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Understanding Peer Effects in Financial Decisions:Evidence from a Field Experiment

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Abstract

Using a high-stakes field experiment conducted in partnership with a large financial brokerage in Brazil, we attempt to disentangle channels through which a person's financial decisions affect his peers'. When someone purchases an asset, his peers may want to purchase it as well, both because they learn from his choice ("social learning") and because his possession of the asset affects others' utility of owning the same asset ("social utility"). We randomize whether one member of a peer pair is allowed to possess an asset that he chose to purchase. 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 own the asset *and* the result of the randomization that determined possession. This allows us to estimate the effects of (a) learning plus possession, and (b) learning alone, relative to a control group, allowing us to separately identify effects of the "social learning" and "social utility" channels. In the control (no information) group, 42% of individuals purchased the asset; this increases to 71% in the "social learning only" group; and, it increases to 93% in the "social learning and social utility" group. We find that *both* "social learning" and "social utility" channels are quantitatively important, and have independent, statistically significant effects on the decision to purchase the asset.

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 several fields of economics and social psychology.[1] 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.[2] 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 (and the income or consumption derived from possessing the asset).[3] Regardless of the channel studied, the empirical evidence of peer effects in financial markets has typically been correlational, and thus difficult to interpret as causal.[4]

Indeed, identifying the causal effect of one's peers' behavior on one's own is notoriously difficult.[5] Correlations in the investment or consumption choices of related individuals might arise without any causal peer effect: for example, peers select into social groups, and this might generate correlated choices; peers share common environments (and changes in those environments), and this, too, might generate correlated choices.

[1] In the economics literature, theoretical models of herding and social learning include Banerjee (1992) and Bikhchandani et al. (1992). Empirical work includes studies ranging from the impact of one's college roommates on one's educational outcomes (Sacerdote, 2001), to the impact of one's community on one's health and economic outcomes (Kling et al., 2007), to the impact of social learning on movie revenues (Moretti, 2011) and on participation in welfare (Bertrand et al., 2000). Social learning has also been studied experimentally in a laboratory setting by Celen and Kariv (2004). Within the social psychology literature, Asch (1951) famously studied individuals' conformity to group norms; in his "social comparison theory," Festinger (1954) posited that one resolves uncertainty by learning from others; Burnkrant and Cousineau (1975) distinguished between "informational" and "normative" reasons why one might conform.

[2] See for instance Bikhchandani and Sharma (2000) and Chari and Kehoe (2004).

[3] 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).

[4] See for instance Hong et al. (2005), Ivkovic and Weisbenner (2007), and Li (2009). Duflo and Saez (2003) and Beshears et al. (2011) are notable exceptions, though they do not attempt to disentangle the effects of learning from peers and peers' possession.

[5] A thorough discussion of the challenge is found in Manski (1993).

More difficult still is to identify *why* one's consumption or investment choices have a social component.[6] Broadly, there are two reasons why a peer's act of purchasing an asset (or product, more generally) would affect one's own choice:

(i) One infers that assets (or products) purchased by others are of higher quality. We refer to this as *social learning*.[7]

(ii) One's utility from possessing an asset depends directly on another individual's level of asset ownership (or product consumption); we call this *social utility*.[8]

If one were to identify random variation in one group member's decision to purchase an asset, one would be able to identify a causal peer effect. However, one would generally have a difficult time identifying whether the peer effect works through informational channels ("social learning") or through direct effects on the utility an individual derives from the asset ("social utility"). The obstacle facing the researcher is that most revealed preferences expressed by a group member (which would be informative to his peers) come hand-in-hand with that group member's *possession* of the product or asset that was revealed to be preferred (which would directly affect other individuals' utility from making a similar decision). To disentangle social learning from social utility one needs to identify, or create, a context in which possession of a product is decoupled from a revealed preference to possess the product.

Our experimental design 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

[6]Banerjee et al. (2011) study the diffusion of microfinance through social networks, and structurally estimate the importance of different potential channels linking peers' decisions.

[7]Identifying social learning is the focus of Moretti (2011) and Cai et al. (2009). They do not try to separately identify the importance of the two channels, but instead try to rule out the importance of social utility in the contexts they study. In our study, we focus on social learning arising from the information one acquires from the fact that one's peer purchased a financial product. 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) and from any change in behavior due to increased salience of a product when consumed by one's peers.

[8]What we call "social utility" can arise from concerns about relative income or consumption ("keeping up with the Joneses"), from greater utility from joint consumption of a good or simply from a desire to conform. Note that even in the absence of truly "social" preferences, one might observe greater demand for an asset simply because a peer holds it: this might arise as a result of competition over scarce consumption goods, for example. Because we do not wish to abuse the term, "social preferences," we prefer the broader "social utility" term, which will include social preferences, as well as general equilibrium-induced differences in demand. Evidence consistent with individuals caring about relative income has been presented by Card et al. (2010) and Luttmer (2005), among others.

social utility. Working with a top financial brokerage in Brazil, we offered a financial asset to pairs of clients who share a social network, either friends or family members.[9] The stakes were high: minimum investments were R$2,000, around 45% of investors' average monthly incomes. 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).[10]

To disentangle the effect of investor 1's possession from the effect of the information conveyed by investor 1's revealed preference (their decision to purchase the asset), 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).[11] 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 implement a second, independent randomization to determine the information received by the associated investor 2's: we randomly assign investor 2 to receive information about investor 1's investment decision and lottery outcome, or to receive no information. Thus, among investor 1's who chose to purchase the asset, the associated investor 2's are randomly assigned to one of three conditions: (1) no information about investor 1's decision; (2) information that investor 1 made a decision to purchase the asset, but was not able to consummate the purchase (so learning occurs *without possession*); and, (3) information that investor 1 made a decision to purchase the asset, and was able to consummate the purchase (so learning occurs, along with possession).[12] A comparison of choices made by investor 2 in conditions (1) and (2) reveals the effect of social learning[13]; a comparison of (3) and (2) reveals the impact

[9] The experimental design is discussed in more detail below. In particular, we describe the characteristics of the financial asset that were required to run the study, as well as the details of the experimental treatments.

[10] The assignment to the roles of investor 1 and investor 2 was random. In addition, we randomized whether investor 2 received information about the investment made by investor 1. This is discussed below.

[11] Individuals understood that their decision to purchase the asset might not be implemented. We do not believe the individuals made their decisions lightly, however, as confirmed purchases were implemented.

[12] Investor 2 also has his decision to purchase confirmed or rejected by lottery, so he is informed about the procedure that investor 1 experienced. We have no reason to expect that investor 2 viewed investor 1's decision as anything other than a revealed preference.

[13] Admittedly, there may be other effects captured by this comparison: one might wish to purchase an asset specifically because one's peer *could not* purchase it. On the other hand, one might feel guilty about purchasing it.

of investor 1's possession of the asset over and above the information conveyed by his purchase[14]; a comparison of (3) and (1) reveals the total effect of these two channels. This design allows us to cleanly identify contributions of social learning and social utility in generating the overall peer effects we observe.[15]

Our experimental evidence suggests that *both* channels through which peer effects work are important. We find that among investor 2's whose peer chose to purchase the asset: uninformed individuals chose to purchase the financial asset at a rate of around 45%; among those informed that investor 1 wanted the asset, but was unable to purchase it, the rate increased to 70%; finally, among those informed that investor 1 wanted the asset, and was able to purchase it, the rate increased to over 90%. There are large, statistically significant peer effects; in addition, we find that each channel – social utility and social learning – is individually economically and statistically significant. We find that individuals learn from their peers, but also that there is an effect of possession beyond learning.

The paper proceeds as follows: in Section 2, we present a simple model illustrating different sources of correlated investment decisions among related individuals, pointing to the challenge of identifying the channels through which peer effects work. Next, in Section 3, we describe in detail our experimental design, which attempts to separately identify those channels; in Section 4, we present our empirical specification and the results of our experiment; finally, in Section 5, we offer concluding thoughts.

We believe that learning about the asset is likely the most important effect captured by this comparison. This view is consistent with our finding that "learning" is significantly greater when investor 2 has a "non-technical" occupation (outside of mathematics, economics, finance, and accounting) than when investor 2 has a technical occupation. We also find that "learning" is greater when investor 1 has a technical occupation. This is discussed in our empirical analysis, below.

[14]It is important to stress that we cannot identify *why* one's peer's possession of the asset matters, but the finding that possession matters, regardless of the channel, is of interest.

[15]It is difficult to quantitatively estimate the effect of possession above learning, as the purchase rate in condition (3) is very close to 100%. Because the purchase rate is bounded above, we estimate what may roughly be thought of as a lower bound of the effect of possession.

2 A simple model of financial decision making with social influence

2.1 Social Learning

Consider an investor i's decision to invest in a risky asset. 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. Prior to the purchase decision each investor receives a signal s_i that is (imperfectly) informative about the return x of the asset. For expositional simplicity we assume that the conditional density $f(x|s_i; s_j)$ satisfies the monotone likelihood ratio property (MLRP) such that, intuitively, higher values of s_i and s_j for $j \neq i$ are indicative of higher values of x. Investor i is willing to invest if and only if

$$\int u(x) f(x|s_i) dx \geq \bar{u}. \tag{1}$$

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 $\bar{s}_{\emptyset}$ such that for any $s_i \geq \bar{s}_{\emptyset}$ investor i is willing to invest. Denote the purchase decision by investor i by $b_i = \{0, 1\}$. Hence, we have

$$b_i = 1 \Leftrightarrow s_i \geq \bar{s}_{\emptyset} \tag{2}$$

Suppose that instead of making her investment choice in isolation, before making her own decision investor i observes the investment decision of investor j which is given by b_j. Assume that investor j made her choice in isolation and hence her decision rule is given by (2). Thus, when investor i observes $b_j = 1$ he correctly infers that $s_j \geq \bar{s}_{\emptyset}$ and he is willing to invest if and only if

$$\int u(x) f(x|s_i; s_j \geq \bar{s}_{\emptyset}) dx \geq \bar{u} \tag{3}$$

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

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

for all s_i.[16] It is straightforward to show by comparing (3) and (1) 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 her choice in isolation. This is because in the former case regardless of his own private information summarized by s_i investor i has additional good information about the asset from observing the purchase of investor j. This is the pure information effect. Denote the threshold for s_i when investor i observes $b_j = 1$ by $\bar{s}_1$ and note that $\bar{s}_1 \leq \bar{s}_\emptyset$. In particular, the decision rule of investor i is now given by

$$b_i = 1 \Leftrightarrow s_i \geq \bar{s}_1 \tag{5}$$

From an empirical standpoint we are interested in comparing the proportion of investors who are willing to purchase the asset when making their decision in isolation relative to the proportion of investors choosing to purchase the asset when they observe another investor's purchase. In both cases the purchase decision depends on the signal realization of s_i and since $\bar{s}_\emptyset \geq \bar{s}_1$ the likelihood of a purchase in the former case is lower than in the latter

$$\Pr(s_i \geq \bar{s}_1) \geq \Pr(s_i \geq \bar{s}_\emptyset) \tag{6}$$

The information effect is present when agents make informational inferences about the quality of a product, in our case the returns of a risky asset, from the choice of other agents. However, as we shall show in the next section purchasing decisions do not only lead investors to update their beliefs about the returns of an asset, but also change the intrinsic desirability of owning the asset for any given level of realized returns. This latter effect is likely to be particularly important for

[16]Note that our simple model uses the canonical ingredients of a social learning model: private signals, s_i and s_j, and a (discrete) action of another player, b_j, which player i uses for informational inference before taking his own action, b_i.

agents who are socially connected. Thus, using observational data on peer effects in investing it is difficult, if not impossible, to separate the pure information effect from the consequences for investing stemming from social preferences.[17]

2.2 Social Utility and Social Learning

We now consider the situation where investors also exhibit "social" utility functions, for example due to a taste for conformity, joint consumption or "Keeping-up-with-the-Joneses" preferences. Thus, peer effects may now come through two channels: social learning and social utility. In practice it is difficult to disentangle these two effects as revealed preference usually goes hand in hand with possession. We denote possession of the asset by investor i by $p_i = \{0, 1\}$. Using the notation of our model in observational data, an investor j's purchase of an asset, $b_j = 1$, typically implies both that investor i infers positive information about the asset, $s_j \geq \bar{s}_\emptyset$, as well as that investor j now possesses the asset, $p_j = 1$.

In our experimental design we attempt to decouple the two effects. We model social utility using a parsimonious approach that encompasses many of the different explanations of social utility highlighted above, all of which result in a *positive* effect of a peer's possession on one's own utility.[18] In particular, we assume that $u(x) = u(x|p_j = 0) \leq u(x|p_j = 1)$ for all x. That is, investor i's utility is higher for all asset return realizations if the asset is also owned by an investor j who is a peer of investor i.

Investor i's decision when making his choice in isolation is still unchanged from the previous section. In particular, investor i is willing to invest if and only if (1) is satisfied and his decision rule is given by (2) with the threshold $\bar{s}_\emptyset$.

Now, when investor i observes that investor j expressed an intention to invest, that is $b_j = 1$, and was allowed to invest, $p_j = 1$, both investor i's utility $u(x|p_j = 1)$ *and* his information about

[17] Note that the degree of social relatedness need not be high for one investor's possession of an asset to affect another investor's utility of possessing the asset. For example, investors may live in the same neighborhood and observe each other's conspicuous consumption; investors may live in the same city and compete over scarce goods (e.g., housing, slots in elite schools, etc.).

[18] One could also imagine a *negative* correlation, for example, out of a desire to insure one's peers.

the asset $f(x|s_i; s_j \geq \bar{s}_\emptyset)$ are affected. Investor i invests if and only if

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

Denote the threshold for s_i above which investor i is willing to invest by $\bar{s}_2$. Thus the decision rule for investor i is given by

$$b_i = 1 \Leftrightarrow s_i \geq \bar{s}_2 \tag{8}$$

To separate the effects of social learning and social utility, we need to decouple willingness to purchase from possession. Consider the situation where investor i observes that investor j expressed an intention to invest, but was not allowed to do so due to capacity constraints. That is to say, investor i knows about investor j's intention to invest and thus infers that $s_j \geq \bar{s}_\emptyset$, but also knows that investor j did not obtain the asset, that is $p_j = 0$ rather than $p_j = 1$. Thus, investor i purchases the asset if and only if (3) is satisfied since $u(x) = u(x|p_j = 0)$ and this leads to the same decision rule as (5) with the threshold $\bar{s}_1$.

The following proposition summarizes our findings.

Proposition 1. *The threshold for the signal s_i above which investor i is willing to purchase the asset (and conversely 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 and obtained the asset, or $\bar{s}_\emptyset \geq \bar{s}_1 \geq \bar{s}_2$ (and $\Pr(s_i \geq \bar{s}_2) \geq \Pr(s_i \geq \bar{s}_1) \geq \Pr(s_i \geq \bar{s}_\emptyset)$).*

Proof. The relationship between $\bar{s}_\emptyset$ and $\bar{s}_1$ follows immediately from comparing the inequalities (1) and (3) and the monotone likelihood ratio property of $f(x|s_i; s_j)$. Similarly, comparison of the inequalities (3) and (7) and $u(x) = u(x|p_j = 0) \leq u(x|p_j = 1)$ establishes that $\bar{s}_1 \geq \bar{s}_2$. Finally, $\Pr(s_i \geq \bar{s}_2) \geq \Pr(s_i \geq \bar{s}_1) \geq \Pr(s_i \geq \bar{s}_\emptyset)$ follows from the ranking of the thresholds. ■

The difference between $\bar{s_1}$ and $\bar{s_2}$ is the result of a difference in possession of the asset.[19] In one

[19] Note that the difference between $\bar{s_1}$ and $\bar{s_2}$ 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

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 pure information effect. Thus, the likelihood of investor i purchasing the asset is lowest when making 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. In the inequalities in Proposition 1, social learning is captured by the difference between $\Pr(s_i \geq \bar{s}_1)$ and $\Pr(s_i \geq \bar{s}_\emptyset)$, and the effect of social utility is the difference between $\Pr(s_i \geq \bar{s}_2)$ and $\Pr(s_i \geq \bar{s}_1)$. The total peer effect is the difference between $\Pr(s_i \geq \bar{s}_2)$ and $\Pr(s_i \geq \bar{s}_\emptyset)$.

The above relationships imply that we should observe investors choosing to invest in the asset more often (relative to the case in which they make their choice in isolation) if they observe another investor (i) expressing an intent to purchase and, *a fortiori*, (ii) expressing an intent to purchase as well as obtaining the asset. Furthermore, our analysis readily extends to the case where 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 x is increasing in s_i and s_j and thus 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, or $E[q_\emptyset^*] \leq E[q_1^*] \leq E[q_2^*]$.*

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

possession in the absence of social learning.

(1), (3) 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. ■

So far we have assumed that investors are *ex ante* identical and that the only respect in which they differ is the realization of the informative signal they receive. In practice, however, some investors are more financially sophisticated than others. One might expect, as a result, that an unsophisticated investor learns more about an asset from the purchase decision of a peer than a sophisticated investor would learn, as the latter is likely to have a very good sense of the asset's quality from his own signal. 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. We make the simplifying assumption that, in contrast to unsophisticated investors, sophisticated investors receive perfectly informative signals.

Proposition 3. *The thresholds $\bar{s}_\emptyset$ and $\bar{s}_1$ 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 sophisticated (i.e., signal s_i is perfectly informative). If investor j is sophisticated then when observing the decision of investor j investor i follows the choice 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, $\bar{s}_\emptyset = \bar{s}_1$. 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 shows that social learning will be limited (in fact, given the assumptions made, will be irrelevant) 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 has very strong effects on investment choice if the investor whose choice is observed is sophisticated.[20]

3 Experimental Design

The primary goal of our experimental design was to generate 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 inability of the peer to make the investment due to a computerized lottery; and, 3) informed of their peer's revealed preference to purchase an asset, and the peer's successful investment due to a computerized lottery. In implementing the design, we (and the brokerage with which we worked) needed to structure a financial asset that possessed particular characteristics, and also implement several stages of randomization in the process of selling the asset.

3.1 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; and, the asset needed to be sufficiently desirable that *some* individuals would choose to purchase it, even in the absence of peer effects.[21] To satisfy these requirements, the brokerage created a new, risky asset specifically for this study. The asset is a combination of an actively-managed mutual fund and a real estate note (the asset is described in detail in the Appendix). The brokerage sold the asset, varying its precise composition, prior to the experiment (to clients other than those in the current study) in order to calibrate a purchase rate of around 50%.

We did not want there to be a secondary market for the asset for several reasons. First, we hope 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

[20]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.

[21]Many of our comparisons of interest are among those investor 2's whose associated investor 1's chose to purchase the asset, and investor 1 never receives any information about his peer.

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 – there was a single opportunity to invest – and structured the asset as having a fixed term with no resale – once the investment decision was made, the investor would simply wait until the asset matured and then collect his returns.

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 for the study.

3.2 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. We initially created the script using Qualtrics, a web-based survey platform. The brokerage did not ask their brokers to use the web-based survey in all of their calls, as this proved cumbersome. The brokers were made intimately familiar with the script, however, and used Excel to generate the randomization needed to execute the experimental design. The brokers entered the results of the randomization and the purchase decisions in an Excel spreadsheet, which they then delivered to the authors.

The sales calls made by brokers possessed several important characteristics. First, and most importantly, the sales calls were extremely natural: sales calls were frequently made by the brokerage in the past; investments resulting from brokers' calls are thus in no way unusual. Second, the experimental calls were made by the individual brokers who were accustomed to working with the clients they called as part of the study; and, the calls only deviated from brokers' typical sales calls as required to implement the experimental design. As far as we know, no client suspected that the calls were being made as part of our experiment.[22] Third, because brokers were compensated based on the assets they sold, they were simply incentivized to sell the asset in each condition (rather than to confirm any particular hypothesis).[23]

Between January 26, and April 3, 2012, brokers called 150 pairs of clients whom brokers had previously identified as having a social connection. 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. In the context of our experiment, 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 assume that without any mention of the offer being made to the other member of the peer pair, there will be no peer effect, though of course this may not be exactly true in reality.[24]

One member of the pair was randomly assigned to the role of "investor 1," while the second member was assigned to the role of "investor 2." 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. In practice, if the investor chose to purchase the asset, 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. One might naturally be concerned that knowledge of the lottery would affect the decision to invest. This would, of course, be of greatest concern to us if any effect of the lottery interacted with treatment status. It is reassuring to know, however, that in the brokerage's initial calls to calibrate the asset's purchase rate – which did not mention the

[22]Thus, our study falls into the category "natural field experiment", according to the classification in Harrison and List (2004).

[23]Thus, brokers would have used the available information in each experimental treatment as effectively as possible. Any treatment effects measured are can be thought of as the effects of information about a peer's choice (or choice plus possession) when that information is "optimally" used by a salesperson. Of course, in reality, information about peers' choices may be received from the peer (rather than from a sales person), or may not be observed at all; the magnitudes of our effects should be interpreted with this in mind.

[24]We have no reason to think that the individual pair members were thinking of each other; however, we asked the brokerage if any client mentioned their peer in the sales call, and the brokerage indicated that this never occurred.

lottery – the purchase rate was very similar to what we observe in the control group in our study: 12 of 25 investors chose to purchase the asset when the lottery was not mentioned, very similar to the control groups in our study.[25]

Following the call to investor 1, the brokerage 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.[26] 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.[27] 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, 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.

3.3 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 the three conditions mentioned above (see Figure 1 for a graphical depiction of the

[25] The purchases all involved new investments; clients were not allowed to transfer money from existing investments to purchase the new asset.

[26] This restriction also limited the ability of investor pairs to coordinate their behavior (for example, organizing side payments). Discussions with the brokerage revealed that 6 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 our results.

[27] To be clear: brokers called 162 investor 1's in order to attain our targeted sample size of 150 pairs successfully reached.

randomization):

One-third were assigned to the "no information," control condition, condition (1). They were offered the asset just as was investor 1, with no mention of an offer made to their peer.[28] Two-thirds received information about their peer's decision to purchase the asset, as well as the outcome of the lottery that determined whether the peer was allowed to invest in the asset. The randomization resulted in approximately one-third of investor 2's in condition (2), 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 (3), 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. Importantly, the investor 1's who chose to purchase the asset were not an unusual subset of the clients in the study. When comparing investor 1's who chose to purchase the asset to those who chose not to purchase it, the means of observables are very similar; we do not find any statistically significant differences (results available upon request). This suggests that we are not getting a selection of investor 1's that invest who are very different from the original pool of clients who were reached.[29]

If the double randomization design was successfully implemented, the investor 2's in conditions (1), (2), and (3) will differ only in the information they receive about their peer's purchase decision and possession of the asset. As a check of the randomization, we present in Table 1, columns 1–3, the individual investors' characteristics for each of the three groups. As expected from the random assignment into each group, the sample is well balanced across the baseline variables (p-values from tests of equality of means across groups are presented in Table 1, column 4).

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. We assign all of these investor 2's to the "no information" condition. We did not reveal their peers' choices because the brokerage did not

[28] We can think of these investor 2's as "positively selected" relative to the set of investor 1's, as the latter were a random sample of investors, while the former are specifically those whose peer chose to purchase the asset.

[29] In addition, we find that in the "no information" condition, investor 2's associated with investor 1's who chose to purchase the asset have a similar purchase rate to investor 2's associated with investor 1's who chose not to purchase the asset.

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 (1). We refer to these investor 2's as those in the "negative selection" condition, as the information they receive is identical to that received by investor 1's and investor 2's in condition (1); however, the investor 2's in the "negative selection" condition are those whose peers specifically chose *not* to purchase the asset.

3.4 Treatment Effects of Interest

Our experimental design allows us to make several interesting comparisons across groups of investors. First, we can disentangle the channels through which peers' purchases affect investment decisions. Consider the set of investor 2's whose peers had chosen to purchase the asset (whether the investment was implemented or not). Among these investor 2's, a comparison of those in conditions (1) and (3) reveal the standard peer effect: in condition (1), there is no peer effect active, as no mention was made of any offer being made to the other member of the peer pair; in condition (3), the investor 2 is told that investor (1) successfully invested in the asset (so both social utility and social learning channels are active).

Comparing investor 2's (again those whose peer chose to purchase the asset) in conditions (1) and (2) will allow us to estimate the impact of social learning from a peer's decision *but without possession*. Comparing investor 2's purchase decisions in conditions (2) and (3) will then allow us to estimate the impact of a peer's *possession alone*, over and above learning from a peer's decision, on the decision to invest.[30]

Finally, consider the comparison of the purchase decisions made by investor 1's and the purchase decisions made by investor 2's whose peer had chosen to purchase the asset, and who are randomly assigned to condition (1). Neither of these groups received any information about decisions made by another individual, but they are different in an important way: while the former group is a

[30]It is important to note 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 2, working against finding any statistically significant peer effects beyond social learning.

random subset of our subject population, the latter is a *selected* sample whose peer had chosen to purchase the asset. Testing for differences in purchasing decisions between these groups thus allows us to estimate the importance of selection on asset purchasing decisions in our sample (the effect of selection can also be seen by comparing investor 1's with those investor 2's whose peer chose *not* to purchase the asset). While selection is not a focus of our analysis, we will examine it in our empirical work, below.

4 Empirical Analysis

Before more formally estimating the effects of the experimental treatments, we present the take-up rates in the raw data across categories of investor 2's (see Figure 2). Among those investor 2's in the "no information" condition (1), the take up rate was around 42%; in the "social learning alone" condition (2), the take-up rate was around 70%; in the "social learning plus social utility" condition (3), the take-up rate was just over 90%. In the raw data, we observe economically and statistically significant peer effects: the difference of around 50 percentage points in take-up rates between conditions (1) and (3) is economically meaningful (and statistically significant at 1%), indicating the relevance of peer effects in portfolio allocations; moreover, we observe sizable and statistically significant effects of learning alone, and of possession above learning as well.[31]

Finally, we do not see a big "selection" effect: investor 2's (in the "no information" condition) whose peer chose not to purchase the asset have a very similar take-up rate to investor 2's whose peer chose to purchase the asset (in condition (1)).

4.1 Regression Specifications

To identify the effects of our experimental treatments, we will estimate regression models of the following form:

[31] The p-value from a test of equality between take-up rates in conditions (1) and (3) – the overall peer effect – is 0.000. The p-value from a test that (2) equals (1) – social learning alone – is 0.040. The p-value from a test that (3) equals (2) – possession's effect above social learning – is 0.047.

$$Y_i = \alpha + \sum_c \beta_c I_{c,i} + \gamma' \mathbf{X}_i + \epsilon_i. \quad (9)$$

Y_i is an investment decision made by investor i: in much of our analysis it will be a dummy variable indicating whether investor i wanted to purchase the asset, but we will 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 most of our analyses, we will focus on investor 2's, so $c \in \{\text{condition (1)}, \text{condition (2)}, \text{condition (3)}, \text{“negative selection”}\}$. In some cases, we will also include investor 1's in our analysis, and they will be assigned their own category c. We always omit the investor 2's in condition (1), that is, those investor 2's associated with a peer who wanted to purchase the asset, but who received no information about their peer. Finally, in some conditions we will include control variables: X_i is a vector of broker fixed effects, and investors' baseline characteristics.

4.2 Empirical Estimates of Social Utility and Social Learning

We first present the treatment effects of interest using an indicator of the investor's purchase decision as the outcome variable, and various specifications, in Table 2. We begin by estimating a model using only investor 2's and not including any controls. These results match the raw data presented in Figure 2: take-up rates are estimated relative to the omitted category, investor 2's in condition (1), who had a take-up rate of 42%. As can be seen in the table, the overall peer effect – the coefficient on the indicator for condition (3) – is over 50 percentage points, and is highly significant.

In addition, social learning and social utility are individually significant. Investor 2's in condition (2) purchased the asset at a rate nearly 29 percentage points higher than those in condition (1), and the difference is significant. This indicates that learning without possession affects the investment decision. The difference between the coefficient on condition (3) and the coefficient on condition (2) indicates the importance of possession *beyond* social learning. Indeed, as can be seen in Table

2, the 22 percentage point difference between these conditions is statistically significant.

In addition to estimating the importance of peer effects, and separately identifying the importance of the social learning and the social utility channels, our experimental design allows us to estimate the importance of selection in generating correlated portfolio allocations among peers. The coefficient on the "negative selection" variable, in column 1 of Table 2, gives us the difference in take-up rates between investor 2's whose peers did not want to purchase the asset and investor 2's whose peers wanted to purchase the asset, but received no information about that (condition (1)). Since neither of these investors received information about their peers, any difference in take-up rates would be associated with selection. In fact, the estimated selection effect is economically small, and it is not statistically significant.

We next present regression results including broker fixed effects (Table 2, column 2), including broker fixed effects and baseline covariates (Table 2, column 3), and a regression using the combined sample of investor 1's and investor 2's in order to have more precision (Table 2, column 4). The overall peer effect, as well as the individual social learning and the social utility channels, estimated using these regression models are very similar across specifications.

The results across specifications in Table 2 indicate sizable peer effects in financial decision making. Moreover, they suggest that both channels through which peer effects work are meaningful in the real world. Though our context is not perfectly general, the results lend support both to models of peer effects emphasizing learning from others as well as to those emphasizing keeping up with the Joneses and other "social utility" channels.

4.3 Alternative Outcomes

We next examine the robustness of our findings in Table 2. First, we consider two alternative outcome variables: the amount invested in the asset, and a dummy variable indicating whether the investment amount was greater than the minimum required. In Tables 3 and 4, we replicate the specifications presented in Table 2, but use the alternative outcome variables. As can be seen in Tables 3 and 4, the results using these alternative outcome variables are very similar to the ones using take-up rates. We observe significant peer effects, significant social learning, and significant effects of possession beyond learning.[32]

4.4 Treatment Effect Heterogeneity

As discussed in Section 2, the importance of the learning channel is likely to depend on investors' financial sophistication. Financially sophisticated investors should put more weight on their own signal relative to information derived from their peers' revealed preferences. Therefore, the learning channel should be less important for more financially sophisticated investors. Similarly, the information revealed by the action 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.

Although we do not observe direct measures of financial sophistication for the investors in our sample, we can use information on their occupations as a proxy. Investors who have technical occupations (for example, professions related to economics, mathematics or statistics) are likely to be more financially sophisticated than those who do not (see the list of occupations in our sample and their coding in Table 6). Therefore, we examine the heterogeneity in the social learning effect according to whether investor 2 or investor 1 has a technical occupation.[33]

To identify the social learning effect for different groups of investors, we compare the take-up rates of investor 2's in conditions 1 and 2.[34] Panel A of Table 5 presents comparisons of means and Panel B reports effects estimated using the full specification of column 4 from Table 2 adding an interaction of the condition 2 dummy with the technical occupation dummy. In columns 1 and 2 of Table 5, we present the social learning effects separately for investor 2's who have, and who do not have, technical occupations, respectively. The social learning effect is large and statistically significant when investor 2 does not have a technical occupation, while it is not statistically significant when investor 2 has a technical occupation. We can reject at 1% that the

[32] The only difference worth noting is that we observe marginally significant differences between investor 1's and investor 2's in condition (1) when examining the likelihood of investing more than the minimum.

[33] Since our experimental design allows us to quantify the importance of the social learning channel, but it only allows us to test for a qualitative effect of possession over and above learning (because our estimates of the latter are more likely to be affected by the upper bound of 100% take-up), we focus on the social learning channel when contrasting the magnitude of peer effects for the different groups of investors.

[34] We drop observations with "undetermined occupations", i.e., occupations that cannot be coded as technical or non-technical, such as broad categories like "teacher" and "retired."

learning channel is the same for the two groups (see Table 5, column 3).[35]

In Table 5, columns 4–6, we focus on learning *from* more or less sophisticated investors. In columns 4 and 5 of Table 5, we present the social learning effects of interest separately for investor 2's whose associated investor 1's have, and who do not have, technical occupations, respectively. While we do not find statistically significant differences in social learning, one can see that the point estimate of the effect of social learning is larger when learning is from an investor 1 who had a technical occupation (see Table 5, column 4). The social learning effect is estimated to be closer to zero when learning is from an investor 1 whose occupation is not technical (see Table 5, column 5). However, we cannot reject the null hypothesis that the effects for these two groups are the same (see Table 5, column 6).

Taken together, our results are broadly consistent with the hypothesis that social learning is more likely to occur when the party receiving information is less financially sophisticated, and when the party sending information is more financially sophisticated. These results are both interesting for their own sake, and also provide evidence that the treatment effects observed in the "social learning alone" condition are truly due to learning, rather than to some other factor.

5 Conclusion

Peer effects are an important, and often confounding, topic of study across the social sciences. Experimental variation can allow one to identify peer effects working through different channels, but often – especially in finance – identifying *why* one's peers choices affect one's own is extremely important. 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 choice, and changes in behavior due to a peer's possession of an asset, both affect investment decisions.

Our findings should be extended in several directions. Most fundamentally, it is important to

[35]We also find that the social utility effect beyond learning is positive for investor 2's who have a technical occupation, and zero for those who does not. However, as mentioned before, this difference is most likely driven by the fact that the take-up rate for the latter group is already close to one with only the learning channel active, leaving not much space for possession beyond learning to have a measurable effect. These results are available upon request.

test the external validity of our experimental findings. To the extent that our results shed some light on financial decision making beyond our experimental context, it is important to understand what our results imply for asset pricing and for policies that attempt to limit financial market instability. For example, if one focuses on the "social learning" channel through which peer effects work, information provision can limit herding based on little actual information. On the other hand, information provision will be less successful in limiting correlated choices among peers if those correlated choices are driven by "social utility."

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's "likes" are merely stated preference; Groupon relies on learning from revealed preference and possession; many products are given away to stimulate "keeping up with the Joneses" or conformity. Studies can compare the effectiveness of these strategies using designs similar to ours. Another setting is technology adoption: Foster and Rosenzweig (1995) and Conley and Udry (2010) identify the important role played by social learning in technology adoption. One might wish to separately identify the importance of learning from a peer's purchase decision and the desire to adopt technologies used by those around you; our design could be applied to this setting as well.

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Appendix

The asset offered to clients in the experiment was a combination of an actively-managed, open-ended long/short mutual fund and a real estate note (LCI, *Letra de Créditto Imobiliário*), for a term of 1 year. A client was required to make a minimum investment of at least R$ 1,000 (approximately US$550) in each individual asset if he chose to purchase the combination asset (for a minimum total investment of approximately US$1,100). The long/short fund seeks to outperform the interbank deposit rate (CDI, *Certificado de Depósito Interbancário*) by allocating assets in fixed-income assets, equity securities and derivatives. The LCI is a low-risk asset which is attractive to personal investors, because it is exempt from personal income tax. The LCI offered in this particular combination had better terms than the real estate notes that were usually offered to clients of the brokerage with which we worked. First, the return of the LCI offered in the experiment was 98% of the CDI, while the best LCI offered to clients outside of the experiment had a return of 97% of the CDI. In addition, the brokerage firm usually required a minimum investment of R$ 10,000 to invest in an LCI, while the offer in the experiment reduced the minimum investment threshold to R$ 1,000.

Figures and tables

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

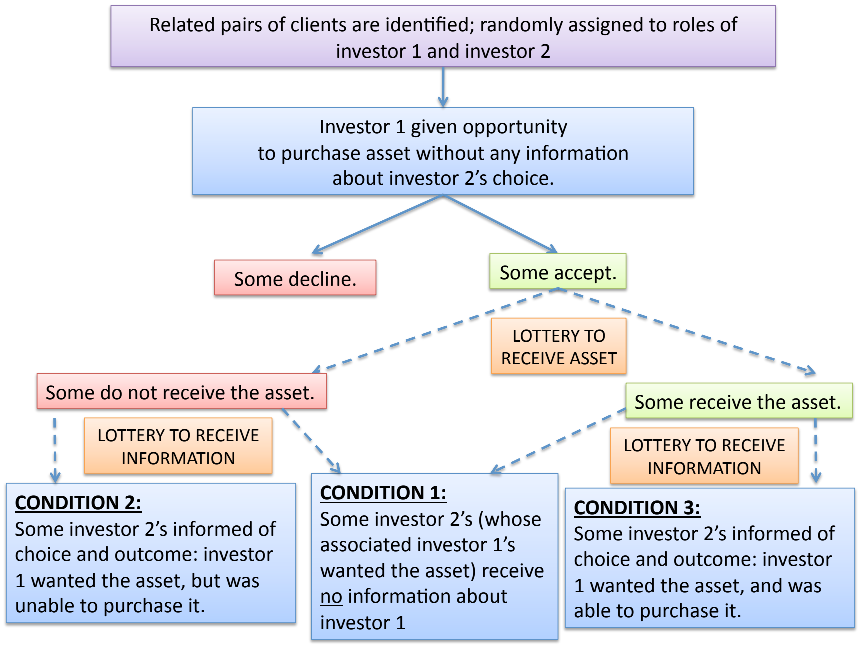
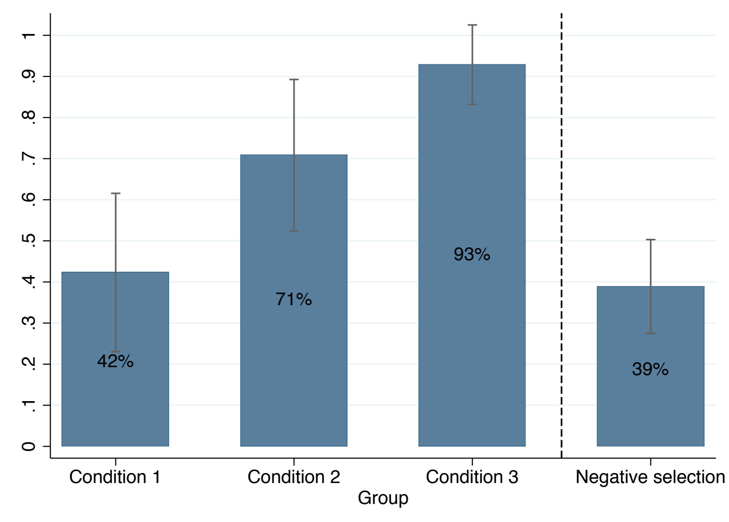


Figure 2: **Investor 2's take-up rates**



Note: This figure presents the mean (and 95% confidence intervals) of take-up rates for each group of investor 2's. Investors in conditions 1 to 3 have peers who wanted the asset. Those in condition 1 had no information about their peers. Those in condition 2 had information that their peers wanted to purchase the asset but had that choice rejected by the lottery. Those in condition 3 had information that their peers wanted and received the asset. Investors in the negative selection group have peers who did not want to purchase the asset (though they received no information about their peer).

Table 1: **Covariates balance**

	Investor 2 conditional on investor 1 wanted to purchase the asset				
	Control N=26	Learning only N=24	Learning + possession N=28	p-value of test (1)=(2)=(3)	N
	(1)	(2)	(3)	(4)	(5)
Age	37.92	34.50	36.75	0.59	78
	(2.16)	(2.55)	(2.98)		
Male	0.654	0.667	0.607	0.90	78
	(0.095)	(0.098)	(0.094)		
Married	0.346	0.208	0.357	0.41	78
	(0.095)	(0.085)	(0.092)		
Single	0.538	0.708	0.536	0.35	78
	(0.100)	(0.095)	(0.096)		
Earnings	4,931	4,950	6,769	0.45	66
	(1,042)	(801)	(1,297)		

Notes: the sample is conditioned on investor 2's whose peers wanted to purchase 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 the corresponding variable is the same for all treatment groups is presented in column 4. The sample size for the earnings variable is smaller due to missing values, and due to the exclusion of one outlier who had reported earnings of R$200,000 per month, while the second highest value was R$30,000. The sample size for the math related profession dummy is smaller as we considered as missing value professions that are indeterminate.

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**	0.287**	0.264**
(Condition (2) - Condition (1))	(0.136)	(0.140)	(0.139)	(0.129)
Learning and possession	0.505***	0.540***	0.551***	0.501***
(Condition (3) - Condition (1))	(0.110)	(0.122)	(0.121)	(0.111)
Negative selection	-0.034	0.011	0.006	0.041
	(0.114)	(0.124)	(0.123)	(0.119)
Investor 1				0.129
				(0.107)
Possession alone	0.220**	0.242**	0.265**	0.237**
(Condition (3) - Condition (2))	(0.106)	(0.109)	(0.118)	(0.104)
Mean (no information; peer chose the asset)	0.423			
(Condition (1))	(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	0.250	0.217

Note: 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 (3), a dummy for condition (2), and a dummy indicating whether the peer did not want to purchase the asset. Investor 2's in condition (1) 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 and the math related profession dummy as this would reduce our sample size. The results including these variables 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 with possession and the coefficient on Learning without possession. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 3: **Peer Effects, Social Learning, Social Utility, and Selection: Amount Invested**

Dependent variable	Amount invested			
	(1)	(2)	(3)	(4)
Learning alone	948.7***	861.3**	792.8*	729.7*
(Condition (2) - Condition (1))	(357.7)	(379.5)	(413.2)	(381.8)
Learning and possession	2,633.2***	2,556.5***	2,528.2***	2,443.5***
(Condition (3) - Condition (1))	(702.9)	(633.0)	(575.8)	(579.8)
Negative selection	-106.8	-3.0	-13.2	80.2
	(239.0)	(272.7)	(300.5)	(300.2)
Investor 1				473.1
				(294.4)
Possession alone	1,684.5**	1,695.1**	1,735.4**	1,713.7**
(Condition (3) - Condition (2))	(731.4)	(721.2)	(684.7)	(672.5)
Mean (no information; peer chose the asset)	884.6			
(Condition (1))	(210.0)			
Broker fixed effects	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
N	150	150	150	300
R^2	0.251	0.277	0.317	0.264

Note: column 1 presents the results of a regression of the amount invested in the asset on a dummy for condition (3), a dummy for condition (2), and a dummy indicating whether the peer did not want to purchase the asset. Investor 2's in condition (1) 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 and the math related profession dummy as this would reduce our sample size. The results including these variables 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 with possession and the coefficient on Learning without possession. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 4: **Peer Effects, Social Learning, Social Utility, and Selection: Invested More than Minimum**

Dependent variable	Invested more than minimum			
	(1)	(2)	(3)	(4)
Learning alone	0.212**	0.203**	0.205**	0.186*
(Condition (2) - Condition (1))	(0.097)	(0.093)	(0.101)	(0.098)
Learning and possession	0.497***	0.475***	0.492***	0.480***
(Condition (3) - Condition (1))	(0.103)	(0.102)	(0.102)	(0.099)
Negative selection	-0.038	-0.029	-0.030	-0.018
	(0.038)	(0.042)	(0.045)	(0.048)
Investor 1				0.095*
				(0.053)
Possession alone	0.286**	0.271**	0.288**	0.294**
(Condition (3) - Condition (2))	(0.131)	(0.130)	(0.133)	(0.129)
Mean (no information; peer chose the asset)	0.038			
(Condition (1))	(0.038)			
Broker fixed effects	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
N	150	150	150	300
R^2	0.338	0.366	0.399	0.295

Note: column 1 presents the results of a regression of a dummy variable equal to one if the investor invested more than the minimum amount on a dummy for condition (3), a dummy for condition (2), and a dummy indicating whether the peer did not want to purchase the asset. Investor 2's in condition (1) 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 and the math related profession dummy as this would reduce our sample size. The results including these variables 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 with possession and the coefficient on Learning without possession. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 5: **Heterogeneity of Social Learning Effects**

	Learning *by* technically skilled?			Learning *from* technically skilled?		
	Yes	No	p-value of test (1)=(2)	Yes	No	p-value of test (1)=(2)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: no controls						
Learning alone	-0.167	0.571**	0.027	0.250	0.125	0.742
(Condition (2) - Condition (1))	(0.221)	(0.228)		(0.216)	(0.309)	
Panel B: full specification						
Learning alone	-0.157	0.538***	0.003	0.250	0.084	0.585
(Condition (2) - Condition (1))	(0.221)	(0.141)		(0.174)	(0.288)	
Mean (no information; peer chose the asset)	0.500	0.375	0.589	0.500	0.444	0.821
(Condition (1))	(0.140)	(0.180)		(0.167)	(0.175)	

Notes: panel A reports comparisons of means; panel B reports coefficients using the full specification from column 4 in Table 2 adding the interaction of the condition 2 dummy with the technical occupation dummy (for investor 2 in columns 1-3 and investor 1 in columns 4-6). Columns 1 and 2 present the learning effects and the take-up rates for investor 2's in the control group (condition 1) respectively for investor 2's who have a technical occupation and for investor 2's who do not have a technical occupation. Investor 2's who are not possible to determine whether they have a technical occupation or not are excluded from this analysis. Column 3 reports the p-value of a test that the learning effect and the baseline take-up rates are the same for these two groups. Columns 4 to 6 are analogous, but examine heterogeneity associated with whether investor 2' s peer (i.e., investor 1) has a technical occupation. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 6: **List of occupations**

Occupation	Technically skilled?	Number of investors in the sample
Business administrator	Yes	34
Lawyer	No	15
Administrative agent	No	3
Systems analyst	Yes	4
Retired (exception: civil servants)	Undetermined	14
Architect	No	5
Social worker	No	1
Professional athlete or sports coach	No	1
Actuary or mathematician	Yes	1
Office assistant or similar occupation	No	2
Bank employee or clerk	Yes	8
Librarian, archivist, museologist or archeologist	No	1
Capitalist receiving income from invested capital	Yes	2
Communications specialist	No	1
Accountant	Yes	6
Coordinator or supervisor	Undetermined	1
Real estate, insurance or securities broker	Yes	5
Interior designer	No	1
Designer	No	1
Economist	Yes	5
Maid	No	1
Entrepreneur	Yes	11
Nurse or nutritionist	No	2
Engineer	Yes	39
Student	Undetermined	31
Pharmacist	No	2
Physical therapist or occupational therapist	No	3
Speech therapy	No	1
Manager	Undetermined	2
Journalist	No	5
Medical doctor	No	13
Member of the judiciary: federal supreme court justice	No	1
Military	Undetermined	6
Professional driver	No	1
Dentist	No	2
Other	Undetermined	29
Retired	Undetermined	1
Aircraft pilot	No	3
Professor	Undetermined	6
Teacher	Undetermined	7
Commercial establishment owner	Yes	2
Industrial establishment owner	Yes	1
Psychologist	No	1
Advertising agent	No	3
Public relations	No	1
Insurance professional	Yes	2
State public servant	Undetermined	2
Federal public servant	Undetermined	2
Electricity, electronics or telecommunications technician	Undetermined	1
Laboratory or X-ray technician	Undetermined	1
Chemical technician	Undetermined	1
Technical specialist	Undetermined	2
Retail or wholesale salesperson	No	4