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Sandro Ambuehl
B. Douglas Bernheim
Fulya Ersoy
Donhatai Harris

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ABSTRACT

Previous research shows that many people seek financial advice from non-experts, and that peer interactions influence financial decisions. We investigate whether such influences are beneficial, harmful, or simply haphazard. In our laboratory experiment, face-to-face communication with a randomly assigned peer significantly improves the quality of private decisions, measured by subjects' ability to choose as if they properly understand their opportunity sets. Subjects do not merely mimic those who know better, but also make better private decisions in novel tasks. People with low financial competence experience greater improvements when their partners also exhibit low financial competence. Hence, peer-to-peer communication transmits financial decision-making skills most effectively when peers are equally uninformed, rather than when an informed decision maker teaches an uninformed peer. Qualitative analysis of subjects' discussions supports this interpretation. The provision of effective financial education to one member of a pair influences the nature of communication but does not lead to additional improvements in the quality of the untreated partner's decisions, particularly in novel tasks.

Sandro Ambuehl
Department of Management UTSC
Rotman School of Management
University of Toronto
105 St. George Street
Toronto, ON, M5S 3E6
CANADA
sandro.ambuehl@utoronto.ca

B. Douglas Bernheim
Department of Economics
Stanford University
Stanford, CA 94305-6072
and NBER
bernheim@stanford.edu

Fulya Ersoy
Loyola Marymount University
Department of Economics
1 LMU Drive
University Hall 4216
Los Angeles, CA 94043
fulya.ersoy@lmu.edu

Donhatai Harris
Department of Economics
University of Oxford
Manor Road Building, Manor Road
Oxford OX13UQ
UNITED KINGDOM
donhatai.harris@economics.ox.ac.uk

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1 Introduction

When making financial decisions, people often seek advice from family and friends, rather than from experts (Bernheim, 1998; Lusardi, 2003, 2008; van Rooij, Lusardi and Alessie, 2011; Lusardi and Mitchell, 2014). A substantial literature shows that these social interactions *affect* personal financial choices,[1] but little is known about the extent to which they improve or degrade the quality of decision making (Hastings, Madrian and Skimmyhorn, 2013).[2] Related literature tentatively points in both directions. On the one hand, even professionally designed educational interventions can fail to improve decision making (Ambuehl, Bernheim and Lusardi, 2018). By comparison, peer-communication would appear to involve the blind leading the blind.[3] On the other hand, a sizable experimental literature finds that when groups make *collective* decisions, they often perform better than individuals (see Charness and Sutter (2012) and Kerr and Tindale (2004) for reviews). In principle, if members of a group internalize the group's decision-making principles, the benefits of social interaction could spill over into their private decisions.

In this paper, we study the effect of peer advice on the quality of financial decision making in an experiment with face-to-face interaction. The choices we study require an understanding of some simple financial principles, but they also implicate personal preferences; there are no right or wrong decisions. We adopt this approach for two reasons. First, most financial decisions depend on idiosyncratic preferences such as patience and risk aversion. Second, the problem of learning from peers becomes more challenging when preferences enter the mix. Merely mimicking a well-informed decision maker is a sensible strategy when everyone shares the same objectives, but can be highly suboptimal when they do not. To benefit from peer-to-peer communication, people must be able to either (i) separate principles from preferences and apply the principles based on their own preferences, or (ii) recognize and mimic those with better information and similar preferences. In contrast to our approach, previous studies that speak to the effects of peer communication on the quality of financial decision making employ tasks with dominant alternatives, which remove preferences from the mix, thereby simplifying the problem of social learning (Hvide and Östberg,

[1] Recent contributions include (Beshears, Choi, Laibson, Madrian and Milkman, 2015; Brown, Collins, Schmeiser and Urban, 2014; Bursztyn, Ederer, Ferman and Yuchtman, 2014; Cai, De Janvry and Sadoulet, 2015; Duflo and Saez, 2003; Hvide and Östberg, 2015; Hong, Kubik and Stein, 2004, 2005; Kast, Meier and Pomeranz, 2016; Ivković and Weisbenner, 2007; Lieber and Skimmyhorn, 2017; McCartney and Shah, 2017)

[2] See Mobius and Rosenblat (2014) for a general review of social learning in economics.

[3] Relatedly, Linnainmaa, Melzer and Previtero (2016) find that even professional financial advisors often make mistakes with their private investments, and successfully convince their clients to do likewise.

2015; Haliassos, Jansson and Karabulut, 2017). For many (but not all) purposes, this simplification is artificial.

We evaluate the quality of decision making using the notion of *financial competence* due to Ambuehl, Bernheim and Lusardi (2018). The method consists of comparing decisions subjects actually make to those they would have made if they properly understood their opportunity sets. The main advantage of this approach is that it allows us to study the quality of decision making in settings where the attractiveness of each alternative depends on preferences (so that dominance-based approaches do not apply), and where decision makers may suffer from consistent misunderstandings (so that approaches based on measures of WARP and GARP violations, e.g. Choi, Kariv, Müller and Silverman (2014), do not apply); see the general methodological discussion in Bernheim and Taubinsky (2018). The approach is non-paternalistic in the sense that it evaluates outcomes according to subjects' own preferences rather than some external judgment.

Our study involves a laboratory experiment. We employ this approach for two reasons. First, we can measure the quality of decision making more accurately in the laboratory than in the field. Studies of field data typically focus on decision problems with arguably dominant alternatives, which limits their applicability (see above), or on indirect measures of decision quality, such as directional changes in behavior and/or financial literacy. As Ambuehl, Bernheim and Lusardi (2018) demonstrate, those indirect measures can be misleading, and may point toward false conclusions about the quality of decision making. Second, inferring peer effects from observational data poses various econometric challenges (Manski, 1993). An experiment allows us to overcome these difficulties through exogenous assignment of treatments and peers.

Our subjects are undergraduate students at the University of Birmingham, UK.[4] University students comprise an important demographic group, the members of which are just beginning to make important personal financial decisions. However, may of them may be ill-equipped to do so: a mere 45% of our sample correctly answer three standard financial literacy questions; see Lusardi 2008. Students are also a target demographic group for many financial education interventions.[5]

[4] We are grateful to Michalis Drouvelis for allowing us to use the Birmingham Experimental Economics Laboratory.

[5] In the UK, financial literacy education for students aged 11-16 became part of the National Curriculum in September 2014, as part of citizenship requirements (House of Commons Library, 2016). Given their age and the time at which we completed the study, the subjects in our sample were not affected by this policy. The National Curriculum covers simple interest in Grades 7, 8, or 9, and compound interest in Grades 10 and 11 (Department for Education, 2014).

Our subjects make decisions concerning investments that accrue compound interest. Optimal choices depend on subjects' idiosyncratic time preferences, as well as their subjective assessments of factors such as experimenter reliability.[6] We compare subjects in a *Communication* treatment with others in a *Solitary* treatment. In both treatments, subjects start by making private decisions about investments. Those in the *Communication* condition then proceed to a face-to-face discussion about similar investments with a randomly assigned peer, while those in the *Solitary* control condition study the same investments on their own. Finally, subjects return to their terminals and make additional private decisions. A third treatment, *Indirect Education*, mirrors the *Communication* treatment, except that half of the subjects complete an education intervention about compound interest that demonstrably improves the quality of their decision making before they speak with peers. This treatment allows us to assess whether the benefits of an effective educational intervention propagate through through social contacts.

We find that peer-communication is, on average, beneficial. It substantially improves the quality of decision making evaluated according to subjects' own preferences, and it does so relative to solitary contemplation. Once we document this finding, we turn our attention to the mechanisms by which communication influences decision quality.

After communicating with a peer, subjects make private decisions involving both the interest-bearing assets they discussed, as well as assets they have not previously encountered. We find that peer-communication improves the quality of subjects' decisions in both cases, and to similar extents. Hence, peer-to-peer communication does not improve the quality of decision making merely because subjects can identify others who are better informed, without comprehending how the other person arrived at their decision. Instead, communication appears to provide our subjects with generalizable decision skills that are applicable beyond the specific problems they discuss.

Next we ask whether subjects learn more effectively from some types of peers than from others. Specifically, we classify subjects according to whether their initial decision quality is in the top or bottom half of our sample. We address two competing hypotheses. The first holds that the transmission of decision making skills simply involves information flowing from the informed to

[6] We remain agnostic, however, about the reasons why a subject may exhibit some specific discount factor. All that matters for our purposes is that subjects have some utility function they attempt to maximize, no matter its determinants.

the uninformed.[7] According to this hypothesis, the better the decision quality of the discussion partner, the larger the beneficial effect of peer communication on a subject's own decision quality. The second hypothesis holds that communication is more effective between people who appreciate each others' gaps in knowledge, reasons for confusion, and preferred pace. According to this hypothesis, the benefits from communication may be largest when peers are most similar, even when greater similarity requires the peer to be less financially competent.[8] Consistent with the second hypothesis, we find that people in the bottom half of the financial competence distribution experience greater improvements when interacting with others in the bottom half than when interacting with others in the top half. Hence, peer-to-peer communication transmits financial decision making skills most effectively when peers are equally uninformed, rather than when an informed decision maker teaches an uninformed peer. Qualitative analysis of subjects' discussions supports this interpretation. Similarly skilled partners engage in longer discussions without discussing more problems or engaging in more small talk. Instead, they appear to discuss each problem in greater depth.[9]

Finally, we study the extent to which peer-to-peer communication can help to augment the effects of beneficial financial education interventions that target limited numbers of consumers by propagating their effects through the population. Based on the preceding discussion, two countervailing mechanisms may be at work. On the one hand, treated consumers acquire new skills, which they may transmit to others. On the other hand, an effective treatment reduces the similarity between treated and untreated consumers, potentially stymying the transmission process. Consumers may also have greater difficulty communicating recently acquired conceptual knowledge. Overall, we find that communication with a treated peer is no more beneficial than communication with an untreated peer (even though the treatment improves the quality of peer's decisions). However, there is an important qualification. Communication with a treated peer is more beneficial for choices involving assets that the pair discussed, and less beneficial for choices involving novel assets.[10] An analysis of the content of conversations reveals that pairs with similar initial skills are

[7] Effects consistent with this mechanism have been documented in different choice contexts by Bursztyn, Ederer, Ferman and Yuchtman (2014) (financial choice) and by Jackson and Bruegmann (2009) (peer learning amongst elementary school teachers).

[8] Effects consistent with this mechanism have been documented by Hoxby and Weingarth (2005) (elementary school students), and by Booij, Leuven and Oosterbeek (2016) and Feld and Zölitz (2017) (university students).

[9] A possibly related finding by Bhattacharya, Hackethal, Kaesler, Loos and Meyer (2012) is that retail investors who receive unbiased financial advice from experts (who have better financial decision making skills than their clients) largely fail to follow it, perhaps because adviser and advisee are too different.

[10] The difference-in-differences is statistically significant, but simple differences are not.

significantly less likely to highlight their similarity when one member is treated. Instead, it becomes more likely that one member of the pair attempts to assert superior expertise. Additionally, while the treatment dramatically increases the frequency at which subjects discuss a heuristic rule that the treatment covers (the Rule of 72), this effect comes at the cost of crowding out discussions of the compound interest formula.

We contribute to three strands of literature. The first concerns peer effects in financial decision making (starting with Duflo and Saez (2003) and Hong, Kubik and Stein (2004)). Bursztyn, Ederer, Ferman and Yuchtman (2014) examine the nature of peer effects among Brazilian investors. They document two types of peer influences: first, investors learn from each other; second, they mimic others' asset holdings. Other papers delve into the effects of peers' characteristics. Ouimet and Tate (2017) study employee stock purchase plans for U.S. public firms, and find that employees who are poorly (highly) informed are most influenced by others who are poorly (highly) informed. Relatedly, Ko and Pirinsky (2017) find that sociability within a county promoted more conservative demand for housing and more stable real estate prices during the 2008 housing bubble, particularly when the number of financially sophisticated residents in an area was high. Haliassos, Jansson and Karabulut (2017) study both the nature of peer effects and the relevance of peers' characteristics, finding that proxies for the quality of financial decisions among refugees in Sweden increase more strongly when neighbors have economics or business education, but only for educated or male-headed households. They conclude that the underlying mechanism involves knowledge transfer rather than mere imitation. We contribute to this literature in several ways. First, we focus on peer effects involving the *quality* of financial decision making, which we measure in a precise and theoretically rigorous way. Second, we study peer effects in a controlled laboratory setting, thereby identifying causal influences and associated mechanisms with greater confidence. Third, we distinguish between a peer's baseline competence and recently acquired skills, showing that people have little ability to transmit the latter.

The second related strand of literature concerns financial education (see Lusardi and Mitchell (2014) and Hastings, Madrian and Skimmyhorn (2013) for reviews, and Miller, Reichelstein, Salas and Zia (2014) and Fernandes, Lynch Jr. and Netemeyer (2014) for meta analyses). Some have argued for targeting interventions at influencers and relying on social diffusion to leverage the effects of financial education (see, e.g., Haliassos, Jansson and Karabulut (2017) and Ouimet and Tate (2017)). Because the indirect beneficial effects of education in our experiment arise from mimicry

rather than from improved conceptual understanding, our experiment calls the effectiveness of many such diffusion strategies into question. Beneficial diffusion may be limited to the transmission of descriptive information; conceptual decision strategies are less likely to propagate through social networks.[11] As an anecdotal example, many consumers appear to have learned the mantra that small investors should achieve diversification through index funds without processing sensible principles for making tradeoffs between diversification and management fees (Hortaçsu and Syverson, 2004).

Third, we contribute to an experimental literature on peer effects in learning. Our paper is most closely related to Kimbrough, McGee and Shigeoka (2017), which studies the transmission of skills for solving Sudoku puzzles. It finds that peer-teching improves learning, but that ability-tracking has a detrimental effect. One important difference from the current study is that our financial decision-making tasks implicate preferences. Additionally, subjects are likely to have different familiarity with, and misconceptions about, financial decision making and Sudoko puzzles. Because of these differences peer communication could be less beneficial in the settings we study.[12] That said, potential instances of "the blind leading the blind" arise even with neutral tasks. For example, Boudreau and McCubbins (2010) find that providing subjects taking a mathematics test with polls of their peers' beliefs about the correct answers leads them to perform less well.

The remainder of this paper proceeds as follows. Section 2 explains the design of our experiment. Section 3 describes our data and performs preliminary analyses. Section 4 presents our main results. Finally, Section 5 explores policy implications and concludes.

2 Design

Measuring decision quality To assess the quality of decision making, we use the *financial competence* approach of Ambuehl, Bernheim and Lusardi (2018). Subjects make substantively equivalent decisions in two frames. In the *complex frame* of the present experiment, a future payment amount is described as an investment that accrues compound interest: "We will invest y

[11] While the data of Haliassos, Jansson and Karabulut (2017) point to a social multiplier of education, their identification is based on whether peers happen to be educated or not. Therefore, while the indirect effects of financial education they document could be caused by the education itself, they could also be a consequence of the different personalities and skills of individuals who chose to educate themselves.

[12] Another difference is that all treatments in Kimbrough, McGee and Shigeoka (2017) involve an education intervention.

tokens in an account with $r\%$ interest per day. Interest is compounded daily. We will pay you the proceeds in t days." We then elicit the present dollar amount the subject considers equivalent to that investment. The *simple frame* is similar, except that we directly reveal the future payment amount to the subjects: "You will receive x tokens in t days". A subject who fully understands the consequences of her decisions should make the same choice in substantively equivalent choice problems even if they are framed differently. A divergence in valuations indicates that the subject lacks the ability to choose her preferred outcome in at least one of the frames. The extent of this divergence provides a measure of the quality of decision making.

The absolute value of the difference in valuations across frames serves as our main outcome variable. Ambuehl, Bernheim and Lusardi (2018) shows that one can interpret this measure as the maximal welfare loss a subject incurs by having to make a choice in the complex frame rather than in the simple frame, as judged by the preferences she reveals in the simple frame.

A potential reservation concerning this welfare measure is that the preferences revealed in the simple frame may be tainted by other cognitive limitations and biases. For example, a subject who suffers from 'present bias' may exhibit excessive impatience in the simple frame, and overestimating compound interest might offset that bias. In principle, one could factor such ancillary biases into the welfare measure (*comprehensive welfare analysis*). In practice, this approach encounters two problems. First, it requires a comprehensive positive and normative understanding of all cognitive biases that may affect the decision in question. It admits neither the existence of unknown biases, nor the possibility of disagreements about the magnitudes or normative implications of other known biases. Second, broadening the scope of the analysis to include other known biases without simultaneously expanding it to include other measures that could address those biases can lead to problematic conclusions—for example, that misleading communication is beneficial if it exaggerates the benefits of compound interest. Arguably, it is better to promote communication that improves comprehension while simultaneously addressing present bias through more targeted measures, such as the provision of commitment opportunities or tax incentives.

Ambuehl, Bernheim and Lusardi (2018) advocate an alternative to comprehensive welfare analysis, which they call *idealized welfare analysis*. The essence of this approach is to conduct welfare analysis under the assumption that other biases will be (but have not yet been) addressed through other measures. For example, when focusing on comprehension of compound interest, one could image that present bias will be (but has not yet been) addressed by offering appropriate com-

mitment opportunities. Idealized welfare analysis provides a conceptually coherent framework for compartmentalizing biases in parallel with measures designed to address them, thereby permitting the analyst to focus on solving one problem at a time.

At first, it might appear that idealized welfare analysis also requires a deep understanding of all decision-making flaws and their solutions.[13] On the contrary, Ambuehl, Bernheim and Lusardi (2018) prove that their measure of financial competence (the absolute difference in valuations across the simple and complex frames) provides a first-order approximation of the idealized welfare effect, up to a multiplicative scalar. Thus, this welfare measure has the right sign, ranks policies in the correct order, and is strictly comparable across different financial decisions.[14]

Design overview Briefly, each subject proceeds through the experiment in five steps, consisting of three decision stages and two interventions. Subjects begin with decisions that reveal their valuations for compound-interest-accruing investments in both the simple and complex frames (*Stage 0*). They then take part in the first intervention. Some subjects receive education pertaining to the decision tasks; others view an unrelated video (a documentary about lions). Subjects then make additional decisions similar to those in the first stage (*Stage 1*). Next, they participate in a face-to-face discussions with a partner concerning compound-interest-accruing investments (with the exception of subjects in a control condition). Finally, they return to their terminals and make additional private decisions similar to those in the previous stages (*Stage 2*).

We pay subjects with Amazon gift cards based on one randomly selected decision, thereby providing them with incentives to choose according to their genuine preferences in every decision. We inform subjects of this compensation scheme at the outset.

Treatments Table 1 presents an overview of our design. We assign each subject to one of three treatments and one of two roles. The roles differ according to the order in which subjects encounter the decision problems.

[13] Formally, suppose the utility function U rationalizes observed choices, while the function V rationalizes idealized choices. Idealized welfare analysis evaluates outcomes according to V rather than U, even though V is not observed and impossible to infer absent a comprehensive understanding of pertinent biases.

[14] The intuition for this formal result is that the welfare loss caused by complex framing in the idealized setting will depend on the alignment between choices in the complex and simple setting. While the choices themselves may be skewed by other biases and cognitive limitations, the measured alignment is not. Similarly, a subject's reasons for using one discount factor rather than another are immaterial for our purposes. Discount factors may represent intertemporal rates of substitution in consumption, but they could also reflect ancillary concerns such as hassle costs and/or subjective perceptions of experimenter reliability (Cohen, Ericson, Laibson and White, 2016). Our welfare measure continues to apply (as an approximation) to settings from which those ancillary factors are removed.

Role	A (Receivers)			B (Senders)	
Treatment	Solitary	Communication	Indirect Educ.	Communication	Indirect Educ.
Stage 0	Choice Problems ($Simple$, $Test_0$)			Choice Problems ($Simple$, $Test_0$)	
Intervention 1	Documentary			Documentary	Education
Stage 1	Choice Problems ($Simple$, $Complex_1$, $Test_1$)			Choice Problems ($Simple$, $Discussed$, $Test_1$)	
Intervention 2	Contemplation	Communication	Communication	Communication	
Stage 2	Choice Problems ($Simple$, $Complex_2$, $Discussed$)			Choice Problems ($Simple$, $Complex_1$, $Complex_2$)	

Table 1: Experiment Structure.

In the *Communication* treatment, subjects converse face-to-face in pairs between Stages 1 and 2. Each pair consists of one subject in role A and one in role B (where the subjects are unaware of these roles). To facilitate discussions, we distribute six sheets to each pair, each describing a compound-interest-accruing investment.[15] Each subject in role B has made private decisions concerning the investments on the decision sheets (in Stage 1) before conversing with their partner. For subjects in role A, these tasks are novel. We recommend that subjects use 15 minutes for discussion, but they are free to end the discussion whenever they like, and can continue with the experiment once they are done. To help subjects break the ice, we ask them to note two questions they plan to ask their partner, and two pieces of advice they may want to give. We provide no explicit incentives for engaging in discussion, but we remind subjects they will complete 18 additional decision tasks in private, which may include the ones we ask them to discuss; consequently, there is a substantial chance that their payment will be determined by one of those decisions. All decision problems are numbered so that subjects can check whether they have seen a problem before. We unobtrusively record all communication, which subjects understand.[16] All subjects in the *Communication* treatment view the unrelated video between Stages 0 and 1.

The *Indirect Education* treatment parallels the *Communication* treatment, but adds financial education for subjects in role B prior to communication. Subjects complete the intervention between

[15] The following is an example. "Decision Task 10. We will invest 6 tokens at 2% for 72 days, compounded daily. You will get whatever is in the account after that time. How many tokens would we have to give you today, so you would be just as happy with receiving those tokens today as with receiving the proceeds in the account in 72 days?" The problem described on every sheet shares this same structure.

[16] See Appendix B.4 for the verbatim instructions.

the first two Stages of decision making. Subjects in role A watch the unrelated video. It is based on the section on compound interest from a popular investment guide, *The Elements of Investing: Easy Lessons for Every Investor*, by Malkiel and Ellis (2013). It focuses on the rule of 72, a simple method for approximating the time it takes for an investment to double. Subjects view videos of narrated slide presentations. The narration is verbatim from the text (with a few minor adjustments), while the slides summarize key points. We enhanced the effectiveness of that intervention by adding practice questions with personalized feedback.[17] See Appendix B.2 for details.

The *Solitary* treatment serves as an overall control—there is no education, and no communication prior to decision making. Just as in the other treatments, subjects converse in pairs about assigned decision problems (and expect to do so from the outset), but the communication takes place after Stage 2 rather than between Stages 1 and 2. Our purpose in preserving peer-to-peer communication in the *Solitary* treatment is to isolate effects on behavior arising from communication per se, rather than from the expectation of communication, which could lead subjects to think harder about the tasks, and thereby make higher-quality decisions.[18] We continue to hand out the six discussion sheets between Stages 1 and 2, and ask subjects to consider these assets in private. Subjects know that their Stage 2 decision tasks may reference these assets.

Decision tasks Each round of decision making concerns a future reward that is either framed as a compound-interest-accruing investment (the complex frame) or presented directly (the simple frame). We elicit valuations for these investments using once-iterated multiple price lists (Andersen, Harrison, Lau and Rutstrom, 2006). On each line of each list, the subject decides between receiving the future reward or V tokens immediately, where V ranges from 0 to 109 tokens. The first and second lists for each task have resolutions of 10 and 1 tokens, respectively.[19] Subjects cannot proceed if they switch back and forth between the future and the present reward as V increases, as such behavior is inconsistent with a well-defined valuation of the future reward. If they do behave inconsistently, an error message prompts them to revisit their decisions. Subjects complete all lists at their own pace.

[17] Ambuehl, Bernheim and Lusardi (2018) use a similar intervention. To maximize effectiveness, we tested several versions using a series of pilot experiments on Amazon Mechanical Turk.

[18] In a preliminary version of the *Solitary* treatment, subjects did not anticipate that they would communicate with anyone. The quality of financial decision making was generally lower, presumably because these subjects lacked the social motivation for good decision making that was present in the other treatments. Blanes i Vidal and Nossol (2011) and Kuhnen and Tymula (2012) document related attempts to save face.

[19] See Appendix B.3 for screenshots of the decision screens.

We employ three sets of complexly framed tasks labelled *Complex*$_1$, *Complex*$_2$, and *Discussed*. Each set contains six tasks. The investments in the *Discussed* set are those the subjects discuss with each other. The tasks in *Complex*$_1$ and *Complex*$_2$ are similar, but different subjects encounter them in different Stages. Table 2 shows the details of each task. Half of the investments in each set concern investments that pay out in 72 days; the other half concern investments that pay out in 48 days. We have chosen the parameters so that the principal amount doubles an integer number of times over the investment period. These properties make it easy for subjects to apply the rule of 72, a heuristic formula for approximating exponential growth covered in the educational intervention. All payments are denominated in tokens, which are worth £0.20 each. Subjects face each combination of time frame and interest rate three times, with varying principal amounts, in order to increase statistical power. In each decision, the investment compounds to approximately (± 2) 24, 58, or 88 tokens. These amounts are located near the top, middle, and bottom of the multiple decision lists, so that any tendency to choose switching points towards the middle of a list does not systematically influence our results.

Task set	*Complex*$_1$	*Complex*$_2$	*Discussed*
	Investment duration: 72 days		
Interest rate	3%	1%	2%
# of doublings	3	1	2
Principal	$\{6, 14, 22\}$	$\{12, 28, 44\}$	$\{3, 7, 11\}$
Future reward	$\{24, 56, 88\}$	$\{24, 56, 88\}$	$\{24, 56, 88\}$
	Investment duration: 48 days		
Interest rate	3%	4.5%	1.5%
# of doublings	2	3	1
Principal	$\{12, 28, 44\}$	$\{3, 7, 11\}$	$\{6, 14, 22\}$
Future reward	$\{24, 56, 88\}$	$\{24, 56, 88\}$	$\{24, 56, 88\}$

Table 2: Task parameters.

As is clear from Table 2, only the framing of investment opportunities differs across the three sets of complexly framed tasks. Moving from one set to the next, we change the framing by varying the principal amount and interest rate while holding the future reward and the length of the delay fixed. The eighteen complexly framed assets have a total six simply framed counterparts. Consequently, we present subjects with only one set of six simply framed tasks, which we label *Simple*. As shown in Table 1, subjects provide valuations for all six simply framed opportunities in all three Stages.

To avoid showing them exactly the same opportunity multiple times, we vary the future rewards in the simply framed tasks by two tokens or less.[20]

Two sets of ancillary tasks, labeled $Test_0$ and $Test_1$, consist of incentivized questions that test the subject's ability to compute compound interest, but do not implicate preferences. In these tasks, subjects evaluate compound interest investments that pay off in t days, and indicate the amount of money to be received in t days (rather than immediately) that they consider equally valuable.[21] We use these responses to construct statistical control variables.

The order in which subjects perform the various sets of decision tasks depends on their roles (see Table 1). Stage 0 is the same for all subjects. Its purpose is to establish baselines for financial knowledge and time preferences. Accordingly, the subjects perform the *Simple* and $Test_0$ tasks, which we intermingle.

The purpose of Stage 1 is to evaluate financial knowledge, time preferences, and the quality of decision making in complexly framed tasks after the first intervention. Tasks include $Test_1$, *Simple*, and a set of complexly framed tasks – $Complex_1$ for subjects in role A, and *Discussed* for subjects in role B. Comparisons between $Test_0$ and $Test_1$ allow us to assess the effects of the intervention on knowledge. Comparisons between the simply framed tasks from Stages 0 and 1 allow us to determine whether the intervention influences time preferences. Comparisons between the complexly and simply framed tasks in Stage 1 allow us to assess the quality of decision making after the first intervention. All Stage 1 decisions are intermingled in an individually randomized order. Substantively equivalent decision problems are never identified as such.

The purpose of Stage 2 is to evaluate time preferences and the quality of decision making in complexly framed tasks, separately for tasks the subjects have discussed as well as for novel tasks, after communication or contemplation. Tasks include *Simple* and either *Discussed* and $Complex_2$ for subjects in role A, or $Complex_1$ and $Complex_2$ for subjects in role B. Comparisons between the simply framed tasks from Stages 1 and 2 allow us to determine whether the second intervention influences time preferences. Comparisons between assessed financial competence (the discrepancies between simply and complexly framed choices) in Stages 1 and 2 allow us to determine the extent to which the second intervention affects decision making quality. For subjects in role A, we can

[20] Specifically, rewards with a 72-day delay are $(26, 59, 90), (25, 58, 92)$, and $(24, 57, 91)$ in Stages 0, 1, and 2, respectively. The corresponding rewards with a 48-day delay are $(24, 58, 89), (24, 57, 90)$, and $(25, 58, 91)$.

[21] The parameters of problems in set $Test_0$ in the format (duration, interest rate, principal, future reward) are given by $(18, 8, 22, 88), (36, 4, 6, 24)$, and $(54, 4, 7, 56)$. For set $Test_1$, they are $(18, 4, 12, 24), (36, 6, 7, 56)$, and $(54, 2.67, 22, 88)$.

separately evaluate the effects on the quality of decision making for tasks they have discussed (*Discussed*) and for tasks they have not previously seen ($Complex_2$). Once again, decisions are randomly intermingled.

Because subjects in role B make decisions about the *Discussed* tasks before communicating while subjects in the A role do not, we will refer to the B subjects as *Senders* and the A subjects as *Receivers*. Notice, however, that communication can flow in either direction, or not at all. Also notice that, in the *Solitary* treatment, all subjects are in role A.

Table 1 summarizes the timing of the decision problems for each role.

Implementation All instructions are displayed on screen and explained via an audio recording to minimize experimenter effects.[22] Subjects proceed at their own pace. They begin with a short video recording of one of the authors (Bernheim), vouching that we will pay subjects exactly the amount we promise them at precisely the time we promise them. There is also a comprehension check that subjects need to pass in order to continue.

Following that check, we measure subjects' comprehension of the mechanics of multiple decision lists. We present them with an initial list that asks them to decide, on each line, whether they prefer to receive x pence, or £1, for a range of values x. Since these are decisions between larger and smaller amounts of money to be received at the same point in time, any switching point other than 100 indicates deficient understanding. In addition, subjects see a completed list, and are required to indicate their payment in case the computer selects a given line for implementation. Subjects proceed regardless of their answers to these questions.[23]

Before participating in the main Stages of the experiment, subjects also complete a short battery of unincentivized psychological questions,[24] as well as three standard financial literacy questions

[22] See Appendix B.4. The videos of the instructions are accessible through these links: https://youtu.be/OHQvUZZKUzM (Preliminary), https://youtu.be/LCIAldy3SvM (Stage 0), https://youtu.be/OkbCO2iV76s (Stage 2-Communication and Indirect Education), and https://youtu.be/xSvxEG_R5WY (Stage 2-Solitary).

[23] These decisions are not incentivized.

[24] This battery consists of a 10-item version of the big-five personality scale (Rammstedt and John, 2007), the Mehrabian and Stefl (1995) conformity scale, as well as Frederick's 2005 three-item scale of cognitive style.

(Lusardi, 2008).[25] At the end of the experiment, they complete an unincentivized demographic survey. They also answer questions about their decision making processes and about their partners.

Because people typically have access to computational tools when making financial decisions, we provided each subject with a calculator that included a function for exponentials.[26]

3 Data Collection and Preliminary Analysis

Data Collection We ran sessions from Fall 2015 to Spring 2016 at the University of Birmingham, UK.[27] On average, sessions lasted 124 minutes, and subjects earned £26.55, including a £12.5 participation fee. We restrict our analysis to subjects who demonstrated comprehension of the experimental procedures (87.12%, or 460 of 528 subjects).[28] Focusing on subjects in the role of Receiver, we have 99, 89, and 75 subjects in the *Communication*, *Indirect Education*, and *Solitary* treatments, respectively.[29]

To study the content of subjects' discussions, we engaged workers on Amazon Mechanical Turk and assigned to of them to transcribe each audio recording. Research assistants at Stanford University reviewed these transcripts and coded qualitative information concerning each discussion, such as whether the subjects mentioned the rule of 72 or discussed market interest rates.[30] Most

[25] We elicit these at the beginning of the study to prevent answers from being influenced by subjects' communication partners. The financial literacy questions are 1) Suppose you had $100 in a savings account and the interest rate was 2% per year. After 5 years, how much do you think you would have in the account if you left the money to grow? *[More than $102, Exactly $102, Less than $102, Don't know]*, 2) Imagine that the interest rate on your savings account was 1% per year and inflation was 2% per year. After 1 year, with the money in this account, would you be able to buy *[More than today, Exactly the same as today, Less than today, Don't know]*, 3) Do you think the following statement is true or false? Buying a single company stock usually provides a safer return than a stock mutual fund. *[True, False, Don't know]*.

[26] Typically, people also have access to the internet. However, Lusardi and Mitchell (2011) find that only about 20% of a representative sample make use of these tools for real financial decisions, and the web-based experiment in Ambuehl, Bernheim and Lusardi (2018) finds that an equally small proportion of experimental subjects use these tools for their experimental decisions.

[27] See Appendix B.1 for details. We chose the University of Birmingham because the subject pool is large and diverse in terms of mathematical skills.

[28] 90.78% of Senders and 85.67% of Receivers passed the initial comprehension check. We retain subjects who understand the mechanics of the decision lists even if they are paired with subjects who do not, since the quality of communication does not depend on an understanding multiple decision lists. Despite of precautions, four subjects participated twice. These subjects may have had multiple accounts in the participant management system. We identify these subjects by their email addresses, which they must supply to receive payment via Amazon gift cards. For these subjects, we only retain the data from their first session.

[29] While 98 Senders in the *Indirect Education* treatment passed the comprehension checks, only 89 Receivers passed.

[30] Word count is recorded separately for each transcript; all other responses are coded based on both transcripts jointly.

recordings were of sufficient quality to allow transcription, yielding qualitative data on the nature of communication for 175 out of 188 pairs in the *Communication* and *Indirect Education* treatments.

Financial competence Throughout, we measure the quality of subjects' decision making by their *financial competence* (Ambuehl, Bernheim and Lusardi, 2018). Formally, we let $V_{j,d}^f$ denote individual j's valuation in decision problem d with framing $f \in \{simple, complex\}$.[31] To compare behavior across rewards of different sizes, we normalize valuations. If the future reward associated with decision problem d is given by r, we let $\delta_{j,d}^f = \frac{V_{j,d}^f}{r}$ denote subject j's *normalized valuation*. For each decision d, subject j's financial competence is then given by

$$c_{j,d} = -\left|\delta_{j,d}^{complex} - \delta_{j,d}^{simple}\right|.$$

To interpret the magnitude of $c_{j,d}$, consider the example of a subject who is willing to pay 80 cents for a complexly framed investment that he would value at \$1 if he properly understood his opportunity set. For that subject, $c_{j,d} = -0.2$.[32]

Our main dependent variable is the extent to which a subject's financial competence *changes* between Stages 1 and 2. By considering changes rather than levels, we difference out individual-level heterogeneity in financial competence and thus obtain more precise estimates. Specifically, we pair each complexly framed decision d in Stage 2 with the unique complexly framed decision d' in Stage 1 that has the same time frame and the same future value. Subject j's improvement on task d is then given by

$$Improvement_{j,d} = c_{j,d} - c_{j,d'}.$$

This procedure yields 12 observations per subject, one for each valuation pair in Stage 2.

Notice that we compare a subject's complexly framed choices in a given Stage of the experiment to her simply framed choices in the same Stage. In a subset of regressions, we instead compare a subject's complexly framed choices in any Stage to the simply framed choices she made in Stage 0. The substantive difference between these two alternatives is that the former gauges subjects' ability to choose in accordance with their goals at the time of choice, whereas the latter gauges

[31] Because we elicit valuations using multiple price lists, they are interval-coded. We use interval midpoints.

[32] The value $c_{j,d} = -0.2$ can also obtain for someone who is willing to pay \$1.20 for a complexly framed investment that he values at \$1 in the simple frame.

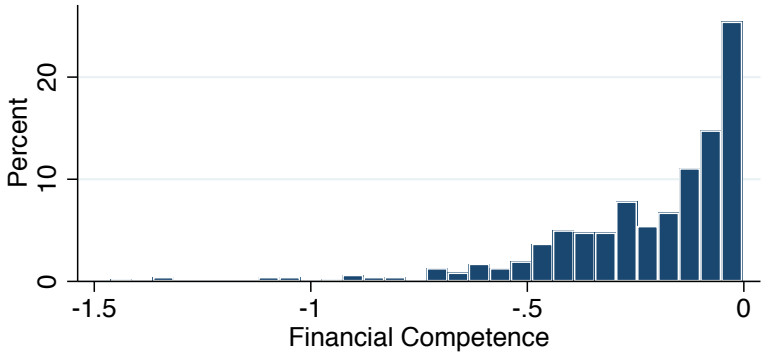


Figure 1: Distribution of financial competence. We display within-subject averages taken across all decisions that involve complex framing.

their ability to choose in accordance with their goals at the beginning of the experiment. Our main conclusions do not differ across these two measures.

Summary statistics and randomization check Figure 1 shows a histogram of financial competence averaged over all decisions, $\bar{c}$, for all subjects in our study who passed the comprehension check on multiple price lists. The distribution of financial competence is skewed, with a mean of -0.22 and a median of -0.14. For 10% of decisions, $\bar{c}$ is larger than -0.015, and the first and third quartiles are -0.33 and -0.05, respectively. The three financial literacy questions subjects answered at the beginning of the study (reproduced in footnote 25) provide a measure of initial financial knowledge. 45.04% answered all of those questions correctly.[33]

Our subjects' behavior is also similar to that documented in existing literature regarding both the extent of temporal discounting and the extent of exponential growth bias (a well-established tendency to underestimate the extent to which compound interest accumulates over time; see Wagenaar and Sagaria (1975), Eisenstein and Hoch (2007), Stango and Zinman (2009), Almenberg and Gerdes (2012), and Levy and Tasoff (2016)). Regarding temporal discounting, subjects value

[33]Similarly, 44.3% of U.S. college graduates answered all three questions correctly (Lusardi and Mitchell, 2014). In our sample, 97.7%, 87.2%, and 49.6% of subjects answered the first, second, and third questions correctly, respectively. We did assess subjects' initial knowledge of the compound interest formula. Assuming subjects discussed the formula if they knew it, we can assess the fraction who knew it based on the fraction of pairs who discussed it. 62.6% of pairs discussed the rule. Because we paired subjects randomly, we estimate the likelihood that any given subject was initially unfamiliar with the formula as $0.374^{1/2} = 61.2\%$.

£1 at £0.87 on average if it is paid with a delay of 48 days, and at £0.84 if it is paid with a delay of 72 days (with a population-level standard deviation of £0.27 in both cases).[34] These discount factors are comparable to those in the literature on discounting over similarly brief time frames (Frederick, Loewenstein and O'Donoghue, 2002).[35] Regarding exponential growth bias, we find that Receivers tend to undervalue compound interest investments by 5.03 percentage points on average (s.e. 1.90 percentage points, clustered by subject) in Stage 1 of the study.[36]

Summary statistics for 36 additional subject-level variables are listed in Appendix A.1. Comparing across our three treatments (*Communication*, *Indirect Education*, and *Solitary*), we reject four tests of joint equality at the 10% level, and an additional three at the 5% level, which is a bit higher than expected by chance. Significant differences appear for the demographic variables gender, age, and credit card ownership. Moreover, differences are present in some of the debriefing questions (e.g. whether subjects had previously talked about the study with others), which possibly reflects the fact that we ran the *Solitary* treatment after the *Communication* treatment.[37] To address these differences, we include statistical controls for subject characteristics in all regressions that involve the *Solitary* treatment. Specifically, we sort control variables into three categories. *Demographic variables* consist of gender, age, age-squared, ethnicity indicators, an indicator for whether English is the subject's first language, an indicator for whether the subject is an international student, and indicator variables for whether the subject lives in a rural, suburban, or urban area. *Financial variables* encompass log household per capita income, dummies for credit card ownership, having used a cash advance, having rolled over credit card debt, and a dummy indicating whether the subject correctly answered all of the three unincentivized financial literacy questions administered at the beginning of the survey. *Psychological and debriefing variables* consist of subjects' performance on the Cognitive Reflection Test (Frederick, 2005), the five dimensions of the big five personality scale (Rammstedt and John, 2007), subjects' conformity score (Mehrabian and Stefl, 1995), and

[34] These numbers include all subjects in all roles who passed the comprehension check about the multiple price list. In Stage 1, the respective numbers are 0.87 and 0.85, and in Stage 2 they are 0.89 and 0.86.

[35] They are, however, smaller than the discount factors elicited over longer time frames (Harrison, Lau and Williams, 2002; Andersen, Harrison, Lau and Rutström, 2014).

[36] Because valuations derived from the multiple price lists method can be affected by the upper and lower bounds of the list (Andersen, Harrison, Lau and Rutstrom, 2006), we perform this analysis separately depending on whether the future value of the investment is in the upper, middle, or lower third of the list. We find significant exponential growth bias for the first two cases but not for the third. The respective numbers are 13.18 (s.e. 1.60), 9.58 (s.e. 1.84), and -7.67 (s.e. 3.47). Moreover, for questions in sets $Test_0$ and $Test_1$, these numbers are 24.29 (s.e. 4.76), -15.62 (s.e. 2.07), and -18.41 (s.e. 1.47).

[37] We conducted the *Solitary* treatment at a later time than the treatments involving communication, as our initial hypotheses focussed on the comparison between the *Communication* treatment and the *Indirect Education* treatment alone.

dummies indicating whether subjects had heard about the study before participating, had talked to others about it, had prepared for it, and wished to be contacted about any follow-up study.

4 Main Analysis

Our analysis proceeds in four parts. We first study the effect of communication with a randomly assigned peer on the quality of financial decision making (Section 4.1). We then examine mechanisms. We investigate whether the beneficial effects of communication reflect genuine learning, or are merely a consequence of subjects' ability to identify high-competence peers and mimic their choices without comprehending the underlying concepts (Section 4.2). We also study how the effectiveness of communication depends on the relative financial sophistication of the subject and her partner (Section 4.3). Finally, we investigate whether the effects of beneficial financial education propagate through peer-to-peer communication (Section 4.4).

4.1 Mean Effect of Communication

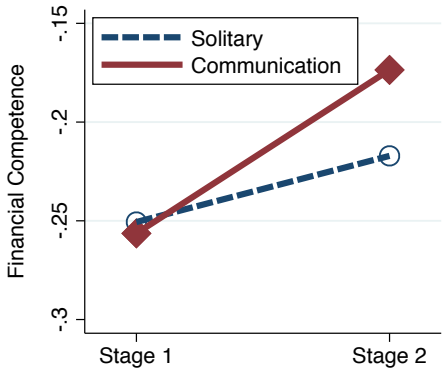
We begin by studying whether communication about financial decisions is helpful, harmful, or haphazard (a case of the blind leading the blind).

Panel A of Figure 2 displays the levels of financial competence across Stages 1 and 2 separately for the *Solitary* and *Communication* treatments. While competence increases slightly in the *Solitary* treatment, the improvement in the *Communication* treatment is substantially larger.[38] In our experiment, communication with a randomly selected peer has a clear beneficial effect on the quality of decision making.

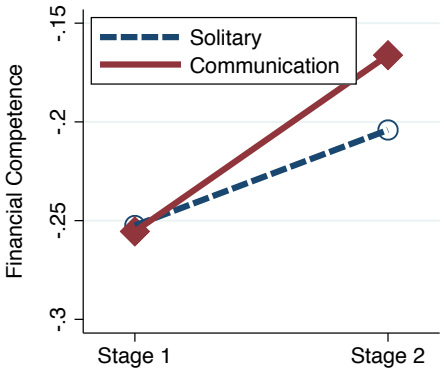
Formally, we regress $Improvement_{j,d}$ on an indicator for the *Communication* treatment, using the *Solitary* treatment as baseline. We focus on subjects in the role of Receiver and pool across discussed decisions (the *Discussed* set) and novel decisions (the $Complex_2$ set). We include a control for Receivers' preexisting level of financial competence (measured by their decisions in the $Test_0$ and $Test_1$ sets).[39] Panel A of Table 3 displays the result. Column 1 shows that the improvement in the *Solitary* treatment is a statistically insignificant 2.2 percentage points. The increase in competence

[38] The increase in the *Solitary* treatment does not necessarily reflect learning. An alternative explanation is the fact that Stage 2 employs a different set of decisions than Stage 1.

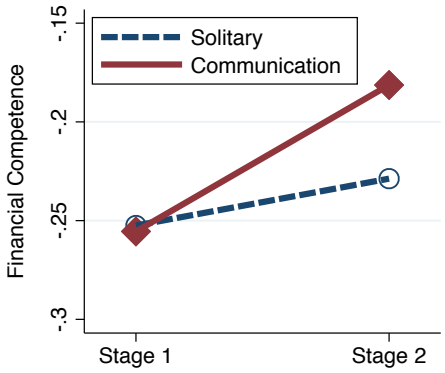
[39] The rationale for this control is that subjects who differ with respect to initial financial competence may improve to varying degrees over the course of the experiment, either due to learning or to regression to the mean. This factor could create apparent treatment effects as an artifact of a difference in starting points across the treatment groups.



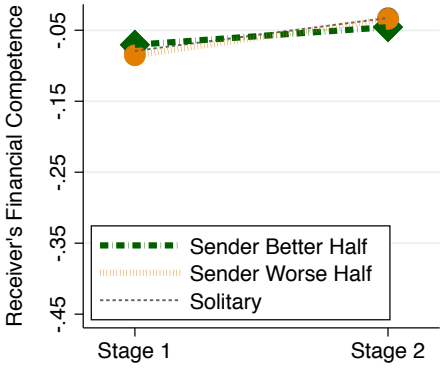
A. Overall effect of communication.



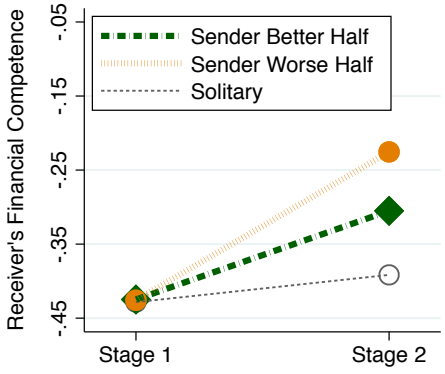
B. Effect on discussed problems.



C. Effect on novel problems.



D. Effect on Receivers in better half



E. Effect on Receivers in worse half

Figure 2: **Overview of results.** Figures depict estimated average financial competence in Stage 1 and Stage 2 for different groups of subjects and questions. All data are from subjects in the role of Receiver only. See Tables 3, 4, and 5 for standard errors of the slope coefficients and statistical tests for differences between them.

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Improvement in Receivers' financial competence before / after communication					
Benchmark choices in simple frame	Contemporaneous			Stage 0		
Improvement in Solitary	0.022	0.021	0.016	0.0001	0.0003	-0.004
	(0.018)	(0.018)	(0.018)	(0.020)	(0.019)	(0.019)
Panel A: *Communication*						
Improvement (compared to Solitary*)*	0.064**	0.066**	0.072***	0.069**	0.068**	0.072***
	(0.027)	(0.028)	(0.027)	(0.028)	(0.028)	(0.027)
Panel B: *Indirect Education*						
Improvement (compared to Solitary*)*	0.073**	0.073***	0.080***	0.067**	0.067**	0.076***
	(0.029)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)
p-value						
Communication = Indirect Education	0.748	0.788	0.747	0.955	0.972	0.871
Controls						
Initial skills	Yes	Yes	Yes	Yes	Yes	Yes
Demographic	-	Yes	Yes	-	Yes	Yes
Financial & Psychological	-	-	Yes	-	-	Yes
Observations	3,156	3,156	3,156	3,156	3,156	3,156
Subjects	263	263	263	263	263	263

Table 3: **Overall effect of communication**. Improvement in financial competence from Stage 1 to Stage 2. Estimates in the *Improvement in Solitary* row are the predicted levels of improvement of a Receiver in the *Solitary* condition, with subjects' initial skills and demographic, financial and psychological characteristics averaged over all subjects in all treatments. Initial skills are measured as the absolute deviation between a subjects' valuation and the true future value in decision sets $Test_0$ and $Test_1$. A full list of demographic, financial and psychological controls is given in Appendix Table A.1. Standard errors are clustered by subject. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$.

of Receivers in the *Communication* treatment is 6.4 percentage points larger than this baseline ($p < 0.05$). This size of this effect is particularly impressive when compared to the average level of financial competence among all Receivers in Stage 1 (26 percent, with a standard error of 1.6 percent). These findings remain substantively unchanged as we include additional control variables (columns 2 - 3).

Because financial competence is defined as the absolute difference in valuations across simple and complex frames, changes in competence could reflect behavioral changes in either frame. To demonstrate that peer communication increases competence primarily through its effect on behavior with complex framing, columns 4 - 6 replicate columns 1 - 3, using the simply framed choice made in Stage 0 as the normative benchmark rather those made contemporaneously with the complexly framed tasks. This alternative measure of financial competence is unaffected by changes in simply framed choices that may result from communication. The estimated treatment effects remain virtually unchanged. Hence, we conclude that communication improves measured financial competence primarily through its effect on choices in the complex frame.

4.2 Effect of communication on discussed and novel tasks

To understand the mechanisms underlying the beneficial effect of peer communication, we now study whether it is limited to the specific decisions the subjects discuss with their peers, or whether it extends to novel tasks. Panels B and C of Figure 2 separate panel A into these two types of tasks (decision sets *Discussed* and $Complex_2$, respectively).[40] While the improvement of subjects in the *Solitary* treatment appears slightly smaller for the novel tasks, there is no such difference for subjects in the *Communication* treatment. This finding suggests that communication improves decision making through conceptual learning, and not merely through choice mimicry.

Formally, we estimate these effects as follows. For each subject, we compute average $improvement_{j,d}$ across the six tasks in each of the two sets of decisions. We estimate a two-equation system using Seemingly Unrelated Regression. The critical explanatory variable is an indicator for whether the subject was in the *Solitary* or *Communication* treatment.

Panel A of Table 4 displays the results. Column 1a shows that a Receiver in the *Communication* treatment improves by an additional 6.6 percentage points for novel questions (the $Complex_2$ set) compared to the *Solitary* treatment. The corresponding number for discussed questions (the

[40] The Stage 1 decisions used are the same in both these panels.

VARIABLES	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)	(5a)	(5b)	(6a)	(6b)
	Improvement in Receivers' financial competence before / after communication											
Benchmark choices in simple frame	Contemporaneous						Stage 0					
Decisions discussed	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Task set	*Compl.*$_2$	*Disc.*	*Compl.*$_2$	*Disc.*	*Compl.*$_2$	*Disc.*	*Compl.*$_2$	*Disc.*	*Compl.*$_2$	*Disc.*	*Compl.*$_2$	*Disc.*
Improvement in Solitary	0.012	0.031	0.014	0.027	0.009	0.022	-0.010	0.010	-0.007	0.007	-0.0110	0.003
	(0.021)	(0.022)	(0.021)	(0.022)	(0.021)	(0.022)	(0.021)	(0.022)	(0.021)	(0.022)	(0.021)	(0.022)
Panel A: *Communication*												
Improvement (compared to Solitary)	0.066**	0.062**	0.065**	0.068**	0.071**	0.073**	0.071**	0.067**	0.067**	0.069**	0.072**	0.071**
	(0.028)	(0.029)	(0.028)	(0.029)	(0.029)	(0.030)	(0.028)	(0.030)	(0.028)	(0.030)	(0.029)	(0.030)
Panel B: *Indirect Education*												
Improvement (compared to Solitary*)*	0.054*	0.092***	0.047	0.099***	0.055*	0.106***	0.047	0.087***	0.041	0.093***	0.048	0.103***
	(0.029)	(0.030)	(0.029)	(0.030)	(0.029)	(0.030)	(0.029)	(0.030)	(0.029)	(0.031)	(0.029)	(0.031)
p-value												
Sender educated = Sender uneducated	0.648	0.290	0.519	0.263	0.541	0.241	0.376	0.459	0.341	0.396	0.393	0.269
Controls												
Initial skills	Yes		Yes		Yes		Yes		Yes		Yes	
Demographic	-		Yes		Yes		-		Yes		Yes	
Financial & Psychological	-		-		Yes		-		-		Yes	
p-values												
discussed = not-discussed if Sender uneducated	0.846		0.885		0.917		0.819		0.918		0.977	
discussed = not-discussed if Sender educated	0.043		0.006		0.005		0.033		0.006		0.003	
Difference in differences	0.016		0.005		0.004		0.010		0.004		0.001	
Subjects	263		263		263		263		263		263	
Observations	526		526		526		526		526		526	

Table 4: **Effect of communication on discussed and novel problems.** Estimation based on a SUR two-equation system. The means of the dependent variable for novel valuation problems are 0.02.1, 0.075, and 0.060 in the *Solitary*, *Communication*, and *Indirect Education* treatments, respectively. For discussed valuation problems they are 0.046, 0.090, and 0.115. A full list of demographic, financial and psychological controls is given in Appendix Table A.1. Initial skills are measured as the absolute deviation between a subjects' valuation and the true future value in decision sets $Test_0$ and $Test_1$. Statistical controls are constrained to have the same coefficients across the two equations. $^{*}p < 0.1, ^{**}p < 0.05, ^{***}p < 0.01$.

Discussed set) is 6.2 percentage points (column 1b). While both of these coefficient estimates are statistically significant at the 5% level, they are not statistically distinguishable from each other ($p = 0.84$).[41] The results remain qualitatively unchanged when we add statistical control variables (columns 2 and 3), and if we isolate the portion of these effects due to changes in behavior in the complex frame by using the simply framed choices from Stage 0 as the normative benchmark (columns 4 - 6).

4.3 Effectiveness of Communication by Subject Characteristics

The mechanisms underlying the effects of peer communication likely depend on the characteristics of the subjects in each pairing. We consider two hypotheses. On the one hand, communication may facilitate the transmission of financial knowledge and decision-making skills from those who have them to those who do not. Under this first hypothesis, a subject's improvement should be larger the more competent her peer. On the other hand, effective skill transmission may require an ability to understand each others' sources of confusion and to address each other's questions and concerns at the appropriate level, and at a comprehensible pace. It may also require people to feel comfortable asking questions without fear of embarrassing themselves (Edmondson, 1999). Under this second hypothesis, the benefits from communication may be largest when peers are most similar, even when greater similarity requires the peer to be less financially competent

To determine which of these hypotheses more accurately describes our data, we classify each subject according to whether her initial financial competence falls into the top or bottom half of the distribution. We perform this classification using the tasks in $Test_0$ and $Test_1$, rather than the tasks we use to define our primary outcome variables (i.e., the decisions in $Complex_1$, $Complex_2$ and *Discussed*).[42] Accordingly, our measured changes in financial competence are not confounded by regression to the mean. To increase statistical power, we pool across the *Communication* and *Indirect Education* treatments.

Panels D and E of Figure 2 display the results. Panel D shows that there is little room for improvement for Receivers who are initially among the more skilled half of decision makers.[43]

[41] Differences between the discussed and novel tasks are differenced out under the assumption that they have the same effects on behavior in the *Solitary* and *Communication* treatments.

[42] For Senders, we also use decision set *Discussed* for classification. Senders make these decisions before meeting their peer, and the inclusion of these data increases statistical power.

[43] If behavior is stochastic even within the simple framing, one cannot expect measured financial competence to equal zero even for a subject who perfectly understands compound interest, and applies this knowledge when making

Communication has little if any effect on the degree of improvement regardless of the peer's skills. In contrast, Receivers who are initially among the less skilled half of decision makers improve substantially when they communicate, as shown in panel E. Significantly, the improvement is *smaller* for low-skill Receivers when they are paired with high-skill Senders rather than low-skill Senders. Thus, peer-to-peer communication transmits financial decision making skills most effectively when peers are equally uninformed, rather than when an informed decision maker teaches an uninformed peer.

We formalize these comparisons by regressing the extent of improvements in financial competence on a dummy variable indicating whether the subject was assigned to the *Communication* treatment or the *Solitary* treatment, as well as dummy variables indicating whether the Receiver and (for subjects in the *Communication* treatment) the Sender fell within the top half or bottom half of decision makers according to initial financial competence.

Results appear in Column 1 of Table 5. Compared to a below-median subject in the *Solitary* treatment, a below-median Receiver who communicates with a below-median Sender improves the most; the additional improvement is 16.4 percentage points ($p < 0.01$). While such Receivers also benefit from communication with above-median Senders ($p < 0.1$), the improvement is larger in the former case (8.3 percentage points, $p < 0.1$). Columns 2 and 3 perform this analysis separately on the sets of novel and discussed decision problems, respectively. The results are similar to the ones in column 1. For below-median Receivers, both the improvement in the *Solitary* condition, as well as the additional improvement due to communication, are slightly smaller for novel than for discussed tasks. No such differences are apparent for above-median receivers.

To evaluate the extent to which our results are due to changes in complexly framed choices as opposed to simply framed choices, columns 4-6 replicate columns 1-3, using the simply framed choices from Stage 0 as the normative benchmark. This modification strengthens our conclusions: the incremental improvement when a below-median Receiver communicates with a below-median Sender rather than an above-median Sender is now statistically significant at the 1% level.

decisions. To estimate the deviation one would expect due to stochasticity in behavior from a subject who is perfectly informed of the implications of a complexly framed decision, we estimate the extent of stochasticity in choice in the simply framed choices alone. We pair simply framed decisions with approximately the same future value (16 with 19, 17 with 20, and 28 with 21). For each subject, we calculate the absolute distance in normalized valuations between members of a pair, and average across the three pairs. The sophisticated half of decision makers exhibit a mean absolute deviation of 0.058 (s.e. 0.094) whereas the corresponding number is 0.105 (s.e. 0.162) for the less sophisticated half.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Improvement in Receiver's financial competence					
Benchmark choices in simple frame	Contemporaneous			Stage 0		
Sets of decision problems						
Discussed	Yes		Yes	Yes		Yes
Complex$_2$	Yes	Yes		Yes	Yes	
Improvement in Solitary condition	0.031*	0.023	0.040**	0.010	0.000	0.020
for bottom half Receiver	(0.016)	(0.016)	(0.017)	(0.017)	(0.017)	(0.018)
Additional improvement from communication if						
Receiver bottom half						
and Sender bottom half	0.164***	0.148***	0.179***	0.183***	0.158***	0.208***
	(0.045)	(0.049)	(0.046)	(0.045)	(0.049)	(0.046)
and Sender top half	0.083*	0.054	0.112**	0.053	0.029	0.078
	(0.044)	(0.044)	(0.049)	(0.044)	(0.044)	(0.050)
Receiver top half						
and Sender bottom half	0.005	0.008	0.002	0.005	0.009	0.002
	(0.019)	(0.021)	(0.020)	(0.017)	(0.018)	(0.019)
and Sender top half	-0.022	-0.022	-0.021	-0.013	-0.006	-0.019
	(0.020)	(0.022)	(0.021)	(0.018)	(0.020)	(0.018)
Control variables						
Initial skills	Yes	Yes	Yes	Yes	Yes	Yes
Receiver Top Half	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	Yes	Yes	Yes	Yes	Yes	Yes
p-values about effect on Receiver						
(R bottom, S bottom) = (R bottom, S top)	0.092	0.065	0.181	0.006	0.010	0.008
(R bottom, S bottom) = (R top, S bottom)	0.001	0.008	0.000	0.000	0.004	0.000
(R bottom, S top) = (R top, S top)	0.029	0.121	0.012	0.160	0.486	0.057
(R top, S bottom) = (R top, S top)	0.191	0.157	0.294	0.321	0.469	0.253
Joint insignificance	0.004	0.022	0.002	0.001	0.019	0.000
Subjects	263	263	263	263	263	263
Observations	3,156	1,578	1,578	3,156	1,578	1,578

Table 5: **Effect of communication by pair characteristics.** We pool Receivers across the *Communication* and *Indirect Education* treatments. This table includes statistical controls for demographics and initial skill levels. Initial skills are measured as the absolute deviation between a subjects' valuation and the true future value in decision sets $Test_0$ and $Test_1$. A full list of demographic controls is given in Appendix Table A.1. We reproduce this table with the addition of financial and psychological controls in Appendix A.3. Doing so does not change the qualitative conclusions or statistical significance. Decision problem fixed effects are included. Standard errors are clustered by subject. $^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$.

An analysis of the contents of subjects' conversations reinforces these inferences.[44] We estimate additional regressions to determine how the content of communication depends on whether the Sender and Receiver are in the same or in different halves of the skill distribution.[45] Panel A of Table 6 displays the results. Column 1 shows that members of pairs we classify as similar are much more likely to highlight similarities than members of pairs we classify as dissimilar (73.6% vs. 42.2%), through statements such as "I'm bad at this too, so let's see whether we can help each other out." Moreover, while heterogenous pairs discuss tasks for 8.26 minutes, similar pairs discuss tasks for 10.15 minutes, nearly 25 percent more (column 2, $p < 0.1$ for differences across pairs). This difference relates to the quality and focus of the conversation: while the number of tasks discussed does not differ according to the pairs' characteristics (column 3), the number of small-talk topics discussed is significantly smaller (column 4).[46]

4.4 Indirect Effects of Financial Education

The beneficial effects of peer communication raise the possibility that social networks may propagate the influence of financial education through the population, magnifying its benefits. Accordingly, in this subsection we examine the indirect effects of financial education on those who have not participated themselves. The preceding results suggest two competing hypotheses. On the one hand, treated consumers acquire new skills, which they may transmit to others. On the other hand, an effective educational treatment reduces the similarity between treated and untreated consumers, potentially stymying the transmission process. Individuals may also have greater difficulty communicating recently acquired conceptual knowledge.

Before investigating the indirect effects of education, we demonstrate that our intervention has a direct beneficial effect on those who participate. We regress Senders' financial competence in Stage

[44] A sample conversation between two of our subjects illustrates the importance of responding to the partner at the right level and pace: *A: But you already have one whole pie. I hope I'm making it clear. So you've got a whole pie, right? This is day zero. You've got to have a pie, but after day one, you gain a slice of that pie, so you have more slices. And on day two, you get even more slices of pie. B: Okay, that seems to make sense. A: Yeah, so that's why you have to add one to it, because you already have this pie. This is one, but this is 0.02. B: On top of that. A: Yeah. The pie is good example? B: Yeah, that was much easier.* Later in the conversation: *B: You've just taught me more maths then I've ever learned, ever.*

[45] Each of our audio recordings uniquely matches a pair of subjects in our data, but we cannot reliably identify whether a pair member is in the role of Sender or Receiver. Hence, all our analysis of discussion content is at the pair level.

[46] Transcribers indicated which of three pre-specified small-talk topics subjects discussed: place of origin, field of degree, and years of study.

VARIABLES	(1) highlight similarities	(2) minutes discussed	(3) # problems discussed	(4) #small talk topics	(5) one person proclaims skills	(6) Rule of 72 discussed	(7) Compounding formula discussed
Panel A: *Communication*							
Different skills	0.341	8.264	4.136	0.659	0.229	0.000	0.667
	(0.132)	(0.487)	(0.196)	(0.119)	(0.037)	(0.000)	(0.135)
Similar skills	0.622	10.154	4.073	0.400	0.137	0.039	0.588
	(0.181)	(0.595)	(0.341)	(0.143)	(0.061)	(0.016)	(0.104)
Panel B: *Indirect Education*							
Different skills	0.386	8.516	4.023	0.432	0.413	0.739	0.391
	(0.105)	(0.664)	(0.797)	(0.176)	(0.13)	(0.092)	(0.061)
Similar skills	0.357	9.499	4.476	0.214	0.349	0.767	0.442
	(0.063)	(0.469)	(0.443)	(0.070)	(0.072)	(0.051)	(0.139)
p-Values							
Effect of similarity							
Communication tr.	0.001	0.091	0.873	0.088	0.284	0.530	0.509
Indirect Education tr.	0.997	0.403	0.380	0.150	0.412	0.886	0.729
Effect of indirect education							
Similar skills	0.009	0.568	0.437	0.239	0.035	0.000	0.038
Dissimilar skills	0.550	0.826	0.800	0.153	0.059	0.000	0.002
All four parameters equal	0.006	0.301	0.817	0.044	0.020	0.000	0.004
Diff-in-diff	0.023	0.576	0.463	0.863	0.868	0.735	0.476
Observations	175	188	171	175	175	175	175

Table 6: **Discussion content by treatment and pair characteristics.** Standard errors based on 10,000 bootstrap samples, clustered by coder. Hypothesis tests based on linear regressions with coder fixed effects. Column 2 does not include coder fixed effects because minutes discussed is measured directly. The number of observations is smaller for column 3 since the measure could not be determined from some of the recordings due to audio quality. $^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$.

1 on an indicator for whether they participated in the education intervention.[47] We find that the direct effect of the intervention is to raise Senders' financial competence. The level of competence of Senders who undergo the intervention is -0.192, compared with -0.274 for those who did not. The difference of 8.2 percentage points is substantial, and corresponds to roughly a one-third increase in competence ($p < 0.1$).[48]

Next we test whether peer communication is more effective if the Sender has participated in the education intervention. Panel B of Table 3 shows that there are no detectable indirect effects of education on a Receivers' degree of improvement, pooling discussed and novel tasks. Depending on the specification, the indirect effect of education leads to an additional improvement that varies between -0.1 and 0.9 percentage points, and is never statistically significant.

Differences do emerge, however, once we distinguish between discussed and novel tasks. Panel B of Table 4 shows that Receivers who are indirectly exposed to the education intervention improve to a significantly greater degree in decision tasks they have discussed than in novel ones (columns 1 - 3). The difference-in-differences, compared to Receivers whose Sender did not receive the education intervention, is statistically significant ($p < 0.05$ or $p < 0.01$, depending on specification).

To what extent do these effects correspond to qualitative differences in conversations? Table 6 shows that pairs whose members are initially in the same half of the competence distribution are substantially less likely to highlight similarities if the Sender participates in the education intervention (column 1, $p < 0.01$). It becomes much more likely, however, that one member of the pair tries to convince the other that he is more knowledgeable than his peer, regardless of their similarity (column 5, $p < 0.05$ for similar pairs and $p < 0.1$ for dissimilar pairs). This finding suggests that education may increase the likelihood that Senders rely on strategies akin to 'proof by intimidation,' which would help Receivers perform better in discussed problems, but would diminish the benefits of peer communication for novel problems.

An additional possibility is that the contents of the education intervention crowd out other methods of decision making. Indeed, in the *Indirect Education* condition three quarters of the pairs discuss the rule of 72—the main substantive content of the education intervention—whereas virtually no one does so in the *Communication* treatment. Meanwhile, 62.6% of pairs discuss the exact compound interest formula, $future\ value = present\ value \cdot (1 + r)^t$ if the Sender has

[47] In contrast to the previous regressions, we control for preexisting financial competence using only the $Test_0$ tasks and not the $Test_1$ tasks, because the educational intervention is administered between these two Stages.

[48] Appendix A.2 presents a more detailed analysis.

not participated in the education intervention, while only 41.6% do so in the *Indirect Education* treatment. If the rule of 72 is an inferior substitute for the exact formula (either because the Sender fails to explain it intelligibly, or because the Receiver fails to apply it correctly), then such crowding out would reduce the effectiveness of peer communication.

5 Conclusion

We have presented an experiment in which communication about financial decisions between randomly paired subjects leads to genuine improvements in the quality of decision making, measured according to the *financial competence* method of Ambuehl, Bernheim and Lusardi (2018).

We have shown that the improvements reflect genuine conceptual learning rather than mimicry of the choices of those who are better informed. The beneficial effects of communication are especially pronounced in interactions between people who are similarly unskilled, and who seem to be more adept at addressing each others questions and concerns at the appropriate level and pace. Subjecting one member of each pair to an effective financial education intervention, however, provides no benefits beyond those arising from communication alone. The intervention provides subjects with more skills to transmit, but decreases the effectiveness of skill transmission by creating skill differentials and by crowding out more effective forms of communication.

Some have argued for targeting interventions at influencers and relying on social diffusion to leverage the effects of financial education (see, e.g., Haliassos, Jansson and Karabulut (2017) and Ouimet and Tate (2017)). Because the indirect beneficial effects of education in our experiment arise from mimicry rather than from improved conceptual understanding, our experiment calls the effectiveness of many such diffusion strategies into question. For the same reason, our results caution against promoting rules of thumb that are appropriate for particular segments of the population, but that may propagate to other segments.[49]

A natural extension of our research would involve the study of peer effects in settings where subjects interact with peers of their own choosing. Another extension would examine interaction

[49] For instance, Ambuehl, Bernheim and Lusardi (2018) show that subjects are highly heterogenous in the extent to which they misestimate the power of compound interest. The majority suffers from exponential growth bias, but many people are well calibrated, and some overestimate the power of compounding. The financial education intervention their subjects receive alters behavior mainly through its rhetorical elements, causing subjects to increase their estimate of the value of interest-bearing assets, regardless of the initial extent of their bias. While this counteracts exponential growth bias on average, this intervention harms subjects who did not initially underestimate the power of compound interest.

among larger groups of individuals. In both settings, subjects' abilities to identify those from whom they can benefit most, and to avoid those who set bad examples, would play important roles in determining the effects of social interaction. We leave these issues for further research.

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