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Understanding Mechanisms Underlying Peer Effects: Evidence from a Field Experiment on Financial Decisions[*]

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Abstract

Using a high-stakes field experiment conducted with a financial brokerage, we implement a novel design to separately identify two channels of social influence in financial decisions, both widely studied theoretically. When someone purchases an asset, his peers may also want to purchase it, both because they learn from his choice ("social learning") and because his possession of the asset directly affects others' utility of owning the same asset ("social utility"). We randomize whether one member of a peer pair who chose to purchase an asset has that choice implemented, thus randomizing his ability to possess the asset. Then, we randomize whether the second member of the pair: (1) receives no information about the first member, or (2) is informed of the first member's desire to purchase the asset *and* the result of the randomization that determined possession. This allows us to estimate the effects of learning plus possession, and learning alone, relative to a (no information) control group. We find that both social learning and social utility channels have statistically and economically significant effects on investment decisions. Evidence from a follow-up survey reveals that social learning effects are greatest when the first (second) investor is financially sophisticated (financially unsophisticated); investors report updating their beliefs about asset quality after learning about their peer's revealed preference; and, they report motivations consistent with "keeping up with the Joneses" when learning about their peer's possession of the asset. These results can help shed light on the mechanisms underlying herding behavior in financial markets and peer effects in consumption and investment decisions.

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1 Introduction

People's choices often look like the choices made by those around them: we wear what is fashionable, we "have what they're having," and we try to "keep up with the Joneses." Such *peer effects* have been analyzed across fields in economics.[1] Motivated by concerns over herding and financial market instability, an especially active area of research has examined the role of peers in financial decisions. Beyond studying *whether* peers affect financial decisions, different *channels* through which peer effects work have generated their own literatures linking peer effects to investment decisions, and to financial market instability. Models of herding and asset-price bubbles, potentially based on very little information, focus on *learning* from peers' choices (Bikhchandani and Sharma, 2000; Chari and Kehoe, 2004). Models in which individuals' relative income or consumption concerns drive their choice of asset holdings, and artificially drive up some assets' prices, focus on peers' *possession* of an asset.[2] In this paper, we use a high-stakes field experiment, conducted with a financial brokerage, to separately identify the causal effects of these channels through which a person's financial decisions are affected by his peers'.

Identifying the causal effect of one's peers' behavior on one's own is notoriously difficult (see, for example, Manski, 1993). Equally difficult is identifying *why* one's consumption or investment choices have a social component. Broadly, there are two reasons why a peer's act of purchasing an asset (or product, more generally) would affect one's own choice. First, one may infer that assets (or products) purchased by others are of higher quality; we refer to this as *social learning*. Second, one's utility from possessing an asset (or product) may depend directly on the possession of that asset (or product) by another individual; we call this *social utility*.

Suppose an investor i considers purchasing a financial asset under uncertainty. In canonical models of herding based on social learning, information that a peer, investor j, purchased the asset will provide favorable information about the asset to investor i: investor j (acting in isolation) would only have purchased the asset if he observed a relatively good signal of the asset's return. The favorable information conveyed by investor j's revealed preference increases the probability that investor i purchases the asset, relative to making a purchase decision in isolation.[3]

A direct effect of investor j's possession of a financial asset on investor i's utility might arise for

[1]Seminal theoretical articles include Banerjee (1992) and Bikhchandani et al. (1992). Early empirical research includes Case and Katz (1991), Katz et al. (2001), Sacerdote (2001), and Zimmerman (2003). Durlauf (2004) surveys the literature on neighborhood effects. Peer effects have also been studied by psychologists and sociologists: influential social psychology research includes Asch (1951) and Festinger (1954); a review of empirical research on peer effects in sociology is presented in Jencks and Mayer (1990).

[2]Preferences over relative consumption can arise from the (exogenous) presence of other individuals' consumption decisions in one's utility function, (e.g. Abel, 1990, Gali, 1994, Campbell and Cochrane, 1999) or can arise endogenously when one consumes scarce consumption goods, the prices of which depend on the incomes (and consumption and investment decisions) of other individuals (DeMarzo et al., 2004, DeMarzo et al., 2008). For an overview, see Hirshleifer and Teoh (2003).

[3]Avery and Zemsky (1998) present a model in which prices adjust in response to herding behavior; however, in our setting there is no asset price adjustment (see also Chari and Kehoe, 2004).

a variety of reasons widely discussed in the finance literature. First, investors may be concerned with their incomes or consumption levels, relative to their peers' ("keeping up with the Joneses", as in Abel, 1990, Gali, 1994, and Campbell and Cochrane, 1999).[4] Second, investor j's possession of a financial asset may affect investor i's utility through "joint consumption" of the asset: peers can follow and discuss financial news together, track returns together, etc. (Taylor, 2011, describes the popularity of "investment clubs" in the 1990s). The impact of a peer's possession of an asset on an individual's utility derived from owning the same asset (for multiple reasons) is the social utility channel.[5]

Typically, investor j's decision to purchase the asset will also imply that investor j possesses the asset. Thus, a comparison of investor i's investment when no peer effect is present to the case in which he observes investor j purchasing an asset will generally identify the *combined* social learning and social utility channels. To disentangle social learning from social utility, one needs to identify, or create, a context in which investor j's decision to purchase an asset is decoupled from investor j's possession of the asset.[6]

Our experimental design (discussed in detail in Section 2) represents an attempt to surmount both the challenge of identifying a causal peer effect, and the challenge of separately identifying the effects of social learning and social utility. Working closely with us, a large financial brokerage in Brazil offered a new financial asset to pairs of clients who share a social relationship. The stakes were high: minimum investments were R$2,000 (over $1,000 U.S. dollars at the time of the study), around 50% of the median investor's monthly income in our sample.

To identify any sort of peer effect on investment decisions, we randomly informed one member of the peer pair, investor 2, of the investment made by the other member of the pair, investor 1 (assignment to the roles of investor 1 and investor 2 was random). To disentangle the effect of investor 1's possession from the effect of the information conveyed by investor 1's revealed preference, we exploit a novel aspect of our experimental design. The financial brokerage with which we worked implemented a *lottery* to determine whether individuals who chose to purchase the asset would actually be allowed to make the investment (see Figure 1 for a graphical depiction of the experimental design). Thus, half of the investor 1's who chose to purchase the asset revealed a preference for the financial asset, but *did not* possess it.

Among investor 1's who chose to purchase the asset, we implemented a second, independent randomization to determine the information received by the associated investor 2's: we randomly

[4]Evidence consistent with individuals caring about relative outcomes has been presented by Luttmer (2005), Fliessbach et al. (2007), and Card et al. (2010), among others.

[5]Note that even in the absence of truly "social" preferences, one might observe greater demand for an asset simply because a peer holds it: for example, this might arise as a result of competition over scarce consumption goods. Because we do not wish to abuse the term, "social preferences," we prefer the broader term, "social utility." Note also that social utility might lead to *negative* correlations between peers' choices (see Clark and Oswald, 1998); for example, one might observe a demand for joint insurance (see, e.g., Angelucci et al., 2012).

[6]In Appendix B (all appendices are online) we present a model of peer effects in financial decisions that features both the social learning and social utility channels, formalizing this discussion.

assigned investor 2 to receive either *no information* about investor 1's investment decision, or to receive information about *both* the investment decision *and* the outcome of the lottery determining possession. Thus, among investor 1's who chose to purchase the asset, the associated investor 2's were randomly assigned to one of three conditions: in condition A, no information about investor 1's decision was provided; in condition B, investor 2's received information that investor 1 made a decision to purchase the asset, but was not able to consummate the purchase (so learning occurred *without possession*); and, in condition C, investor 2's received information that investor 1 made a decision to purchase the asset, and was able to consummate the purchase (so learning occurred, along with possession). A comparison of choices made by investor 2's in conditions A and B reveals the effect of social learning; a comparison of conditions B and C reveals the impact of investor 1's possession of the asset over and above the information conveyed by his purchase, that is, social utility; a comparison of conditions A and C reveals the total effect of these two channels.

Our experimental evidence suggests that *both* channels through which peer effects work are economically and statistically significant. Among investor 2's whose peer chose to purchase the asset we find the following: in the "no information" condition A, 42% chose to purchase the asset; in the "social learning only" condition B, the take-up rate increased to 71%; finally, in the "social learning plus social utility" condition C, the rate increased to 93%. Not only do individuals learn from their peers, but there is also an effect of possession beyond learning.

To better understand investors' decision making in the different conditions, and to help us evaluate alternative interpretations of the treatment effects we observe, we partnered with the brokerage to conduct a follow-up survey of the investors in the study (see Section 2.4 for details). We first analyze the social learning channel, presenting evidence of positive belief updating among investor 2's who learned about their peers' purchase decisions, and of heterogeneous social learning effects consistent with a model in which unsophisticated investors learn more from others' purchases, and sophisticated investors' purchases are more influential. We also find evidence suggestive of social utility concerns among investors who chose to purchase the asset in condition C. The evidence from the follow-up survey additionally helps us rule out several alternative interpretations of the treatment effects we observe, as well as confounding factors (we discuss alternative hypotheses and limitations of our study further in Section 3).

Our work contributes most directly to the empirical literature on peer effects in investment decisions, some observational (e.g., Hong et al., 2004, Hong et al., 2005, Ivkovic and Weisbenner, 2007, Brown et al., 2008, Li, 2009, and Banerjee et al., 2011), some experimental (e.g., Duflo and Saez, 2003, Beshears et al., 2011). Our paper goes beyond the existing literature by using experimental variation to separately identify the causal roles of different *channels* of peer effects. Disentangling these channels is of more than academic interest: it can provide important, policy-relevant evidence on the sources of herding behavior in financial markets. Our findings of significant social learning *and* social utility effects suggest that greater information provision might mitigate

– but not eliminate – herding behavior.

Our paper also contributes to the broader empirical literature on social learning and peer effects.[7] As in our work, several recent papers use information shocks to identify causal peer effects (e.g., Frey and Meier, 2004, Chen et al., 2010, Ayres et al., 2009, Costa and Kahn, 2010, and Allcott, 2011). However, the information shocks they exploit do not allow for the separate identification of the channels through which peer effects work. Identifying the effect of social learning alone is the focus of Cai et al. (2009) and Moretti (2011); they try to rule out the existence of peer effects through other channels (e.g., joint consumption), but they do not experimentally manipulate the social utility channel. Cai et al. (2012) use experimental variation in the field to identify the effects of different types of social learning; Maertens (2012) uses non-experimental methods to study different channels of social influence; Cooper and Rege (2011) attempt to distinguish among peer effect channels in the lab. Our work is the first we know of that uses experimental variation in the field to isolate the effect of social learning *and* the separate effect of social utility. Our results corroborate models of social learning such as Banerjee (1992) and Bikhchandani et al. (1992), but indicate that peers' purchasing decisions have effects beyond social learning as well.

Finally, our experimental design, which allows us to separately identify the channels through which peer effects work, represents a methodological contribution. As we discuss in the conclusion, our design could be applied toward the understanding of social influence in marketing, technology adoption, and health-promoting behavior.

The paper proceeds as follows: in Section 2, we describe in detail our experimental design, which attempts to separately identify the channels through which peer effects work; in Section 3, we present our empirical specification and the results of our experiment, and discuss our findings; finally, in Section 4, we offer concluding thoughts.

2 Experimental Design

The primary goal of our design was to decouple a peer's decision to purchase the asset from his possession of the asset. We generated experimental conditions in which individuals would make decisions: 1) uninformed about any choices made by their peer; 2) informed of their peer's revealed preference to purchase an asset, but the (randomly determined) inability of the peer to make the investment; and, 3) informed of their peer's revealed preference to purchase an asset, and the peer's (randomly determined) successful investment.

[7] Empirical work on peer effects has studied a wide range of outcomes, for example, education, compensation, and charitable giving (Sacerdote, 2001; Carrell and Hoekstra, 2010; de Giorgi et al., 2010; Duflo et al., 2011; Card and Giuliano, 2011; Shue, 2012; DellaVigna et al., 2012); the impact of one's peers and community on social indicators and consumption (Bertrand et al., 2000; Kling et al., 2007; Bobonis and Finan, 2009; Dahl et al., 2012; Grinblatt et al., 2008; Kuhn et al., 2011); and, the impact of coworkers on workplace performance (Guryan et al., 2009; Mas and Moretti, 2009; Bandiera et al., 2010). Herding behavior and informational cascades (Celen and Kariv, 2004) and the impact of cultural primes on behavior (Benjamin et al., 2010) have been studied in the lab.

2.1 Designing the Asset

The asset being offered needed to satisfy several requirements. Most fundamentally, there needed to be a possibility of learning from one's peers' decisions. In addition, because many of our comparisons of interest are among investor 2's whose associated investor 1's chose to purchase the asset, the asset needed to be sufficiently desirable that *enough* investor 1's would choose to purchase it. To satisfy these requirements, the brokerage created a new, risky asset specifically for this study. The asset is a combination of an actively-managed, open-ended long/short mutual fund and a real estate note (*Letra de Crédito Imobiliário*, or LCI) for a term of one year. The long/short fund seeks to outperform the interbank deposit rate (CDI, *Certificado de Depósito Interbancário*) by allocating investment funds to fixed-income assets, equity securities, and derivatives. The LCI is a low-risk asset that is attractive to personal investors because it is exempt from personal income tax; it can be thought of as an appealing, high-yield CD.

The LCI offered in this particular combination had somewhat better terms than the real estate notes that were usually offered to clients of the brokerage, thus generating sufficient demand to meet the experiment's needs. First, the return of the LCI offered in the experiment was 98% of the CDI, while the best LCI offered to clients outside of the experiment had a return of 97% of the CDI. In addition, the brokerage firm usually required a minimum investment of R$10,000 to invest in an LCI, while the offer in the experiment reduced the minimum investment threshold to R$1,000 (the long/short fund also required a minimum investment of R$1,000). The brokerage piloted the sale of the asset (without using a lottery to determine possession), to clients other than those in the current study, in order to ensure a purchase rate of around 50%.

Another requirement was that there be no secondary market for the asset, for several reasons. First, we hoped to identify the impact of learning from peers' decisions to purchase the asset, rather than learning from peers based on their experience possessing the asset. Investor 2 may have chosen not to purchase the asset immediately, in order to talk with investor 1, then purchase the asset from another investor. We wished to rule out this possibility. In addition, we did not want peer pairs to jointly make decisions about selling the asset. Finally, we did not want investor 2 to purchase the asset in hopes of selling it to investor 1 when investor 1's investment choice was not implemented by the lottery. In response to these concerns, the brokerage offered the asset only at the time of their initial phone call to the client and structured the asset as having a fixed term with no resale.

A final requirement, given our desire to decouple the purchase decision from possession, was that there must be limited entry into the fund to justify the lottery to implement purchase decisions. The brokerage was willing to implement the lottery design required, justified by the supply constraint for the asset they created. At the individual level, the maximum investment in the LCI component was set at R$10,000.

2.2 Selling the Asset

To implement the study, we designed (in consultation with the financial brokerage) a script for sales calls that incorporated the randomization necessary for our experimental design (the translated script is available in Appendix C).[8] The brokerage required that calls be as natural as possible: sales calls had frequently been made by the brokerage in the past, and our script was made as similar as possible to these more typical calls. In addition, the experimental calls were made by the individual brokers who were accustomed to working with the clients they called as part of the study. Thus, we (and the brokerage) expected that clients would trust the broker's claims about their peer's choices, and to believe that the lottery would be implemented as promised.

Between January 26, and April 3, 2012, brokers called 150 pairs of clients whom the brokerage had previously identified as having a social connection (48% are members of the same family, and 52% are friends; see Appendix Table A.1).[9] Information on these clients' social relationships was available for reasons independent of the experiment: the firm had made note of referrals made by clients in the past. This is particularly important because clients' social relationships would not have been salient to those whose sales call did not include any mention of their peer. We thus believe that without any mention of the offer being made to the other member of the peer pair, there should be practically no peer effect.[10]

One member of the pair was randomly assigned to the role of "investor 1," and the other assigned to the role of "investor 2."[11] Investor 1 was called by the brokerage and given the opportunity to invest in the asset without any mention of their peer. The calls proceeded as follows. The asset was first described in detail to investor 1. After describing the investment strategy underlying the asset, the investor was told that the asset was in limited supply; in order to be fair to the brokerage's clients, any purchase decision would be confirmed or rejected by computerized lottery (this is not as unusual as it may appear; for example, Instefjord et al. (2007) describe the use of lotteries to allocate shares when IPO's are oversubscribed). If the investor chose to purchase the asset, he was asked to specify a purchase amount (investors were not allowed to convert existing

[8]We created the script using Qualtrics, a web-based survey platform. Occasionally, Qualtrics was abandoned when the website was not accessible, and the brokers used Excel to generate the randomization needed to execute the experimental design. Treatment effects are very similar if we restrict ourselves to the Qualtrics calls (results available upon request).

[9]The sample size was limited by the number of previously-identified socially-related pairs of clients, as well the brokerage's willingness to commit time to the experiment. The brokerage agreed (in advance of the calls) to reach 300 clients. A photo of the brokerage making sales calls as part of the experiment (with our research assistant present) is included in Appendix C.

[10]We also asked the brokerage if any investor spontaneously mentioned their peer in the sales call, and the brokerage indicated that this never occurred. If an individual in condition A had thought about his peer's potential offer and purchase of the same asset, our measured peer effects would be attenuated.

[11]A comparison of the characteristics of investor 1's and investor 2's can be seen in Appendix Table A.2, columns 1 and 2. The randomization resulted in a reasonable degree of balance across groups: 4 of 5 tests of equality of mean characteristics across groups have p-values above 0.10. One characteristic, gender, is significantly different across groups.

investments with the brokerage, and thus allocated new resources in order to purchase the asset). Then, a computer would generate a random number from 1 to 100 (during the phone call), and if the number was greater than 50, the investment would be authorized.[12]

Following the call to investor 1, the same broker called the associated investor 2. The brokers were told that, for each pair, both investors had to be contacted *on the same day* to avoid any communication about the asset that might contaminate the experimental design. Only 6 out of 150 investor 2's had communicated with their associated investor 1's about the asset prior to the phone call from the brokerage (dropping these 6 observations does not affect any of our results). If the broker did not succeed in reaching investor 2 on the same day as the associated investor 1, the broker would not attempt to contact him again; this outcome occurred for 12 investor 1's, who are not included in our empirical analysis. Thus, brokers called 162 investor 1's in order to attain our sample size of 150 pairs successfully reached.

When the broker reached investor 2, he began the script just as he did for investor 1: describing the asset, including the lottery to determine whether a purchase decision would be implemented. Next, during the call, the broker implemented the experimental randomization and attempted to sell the asset under the experimentally-prescribed conditions (described next). If investor 2 chose to purchase the asset, a random number was generated to determine whether the purchase decision would be implemented, just as was the case for investor 1.

2.3 Randomization into Experimental Conditions

The experimental conditions were determined as follows. Among the group of investor 1's who chose to purchase the asset, their associated investor 2's were randomly assigned to receive information about investor 1's choice and the lottery outcome, or to receive no information. There was thus a "double randomization" – first, the lottery determining whether investor 1 was able to make the investment, and second, the randomization determining whether investor 2 would be informed about investor 1's investment choice and the outcome of the first lottery.

This process assigns investor 2's whose associated investor 1's chose to purchase the asset into one of three conditions (refer to Figure 1); investor characteristics across the three experimental conditions can be seen in Table 1 (we generally present means of the various investor characteristics, with the exception of the earnings variable, the median of which is shown in order to mitigate the influence of outliers). One-third were assigned to the "no information," control, condition A. Half of these come from the pool of investor 2's paired with investor 1's who wanted the asset but were not authorized to make the investment, and half from those paired with investor 1's who wanted

[12] Among investor 1's who wanted to purchase the asset, a comparison of the characteristics of investor 1's whose purchase decision was authorized and investor 1's whose purchase decision was not authorized can be seen in Appendix Table A.2. The randomization resulted in a reasonable degree of balance across groups: 5 of 6 tests of equality of mean characteristics across groups have p-values above 0.10. One characteristic, gender, is significantly different across groups.

to make the investment and were authorized to make it. Investor 2's in condition A were offered the asset just as was investor 1, with no mention of an offer made to their peer.

Two-thirds received information about their peer's decision to purchase the asset (but not the magnitude of the desired investment), as well as the outcome of the lottery that determined whether the peer was allowed to invest. The randomization resulted in approximately one-third of investor 2's in condition B, in which they were told that their peer purchased the asset, but had that choice rejected by the lottery. The final third of investor 2's were in condition C, in which they were told that their peer purchased the asset, and had that choice implemented by the lottery.

The three conditions of investor 2's whose associated investor 1's wanted to purchase the asset are the focus of our analysis. Given the double randomization in our experimental design, investor 2's in conditions A, B, and C should have similar observable characteristics, and should differ only in the information they received. As a check of the randomization, we present in Table 1 the individual investors' characteristics for each of the three groups, as well as tests of equality of the characteristics across groups. As expected from the random assignment, the sample is well balanced across the baseline variables.

Along with the three conditions of interest, in some analyses we will consider those investor 2's whose associated investor 1 chose not to invest in the asset (the characteristics of these investor 2's can be seen in Appendix Table A.1, column 7). We assign these investor 2's to their own "negative selection" condition A^{neg}, in which they receive no information about their peer. We did not reveal their peers' choices because the brokerage did not want to include experimental conditions in which individuals learned that their peer *did not* want the asset. These individuals were offered the asset in exactly the same manner as were investor 1's and investor 2's in condition A. We refer to this condition as "negative selection," because the investor 2's in condition A^{neg} are those whose peers specifically chose *not* to purchase the asset.

Our experimental design allows us to estimate overall peer effects, and to disentangle the channels through which peers' purchases affect investment decisions. A comparison of those in conditions A and C reveals the standard peer effect. A comparison of investors in conditions A and B allows us to estimate the impact of social learning resulting from a peer's decision *but without possession.* Comparing investor 2's in conditions B and C will then allow us to estimate the impact of a peer's *possession alone*, over and above learning from a peer's decision.[13] In addition to identifying these peer effects, we will examine the role of selection into peer pairs according to preferences, by comparing investor 2's in condition A to those in the condition A^{neg}.

[13]It is important to emphasize that our estimated effect of possession is conditional on investor 2 having learned about the asset from the revealed preference of investor 1 to purchase the asset. One might imagine that the effect of possession of the asset by investor 1 *without* any revealed preference to purchase the asset could be different. It is also important to point out that the estimated effect of "possession" is difficult to interpret quantitatively: the measured effect is bounded above by 1 minus the take-up in condition B, working against finding any statistically significant peer effects beyond social learning.

2.4 Follow-up Survey

Between November 26, and December 7, 2012, the brokerage conducted a follow-up survey with a subset of the clients from the main study; investors were told (truthfully) that the brokerage wished to learn about its clients in order to provide them with more individualized services and information. The follow-up survey was conducted with two primary goals for the purposes of our work: first, to measure investors' financial sophistication; and second, to collect information that could be used to better understand the decision making processes behind the choices of investor 2's (for the English language survey questionnaires, see Appendix C).

In our analysis below, we examine heterogeneity in social learning effects among investor 2's in conditions A and B, depending on whether investor 2, or the associated investor 1, is financially sophisticated. To measure the relevant set of investors' financial sophistication, the brokerage contacted the investor 2's in conditions A and B, as well as their associated investor 1's, and asked them to assess their own financial knowledge; in addition, the brokerage asked these investors a series of objective questions measuring financial literacy.[14] For summary statistics on the financial sophistication survey questions, see Appendix Table A.3, Panel A.

We also collected survey evidence that can help us understand the decision making of investor 2's across experimental conditions A, B, and C. In particular, we asked about several aspects of investors' decisions: (i) how investors viewed the lottery that determined whether purchase decisions were implemented (surveying investors in conditions A, B, and C); (ii) how investors responded to information about their peer's purchase decision and lottery result, as well as whether the information provided by brokers was credible (surveying investors in conditions B and C); (iii) whether investors' decisions were specifically affected by their peer's lost lottery (condition B); and, (iv) whether social utility considerations affected investors' decisions to purchase the asset (investors in condition C who chose to purchase the asset). For summary statistics on the decision making survey questions, see Appendix Table A.3, Panel B.

It is important to highlight two weaknesses of the follow-up survey. First, investors may have responded in ways that they thought would please the surveyor. It is important to note, however, that the vast majority (over 90%) of survey calls were *not* made by the investor's usual broker, but by another broker at the firm, with whom investors did not have a personal relationship. This mitigates concerns about surveyor demand effects (results are very similar excluding surveys in which the survey was conducted by the broker who made the experimental sales call, see Appendix Table A.4). In addition, many of the questions asked, such as those regarding financial sophistication or the updating of beliefs, did not have an answer that would be viewed more favorably by

[14]The specific questions come from the National Financial Capability Survey (translated into Portuguese), and have been used in studies both in the US and in other countries (Lusardi and Mitchell, 2011a,b). Investor 2's other than those in conditions A and B, and investor 1's other than those associated with investor 2's in conditions A and B, were not asked these financial sophistication questions to reduce the brokerage's time commitment to the follow-up survey.

the brokerage. Second, the questions aimed at understanding investors' decision making were not open-ended, but were directed toward the mechanisms of interest. This was necessary in order to limit the time committed by the brokerage (and investors) to the follow-up survey, and to reduce the amount of noise present in the survey responses. These weaknesses should be kept in mind when interpreting the follow-up survey evidence.

The brokerage conducted the follow-up survey over the phone, calling investor 2's in conditions A, B, and C, and investor 1's associated with investor 2's in conditions A and B, up to three times each; the brokerage was able to reach 90.4% of the investors called.

3 Empirical Analysis

3.1 Regression Specification

To identify the experimental treatment effects, we estimate regression models of the following form:

$$Y_i = \alpha + \sum_c \beta_c I_{c,i} + \gamma' \mathbf{X}_i + \epsilon_i. \tag{1}$$

Y_i is an investment decision made by investor i: in much of our analysis it is a dummy variable indicating whether investor i wanted to purchase the asset, but we also consider the quantity invested, as well as an indicator that the investment amount was greater than the minimum required. The variables $I_{c,i}$ are indicators for investor i being in category c, where c indicates the experimental condition to which investor i was assigned. In all of our regressions, the omitted category of investors to which the others are compared is investor 2's in condition A: investor 2's associated with a peer who wanted to purchase the asset, but who received no information about their peer. In much of our analysis, we focus on investor 2's, so $c \in \{\text{condition } B, \text{condition } C, \text{condition } A^{neg}\}$. In some cases, we include investor 1's in our analysis, and they will be assigned their own category c. Finally, in some specifications we include control variables: X_i is a vector that includes broker fixed effects and investor characteristics.

3.2 Empirical Estimates of Peer Effect Channels

We first present the treatment effects of interest using an indicator of the investor's purchase decision as the outcome variable, and various specifications estimated using OLS, in Table 2 (results are very similar using probit or logit models; see Appendix Tables A.5 and A.6). We begin by estimating a model using only investor 2's and not including any controls, in Table 2, column 1. These results are equivalent to comparing means in the raw data (which are presented in Appendix Figure A.1). Treatment effects are estimated relative to the omitted category, investor 2's in condition A, who had a take-up rate of 42%. In the "social learning alone" condition B, the take-up rate increased to 71%, and the nearly 30 percentage point increase is statistically significant; in the "social learning plus social utility" condition C, the take-up rate was 93%, significantly larger than the take-up rate in *both* conditions A and B.[15] These differences represent economically and statistically significant overall peer effects and indicate that social learning *without possession* affects the investment decision, as does possession *beyond* social learning. Finally, the coefficient on the indicator for condition A^{neg} is economically small, and it is not statistically significant, suggesting that "selection" effects are small in our setting.

A natural question about Table 2 is whether our statistical inferences are sound, given the relatively small number of observations in each experimental condition. As an alternative to standard t-tests to determine statistical significance, we ran permutation tests with 10,000 repetitions for pairwise comparisons of take-up rates across conditions A, B, C, and A^{neg}. To run the permutation tests, we randomly assign "placebo treatment" status to investors in the conditions of interest, 10,000 times, and calculate a distribution of "placebo treatment effects." We then compare the size of the treatment effects we find using the *actual* treatment assignment to the distribution of "placebo treatment effects." While the permutation test is *not* an exact test, it can complement our inferences using t-tests. For our main comparisons, we find p-values that are trivially larger using permutation tests than using t-tests, but our inferences are unchanged, suggesting that inferences using t-tests are valid (see Appendix Table A.8, Panel A, column 1).

We next present regression results including broker fixed effects (Table 2, column 2) and including both broker fixed effects and baseline covariates (column 3); then, we estimate a regression including these controls and using the combined sample of investor 1's and investor 2's in order to have more precision (column 4). The overall peer effect, as well as the individual social learning and social utility channels, estimated using these alternative models are very similar across specifications (consistent with successful randomization across conditions).[16]

We now delve more deeply into our data, and analyze investors' responses to the follow-up survey, in order to better understand the treatment effects we observe. We first present additional evidence on each of the two channels of social influence we study. Then, we discuss potential concerns with our experimental design and the interpretation of our results.

[15] The p-value from a test of equality between take-up rates in conditions A and C – the overall peer effect – is 0.000. The p-value from a test that condition B equals condition A – social learning alone – is 0.043. The p-value from a test that condition C equals condition B – possession's effect above social learning – is 0.044. Note that one might wish to compare take-up rates in conditions B and C to a broader "no information" control group than condition A. We use data on investors 1's to estimate the take-up rate of positively-selected individuals using GMM, imposing the overidentifying restriction that investor 1's take-up rate is a weighted average of investor 2's in conditions A and A^{neg}. While social learning effects are smaller, our results are qualitatively unchanged (see Appendix Table A.7). We prefer using individuals in condition A as the control group as it is most internally valid: investor 1's calls came earlier in the day than calls to investor 2's, and did not include the information randomization that was part of the calls made to the latter (a test of the overidentifying restriction in the GMM model also nearly rejects the null).

[16] Examining alternative outcomes – the amount investors chose to invest in the asset, or a dummy variable indicating whether the investment amount was greater than the minimum required – yields very similar results (see Appendix Figure A.2 and Table A.9; for p-values calculated using permutation tests, see Table A.8, Panel A, columns 2–3).

3.3 Understanding the Social Learning Treatment Effects

Heterogeneity of social learning effects by financial sophistication. When observing a peer's purchase of an asset, an investor with greater financial sophistication (and therefore a more precise signal of asset quality) should put less weight on information derived from their peer's revealed preference, relative his own signal of the quality of the asset. Similarly, the information conveyed by the revealed preference of one's peer should be more influential if this peer is more financially sophisticated, and is thus likely to have received a more precise signal of the asset's quality (see Appendix B for a formal treatment of these arguments). We thus expect that the social learning channel should be more important for less financially sophisticated investor 2's, and when investor 1's were more financially sophisticated.

It is important to keep in mind that financial sophistication is not randomly assigned in our study, so it might be correlated with some other, unobserved characteristic. In addition, testing for heterogeneous treatment effects divides our sample into small cells; thus, evidence of heterogeneous treatment effects should be interpreted cautiously. Small cell sizes also prevent us from combining into a single analysis the study of social learning *by* sophisticated and unsophisticated investors with the study of social learning *from* sophisticated and unsophisticated investors. Still, exploring heterogeneous treatment effects is both interesting from a theoretical standpoint – since it is a natural extension of a social learning framework – and it can also provide suggestive evidence that our measured social learning treatment effects are not driven by other factors.

We construct two measures of investors' financial sophistication using responses to several questions included in our follow-up survey (for the English version of the questionnaire used in the survey, see Appendix C; for summary statistics on the financial sophistication survey questions, see Appendix Table A.3, Panel A). Our first measure captures investors' self-assessments of their financial sophistication, on a 1 to 7 scale. We define "financially sophisticated" investors as those who reported a number greater than or equal to 4, producing the most even split of our sample. Our second measure captures investors' objective financial knowledge, based on four questions testing respondents' understanding of important concepts for investing: compounding; inflation; diversification; and, the relationship between bond prices and interest rates. The objective measure defines "financially sophisticated" investors as those who correctly answered 3 or more questions, again producing the most even split of our sample. Our two measures of financial sophistication have a correlation of around 0.4 within-investor; across peers in each pair, the correlation is just 0.06 for the self-assessed measure, and -0.11 for the objective measure. For brevity, in the text we will present tests of heterogeneity in social learning across both investor 1's and investor 2's levels of financial sophistication using only the self-assessed measure (in Appendix Table A.10, we present the same specifications shown in the text, but using the objective measure, and our results are very similar).

We present regression estimates of social learning effects (estimated from the sample of investor

2's in conditions A and B), with the take-up rate as the outcome variable, for different categories of investor 2's. In Table 3, Panel A, we present social learning treatment effects estimated from regressions without controls (i.e., comparisons of means). In columns 1–2, we estimate separate social learning effects for financially sophisticated and financially unsophisticated investor 2's, respectively. We regress the investment decision dummy variable on a "financially sophisticated" indicator; an interaction between a condition B indicator and the financially sophisticated indicator; and, an interaction between a condition B indicator and a "financially unsophisticated" indicator. In columns 4–5, we estimate separate social learning effects for investor 2's associated with financially sophisticated and financially unsophisticated investor 1's, respectively. We estimate regressions analogous to columns 1–2, but substitute indicators of the associated investor 1's financial sophistication for the indicators of investor 2's financial sophistication. Panel B presents estimated social learning effects from models that include baseline controls and broker fixed effects.

The results in Table 3 match our predictions. First, in columns 1–2, we observe small, statistically insignificant social learning effects on financially sophisticated investor 2's, and large, significant effects on unsophisticated investor 2's.[17] Column 3 shows that the difference between the treatment effects for sophisticated and unsophisticated investor 2's is also statistically significant. Next, in columns 4–5, we find large, statistically significant social learning effects among investor 2's associated with financially sophisticated investor 1's, and small, insignificant effects among investor 2's associated with financially unsophisticated investor 1's (take-up rates across sub-groups are presented in Appendix Figures A.3.1 and A.3.2). We find results that are very similar using the objective measure of financial knowledge (see Appendix Table A.10) or using alternative outcomes (amount invested or an indicator of an investment larger than the minimum; see Appendix Tables A.11 and A.12). To address concerns about statistical inferences given the small cell sizes, we ran permutation tests with 10,000 repetitions for each subgroup's social learning effect, and our inferences are unaffected (p-values presented in Appendix Table A.8, Panel B).

Evidence of updated beliefs. We believe that investor 2's higher take-up rate in condition B, and a component of their higher take-up rate in condition C, resulted from positively updating their beliefs about the asset after hearing that their associated investor 1 chose to purchase it. While brokers did not elicit prior or posterior beliefs during the initial sales call, in the follow-up survey, investors in conditions B and C were directly asked whether the fact that their associated investor 1 wanted to purchase the asset affected their beliefs about the quality of the asset. We find that 67% of investor 2's in conditions B and C reported positively updating their beliefs about the quality of the asset after learning that their peer chose to purchase it, consistent with a social

[17] Unsophisticated investor 2's have a lower take-up rate in condition A than do sophisticated investor 2's. This may be a result of sampling variation (the difference in take-up rates is not statistically significant) or a result of different prior beliefs about the asset in the absence of any peer effect. It is important to note that even if take-up rates in condition A were switched across groups, we would continue to find significant social learning effects among unsophisticated investor 2's and no significant social learning effects among sophisticated investor 2's.

learning effect; and, individuals who positively updated their beliefs were statistically significantly more likely to purchase the asset (the difference in take-up rates is 31 percentage points).

Our hypotheses regarding heterogeneous social learning effects according to investor 1's and investor 2's financial sophistication suggest that unsophisticated investor 2's should have been more likely to positively update their beliefs; and, purchase decisions by sophisticated investor 1's should have led to more positive belief updating. Indeed, we find these patterns in the follow-up survey data. Among unsophisticated investor 2's in condition B, 92% reported positively updating their beliefs about the quality of the asset; among sophisticated ones, only 11% did (the p-value of this difference is less than 0.01). Among investor 2's in condition B associated with sophisticated investor 1's, we find that 69% positively updated their beliefs; among those associated with unsophisticated investor 1's, only 33% updated positively (the p-value of this difference is 0.16).

3.4 Understanding the Social Utility Treatment Effects

The finance literature has pointed to different reasons why one's peer's possession of an asset might directly affect one's utility from possessing the same asset. In the follow-up survey, the investor 2's in condition C who chose to purchase the asset were asked about two particular mechanisms. First, they were asked about the importance of relative income or consumption concerns: whether earning the same return as their peer was important to their decision; whether fear of missing out on a return their peer might earn was important; and, whether they thought about what their peer might *do* with the returns from the asset. Second, they were asked one question relating to the importance of the "joint consumption" value of the financial asset: whether anticipated discussions of the asset with their peer were important to their decision.

The results indicate that both mechanisms were important. First, regarding "keeping up with the Joneses" motives: 60% of respondents reported that wanting to earn the same financial return as their peer was a significant factor in their decision; 80% of them reported that they thought about what their peer could do with the return from the asset; 32% reported that the fear of not having a return that their peer could have was a significant factor in their decision. We also find evidence of a "joint consumption" channel: 44% reported that a significant factor in their purchase decision was that they could talk with their peer about the asset. Although we cannot cleanly identify the relative importance of these different mechanisms, the evidence from the follow-up survey suggests that relative income and consumption concerns and a desire for "joint consumption" both played a role in generating the social utility effects we observe. Only 4% of respondents did not point to any of these social utility factors as a relevant element in their decision making process.

3.5 Alternative Hypotheses and Confounding Factors

In an ideal experiment, condition B would have differed from condition A only because of social learning; and, condition C would have differed from condition B only in the added effect of social utility. In practice, there may have been other differences across conditions; here we discuss whether they were likely to have played an important role in generating the treatment effects we find.

Effects of the lottery to authorize investments. One might wonder if the presence of the lottery distorted decisions by making the asset appear to be scarce and desirable. We do not believe this was the case. First, the asset used in the study could not be re-sold on the market following purchase, so the lottery did not send a signal about external demand. Second, we can compare the take-up rates in our experiment to those in a prior pilot study *without* a lottery to authorize investments: the purchase rate in the pilot study was 48% – very similar to what we observe among investors in our study receiving no information about their peers. Evidence from the follow-up survey is also informative: we asked investor 2's in conditions A, B, and C whether the presence of the lottery was a significant factor in their purchase decision. Only 4.3% of respondents reported that it was (and our results are robust to dropping them from our analysis). Finally, we find suggestive evidence in the follow-up survey that investor 2's in condition B did not update their views about the asset's quality (or about their likelihood of winning the lottery) after learning about their peer's unsuccessful lottery outcome.

More generally, because all conditions included the lottery, it is unlikely that a "level effect" of the lottery could generate the peer effects we observe. However, an important question is whether the lottery interacted with the information provided in condition B or C. For example, investor 2's might feel guilt possessing an asset that their peer was prevented from acquiring; or, they might especially desire an asset their peer explicitly could not acquire – a desire to "get ahead of the Joneses." However, we are reassured by our findings of heterogeneous treatment effects (in Table 3): it is difficult to tell a story in which the desire to get ahead of one's peer is concentrated among the financially unsophisticated, and among investor 2's whose associated investor 1 is financially sophisticated.

Another concern is that learning that investor 1 *possessed* the asset might have enhanced the revealed preference signal in condition C, relative to condition B. Investor 2's in condition B might have believed that their associated investor 1's did not really choose to purchase the asset. However, in the follow-up survey, we asked investor 2's in conditions B and C if they believed the information provided by the broker, and 97% replied "yes." A related possibility is that investor 2's in conditions B and C viewed the lottery outcome as a signal of whether investor 1's chose to follow through with their purchase decision. In the follow-up survey, we asked investor 2's in conditions A, B, and C if they thought they could have changed their choice after the realization of the lottery; 94% of them answered "no." Thus, it is unlikely that investor 2's viewed the purchase decision as non-binding. Our results are robust to dropping investors responding to either of these questions differently from the majority (results available upon request).

We also examine direct evidence on belief updating in conditions B and C. Consistent with a stronger revealed preference signal in condition C, in the follow-up survey, we find evidence of more frequent positive belief updating in condition C than in condition B: 74% compared to 57%, respectively (the p-value on the difference is 0.23). However, we have reason to believe that it does not explain the treatment effect we observe in condition C. First, when we estimate our empirical model, using a positive belief update as the outcome and including controls, we find that the estimated coefficient on the condition C indicator is just 0.06 (with a p-value of 0.74; results available upon request). Conditional on controls, the differential belief updating across conditions was very small, and unlikely to have driven our treatment effects. In addition, we examine whether there was differential take-up across experimental conditions B and C among investors who *did not* report positively updating their beliefs. We find suggestive evidence of social utility effects: among investor 2's who did not positively update their beliefs, the purchase rate was 56% in condition B, and 71% in condition C, suggesting that investors in condition C were more likely to have additional motives for purchasing the asset.

Investments outside the study. One might worry that investor 2's in condition B purchased the asset in order to transfer it (perhaps in exchange for a side payment) to their associated investor 1's. However, our design makes the arrangement of side payments unlikely: investor 1's did not know that their associated investor 2's would receive the offer, and so were unlikely to initiate this strategy (and there was limited time between calls to investor 1's and investor 2's); investor 2's were unable to communicate with investor 1's after receiving their offers, prior to making their investment decisions.[18] We can also address this concern with our experimental data. One might expect side payments to be most common among peers who are family members, who would have an easier time coordinating such payments. In fact, we find that the treatment effects from social learning are *not* stronger among family members (see column 1 of Appendix Table A.13).

One might also think that knowing that a peer desired to purchase an asset provides an indication of that peer's portfolio, or future asset purchases. As a result, the social learning condition could contain some (anticipated or approximate) possession effect. However, the specific asset sold in the study was not otherwise available; even if an investor wished to approximately reconstruct the asset, this would have been difficult. The real estate note (*LCI*) component is usually not available to this set of clients. In addition, the minimum investment in a real estate note is usually R$10,000 (instead of R$1,000 in our study). Finally, there is no reason to expect possession effects based on inferences about investor 1's portfolio to drive investment decisions so disproportionately among unsophisticated investor 2's, and in response to choices made by sophisticated investor 1's.

Variation across sales calls. One important concern with our design is that in condition A, brokers never mentioned another investor's choice, while in conditions B and C they did. Investor

[18] This also reduced the likelihood of investor 1 engaging in peer pressure, as in Calvo-Armengol and Jackson (2010).

2's in condition B or C might have made their investment decisions thinking about the possibility of their choices being discovered by their peers. However, all but five investors were known to have links with only one other client (their associated investor 1). Thus, once the offer was made to investor 1, investor 2 typically had no other peer who might receive the offer (our results are robust to dropping the 5 investor 2's who were part of larger networks of clients, available upon request). In the follow-up survey, we asked investor 2's in conditions B and C if they were concerned that their purchase decision would be revealed to other clients. Only 11% of the respondents replied "yes," and our results are robust to dropping these investors (results available upon request). If investor 2's were concerned about their associated investor 1's asking about the asset, the lottery to implement a purchase decision provided investor 2's with cover for a non-conforming choice.

Another concern is that brokers could exert differential effort toward selling the asset under different experimental conditions. Fortunately, we believe that the impact of the supply side on our measured treatment effects was likely limited. First, because brokers were compensated based on the assets they sold, they were incentivized to sell the asset in all conditions, rather than to confirm any particular hypothesis. Second, if broker effort did vary across conditions, one might have expected brokers to learn how to use the information in the various conditions more effectively as they made more sales calls. However, we find that treatment effects do *not* significantly vary with broker experience (see column 2 of Appendix Table A.13).

Finally, hearing a peer mentioned might increase the attention paid to the broker's sales pitch. However, brokers provided the information about the asset (in a double-blind manner) *prior* to mentioning investor 1's choice. In addition, our findings of heterogeneous treatment effects are suggestive of actual learning: one's ears are likely to perk up when hearing any peer's name; but, one is more likely to *learn* from the choice of a sophisticated friend, just as we find.

3.6 External Validity

A final important concern with our design regards the external validity of the findings. There are several important qualifications to the generality of the treatment effects we estimate. First, the type of social learning on which we focus is that of classic models, such as Banerjee (1992) and Bikhchandani et al. (1992): learning that occurs upon observation of the revealed preference decision to purchase made by a peer. We abstract away from the additional information one might acquire *after* a peer's purchase (e.g., by talking to the peer and learning about the quality of a product, as in Kaustia and Knüpfer, 2012) and from any change in behavior due to increased salience of a product when consumed by one's peers. These channels are shut down in our study because of the design of the financial asset, but are likely important as well.

Second, our treatment effects are estimated from the behavior of a particular sample of investors. The peers we study are very close – often friends or family – in contrast to other work in this area, which focuses on co-workers, and finds smaller peer effects on investment decisions (e.g., Duflo and

Saez, 2003 and Beshears et al., 2011). The peers we study formed their associations naturally, and endogenously (Carrell et al., forthcoming, find very different influence patterns comparing naturally-occurring peer groups to artificially-created groups). Thus, both social learning and social utility might be especially pronounced in our setting. Our comparisons among investor 2's in conditions A, B, and C are also *conditional* on investor 1 choosing to purchase the asset. If the associated investor 2's were thus unusual, one might question the external validity of our estimates even within our sample. In fact, when comparing investor 1's who chose to purchase the asset to those who chose not to purchase it, one sees that their observable characteristics are very similar (see Appendix Table A.1, columns 3 and 4). Investor 2's in conditions A and A^{neg} are also similar in their observable characteristics (see Appendix Table A.1, columns 6 and 7), and had similar take-up rates (see Table 2), suggesting that conditioning on investor 1's wanting to purchase the asset does not produce an unusual subsample from which we estimate peer effects. Because we study the behavior of investors who had referred (or had been referred by) other clients to the brokerage in the past, one might wonder how different our sample of investors is from other clients of the brokerage. When we compare the observable characteristics of the investors in our study to those of the full set of the brokerage's clients from the firm's main office, we find that they are roughly similar, though not identical (see Appendix Table A.1, column 8).

Finally, one might question the representativeness of the third-party communication studied in our experiment. Peers often communicate among themselves, rather than being informed by a broker trying to make a sale. Our goal of disentangling separate channels of peers' influence required *control* over information flows that are typically endogenous. In interpreting the magnitude of our effects, one should consider the likelihood of information transfer in the real world; our design estimates the impact of information about one's peers conditional on receiving it. Moreover, the sales calls we study are widely used: the brokerage informed us that such calls account for approximately 70% of its sales.[19]

4 Conclusion

Peer effects are an important, and often confounding, topic of study across the social sciences. In many settings – particularly in finance – identifying *why* a person's choices are affected by his peers' is extremely important, beyond identifying peer effects overall. Our experimental design not only allows us to identify peer effects in investment decisions, it also decouples revealed preference from possession, allowing us to provide evidence that learning from one's peer's purchase decision and changing behavior due to a peer's possession of an asset *both* affect investment decisions.

Our findings indicate that social learning from peers matters for financial decisions, especially

[19] While brokers generally do not provide information about *specific* clients' purchases, brokers regularly discuss the behavior of other investors in their sales calls. It is also worth noting that in the U.S., investors commonly turn to brokers for financial advice and to undertake transactions (see Hung et al., 2008).

among unsophisticated investors. This may, in some instances, increase welfare, as uninformed investors can benefit from the knowledge of sophisticated peers. On the other hand, inefficient herds and excessive asset price volatility may occur when individuals ignore their private information, or lack information about the financial markets in which they are participating (Banerjee, 1992; Bikhchandani et al., 1992; Avery and Zemsky, 1998; Chari and Kehoe, 2004). In this case, one might wish to educate unsophisticated individuals or provide more information about assets' quality to increase investors' reliance on their private information and reduce herding. Importantly, our finding of significant social utility effects suggests that information provision will not reduce herding as much as one would expect from a model that includes only social learning effects: even if individuals are financially sophisticated, and have very precise private signals of asset quality, they may choose to follow their peers for social utility reasons.

Our work should be extended in several directions. Most fundamentally, it is important to determine their external validity. One might be interested in whether our findings extend to assets with different expected returns or different exposures to risk; or, to investment decisions made from a larger choice set. One might also wish to study whether information transmitted directly among peers has a different effect from information transmitted through brokers. The selection of information transmitted by brokers and by peers is endogenous, and studying the process determining *which* information gets transmitted, and to whom, is of great interest. Studying information transmission through a larger network of individuals is important as well.

In addition to the context of financial decision making, our experimental design could be used in other settings to identify the channels through which peer effects work. In marketing, various social media rely on different peer effect channels: Facebook "likes", Groupon sales, and product give-aways all rely on some combination of the channels studied here. Future work can compare the effectiveness of these strategies, and their impact through different channels, using designs similar to ours. One could also apply our experimental design to the study of technology adoption: one might wish to distinguish between learning from a peer's purchase decision and the desire to adopt technologies used by others. Finally, health-promoting behavior often is affected both by learning from peers' purchases and by peers' actual use of health care technology (e.g., vaccination or smoking cessation).[20] In these settings and others, separately identifying the roles of social learning and social utility might be of interest to policymakers.

[20] Foster and Rosenzweig (1995), Conley and Udry (2010) and Dupas (forthcoming) identify the important role played by social learning in technology adoption; Kremer and Miguel (2007) study the transmission of knowledge about de-worming medication through social networks; and, Sorensen (2006) studies social learning in employees' choices of health plans. Social utility might exist in these settings because using a technology (or adopting a behavior) might be easier or less expensive when others nearby use (or adopt) it, or because one wishes not to fall behind those living nearby.

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Figures and Tables

Figure 1: **Experimental design "roadmap"**

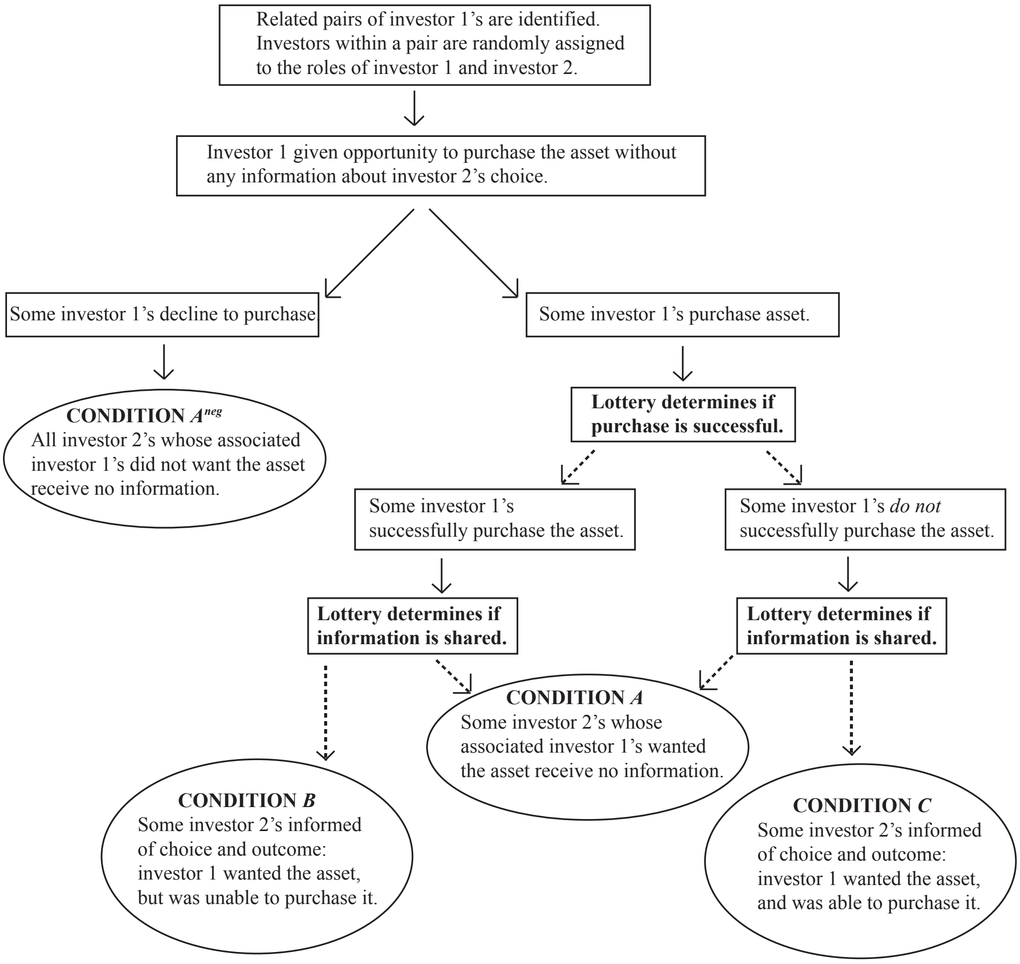


Table 1: **Covariates balance**

	Investor 2 conditional on investor 1 wanted to purchase the asset							
	Condition A	Condition B	Condition C	p-value of test:				N
	N=26	N=24	N=28	A=B=C	A=B	A=C	B=C	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age	37.92	34.50	36.75	0.59	0.31	0.75	0.57	78
	(2.16)	(2.55)	(2.98)					
Gender (=1 If male)	0.654	0.667	0.607	0.90	0.93	0.73	0.66	78
	(0.095)	(0.098)	(0.094)					
Married	0.385	0.250	0.357	0.56	0.31	0.84	0.41	78
	(0.097)	(0.090)	(0.092)					
Single	0.538	0.708	0.643	0.47	0.22	0.44	0.62	78
	(0.100)	(0.095)	(0.092)					
Earnings	4,000	4,000	4,500	0.81	0.79	0.68	0.52	67
	(782)	(534)	(1,941)					
Relationship with	0.46	0.67	0.46	0.24	0.15	0.98	0.14	78
investor 1 (=1 if family)	(0.10)	(0.10)	(0.10)					

Notes: The sample is conditioned on investor 2's whose associated investor 1's wanted to purchase the asset. Those in condition A had no information about their peers. Those in condition B had information that their peers wanted to purchase the asset but had that choice rejected by the lottery. Those in condition C had information that their peers wanted and received the asset. Each line presents averages of the corresponding variable for each treatment group. Robust standard errors in parentheses. For each variable, the p-value of an F-test that the mean of the corresponding variable is the same for all treatment groups is presented in column 4. The p-values of F-tests on pairwise treatment group comparisons are presented in columns 5 to 7. For earnings, we present the median and the p-value of a test that the median of this variable is the same for all treatment groups. The sample size for the earnings variable is smaller due to missing values.

Table 2: **Peer Effects, Social Learning, Social Utility, and Selection: Take-up Rates**

Dependent variable	Wanted to purchase the asset			
	(1)	(2)	(3)	(4)
Learning alone	0.285**	0.298**	0.328**	0.278**
(Condition B - Condition A)	(0.136)	(0.140)	(0.134)	(0.127)
Learning and possession	0.505***	0.540***	0.552***	0.500***
(Condition C - Condition A)	(0.110)	(0.122)	(0.123)	(0.111)
Negative selection	-0.034	0.011	-0.005	0.042
(Condition A^{neg} - Condition A)	(0.114)	(0.124)	(0.118)	(0.117)
Investor 1				0.128
				(0.106)
Possession alone	0.220**	0.242**	0.224*	0.222**
(Condition C - Condition B)	(0.106)	(0.109)	(0.124)	(0.108)
Mean (no information; peer chose the asset)	0.423			
(Condition A)	(0.099)			
Broker fixed effects	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
N	150	150	150	300
R^2	0.186	0.228	0.283	0.219

Notes: Column 1 presents the results of a regression of a dummy variable equal to one if the investor wanted to purchase the asset on a dummy for condition C, a dummy for condition B, and a dummy indicating whether the associated investor 1 did not want to purchase the asset (Condition A^{neg}). Investor 2's in condition A is the omitted group. This regression uses only the sample of investor 2's. The regression presented in column 2 includes broker fixed effects. The regression presented in column 3 includes the covariates presented in Table 1. We did not include earnings as this would reduce our sample size (results including earnings are similar). The regression presented in column 4 combines the sample of investors 1 and 2, and includes an indicator variable for investor 1. Standard errors are clustered at the pair level. In all columns, "Possession alone" gives the difference between the coefficient on "Learning and possession" and the coefficient on "Learning alone." * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3: **Heterogeneity of Social Learning Effects - Self-Assessed Measure of Financial Sophistication**

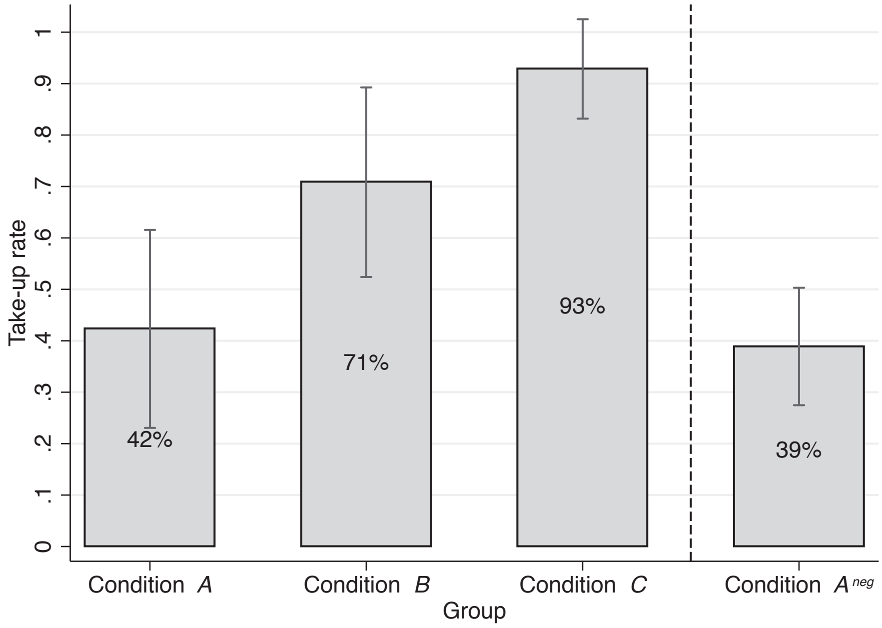
	Investor 2 is financially sophisticated			Associated investor 1 is financially sophisticated		
	Yes (1)	No (2)	p-value of test (1)=(2) (3)	Yes (4)	No (5)	p-value of test (4)=(5) (6)
Panel A: no controls						
Learning alone	-0.017	0.583***	0.047	0.380**	0.127	0.422
(Condition B - Condition A)	(0.227)	(0.185)		(0.171)	(0.261)	
Panel B: full specification						
Learning alone	0.059	0.687**	0.070	0.365**	0.068	0.527
(Condition B - Condition A)	(0.363)	(0.323)		(0.174)	(0.409)	
Mean (no information; peer chose the asset)	0.462	0.333	0.566	0.389	0.429	0.864
(Condition A)	(0.145)	(0.165)		(0.120)	(0.195)	

Notes: Panel A presents social learning treatment effects estimated from regressions without controls (i.e., comparisons of means) using investor 2's in conditions A and B. In columns 1–2, we estimate separate social learning effects for financially sophisticated and financially unsophisticated investor 2's, respectively. We regress the investment decision dummy variable on a "financially sophisticated" indicator; an interaction between a Condition B indicator and the financially sophisticated indicator; and, an interaction between a Condition B indicator and a "financially unsophisticated" indicator. Take-up rates for investor 2's in Condition A who are sophisticated, and unsophisticated, are listed in the last row. Column 3 shows the p-value from a test of equal social learning effects for sophisticated and unsophisticated investor 2's. In columns 4–5, we estimate regressions analogous to columns 1–2, but studying heterogeneous social learning effects depending on the associated investor 1's financial sophistication, instead of investor 2's financial sophistication. Take-up rates for investor 2's in Condition A associated with sophisticated, and unsophisticated, investor 1's are listed in the last row. Column 6 shows the p-value from a test of equal social learning effects for investor 2's associated with sophisticated and unsophisticated 1's. Panel B presents estimated social learning effects from models analogous to those in Panel A, but also including baseline covariate controls and broker fixed effects. The financial sophistication variable is based on the self-assessment question conducted in the follow-up survey described in the text. Investors rated their financial knowledge from 1 (very low) to 7 (very high). Investors who reported 4 or higher were classified as financially sophisticated. We had to exclude 7 out of 50 investor 2's in conditions A and B from the regressions reported in columns 1–2, because we do not have information on financial sophistication for them. We had to exclude 3 out of 50 investor 2's in conditions A and B from the regressions reported in columns 4–5, because we do not have information on financial sophistication for their associated investor 1's.
* significant at 10%; ** significant at 5%; *** significant at 1%.

ONLINE APPENDIX
(NOT FOR PUBLICATION)

Appendix A: Appendix Figures and Tables

Figure A.1: **Investor 2's take-up rates**



Note: This figure presents the mean (and 95% confidence interval) of the take-up rate for each group of investor 2's. Investors in conditions A to C have peers who wanted the asset. These investors were randomly allocated to one of these 3 groups. Those in condition A had no information about their peers. Those in condition B had information that their peers wanted to purchase the asset but had that choice rejected by the lottery. Those in condition C had information that their peers wanted and received the asset. Investors in condition A^{neg} have peers who did not want to purchase the asset (and received no information about their peer).

Figure A.2: **Investor 2's Alternative Outcomes**

Figure A.2.1: Amount invested

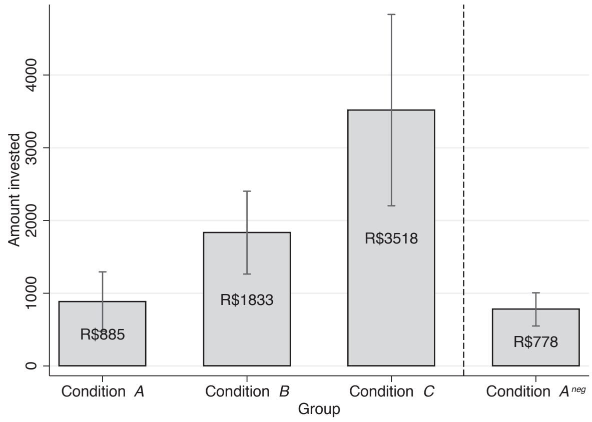
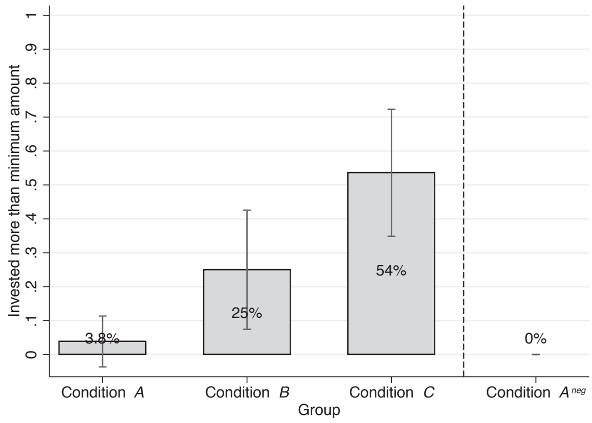


Figure A.2.2: Invested more than the minimum



Note: Panel A.2.1 presents the mean (and 95% confidence interval) of amount invested for each group of investor 2's. Panel A.2.2 presents the mean (and 95% confidence interval) of a dummy variable equal to one if the investor invested more than the minimum amount for each group of investor 2's. Investors in conditions A to C have peers who wanted the asset. These investors were randomly allocated to one of these 3 groups. Those in condition A had no information about their peers. Those in condition B had information that their peers wanted to purchase the asset but had that choice rejected by the lottery. Those in condition C had information that their peers wanted and received the asset. Investors in condition A^{neg} have peers who did not want to purchase the asset (and received no information about their peer).

Figure A.3: **Heterogeneity of Social Learning Effects - Self-Assessed Measure of Financial Literacy**

Figure A.3.1: Investor 2 is financially sophisticated

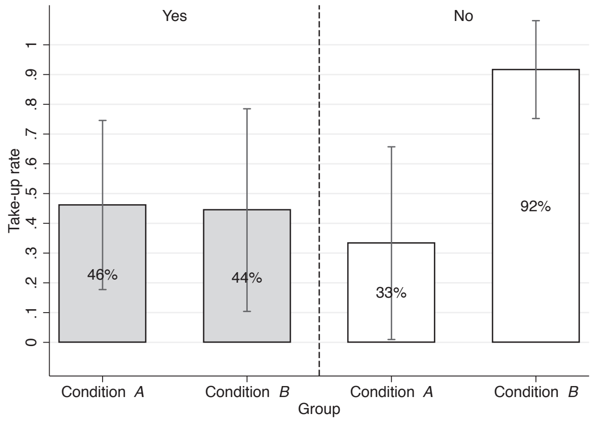
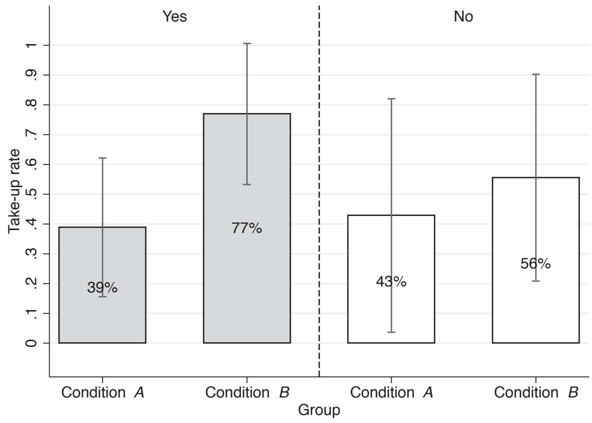


Figure A.3.2: Associated investor 1 is financially sophisticated



Note: Figure A.3.1 presents the mean (and 95% confidence intervals) of take-up rates for investor 2's in conditions A and B, separately for those who are and who are not financially sophisticated. Figure A.3.2 presents the take-up rates separately for those whose associated investor 1's are and who are not financially sophisticated. Investors in conditions A and B have peers who wanted the asset. Those in condition A had no information about their peers. Those in condition B had information that their peers wanted to purchase the asset but had that choice rejected by the lottery. The financial sophistication variable is based on a self-assessment question conducted in a follow-up survey, where investors were asked to rank their level of financial sophistication from 1 (very low) to 7 (very high). Investors who reported 4 or higher were classified as financially sophisticated.

Table A.1: **Characteristics of the Experimental Sample**

	Experimental Sample							Universe
		Investor 1			Investor 2			
	Full Sample	All	Wanted the asset?		All	Peer wanted the asset?		
			Yes	No		Yes	No	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age	38.15	39.12	39.60	38.60	37.18	36.45	37.97	34.14
	(0.80)	(1.14)	(1.60)	(1.62)	(1.12)	(1.50)	(1.68)	(0.16)
Gender (=1 If male)	0.680	0.747	0.769	0.722	0.613	0.641	0.583	0.729
	(0.027)	(0.036)	(0.048)	(0.053)	(0.040)	(0.055)	(0.059)	(0.006)
Married	0.413	0.440	0.436	0.444	0.387	0.333	0.444	0.340
	(0.028)	(0.041)	(0.057)	(0.059)	(0.040)	(0.054)	(0.059)	(0.006)
Single	0.557	0.527	0.513	0.542	0.587	0.628	0.542	0.647
	(0.029)	(0.041)	(0.057)	(0.059)	(0.040)	(0.055)	(0.059)	(0.006)
Earnings	4,500	5,000	5,000	5,000	4,000	4,000	3,500	3,200
	(256)	(499)	(501)	(775)	(507)	(504)	(650)	(126)
Relationship with associated investor (=1 if family)	0.48	0.48	0.53	0.43	0.48	0.53	0.43	-
	(0.03)	(0.04)	(0.06)	(0.06)	(0.04)	(0.06)	(0.06)	
N	300	150	78	72	150	78	72	5506

Notes: Column 1 presents the characteristics of the experimental sample, combining investor 1's and investor 2's. Column 2 presents the sample characteristics of investor 1's in the experimental sample, while columns 3 and 4 present the information for investor 1's who wanted and who did not want the asset, respectively. Column 5 presents the characteristics of investors 2's in the experimental sample, while columns 6 and 7 present the information for investor 2's whose peers wanted and did not want the asset, respectively. Column 8 presents the characteristics of the universe of investors in the main office of the brokerage. Each line presents averages of the corresponding variable. For earnings, we present the median value instead of the mean due to large outliers. The sample size for the earnings variable is smaller due to missing values. The omitted value for "Relationship with associated investor" is "friends". This variable is not defined for investors outside the experiment's sample.

Table A.2: **Covariates Balance - Other Randomizations**

	Assignment to investor 1 or investor 2				Lottery for investor 1's who wanted the asset			
	Investor 1	Investor 2	p-value of test (1)=(2)	N	Won	Lost	p-value of test (5)=(6)	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age	39.12	37.18	0.22	300	39.47	39.71	0.94	78
	(1.14)	(1.12)			(2.34)	(2.23)		
Gender (=1 If male)	0.747	0.613	0.01	300	0.861	0.690	0.07	78
	(0.036)	(0.040)			(0.058)	(0.072)		
Married	0.440	0.387	0.35	300	0.472	0.405	0.56	78
	(0.041)	(0.040)			(0.084)	(0.077)		
Single	0.527	0.587	0.30	300	0.528	0.500	0.81	78
	(0.041)	(0.040)			(0.084)	(0.078)		
Earnings	5,000	4,000	0.22	270	5,000	5,000	0.59	74
	(499)	(507)			(925)	(754)		
Relationship with peer (=1 if family)	-	-	-	-	0.44	0.60	0.19	78
					(0.08)	(0.08)		

Notes: Columns 1 and 2 present the averages of the corresponding variable, respectively, for investors assigned to be in the role of investor 1 and for those assigned to be in the role of investor 2. Robust standard errors in parentheses. Relationship with peer is not considered in this comparison since this variable is equal for both groups by construction. Column 3 presents the p-value of an F-test that the mean of the corresponding variable is the same for these two groups. Column 5 presents the averages for investor 1's who wanted the asset and won the lottery, while column 6 presents the averages for investor 1's who wanted the asset but did not win the lottery. Column 7 presents the p-value of an F-test that the mean of the corresponding variable is the same for these two groups. For earnings, we present the median and the p-value of a test that the median of this variable is the same for the corresponding groups. The sample size for the earnings variable is smaller due to missing values.

Table A.3: **Follow-up Survey**

Question	Universe	Sample size	Results
	Panel A: Financial Literacy Survey		
1. Self-assessed financial literacy (range: 1-7)	Investor 2's in conditions A and B, and their associated investor 1's	90 (out of 100)	Mean: 3.8 Standard deviation: 1.7 Proportion $\geq$ 4: 58.89%
2. Interest rate compounding question	Investor 2's in conditions A and B, and their associated investor 1's	90 (out of 100)	Correct: 85.56%
3. Inflation question	Investor 2's in conditions A and B, and their associated investor 1's	90 (out of 100)	Correct: 85.56%
4. Diversification question	Investor 2's in conditions A and B, and their associated investor 1's	90 (out of 100)	Correct: 67.78%
5. Bond prices question	Investor 2's in conditions A and B, and their associated investor 1's	90 (out of 100)	Correct: 14.44%
Questions (2)-(5)			0 correct answers: 5.56% 1 correct answer: 5.56% 2 correct answers: 32.22% 3 correct answers: 43.33% 4 correct answers: 13.33%
	Panel B: Questions Regarding the Sales Call		
1. Effect of lottery on purchase decision	Investor 2's in conditions A B and C	69 (out of 78)	No: 95.65%
2. Believed purchase decision could have been changed after lottery	Investor 2's in conditions A B and C	69 (out of 78)	No: 94.20%
3. Peer's lottery result affected beliefs about own lottery	Investor 2's in conditions B and C	47 (out of 52)	No: 100%
4. Peer's lottery result affected beliefs about quality of the asset	Investor 2's in conditions B and C	47 (out of 52)	No: 97.87%
5. Was (not) wanting something your peer could not have a significant factor in decision?	Investor 2's in condition B	20 (out of 24)	No: 100%
6. Effect of peer decision on beliefs about quality of the asset	Investor 2's in conditions B and C	48 (out of 52)	Positive update: 66.67% Negative update: 2.08% No update: 31.24%
7. Was wanting to have the same financial return as your peer a significant factor in decision?	Investor 2's in condition C who wanted the asset	25 (out of 26)	Yes: 60%
8. Was wanting to have the same asset as your peer to talk about the asset a significant factor in decision?	Investor 2's in condition C who wanted the asset	25 (out of 26)	Yes: 44%
9. Did you think about what your peer could do with the return?	Investor 2's in condition C who wanted the asset	25 (out of 26)	Yes: 80%
10. Was the fear of not having a return your peer could have a significant factor in decision?	Investor 2's in condition C who wanted the asset	25 (out of 26)	Yes: 32%
11. Did you believe the information provided by the broker?	Investor 2's in conditions B and C	47 (out of 52)	Yes: 97.87%
12. Were you concerned about your decision being revealed to other clients?	Investor 2's in conditions B and C	47 (out of 52)	No: 89.36%

Notes: the follow-up survey was conducted between November 26, and December 7, 2012. From the universe of investor 2's in conditions A-C and investor 1's associated with investor 2's in conditions B or C (128 investors in total), we collected information on 117 investors. Not all of those investors were asked all of the questions. This table reports, for each question, which investors answered it, the number of responses, and the results.

Table A.4: **Follow-up Survey - Excluding Investors Interviewed by Same Broker**

Question	Universe	Sample size	Results
	Panel A: Financial Literacy Survey		
1. Self-assessed financial literacy (range: 1-7)	Investor 2's in conditions *A* and *B*, and their associated investor 1's	80 (out of 100)	Mean: 3.9 Standard deviation: 1.7 Proportion $\geq$ 4: 61.25%
2. Interest rate compounding question	Investor 2's in conditions *A* and *B*, and their associated investor 1's	80 (out of 100)	Correct: 83.75%
3. Inflation question	Investor 2's in conditions *A* and *B*, and their associated investor 1's	80 (out of 100)	Correct: 85.00%
4. Diversification question	Investor 2's in conditions *A* and *B*, and their associated investor 1's	80 (out of 100)	Correct: 67.50%
5. Bond prices question	Investor 2's in conditions *A* and *B*, and their associated investor 1's	80 (out of 100)	Correct: 16.25%
Questions (2)-(5)			0 correct answers: 6.25% 1 correct answer: 6.25% 2 correct answers: 31.25% 3 correct answers: 41.25% 4 correct answers: 15.00%
	Panel B: Questions Regarding the Sales Call		
1. Effect of lottery on purchase decision	Investor 2's in conditions *A* *B* and *C*	64 (out of 78)	No: 95.31%
2. Believed purchase decision could have been changed after lottery	Investor 2's in conditions *A* *B* and *C*	64 (out of 78)	No: 93.75%
3. Peer's lottery result affected beliefs about own lottery	Investor 2's in conditions *B* and *C*	45 (out of 52)	No: 100%
4. Peer's lottery result affected beliefs about quality of the asset	Investor 2's in conditions *B* and *C*	45 (out of 52)	No: 97.78%
5. Was (not) wanting something your peer could not have a significant factor in decision?	Investor 2's in condition *B*	20 (out of 24)	No: 100%
6. Effect of peer decision on beliefs about quality of the asset	Investor 2's in conditions *B* and *C*	46 (out of 52)	Positive update: 67.39% No update: 32.61%
7. Was wanting to have the same financial return as your peer a significant factor in decision?	Investor 2's in condition *C* who wanted the asset	24 (out of 26)	Yes: 62.50%
8. Was wanting to have the same asset as your peer to talk about the asset a significant factor in decision?	Investor 2's in condition *C* who wanted the asset	24 (out of 26)	Yes: 41.67%
9. Did you think about what your peer could do with the return?	Investor 2's in condition *C* who wanted the asset	24 (out of 26)	Yes: 79.17%
10. Was the fear of not having a return your peer could have a significant factor in decision?	Investor 2's in condition *C* who wanted the asset	24 (out of 26)	Yes: 33.33%
11. Did you believe the information provided by the broker?	Investor 2's in conditions *B* and *C*	45 (out of 52)	Yes: 97.78%
12. Were you concerned about your decision being revealed to other clients?	Investor 2's in conditions *B* and *C*	45 (out of 52)	No: 88.89%

Notes: this table replicates Table A.3 excluding 11 investors who were interviewed by the same broker who made the sales call.

Table A.5: **Probit Average Marginal Effects - Peer Effects, Social Learning, Social Utility, and Selection: Take-up Rates**

Dependent variable	Wanted to purchase the asset			
	(1)	(2)	(3)	(4)
Learning alone	0.285**	0.301**	0.357***	0.282**
(Condition B - Condition A)	(0.138)	(0.142)	(0.125)	(0.128)
Learning and possession	0.505***	0.536***	0.545***	0.525***
(Condition C - Condition A)	(0.104)	(0.109)	(0.104)	(0.103)
Negative selection	-0.034	0.018	0.002	0.055
(Condition A^{neg} - Condition A)	(0.106)	(0.129)	(0.108)	(0.115)
Investor 1				0.133
				(0.096)
Possession alone	0.220**	0.234**	0.188*	0.243**
(Condition C - Condition B)	(0.108)	(0.103)	(0.103)	(0.117)
Mean (no information; peer chose the asset)	0.423			
(Condition A)	(0.099)			
Broker fixed effects	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
N	150	150	150	300

Notes: This table replicates the results from Table 2 using Probit models instead of ordinary least squares regressions. The coefficients presented are average marginal effects. Standard errors are bootstrapped and clustered at the pair level in column 4. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.6: **Logit Average Marginal Effects - Peer Effects, Social Learning, Social Utility, and Selection: Take-up Rates**

Dependent variable	Wanted to purchase the asset			
	(1)	(2)	(3)	(4)
Learning alone	0.285**	0.295**	0.355***	0.275**
(Condition B - Condition A)	(0.138)	(0.143)	(0.124)	(0.127)
Learning and possession	0.505***	0.542***	0.556***	0.527***
(Condition C - Condition A)	(0.104)	(0.112)	(0.103)	(0.106)
Negative selection	-0.034	0.018	-0.006	0.052
(Condition A^{neg} - Condition A)	(0.106)	(0.131)	(0.107)	(0.116)
Investor 1				0.132
				(0.096)
Possession alone	0.220**	0.247**	0.202*	0.252**
(Condition C - Condition B)	(0.108)	(0.105)	(0.104)	(0.120)
Mean (no information; peer chose the asset)	0.423			
(Condition A)	(0.099)			
Broker fixed effects	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
N	150	150	150	300
Broker fixed effects	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
N	150	150	150	300

Notes: This table replicates the results from Table 2 using Logit models instead of ordinary least squares regressions. The coefficients presented are average marginal effects. Standard errors are bootstrapped and clustered at the pair level in column 4. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.7: **GMM Results**

Panel A: Treatment Effects

Learning and possession ($c-a$)	0.384***
	(0.085)
Learning alone ($b-a$)	0.164
	(0.116)
Possession alone ($c-b$)	0.220**
	(0.105)
Negative selection ($n-a$)	-0.116
	(0.106)

Panel B: GMM Coefficients

c	0.929***	b	0.708***
	(0.049)		(0.093)
a	0.545***	n	0.429***
	(0.052)		(0.069)
p	0.485***		
	(0.035)		

Hansen's J chi2(1) = 2.60863 (p = 0.1063)

Notes: This table presents results using a GMM model, where the overidentifying restriction is that investor 1's take-up rate is a weighted average of investor 2's in conditions A and A^{neg}. More specifically, the moment conditions are: $E[Y|\text{Condition } C] = c$, $E[Y|\text{Condition } B] = b$, $E[Y|\text{Condition } A] = a$, $E[Y|\text{Condition } A^{neg}] = n$, $E[Y|\text{Investor 1}] = p$, and $p = p \cdot a + (1-p) \cdot n$. Panel A presents the treatment effects, while Panel B presents the GMM coefficients. We also present the p-value of Hansen's J over identifying test. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.8: **Permutation Tests (p-values)**

Dependent variable	Take-up rates	Amount invested	Invested more than minimum
	(1)	(2)	(3)
Panel A: Main results			
Learning alone (Condition B - Condition A)	[0.052]*	[0.012]**	[0.047]**
Learning and possession (Condition C - Condition A)	[0.000]***	[0.000]***	[0.000]***
Possession alone (Condition C - Condition B)	[0.063]*	[0.011]**	[0.047]**
Negative selection (Condition A^{neg} - Condition A)	[0.812]	[0.646]	[0.270]
Panel B: Heterogeneity			
Learning *by*			
Sophisticated	[0.922]	[0.675]	[0.324]
Non-sophisticated	[0.008]***	[0.004]***	[0.083]*
Difference	[0.053]*	[0.071]*	[0.428]
Learning *from*			
Sophisticated	[0.038]**	[0.009]***	[0.028]**
Non-sophisticated	[0.801]	[0.816]	[1.000]
Difference	[0.434]	[0.155]	[0.028]**

Notes: This table presents the results of two-sided permutation tests with 10,000 replications for the main results in the paper. For each pairwise comparison, we randomly reassign the experimental treatment conditions, drawing treatment assignments (without replacement) in the same ratios as the actual experimental treatment assignments. Based on these "placebo" treatment assignments, we calculate "placebo treatment effects" using 10,000 independent reassignments. The distribution of "placebo treatment effects" from the 10,000 reassignments approximates the distribution of our estimator under the null hypothesis that the treatment effects are zero. We calculate p-values from the permutation tests as the proportion of "placebo treatment effects" that are greater (in absolute value) than the estimated treatment effects using the actual experimental treatment assignments. Panel A reports p-values from permutation tests for pairwise comparisons of the conditions of interest using three different outcome variables: take-up rates, amount invested, and a dummy variable indicating whether the investor invested more than the minimum amount. Panel B reports p-values from permutation tests for the heterogeneity results using the self-assessed measure of financial literacy. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.9: **Peer Effects, Social Learning, Social Utility, and Selection: Alternative Outcomes**

Dependent variable	Amount invested		Invested more than minimum	
	(1)	(2)	(3)	(4)
Learning alone	948.7***	715.2*	0.212**	0.173*
(Condition B - Condition A)	(357.7)	(394.5)	(0.097)	(0.095)
Learning and possession	2,633.2***	2,521.4***	0.497***	0.485***
(Condition C - Condition A)	(702.9)	(611.9)	(0.103)	(0.101)
Negative selection	-106.8	123.9	-0.038	-0.016
(Condition A^{neg} - Condition A)	(239.0)	(308.6)	(0.038)	(0.049)
Investor 1		503.8*		0.097*
		(300.1)		(0.053)
Possession alone	1,684.5**	1,806.1**	0.286**	0.311**
(Condition C - Condition B)	(731.4)	(727.0)	(0.131)	(0.128)
Mean (no information; peer chose the asset)	884.6		0.038	
(Condition A)	(210.0)		(0.038)	
Broker fixed effects	No	Yes	No	Yes
Controls	No	Yes	No	Yes
N	150	300	150	300
R2	0.251	0.264	0.338	0.295

Notes: Columns 1 and 2 replicate the regressions in columns 1 and 4 of Table 2 using the amount invested in the asset instead of take-up rate as dependent variable. Columns 3 and 4 replicate the regressions in columns 1 and 4 of Table 2 using a dummy variable equal to one if the investor invested more than the minimum amount as dependent variable. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.10: **Heterogeneity of Social Learning Effects - Objective Measure of Financial Sophistication**

	Investor 2 is financially sophisticated			Associated investor 1 is financially sophisticated		
	Yes	No	p-value of test (1)=(2)	Yes	No	p-value of test (4)=(5)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: no controls						
Learning alone	0.196	0.394*	0.533	0.386**	0.100	0.349
(Condition B - Condition A)	(0.227)	(0.218)		(0.175)	(0.246)	
Panel B: full specification						
Learning alone	0.031	0.892**	0.085	0.399*	-0.111	0.291
(Condition B - Condition A)	(0.367)	(0.450)		(0.210)	(0.408)	
Mean (no information; peer chose the asset)	0.429	0.375	0.816	0.400	0.400	1.000
(Condition A)	(0.139)	(0.180)		(0.132)	(0.162)	

Notes: This table replicates the results from Table 3 using an objective (instead of self-assessed) measure of financial literacy, based on four financial literacy questions conducted in a follow-up survey. Investors who answered 3 or more questions correctly were classified as financially sophisticated. See Appendix C for an English version of the financial literacy questions. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.11: **Heterogeneity of Social Learning Effects - Amount Invested**

	Investor 2 is financially sophisticated			Associated investor 1 is financially sophisticated		
	Yes	No	p-value of test (1)=(2)	Yes	No	p-value of test (4)=(5)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: no controls						
Learning alone	222.2	1,750.0***	0.057	1,320.5***	254.0	0.147
(Condition B - Condition A)	(608.7)	(488.3)		(498.3)	(522.4)	
Panel B: full specification						
Learning alone	201.4	1,791.0**	0.069	1,152.1***	513.8	0.547
(Condition B - Condition A)	(828.9)	(815.9)		(417.1)	(912.1)	
Mean (no information; peer chose the asset)	1,000.0	666.7	0.478	833.3	857.1	0.960
(Condition A)	(322.7)	(329.6)		(262.2)	(390.0)	

Notes: This table replicates the results from Table 3 using the amount invested in the asset instead of take-up rate as dependent variable. The financial sophistication variable is based on the self-assessment question conducted in the follow-up survey described in the text. Investors rated their financial knowledge from 1 (very low) to 7 (very high). Investors who reported 4 or higher were classified as financially sophisticated. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.12: **Heterogeneity of Social Learning Effects - Invested More than Minimum**

	Investor 2 is financially sophisticated			Associated investor 1 is financially sophisticated		
	Yes (1)	No (2)	p-value of test (1)=(2) (3)	Yes (4)	No (5)	p-value of test (4)=(5) (6)
Panel A: no controls						
Learning alone	0.145	0.333**	0.394	0.329**	0.000	0.036
(Condition B - Condition A)	(0.165)	(0.143)		(0.152)	(0.000)	
Panel B: full specification						
Learning alone	0.135	0.233	0.673	0.235*	0.213	0.928
(Condition B - Condition A)	(0.148)	(0.161)		(0.135)	(0.172)	
Mean (no information; peer chose the asset)	0.077	0.000	0.333	0.056	0.000	0.334
(Condition A)	(0.078)	(0.000)		(0.056)	(0.000)	

Notes: This table replicates the results from Table 3 using a dummy variable equal to one if the investor invested more than the minimum amount instead of take-up rate as dependent variable. The financial sophistication variable is based on the self-assessment question conducted in the follow-up survey described in the text. Investors rated their financial knowledge from 1 (very low) to 7 (very high). Investors who reported 4 or higher were classified as financially sophisticated. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.13: **Robustness Tests**

Interaction of the treatment effects with:	Relationship with investor 1 (=1 if family) (1)	Broker experience within the experiment (2)
Learning alone	0.077	-0.001
	(0.305)	(0.008)
Learning and possession	0.417*	-0.003
	(0.232)	(0.008)
Possession alone	0.340	-0.001
	(0.220)	(0.007)

Notes: This table presents coefficients on the interactions of the variables at the column heading with the treatment effects of interest. These results are based on the regressions used in the full specification of column 4 from Table 2, including interactions of the group dummies ($I_{c,i}$, where $c \in \{\text{Condition } B, \text{Condition } C, \text{Condition } A^{neg}, \text{investor } 1\}$) with the corresponding variables. We also include the main effect of the corresponding variable. In column 1, we interact the treatment effects with a dummy variable equal to one if the investors 1 and 2 are family members. The omitted category is "friends". In column 2, we interact the treatment effects with a variable indicating the number of calls that the broker had made before the day of the call. * significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix B: A Simple Model of Financial Decisions Under Social Influence

Our model studies an investment decision made by an individual under several conditions. First, we present the investment decision under uncertainty, but with no social influence. Second, we present the investment decision with social learning present, using the ingredients of a canonical social learning model: a peer makes an investment acting on a private signal, and this action can be used by another investor to make an informational inference before taking his own action. Third, we allow the ownership of an asset to affect a socially-related investor's utility of owning the asset, aside from any learning – that is, we allow for a social utility effect. A peer's purchase decision typically will produce both social learning and social utility effects; we consider a case in which both effects are active (the full "peer effect") and a case in which the revealed preference purchase decision is de-coupled from possession. This de-coupling allows one to observe each channel through which peer effects work, and motivates our experimental design.

Investment without Peer Effects

Consider an investor i's decision to invest in a risky asset.[21] The asset's return is given by x, with probability density function $f(x)$, and investor i's utility is $u_i(x) = u(x)$ for all i. In our field experiment, investors received calls from brokers who offered them a financial asset for purchase. The brokers attempted to convey the same information about the asset in every call using a pre-specified script; thus, the information they provided can be thought of as a signal, s_i, coming from a single distribution, with probability density function $g(s_i)$. Importantly, not every investor would have received exactly the same information: calls evolve in different ways, investors ask different questions about the asset, etc., meaning that each investor received a different signal realization, s_i, from the common distribution of signals.

For expositional simplicity, assume that the conditional density $f(x|s_i)$ satisfies the monotone likelihood ratio property (MLRP) such that, intuitively, higher values of s_i are indicative of higher values of x. Under these conditions, investor i is willing to invest if and only if

$$\int u(x)f(x|s_i)dx \geq \bar{u}, \tag{2}$$

where $\bar{u}$ denotes the outside option of the investor. Given that $f(x|s_i)$ satisfies MLRP and given mild monotonicity assumptions on the utility function $u(\cdot)$ of the investor, there exists a unique threshold $\bar{s}_1$ such that for any $s_i \geq \bar{s}_1$ investor i is willing to invest. Denote the decision to buy

[21] Note that we implicitly assume that when investing in isolation, investor i does not take into consideration any investor j ($j \neq i$) at all – he is "unaware." In the context of our experiment, we believe that this assumption is reasonable, as we discuss in the text.

the asset made by investor i by $b_i = \{0, 1\}$. Hence, for an investor making a purchase decision in isolation, we have

$$b_i = 1 \Leftrightarrow s_i \geq \bar{s}_1. \tag{3}$$

Investment with Social Learning Alone

Suppose that instead of making his investment choice in isolation, before making his own decision, investor i observes the investment decision of investor j which is given by b_j. Assume that investor j made his choice $b_j = 1$ in isolation and hence his decision rule is given by (3).[22] Thus, when investor i observes $b_j = 1$ he correctly infers that $s_j \geq \bar{s}_1$ and he is willing to invest if and only if

$$\int u(x) f(x|s_i; s_j \geq \bar{s}_1) dx \geq \bar{u}. \tag{4}$$

Furthermore, given that $f(x|s_i; s_j)$ satisfies MLRP we have

$$\int u(x) f(x|s_i; s_j \geq \bar{s}_1) dx \geq \int u(x) f(x|s_i) dx, \tag{5}$$

for all s_i. It is straightforward to show by comparing (4) and (2) that the signal realization threshold for investor i that is necessary to induce purchase of the asset is lower when $b_j = 1$ is observed than when investor i makes his choice in isolation. This is because in the former case, regardless of his own private information summarized by s_i, investor i has additional favorable information about the asset from observing the purchase of investor j. This is the pure social learning effect.

Denote the threshold for s_i when investor i observes $b_j = 1$ by $\bar{s}_2$ and note that $\bar{s}_2 \leq \bar{s}_1$. In particular, after observing a purchase decision made by investor j, the decision rule of investor i is given by

$$b_i = 1 \Leftrightarrow s_i \geq \bar{s}_2. \tag{6}$$

Social Utility and Social Learning

We now consider the situation in which both social utility and social learning effects are present. Our focus (following much of the literature on peer effects in financial decisions) is on social utility effects that result in a *positive* effect of a peer's possession of an asset (denoted by $p_j = \{0, 1\}$) on one's own utility.[23] In particular, when investor i considers purchasing the asset, we assume that $u(x|p_j = 1) \geq u(x|p_j = 0)$ for all x. That is, investor i's utility is higher for all asset return realizations if the asset is also possessed by an investor j who is a peer of investor i. Using the

[22] We focus on the case of investor i observing that investor j chose to purchase the asset (rather than choosing *not* to purchase it) because in the experimental design, we were not allowed to inform investors that their peer chose not to purchase the asset.

[23] One could also imagine a *negative* correlation, for example, out of a desire to insure one's peers, or to differentiate oneself. See Clark and Oswald (1998).

notation of our model, an investor j's purchase of an asset, $b_j = 1$, typically implies both that investor i infers favorable information about the asset, $s_j \geq \bar{s}_1$, and that investor j now possesses the asset, $p_j = 1$, which might affect investor i's utility of owning the asset (due to a taste for joint consumption, "keeping-up-with-the-Joneses" preferences).

When investor i observes that investor j expressed an intention to invest, $b_j = 1$, and was allowed to invest, $p_j = 1$, both investor i's utility $u(x|p_j = 1)$ *and* his information about the asset $f(x|s_i; s_j \geq \bar{s}_1)$ are affected, relative to his choice in isolation (that is, relative to $u(x) = u(x|p_j = 0)$ and $f(x|s_i)$).[24] In this case, one observes the "full" peer effect, and investor i invests if and only if

$$\int u(x|p_j = 1) f(x|s_i; s_j \geq \bar{s}_1) dx \geq \bar{u}. \tag{7}$$

Denote the threshold for s_i above which investor i is willing to invest when exposed to both peer effects channels by $\bar{s}_3$. Then, the decision rule for investor i is given by

$$b_i = 1 \Leftrightarrow s_i \geq \bar{s}_3. \tag{8}$$

To separate the effects of social learning and social utility, we need to decouple willingness to purchase (and the informative signal of the purchase decision) from possession. Consider the situation where investor i observes that investor j expressed a revealed preference to invest, but was not allowed to do so (perhaps due to capacity constraints). In this case, investor i infers that $s_j \geq \bar{s}_1$, but also knows that investor j did not obtain the asset, so $p_j = 0$. This condition is equivalent to the "social learning alone" problem discussed above: there is no direct effect of possession on investor i's utility from the asset, but there is social learning. Thus, investor i purchases the asset if and only if (4) is satisfied (since $u(x) = u(x|p_j = 0)$) and this leads to the same decision rule as (6) with the threshold $\bar{s}_2$.

The following proposition summarizes investor i's purchase decisions across conditions.

Proposition 1. *The threshold for the signal s_i above which investor i is willing to purchase the asset (and, the likelihood of a purchase of the asset by investor i) is highest (lowest) when the investor makes his decision in isolation, lower (higher) when he observes that investor j intended to purchase the asset but did not obtain it, and lowest (highest) when investor j intended to purchase the asset, and obtained it: $\bar{s}_1 \geq \bar{s}_2 \geq \bar{s}_3$ (and $\Pr(s_i \geq \bar{s}_3) \geq \Pr(s_i \geq \bar{s}_2) \geq \Pr(s_i \geq \bar{s}_1)$).*

Proof. The relationship between $\bar{s}_1$ and $\bar{s}_2$ follows immediately from comparing the inequalities (2) and (4) and the monotone likelihood ratio property of $f(x|s_i; s_j)$. Similarly, comparison of the inequalities (4) and (7) and $u(x) = u(x|p_j = 0) \leq u(x|p_j = 1)$ establishes that $\bar{s}_2 \geq \bar{s}_3$. Finally, $\Pr(s_i \geq \bar{s}_3) \geq \Pr(s_i \geq \bar{s}_2) \geq \Pr(s_i \geq \bar{s}_1)$ follows from the ranking of the thresholds. ■

[24] We are assuming here that the utility function discussed above, $u(x)$, is the same as $u(x|p_j = 0)$ here. In addition, we are assuming that investor j made his decision in isolation.

The difference between $\bar{s_2}$ and $\bar{s_3}$ is the result of a difference in investor j's possession of the asset.[25] In one situation investor j received favorable information and expressed an intent to purchase the asset, but was unable to execute the purchase due to supply restrictions. In the other situation investor j received a favorable signal and was also able to obtain the asset. Thus, in the two cases investor i infers the same information (via investor j's choice) about the potential returns of asset x. However, only in the latter case is investor i's utility directly influenced by the investment *outcome* (and not just the purchase *intention*) of investor j. This is the social utility effect that raises the expected utility of purchasing the asset for investor i over and above the social learning effect. In the inequalities in Proposition 1, the effect of social learning is captured by the difference between $\Pr(s_i \geq \bar{s}_2)$ and $\Pr(s_i \geq \bar{s}_1)$, and the effect of social utility is the difference between $\Pr(s_i \geq \bar{s}_3)$ and $\Pr(s_i \geq \bar{s}_2)$. The total peer effect is the difference between $\Pr(s_i \geq \bar{s}_3)$ and $\Pr(s_i \geq \bar{s}_1)$.

Our analysis readily extends to the case in which investor i's investment choice is continuous rather than limited to a binary decision. In particular, since $f(x|s_i; s_j)$ satisfies MLRP, the optimal investment in the asset is increasing in s_i and s_j and the expected equilibrium investment amounts will follow exactly the prediction regarding purchase rates in Proposition 1. Suppose individual i chooses an investment magnitude q_i^*, rather than making a binary investment decision. Since $f(x|s_i; s_j)$ satisfies MLRP, the optimal investment in the asset is increasing in s_i and s_j and we can rank the expected equilibrium investment amounts.

Proposition 2. *The expected equilibrium investment amount q_i^* of investor i is lowest when the investor makes his decision in isolation, higher when he observes that investor j intended to purchase the asset but did not obtain it, and highest when investor j intended to purchase, and obtained, the asset.*

Proof. The inference problem of investor i is the same as in Proposition 1. Thus, for a given signal s_i the described relationship holds for the actual equilibrium investment amount and follows immediately from comparing the expression for the utilities on the left-hand side of the inequalities (2), (4) and (7) and by noting that the optimal investment amount is increasing in s_i and s_j. Finally, taking expectations over the signal realizations s_i yields the ranking in expected investment amounts. ∎

[25]Note that the difference between $\bar{s_2}$ and $\bar{s_3}$ measures the impact of possession conditional on the presence of social learning. This is consistent with our experimental design, in which we are not able to measure the impact of possession in the absence of social learning.

Heterogeneous Investors

In practice, some investors are more financially sophisticated than others, and one would expect that this variation will affect the peer effects we study here – especially the impact of social learning. In particular, an unsophisticated investor may have much more to learn about an asset from the purchase decision of their peer than does a sophisticated investor, as the sophisticated investor likely has a very good sense of the asset's quality from his signal alone. Differing financial sophistication can be captured in our model by allowing the signals s_i and s_j to be drawn from distributions with differing precision. For simplicity, we make the assumption that, in contrast to unsophisticated investors, sophisticated investors receive perfectly informative signals. This assumption generates the following prediction of heterogeneous effects of social learning.

Proposition 3. *The thresholds $\bar{s}_1$ and $\bar{s}_2$ for the signal s_i above which investor i is willing to purchase the asset (and hence the likelihood of investor i purchasing the asset) are identical if investor i is financially sophisticated (i.e., signal s_i is perfectly informative). If investor j is sophisticated, then investor i follows the choice of investor j when observing the decision of investor j.*

Proof. If s_i is perfectly informative (i.e., investor i is sophisticated), then s_i is a sufficient statistic for x. As a result, s_j, and hence the purchase decision of investor j, has no informational value for sophisticated investor i and does not influence the threshold $\bar{s}_1$. Hence, $\bar{s}_1 = \bar{s}_2$. If s_j is perfectly informative, then investor j knows the value of x and makes a perfectly informed investment decision. As a result, investor i follows investor j's choice. ■

Proposition 3 suggests that social learning will be limited (in fact, given the simplifying assumptions made, will be nonexistent) for sophisticated investors. These investors are sufficiently well-informed that they are not influenced by the revealed preference of another investor. The proposition further shows that social learning will have relatively strong effects on investment choices if the investor whose choice is observed is sophisticated.[26]

[26] We have assumed that sophisticated investors receive perfectly informative signals. Our results can be extended to the case in which sophisticated investors receive more informative, but still imperfectly informative, signals. While results for general distributions of x, s_i and s_j that satisfy MLRP do not exist, it is straightforward to show that for binary signal structures, the impact of social learning will be relatively small when the observing investor is sophisticated and relatively large when the observed investor is sophisticated. Finally, it is worth noting that, another investor's *possession* of the asset could still affect financially sophisticated investors' choices; similarly a financially unsophisticated investor's purchase decision – when accompanied by possession – could influence a peer's choice. Both of these effects would work through the social utility channel. Thus, we emphasize that these predictions of heterogeneous treatment effects apply to social learning effects alone, but not necessarily the overall peer effect.

Appendix C: Experimental Documentation

We enclose here English versions of the Qualtrics scripts used by the brokers in the sales phone calls, first to investor 1's and then to investor 2's. Then we enclose English versions of the follow-up survey questionnaires. After these documents, we enclose a picture of the implementation of the experiment, displaying the brokers and the RA.

Client number

Name of broker making phone call

Client number

Introduction

Description of asset

Combination of two investments:
- Fundo Long-Short multi-mercado (read brochure)
- LCI de 98% do CDI (read brochure)

Minimum investment:
- R$1,000 in LCI and R$1,000 in Fundo Long-Short
Maximum investment:
-R$10,000 in LCI and no limits in Fundo Long-Short

Observations to be told to client:
1) Special LCI usually not avaliable to clients. LCI typically available to clients has return of 97% of CDI and minimum investment of R$10,000

2) Emphasize that product can only be purchased during this call (take it or leave it): will not be sold on other occasions

3) Remind that LCI is exempt from income tax

4) Explain that only new resources will be accepted (and not resources already invested with the brokerage)

Limited supply

This is a special asset, only available in limited supply, and only to special clients l ke you.

As so, unfortunately, some of the clients that want the asset will not be able to actually purchase it.

Since we are a company that always wants to be as fair as possible, we want to give a chance to all the special clients we are calling and who are interested in the product. In addition to that, we would like to give the same chance to everyone.

Because of that, we will use a lottery to determine which clients will actually be able to implement the purchase, among those that chose to purchase the asset.

In this lottery half (50%) of the clients that choose to purchase the asset will have their choices authorized and implemented.

The lottery consists in drawing a random integer number between 1 and 100. If the number is 50 or less, the lottery will not authorize the investment. If the number is greater than 50, the lottery will authorize and make the investment.

It is important that you know that the decision you will make now if final. If you decide to purchase the asset, you will be authorizing the purchase. Therefore, if the lottery authorizes the purchase, the investment will be made.

Take advantage of this great opportunity to buy this exclusive product!

Investment decision

Ask the client what their decision is

- Wants to invest
- Does not want to invest

How much does he want to invest in the Fundo Long-Short multi-mercado?

How much does he want to invest in the LCI?

Investment authorization

A random number will now be drawn to determine whether or not you will be able to actually make the investment. The random number is ${e://Field/random}

Due to the outcome of the lottery, your investment was not authorized.

Due to the outcome of the lottery, your investment was authorized.

Was the investment authorized?

- Yes
- No

End of Call and Summary

Finish the call

This is the summary of the call. Please put the following information in your Excel spreadsheet of communication with the clients:

Client number: ${q://QID20/ChoiceTextEntryValue}

Did this client want to invest in the product? Yes

Amount invested in the Fundo Long-Short: ${q://QID18/ChoiceTextEntryValue}

Amount invested in the LCI: ${q://QID26/ChoiceTextEntryValue}

Was this client authorized to make the investment? Yes

This is the summary of the call. Please put the following information in your Excel spreadsheet of communication with the clients:

Client number: ${q://QID20/ChoiceTextEntryValue}

Did this client want to invest in the product? Yes

Amount invested in the Fundo Long-Short: ${q://QID18/ChoiceTextEntryValue}

Amount invested in the LCI: ${q://QID26/ChoiceTextEntryValue}

Was this client authorized to make the investment? No

This is the summary of the call. Please put the following information in your Excel spreadsheet of communication with the clients:

Client number: ${q://QID20/ChoiceTextEntryValue}

Did this client want to invest in the product? No

Amount invested in the Fundo Long-Short: 0

Amount invested in the LCI: 0

Was this client authorized to make the investment? N/A

Client number

Name of broker making phone call

Client number

Number of client of the (first) friend of this investor

Previous Choice by FRIEND 1

Did the first friend of this investor want to invest in this asset?

- Yes
- No

Was the first friend of this investor authorized to make the investment?

- Yes
- No

Introduction

Description of Asset

Combination of two investments:
- Fundo Long-Short multi-mercado (read brochure)
- LCI de 98% do CDI (read brochure)

Minimum investment:
- R$1,000 in LCI and R$1,000 in Fundo Long-Short
Maximum investment:
-R$10,000 in LCI and no limits in Fundo Long-Short

Observations to be told to client:
1) Special LCI usually not avaliable to clients. LCI typically available to clients has return of 97% of CDI and minimum
investment of R$10,000

2) Emphasize that product can only be purchased during this call (take it or leave it): will not be sold on other occasions

3) Remind that LCI is exempt from income tax

4) Explain that only new resources will be accepted (and not resources already invested with the brokerage)

Limited Supply

This is a special asset, only available in limited supply, and only to special clients l ke you.

As so, unfortunately, some of the clients that want the asset will not be able to actually purchase it.

Since we are a company that always wants to be as fair as possible, we want to give a chance to all the special clients we are calling and who are interested in the product. In addition to that, we would like to give the same chance to everyone.

Because of that, we will use a lottery to determine which clients will actually be able to implement the purchase, among those that chose to purchase the asset.

In this lottery half (50%) of the clients that choose to purchase the asset will have their choices authorized and implemented.

The lottery consists in drawing a random integer number between 1 and 100. If the number is 50 or less, the lottery will
not authorize the investment. If the number is greater than 50, the lottery will authorize and make the investment.

It is important that you know that the decision you will make now if final. If you decide to purchase the asset, you will be authorizing the purchase. Therefore, if the lottery authorizes the purchase, the investment will be made.

Take advantage of this great opportunity to buy this exclusive product!

Only Learning Treatment

Before asking whether or not the client wants to purchase the asset, tell him the information associated with the choice of the first friend and the outcome of the lottery for the first friend:

"We would like to inform you, before you make your decision, that [FIRST FRIEND'S NAME], your [RELATIONSHIP TO THIS CLIENT], received the same offer today. He/she chose to purchase the product. However, the lottery did not authorize him/her to make the purchase, so he/she will not make the investment."

SUMMARIZING: He/she wanted to make the investment but was not able to invest.

Possession and Learning Treatment

Before asking whether or not the client wants to purchase the asset, tell him the information associated with the choice of the first friend and the outcome of the lottery for the first friend:

"We would like to inform you, before you make your decision, that [FIRST FRIEND'S NAME], your [RELATIONSHIP TO THIS CLIENT], received the same offer today. He/she chose to purchase the product. The lottery authorized him/her to make the purchase, so he/she will make the investment."

SUMMARIZING: He/she wanted to make the investment and was able to invest.

Investment Decision

Ask the client what their decision is

- Wants to invest
- Does not want to invest

How much does he want to invest in the Fundo Long-Short multi-mercado?

How much does he want to invest in the LCI?

Investment Authorization

A random number will now be drawn to determine whether or not you will be able to actually make the investment. The random number is ${e://Field/random}

Due to the outcome of the lottery, your investment was not authorized.

Due to the outcome of the lottery, your investment was authorized.

Was the investment authorized?

- Yes
- No

Relationship with First Investor, End of Call, and Summary

Had you previously heard about this offer/this product from [FIRST FRIEND'S NAME]?

- Yes
- No

What is your degree of relationship with [FIRST FRIEND'S NAME]? Examples: sibling, parent, friend, co-worker, etc.

Finish the phone call

This is the summary of the call. Please put the following information in your Excel spreadsheet of communication with the clients:

Client number: ${q://QID27/ChoiceTextEntryValue}

First friend's client number: ${q://QID30/ChoiceTextEntryValue}

Did the first friend want to invest in the product? ${q://QID21/ChoiceGroup/SelectedChoices}

Was the first friend authorized to make the investment? ${q://QID25/ChoiceGroup/SelectedChoices}

Did this client (second friend) want to invest in the product? Yes

Amount invested in the Fundo Long-Short: ${q://QID28/ChoiceTextEntryValue}

Amount invested in the LCI: ${q://QID38/ChoiceTextEntryValue}

Was this client authorized to make the investment? Yes

This is the summary of the call. Please put the following information in your Excel spreadsheet of communication with the clients:

Client number: ${q://QID27/ChoiceTextEntryValue}

First friend's client number: ${q://QID30/ChoiceTextEntryValue}

Did the first friend want to invest in the product? ${q://QID21/ChoiceGroup/SelectedChoices}

Was the first friend authorized to make the investment? ${q://QID25/ChoiceGroup/SelectedChoices}

Did this client (second friend) want to invest in the product? Yes

Amount invested in the Fundo Long-Short: ${q://QID28/ChoiceTextEntryValue}

Amount invested in the LCI: ${q://QID38/ChoiceTextEntryValue}

Was this client authorized to make the investment?: No

This is the summary of the call. Please put the following information in your Excel spreadsheet of communication with the clients:

Client number: ${q://QID27/ChoiceTextEntryValue}

First friend's client number: ${q://QID30/ChoiceTextEntryValue}

Did the first friend want to invest in the product? ${q://QID21/ChoiceGroup/SelectedChoices}

Was the first friend authorized to make the investment? ${q://QID25/ChoiceGroup/SelectedChoices}

Did this client (second friend) want to invest in the product? No

Amount invested in the Fundo Long-Short: 0

Amount invested in the LCI: 0

Was this client authorized to make the investment?: N/A

Follow-up Survey

Financial Literacy Survey

This survey was administered to investor 2's in conditions 1 and 2, and to their associated investor 1's.

(1) On a scale from 1 to 7, where 1 means very low and 7 means very high, how would you assess your overall financial knowledge?

1. Very low
2.
3.
4.
5.
6.
7. Very high

(2) Suppose you had $100 in a savings account and the interest rate was 8% per year. After 5 years, how much do you think you would have in the account if you left the money in the account to grow:

a. More than $108
b. Exactly $108
c. Less than $108
d. Do not know
e. Refuse to answer

(3) Imagine that the interest rate on your savings account was 5% per year and inflation was 7% per year. After 1 year, using the money that will be in the account, would you be able to buy:

a. More than what you can buy today
b. Exactly the same as what you can buy today
c. Less than what you can buy today
d. Do not know
e. Refuse to answer

(4) Do you think that the following statement is true or false? *"Buying a single company stock usually provides a safer return than a stock mutual fund."*

a. True
b. False

c. Do not know
d. Refuse to answer

(5) If interest rates rise, what will typically happen to bond prices?

a. They will rise
b. They will fall
c. They will stay the same
d. There is no relationship between bond prices and the interest rates
e. Do not know
f. Refuse to answer

Questions Regarding the Sales Call

(1) *For investor 2's in conditions 1, 2, and 3*

When the asset was offered to you in the beginning of the year, we had to use a lottery given that the asset was in limited supply. At that moment, you decided to purchase (not purchase) the asset. Was the presence of the lottery a significant factor in your decision?

a. Yes
b. No

(2) *For investor 2's in conditions 1, 2, and 3*

Before the result of the lottery, you made a purchase decision. Did you believe you could have changed your decision after the lottery?

a. Yes
b. No

(3) *For investor 2's in conditions 2 and 3*

When the asset was offered to you, you were informed that *[NAME OF THE ASSOCIATED INVESTOR 1]* wanted the asset, but that he/she lost the lottery (and he/she won the lottery).

In the lottery, you had 50% chance of winning and 50% chance of losing, independently of the result for *[NAME OF THE ASSOCIATED INVESTOR 1]*. When you were informed that *[NAME OF THE ASSOCIATED INVESTOR 1]* lost (won) the lottery, how did this affect your beliefs about the likelihood of winning the lottery?

a. It would be more likely to win the lottery
b. It would be less likely to win the lottery
c. The likelihood of winning the lottery would remain unchanged.

(4) *For investor 2's in conditions 2 and 3*

You were informed that *[NAME OF THE ASSOCIATED INVESTOR 1]* lost (won) the lottery. How did this affect your beliefs about the quality of the asset?

a. This should be a better investment.

b. This should be a worse investment.

c. No effect.

(5) *For investor 2's in condition 2*

Was wanting (not wanting) an asset that *[NAME OF THE ASSOCIATED INVESTOR 1]* could not have because he/she lost the lottery a significant factor in your decision?

a. Yes.

b. No.

(6) *For investor 2's in conditions 2 and 3*

You were informed that *[NAME OF THE ASSOCIATED INVESTOR 1]* wanted to purchase the asset. How did this affect your beliefs about the quality of the asset?

a. This should be a better investment.

b. This should be a worse investment.

c. No effect.

(7) *For investor 2's in condition 3 who decided to purchase the asset*

Was wanting to earn the same financial returns that *[NAME OF THE ASSOCIATED INVESTOR 1]* would earn a significant factor in your decision?

a. Yes.

b. No.

(8) *For investor 2's in condition 3 who decided to purchase the asset*

Was wanting the same asset that *[NAME OF THE ASSOCIATED INVESTOR 1]* had so that you could discuss the asset with him/her a significant factor in your decision?

a. Yes.

b. No.

(9) *For investor 2's in condition 3 who decided to purchase the asset*

Did you think about what *[NAME OF THE ASSOCIATED INVESTOR 1]* could do with the return from the asset when you made your decision?

a. Yes.

b. No.

(10) *For investor 2's in condition 3 who decided to purchase the asset*

You were informed that *[NAME OF THE ASSOCIATED INVESTOR 1]* had the asset. Was the fear of not having a return he/she could have a significant factor in your decision?

a. Yes.

b. No.

(11) *For investor 2's in conditions 2 and 3*

The broker informed you that *[NAME OF THE ASSOCIATED INVESTOR 1]* wanted to purchase the asset. Did you believe in this information?

a. Yes.

b. No.

(12) *For investor 2's in conditions 2 and 3*

Your choices were never revealed to other clients. Still, were you concerned about this possibility when you decided to purchase (not to purchase) the asset?

a. Yes.

b. No.

Figure A.4: **Picture from the implementation**