window from the Analysis Sample (see Table 2) in which an employee chose to participate. The dependent variable is an indicator variable that takes the value of one if the employee sold the shares acquired during the window within the time frame in the Column header. The count of observations is rounded to the nearest 1,000. Standard errors are clustered at the firm-month level. ***, **, * indicate statistiical significance as the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	1 Week	1 Month	2 Months	3 Months	1 Year	Continuous
Age_3035 (IV)	0.013 ***	0.019 ***	0.028 ***	0.028 ***	0.040 ***	(0.126) ***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.032)
Age_3540 (IV)	0.014 ***	0.024 ***	0.031 ***	0.033 ***	0.049 ***	(0.163) ***
	(0.005)	(0.006)	(0.006)	(0.007)	(0.007)	(0.037)
Age_4045 (IV)	(0.007)	0.001	0.006	0.008	0.023 ***	(0.004)
	(0.006)	(0.006)	(0.007)	(0.007)	(0.008)	(0.041)
Age_4550 (IV)	(0.017) ***	0.018 **	(0.013)	(0.012)	0.008	0.104
	(0.006)	(0.008)	(0.008)	(0.009)	(0.009)	(0.045)
Age_5055 (IV)	(0.022) ***	(0.027) ***	(0.029) ***	(0.032) ***	(0.029) ***	0.098 *
	(0.007)	(0.008)	(0.008)	(0.009)	(0.009)	(0.050)
Age_5560 (IV)	(0.043) ***	(0.058) ***	(0.069) ***	(0.075) ***	(0.086) ***	0.324 ***
	(0.008)	(0.009)	(0.009)	(0.010)	(0.011)	(0.063)
Age_60 (IV)	0.058 ***	(0.080) ***	(0.078) ***	(0.087) ***	(0.093) ***	0.450 ***
	(0.010)	(0.011)	(0.012)	(0.013)	(0.015)	(0.090)
Female (IV)	0.049 ***	(0.063) ***	0.069 ***	(0.074) ***	(0.083) ***	0.368 ***
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.021)
Option Holdings	(0.009) ***	(0.013) ***	(0.016) ***	(0.181) ***	(0.027) ***	0.099 ***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.014)
Restricted Stock Holdings	(0.019) ***	(0.030) ***	(0.035) ***	(0.039) ***	(0.053) ***	0.156 ***
	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)	(0.015)
Zero Option Holdings	0.090 ***	(0.134) ***	(0.154) ***	(0.168) ***	(0.231) ***	0.930 ***
	(0.015)	(0.018)	(0.020)	(0.021)	(0.024)	(0.121)
Zero Restricted Stock Holding	0.053 ***	0.039 ***	0.027 *	0.011	(0.073) ***	(0.128)
	(0.012)	(0.015)	(0.016)	(0.017)	(0.022)	(0.081)
Income_25K50K (IV)	0.004	0.009	0.011	0.005	0.016	(0.024)
	(0.010)	(0.012)	(0.012)	(0.012)	(0.014)	(0.076)
Income_50K100K (IV)	0.011	0.012	0.013	0.007	0.015	(0.051)
	(0.009)	(0.010)	(0.010)	(0.010)	(0.012)	(0.066)
Income_100K200K (IV)	0.022 **	0.026 ***	0.033 ***	0.029 ***	0.038 ***	(0.154) **
	(0.009)	(0.010)	(0.011)	(0.011)	(0.012)	(0.069)
Income_200K (IV)	0.016 *	0.026 **	0.032 ***	0.031 ***	0.055 ***	(0.165) **
	(0.009)	(0.011)	(0.012)	(0.012)	(0.013)	(0.075)
Mean_Sell_2Weeks	0.086 ***					
	(0.015)					
Mean_Sell_1Month		0.078 ***				
		(0.014)				
Mean_Sell_2Months			0.075 ***			
			(0.014)			
Mean_Sell_3Months				0.063 ***		
				(0.014)		
Mean_Sell_1Year					0.074 ***	
					(0.013)	
ln(Days to First Sale)						0.118 ***
						(0.017)
CBSA-Month Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Month Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Observations	85,000	85,000	85,000	85,000	85,000	59,000
R-squared	0.239	0.259	0.269	0.275	0.294	0.371

Table 6. Cross-sectional Differences in Peer Effects by Employee Characteristics

"Choice" indicates the ESPP decision that is considered in the Column. As indicated in the Column headers, Columns 1, 3, and 5 analyze the employees decision to participate in an ESPP (and so mean_choice is the participation rate in the worker's firm-location-month in the excluded random sample) on the Analysis Sample (See Table 2). Columns 2, 4, and 6 analyze the employee's decision to sell shares purchased within an ESPP conditional on participating (and so mean_choice is the rate at which participators sell their shares within two weeks of purchase in the worker's firm-location-month in the excluded random sample). Standard controls are the controls from Tables 3 and 4. The count of observations is rounded to the nearest 1,000. Standard errors are clustered at the firm-month level. ***, **, * indicate statisticial significance as the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)
	Participate	Sell 2 Weeks	Participate	Sell 2 Weeks
Mean_choice	0.1515 ***	0.0979 ***	0.1861 ***	0.1436 ***
	(0.009)	(0.015)	(0.013)	(0.013)
Female * Mean_choice	-0.0382 ***	-0.1108 ***		
	(0.008)	(0.018)		
Age_3035 * Mean_choice			-0.0096	
			(0.012)	
Age_3540 * Mean_choice			-0.0125	
			(0.013)	
Age_4045 * Mean_choice			-0.0466 ***	
			(0.014)	
Age_4550 * Mean_choice			-0.0696 ***	
			(0.015)	
Age_5055 * Mean_choice			-0.0797 ***	
			(0.016)	
Age_5560 * Mean_choice			-0.1305 ***	
			(0.018)	
Age_gt60 * Mean_choice			-0.145 ***	
			(0.027)	
Age_3050 * Mean_choice				-0.0575 **
				(0.027)
Age_50 * Mean_choice				-0.2082 ***
				(0.034)
Income_25K50K * Mean_choice				
Income_50K100K * Mean_choice				
Income_100K200K * Mean_choice				
Income_200K * Mean_choice				
Standard Controls	Yes	Yes	Yes	Yes
CBSA-Month Fixed Effect	Yes	Yes	Yes	Yes
Firm-Month Fixed Effect	Yes	Yes	Yes	Yes
R-Squared	0.288	0.249	0.273	0.249
Observations	206,000	85,000	206,000	85,000

Table 7.
Panel A. Correlations Across Measures of Informativeness

	HO	HI
HI	-0.0065 ***	
HE	0.0364 ***	0.0043 ***

Panel B. Employee Characteristics by Measure of Informativeness

	High Information Occupation		High Investment Experience		High Income	
	Yes	No	Yes	No	Yes	No
Female	31.36%	28.14%	11.34%	28.63%	25.04%	30.59%
Age	42.61	47.58	48.55	47.08	50.06	44.13
RS hold	228.3	208.4	404.7	212.3	673.8	144
Opt hold	18,233	23,096	31,456	22,777	72,912	10,948

Panel C. Mean ESPP Participation Rate by Occupation, Investment Experience and Income

	ESPP	count
Occupation		
Accounting	43.9%	22,790
Engineer	51.9%	58,890
Finance	46.2%	7,438
HR	41.9%	12,978
Administration	36.0%	14,125
Consulting	35.4%	13,173
health care	37.5%	13,095
IT	47.3%	130,123
Marketing	43.5%	18,333
Investment Experience		
Unknown	47.2%	839,135
None	53.6%	30,428
Limited	60.0%	10,988
Good	60.8%	9,851
Excellent	61.3%	3,569
Household Income Category		
< $15,000	45.30%	17,005
$15,000-25,000	30.60%	8,812
$25,000-50,000	33.30%	61,588
$50,000-100,000	44.70%	247,690
$100,000-200,000	46.70%	262,279
> $200,000	37.60%	83,243

Table 8. Cross-sectional Differences in Peer Effects by Informativeness

The sample is the Analysis Sample (See Table 2). The dependent variable is an indicator variable that takes the value of one if the employee participated in the firm's ESPP. High information is defined as belonging to a high information occupation in Column 1, to reporting "good" or "excellent" investment experience in Column 2, or to recieving income above $200,000 in Column 3. The count of observations is rounded to the nearest 1,000. Standard errors are clustered at the firm-month level. ***, **, * indicate statisticial significance as the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)
Mean Participation Rate of High Information Influencers	0.049 ***	0.0481	0.0241 **
	(0.014)	(0.036)	(0.012)
Mean Participation Rate of Low Information Influencers	0.0993 ***	0.3067 ***	0.1588 ***
	(0.023)	(0.086)	(0.020)
Responder is High Information	0.0242 ***	0.0209	-0.0199 **
	(0.006)	(0.028)	(0.008)
Mean Participation Rate of High Information Influencers * Responder is High Information	0.0348 **	0.1884 ***	0.0484 ***
	(0.015)	(0.051)	(0.016)
Mean Participation Rate of Low Information Influencers * Responder is High Information	-0.0241	0.0759	-0.1042 ***
	(0.017)	(0.072)	(0.018)
Standard Controls	Yes	Yes	Yes
CBSA-Month Fixed Effect	Yes	Yes	Yes
Firm-Month Fixed Effect	Yes	Yes	Yes
R-Squared	0.296	0.228	0.278
Observations	105,000	36,000	119,000

Table 9. Cross-sectional Differences in Peer Effects by Presence of Informed Peer

The sample drops non-informed peers. Columns 4-6 also drop firm-locations where total employment is below the 10th or above the 90th percentiles. Columns 7-9 drop all firm-lcoations in the same state as the firm's headquarters. The dependent variable is an indicator variable that takes the value of one if the employee participated in the firm's ESPP. High information is defined as belonging to a high information occupation in Columns 1, 4 and 7, to reporting "good" or "excellent" investment experience in Columns 2, 5, and 8, or to recieving income above $200,000 in Columns 3, 6, and 9. The count of observations is rounded to the nearest 1,000. Standard errors are clustered at the firm-month level. ***, **, * indicate statisticial significance as the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Mean Participation Rate of Low Information Influencers	0.1238 ***	0.2423 ***	0.1511 ***	0.1305 ***	0.2616 ***	0.1546 ***	0.1076 ***	0.2497 ***	0.1241 ***
	(0.015)	(0.021)	(0.014)	(0.016)	(0.022)	(0.014)	(0.021)	(0.034)	(0.019)
No High Information Types at Firm-Location-Month	0.0193 **	0.0492 ***	0.0218 **	0.0178 *	0.0447 ***	0.0152 *	0.0288 **	0.0488 **	0.024 *
	(0.009)	(0.012)	(0.009)	(0.010)	(0.013)	(0.009)	(0.013)	(0.020)	(0.013)
Mean Participation Rate of Low Information Influencers * No High Information Types at Firm-Location-Month	-0.0565 ***	-0.1145 ***	-0.0383 **	-0.0502 ***	-0.1167 ***	-0.0297 *	-0.0719 ***	-0.1451 ***	-0.028
	(0.017)	(0.020)	(0.015)	(0.018)	(0.021)	(0.016)	(0.023)	(0.033)	(0.021)
Standard Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CBSA-Month Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Month Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.316	0.296	0.305	0.317	0.296	0.307	0.367	0.343	0.359
Observations	108,000	169,000	145,000	96,000	150,000	128,000	46,000	71,000	59,000

Table 10. Mean ESPP Rate of Participation By Presence of High Information Peer in a Firm-Location-Month

	mean ESPP	Count
HO at location	46.70%	860,711
no HO at location	36.30%	239,765
HE at location	50.60%	453,027
no HE at location	39.20%	760,463
HI at location	45.40%	873,651
no HI at location	38.80%	281,005

Table 11. Peer Effects by CBSA Population Density

The sample is the Analysis Sample (See Table 2) in Column 1 and the subsample of ESPP participants from the Analysis Sample in Columns 2. The dependent variable is indicated in the column header. Density_Quartile`x' is an indicator variable that takes the value of 1 if the population-weighted population density of the worker's CBSA as measured in 2000 is in the `x'th quartile of the distribution. Standard controls are the controls from Tables 3 and 4. The count of observations is rounded to the nearest 1,000. Standard errors are clustered at the firm-month level. ***, **, * indicate statisticial significance as the 1%, 5%, and 10% level, respectively.

	(1)	(2)
	Participate	Sell 2 Weeks
Mean_Participate	0.128 ***	
	(0.013)	
Mean_Participate*Density_Quartile2	0.000	
	(0.014)	
Mean_Participate*Density_Quartile3	0.021	
	(0.016)	
Mean_Participate*Density_Quartile4	0.048 ***	
	(0.015)	
Mean_Sell_2Weeks		0.019
		(0.028)
Mean_Sell_2Weeks*Density_Quartile2		0.086 **
		(0.035)
Mean_Sell_2Weeks*Density_Quartile3		0.035
		(0.040)
Mean_Sell_2Weeks*Density_Quartile4		0.061 *
		(0.034)
Standard Controls	Yes	Yes
CBSA-Month Fixed Effect	Yes	Yes
Firm-Month Fixed Effect	Yes	Yes
R-Squared	0.285	0.247
Observations	205,000	85,000