Investor Behavior and the Benefits of Direct Stock Ownership[*†]

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[†] Our report was originally titled, "Investor Behavior and the Benefits of Dispersed Stock Ownership." We adjusted the title because our analyses examine the benefits of direct individual ownership and not ownership dispersion *per se*.

Investor Behavior and the Benefits of Direct Stock Ownership

ABSTRACT

Using an experiment to rule out reverse causality, we examine whether a small investment in a company's stock leads investors to purchase more of the company's products and adopt other views and preferences that benefit the company. We pre-register our research methods, hypotheses, and supplemental analyses via the *Journal of Accounting Research*'s registration based editorial process. We find little evidence consistent with these hypotheses for the average investor in our sample using our planned univariate hypothesis tests, and planned Bayesian parameter estimation shows substantial downward belief revision for more optimistic *ex ante* expectations of the treatment effects. In planned supplemental analyses, however, we do find that the effects of ownership on product purchase behavior and on regulatory preferences are intuitively stronger for certain subgroups of investors—namely, for investors who are most likely to purchase the types of products offered by the company and for investors who are most likely to vote on political matters. The results contribute to our understanding of the benefits of direct stock ownership and are informative to public company managers and directors.

Keywords: Direct Stock Ownership; Investor Behavior; Bayesian Analysis; Registered Report

JEL Codes: M41; G32; G40

1. Introduction

Theory and empirical work consider a variety of benefits of stock ownership by financial intermediaries such as pension funds and banks for dimensions of firms' operating performance (e.g., Almazan, Hartzell, and Starks [2005], Aghion, Van Reenen, and Zingales [2013], Chen, Harford, and Li [2007]). In contrast, relatively few papers examine the benefits of direct, individual ownership on operating performance. In this study, which was pre-registered via the *Journal of Accounting Research*'s registration based editorial process, we predict that even small amounts of stock ownership can change individual investors' behaviors in ways that benefit the firm. In particular, we predict that stock ownership leads investors to purchase more of the firm's products and adopt other views and preferences that benefit the firm. We further expect these effects are not limited to the investors themselves; i.e., we expect investors' altered behavior to spread within social networks to influence the behavior of friends, family, and colleagues. Although our primary focus is on product and regulatory preferences that could positively affect the firm's operating performance, we also provide evidence on other potential capital market benefits associated with direct stock ownership—e.g., the effects on investors' earnings expectations and assessments of financial reporting and earnings quality.

A key challenge to identify the effect of stock ownership on individual investor behaviors is to rule out reverse causality. For example, the few prior papers that examine the effect of stock ownership on product preferences (including the propensity for repeat patronage and other brand-loyal behaviors) rely on survey evidence from Nordic countries (Aspara [2009], Aspara, Nyman, and Tikkanen [2008]). Although these papers find an association between stock ownership and current product preferences or future purchase intentions, prior literature also provides consistent evidence that investors follow the popular investment mantra to "buy what

you know" (Aspara and Tikkanen [2011], Frieder and Subrahmanyam [2005], Keloharju, Knüpfer, and Linnainmaa [2012], MacGregor, Slovic, Dreman, and Berry [2000], Schoenbachler, Gordon, and Aurand [2004]), which suggests that purchase behavior and beliefs about a company also influence the choice to invest. In contrast to prior archival and survey evidence, we address the reverse causality confound by generating data using an experiment in which investors are randomly assigned stock ownership. After several months, we collect data on actual purchase behavior and on a number of investors' other views and preferences.[1]

Graduate business students enrolled in introductory financial accounting classes at three universities form our sample of individual investors. We conduct the experiment at multiple universities to obtain a large sample of participants and to address certain alternative explanations for the results, as discussed further below. At the beginning of the academic term, students are told that if they opt in they will be given a $20 investment in a publicly traded company, and in exchange for their participation they will receive the market value of their stock as of a pre-specified date in the following academic term. Upon opting into the study, participants are randomly assigned ownership in either Starbucks Corporation or one of three control companies. Participants learn the names of all companies included in the experiment to ensure any unintentional endorsement of each company is constant across conditions.[2] Figure 1 illustrates the timing of the experiment.

< FIGURE 1 >

[1] Prior papers that study the association between stock ownership and product preferences typically do not collect data on actual purchases. One exception to this is Keloharju et al. (2012), who examine evidence from Finland that gifts or inheritances of stock in brokerage companies are associated with subsequent patronage of those companies. The implicit assumption of these tests is that inheritances and gifts of stock are not associated with the recipient's *ex ante* product preferences or purchase intentions and are not viewed as an implicit recommendation by the donor. They find mixed evidence: gifts, but not inheritances, have a statistically significant association with subsequent patronage.

[2] Altogether, participants receive an investment in one of four companies—Starbucks Corporation, Microsoft Corporation, Procter & Gamble Company, or 3M Company. Roughly half of the participants at each university receive an investment in Starbucks; all other participants receive an investment in one of the other three companies.

We collect data using an in-class poll and an online survey. The in-class poll occurs during the academic term and examines whether stock ownership differentially affects participants' stated preference for Starbucks products to be served at a business school event—i.e., participants' use of others' money to purchase Starbucks products. We distribute the online survey at the end of the academic term. Within this survey, participants indicate how likely it is they would vote for a possible regulatory action that would negatively affect Starbucks' financial performance and use credit and debit card statements and their Starbucks Rewards Account (if applicable) to provide information on their Starbucks product purchases since receiving the investment. Participants also answer a variety of questions throughout the survey to inform potential mechanisms through which stock ownership could affect product and regulatory preferences and to inform potential moderating variables.

A total of 269 participants provide data for the study. In our planned hypothesis tests, we do not find statistically significant evidence that, relative to the average control company investor, the average Starbucks investor in our sample spends more of her own money on Starbucks products, indicates more interest in having Starbucks products served at a business school event, or expresses regulatory views more closely aligned with Starbucks' financial interests. These null findings are not sensitive to a number of reasonable adjustments to the data or design of the analyses, and Bayesian parameter estimation shows substantial downward belief revision for more optimistic *ex ante* expectations of the average effects of stock ownership on the dependent variables. However, certain individuals are likely to be more susceptible to the hypothesized treatment effects than others. For instance, individuals who do not already buy the types of products sold at Starbucks are unlikely to change their purchase habits dramatically due to stock ownership. Similarly, individuals who are not engaged in political and regulatory

matters are unlikely to begin forming stronger opinions due to stock ownership. Consistent with these arguments, planned cross-sectional analyses show that the effects of Starbucks ownership on product purchases and on regulatory preferences are intuitively stronger for certain subgroups of participants. Specifically, we find that the effect of ownership on purchase behavior is greater for participants who report purchasing more coffee- and tea-related beverages, and that the effect of ownership on regulatory preferences is stronger for subgroups that are more likely to vote on US political and regulatory matters—namely, older participants and participants who are US Nationals. In other planned supplemental analysis, we find that relative to control company investors, Starbucks investors report experiencing greater discomfort voting for political measures that would undermine Starbucks' financial performance, suggesting that ownership has at least a marginal effect on cognitions about regulation.

Few of the remaining planned supplemental analyses yield statistically significant differences between Starbucks investors and control company investors. For example, we find no evidence that closer social connections to Starbucks investors affect individuals' own preferences and behavior. We also find no evidence that Starbucks investors have more positive earnings expectations or assessments of reporting or earnings quality for Starbucks, on average.

A unique feature of our empirical approach is that we can draw inferences from real-world purchase data without compromising the most critical benefit of experimentation—random assignment. Although random assignment is likely to rule out most confounds that would be present in a non-experimental setting (e.g., reverse causality), the out-of-laboratory setting does trade off some control over the experimental stimulus to improve the external validity of the findings. For instance, participants are subject to actual stock price changes of the invested-in companies as they happen, so investment returns are not equivalent across experimental

conditions. Further, although we do not explicitly tell participants that their investment positions delineate experimental conditions, participants are not blind to these conditions. Thus, we conduct several planned supplemental analyses to address the concern that the generally null findings are due to potential confounds rather than the absence of economically meaningful treatment effects. For instance, we examine the possibility that differences in discretionary income due to differences in stock returns could account for the similar preferences for Starbucks products across investor groups. We also examine the possibility that participants feel a sense of competition with classmates invested in other stocks, which would be difficult to generalize beyond the experimental setting. Finally, we examine the effect of potential selection biases, given that some (albeit very few) participants begin but fail to complete the online survey. None of our tests suggest that any of these issues meaningfully affect the results.

Our study contributes in several ways. Whereas prior work emphasizes the monitoring benefits of large financial intermediaries on company performance, we examine a variety of potential benefits of direct, individual ownership. This examination is important given that the benefits of direct ownership *in the aggregate* could improve firm performance, particularly to the extent investors influence others to adopt their preferences and behaviors. While the results of our registered analyses do not support the hypothesis that small amounts of stock ownership cause the *average* investor's purchase decisions and other views and preferences to become more favorable to the company, our planned supplemental analyses suggest that the treatment effects are stronger for investors who do not need to dramatically alter their behavior to benefit the company. For instance, the treatment effects are stronger when there is a better "match" between firm and owner—e.g., for owners of Starbucks who tend to spend relatively more money at coffee shops. These findings suggest that characteristics of the firm's individual investors are

important determinants of the magnitude of the benefits direct ownership provides the firm. More broadly, our findings build on prior evidence that increases in a company's ownership base are associated with stock price increases (e.g., Amihud, Mendelson, and Uno [1999], Lehavy and Sloan [2008]) and shed light on the potential motivations for stock splits, employee stock purchase programs, and other efforts to make shares more affordable to retail investors, particularly for consumer products companies.

By examining the effects of stock ownership on investors' behavior and preferences, our study also adds to the growing literature in accounting that examines the effects of investment position on individuals' judgments and decisions (e.g., Elliott, Rennekamp, and White [2016], Fanning, Agoglia, and Piercy [2015], Hales [2007], Seybert and Bloomfield [2009], Thayer [2011]). Using laboratory-based designs, these papers provide evidence that stock ownership can result in biased expectations for future earnings and risk, *ceteris paribus*. Results from our planned supplemental analyses complement this prior work. Specifically, because our participants hold stock for several months (instead of for several minutes), our results highlight the potential existence of variables that could moderate the effects previously documented in the laboratory.

2. Registered Hypothesis Development

Prior research suggests that individual investors do not base their investment decisions solely on expected returns and risk (e.g., Aspara and Tikkanen [2011, 2010], Fama and French [2007], Keloharju et al. [2012], MacGregor et al. [2000]). For example, investors buy the stock of companies they know and like and avoid buying the stock of companies with corporate values that conflict with their own self-concepts (e.g., people who see themselves as eco-friendly tend to avoid buying stock in oil companies). Whether the theoretical relations work in the opposite

direction—whether investment induces psychological biases or changes inputs to investors' utility such that they make different decisions that impact the company—remains an outstanding empirical question. In the following sections, we propose a number of reasons stock ownership could alter individuals' behavior in ways that benefit the invested-in company.

2.1 THE BELIEF-IN-INFLUENCE EXPLANATION

First, an investor might make decisions in an effort to influence the company's stock price (and, therefore, her investment value). It is not necessary for an individual's behavior to *actually* affect the company's stock price for this process to explain a change in behavior—it is only necessary that an individual believe an effect is possible. This potential explanation is predicated on individuals' tendency to believe they can influence outcomes they demonstrably cannot (Langer [1975]) and to exaggerate the likelihood of small probability events (e.g., Fox and Tversky [1998], Lichtenstein, Slovic, Fischhoff, Layman, and Combs [1978]). For example, Feddersen [2004] concludes that prior findings on rational choice theory in political science suggest that "voters participate because they hope to influence the ultimate outcome of the election," even when the probability that a single individual's vote can change the outcome of the election is extremely small.[3]

2.2 THE AFFECT EXPLANATION

Second, stock ownership could intensify an individual's positive feelings for a company, which can alter how she makes decisions that concern the company (e.g., whether to purchase a company's products (Li and Petrick [2008])). For instance, stock ownership likely causes an individual to identify more closely with the company (Tajfel and Turner [1986]). Because individuals are motivated to hold positive views of themselves and their associations, greater

[3] For example, the probability of a voter affecting the outcome of a close national election in the US is roughly one in ten million (Gelman, King, and Boscardin [1998]).

identification can breed additional company-specific affect (Greenwald and Banaji [1995]). In addition, assuming an investor gathers additional information about the invested-in company (e.g., via social media feeds or company disclosures), the investment should increase the investor's familiarity with the company. Increased familiarity should breed additional positive affect, *ceteris paribus* (Moreland and Zajonc [1982], Zajonc [1980]). Because individuals tend to rely on affective feelings when making important judgments and decisions (Slovic, Finucane, Peters, and MacGregor [2007]), it follows that stock ownership is likely to increase investors' company-specific affect, and, in turn, alter their behaviors that impact the company.[4]

2.3 THE COGNITIVE DISSONANCE EXPLANATION

Third, stock ownership could trigger feelings of cognitive dissonance when the investor considers taking actions that do not support the invested-in company. Festinger [1957] defines cognitive dissonance as the discomfort individuals feel when they hold multiple simultaneous cognitions or beliefs that are in conflict. For example, an individual experiences cognitive dissonance when she acts in a way that is inconsistent with her beliefs about how she *should* act. To the extent investors believe they should support the invested-in company, investors would experience cognitive dissonance when taking actions that do not support the company despite the existence of available alternatives that would (e.g., when buying coffee from a competitor instead of from Starbucks).

Investors can alter either their cognitions or their behaviors to reduce the dissonance they experience (Festinger [1957], Gilbert, Pinel, Wilson, Blumberg, and Wheatley [1998]). For

[4] We expect investment position to drive any observed difference in affect between treatment and control groups, which should, in turn, drive any observed differences in behavior across conditions. However, it is also possible that an investment position causes an investor to change her behaviors in ways that breed additional company-specific affect (e.g., the investor purchases more of the company's products, and the act of purchasing products increases affect for the company). Given the proximate cause for the change in affect would be the exogenous investment, we do not view this possibility as an alternative explanation for differences in our dependent measures between groups.

example, an investor could reduce dissonance by telling herself that she is only one person and her choices could not meaningfully impact a company's performance. Alternatively, the same investor could reduce dissonance by making an effort to avoid purchasing substitute products from the company's competitors. The results of prior research suggest that, for decisions viewed as reversible, individuals tend to favor behavioral changes over cognition changes (Gilbert and Ebert [2002]). We posit that, prior to making a decision that could affect the invested-in company, investors are likely to think about the cognitive dissonance they *would* feel should they choose an action that does not benefit that company—i.e., company-relevant decisions should be seen as reversible *ex ante*. Consequently, we expect investors to manage their behavior such that they avoid experiencing cognitive dissonance.[5]

2.4 REGISTERED HYPOTHESES

We expect any attempt to influence the value of an investment position, any heightened affect for the invested-in company, or any dynamic avoidance of cognitive dissonance to affect investors' behaviors in ways that benefit the company. Our primary hypotheses, both stated in alternative form:

H1: *A small investment in a company's stock causes investors to purchase more of the company's products.*

H2: *A small investment in a company's stock causes investors to hold regulatory views more closely aligned with the financial interests of the company.*

3. Registered Empirical Design

3.1 GENERAL PROCEDURES

Graduate business students enrolled in introductory financial accounting classes at three separate universities form our sample of individual investors. Relying on graduate business

[5] If, instead, investors reduce their feelings of cognitive dissonance by altering their beliefs, we would expect to observe no effect of investment on behavior via heightened anticipated feelings of cognitive dissonance.

students ensures that all participants at a given university have similar access to Starbucks locations and roughly comparable time constraints, characteristics that should reduce statistical noise in the analyses. Further, the experimental task is neither complex nor requires pre-existing finance or accounting knowledge, implying that the participants are a good match for the task (Elliott, Hodge, Kennedy, and Pronk [2007], Libby, Bloomfield, and Nelson [2002]).

Despite the additional complexity that comes with recruiting participants from multiple universities, we believe this element of the design is critical for two reasons. First, using multiple universities reduces some of the risk inherent in any non-laboratory experiment. For example, it could be that some unpredictable event disrupts our ability to collect data at one of the universities. While certain institutional characteristics differ across the universities, we do not believe this is a shortcoming of the design, as we standardize all non-indicator variables *within each university* before pooling the observations across universities. Second, relying on only one university would limit our sample size and therefore the statistical power of our tests.[6]

We collect data by conducting a between-participants experiment in which we randomly assign ownership in Starbucks or in one of three control companies.[7] We use Starbucks as the treatment company to maintain consistency in our measures and because Starbucks stores are ubiquitous and familiar. We use three control companies to minimize participants' sense of competition with their classmates. As discussed further below, we use 3M, Microsoft, and

[6] *Ex ante*, we use pilot data to estimate the sample size needed to detect a treatment effect of practical importance with reasonable power. For example, given α = 0.10 (one-tailed) and 1 – β = 0.80 (Tabachnick and Fidell [2007]), we require a sample size of approximately 90 participants to detect a treatment effect of $30 in Starbucks purchases. Soliciting participants from multiple universities increases the chances we obtain this minimum sample size.

[7] For the two universities that utilize student workgroups, we randomly assign stock ownership based on students' membership in these groups, which are constructed by the university's graduate business program office before classes begin. These workgroups comprise 5-6 students each and constitute the teams for group work throughout the academic term. The primary reason for assigning ownership based on workgroup membership is that we believe it improves the power of the tests. If workgroups were to include both Starbucks and control company investors, then the predicted treatment effects could be attenuated, as Starbucks investors' behavior and preferences would be more likely to affect control company investors' behavior and preferences (the design's counterfactual).

Procter & Gamble as the control companies because these companies are sufficiently *dissimilar* to Starbucks, so as to avoid exacerbating or "washing out" treatment effects.

At the beginning of the academic term, the course instructor at each university introduces the study in class. Within this introduction, instructors announce that each participant will be given a $20 investment in a publicly traded company as part of an investment project and that students should expect a follow-up email after the class period for additional details. This email informs students that, in exchange for participating, they will receive the market value of their initial $20 investment as of a pre-determined date in the following academic term.[8] Thus, participants ultimately receive more or less than $20, depending on the assigned company's stock price performance during the investment period. The purpose of this compensation structure is to maximize the generalizability of our findings to real-world investors.

Students opt in to the investment project by clicking on a hyperlink within the body of the email, which brings them to a webpage. After the student inputs a few pieces of information (e.g., preferred name and email address), the webpage displays the stock the student has been assigned. By having students opt in to receive the investment, we filter out students who are least incentivized by the compensation—that is, those for whom $20 is a weak incentive—and therefore least likely to complete the survey at the end of the term. Removing these students from the sample *before* they know their specific investment eliminates the possibility that their non-responses could be attributed to the experimental manipulation *ex post*, which would present selection concerns.[9] In addition to displaying the name of the stock to which the student is

[8] To ensure participants' product purchase behavior is at least *capable* of influencing Starbucks' stock price performance, we design the investment period to extend beyond Starbucks' next two quarterly earnings announcements (approximately four to five months from the investment date). Another reason for designing the investment period to extend into the next academic term is to ensure the experimental treatment is still present when participants complete the online survey instrument at the end of the academic term.

[9] Having students opt in has three additional benefits. First, choosing to invest should instill in participants a greater sense of ownership of their investment position. Second, the act of opting in psychologically commits participants to

assigned, the webpage lists the names of all four companies included in the study. By identifying the four companies, we ensure that any unintentional endorsement of the company whose stock participants receive is constant across experimental conditions. If participants were to only view the company they are assigned, any perceived endorsement of that company could provide an alternative explanation for positive findings.

After opting in, participants receive individualized monthly emails updating them on the current value of their investments. These emails mimic the statements investors commonly receive from traditional brokerage firms. At the end of the academic term, participants receive a survey request via email. Because completed survey data are necessary to measure product purchase behavior, we maximize the response rate to the survey by offering respondents entry into a drawing for one of 30 $50 Amazon.com gift cards and by following up with participants who do not respond. At the conclusion of the investment period, we pay participants the current market value of their respective investments inclusive of dividends during the investment period.

3.2 DEPENDENT VARIABLES

3.2.1 Product Purchase Measures. At the end of the academic term (approximately three months into the investment period), participants complete an online survey. As the final component of this survey, participants provide information about their Starbucks purchases over the purchase period.[10] Critically, participants are not aware of our intent to collect product purchase data until *after* spending behavior has been determined and *after* they answer all questions designed to inform our potential explanations. This element is a significant strength of

complete our survey instrument at the end of the academic term. Third, clicking on the link in the email ensures that all participants in our final sample have attended to the experimental stimulus.

[10] In a pilot study, 70 graduate business students at one of the three universities reported that they purchase Starbucks products seven times per month, on average (responses ranged from '0' to '15+' times per month). Product purchase behavior that is both relatively frequent and variable across students supports the use of Starbucks as our treatment company for the product purchase measures and graduate business students as our participant pool. Note that no treatment (i.e., investment) was present in this pilot study.

our research design as it means that demand effects are unlikely to provide an explanation for our results. We are also careful to select control companies that do not directly compete with Starbucks to avoid a potential product substitution effect. If the control group were to comprise investors in another food and beverage company (e.g., McDonald's), any observed difference between conditions could either be a result of Starbucks investors spending more at Starbucks because they own Starbucks stock or control company investors spending *less* at Starbucks because they are spending more at McDonald's.

To maximize the accuracy of the purchase data, we instruct participants who use credit or debit cards at Starbucks locations to access their accounts online to retrieve their purchase information. Online bank accounts typically allow users to easily export transaction data over a specified time period. Participants can then clean the data of any sensitive identifiers and eliminate information related to non-Starbucks purchases before uploading the file to the survey website. Participants who use a Starbucks card or the Starbucks App can allow us to retrieve their transaction history directly from Starbucks by providing us permission to do so and their Starbucks card number. Participants who prefer not to upload a file or prefer not to provide us with the requested information can instead use a template to self-report their purchases. Because participants' recall for cash purchases is likely noisy and potentially biased, we do not ask for this information.[11]

In addition to gathering data on participants' own product purchase decisions, we capture participants' preference to spend money that is *not* their own at Starbucks. To collect these data, the graduate program office at each university (with the relevant course instructor's help) solicits

[11] Pilot (actual) participants report that when purchasing Starbucks products they use a credit/debit card, a registered Starbucks card, and cash/other form of payment, 68 (65), 20 (17), and 12 (18) percent of the time, respectively, on average. Further, pilot participants report purchasing Starbucks products at Starbucks locations 75% of the time, on average, implying that these purchases should be observable via participants' credit/debit card and Starbucks card data. Overall, these results provided some assurance that our product purchase data are relatively complete.

students' preferences for the products served at an upcoming event via an in-class poll approximately halfway through the academic term. Within the poll, students indicate whether they would prefer the program office to purchase and serve coffee and other beverages from Starbucks or from either of two competitors. The poll also includes another question about food preferences, which we do not use in this study. The poll is not anonymous, as we can only use data from students who opt in to our study and because we must link each vote to a particular individual's online survey responses.[12] Later in the academic term, the graduate program office at each university facilitates an event in which they serve the beverage and food items that receive the most votes.

3.2.2 Regulatory Alignment Measure. At the beginning of our online survey, participants indicate how likely it is they would vote for a hypothetical statewide increase in the minimum wage to $15 per hour. A number of cities and states have considered minimum wage increases to $15 per hour—a feature that enhances the generalizability of our findings.[13] We inform participants that the minimum wage increase is most likely to affect businesses that currently pay a significant number of employees less than $15 per hour. We state that opponents of a $15 per hour minimum wage argue that increasing the minimum wage generally hurts the profitability and value of these businesses (e.g., Draca, Machin, and Van Reenen [2011]). We also note that proponents of a $15 per hour minimum wage generally argue that the increase is fair and necessary to keep low-income families above the poverty line. We intentionally choose control companies that are less likely than Starbucks to pay employees below $15 per hour and are

[12] Once our data collection efforts are complete, we strip any identifying information from our data set. That is, our final data set only distinguishes observations via anonymous observation numbers.

[13] Prior opinions on minimum wage increases could be strong relative to any treatment effect. Consistent with the idea that an investment in Starbucks *could* influence participants' views, only 11% of pilot participants indicate being *0% Likely* to vote for a $15 per hour statewide minimum wage (i.e., the treatment could reduce the likelihood of voting for the measure for 89% of pilot participants).

therefore less likely to be negatively affected by this regulatory measure.[14] This design choice lowers the likelihood that any treatment effect would be "washed out" due to control company investors also becoming less likely to support a minimum wage increase.

3.3 OTHER PREFERENCES AND PERCEPTIONS

Consistent with prior research, it is possible that stock ownership affects preferences and perceptions beyond those related to product purchases or regulatory matters (e.g., Hales [2007]). To provide evidence on these potential effects, we ask several additional questions within the online survey. We ask participants to gauge their confidence that Starbucks' financial statements accurately represent Starbucks' financial performance and position; how likely they believe it is that Starbucks will exceed market expectations when it reports earnings the following month; and to what extent they believe Starbucks' financial reporting choices are consistent over time, a measure intended to capture perceptions of earnings quality (Dichev, Graham, Harvey, and Rajgopal [2013]). By measuring investors' perceptions after holding stock for several months, we provide evidence on how real-world complexities influence relations previously documented in the laboratory.[15]

3.4 PROCESS QUESTIONS AND ADDITIONAL MEASURES

Within the survey instrument, participants also answer questions relevant to explaining any observed differences in our dependent variables across investor groups. Specifically,

[14] Results from our pilot study support our choice. On average, pilot participants tend towards agreement with the statement "If passed, I believe this bill is likely to negatively impact Starbucks' financial performance" ($p = 0.03$, two-tailed), but tend towards disagreement with the statement "If passed, I believe this bill is likely to negatively impact Microsoft's financial performance" ($p < 0.01$, two-tailed).

[15] After our proposal was approved, we adjusted the survey to address the possibility that control company investors could become disinterested in the survey or believe the survey was not meant for them given its strict focus on Starbucks. In particular, we added a short description early in the survey informing participants that the survey includes questions about both the company they are invested in and another company. As a result, Starbucks investors answered questions about Starbucks followed by questions about a randomly selected control company, while control company investors answered questions about Starbucks followed by questions about the company they were invested in.

participants answer questions to indicate (1) their beliefs about whether their own behavior could matter for Starbucks' stock price, (2) their general affect (feelings) towards Starbucks, and (3) their anticipated feelings of discomfort when choosing not to support Starbucks when presented with opportunities to do so.

We ask participants several additional questions throughout the survey instrument. To ensure any observed difference in product purchase behavior is not driven by a sense of competition (e.g., that control company investors do not exhibit an anti-Starbucks mindset simply because they want their classmates to "lose"), we ask participants whether they viewed the investment project as a competition, and, if so, who or what they felt they were competing against. Because stock ownership could affect information gathering, we ask participants how often they accessed information about Starbucks. To provide evidence on the potential for word-of-mouth effects, we ask participants to approximate how many times a month they talked about Starbucks to people outside of their graduate program and to rate the tone of these comments. We also ask how frequently they met friends, family, and colleagues at Starbucks locations during the academic term.

Finally, we ask about various individual characteristics important for supplemental analyses. To measure social connectedness, we ask participants to name the three classmates with whom they spend the most *non*-classwork-related time and to estimate how many hours a week they spend meeting with members of their workgroups, if applicable. We also ask questions about participants' coffee and tea drinking habits and solicit participants' prior investing experience, the number of individual stocks that participants are currently invested in, and whether participants owned Starbucks stock in a personal investment account at the

beginning of the investment period. To conclude the survey, participants provide some additional demographic information and submit their product purchase data (as described above).

4. Planned Analyses and Related Results

4.1 SAMPLE AND DESCRIPTIVE STATISTICS

We start with 403 possible participants, the total number of entering graduate business students across the three universities. Of these, 280 students voluntarily opt in to the study at the beginning of the academic term; 136 are randomly assigned ownership in Starbucks and 144 are randomly assigned ownership in one of the three control companies. Approximately 96% (269 / 280) of participants begin and 89% (250 / 280) of participants ultimately complete the online survey.[16] We use all data available for each analysis; however, for all analyses, we only include participants who indicate they did *not* own stock in Starbucks as of the investment date.[17]

Table 1 presents descriptive statistics for the final sample and defines all variables used in the following analyses. None of the primary dependent variables appear to suffer from obvious floor or ceiling effects, and we find that the demographic characteristics of the participants roughly match those of most graduate business programs: 65% of participants are male, the median age is 28, and 59% are US Nationals.

< TABLE 1 >

4.2 MANIPULATION CHECK

To assess the effectiveness of the investment manipulation, we ask participants to identify the company they were given an investment in at the beginning of the academic term. All participants correctly answered this manipulation check.

[16] There is no significant difference between the expected and observed proportions of Starbucks investors who complete the online survey in its entirety (49% (136 / 280) vs. 48% (119 / 250); $\chi^2 = 0.09$; $p = 0.76$).
[17] This criterion reduces our final sample size by approximately 3%.

4.3 PLANNED HYPOTHESIS TESTS

4.3.1 Hypothesis 1 – Product Purchase Behavior. To test H1, we rely on participants' submitted purchase data for the purchase period (*OwnMoney*) and participants' preference for ordering Starbucks beverages—versus beverages from other popular chains—for a business school event (*OthersMoney*). *OwnMoney* is a continuous variable that takes only non-negative values.[18] As for all other non-indicator variables in the analyses below, we standardize *OwnMoney* by university. That is, separately for each university, we transform the variable by subtracting the mean value and dividing by the standard deviation of participants' total purchases. This transformation is necessary to account for university-specific factors (e.g., differing product prices, length of the academic term) that could cause clustering in each measure by university. *OthersMoney* is an indicator variable equal to one if the participant votes for Starbucks and zero otherwise.

Because other variables that could potentially influence product purchase behavior should be randomized across investment conditions (e.g., discretionary income and coffee-drinking habits), we test H1 by conducting an independent samples *t*-test (chi-square test) to compare the mean of *OwnMoney* (*OthersMoney*) across investor groups. Panels A and B of Table 2 report the results of these planned hypothesis tests. The results are directionally consistent with H1 but do not exhibit a statistically significant difference between Starbucks and control company investors for either *OwnMoney* ($p = 0.74$) or *OthersMoney* ($p = 0.81$).[19]

[18] We set *OwnMoney* to missing for participants who indicate using cash or unregistered gift cards more than 50% of the time when purchasing products at Starbucks locations. This restriction reduces our sample size for analyses using *OwnMoney* by approximately 6%.

[19] We conduct several robustness tests to examine the sensitivity of the null results for *OwnMoney*. Inferences are unchanged when we conduct the same analysis after log-transforming *OwnMoney* to account for right skewness or use a Tobit model. We also find that inferences are unchanged when we regress *OwnMoney* on *Treatment*, *SelfReported* (an indicator variable equal to one for self-reported data and zero otherwise), and the interaction of *Treatment* and *SelfReported*, to examine the sensitivity of the findings to the inclusion of self-reported data (i.e.,

< TABLE 2 >

4.3.2 Hypothesis 2 – Regulatory Alignment. To test H2, participants use a 101-point scale with endpoints *0% Likely* and *100% Likely* to indicate how likely it is they would vote for a $15 per hour statewide minimum wage. Because the hypothetical bill is expected to harm Starbucks' profitability if enacted, lower values on the scale correspond to greater regulatory alignment with Starbucks' financial interests. Thus, we reverse code the scale such that *higher* levels of the dependent measure, *RegulatoryAlignment*, indicate greater regulatory alignment with Starbucks' interests. As reported in Table 2, Panel C, although results of this planned hypothesis test are directionally consistent with H2, *RegulatoryAlignment* does not differ statistically across investor groups ($p = 0.72$).

4.4 PLANNED ANALYSES OF PROCESS MEASURES

As discussed in Section 2, we expect several mechanisms could explain any observed effects of stock ownership on the dependent variables. However, given we cannot reject the null hypotheses based on the tests reported in Section 4.3, we omit all planned mediation analyses. Instead, we supplement our main findings with several comparisons of the process variables across investor groups.

The process measures include *Influence (Purchasing)*, *Influence (Voting)*, *Affect*, *Dissonance (Purchasing)*, and *Dissonance (Voting)*. *Influence (Purchasing)* measures a participant's agreement with the statement, "I believe my purchases of Starbucks products could matter for Starbucks' stock price." *Influence (Voting)* measures a participant's agreement with the statement, "Given I can participate in the popular vote, I believe my vote could matter for whether this bill is passed." *Affect* measures a participant's general positive feelings towards

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product purchase data that participants choose to manually input into the survey, as opposed to data participants upload as an exported file or data we obtain from Starbucks directly).

Starbucks.[20] *Dissonance (Purchasing)* measures a participant's agreement with the statement, "I would feel discomfort if I were to purchase products from a Starbucks competitor that I could purchase from Starbucks." *Dissonance (Voting)* measures a participant's agreement with the statement, "I would feel discomfort if I were to vote in favor of a political measure that would likely harm Starbucks' financial performance." Participants indicate their responses to the *Affect* measures using fully labeled five-point scales with endpoints *Do Not Agree* (1) and *Completely Agree* (5). Participants indicate their responses to the *Influence* and *Dissonance* measures using fully labeled seven-point scales with endpoints *Strongly Disagree* (1) and *Strongly Agree* (7).

Table 3 reports the results of these planned analyses. In Panel A we examine the difference in the correlation between the primary dependent variables and the relevant measure of belief-in-influence across conditions. Although a belief that an individual's behavior can affect Starbucks' stock price should not vary based on stock assignment, this belief should be more *relevant* for Starbucks investors than it is for control company investors. For example, a belief that buying Starbucks products will affect Starbucks' stock price should not influence control company investors' Starbucks purchase behavior, given that a change in Starbucks' stock price is irrelevant to control company investors' payoff. Thus, for the belief-in-influence explanation to hold, the correlation between participants' beliefs about their ability to influence Starbucks and each dependent variable should be stronger for Starbucks investors than for control company investors. The results do not suggest that there are statistically significant differences in these correlations across investor groups (all $p > 0.21$).

[20] Specifically, participants communicate their agreement with three statements—"Starbucks is a company I have a good feeling about," "Starbucks is a company I trust," and "Starbucks is a company I admire and respect." We find that responses to these statements are internally consistent ($\alpha = 0.92$), so we average them together to create one variable, *Affect.*

Because our theory suggests that stock ownership could directly affect both affect and anticipated feelings of cognitive dissonance, in Panel B we compare the means of these process measures across investor groups. Results suggest that, relative to control company investors, Starbucks investors do not display greater affect for Starbucks nor do they anticipate greater dissonance when considering patronizing a Starbucks competitor (both $p > 0.40$).[21,22] However, Starbucks investors do report anticipating greater discomfort than control company investors from voting for a measure that would harm Starbucks' financial performance ($p = 0.03$), suggesting stock ownership does affect investors' cognitions about political and regulatory matters.

< TABLE 3 >

4.5 PLANNED SUPPLEMENTAL ANALYSES

4.5.1 Individual Characteristics as Potential Moderators. We examine several individual characteristics that plausibly moderate the relation between stock ownership and the dependent variables examined above: income, gender, age, whether the participant is a US National, the number of other investments the participant holds, and the number of times per week the participant reports purchasing coffee or tea.

We estimate the following regressions to test for potential moderating factors:

$$(1a) \quad FirmBenefit = \phi_0 + \phi_1 * Treatment + \phi_2 * Characteristic_i + \phi_3 * Treatment * Characteristic_i + \varepsilon$$

[21] Despite these null findings, Starbucks investors do report accessing information about Starbucks (e.g., via news articles, social media feeds, financial statement filings, press releases, etc.) more often than control company investors ($p = 0.06$, untabulated). This result provides evidence that participants actively responded to the experimental stimulus.

[22] In untabulated analyses, we also find that an individual's affect for Starbucks, but not her anticipated feelings of cognitive dissonance, is positively associated with *OwnMoney* ($p = 0.01$). In addition, we find that Starbucks investors tend strongly towards disagreement with the idea that they experience discomfort when purchasing products from a Starbucks competitor that they could have purchased from Starbucks ($p < 0.01$). Collectively, these results are consistent with the theory discussed in Section 2.3 inasmuch as greater affect should lead to greater patronage but an individual can manage cognitive dissonance by either altering her behavior *or* by altering her beliefs (Festinger [1957], Gilbert et al. [1998]).

$$(1b)\quad FirmBenefit = \omega_0 + \omega_1 * Treatment + \sum_{i=1}^{6} \omega_{i+1} * Characteristic_i + \sum_{i=1}^{6} \omega_{i+7} * Treatment * Characteristic_i + \varepsilon$$

where *FirmBenefit* is *OwnMoney*, *OthersMoney*, or *RegulatoryAlignment*, *Treatment* is as defined above, and $Characteristic_i$ corresponds to a single individual characteristic. Note that *i* indexes the six proposed characteristics, implying that Equation (1a) is run separately for each characteristic.

Table 4 presents the results of these planned supplemental analyses. Most of the interactions are statistically insignificant. However, we do find that the interaction of *Treatment* and *CoffeeTeaDrinks* in Panel A is positive and significant ($p = 0.03$), suggesting that the effect of the treatment on *OwnMoney* is greater among individuals who report purchasing more coffee- or tea-related beverages. Untabulated results suggest this moderating effect is driven by participants who report purchasing the highest levels of coffee and tea and is economically significant: the effect of the treatment on *OwnMoney* is approximately $17 greater for a one standard deviation increase in *CoffeeTeaDrinks*.[23] This finding reinforces the simple intuition that stock ownership is more effective among individuals who can substitute away from a competitor in favor of the firm, rather than among individuals who would need to begin purchasing a type of product when they would otherwise not do so.

In Table 4, Panel C we also find evidence that both age and nationality moderate the effect of stock ownership on *RegulatoryAlignment.* Specifically, we find that the effect of stock

[23] However, we note that the moderating effect does not appear to be linear, as the most avid coffee and tea purchasers drive this result. For example, we find that this cross-sectional effect becomes statistically insignificant if we exclude from the sample those participants for whom *CoffeeTeaDrinks* is in 95th percentile (i.e., those who purchase ten or more coffee- or tea-related beverages per week).

ownership on *RegulatoryAlignment* is greater for older individuals and for US Nationals (both $p < 0.08$), subgroups that are more likely to vote on political and regulatory matters in the US. Untabulated results show that the effect of the treatment on *RegulatoryAlignment* is approximately 16 points greater if the participant is a US National and about seven points greater for a one standard deviation increase in *Age*. An unplanned follow-up analysis reveals that the simple *t*-test of *RegulatoryAlignment* across conditions is statistically significant when we restrict the sample to US Nationals ($p = 0.09$, N = 152, untabulated). Collectively, these findings suggest that while stock ownership may not have a strong influence on the *average* individual's purchasing behavior or regulatory preferences, the effects of ownership have intuitively stronger effects for certain subgroups of individuals.

< TABLE 4 >

4.5.2 The Effect of Social Connections. Prior evidence suggests that decision-relevant information is circulated by word-of-mouth and individuals learn behaviors by observing the behaviors of others (Banerjee [1992], Bikhchandani, Hirshleifer, and Welch [1992], Ellison and Fudenberg [1993, 1995]). Consistent with this evidence, we expect a social connection with an individual investor to influence behavior in ways that benefit the company, and for this influence to be incremental to the effects of ownership itself.

To provide evidence on these effects, we estimate the following equations:

$$(2a) \quad FirmBenefit = \lambda_0 + \lambda_1 * Treatment + \lambda_2 * WorkGroupTime + \lambda_3 * Treatment * WorkGroupTime + \varepsilon$$

$$(2b) \quad FirmBenefit = \rho_0 + \rho_1 * Treatment + \rho_2 * StarbucksLinks + \rho_3 * Treatment * StarbucksLinks + \varepsilon$$

where *FirmBenefit* and *Treatment* are as defined above. *WorkGroupTime* equals the average number of hours per week the participant reports spending with workgroup members over the

course of the academic term, where applicable.[24] Because stock is assigned to participants based on group membership for two universities, more time spent with group members is indicative of closer connections to Starbucks investors for other Starbucks investors and closer connections to control company investors for other control company investors. *StarbucksLinks* equals the number of Starbucks investors (out of a maximum of three) with whom the participant "spends the most non-classwork-related time."

Table 5, Panel A presents the results of these planned analyses. The coefficient on the interaction in Equation (2a), λ_3, is not statistically significant for any of the three primary dependent variables (all $p > 0.34$). Thus, we cannot reject the null that the effects of stock ownership on Starbucks investors' purchasing behavior and regulatory preferences are similar in the presence of reinforcing social connections. Similarly, neither ρ_2 nor ρ_3 are statistically significant in any of the three estimations of Equation (2b) ($p > 0.18$). Together, these results do not support the notion that individuals are more likely to behave in ways that benefit a particular company if they have a social connection with investors of that company.

< TABLE 5 >

Finally, we examine self-reported evidence of social network effects in Table 5, Panel B. As part of the online survey, participants estimate how many times a month they met friends, family, or colleagues at a Starbucks location during the academic term (*NumberMeetings*) and how many times a month they discussed Starbucks outside of class (*FreqComments*). Participants also rate the tone of their comments about Starbucks using a seven-point, fully-labeled scale with endpoints *Very Negative* (-3) and *Very Positive* (+3) (*ToneComments*). Although these planned analyses do not provide evidence that Starbucks investors discuss

[24] Due to the decision of one university's graduate business program office to omit workgroups in the 2016-2017 academic year, the sample size for this analysis is smaller than the sample size for the analyses reported above. We note that this university has the smallest graduate business school enrollment of the three.

Starbucks more frequently or more positively than control company investors (both $p > 0.32$), they do show that Starbucks investors report meeting friends, family, or colleagues at Starbucks locations more frequently than control company investors ($p = 0.09$). This result provides some evidence that stock ownership can cause an investor to gravitate towards visiting an invested-in company, but our previous results reflect that they do not spend significantly more of their own money during these visits.

4.5.3 The Effect of Stock Ownership on Other Preferences and Perceptions. Prior laboratory-based studies suggest that stock ownership can affect investors' perceptions of future cash flows and risk. In a similar spirit, we examine whether, relative to control company investors, Starbucks investors express greater confidence in the accuracy of Starbucks' financial reporting (*ReportingConfidence*), are more likely to believe Starbucks makes consistent reporting choices (*PerceivedConsistency)*, or have higher expectations for Starbucks' earnings (*EarningsBeatLikelihood*). Participants respond to these three measures using 101-point scales with labeled endpoints (specific measures and labels are reported in Table 6). As reported in Table 6, there are no significant differences across conditions for any of these variables (all $p > 0.23$). These findings are not in line with the robust relation between stock ownership and perceptions of future cash flows and risk previously documented in the laboratory (e.g., Hales [2007]). One potential explanation for our null findings is that participants have strong prior beliefs about Starbucks' performance and financial reporting, so the incremental effect due to stock ownership is low and difficult to detect. It is also possible that other factors not captured in the laboratory setting moderate the relations previously documented.

< TABLE 6 >

4.6 PLANNED ANALYSES TO ADDRESS POTENTIAL ALTERNATIVE EXPLANATIONS

Our empirical design relies on random assignment to rule out most confounds that would be present in a non-experimental setting, such as issues related to reverse causality and self-selection. For example, the design ensures that our findings are not influenced by the tendency for investors to buy stock in companies they patronize. Moreover, we are able to randomize across conditions factors previously documented to be influential in investors' perceptions and behavior (e.g., gender, investing experience, etc.). However, it is possible that there are alternative explanations for our findings, partly because the experimental design is neither laboratory-based nor restricted to a short experimental time frame. We briefly discuss some of these alternative explanations and their relation to our results.

4.6.1 A Competition Effect. It is possible that a participant perceives her investment to be part of a competition and that this sense of competition influences her judgments and decisions. For example, a student invested in a control company could shift away from purchasing products from Starbucks if she seeks to (and believes she can) undermine other students' investments, consistent with a sense of competition among classmates that is not easily generalizable beyond the experimental setting. It is also possible that students do the opposite. For example, control company investors could purchase *more* Starbucks products in an effort to help Starbucks investors. This behavior would bias against us finding results.

We make several design decisions to minimize any sense of competition as a potential confounding factor. For instance, we inform students that there are four companies to which they may be assigned (hoping to disperse any feelings of direct competition), and we design the compensation structure so each participant's payoff is increasing *only* in the invested-in company's performance (removing any economic incentive to compete). We also measure as

part of the survey instrument whether students viewed the investment project as a competition, and we reexamine the results after excluding individuals who state that they viewed the project as a competition with other students in the class. These analyses produce inferentially identical results to those reported above.

4.6.2 A Wealth Effect. Another possible alternative explanation is that differences across conditions in stock returns make participants in certain conditions feel richer than those in other conditions. For example, it is possible that the average Starbucks investor does not purchase more Starbucks products than the average control company investor because she feels *poorer* than the average control company investor. *Ex post*, this explanation is unlikely because the stock returns of Starbucks and the control companies are relatively similar over the purchase period; Starbucks returned approximately 1.4%, 3M -0.5%, Microsoft 8.7%, and P&G -3.3%. We also find that the difference in *OwnMoney* between Starbucks and control company investors is not significant when we restrict the pool of control company investors to any one of the three control companies.[25]

4.6.3 Selection Concerns. As discussed in Section 4.1, not all participants who opt in to the study at the beginning of the academic term complete the online survey at the end of the term. Further, some participants begin the online survey but do not complete the last portion of it (related to Starbucks purchase data)—i.e., we observe some level of attrition.[26] Although factors unrelated to the treatment that cause attrition should be randomized across conditions (e.g.,

[25] To provide further evidence on the effect of an investment's performance on investors' willingness to take actions that benefit the invested-in company, we also conduct an analysis that exploits time series variation in the dependent variable, *OwnMoney*, and Starbucks stock returns over the purchase period. Specifically, we test whether concurrent or trailing returns are differentially associated with product purchase behavior for Starbucks versus control company investors. Untabulated results do not support the notion that weekly Starbucks stock returns moderate the relation between Starbucks ownership and weekly Starbucks product purchases ($p > 0.58$).

[26] In addition to making every attempt to maximize our overall response rate, we also take steps to reduce attrition. For example, we provide instructions that minimize the time necessary to provide purchase data and we follow up with participants who begin but do not complete the entire survey.

concerns about privacy), *unbalanced* attrition rates between treatment and control investors could be indicative of underlying selection issues. For example, it could be that control company investors who make more Starbucks purchases are less willing to provide purchase data—a situation that would bias in favor of H1. This scenario is plausible if control company investors feel less motivated than Starbucks investors to provide purchase information *and* believe the effort required to report this information is increasing in the number of purchases made.

To alleviate such selection concerns, we offer participants entry into a drawing for one of 30 $50 Amazon.com gift cards in exchange for completing the survey, as noted above. Entry into the drawing is in addition to the right to receive the market value of their $20 investment and should provide participants with an incentive to complete the survey that is independent of their feelings about the companies or related stock price changes. We also designed the instructions to minimize any perceived relation between the effort required to provide purchase data and the amount of purchases made at Starbucks. The number of Starbucks purchases an individual makes over the purchase period should not substantially affect the effort it takes to export and filter credit card transaction history or to provide us with the information we need to collect the transaction data captured on a Starbucks card. Thus, even if control company investors are less motivated to provide information on their Starbucks purchases relative to Starbucks investors, participants' propensity to complete the survey should not be strongly correlated with *OwnMoney*. In other words, any unbalanced attrition that results from lower motivation is unlikely to cause bias in our tests.

Still, we recognize that unanticipated or unknown selection issues could affect the results. *Ex post* we expect these issues to be minor, as only 19 of the 269 participants who begin the online survey fail to complete it. Nonetheless, we implement Heckman's [1976] two-step

estimator to attempt to correct for potential response bias related to *OwnMoney*. In the first stage, we estimate a probit model to predict participants' decision to provide purchase data in the survey instrument. Explanatory variables for the decision to respond include basic demographic variables (including income, nationality, age, and gender), process measures (specifically, *Affect* and *Dissonance*), and the time it takes for the participant to complete preceding portions of the survey after starting the survey, which is intended to satisfy the exclusion restriction.[27] In the second stage, we regress *OwnMoney* on *Treatment* and the inverse Mills ratio using OLS, correcting for heteroskedasticity (e.g., Bushway, Johnson, and Slocum [2007]). Our inferences are unchanged using this approach.[28]

4.6.4 Issues of Statistical Power. Finally, it could be that our tests lack sufficient power to detect a treatment effect of practical importance. Given the observed distributions of our three primary dependent variables, the hypothesis tests reported in Section 4.3 should detect a treatment effect at the $\alpha = 0.10$ (one-tailed) level of \$19.90 in *OwnMoney*, of 16% in *OthersMoney*, and of nine points on the 101-point *RegulatoryAlignment* scale with power $(1 - \beta) = 0.80$. Overall, these analyses suggest that the levels of the treatment effects necessary to detect significant differences between conditions are reasonable (Tabachnick and Fidell [2007]).

Nonetheless, we recognize there are a variety of potential determinants of the dependent variables we study, and controlling for some of these determinants could improve our ability to detect treatment effects that exist. Accordingly, we include control variables in a series of

[27] Inferences are unchanged if we exclude purchases made after the date the online survey was first distributed from *OwnMoney* and include an additional variable in the first stage intended to satisfy the exclusion restriction—namely, the time it took for the participant to begin the survey after receiving the survey request.

[28] In our approved proposal, we proposed using another variable meant to satisfy the exclusion criteria—whether the participant begins the survey on a computer versus a mobile device. However, to maximize our response rate, we opted to explicitly direct individuals to complete the survey on a computer, which forced us to drop this variable.

multivariate robustness tests to improve the statistical power of the tests.[29] Untabulated results show that controlling for these additional determinants substantially improves the explanatory power of the regressions. For example, including the controls for *OwnMoney* raises the R^2 of the regression from 0.001 (when *Treatment* is the only explanatory variable) to roughly 0.20. However, the inclusion of these variables does not change our inferences for any of the three primary dependent variables.

4.7 PLANNED BAYESIAN PARAMETER ESTIMATION

We supplement our main findings with Bayesian data analysis techniques, which are of particular interest given our planned null hypothesis significance tests are inconclusive. Bayesian parameter estimation allows a Bayesian to (1) update her beliefs about the relative credibility of all candidate values of δ (where, for our purposes, δ represents the relation between stock ownership and a dependent variable), and (2) accept (not merely reject) a null hypothesis (Kruschke [2011]).

A Bayesian begins with a belief about each possible value of δ—that is, a probability density function over the space of all possible δ—and revises these beliefs according to Bayes' rule upon receiving additional information about δ. Because different readers likely hold different prior beliefs about the distribution of possible δ, we identify multiple prior distributions and present a posterior distribution for each prior. This community of priors includes both *skeptical priors* (normal distributions centered at δ = 0) and *enthusiastic priors* (e.g., for *OwnMoney*, normal distributions centered at δ = $40) to ensure we address the perspectives of a

[29] For *OwnMoney*, controls include age, gender, income, nationality, whether the participant does or does not drink coffee or tea, the number of times per week the participant reports purchasing coffee or tea beverages, and whether the participant shares the credit or debit card that she uses to make purchases at Starbucks with another individual. For *OthersMoney*, controls include age, gender, income, nationality, and whether the participant does or does not drink coffee or tea. For *RegulatoryAlignment*, controls include age, gender, income, nationality, and political leaning (*fiscally liberal* to *fiscally conservative*). Untabulated tests show that none of these control variables have a statistically significant correlation with the randomly assigned treatment, as expected.

wide range of individuals (Kass and Greenhouse [1989], Speigelhalter, Freedman, and Parmar [1994]).[30] We also pair each prior mean with two different variances, which correspond to low versus high confidence prior beliefs.

Table 7 reports the results of these analyses. To aid interpretation, we calculate an interval ($a < \delta < b$) that represents the most credible values of δ—the 95% *highest posterior density* (HPD) interval (Hoff [2009])—for each posterior distribution.[31] The results show belief revision for each of the three dependent variables—slightly upward for skeptical priors and substantially downward for enthusiastic priors. For example, the enthusiastic, low confidence prior for *OwnMoney* corresponds to a mean posterior belief about the effect of the treatment on *OwnMoney* (δ) of roughly $22 (down from $40). Similarly, the variance of the posterior distribution is roughly half the variance of the prior distribution.

< TABLE 7 >

Because even values close to zero are effectively zero for practical purposes, the Bayesian can also establish a *region of practical equivalence* (ROPE) around $\delta = 0$ (Kruschke [2011]). Whereas values outside of the ROPE are values of δ that she believes are economically meaningful, values within the ROPE are values of δ that she believes are equivalent to zero for practical purposes. The decision rule is then as follows: If the HPD interval lies entirely inside (outside) the Bayesian's ROPE, then she accepts (rejects) the null hypothesis for practical purposes; else, she suspends judgment. For example, consider a Bayesian with a prior belief about the effect of an investment in Starbucks on Starbucks purchases (δ) that is normally

[30] For interpretability, we do not standardize *OwnMoney* or *RegulatoryAlignment* in these analyses. Including university fixed effects does not meaningfully affect the results.

[31] The 95% HPD interval has two features: (1) the posterior probability of the HPD interval is 95% (i.e., the cumulative credibility of all δ in (a, b) is 95%), and (2) the minimum density of any point in the HPD interval is greater than or equal to the density of any point outside the interval (i.e., each value of δ in (a, b) is considered to be more credible than the most credible value outside (a, b)).

distributed with mean zero and variance 100. Assume also that this Bayesian believes that no less than a $20 change in money spent at Starbucks over the treatment period is economically meaningful. Because the 95% HPD given this prior (-10.99, 16.39) falls within the Bayesian's ROPE (-20, 20) (see Table 7), she would accept the null hypothesis of no effect. We leave it to the reader to determine a meaningful ROPE and to use this ROPE to draw her own conclusions.

5. Conclusion

In this study, which was pre-registered via the *Journal of Accounting Research*'s registration based editorial process, we examine the effects of public company stock ownership on individual investors' product preferences, purchase decisions, and other views and preferences that could affect the company's performance. In contrast to prior survey evidence, we find very little evidence that a small investment in a public company's stock causes the average investor to change her product purchase behavior or regulatory preferences. Consistent with the generally null findings for our measures using frequentist statistics, Bayesian parameter estimation shows substantial downward belief revision for more optimistic *ex ante* expectations of the effects of stock ownership on the dependent variables for the average investor.

In planned supplemental analysis, however, we do find evidence that the effects of Starbucks ownership on product purchases and on regulatory preferences are stronger for certain, intuitive subgroups of participants. Specifically, we find that the effect of ownership on purchase behavior is greater for participants who report purchasing more coffee- and tea-related beverages, and the effect of ownership on regulatory preferences is stronger for subgroups that are more likely to vote on US political and regulatory matters—namely, older participants and participants who are US Nationals. We also find that, relative to control company investors, Starbucks investors report experiencing greater discomfort voting for political measures that

would undermine Starbucks' financial performance, suggesting that ownership has at least a marginal effect on cognitions about regulation.

There are a number of avenues for future work to extend our study. As with any experiment, a potential concern is that the findings do not generalize to a more "real world" setting. For example, even though the opt-in process should screen out participants for whom the compensation is too low to be meaningful, it may be that graduate business students respond differently to stock ownership than other individual investors given that a $20 investment is smaller than the typical investment holding. It is also possible that the effects of ownership on purchase behavior and other preferences are different when the duration of ownership is longer or when the date of the sale of the stock is not predetermined. Further, it may be that the incremental, causal effects of stock ownership on behaviors that benefit the company are different when investors choose which stocks they own (i.e., when real-world investment self-selection is incorporated into the design). Addressing these limitations with alternative research designs would be a fruitful avenue for future work.

Future work could also build on our study in other ways. Evidence from the cross-sectional tests suggest that the effects of ownership on investor behaviors depend on the relevance of the dependent variable to the investor. For example, we find that the effect of the $20 investment on product purchases is substantially greater for the most avid coffee and tea purchasers. These results raise the possibility that ownership assigned based on a company's own screening mechanisms—rather than random assignment across a broad cross-section of individuals—has stronger relations to outcome variables of interest to the firm.[32] Future work could also investigate the duration of the benefits of stock ownership, as investors who alter their

[32] For example, Domino's Pizza, Inc. recently began giving away ten free shares of its stock to randomly selected members of its loyalty program. See: http://www.prnewswire.com/news-releases/dominos-piece-of-the-pie-rewards-program-just-became-more-rewarding-300372465.html.

behaviors or preferences in response to ownership may develop habits or preferences that benefit the company even after the investor no longer owns the stock. Further evidence on potential moderating effects could be informative as well, including evidence of the sensitivity of the effects to large stock price movements or highly publicized positive or negative news.

APPENDIX
Deviations from our approved proposal

Subsequent to approval of our proposal, we implemented two changes. We describe each change below:

(1) After our proposal was approved, we adjusted our experimental instrument to address the possibility that control company investors could become disinterested in the survey or believe the survey was not meant for them given its strict focus on Starbucks. In particular, we added a short description early in the survey informing participants that the survey includes questions about both the company they are invested in *and* another company. As a result, Starbucks investors answered questions about Starbucks followed by questions about a randomly selected control company, while control company investors answered questions about Starbucks followed by questions about the company they were invested in. We note this deviation in footnote 15 of the registered report.

(2) In our approved proposal, we proposed using three variables meant to satisfy the exclusion criteria for our Heckman two-step estimator: (1) the time it takes for participants to complete preceding portions of the survey after starting the survey, (2) the time it takes for participants to begin the survey after receiving the survey request, and (3) whether participants begin the survey on a computer versus a mobile device. However, to maximize our response rate, we opted to explicitly direct individuals to complete the survey on a computer, which forced us to drop this third variable. We note this deviation in footnote 28 of the registered report.

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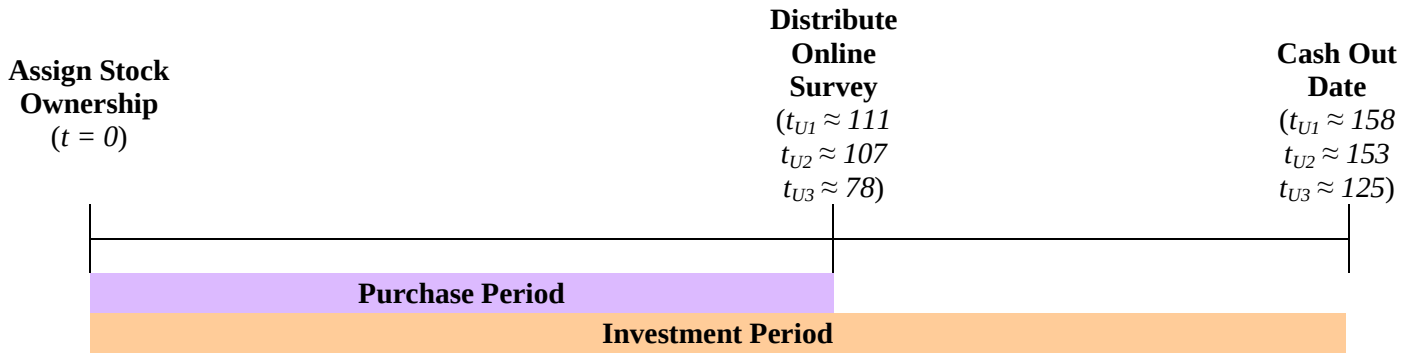
F I G U R E 1

Timeline of experimental procedures



Investment Period: All experimental procedures occur during the investment period. The investment period is university-specific, as it starts at the beginning of the academic term for all participants. For all universities, the investment period ends on the same pre-specified date in the following academic term. Throughout the investment period, participants receive statements monthly to keep them up to date on how their investment is performing.

Purchase Period: The purchase period lasts the length of the academic term. Participants make Starbucks product purchase decisions throughout the purchase period and communicate their preferred beverage supplier (Starbucks or one of two competitors) for a business school event via an in-class poll.

Online Survey: At the end of the academic term, we distribute our primary survey instrument via email. Participants communicate the likelihood they would vote for a statewide minimum wage increase to $15 per hour, provide process measures, provide data on their Starbucks product purchases during the purchase period, and answer questions related to other preferences and perceptions.

Cash Out Date: We pay participants the market value of their investment plus any dividends.

Figure 1 illustrates the timing of our registered experimental procedures. We run the experiment at three different universities (denoted "U1," "U2," and "U3"). Participants receive news of their $20 investment shortly after the beginning of the academic term on *t (day)* = 0, receive access to our primary survey instrument shortly after the end of the academic term on $t \approx 111$ (U1), $t \approx 107$ (U2), or $t \approx 78$ (U3), and receive the market value of their investment as of $t \approx 158$ (U1), $t \approx 153$ (U2), or $t \approx 125$ (U3). Because participants have a two-week window to opt into the experiment, the exact lengths of the purchase and investment periods vary slightly by participant.

T A B L E 1

Descriptive statistics

	25th pctile	Median	75th pctile	Mean	N obs
Primary dependent variables					
OwnMoney	2.47	14.93	43.42	38.26	221
OthersMoney	0	0	1	0.42	246
RegulatoryAlignment	18	39	80	46.54	258
Potential process variables					
Influence (Purchasing)	2	4	5	3.91	258
Influence (Voting)	5	5	6	5.15	258
Affect	2.67	3.33	4	3.33	258
Dissonance (Purchasing)	1	2	4	2.61	258
Dissonance (Voting)	2	3	5	3.61	258
Potential moderating variables					
Income	1	2	4	2.76	258
Female	0	0	1	0.35	258
Age	26	28	31	28.82	257
USNational	0	1	1	0.59	258
NumberInvestments	0	0	2	2.23	258
CoffeeTeaDrinks	1	3	5	3.68	258
Social connections variables					
WorkGroupTime	6	11	20	14.26	219
StarbucksLinks	0	1	2	1.20	258
NumberMeetings	0	2	4	2.56	258
FreqComments	2	3	5	4.97	258
ToneComments	4	5	6	5.20	257
Financial reporting variables					
ReportingConfidence	69	78	85	74.35	258
PerceivedConsistency	60	71	80	69.75	258
EarningsBeatLikelihood	50	60	71	59.59	258
Other variables					
SelfReported	0	0	1	0.46	213
CreditCardShare	0	0	0	0.17	242
PoliticalLeaning	-2	0	3	0.22	258
CoffeeTeaYN	1	1	1	0.92	258
Access	1	2	4	3.13	258

Table 1 presents descriptive statistics (25th percentile, median, 75th percentile, mean, and number of observations). The descriptive statistics for non-indicator variables are presented before standardization within university. *OwnMoney* is a continuous variable equal to the participant's total reported purchases of Starbucks products. *OthersMoney* is an indicator variable equal to one if the participant votes for Starbucks beverages to be supplied at a business school event during the academic term and zero otherwise. *RegulatoryAlignment* is a continuous variable equal to 100 minus the participant's reported likelihood of voting for a $15 per hour

statewide minimum wage (on a 101-point scale). *Influence* is a discrete variable ([1 to 7]) corresponding to a fully-labeled scale with endpoints *Strongly Disagree* and *Strongly Agree* for the statement, "I believe my purchases of Starbucks products could matter for Starbucks' stock price" (*Purchasing*) or the statement, "Given I can participate in the popular vote, I believe my vote could matter for whether this bill is passed" (*Voting*). *Affect* is an average of three discrete variables ([1 to 5]) corresponding to a fully-labeled scale with endpoints *Do Not Agree* and *Completely Agree* for the statements, "Starbucks is a company I have a good feeling about," "Starbucks is a company I trust," and "Starbucks is a company I admire and respect." *Dissonance* is a continuous variable ([1 to 7]) corresponding to a fully-labeled scale with endpoints *Strongly Disagree* and *Strongly Agree* for the statement, "I would feel discomfort if I were to purchase products from a Starbucks competitor that I could purchase from Starbucks" (*Purchasing)* or the statement, "I would feel discomfort if I were to vote in favor of a political measure that would likely harm Starbucks' financial performance" (*Voting*). *Income* is a discrete variable ([1 to 6]) corresponding to responses *$50,000 or less*, *Between $50,001 and $75,000*, *Between $75,001 and $100,000*, *Between $100,001 and $125,000*, *Between $125,001 and 150,000*, or *More than $150,000* for the question, "What was your annual household income prior to beginning your graduate program?" *Female* is an indicator variable equal to one if the participant is female and zero otherwise. *Age* is a continuous variable equal to the age of the participant. *USNational* is an indicator variable equal to one if the participant is a US National and zero otherwise. *NumberInvestments* is a discrete variable equal to the number of different companies the participant is invested in (not through a mutual fund or similar investment vehicle) at the end of the purchase period. *CoffeeTeaDrinks* is a continuous variable equal to the number of times per week the participant reports purchasing coffee- or tea-related beverages. *WorkGroupTime* is a continuous variable equal to the number of hours a week the participant spends meeting with members of her workgroup throughout the academic term. *StarbucksLinks* is a discrete variable ([0 to 3]) equal to the number of Starbucks investors (out of a maximum of three) in the graduate program with whom the participant spends the most non-classwork-related time. *NumberMeetings* is a continuous variable equal to the number of times a month the participant meets family, friends, or colleagues at a Starbucks location during the academic term. *FreqComments* is a continuous variable equal to the number of times a month the participant talked to individuals outside of class about Starbucks during the academic term. *ToneComments* is a discrete variable ([-3 to +3]) corresponding to a scale *Very Negative* to *Very Positive* for the question, "When you talk about Starbucks, what is typically the tone of your comments?" *ReportingConfidence* is a discrete variable ([0 to 100]) corresponding to a scale *Not at All Confident* to *Very Confident* for the question, "How confident are you that Starbucks' financial statements accurately depict the financial performance and position of Starbucks?" *PerceivedConsistency* is a discrete variable ([0 to 100]) corresponding to a scale *Not at All Consistent* to *Very Consistent* for the question, "How consistent do you believe Starbucks' financial reporting choices are over time?" *EarningsBeatLikelihood* is a discrete variable ([0 to 100]) corresponding to a scale *Not at All Likely* to *Very Likely* for the question, "How likely is it that Starbucks will exceed market expectations when it reports earnings next month?" *SelfReported* is an indicator variable equal to one if the participant self-reports by manually inputting purchase data and zero otherwise. *CreditCardShare* is an indicator variable equal to one if the participant reports sharing a credit card that is used to make Starbucks purchases with another individual and zero otherwise. *PoliticalLeaning* is a discrete variable ([-5 to 5]) corresponding to a participant's indication of their fiscal political preference (in general) on a scale from *Fiscally Liberal* to *Fiscally Conservative*. *CoffeTeaYN* is an indicator variable equal to one if the participant drinks coffee or tea and zero otherwise. *Access* is a continuous variable equal to the number of times a month the participant reports accessing information about Starbucks during the academic term (e.g., via news articles, social media feeds, financial statement filings, press releases, etc.).

T A B L E 2

Comparisons across experimental conditions of OwnMoney, OthersMoney, and *RegulatoryAlignment*

Panel A: *OwnMoney*

	Starbucks investors (n)	Control investors (n)	Mean difference	t-Statistic (p-Value)
Mean	0.023	-0.022	0.045	0.34
	(106)	(115)		(0.737)

Panel B: *OthersMoney*

	Starbucks investors (n)	Control investors (n)	Mean difference	Chi-squared (p-Value)
Mean (%)	42.6%	41.1%	1.5%	0.06
	(122)	(124)		(0.812)

Panel C: *RegulatoryAlignment*

	Starbucks investors (n)	Control investors (n)	Mean difference	t-Statistic (p-Value)
Mean	0.022	-0.022	0.044	0.36
	(127)	(131)		(0.722)

Table 2 presents the results of planned hypothesis tests that compare *OwnMoney*, *OthersMoney*, and *RegulatoryAlignment* across experimental conditions. Panel A presents the mean of *OwnMoney* by condition (Starbucks investors or control company investors) and shows the result of the independent samples t-test. Panel B presents the mean of *OthersMoney* by condition (Starbucks investors or control company investors) and shows the result of the chi-squared test. Panel C presents the mean of *RegulatoryAlignment* by condition (Starbucks investors or control company investors) and shows the result of the independent samples t-test. *OwnMoney* is a continuous variable equal to the participant's total reported purchases of Starbucks products. *OthersMoney* is an indicator variable equal to one if the participant votes for Starbucks beverages to be supplied at a business school event during the academic term and zero otherwise. *RegulatoryAlignment* is a continuous variable equal to 100 minus the participant's reported likelihood of voting for a $15 per hour statewide minimum wage (on a 101-point scale). *OwnMoney* and *RegulatoryAlignment* are standardized to have a mean of zero and standard deviation of one within each university. *, **, and *** denote statistical significance at the two-tailed 10%, 5%, and 1% levels, respectively.

T A B L E 3

Comparisons across experimental conditions of process variables

Panel A: *Influence*

	Correlation for Starbucks investors (n)	Correlation for control investors (n)	Difference	t-Statistic (p-Value)
OwnMoney and *Influence (Purchasing)*	0.100 (106)	-0.067 (115)	0.167	1.24 (0.215)
OthersMoney and *Influence (Purchasing)*	-0.039 (122)	0.071 (124)	-0.110	-0.87 (0.387)
RegulatoryAlignment and *Influence (Voting)*	-0.174 (127)	-0.236 (131)	0.062	0.41 (0.680)

Panel B: *Affect* and *Dissonance*

	Mean for Starbucks investors (n)	Mean for control investors (n)	Difference	t-Statistic (p-Value)
Affect	0.011 (127)	-0.010 (131)	0.021	0.17 (0.867)
Dissonance (Purchasing)	0.052 (127)	-0.051 (131)	0.103	0.83 (0.406)
Dissonance (Voting)	0.137 (127)	-0.133 (131)	0.270	2.19** (0.029)

Table 3 presents the results of planned supplementary analyses that compare the proposed process variables, *Influence (Purchasing)*, *Influence (Voting)*, *Affect*, *Dissonance (Purchasing)*, and *Dissonance (Voting)*, across experimental conditions. Panel A presents the correlations between *Influence (Purchasing)* or *Influence (Voting)* and the corresponding dependent variables, *OwnMoney*, *OthersMoney*, and *RegulatoryAlignment*, by condition (Starbucks investors or control company investors), and presents the results of a statistical comparison of these correlations across conditions. Panel B presents the mean of *Affect*, *Dissonance (Purchasing)*, and *Dissonance (Voting)* by condition (Starbucks investors or control company investors) and shows the results of the independent samples t-tests. *OwnMoney* is a continuous variable equal to the participant's total reported purchases of Starbucks products. *OthersMoney* is an indicator variable equal to one if the participant votes for Starbucks beverages to be supplied at a business school event during the academic term and zero otherwise. *RegulatoryAlignment* is a continuous variable equal to 100 minus the participant's reported likelihood of voting for a $15 per hour statewide minimum wage (on a 101-point scale). *Influence* is a discrete variable ([1 to 7]) corresponding to a fully-labeled scale with endpoints *Strongly Disagree* and *Strongly Agree* for the statement, "I believe my purchases of Starbucks products could matter for Starbucks' stock price" (*Purchasing*) or the statement, "Given I can participate in the popular vote, I believe my vote could matter for whether this bill is passed" (*Voting*). *Affect* is an average of three discrete variables ([1 to 5]) corresponding to a fully-labeled scale with endpoints *Do Not Agree* and *Completely Agree* for the statements, "Starbucks is a company I have a good feeling about," "Starbucks is a company I trust," and "Starbucks is a company I admire and respect." *Dissonance* is a continuous variable ([1 to 7]) corresponding to a fully-labeled scale with endpoints *Strongly Disagree* and *Strongly Agree* for the statement, "I would feel discomfort if I were to purchase products from a Starbucks competitor that I could purchase from Starbucks" (*Purchasing)* or the statement,

"I would feel discomfort if I were to vote in favor of a political measure that would likely harm Starbucks' financial performance" (*Voting*). All non-indicator variables are standardized to have a mean of zero and standard deviation of one within each university. *, **, and *** denote statistical significance at the two-tailed 10%, 5%, and 1% levels, respectively.

T A B L E 4

Individual characteristics as potential moderators

$$FirmBenefit = \phi_0 + \phi_1 * Treatment + \phi_2 * Characteristic_i + \phi_3 * Treatment * Characteristic_i + \varepsilon$$

$$FirmBenefit = \omega_0 + \omega_1 * Treatment + \sum_{i=1}^{6} \omega_{i+1} * Characteristic_i + \sum_{i=1}^{6} \omega_{i+7} * Treatment * Characteristic_i + \varepsilon$$

Panel A: *OwnMoney*

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Intercept	-0.026	-0.146	-0.024	0.163	-0.023	-0.019	0.078
	(-0.28)	(-1.33)	(-0.26)	(1.06)	(-0.24)	(-0.22)	(0.50)
Treatment	0.049	0.155	0.043	-0.001	0.046	0.073	-0.028
	(0.37)	(0.95)	(0.32)	(-0.01)	(0.34)	(0.59)	(-0.13)
Income	0.127						0.148
	(1.47)						(1.59)
Female		0.421**					0.394**
		(2.08)					(2.14)
Age			0.168*				0.117
			(1.73)				(1.24)
USNational				-0.290			-0.348*
				(-1.51)			(-1.86)
NumberInvestments					0.010		0.006
					(0.12)		(0.08)
CoffeeTeaDrinks						0.270***	0.250***
						(3.08)	(2.87)
Treatment x Income	-0.029						0.010
	(-0.22)						(0.07)
Treatment x Female		-0.384					-0.306
		(-1.35)					(-1.17)
Treatment x Age			-0.334**				-0.247*
			(-2.39)				(-1.81)
Treatment x USNational				0.058			0.291
				(0.21)			(1.10)
Treatment x NumberInvestments					-0.010		-0.009
					(-0.07)		(-0.07)
Treatment x CoffeeTeaDrinks						0.281**	0.288**
						(2.21)	(2.25)
N	221	221	221	221	221	221	221
Adj R^2	0.001	0.007	0.013	0.004	-0.013	0.163	0.189

Panel B: *OthersMoney*

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Intercept	-0.362**	-0.588***	-0.382**	0.043	-0.362*	-0.358*	-0.127
	(-1.98)	(-2.58)	(-2.06)	(0.15)	(-1.96)	(-1.96)	(-0.36)
Treatment	0.062	0.269	0.086	0.300	0.022	0.063	0.382
	(0.24)	(0.83)	(0.33)	(0.74)	(0.08)	(0.24)	(0.77)
Income	0.083						0.462**
	(0.49)						(2.12)
Female		0.688*					0.558
		(1.76)					(1.35)
Age			-0.301				-0.460**
			(-1.54)				(-2.09)
USNational				-0.659*			-0.743*
				(-1.75)			(-1.75)
NumberInvestments					-0.329		-0.363
					(-1.46)		(-1.46)
CoffeeTeaDrinks						0.200	0.159
						(1.04)	(0.74)
Treatment x Income	-0.153						-0.275
	(-0.58)						(-0.84)
Treatment x Female		-0.632					-0.539
		(-1.16)					(-0.92)
Treatment x Age			0.395				0.507*
			(1.47)				(1.67)
Treatment x USNational				-0.511			-0.346
				(-0.95)			(-0.59)
Treatment x NumberInvestments					-0.220		-0.206
					(-0.61)		(-0.52)
Treatment x CoffeeTeaDrinks						0.062	-0.025
						(0.23)	(-0.08)
N	246	246	245	246	246	246	245
Pseudo R^2	0.001	0.010	0.009	0.038	0.023	0.009	0.086

Panel C: *RegulatoryAlignment*

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Intercept	-0.024	0.075	-0.017	-0.181	-0.024	-0.021	-0.022
	(-0.28)	(0.72)	(-0.20)	(-1.34)	(-0.28)	(-0.24)	(-0.15)
Treatment	0.052	0.081	0.043	-0.226	0.050	0.037	-0.217
	(0.43)	(0.53)	(0.35)	(-1.21)	(0.40)	(0.30)	(-1.01)
Income	0.100						0.127
	(1.24)						(1.40)
Female		-0.310*					-0.309*
		(-1.66)					(-1.70)
Age			-0.116				-0.170*
			(-1.33)				(-1.93)
USNational				0.261			0.158
				(1.51)			(0.87)
NumberInvestments					0.066		0.017
					(0.86)		(0.22)
CoffeeTeaDrinks						-0.050	-0.026
						(-0.60)	(-0.32)
Treatment x Income	0.163						0.068
	(1.31)						(0.50)
Treatment x Female		-0.037					0.063
		(-0.14)					(0.25)
Treatment x Age			0.223*				0.228*
			(1.78)				(1.80)
Treatment x USNational				0.496**			0.469*
				(2.04)			(1.84)
Treatment x NumberInvestments					0.013		-0.086
					(0.10)		(-0.64)
Treatment x CoffeeTeaDrinks						-0.227*	-0.181
						(-1.83)	(-1.48)
N	258	258	257	258	258	258	257
Adj R^2	0.024	0.014	0.001	0.068	-0.006	0.025	0.106

Table 4 presents the results of planned supplementary analyses that examine variables that potentially moderate the relation between *Treatment* and *OwnMoney* (Panel A), between *Treatment* and *OthersMoney* (Panel B), and between *Treatment* and *RegulatoryAlignment* (Panel C). The analyses for *OwnMoney* and for *RegulatoryAlignment*, presented in Panels A and C, respectively, use OLS. The analysis for *OthersMoney*, presented in Panel B, uses a probit model. *OwnMoney* is a continuous variable equal to the participant's total reported purchases of Starbucks products. *OthersMoney* is an indicator variable equal to one if the participant votes for Starbucks beverages to be supplied at a business school event during the academic term and zero otherwise. *RegulatoryAlignment* is a continuous variable equal to 100 minus the participant's reported likelihood of voting for a $15 per hour statewide minimum wage (on a 101-point scale). *Treatment* is an indicator variable equal to one if the participant is assigned ownership in Starbucks and zero otherwise. *Income* is a discrete variable ([1 to 6])

corresponding to responses *$50,000 or less*, *Between $50,001 and $75,000*, *Between $75,001 and $100,000*, *Between $100,001 and $125,000*, *Between $125,001 and 150,000*, or *More than $150,000* for the question, "What was your annual household income prior to beginning your graduate program?" *Female* is an indicator variable equal to one if the participant is female and zero otherwise. *Age* is a continuous variable equal to the age of the participant. *USNational* is an indicator variable equal to one if the participant is a US National and zero otherwise. *NumberInvestments* is a discrete variable equal to the number of different companies the participant is invested in (not through a mutual fund or similar investment vehicle) at the end of the purchase period. *CoffeeTeaDrinks* is a continuous variable equal to the number of times per week the participant reports purchasing coffee or tea. All non-indicator variables are standardized to have a mean of zero and standard deviation of one within each university. *, **, and *** denote statistical significance at the two-tailed 10%, 5%, and 1% levels, respectively.

T A B L E 5

The effects of social connections

$$FirmBenefit = \lambda_0 + \lambda_1 * Treatment + \lambda_2 * WorkGroupTime + \lambda_3 * Treatment * WorkGroupTime + \varepsilon$$

$$FirmBenefit = \rho_0 + \rho_1 * Treatment + \rho_2 * StarbucksLinks + \rho_3 * Treatment * StarbucksLinks + \varepsilon$$

Panel A: ***Field Measures of Social Connections***

	OwnMoney		*OthersMoney*		*RegulatoryAlignment*	
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	-0.071	-0.024	-0.413**	-0.340*	0.011	-0.033
	(-0.70)	(-0.26)	(-2.07)	(-1.81)	(0.12)	(-0.37)
Treatment	0.146	0.079	0.067	0.096	-0.026	0.051
	(1.00)	(0.58)	(0.24)	(0.36)	(-0.19)	(0.40)
WorkGroupTime	0.046		0.344*		-0.052	
	(0.41)		(1.66)		(-0.53)	
StarbucksLinks		-0.014		0.082		-0.057
		(-0.14)		(0.39)		(-0.58)
Treatment x WorkGroupTime	0.094		-0.267		0.109	
	(0.62)		(-0.95)		(0.80)	
Treatment x StarbucksLinks		-0.183		-0.347		0.080
		(-1.34)		(-1.26)		(0.62)
N	187	221	214	246	219	258
Adj R^2	0.001	0.009			-0.011	-0.010
Pseudo R^2			0.011	0.007		

Panel B: ***Self-Reported Measures of Social Connections***

	Starbucks investors (n)	Control investors (n)	Mean difference	t-Statistic (p-Value)
NumberMeetings	0.107	-0.104	0.211	1.70*
	(127)	(131)		(0.090)
FreqComments	0.062	-0.060	0.122	0.98
	(127)	(131)		(0.328)
ToneComments	0.060	-0.057	0.117	0.94
	(126)	(131)		(0.347)

Table 5 presents the results of planned supplementary analyses that examine the cross-sectional effects of social connections on *OwnMoney*, *OthersMoney*, and *RegulatoryAlignment* (Panel A) as well as statistical comparisons across experimental conditions of *FreqComments*, *ToneComments*, and *NumberMeetings* (Panel B). *OwnMoney* is a continuous variable equal to the participant's total reported purchases of Starbucks products. *OthersMoney* is an indicator variable equal to one if the participant votes for Starbucks beverages to be supplied at a business school event during the academic term and zero otherwise. *RegulatoryAlignment* is a continuous variable equal to 100 minus the participant's reported likelihood of voting for a $15 per hour statewide minimum wage (on a 101-point scale). *Treatment* is an indicator variable equal to one if the participant is assigned ownership in Starbucks and zero otherwise.

WorkGroupTime is a continuous variable equal to the number of hours a week the participant spends meeting with members of her workgroup throughout the academic term. *StarbucksLinks* is a discrete variable ([0 to 3]) equal to the number of Starbucks investors (out of a maximum of three) in their graduate program with whom the participant spends the most non-classwork-related time. *NumberMeetings* is a continuous variable equal to the number of times a month the participant meets family, friends, or colleagues at a Starbucks location during the academic term. *FreqComments* is a continuous variable equal to the number of times a month the participant talked to individuals outside of class about Starbucks during the academic term. *ToneComments* is a discrete variable ([-3 to +3]) corresponding to a scale *Very Negative* to *Very Positive* for the question, "When you talk about Starbucks, what is typically the tone of your comments?" All non-indicator variables are standardized to have a mean of zero and standard deviation of one within each university. *, **, and *** denote statistical significance at the two-tailed 10%, 5%, and 1% levels, respectively.

T A B L E 6

Other investor preferences and perceptions

	Starbucks investors (n)	Control investors (n)	Mean difference	t-Statistic (p-Value)
ReportingConfidence	-0.075	0.073	-0.148	-1.20
	(127)	(131)		(0.233)
PerceivedConsistency	-0.034	0.033	-0.068	-0.55
	(127)	(131)		(0.585)
EarningsBeatLikelihood	-0.014	0.014	-0.028	-0.23
	(127)	(131)		(0.820)

Table 6 presents the results of planned supplementary analyses that compare *ReportingConfidence*, *PerceivedConsistency*, and *EarningsBeatLikelihood* across experimental conditions. *ReportingConfidence* is a discrete variable ([0 to 100]) corresponding to a scale *Not at All Confident* to *Very Confident* for the question, "How confident are you that Starbucks' financial statements accurately depict the financial performance and position of Starbucks?" *PerceivedConsistency* is a discrete variable ([0 to 100]) corresponding to a scale *Not at All Consistent* to *Very Consistent* for the question, "How consistent do you believe Starbucks' financial reporting choices are over time?" *EarningsBeatLikelihood* is a discrete variable ([0 to 100]) corresponding to a scale *Not at All Likely* to *Very Likely* for the question, "How likely is it that Starbucks will exceed market expectations when it reports earnings next month?" All non-indicator variables are standardized to have a mean of zero and standard deviation of one within each university. *, **, and *** denote statistical significance at the two-tailed 10%, 5%, and 1% levels, respectively.

T A B L E 7

Bayesian analyses for OwnMoney, OthersMoney, and RegulatoryAlignment

Panel A: *OwnMoney*

	Prior mean	Prior variance	Posterior mean	Posterior variance	Posterior 95% interval
Enthusiastic, high confidence	40	10	36.49	8.62	30.72 – 42.34
Enthusiastic, low confidence	40	100	21.70	47.28	8.18 – 35.14
Skeptical, high confidence	0	10	0.39	8.85	-5.32 – 6.29
Skeptical, low confidence	0	100	2.45	48.29	-10.99 – 16.39

Panel B: *OthersMoney*

	Prior mean	Prior variance	Posterior mean	Posterior variance	Posterior 95% interval
Enthusiastic, high confidence	0.2	0.01	0.180	0.0088	0.001 – 0.371
Enthusiastic, low confidence	0.2	0.1	0.113	0.0403	-0.265 – 0.508
Skeptical, high confidence	0	0.01	0.005	0.0091	-0.175 – 0.190
Skeptical, low confidence	0	0.1	0.024	0.0388	-0.359 – 0.418

Panel C: *RegulatoryAlignment*

	Prior mean	Prior variance	Posterior mean	Posterior variance	Posterior 95% interval
Enthusiastic, high confidence	20	10	12.94	6.44	8.00 – 17.76
Enthusiastic, low confidence	20	100	2.98	14.54	-4.59 – 10.49
Skeptical, high confidence	0	10	0.01	6.43	-5.01 – 4.97
Skeptical, low confidence	0	100	-0.01	15.66	-8.00 – 7.87

Table 7 presents the results of planned Bayesian parameter estimation, a supplementary analysis, for each dependent variable (*OwnMoney*, *OthersMoney*, or *RegulatoryAlignment*). Each panel presents the parameters of various prior beliefs, the posterior beliefs after Bayesian updating, and the 95% highest posterior density interval. For Panels A and C, we assume an uninformative prior for the intercept, a normal likelihood function with an uninformative prior of the function's variance, and a prior for the slope coefficient distributed normally. *OwnMoney* is a continuous variable equal to the participant's total reported purchases of Starbucks products. *OthersMoney* is an indicator variable equal to one if the participant votes for Starbucks beverages to be supplied at a business school event during the academic term and zero otherwise. *RegulatoryAlignment* is a continuous variable equal to 100 minus the participant's reported likelihood of voting for a $15 per hour statewide minimum wage (on a 101-point scale). *OwnMoney* and *RegulatoryAlignment* are not standardized for these analyses.