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ABSTRACT

Using a field experiment conducted with a financial brokerage, we attempt to disentangle channels through which a person's financial decisions affect his peers'. When someone purchases an asset, his peers may also want to purchase it 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 possession of the asset. Then, we randomize whether the second member of the pair: 1) receives no information about his peer, or 2) is informed of his peer's desire to purchase the asset and the result of the randomization determining possession. We thus estimate the effects of: (a) learning plus possession, and (b) learning alone, relative to a control group. In the control group, 42% of individuals purchased the asset, increasing to 71% in the "social learning only" group, and to 93% in the "social learning and social utility" group. These results suggest that herding behavior in financial markets may result from social learning, and also from a desire to own the same assets as one's peers.

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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 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 consequent income or consumption).[3]

Identifying the causal effect of one's peers' behavior on one's own is notoriously difficult.[4] Correlations in the investment or consumption choices of socially-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. In the context of financial decisions, experimental work surmounting these challenges has been done by Duflo and Saez (2003) and Beshears et al. (2011), among others.

[1]In the economics literature, theoretical models of herding and social learning include Banerjee (1992) and Bikhchandani et al. (1992). Becker (1991) studies markets in which a consumer's demand for a product depends on the aggregate demand for the good. Empirical work has studied a wide range of contexts: the impact of classmates or friends on education, compensation, and other outcomes (Sacerdote, 2001; Duflo et al., 2011; Card and Giuliano, 2011; Shue, 2012); the impact of one's peers and community on social indicators (Bertrand et al., 2000; Kling et al., 2007; Bobonis and Finan, 2009; Dahl et al., 2012); and, the impact of coworkers on workplace performance (Guryan et al., 2009; Mas and Moretti, 2009). Durlauf (2004) surveys the literature on neighborhood effects. Within the social psychology literature, Asch (1951) studied individuals' conformity to group norms; Festinger (1954) posited that one resolves uncertainty by learning from others; Burnkrant and Cousineau (1975) distinguished between "informational" and "normative" reasons for conformity.

[2]See for instance Bikhchandani and Sharma (2000) and Chari and Kehoe (2004). Social learning has also been studied experimentally in a laboratory setting by Celen and Kariv (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]An early, thorough discussion of the challenge of identifying causal peer effects is found in Manski (1993). Unsurprisingly, the empirical evidence of peer effects in financial markets has typically been correlational (e.g., Hong et al. (2005), Ivkovic and Weisbenner (2007), and Li (2009)).

Equally difficult is identifying *why* one's consumption or investment choices have a social component.[5] 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*.[6]

(ii) One's utility from possessing an asset (or product) depends 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 (e.g., Banerjee (1992) and Bikhchandani et al. (1992)), information that a peer, investor j, purchased the asset will provide favorable information about the asset to investor i: investor j would only have purchased the asset if he observed a relatively good signal of the asset's return.[7] 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 without observing his peer's decision.[8]

In our study, we focus on social learning arising only 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.[9] The impact of learning from a peer's purchase decision is the social learning channel.

Typically, investor j's decision to purchase the asset will also imply that investor j possesses the asset. If investor j's possession of the asset directly affects investor i's utility of owning the

[5] Banerjee et al. (2011) study the diffusion of microfinance through social networks, and structurally estimate the importance of different potential channels linking peers' decisions. Cooper and Rege (2011) attempt to distinguish among peer effect channels in the lab.

[6] Identifying social learning alone is the focus of Cai et al. (2009) and Moretti (2011); they try to rule out the importance of peer effects through other channels (e.g., joint consumption) in the contexts they study.

[7] We assume that investor j here made a choice in isolation, i.e., without peer effects.

[8] Avery and Zemsky (1998) present a model in which information based herds do not occur, due to price adjustments; however, in our setting there is no asset price adjustment (see also Chari and Kehoe (2004)).

[9] The asset sold in our field experiment was designed to make this abstraction possible. This is discussed in detail in Section 2.1.

same asset, then observing investor j purchasing the asset (implying investor j's possession of the asset) can increase the likelihood that investor i purchases the asset for reasons other than social learning.

In this work, we pool together individuals' concerns about relative income or consumption ("keeping up with the Joneses"), greater utility from joint consumption of a good, etc.[10] 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.

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 (in Appendix A we present a formal model of peer effects in financial decisions that features both the social learning and social utility channels).

Disentangling the channels through which peer effects impact financial decisions is of more than academic interest. Identifying causal effects of the individual channels can provide important evidence on the sources of herding behavior in financial markets. For example, finding a causal effect of the social learning channel suggests that information provision can limit herding based on little actual information. On the other hand, if there is evidence for herding driven by factors other than social learning, improved information provision will not eliminate correlated choices among peers.

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 social utility. Working closely with us, a large financial brokerage in Brazil offered a new financial asset, designed exclusively for our experiment, to pairs of clients who share a social relationship

[10]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," which will include social preferences, as well as general equilibrium-induced differences in demand. Note also that in principle, social utility might lead to *negative* correlations between peers' choices (see Clark and Oswald (1998)), but we focus here on the case of positive social utility effects, as these are predominant in the literature on peer effects in financial decisions. 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.

(either friends or family members).[11] 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; furthermore, investors were not allowed to convert existing investments with the brokerage to purchase the asset and thus were required to allocate new resources.

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).[12] To disentangle the effect of investor 1's possession from the effect of the information conveyed by investor 1's revealed preference (his 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).[13] 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 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: (1) no information about investor 1's decision[14]; (2) information that investor 1 made a decision to purchase the asset, but was not able to consummate the purchase (so learning occurred *without possession*[15]); and, (3) information that investor

[11] The experimental design is discussed in more detail in Section 2. In particular, we describe the sample of investors, the characteristics of the financial asset, as well as the details of the experimental treatments.

[12] The assignment to the roles of investor 1 and investor 2 was random.

[13] Individuals understood that their decisions to purchase the asset might not be implemented. We do not believe that the investors made their decisions lightly: take-up rates in the brokerage's pilot sale of the asset (where no lottery was used) were similar to those in the control (no information about one's peer) condition in this study. Furthermore, purchase decisions that were confirmed by the lottery were actually implemented.

[14] We attempted to ensure that no information spread independently of the brokers' phone calls by requiring that calls be made to both investors on the same day. 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 our results.

[15] In Section 3.4, we discuss various other channels through which peer effects work that might be active in this condition.

1 made a decision to purchase the asset, and was able to consummate the purchase (so learning occurred, along with possession).[16] A comparison of choices made by investor 2 in conditions (1) and (2) reveals the effect of social learning; a comparison of (3) and (2) reveals the impact of investor 1's possession of the asset over and above the information conveyed by his purchase[17]; 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.[18]

Our experimental evidence suggests that *both* channels through which peer effects work are important. Among investor 2's whose peer chose to purchase the asset we find the following: individuals in the no information control condition chose to purchase the asset at a rate of around 42%; among those informed that investor 1 wanted the asset, but was unable to purchase it, the rate increased to 71%; finally, among those informed that investor 1 wanted the asset, and was able to purchase it, the rate increased to 93%.[19] 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. This is true not only for the purchase decision, but also for the decision of whether to invest more than the minimum investment amount, and how much to invest in the asset.

Our design allows us to examine another aspect of peer effects: the role played by selection into a socially-related pair in generating correlated choices among peers. In particular, when investor 1 chose *not* to purchase the asset, his associated investor 2 was assigned to a "negative selection" condition, in which no information was received about about the peer.[20] We refer to these investor

[16]Investor 2 also had his decision to purchase confirmed or rejected by lottery, so he was made aware of this randomization in all of the conditions. We have no reason to expect that investor 2 viewed investor 1's decision as anything other than a revealed preference.

[17]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.

[18]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 beyond learning.

[19]The take-up rate in the control group is relatively high by design: the firm offered an appealing asset that was not available outside of the experiment. We discuss the the asset in detail in Appendix B.

[20]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.

2's as the "negative selection" condition, because the information they receive is identical to that received by investor 2's in the control condition (1); however, the investor 2's in the "negative selection" condition are those whose peers specifically chose *not* to purchase the asset. Interestingly, we do not find large selection effects: the take-up rate in the "negative selection group was 39%, not very different from the take-up rate in condition (1).

If investors exhibit different levels of financial sophistication, a natural extension of our analysis of social learning is to explore heterogeneity in that dimension. One would expect that an investor with a high degree of financial sophistication is less likely to follow the revealed preference decisions of other investors, because he will rely more on his own assessment of the quality of an asset. On the other hand, an investor with a low degree of financial sophistication will be more likely to be influenced by the revealed preference decisions of other investors. Analogously, financially sophisticated investors may be more likely than unsophisticated investors to influence their peers through the social learning channel, given their greater knowledge of financial investments.

Using occupational categories to indicate financial sophistication[21] ("sophisticated" investors have occupations in engineering, finance, accounting, etc.), we find that there is *no* significant effect of social learning among sophisticated investor 2's, and a large, significant social learning effect among unsophisticated investor 2's. The difference in the treatment effects across these groups is statistically significant as well. We find weaker evidence suggesting that investor 2's exhibit larger social learning effects when their associated investor 1 is sophisticated than when he is unsophisticated. These results suggest that learning about the asset plays the predominant role in the social learning effects we observe, and that competing stories do not seem to be driving our findings in the social learning condition.[22]

A final important concern with our design, common to many experimental studies, regards the external validity of the findings. In the discussion of our empirical results, in Section 3.4, we present evidence suggesting that our results do capture relevant effects, albeit not representative of all peer effects in financial decisions. Importantly, we show: (i) the set of investors in our study

[21] Goetzmann and Kumar (2008), Calvet et al. (2009), and Abreu and Mendes (2010) find that investors' occupations are correlated with measures of their financial sophistication.

[22] We assess some alternative interpretations of our findings and some potential confounding factors in the discussion of our empirical results in Section 3.4.

who chose to purchase the asset are similar to those who do not want to make the purchase – this is important because our treatment effects of interest condition on investor 1's wanting to purchase the asset; (ii) the characteristics of our sample of investors – selected because they shared a social connection with another client – are roughly similar to those of the full set of clients of the main office of the brokerage with which we worked; (iii) sales calls similar to those used in the study are common, accounting for a large fraction of the brokerage's sales – this indicates the importance of communication from brokers to clients in financial decision making.[23]

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 the decision to purchase the asset from possession of the asset. To this end, 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. We now describe the design in detail; in particular, the structuring of a financial asset that possessed particular characteristics, and the implementation of multiple stages of randomization in the process of selling the asset.

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; and, the asset needed to be sufficiently

[23]While brokers generally do not provide information about specific clients' purchases, discussions with the brokerage indicate that brokers do discuss the behavior of other investors in making their sales calls.

desirable that *some* individuals would choose to purchase it, even in the absence of peer effects.[24] To satisfy these requirements, the brokerage created a desirable, 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 Appendix B). 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 – 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.[25]

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

[24] 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 (i.e., about investor 2).

[25] There was a maximum allowable investment of R$10,000 per client on the real estate note component of the investment; one investor invested the maximum amount.

script is available in Appendix D).[26] The sales calls made by brokers possessed several important characteristics. First, and most importantly, the calls were extremely natural: sales calls had frequently been made by the brokerage in the past; investments resulting from brokers' calls are thus in no way unusual.[27] We believe that no client suspected that the calls were being made as part of an experiment.[28]

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. Thus, clients would have trusted the broker's claims about their peer's choices, and would have believed that the lottery would be implemented as promised. 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).[29]

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 Table 1).[30] It is important to note that, although the investors in this sample are not a random sample of the brokerage's clients, we find that their observable characteristics are

[26]We created the script using Qualtrics, a web-based platform that brokers would access and use to structure their call; although brokers were not able to use the web-based script in all of their calls, two-thirds of the calls used the Qualtrics script (it was occasionally abandoned when the website was not accessible). The brokers were made very familiar with the script, however, and used Excel to generate the randomization needed to execute the experimental design when unable to use Qualtrics. The treatment effects are not significantly different if we restrict ourselves to the pairs whose calls followed the Qualtrics script (results available upon request). The brokers entered the results of the randomization and the purchase decisions in an Excel spreadsheet, which they then delivered to the authors.

[27]The brokerage communicated to us that approximately 70% of their sales were achieved through such sales calls.

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

[29]Thus, brokers would have used the available information in each experimental treatment as effectively as possible. Any treatment effects measured 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 salesperson), or may not be observed at all; the magnitudes of our effects should be interpreted with this in mind.

[30]In Table 1, we generally present means of the various investor characteristics. The exception is the earnings variable, the median of which is shown to mitigate the influence of outliers: while the median monthly income in our sample is R$4,500, one investor had monthly earnings of R$200,000. In addition to the brokerage's record of a pair's social relationship, investor 2 was directly asked about the nature of his relationship with investor 1, after investor 2 had decided whether to purchase the asset (investor 1 was not asked about investor 2 at all). Finally, note that one of the authors (Bursztyn) was present for some of the phone calls. In addition, for those and many of the other calls, we had a research assistant present; see the attached image in Appendix D. We discuss the implications of broker monitoring in Section 3.4.

roughly similar to the full set of clients of the brokerage's main office (see Table 1, columns 1 and 8).

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.[31] 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. If the second member of the peer pair had thought about his peer's potential offer and purchase of the same asset, our measured effects would be attenuated. However, data from the pilot calls made by the brokers prior to the experiment suggest that these potential peer effects in the control condition are not of great importance: the brokerage called clients who were *not* socially connected to other clients of the brokerage, and their purchase rate of the asset was very similar to that of our control condition. We thus believe 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.[32]

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."[33] 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.[34] If the investor chose to purchase the asset, he was asked to specify a purchase amount. 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; the investor was informed of

[31] Some clients were known to have links to more than one other client. We only called a single investor 1 and a single investor 2 from these "networks" containing more than one individual to ensure that investors did not learn about the asset outside of the brokers' sales calls.

[32] We also asked the brokerage if any client mentioned their peer in the sales call, and the brokerage indicated that this never occurred.

[33] A comparison of the characteristics of investor 1's and investor 2's can be seen in Appendix C, Table A.1, columns 1 and 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.

[34] This is not as unusual as it may appear at first glance: for example, Instefjord et al. (2007) describe the use of lotteries to allocate shares when IPO's are oversubscribed.

the details of the lottery before making his purchase decision.[35] 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 lottery, the purchase rate was 12 of 25, or 48% – very similar to what we observe among investors in our study receiving no information about their peers.

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.[36] 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.[37] 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.

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

[35] 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 C, Table A.1.The randomization resulted in a reasonable degree of balance across groups: 6 of 7 tests of equality of mean characteristics across groups have p-values above 0.10. One characteristic, gender, is significantly different across groups.

[36] This restriction also limited the ability of investor pairs to coordinate their behavior (for example, organizing side payments or transfers of the asset between friends). As noted above, 6 investor 2's had communicated with their associated investor 1's about the asset prior to the phone call from the brokerage, but dropping these 6 observations does not affect our results.

[37] Thus, to be clear, brokers called 162 investor 1's in order to attain our sample size of 150 pairs successfully reached.

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 (Figure 1 presents a graphical depiction of the randomization); investor characteristics across the three experimental conditions can be seen in Table 2. One-third were assigned to the "no information," control condition, condition (1). 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 to make the investment and were authorized to make it. Investor 2's in condition (1) were offered the asset just as was investor 1, with no mention of an offer made to their peer.[38]

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.[39] 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, as can be seen in Table 1, columns 3 and 4. This suggests that the selection of investor 1's who invest are not very different from the original pool of clients who were reached.[40]

Given the double randomization in our experimental design, investor 2's in conditions (1), (2), and (3) should have similar observable characteristics, and would differ only in the information

[38] 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.

[39] Investor 2's were not informed of investor 1's desired investment size, only whether investor 1 wished to purchase the asset.

[40] In addition, Table 1, columns 6 and 7, also suggests that investor 2's associated with investor 1's who chose to purchase the asset are similar to investor 2's associated with investor 1's who chose not to purchase the asset.

they received. As a check of the randomization, we present in Table 2 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 into each group, 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 Table 1, column 7). We assign these investor 2's to their own "negative selection" condition, 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 (1). We refer to these investor 2's as those in the "negative selection" condition, because the information they receive is identical to that received by 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.

2.4 Treatment Effects of Interest

Our experimental design allows us to make several interesting comparisons across groups of investors. First, we can estimate overall peer effects, working through both social learning and social utility channels. 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).

Second, we can disentangle the channels through which peers' purchases affect investment decisions. 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 resulting 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.[41]

In addition to identifying these peer effects, we will examine the role of "selection", by comparing investor 2's in condition (1) to those in the "negative selection" condition; we will also test for heterogeneous effects of social learning according to the financial sophistication (proxied by occupation) of investor 2's and investor 1's.

3 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%. These differences represent economically and statistically significant peer effects: the difference of around 50 percentage points in take-up rates between conditions (1) and (3) is large (and statistically significant at 1%), indicating the relevance of peer effects in financial decisions; moreover, we observe sizable and statistically significant effects of learning alone, and of possession above learning as well.[42] Interestingly, we do not see a big "selection" effect: there was a 42% take-up rate in condition (1) and 39% in the "negative selection" group (the difference is not statistically significant).

3.1 Regression Specification

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

[41]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 2, working against finding any statistically significant peer effects beyond social learning.

[42]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.

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

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 all of our regressions, the omitted category of investors to which the others are compared will be 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. In most of our analyses, we will focus on investor 2's, so $c \in \{\text{condition (2)}, \text{condition (3)}, \text{“negative selection”}\}$. In some cases, we will also include investor 1's in our analysis, and they will be assigned their own category c. Finally, in some specifications we will include control variables: X_i is a vector that includes broker fixed effects and investors' baseline characteristics.

3.2 Empirical Estimates of Social Utility and Social Learning

We first present the estimated treatment effects of interest using an indicator of the investor's purchase decision as the outcome variable, and various specifications, in Table 3. We begin by estimating a model using only investor 2's and not including any controls, in Table 3, column 1. These results match the raw data presented in Figure 2: treatment effects 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 3, the 22 percentage point difference between these conditions is statistically significant.[43]

Finally, the coefficient on the "negative selection" variable, in column 1 of Table 3, 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, with neither group receiving information about their peers. As suggested by Figure 2, the estimated selection effect is economically small, and it is not statistically significant.

We next present regression results including broker fixed effects (Table 3, column 2) and including both broker fixed effects and baseline covariates (Table 3, 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 (Table 3, column 4). The overall peer effect, as well as the individual social learning and social utility channels, estimated using these alternative regression models are very similar across specifications, providing evidence that our randomization across conditions was successful.

We also 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 4 and 5, we replicate the specifications presented in Table 3, but use the alternative outcome variables. As can be seen in Tables 4 and 5, the results using these alternative outcomes closely parallel those using take-up rates. We observe significant peer effects, significant social learning, and significant effects of possession beyond learning.[44]

The results across specifications in Table 3 through 5 indicate sizable peer effects in financial decision making. Moreover, they suggest that both channels through which peer effects work are important. 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. We discuss in more detail the external validity of the

[43] The possession effect could also capture a demand for joint insurance: closely-related peers might want to diversify the risk in their investments. Joint insurance would imply a reduction of the measured effects of possession above learning and thus attenuate our findings relating to that channel. Interestingly, we find that friends choose assets similar to their peers', rather than trying to diversify their investments.

[44] The only difference worth noting is that we observe marginally significant differences between investor 1's and investor 2's in condition (1) using these two outcomes.

magnitudes of our coefficients in Section 3.4.

3.3 Treatment Effect Heterogeneity

While we estimate sizable average treatment effects of social learning, as discussed above, the importance of the social learning channel is likely to depend on investors' financial sophistication. Financially sophisticated investors should put more weight on their own signal of the quality of the asset relative to information derived from their peers' revealed preferences. Therefore, the learning channel should be less important for more financially sophisticated investor 2's. 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 (see Appendix A for a formal treatment of this argument). Exploiting this source of heterogeneity is not only interesting from a theoretical standpoint – since it is a natural extension of a social learning framework – but it also helps rule out other alternative explanations driving our measured social learning effects (as we discuss in Section 3.4).

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.[45] Investors who have technical occupations (for example, professions related to engineering, finance, accounting, etc.) are likely to be more financially sophisticated than those who do not (see the list of occupations in our sample and their coding in Appendix C, Table A.2). Therefore, we examine the heterogeneity in the social learning effect according to whether investor 2 or investor 1 has a technical occupation.[46]

To identify the social learning effect for different groups of investors, we compare the take-up rates of investor 2's with differing degrees of financial sophistication in conditions (1) and (2).[47] In Figure 3.1, we present raw take-up rates in conditions (1) and (2) for investor 2's who have, and who do not have, technical occupations, respectively. One sees no significant effect of social

[45] Goetzmann and Kumar (2008), Calvet et al. (2009), and Abreu and Mendes (2010) find that investors' occupations are correlated with measures of their financial sophistication.

[46] 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.

[47] 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 among investor 2's with technical occupations; on the other hand, the impact of social learning is very large among those without technical occupations. In Figure Figure 3.2, we present raw take-up rates in conditions (1) and (2) for investor 2's whose associated investor 1's have, and who do not have, technical occupations, respectively. One sees moderate social learning effects among both groups of investor 2's. The effect appears larger for those learning from investor 1's with technical occupations, but the cell sizes are small, and confidence intervals are correspondingly large.

We present regression estimates in Table 6. Panel A presents comparisons of means for investors who have, and who do not have, technical occupations (columns 1-2); and, whose associated investor 1's have, and do not have, technical occupations (columns 4-5). Panel B, columns 1-2, reports effects estimated using the full specification of Table 3, column 4, but adding an interaction of the condition (2) dummy with a dummy indicating that the investor has a technical occupation, and also an interaction of the condition (2) dummy with with a dummy indicating that the investor does not have a technical occupation. Panel B, columns 3-4, reports an analogous specification, but including an interaction of the condition (2) dummy with a dummy indicating that the associated investor 1 has a technical occupation, and also an interaction of a condition (2) dummy with a dummy indicating that the associated investor 1 does not have a technical occupation.[48]

In columns 1 and 2 of Table 6, one sees that 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. While sophisticated and unsophisticated investor 2's have similar take-up rates in the control condition (1), we can reject at 5% or better that the social learning effect is the same for the two groups (see Table 6, column 3).[49] In Appendix C, Tables A.3 and A.4, we show that social learning's effects on investment amounts, and on an

[48]Note that we do not combine 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. One might expect that the greatest effect of social learning would occur when unsophisticated investors learn from sophisticated ones; however, our sample size prevents us from running this sort of test. Finally, it is important to note that sophisticated investors (both investor 1's and 2's) are somewhat more likely to have sophisticated peers, but this positive assortative matching is not statistically significant. Data are available from the authors upon request.

[49]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 do not. However, 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 little space for possession beyond learning to have a measurable effect. These results are available upon request.

indicator that the investment amount was greater than the minimum, are also significantly larger when investor 2 does not have a technical occupation.

In Table 6, columns 4-6, one sees that social learning from investor 1's with technical occupations is not significantly greater than social learning from investor 1's with non-technical occupations. The point estimate of the effect of social learning is larger when learning is from an investor 1 who had a technical occupation, but this difference should be interpreted cautiously.[50]

3.4 Discussion

Our results thus far present evidence strongly suggesting an important role played by social learning alone and by possession above learning in driving individuals' financial decisions. However, experiments in the field are typically imperfect, and ours is no exception. Thus, we now discuss, in turn, alternative hypotheses (or confounding factors) that might contaminate our experimental treatments; whether supply-side (broker) behavior played an important role in generating the observed treatment effects; and, the external validity of our findings.

3.4.1 Alternative Hypotheses and Confounding Factors

One important concern in interpreting our results is that factors other than learning from investor 1's revealed preference are present in the social learning condition (condition (2)). Investor 2's might have had different behavioral responses to their peer's loss of the lottery. For instance, they might have felt guilt purchasing an asset that their peer wanted but could not purchase; alternatively, they might have wanted the asset even more, to "get ahead of the Joneses." These two behavioral responses would have opposite implications in terms of changes in the magnitudes of the treatment effects observed in the social learning condition. In evaluating this concern, we are reassured by our results (in Table 6) showing heterogeneous effects of social learning depending on financial sophistication (proxied by occupation). They suggest that the alternative stories do not drive our findings, unless guilt or a desire to get "ahead of the Joneses" is systematically stronger

[50]In Appendix C, Tables A.3 and A.4, we show that social learning's effects on investment amounts, and on an indicator that the investment amount was greater than the minimum, are not clearly related to the occupation of the associated investor 1.

among individuals with more- or less-technical occupations.

Another potential concern within the social learning condition relates to side payments. Since investor 1's wanted, but could not get, the asset, some investor 2's may have felt tempted to make the investment and pass it along, or sell it, to their peer. Our design reduces the impact of this type of concern for several reasons. First, investor 1's who lost the lottery did not know that investor 2's would receive the offer, so investor 1's would likely not have initiated this strategy following their sales call. Second, even had they suspected that their friend would receive the offer, there was limited time between calls to investor 1's and investor 2's – indeed, only 6 out of 150 investor 2's reported that they heard about the asset from their associated investor 1. Finally, once investor 2 received the offer, he was unable to communicate with investor 1 prior to making his investment decision in order to facilitate coordination.

We can also address this concern to some extent with our experimental data. One might expect side payments, if present and important, to be most prominent among peers who are family members, as family members would have an easier time coordinating such payments than would mere friends or coworkers. This would tend to drive up the estimated social learning effect for pairs who are family members. In column 1 of Table 7, we consider the full specification from column 4 of Table 3, but also including interactions of each of our treatment (i.e., condition c) dummy variables with a dummy variable equal to one if investor 1 and investor 2 are family members. The results suggest that the treatment effects from social learning are not stronger among family members – the point estimate of the interaction is almost exactly zero.

One might also think that knowing that a peer desired to purchase an asset (even if he was unable to make the purchase) provides an indication of that peer's portfolio (or future asset purchases outside of the study). As a result, the social learning condition could potentially also contain some (anticipated or approximate) possession effect. This inference, however, is a sophisticated one, and we would expect financially sophisticated clients to be more likely to make it. However, as mentioned above, the effects of social learning are actually very *small* for clients with technical occupations, arguing against this hypothesis.

There are also some concerns that could affect both the "learning alone" and the "learning plus possession" conditions. When individuals observe their peers desiring to purchase an asset, they might update their priors about the existing demand for the asset and thus about future asset prices. However, in our experiment, the asset is only sold during the broker's call, and resale is not possible; thus, this concern does not seem to be relevant. Another potential issue relates to trust in the information provided by the brokers during the phone calls. However, we have no reason to think that clients would mistrust brokers with whom they have had an ongoing relationship (especially regarding easily verifiable claims).

3.4.2 Changes in Supply Side Behavior

Our study manipulates information received by agents on the demand side of a financial market. Of course, there is a supply side in this market as well, and one could be concerned that supply-side factors interact with our measured treatment effects. Brokers could exert differential effort toward selling the asset under different experimental conditions; they could try to strategically sort subjects across conditions, overturning our randomization; more generally, the experiment was not double blind, which might affect the implementation of the design.

Fortunately, we believe that the impact of these various concerns was likely small, for several reasons. First, as mentioned above, 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). Brokers would have used the available information in each experimental treatment as effectively as possible.

As a more direct check of supply-side effects, we examine the impact of broker experience on the treatment effects we estimate. Within the experiment, we view broker experience as a measure of broker knowledge of our study, and hence, the "double-blindness" of a phone call. We thus estimate the full specification of column 4 from Table 3, but including an interaction of the treatment dummies with a measure of broker experience: for each date of the study, we calculate the number of calls each broker had made before that date. The estimates of these interactions, presented in column 2 of Table 7, show that broker experience does not significantly affect the estimated treatment effects.

Brokers adjusting their effort across conditions or trying to overturn our randomization would also likely be correlated with broker experience within the study: both of these would be more profitably executed with some knowledge of investors' responses to the information provided in different treatments.[51] The lack of sizable effects from the interactions of broker experience with the treatment dummies suggests that these concerns do not drive our findings.

Finally, our research assistant randomly visited the brokerage on 6 out of the 12 dates on which sales calls were made, and monitored the brokers to check that they were following the script. In column 3 of Table 7, we present the results of estimating estimate the full specification of column 4 from Table 3, but including an interaction of the treatment dummies with a dummy variable indicating whether the research assistant was present at the brokerage. The results indicate that research assistant presence does not significantly affect our treatment effects; this provides further evidence that the experiment was implemented as designed, even when brokers were not closely monitored.

3.4.3 External Validity

A final important concern with our design regards the external validity of the findings. There are several reasons to question the generality of the treatment effects we estimate. First, our comparisons among investor 2's in conditions (1), (2), and (3) are conditional on investor 1 wishing to purchase the asset. If this was an unusual sample of investor 1's, perhaps the associated investor 2's were unusual as well, and thus reveal peer effects that cannot be viewed as representative even within our experimental 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 Table 1, columns 3 and 4). The investor 2's associated with investor 1's who chose to purchase the asset are also similar to those associated with investor 1's who chose not to purchase it (see Table 1, columns 6

[51] For example, if brokers knew that investors were more likely to purchase the asset in the "learning and possession" condition (3), they may have been willing to exert more costly effort to make a sale in that condition. If certain types of investor 2's were more responsive to the information provided in a particular experimental condition, brokers might have tried to shift them into the relevant condition.

and 7).[52] We also 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 (Table 3). This suggests that conditioning on investor 1's wanting to purchase the asset does not produce an unusual subsample from which we estimate treatment effects.

A second question is how different our sample of investors is from other clients of the brokerage – perhaps individuals who had referred (or had been referred by) other clients in the past (and who were thus selected into our study) are a highly atypical sample. In Table 1, column 8, we present characteristics of the full set of the brokerage's clients from the firm's main office.[53] Although the clients in our study's sample are not a random sample of the brokerage's clients, we find that their observable characteristics are roughly similar to the full set of clients of the main office.

One may question the representativeness of the form of communication studied in our experiment. Certainly, peers often communicate among themselves, rather than being informed about each other's activity by a broker trying to make a sale. While our study focuses on only one type of communication generating peer effects, we believe it is important: as noted above, approximately 70% of the brokerage's sales were derived from sales calls, so information conveyed through such calls is extremely relevant to financial decisions in this setting. Of course, in interpreting the magnitude of our effect, one might wish to consider the likelihood of information transfer in the real world; our design estimates the impact of information about one's peers conditional on receiving information – the endogenous acquisition of information is not studied here.[54]

Finally, it is important to note that the type of social learning we focus on is that of classic models, such as Banerjee (1992) and Bikhchandani et al. (1992): learning that occurs upon observation of the revealed decision of purchase by a peer. Of course, there are other types of social learning that might occur in finance, such as learning about the existence of an asset, or learning occurring after the purchase (when to sell the asset, when to buy more); these channels are shut

[52]This is a comparison of investor 2's in conditions (1), (2), and (3) to investor 2's in the "negative selection" condition.

[53]The majority of the individuals in our experimental sample were selected from this office's clients, though some were selected from other offices.

[54]Duflo and Saez (2003)) present evidence that information about investment opportunities does flow naturally to peers; our goal of disentangling separate channels of peers' influence required *control* over these information flows.

down in our study because of the design of the financial asset. However, these other forms of social learning are likely important as well.

4 Conclusion

Peer effects are an important, and often confounding, topic of study across the social sciences. In addition, 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. Perhaps the most important implication of these findings is support for models of social-learning based herding in financial markets, and also for herding based purely on a desire to own assets possessed by one's peers.

Our findings should be extended in several directions. Most fundamentally, it is important to determine their external validity. We are limited to studying a single asset; a single mechanism through which peers' choices were communicated; and, pairs of socially-related individuals. 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. Of course, the selection of information transmitted by brokers and by peers will be endogenous, and studying the process determining *which* information gets transmitted, and to whom, would be of great interest. Finally, studying information transmission through a broader network of socially-related individuals is important as well.

To the extent that our results do shed light on financial decision making beyond our experimental context, it is important to understand what they imply for asset pricing and for policies that attempt to limit financial market instability. For example, herds based on social learning could be mitigated if more, and better, information is made available to investors. 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. Our findings suggest that information provision could reduce market instability, but only to a certain point, because a significant component of the peer effects we find was generated by social utility.[55]

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 the importance of learning from a peer's purchase decision and the desire to adopt technologies used by people nearby.[56] 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).[57] In these settings and others, separately identifying the roles of social learning and social utility might be of interest to policymakers.

[55] Understanding *why* a peer's possession of an asset affects his decisions (beyond learning) is an important next step in clarifying the implications of our findings here.

[56] Foster and Rosenzweig (1995) and Conley and Udry (2010) identify the important role played by social learning in technology adoption. Social utility might exist in this setting because using a technology might be easier or less expensive when others nearby use it (network externalities); because one wishes not to fall behind those living nearby; etc.

[57] One can also learn about health care products from a peer's experience of the product – a type of learning we do not consider in this study. Kremer and Miguel (2007) study the transmission of knowledge about de-worming medication through social networks.

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Figures and Tables

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

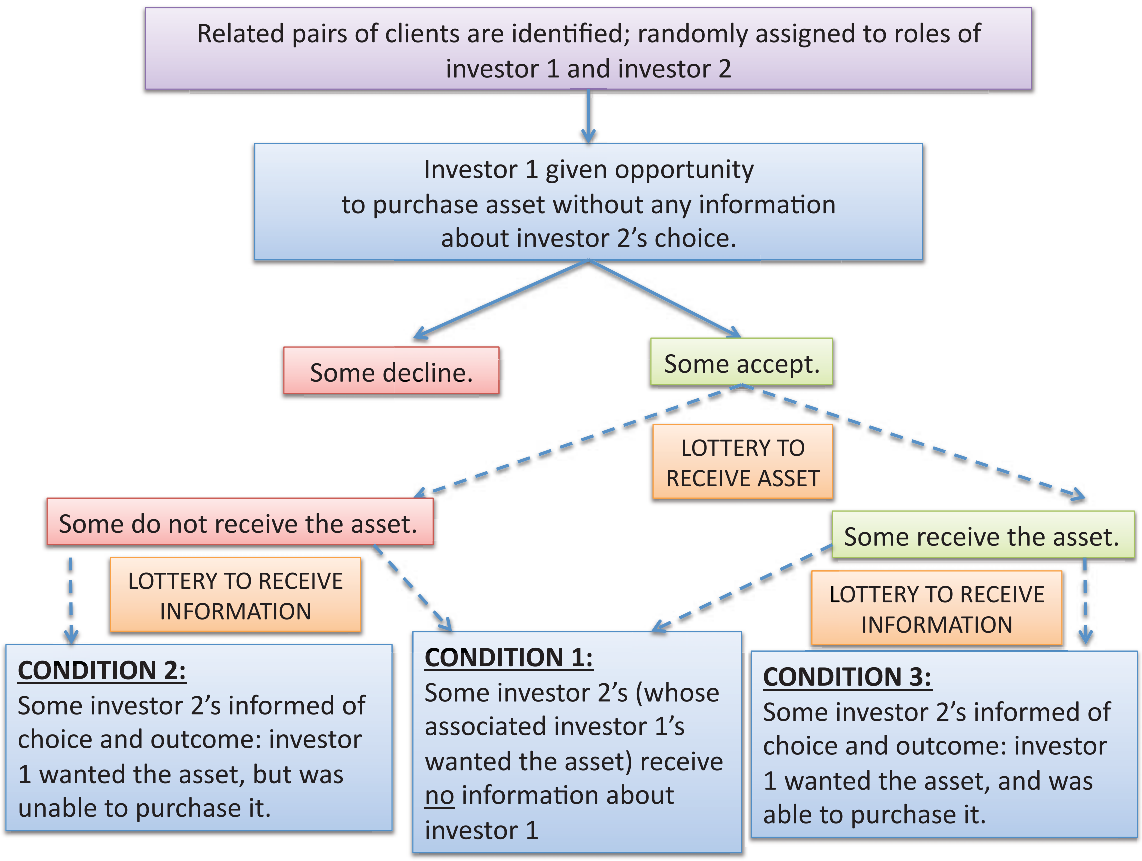
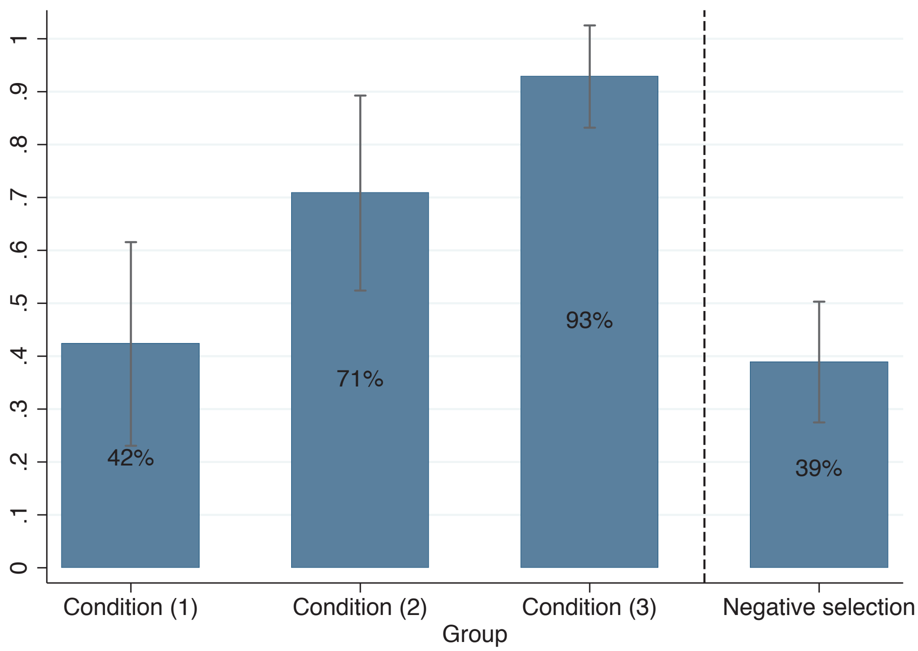


Figure 2: **Investor 2's take-up rates**



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. These investors were randomly allocated to one of these 3 groups. 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 (and received no information about their peer).

Figure 3: **Heterogeneity of Social Learning Effects**

Figure 3.1: Investor 2 has a technical occupation

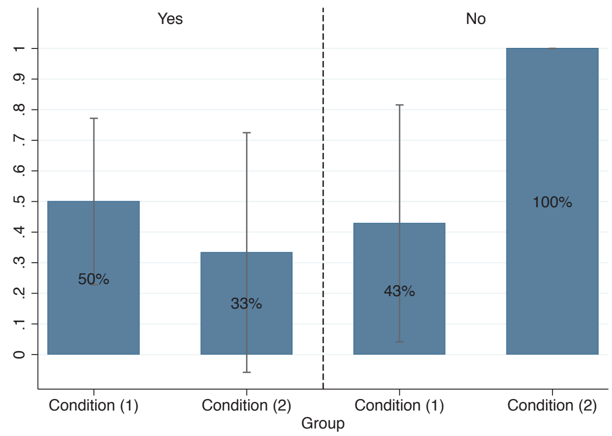
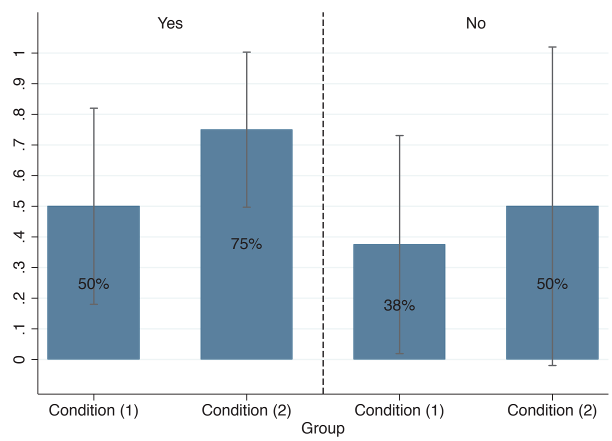


Figure 3.2: Associated investor 1 has a technical occupation



Note: Panel A presents figures with the mean (and 95% confidence intervals) of take-up rates for investor 2's in conditions (1) and (2), separately for those who have and who do not have a technical occupation. Panel B presents these figures separately for those whose associated investor 1's have and who do not have a technical occupation. Investors in conditions (1) and (2) 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.

Table 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)
Technical Occupation	0.62	0.64	0.69	0.58	0.59	0.55	0.64	0.59
	(0.03)	(0.05)	(0.06)	(0.07)	(0.05)	(0.07)	(0.07)	(0.01)
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 sample size for the technical occupation dummy is smaller because we considered as missing values professions that are indeterminate. The omitted value for "Relationship with associated investor" is "friends". This variable is not defined for investors outside the experiment's sample.

Table 2: **Covariates balance**

	Investor 2 conditional on investor 1 wanted to purchase the asset							
	Control	Learning only	Learning + possession	p-value of test:				N
				(1)=(2)=(3)	(1)=(2)	(1)=(3)	(2)=(3)	
	N=26	N=24	N=28					
	(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)					
Technical Occupation	0.67	0.40	0.53	0.29	0.12	0.44	0.48	51
	(0.11)	(0.13)	(0.13)					
Relationship with investor 1 (=1 if family)	0.46	0.67	0.46	0.24	0.15	0.98	0.14	78
	(0.10)	(0.10)	(0.10)					
Joint test				0.15	0.26	0.86	0.29	

Notes: The sample is conditioned on investor 2's whose associated investor 1's 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 of the corresponding variable is the same for all treatment groups is presented in column 4. The p-value of a joint test of equality for all variables is also presented. 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. The sample size for the technical occupation dummy is smaller because we considered as missing values professions that are indeterminate.

Table 3: **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 (2) - Condition (1))	(0.136)	(0.140)	(0.134)	(0.127)
Learning and possession	0.505***	0.540***	0.552***	0.500***
(Condition (3) - Condition (1))	(0.110)	(0.122)	(0.123)	(0.111)
Negative selection	-0.034	0.011	-0.005	0.042
	(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 (3) - Condition (2))	(0.106)	(0.109)	(0.124)	(0.108)
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	0.283	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 (3), a dummy for condition (2), and a dummy indicating whether the associated investor 1 did not want to purchase the asset ("Negative selection"). 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 2. We did not include earnings or the technical occupation dummy as this would reduce our sample size (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 and possession" and the coefficient on "Learning alone." * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4: **Peer Effects, Social Learning, Social Utility, and Selection: Amount Invested**

Dependent variable	Amount invested			
	(1)	(2)	(3)	(4)
Learning alone	948.7***	861.3**	825.3*	715.2*
(Condition (2) - Condition (1))	(357.7)	(379.5)	(421.2)	(394.5)
Learning and possession	2,633.2***	2,556.5***	2,564.8***	2,521.4***
(Condition (3) - Condition (1))	(702.9)	(633.0)	(613.4)	(611.9)
Negative selection	-106.8	-3.0	-6.4	123.9
	(239.0)	(272.7)	(305.3)	(308.6)
Investor 1				503.8*
				(300.1)
Possession alone	1,684.5**	1,695.1**	1,739.6**	1,806.1**
(Condition (3) - Condition (2))	(731.4)	(721.2)	(743.4)	(727.0)
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	0.316	0.264

Notes: 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 associated investor 1 did not want to purchase the asset ("Negative selection"). 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 2. We did not include earnings or the technical occupation dummy as this would reduce our sample size (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 and possession" and the coefficient on "Learning alone." * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: **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**	0.186*	0.173*
(Condition (2) - Condition (1))	(0.097)	(0.093)	(0.098)	(0.095)
Learning and possession	0.497***	0.475***	0.481***	0.485***
(Condition (3) - Condition (1))	(0.103)	(0.102)	(0.104)	(0.101)
Negative selection	-0.038	-0.029	-0.033	-0.016
	(0.038)	(0.042)	(0.046)	(0.049)
Investor 1				0.097*
				(0.053)
Possession alone	0.286**	0.271**	0.295**	0.311**
(Condition (3) - Condition (2))	(0.131)	(0.130)	(0.132)	(0.128)
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
R2	0.338	0.366	0.402	0.295

Notes: 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 associated investor 1 did not want to purchase the asset ("Negative selection"). 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 2. We did not include earnings or the technical occupation dummy as this would reduce our sample size (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 and possession" and the coefficient on "Learning alone." * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6: **Heterogeneity of Social Learning Effects**

	Investor 2 has a technical occupation			Associated investor 1 has a technical occupation		
	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	-0.167	0.571***	0.027	0.250	0.125	0.749
(Condition (2) - Condition (1))	(0.249)	(0.198)		(0.215)	(0.323)	
Panel B: full specification						
Learning alone	-0.153	0.543***	0.003	0.208	0.077	0.659
(Condition (2) - Condition (1))	(0.227)	(0.139)		(0.177)	(0.286)	
Mean (no information; peer chose the asset)	0.500	0.429	0.771	0.500	0.375	0.620
(Condition (1))	(0.140)	(0.197)		(0.168)	(0.182)	

Notes: Panel A reports comparisons of means for each group; panel B reports coefficients using the full specification of column 4 from Table 3 with the condition 2 dummy interacted with a dummy indicating that the relevant investor (either 1 or 2) has a technical occupation, and also interacted with a dummy indicating that the relevant investor does not have a technical occupation. We also include a technical occupation dummy. Columns 1 and 2 present the learning effects (investor 2's in condition (2)) and the take-up rates for investor 2's in the control group (condition (1)) for investor 2's who have a technical occupation and for investor 2's who do not have a technical occupation, respectively. 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. Investor 2's for whom it is not possible to determine whether they have a technical occupation or not are excluded from this analysis. Columns 4 to 6 are analogous, but examine heterogeneity associated with investor 2' s peer's (i.e., investor 1's) having a technical occupation. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 7: **Robustness Tests**

Interaction of the treatment effects with:	Relationship with investor 1 (=1 if family) (1)	Broker experience within the experiment (2)	RA was present during the call (3)
Learning alone	0.077	-0.001	0.140
	(0.305)	(0.008)	(0.268)
Learning and possession	0.417*	-0.003	0.248
	(0.232)	(0.008)	(0.234)
Possession alone	0.340	-0.001	0.108
	(0.220)	(0.007)	(0.203)

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 3, including interactions of the group dummies ($I_{c,i}$, where $c \in \{\text{condition (2)}, \text{condition (3)}, \text{“negative selection”}, \text{investor 1}\}$) 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 made before the day of the call. In column 3, we interact the treatment effects with a variable indicating whether our research assistant was present during the call. * significant at 10%; ** significant at 5%; *** significant at 1%.

APPENDIX
(NOT FOR PUBLICATION)

Appendix A: 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.[58] 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 the asset made by investor i by $b_i = \{0, 1\}$. Hence, for an investor making a purchase decision in isolation, we have

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

[58] 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.

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).[59] 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.[60] 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 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 $u(x) = u(x|p_j = 0)$

[59] 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.

[60] 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).

and $f(x|s_i)$).[61] 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. ∎

The difference between $\bar{s_2}$ and $\bar{s_3}$ is the result of a difference in investor j's possession of the asset.[62] 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

[61] 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.

[62] 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.

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. ■

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.[63]

[63]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 B: The Financial Asset

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. As noted in the text, the maximum investment in the LCI component was R$10,000.

Appendix C: Appendix Tables

Table A.1: **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)		
Technically skilled	0.64	0.59	0.47	195	0.73	0.66	0.55	55
	(0.05)	(0.05)			(0.09)	(0.09)		
Relationship with peer (=1 if family)	-	-	-	-	0.44	0.60	0.19	78
					(0.08)	(0.08)		
Joint test			0.15				0.23	

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. The p-value of a joint test of equality for all variables is also presented. 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. The sample size for the technical occupation dummy is smaller as we considered as missing value professions that are indeterminate.

Table A.2: **List of occupations**

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

Table A.3: **Heterogeneity of Social Learning Effects - Amount Invested**

	Investor 2 has a technical occupation			Associated investor 1 has a technical occupation		
	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	-404.8	1,920.6***	0.004	650.0	750.0	0.927
(Condition (2) - Condition (1))	(513.6)	(538.7)		(523.5)	(955.0)	
Panel B: full specification						
Learning alone	-807.4	1,797.9***	0.004	929.7	-635.2	0.326
(Condition (2) - Condition (1))	(764.1)	(480.3)		(660.7)	(1,314.4)	
Mean (no information; peer chose the asset)	1,071.4	857.1	0.673	1,100.0	750.0	0.515
(Condition (1))	(309.0)	(393.3)		(381.0)	(363.1)	

Notes: Panel A reports comparisons of means for each group; panel B reports coefficients using the full specification of column 4 from Table 3 with the condition 2 dummy interacted with a dummy indicating that the relevant investor (either 1 or 2) has a technical occupation, and also interacted with a dummy indicating that the relevant investor does not have a technical occupation. We also include a technical occupation dummy. Columns 1 and 2 present the learning effects (investor 2's in condition (2)) and the take-up rates for investor 2's in the control group (condition (1)) for investor 2's who have a technical occupation and for investor 2's who do not have a technical occupation, respectively. 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. Investor 2's for whom it is not possible to determine whether they have a technical occupation or not are excluded from this analysis. Columns 4 to 6 are analogous, but examine heterogeneity associated with investor 2' s peer's (i.e., investor 1's) having a technical occupation. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.4: **Heterogeneity of Social Learning Effects - Invested More than Minimum**

	Investor 2 has a technical occupation			Associated investor 1 has a technical occupation		
	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	-0.071	0.444**	0.011	0.067	0.250	0.512
(Condition (2) - Condition (1))	(0.073)	(0.176)		(0.153)	(0.230)	
Panel B: full specification						
Learning alone	-0.042	0.371**	0.022	0.040	0.097	0.842
(Condition (2) - Condition (1))	(0.088)	(0.158)		(0.139)	(0.258)	
Mean (no information; peer chose the asset)	0.071	0.000	0.336	0.100	0.000	0.335
(Condition (1))	(0.072)	(0.000)		(0.101)	(0.000)	

Notes: Panel A reports comparisons of means for each group; panel B reports coefficients using the full specification of column 4 from Table 3 with the condition 2 dummy interacted with a dummy indicating that the relevant investor (either 1 or 2) has a technical occupation, and also interacted with a dummy indicating that the relevant investor does not have a technical occupation. We also include a technical occupation dummy. Columns 1 and 2 present the learning effects (investor 2's in condition (2)) and the take-up rates for investor 2's in the control group (condition (1)) for investor 2's who have a technical occupation and for investor 2's who do not have a technical occupation, respectively. 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. Investor 2's for whom it is not possible to determine whether they have a technical occupation or not are excluded from this analysis. Columns 4 to 6 are analogous, but examine heterogeneity associated with investor 2' s peer's (i.e., investor 1's) having a technical occupation. * significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix D: Experimental Documentation

We enclose here English versions of the Qualtrics scripts used by the brokers in the phone calls, first to investor 1's and then to investor 2's. 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

Figure A.1: **Picture from the implementation**