Social Transmission of Financial Decision Making Skills.

A case of the blind leading the blind?
PRELIMINARY AND INCOMPLETE

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Abstract

Often, people consult with others for advice before they make financial decisions. Previous research argues that such communication amounts to a case of the blind leading the blind. In this paper, we document that it can be beneficial, and explore mechanisms. In our laboratory experiment, subjects make private decisions about investments involving compound interest both before and after they communicate with a randomly assigned partner. Communication not only improves decision making for the specific tasks they have sought advice about, but subjects successfully generalize these skills to novel decision problems. We find that communication is most beneficial when pair members' skills are at similar levels—the transmission of financial competence requires a common language, and is not merely a case of information flowing from those who have it to those who do not. Finally, communication leads subjects to reevaluate their privately revealed time preferences. Discount rates move towards the communication partners' rate, and do so to a larger extent if the partner is more patient. We suggest policies to improve the quality of financial decision making.

Keywords: Communication, Financial Literacy, Financial Competence, Behavioral Welfare Economics, Experimental Economics, Exponential Growth Bias

1 Introduction

Often, people consult with non-experts such as family and friends for advice before they make financial decisions, sometimes exclusively (Lusardi, 2003, 2008; van Rooij et al., 2011; Lusardi and Mitchell, 2014; Bernheim, 1998). What are the effects of such communication? A substantial literature shows that social interaction *affects* personal financial decision making (Beshears et al., 2015; Brown et al., 2014; Bursztyn et al., 2014; Cai et al., 2015; Duflo and Saez, 2003; Hvide and Östberg, 2014; Hong et al., 2004, 2005; Kast et al., 2016; Ivković and Weisbenner, 2007). Less is known, however, about the extent to which such interaction is beneficial or harmful (Hastings et al., 2013).[1] Previous research paints an ambiguous picture. On the one hand, some research argues that relying on non-experts for advice about financial decisions leads to little more than people copying each others' mistakes.[2] For instance, Bernheim (1998) concludes that "in the majority of cases, reliance on parents, relatives, and friends amounts to the blind leading the blind". Indeed, given Ambuehl, Bernheim and Lusardi (2016)'s demonstration that even carefully designed, professional, well-informed communication can fail to improve decision making, why should one expect the average peer to succeed at this? On the other hand, a literature in group decision making documents that groups, required to choose as a single unit, often make better self-interested decisions than individuals, in contexts as diverse as strategic choice, inferential reasoning, and decision making under risk (see Charness and Sutter (2012) and Kerr and Tindale (2004) for reviews).

Given how frequently financial decisions are affected by communication with peers, it is important to study whether such communication improves or impairs subjects' decision making, and to examine the mechanisms through which such changes obtain. If communication leads to better decision making, is it because subjects acquire decision making skills that they can generalize to novel decision problems, or do they merely mimic others without understanding the implications of their choices? When is communication most beneficial? Is the transmission of financial decision making skills a process by which skills spill over from those who have them to those who do not, or does communication work more effectively between individuals of similar skill levels who can address each others concerns at the appropriate level and pace? Finally, do subjects really learn how to make financial choices that more closely align with their preferences, or do they instead re-evaluate their preferences, perhaps towards those of the peer?

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

[2] Linnainmaa et al. (2016) find that even professional financial advisers often make investment mistakes themselves, and successfully convince their clients to do likewise. In a more stylized context, Boudreau and McCubbins (2010) find that providing subjects with polls conducted amongst the subjects' peers about which answer to mathematics questions is correct often leads them to perform less well and lose money.

We answer these questions in the context of a laboratory experiment in which subjects privately make financial decisions that involve the concept of compound interest, after having discussed a subset of them face-to-face with a randomly chosen peer. By comparing these decisions to a control condition in which subjects do not communicate with anybody, but instead solitarily contemplate the decisions others discussed, we can assess whether communication improves the quality of decision making. By comparing the quality of decisions for novel choice problems to those that subjects discussed, we can determine the extent to which subjects merely mimic others, and the extent to which they acquire genuine skills that they can deploy flexibly. By measuring discount factors, we quantify the effect of communication on revealed preferences. Finally, in an additional treatment condition, one member of each pair proceeds through an effective financial education intervention. This allows us to study the indirect effect of financial education on peers who have not directly participated.

We evaluate the quality of decision making using the notion of *financial competence* developed in Ambuehl, Bernheim and Lusardi (2016). It compares the decisions subjects actually make to those they would have made under a complete understanding of their opportunity set. This method has multiple virtues. First, it uses subjects' own preferences as the welfare relevant benchmark, and is thus non-paternalistic. The types of external judgments of consumers' choices that are common in policy discussions, such as whether they are 'sufficiently patient' or 'save enough,' are entirely avoided. Second, it obviates the need for parametric models of decision making. Third, it yields a quantitative measure of decision quality one can formally interpret as an index of consumer welfare with the framework of Bernheim and Rangel (2009).

We document five results. First, in our experiment, communication improves decision quality. After communicating with a randomly selected peer, the choices subjects make correspond more closely to those they would make under a complete understanding of their opportunity set. Second, the improvements reflect conceptual learning rather than mimicry of others who know better. Indeed, people who communicate are able to make better decisions even for novel problems. They learn conceptually, and are able to generalize, at least to the limited extent in our setting.[3] Third, an unsophisticated individual learns more when interacting with another unsophisticated individual than when interacting with a sophisticated individual. Hence, the transmission of financial decision making skills is not a process by which information flows from those who have it to those who do not, as has been found in other contexts of peer learning (e.g. Jackson and Bruegmann (2009)). Rather, financial competence is transmitted more effectively between people with similar skill levels, who can address each others' concerns at the appropriate level and pace. By extension, making one individual more

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[3]This result relates to a literature on *transfer learning* in the field of education that documents that people often have difficulties applying recently acquired knowledge in a new domain (see Barnett and Ceci (2002) for a review). When people improve their understanding through communication with similarly able peers, however, they appear to (partly) overcome these difficulties.

sophisticated than their peers may, counterintuitively, reduce their value to friends. We test the latter implication directly by educating some of the subjects, making them more sophisticated, and seeing how this affects their peers. We find, fourth, that people cannot effectively communicate financial decision making skills they have just newly acquired. Their peers do make slightly better decisions for the exact questions they discussed, but are unable to generalize to novel problems. This result holds even though the financial education intervention we examine significantly improves the quality of the decision making for the individual receiving the education. There is, apparently, a difference between understanding something and acquiring the language necessary to explain it to someone else.

Fifth, communication does not only improve people's ability to make choices that align more closely with what they would choose under a complete understanding of their opportunity set, it also changes their underlying preferences. The rate of time preference of a subject who communicates with a partner moves towards the partner's discount rate by an average of 12 percent of the distance. This effect is driven primarily by impatient subjects who assimilate to more patient peers, rather than by the patient becoming more impatient. According the non-paternalistic approach to decision making we employ in this paper this change is neither good nor bad, by definition. For those who adopt the paternalistic perspective that most people are insufficiently future-oriented, this result suggests that the benefits of communication are not limited to decisions that are improperly understood; they are also present when subjects understand the pertinent consequences.

Given there are, in this experiment, significant upsides, but very limited downsides to communication about financial decisions, our results suggest that the quality of financial decision making might be improved by encouraging communication. More specifically, our findings suggest how to improve the design and administration of financial education interventions. Such interventions may attempt to harness power of communication by interactive tasks. The effectiveness of interventions that use peer communication can be maximized by having communication occur between people who are on similar levels of understanding. In fact, ours is not the only paper to find that subjects benefit the most from discussing with similar peers, even if both of them are relatively unsophisticated. In field experiments in university contexts, both Booij et al. (2016) and Feld and Zölitz (2016) obtain similar results.[4]

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[4]On the one hand, Booij et al. (2016) find that low-ability students profit most from a switch from random allocation to economics tutorial sessions to a system of three-way tracking. After the switch, these students have more positive interactions with peers and become more involved. Also in a university context, Feld and Zölitz (2016) find that low-achieving university students can be harmed when paired with high-achieving peers, due to the altered nature of the group interaction. To the extent that our results show that communication can help subjects help themselves, even if they are both on the lower end of the skill distribution, they are related to Michaelsen et al. (1989). In that experiment, 97% of groups make decisions that are better than those of their best member. Apparently, communicating with others about problems does not only transfer competence, it can actually generate it. A complementary finding by Bhattacharya et al. (2012) is that retail investors who receive unbiased financial advise from experts (who have better financial decision making skills than their clients) largely fail to follow it, perhaps because adviser and advisee are so different. We caution, however, that our finding may depend on context. Jackson and Bruegmann (2009), for instance, show that teachers become more effective the more effective their teacher peers are.

At the same time, our results caution against targeting interventions at influencers and relying on social diffusion. Because peers who have not themselves participated in an education intervention sometimes simply mimic others without genuinely understanding the reasons for this choice, it may be ill-advised to provide rules of thumb that are appropriate only for particular segments of the population if they then share them with those for whom the rules are inappropriate.[5]

We contribute more broadly to the literature on financial education by beginning to fill a significant gap in the evaluation of such programs. As noted by Hastings et al. (2013), no study that attempted to evaluate a financial education intervention has allowed individuals to access other sources of information to assess whether individuals understand their limitations and can compensate for them by engaging other sources of expertise. In this paper we show that individuals are indeed able to improve their decision making through such strategies.

Finally, this paper contributes to a literature on conformity in economic choice (e.g. Bernheim (1994), Cai et al. (2009), Lahno and Serra-Garcia (2015), Goeree and Yariv (2015)). Our finding that subjects assimilate their discount factors towards their peers' documents the phenomenon in a novel environment. Additionally, our result that this assimilation is more pronounced if the partner is more patient than relates to Chen et al. (2010). These authors also find highly asymmetric effects of social information. Those authors find that providing subscribers to a movie rating site with information the median user's total number of movie ratings causes those with a below-median number of ratings to increase their number of ratings to a much larger extent than it causes those with an above-median number to decrease them.

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

2 Design

Each subject participates in one of three treatments. In the *Communication* treatment, subjects converse in pairs after making certain decisions but before making others, as detailed below. The *Communication with Education* treatment adds financial education for one member of each pair prior to communication. A *Solitary* treatment serves as an overall control—there is no education, and no communication prior to decision making. Even subjects in the *Solitary* condition communicate with another subject, but only after having made all decisions, so no information exchanged during that

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[5]A related phenomenon are high-load index funds, which have become much more popular in recent times. Many financial education interventions suggest that individual investors purchase index funds, because they achieve broad diversification at low cost (e.g. Malkiel and Ellis (2013)). If people only retain that 'index funds are good', but not the reason why, financial institutions may profit from supplying index funds with high loads (Hortaçsu and Syverson, 2004).

stage can influence any decisions. Nonetheless, the expectation of communication could affect choices by, in effect, incentivizing subjects to think harder about the tasks. By holding that factor constant, we can isolate the effects of the communication itself through comparisons across treatments.

Decision tasks. Following the *financial competence* approach (Ambuehl et al., 2016), there are two main types of decision tasks. They present subjects with the same opportunity sets, but differ in framing. The first type, labeled C (for 'complexly framed'), consists of tasks that implicate preferences while drawing on the subject's knowledge of a financial concept. Specifically, we present a future reward as an investment problem involving compound interest (e.g. "We will invest y tokens in an account with $r\%$ interest per day. Interest is compounded daily. We will pay you the proceeds in t days."). Subjects indicate the amount of money, received within two days, they consider as good as receiving payoff from the investment at the future date.

The second type of decisions, labeled S (for 'simply framed'), present future rewards transparently, rather than as an investment, and hence do not require knowledge of compound interest (e.g. "You will receive x tokens in t days"). For every C-task a subject completes, she completes a corresponding S-task that leads to the same future reward, and hence is substantively equivalent.

Following Ambuehl, Bernheim and Lusardi (2016) we argue that 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 what she desires, in at least one of the frames. The extent of this divergence is a measure of the quality of decision making, and our main dependent variable. As detailed in Ambuehl, Bernheim and Lusardi (2016), it can be formally interpreted as the amount of welfare a subject loses from having to make a choice in the complex rather than in the simple framing, as judged by her own preferences revealed in the simple framing.

An ancillary type of tasks, labeled T (for 'test'), consists of incentivized questions that test the subject's ability to compute compound interest, but do not involve any preference element. In these tasks, subjects are presented with a compound interest investment that pays off in t days, and are asked what amount of money to be received in t days they consider equally valuable. They serve as statistical control variables.

Stages. Panel A of Table 1 summarizes the overall structure of the experiment. We divide decision tasks between three stages, labelled 0, 1, and 2, with some intervening activities, as follows. Stage 0 consists of S-tasks and T-tasks. It serves an ancillary role; the choices subjects make in this stage will serve as an instrumental variable to address attenuation bias in our analysis on preference-reevaluation. Next, in the *Communication with Education* treatment, one subject in each assigned pair views a video

explaining the concept of compound interest (details below). The other subject views an unrelated video of equal length, as do all subjects in all other treatments (a documentary about lions).

Stage 1 includes C, S, and T decision tasks. Comparisons of equivalent C and S tasks allow us to assess the quality of decision making prior to communication (but after education if the subject has received it). Cross-subject comparisons allow us to assess the effect of our educational intervention on the quality of decision making. T tasks allow us to classify subjects according to preexisting knowledge about compound interest in a way that circumvents statistical issues such as regression to the mean.

Next, we allocate 15 minutes either to discussion within assigned pairs (in the *Communication* treatments), or private contemplation (in the *Solitary* treatment). Communication and contemplation are both directed. We instruct the subjects to focus on particular complexly framed decision problems, which we will call the D tasks, as these relate directly to the discussion / contemplation stage. We hand out sheets of paper with these tasks just prior to this stage.

Stage 2 consists of C and S decision tasks. Comparisons between equivalent tasks again allow us to assess the quality of decision making, but this time after communication. By comparing our measure of the quality of decision making before and after the communication phase (and adjusting for any baseline improvement or deterioration in the *Solitary* control), we can assess the degree to which communication improves decisions. Similar comparisons for the S tasks alone allow us to assess the effect of communication on underlying preferences.

Finally, in the *Solitary* treatment, subjects converse in assigned pairs, for reasons discussed above.[6]

Roles. In each treatment, subjects are assigned one of two roles, A or B. There are two differences between the roles. First, in the *Communication with Education* treatment, subjects A participate in the education intervention while subjects B do not. This allows us to assess the indirect effect that education exerts on others through communication. Second, both in the *Communication* and in the *Communication with Education* treatment, the A subjects perform the D tasks that will be the focus of the discussion stage already in the preceding stage 1. B subjects, by contrast, perform these tasks only in stage 2. Accordingly, pairs discuss tasks that A subjects have already thought through, but B subjects have not. For these reasons, we tend to think of A subjects as *Senders* and B subjects as *Receivers*. Notice, however, that communication can flow in either direction, or not at all. Finally, in the *Solitary* treatment, all subjects are in role B. Panel A of Table 1 details the timing of the decision problems for each role.

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[6]In a preliminary Solitary treatment, subjects did not anticipate that they would communicate with anyone at all. In that treatment, financial competence was worse throughout, presumably because these subjects lacked the kind of social motivation for good decision making that was present in the other treatments. Related attempts to save face have been documented by Blanes i Vidal and Nossol (2011) and Kuhnen and Tymula (2012).

A. Experiment Structure

Treatment	Communication with Education		Communication without Education	Solitary
Role	B	A	A and B	Only B
			Like Communication with Education treatment, except	
Stage 0 decisions	S_0, T_0	S_0, T_0		
	Documentary	Education	A subjects also watch documentary	
Stage 1 decisions	C_1, S_1, T_1	D, S_1, T_1		
	Discussion	Discussion		Solitary contemplation instead of discussion
Stage 2 decisions	D, C_2, S_2	C_1, C_2, S_2		

B. Decision Problems

Timeframe		Set C_1	Set C_2	Set D
72	Interest rate	3%	1%	2%
	# doublings	3	1	2
	Principal	$\{6, 14, 22\}$	$\{12, 28, 44\}$	$\{3, 7, 11\}$
	Future reward	$\{24, 56, 88\}$	$\{24, 56, 88\}$	$\{24, 56, 88\}$
48	Interest rate	3%	4.5%	1.5%
	# 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 1: Decision problems and timeline. Within each stage, all decisions were presented in an individually randomized order. Equivalent decision problems were not identified as such. Parameters of problems in sets T_0 and T_1 are structured slightly differently. In the format (principal, interest rate, duration, future reward) set T_0 consists of the problems $(6, 4, 36, 24), (7, 4, 56, 54), (22, 8, 18, 88)$, and set T_1 consists of $(7, 6, 36, 56), (22, 2.67, 54, 88), (12, 4, 18, 24)$.

An important feature of our design is that it allows us to distinguish between three mechanisms that may account for effects of communication on the quality of decisions: choice mimicry (I take a specific action because you have recommended it), conceptual learning (I take actions that more successfully achieve my objectives), and preference mimicry (I take actions that I think you would prefer), by using the three categories of tasks that B subjects perform in stage 2. If we find that communication improves the quality of decision making only for the tasks the subjects have discussed (D and not C_2), we conclude that the mechanism involves choice mimicry rather than conceptual learning. But if we find that the improvement is the same regardless of whether the task was discussed, we conclude that the mechanism involves conceptual learning rather than choice mimicry. More generally, a comparison of the measured improvement for these two task blocks, allows us to infer the relative importance of these mechanisms. Finally, we assess the importance of preference mimicry by using the simply framed tasks to compare the degree of patience expressed across stages 1 and 2.

Details concerning tasks.   Panel B of Table 1 provides details about the complexly framed tasks. Each set of tasks consists of six decisions. Half of them concern a payout in 72 days, and the other half concern a payout in 48 days. Subjects face each combination of timeframe and interest rate three times, with varying principals, in order to increase statistical power. In each decision, the investment compounds to 24, 58, or 88 tokens, or to an amount within two units of these numbers. We choose the parameters such that the principal doubles an integer number of times over the investment period. This facilitates application of the rule of 72, a heuristic for approximating exponential growth that is the focus of the intervention in the *Communication with Education* treatment. In each task we elicit the amount of money to be received within two days of the experiment that the subject considered as good as the investment. We do so using a once-iterated multiple price list ranging from 0 to 109 tokens, with a resolution of 10 tokens in the first stage, and 1 token in the second (Andersen et al., 2006).[7] Subjects can proceed only if they have a single switching point; in case of multiple switches an error message prompted them to change their decisions.[8] Subjects complet all lists at their own pace.

Details concerning communication.   Within each treatment, we randomly paired subjects who then communicated face-to-face. We hand out sheets of paper listing the decision problems in set D.[9] We recommend that subjects discuss for 15 minutes, but they are free in their choice of how long to discuss, and can continue with the experiment once they are done. We provide no explicit

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[7]The amounts the investments compound to are located near the top, middle, and bottom of the multiple decision lists. We aimed for balance across these locations so that framing effects such as the tendency to choose switching points towards the middle of a list do not influence our results.

[8]See Appendix E.5 for screenshots of the decision screens.

[9]See Appendix E.6 for an example of a discussion sheet.

incentive for the discussion, but we remind subjects that after the discussion stage they will complete 18 additional decision tasks, which might include those on the decision sheets, and that there is therefore 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 had discussed a problem before. We do not direct their attention to this fact. Before the discussion stage, we ask subjects to write down two questions they might want to ask their partner, and two pieces of advice they may want to give. We unintrusively record all communication, and subjects are aware of this.

Education Intervention We use an extended version of the education intervention employed in Ambuehl et al. (2016) that 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).[10] Its main substance is the rule of 72, a method for approximating the time it takes for an investment to double. We improved the effectiveness of that intervention by adding practice questions with personalized feedback. See Appendix E.1 for details.

Implementation and payment Subjects are paid via Amazon gift cards for one randomly selected decision, and are aware of this from the beginning of the experiment. We place a calculator with the capacity to calculate exponentials on each subjects' terminal, since people typically have access to such tools when making financial decisions.[11]

All instructions are displayed on screen and explained via an audio recording to minimize experimenter effects.[12] 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 exactly the time we promise them. They end with an understanding check that subjects needed 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 one pound, 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 an insufficient understanding.[13] In addition, subjects see a completed list, and have to

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[10]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.

[11]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 et al. (2016) finds that an equally small proportion of experimental subjects use these tools for their experimental decisions.

[12]See Appendix E.4 for screenshots. 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 https://youtu.be/xSvxEG_R5WY (Stage 2-Solitary).

[13]These decisions were not incentivized.

indicate their payment in case the computer will select a given line for implementation. Subjects can proceed regardless of their answers to these questions.

Before participating in the main stages of the experiment, subjects also complete a short battery of unincentivized psychological measurements,[14] as well as three standard financial literacy questions (Lusardi, 2008).[15] At the end of the experiment, they complete an unincentivized demographics survey and answer questions about their decision making processes and thoughts about their partners.

3 Data and Preliminary Analysis

Data We ran our sessions from fall 2015 to spring 2016 at the University of Birmingham, UK.[16] The average session lasted 123.75 minutes (s.d. 20.01 minutes). On average, subjects earned £26.55, including a £12.5 participation fee; earnings ranged from a low of £14.5 to a high of £32.5. Because our interest is in understanding how communication affects financial competence, rather than the understanding of the mechanics of multiple decision lists, our analysis only includes subjects who passed the understanding checks on multiple decision lists (87.8%, or 460 of 524 subjects).[17] We do, however, retain subjects who understood the mechanics of the decision lists, but were paired with another subject who did not, since understanding multiple decision lists is not a prerequisite for being an effective discussion partner. In spite of our precautions to avoid repeat participants, four subjects participated in both treatments.[18] For these subjects, we only retain the data from the first session they participated in. This leaves us with 99 Receivers in the *Communication* treatment, 89 subjects in the *Communication with Education* treatment, and 75 subjects in the *Solitary* treatment.

Time preferences The average discount rates implied by the simply framed choices in stage 0 are 0.87 and 0.84 for the 48 and 72 days timeframes, respectively, with a standard deviation of 0.27 in both cases.[19] In stage 1, the respective numbers are 0.87 and 0.85, and in stage 2 they are 0.89 and 0.86. This is comparable to the discount rates elicited in the literature (Frederick et al., 2002).

Randomization check Appendix Table A.6 reports the means of 36 variables across treatments. Out of these, 4 differ across treatments at the 10% level, and an additional 3 differ at the 5% level in

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[14]This battery consisted 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 3-item scale of cognitive style.

[15]We elicit these at the beginning of the study to prevent answers from being influenced by subjects' communication partners.

[16]We chose the University of Birmingham after learning that the universities we were affiliated with at the time did not provide sufficiently large pools of subjects. Appendix E.3 tabulates the specifics of each session.

[17]90.78% among Senders and 85.67% among Receivers.

[18]These subjects might have multiple accounts in the participant management system. We identify these subjects via identical email addresses, which they need to enter to receive payment via Amazon gift cards.

[19]These numbers include all subjects in all roles who passed the understanding check about the multiple price list.

a joint test. This is higher than expected by chance. The significant differences occur for the demographic variables gender, age, and credit card ownership. Moreover, differences occur for debriefing questions (e.g. whether subjects had previously talked about the study to others) which likely reflect the fact that we ran the *Solitary* treatment after the *Communication* treatment. To address these differences, we conduct comparisons involving the *Solitary* treatment using regressions that control for various characteristics of the subjects.[20],[21]

Exponential growth bias   Exponential-growth bias is the well-established tendency to underestimate the extent to which compound interest accumulates over time (Wagenaar and Sagaria, 1975; Eisenstein and Hoch, 2007; Stango and Zinman, 2009; Almenberg and Gerdes, 2012; Levy and Tasoff, 2016). We reproduce this result in our sample. Focusing on the choices Receivers make in the second stage, we find that, on average, they undervalue compound interest investments by a significant 5.03 percentage points (s.e. 1.90, clustered by subject).[22]

4   Main Analysis

Financial competence   Throughout, we measure the quality of subjects' decision making by their *financial competence*—the extent to which their valuations in substantively equivalent decision problems align across the two framings. We use the absolute difference in valuations as measure of misalignment. As shown in Ambuehl et al. (2016), this can be interpreted as the maximal amount of welfare a subject can lose from differently valuing the same choice option when it is framed differently.

We let $V_{j,d}^{f}$ denote individual j's valuation in decision problem d with framing $f \in \{simple, complex\}$.[23] To compare behavior across rewards of different sizes, we normalize valuations. If the future reward

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[20]We conducted the *Solitary* treatment at a later time than the treatments involving communication. This is because we had originally intended to focus on a slightly different question.

[21]We sort control variables into three categories. *Demographic variables* consist of gender, age, age^2, ethnicity dummies, a dummy for whether English is a subject's first language, a dummy for whether a subject is an international student, and a dummy 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, whether and what kind of financial assistance subjects receive for their studies, and a dummy indicating whether they correctly answered all of 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-5 personality scale, and dummies about whether subjects had heard about the study before, had talked to others about it, had prepared for it, and wished to be contacted in case there would be a follow-up study.

[22]Because valuations in multiple price lists are affected by how far they are from the upper and lower bounds of the list (Andersen et al., 2006) we perform these estimations separately depending 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; and the reverse for the latter. 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 T_0 and T_1 these numbers are 24.29 (s.e. 4.76), -15.62 (s.e. 2.07), and -18.41 (s.e. 1.47). Appendix B.2 redoes our main analysis on the subsample of subjects who suffer from exponential growth bias (that is, indicate lower average valuations in complexly than in simply framed problems for sets C_1 and S_1). All of our results are qualitatively unchanged.

[23]Because we elicited valuations using multiple price lists, they are interval-coded. We use interval midpoints for analysis.

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associated with decision problem d is given by r, we let $\delta^f_{j,d} = \frac{V^f_{j,d}}{r}$ denote subject j's *normalized valuation*. For decision d subject j's financial competence is then given by

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

Notice that *lower* values signify *higher* financial competence. Notice also that we use the simply framed choices a subject makes in stage s of the experiment to assess their quality of their choices in the complexly framed decisions in the same stage; an assumption that we relax in Appendix Section D.1.

In this experiment, we study the extent to which subjects' financial competence *changes* between stages 1 and 2. By considering changes rather than levels of financial competence we difference out individual-level heterogeneity in financial competence and thus obtain more precise estimates. We define the improvement on the question-level. 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 timeframe and the same future value. Subject j's improvement on task d is then given by

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

which is positive for a subject whose decision making improves from stage 1 to stage 2. This yields 12 observations per subject, one for each valuation pair in stage 2.

Overall financial competence   Figure 1 shows a histogram of financial competence averaged over all decisions, $\bar{c}$, for all subjects in our study who passed the understanding check on multiple price lists. To interpret the magnitudes, consider, for example, that a value of $\bar{c} = 0.2$ obtains for somebody who is willing, on average, to pay 80 cents for a complexly framed investment that he would value at \$1 if he properly understood his opportunity set.[24] 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 smaller than 0.015, and the first and third quartiles are at 0.05 and 0.33, respectively. (The handful of cases with $\bar{c} > 1$ represent subjects who exhibit dramatically higher valuations in the complex than in the simple frame.)

Regression specifications   We will linearly regress $Improvement_{j,d}$ on various treatment indicators, and cluster standard errors on the subject level. An advantage of this dependent variable is interpretability. For subjects who are initially heterogenous in financial competence, however, it might change to different extents over the course of the experiment. (We explicitly investigate this effect in

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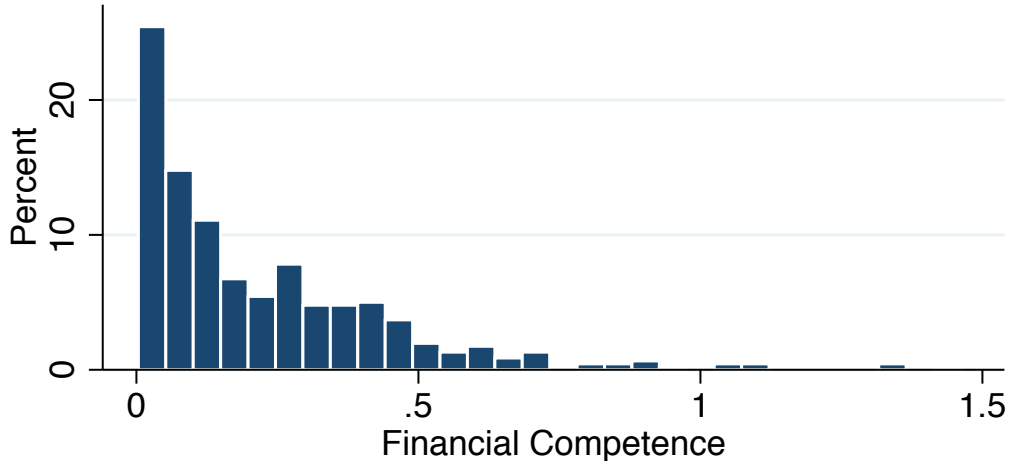
Figure 1: Distribution of financial competence. We display within-subject averages taken across all decisions that involve a complex framing.

Section 4.) Through such a mechanism, initial differences in financial competence could artefactually cause apparent treatment effects. To prevent this issue, we therefore include a control for Receivers' preexisting level of financial competence, as measured by the decisions made in sets T_0 and T_1 in all regressions in which $Improvement_{j,d}$ is the dependent variable.

Direct effect of the education intervention   Our main focus is on the decision making of subjects in the role of Receivers. To assess whether we could plausibly detect a difference in Receivers' decision making depending on whether their partner participated in the education intervention, we check whether the intervention improved the Senders' decision making.[25] We regress Senders' financial competence in stage 1 on an indicator of whether they have participated in the education intervention. We find that subjects who participate have a competence of 0.196 compared to 0.284 for those who did not, an increase in 8.8 percentage points. This corresponds to a 31% increase in competence, significant at the 5% level, and decreases slightly as a larger array of control variables are included. Appendix D.4 presents a more detailed analysis.

A case of the blind leading the blind?   We now study the central question: Is communication about financial decisions a case of the blind leading the blind, or does it help people make better decisions? To answer, we regress $Improvement_{j,d}$ on indicators for the *Communication* and *Communication with Education* treatments, using the *Solitary* treatment as baseline. We pool across decisions in sets D and C_2, and retain subjects in the role of Receiver only.

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[25]Unlike in section 3, we control for preexisting financial competence using only the respective measure elicited in stage 0, not that elicited in stage 1, because the educational intervention is administered between these two stages.

We find that communication with a randomly selected peer has a significant beneficial effect on the quality of decision making, as Table 2 shows. According to the specification in Column 1, Receivers in the *Communication* treatment experience an improvement in financial competence that is 6.4 percentage points larger than that of subjects in the *Solitary* treatment. The latter is a statistically insignificant 2.2 percentage points. The effect of communication is sizable compared to the average level of financial competence amongst all Receivers in stage 1, which is given by 0.26 (s.e. of the mean 0.016). These findings remain substantively unchanged as we include increasingly large sets of statistical control variables (Columns 2 - 4).

Notably, Receivers do not improve by a significantly larger extent if their discussion partner has completed the education intervention, even though the intervention leads to a significant improvement in Senders' financial competence of 8.8 percentage points. The indirect effect is an order of magnitude smaller, ranging between 0.5 and 0.9 percentage points, depending on specification. To the extent people learn from each other, the small indirect effect of effective education is a puzzle. This leads naturally into our next set of questions—when and through which mechanisms do people benefit from talking to each other?

Do subjects genuinely learn by communicating, or do they blindly copy each other?
We now separately estimate the effect of communication on tasks the subjects have, and have not, discussed with their peer (decision sets D and C_2, respectively). Decisions about the latter will improve only to the extent that communication endows Receivers with skills they can generalize to new, somewhat different decision problems. The additional improvement for decisions in set D then captures the extent of mimicking the choices of others who know better, without understanding the reasons for their choices.

For each subject we therefore have two distinct dependent variables, the improvement in choice quality for the discussed, and for the novel decision problems, respectively. We estimate this system using seemingly unrelated regression.[26] For each subject, we average $improvement_{j,d}$ separately across the 6 discussed questions, and across the 6 novel questions and thus obtain two observations per subject.

We find that the significant improvements in the *Communication* treatment are almost entirely driven by conceptual learning, as Table 3 shows. According to the specification in Column 1, a Receiver in the *Communication* treatment improves by an additional 6.6 percentage points on the novel questions in set C_2 compared to the *Solitary* treatment. Importantly, this additional improvement is statistically indistinguishable from the improvement the subjects experience for the discussed questions

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[26]The SUR framework is appropriate, since subjects' valuations for the valuation tasks in set D are benchmarked against the same simply framed choices as their valuations in set C_2. This introduces the kind of correlation between the error terms in the two equations that SUR is designed to account for efficiently.

VARIABLES	(1)	(2)	(3)	(4)
	Improvement in Receivers' financial competence			
	before / after communication			
Improvement in Solitary	0.022	0.021	0.025	0.016
	(0.018)	(0.018)	(0.018)	(0.018)
Additional improvement				
from communication				
with uneducated Sender	0.064**	0.066**	0.060**	0.072***
	(0.027)	(0.028)	(0.028)	(0.027)
with educated Sender	0.073**	0.073***	0.068**	0.080***
	(0.029)	(0.028)	(0.027)	(0.028)
Indirect effect of education	0.009	0.007	0.008	0.008
	(0.027)	(0.026)	(0.026)	(0.025)
Controls				
Initial skills	Yes	Yes	Yes	Yes
Demographic	-	Yes	Yes	Yes
Financial	-	-	Yes	Yes
Psychological	-	-	-	Yes
Observations	3,156	3,156	3,156	3,156
Subjects	263	263	263	263

Table 2: **Main effect of communication**. Improvement in financial competence from stage 1 to stage 2. The estimates in row 1 are the predicted levels of improvement of a Receiver in the no-communication condition characteristics equal the average amongst 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 T_0 and T_1. Standard errors clustered by subject. $^*p < 0.1,$ $^{**}p < 0.05,$ $^{***}p < 0.01$.

in set D. Hence, for subjects in the *Communication* treatment, the role of choice mimicking is negligible.[27]

Conceptual learning also plays a substantial role for Receivers in the *Communication with Education* treatment, although, if anything, slightly less so than in the *Communication* treatment.[28] Choice mimicking, by contrast, significantly gains in importance. Receivers whose partner had participated in the education intervention improve to a significantly larger extent for tasks they had discussed than for novel tasks. Apparently, if subjects newly acquire skills, others can recognize this and improve their own choices by blindly following their advise. However, subjects cannot effectively transmit this newly acquired knowledge in a way that improves peers' genuine conceptual understanding.

Neither of these conclusions change as we include increasingly large sets of statistical control variables (Columns 2 - 4).

When is communication most beneficial?   We now study who benefits most from communication, both depending on their own characteristics, and depending on being paired with specific peers. The results will help more effectively deploy communication as a tool to improve decision making.

We begin by asking how Receivers' preexisting competence affects how much they benefit from communication. Is communication with peers effective only for subjects who already have a sufficient skill base to build upon (in which case we would expect more competent Receivers to improve more), or is it those who have the largest gaps in knowledge to fill (in which case we would expect the less competent to improve more)?

To answer this question, we classify Receivers into two groups, those who were initially in the better half of decision makers, and those who were in the worse half. Because our dependent variable is the *improvement* in financial competence from stage 1 to stage 2, however, we cannot use decisions in set C_1 to classify subjects, as this might generate artifactual treatment effects through regression to the mean. Instead, we use decision sets T_0 and T_1 for classification, and reserve decision sets C_1, C_2 and D to measure changes in financial competence. In addition, for the novel questions in set C_2, we pool Receivers across the *Communication* and *Communication with Education* treatments, in order to increase statistical power. We do this because Table 3 shows that Receivers' improvement does not depend on whether their discussion partner has completed the education intervention to a statistically detectable extent. We also pool across these treatments for the discussed questions in set D and keep in mind that the resulting estimates represent a heterogenous treatment.

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[27] As a qualification to the statements in this paragraph, notice that there are slight differences between the discussed and novel tasks, which may act as a confounding factor. By comparing how the difference between improvements across discussed and novel questions depends on whether the Sender participated in the education intervention, however, this confounding factor is differenced out.

[28] This suggests that the education intervention makes it harder for Senders to communicate genuine conceptual knowledge, perhaps because the eduction intervention licenses Senders to brush off quests for explanation by appeals to authority.

VARIABLES	(1)		(2)		(3)		(4)	
	Improvement in Receivers' financial competence before / after communication							
Decisions discussed	No	Yes	No	Yes	No	Yes	No	Yes
Task set	C_2	D	C_2	D	C_2	D	C_2	D
Improvement in Solitary	0.012	0.031	0.014	0.027	0.018	0.031	0.009	0.022
	(0.021)	(0.022)	(0.021)	(0.022)	(0.021)	(0.022)	(0.022)	(0.022)
Additional improvement from communication								
with uneducated Sender	0.066**	0.062**	0.065**	0.068**	0.058**	0.062**	0.071**	0.073**
	(0.028)	(0.029)	(0.028)	(0.029)	(0.028)	(0.029)	(0.029)	(0.030)
with educated Sender	0.054*	0.092***	0.047	0.099***	0.043	0.093***	0.055*	0.106***
	(0.029)	(0.030)	(0.029)	(0.030)	(0.029)	(0.030)	(0.029)	(0.030)
p-value								
educated = uneducated	0.648	0.290	0.519	0.263	0.561	0.262	0.541	0.241
Controls								
Initial financial competence	Yes		Yes		Yes		Yes	
Demographic	-		Yes		Yes		Yes	
Financial	-		-		Yes		Yes	
Psychological	-		-		-		Yes	
p-values								
discussed = not-discussed if Sender uneducated	0.846		0.885		0.834		0.917	
discussed = not-discussed if Sender educated	0.043		0.006		0.007		0.005	
Difference in differences	0.016		0.005		0.006		0.004	
Subjects	263		263		263		263	
Observations	526		526		526		526	

Table 3: **Conceptual learning or choice mimicking?** Improvement in financial competence for discussed and novel valuation problems, estimated SUR in a two-equation system. The means of the dependent variable for the novel valuation problems are 0.021 and 0.075 in the Solitary and Communication treatments, respectively. For the discussed valuation problems they are 0.046 and 0.090. Statistical controls are constrained to have the same coefficients across the two equations. $^*p < 0.1,$ $^{**}p < 0.05,$ $^{***}p < 0.01.$

We find that for the novel questions, it is the Receivers who are initially in the worse half who benefit most from communication, as Column 1 of Table 4 shows. The effect on those in the better half is close to zero. This result is not simply because those in the better half have no room left to improve. Mean financial competence in stage 1 (as measured by tasks in set C_1) is 0.426 (s.e. of mean 0.023) amongst the initially worse half of decision makers (as measured by tasks in sets T_0 and T_1), and a still sizable 0.078 (s.e. of mean 0.008) amongst the better half.[29] This results holds also for the questions in set D, which subjects had discussed with their partner (Column 2).

We now study how these effects depend on the characteristics of the pair, including the communication partner's financial competence. Can the transmission of financial decision skills be understood as a process in which information flows from those who have it to those who do not? In this case, a subjects' improvement in decision quality will be larger the more financially competent her discussion partner. Alternatively, might the transmission of decision skills occur more effectively between subjects who are at a similar level of comprehension, and are thus able to address each other's questions and concerns at the appropriate level and pace? In this case, we would expect that Receivers' improvements are largest when they are paired with a Sender in the same half of the skill distribution, even if both of them are in the worse half.

Formally, we apply the same classification to Senders that we applied to Receivers, performing a median split depending on their preexisting ability to make good choices, as measured in decision sets T_0 and T_1. Unlike for the Receivers, we can additionally use decisions in set D to classify Senders as a means to improve the precision of our estimates.[30]

We find that being at a similar level of comprehension plays a vital role for the effective transmission of financial decision skills. By contrast, the idea that financial competence is a process of information flow from those who possess it to those who do not does not adequately describe our data. Focusing on the novel questions, Column 3 shows that compared to subjects who do not communicate at all, it is the Receivers in the pairs consisting of two below-median subjects who improve by the most significantly larger extent, compared to subjects in the *Solitary* treatment. While below-median Receivers also benefit from a pairing with an above-median-Sender, they do so to a substantially lesser extent. Column 4 shows that a similar effect applies for the discussed questions. In this case, however,

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[29]Averaged across treatments, clustered by subject. Notice that if subjects' choices exhibit stochastic elements 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 decisions. To account for this, we estimate upper bounds on the mean absolute deviation that is due to stochasticity in choice. Stage 1 simply framed choices vary in both timeframe and future value, so that we do not observe subjects make the exactly same simply framed choice twice. We first consider all pairs of simply framed decisions with approximately the same future value (we pair decisions 16 and 19, 17 and 20, as well as 28 and 21). Under this assumption, 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. Grouping decisions with the same timeframe (we group decisions 16, 17, 18, and 19, 20, 21) we obtain the looser bounds 0.092 (s.e. 0.111) and 0.137 (s.e. 0.152).

[30]We cannot use the Receiver's decisions in set D for classification, because they are only made after discussion. Neither can we use their decisions in set C_1 for classification, as these are used to define the dependent variable.

the difference in improvements between below-median-Receivers who discuss with below rather than above-median-Senders is slightly smaller and no longer statistically significant, perhaps because choice mimicking partially substitutes for the necessity to transfer skills via a common language.[31]

Finally, we study whether the results in this paragraph are merely because some pairs choose to communicate for a longer amount of time, or whether there is something different about the quality of communication, so that its effectiveness differs across pairs even conditional on how long it took.

Discussion time does differ across pairs. On average, members of pairs in which both are below the median discuss the longest. They take 2.33 more minutes than subjects in the second longest-discussing pair, which consist of a below-median Receiver paired with an above-median Sender (s.e. 1.21, $p = 0.055$). Discussion time does not, however, *entirely* explain the effect of the pair characteristics on improvement. To show this, we replicate Column 3 and 4, but include the time subjects spent discussing as a control variable. (We only consider the subset of subjects in the communication treatments alone, because data on decision time in the *Solitary* treatment were recorded differently than in the other treatments.[32]) Clearly, discussion time is an endogenous variable, but if the effects are mediated *entirely* by this variable, the other explanatory variables should attenuate significantly once we include this control.

Columns 5 and 6 display the results. We find that the results from Columns 3 and 4 remain substantively unchanged predictors of the extent to which subjects gain from communication. Even controlling for the time spent communicating we find that below-median Receivers improve by a significantly larger extent if they communicate with a below rather than above-median Sender (at the 10% level). Hence, communication affects the quality of decision making above and beyond the mere effect of discussion time. It is the quality of the discussion that matters, not merely the quantity.

In Appendix C.17, we further investigate who benefits from communication by characterizing people according to their perceived similarity with their partner, their gender, and their race. We find no effect of perceived similarity on learning. The data do show, however, that both male and female Receivers learn better from communicating with male rather than female Senders, even when flexibly controlling for financial competence. Additionally, we find that white Senders help non-white Receivers improve by a larger extent than they help white Receivers.

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[31]Appendix Table B.16 replicates the foregoing specifications, but with the *level* of financial competence in stage 2 as the dependent variable, rather than with the extent of improvement from stage 1 to stage 2. By considering levels instead of improvements, individual heterogeneity is no longer differenced out, resulting in less precise estimates. Nonetheless, we find that compared to subjects who do not discuss financial decisions with anyone, it is particularly the pairs consisting of a Sender and a Receiver that are both below median that arrive at the most significantly higher level of financial competence.

[32]In the *Solitary* treatment, the experimenter started measuring the time subjects spent discussing after having received the sheets with the problems. In the no-communication treatment, by contrast, the time measurement also includes the time spend handing out these sheets.

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Improvement in Receiver's financial competence					
Sample						
Communication treatments	Yes	Yes	Yes	Yes	Yes	Yes
Solitary treatment	Yes	Yes	Yes	Yes	No	No
Questions discussed	No	Yes	No	Yes	No	Yes
Effect of communication if						
Receiver bottom half	0.100**	0.145***				
	(0.039)	(0.041)				
and Sender bottom half			0.148***	0.179***	-	-
			(0.049)	(0.046)		
and Sender top half			0.054	0.112**	-0.088*	-0.059
			(0.044)	(0.049)	(0.049)	(0.049)
Receiver top half	-0.006	-0.008				
	(0.019)	(0.017)				
and Sender bottom half			0.007	0.001	-0.122*	-0.200***
			(0.021)	(0.020)	(0.070)	(0.077)
and Sender top half			-0.022	-0.020	-0.139*	-0.216***
			(0.022)	(0.021)	(0.072)	(0.077)
Level of improvement						
in Solitary condition	-0.006	0.040**	-0.006	0.040**		
	(0.023)	(0.018)	(0.023)	(0.018)		
in rs communication condition					0.144***	0.222***
					(0.050)	(0.046)
Control variables						
Communication duration					0.002	0.003
					(0.003)	(0.003)
Initial financial competence	Yes	Yes	Yes	Yes	Yes	Yes
Control subject bottom half	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	Yes	Yes	Yes	Yes	Yes	Yes
p-values						
$R = r$	0.014	0.000				
$rs = rS$			0.065	0.181		
$rs = Rs$			0.007	0.000		
$rs = RS$			0.001	0.000		
$rS = Rs$			0.335	0.036	0.583	0.034
$rS = RS$			0.125	0.012	0.415	0.021
$Rs = RS$			0.181	0.319	0.438	0.511
Joint insignificance	0.036	0.002	0.024	0.002	0.191	0.053
Subjects	263	263	263	263	188	188
Observations	1,578	1,578	1,578	1,578	1,128	1,128

Table 4: **Who benefits most from communication?** In Columns (5) and (6), groups in which both Sender and Receiver are in the bottom half of competence are chosen as the base group. In hypothesis tests, R and r stand for Receiver in top and bottom half, respectively, and S and s stand for Sender in top and bottom half, respectively. Standard errors clustered by subject. Decision problem fixed effects included. Communication duration measured in minutes. For brevity, we report this table using demographic statistical controls only. Appendix B.3 replicates this table with the addition of financial, as well as financial and psychological controls.

Does communication change time preferences? Communication might not only change subjects' understandings of the consequences of each choice, but also how desirable they find these consequences. While, according to the non-paternalistic approach we take in this study, such a change is neither good nor bad, the findings are relevant for those taking the paternalistic view that people should become more or less patient. Hence, we study whether and how communication changes revealed time preferences.

Specifically, we study whether communication leads subjects to assimilate their discount factor towards their partner's. We estimate the following model

$$\bar{\delta}_2^i = \beta_0 + \beta_1 \bar{\delta}_1^i + \beta_2 \bar{\delta}_1^{-i} + \epsilon_{i,k} \tag{1}$$

where $\bar{\delta}_1^i$ and $\bar{\delta}_2^i$ are subject i's mean valuation of the simply framed future rewards in stages 1 and 2, respectively, and $\bar{\delta}_1^{-i}$ is i's partner's mean valuations of the simply framed future rewards in stage 1. We hypothesize that subjects' discount rate in stage 2 is a convex combination of their own and their partner's stage-1 discount rate, since communication might lead subjects to assimilate their discount rate towards their partner's from the starting point of their own stage 1 discount rate.

We estimate two versions of model (1). In the first, we impose no constraints. This allows us to test whether stage-2 discount rates are indeed a convex combination of own and partner's stage-1 discount rates. In the second, we force the coefficients β_1 and β_2 to sum to one, and can thus interpret β_2 as the percentage by which the Receiver's rate of time preference adjusts towards the Sender's. Because simply framed choices in stage 1 are a potentially noisy measure of subjects' discount factors, OLS-estimates of model (1) would suffer from attenuation bias. We address this by instrumenting both discussion partners' stage-1-discount rates with their respective stage-0 discount rates.[33]

Column 1 of Table 5 displays the results, separately for Receivers in the *Communication* and *Communication with Education* treatments. In both groups we find that Receivers' discount rates move by 12 to 14 percent towards their partners'. There is no significant difference in the effect sizes across the two treatments. Hence, to increase statistical power, we pool the data. Column 2 shows the corresponding estimates, and finds a 12.9 percent movement towards the partner's time preferences. Notably, estimates of this magnitude obtain regardless of whether we restrict the coefficients to sum to one. Indeed, we cannot reject the hypothesis that they do.

Equation (1) incorporates the assumption that all subjects' discount rates move the same percentage of the distance towards their partner's. In Column 3, we relax this restriction. We split the

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[33]Gillen et al. (2016) espouse a similar approach. Our instruments readily pass the weak instruments test. Shea's partial and adjusted partial R^2 coefficients exceed 0.5 for both own and partner's stage 1 discount rates.

sample into two groups, depending on whether the Sender a subject is paired with is in the more or less patient half of Senders in the decisions made in stage 0 of the experiment.

We find that Receivers assimilate to more patient Senders by more than twice the extent they do to the less patient Senders, as Column 3 shows. Hence, on average, discussing with others makes subjects more patient.[34]

It is worth noting that in this experiment we employ a money now / money later task. Such tasks have been criticized as a means of eliciting discount factors because they might invite considerations other than time preferences, such as the perceived probability of receiving payment (Halevy, 2014). For our experiment, this critique is of secondary relevance. Regardless of the interpretation of choices in the simply framed task, in our experiment communication changes subjects' behavior in a way that their assessment of receiving payment at a future date becomes more similar to the partner's, in particular if that person values money at a later date more highly.

4.1 Discussion

Our study pairs subjects randomly, in order to aid identification, whereas in non-experimental decisions, individuals are likely to seek the the help of others they are acquainted with, such as friends and family. We partially address this issue by studying how our results depend on whether subjects in our random pairs share demographic characteristics, and on how they perceive each other. In our setting, these variables exhibit only a minor influence.

Finally, because choices in the simply framed decisions systematically differ before and after discussion, it is not evident which simply framed choices should be used as the welfare-relevant benchmark (before discussion, after discussion, or contemporaneous). Indeed, there are arguments for each of them, and none can easily be excluded based on the notion of characterization failure. In the previous analyses we have used contemporaneous choices (i.e. we have used stage i simply framed choices to assess the welfare loss from stage i complexly framed choices). In Appendix D.1, we rigorously apply the Bernheim and Rangel (2009) approach. We explicitly account for the ambiguity in welfare-relevant choices, and replicate the entire analysis.

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[34]In our sample, the mean discount factor in stage 1 amongst Receivers is 0.863. The respective mean discount factors among the more and less patient halves of Senders are 0.993 and 0.744. According to the results in Column 3 (restricted estimates), the mean discount rate amongst Receivers in stage 2 should therefore equal 0.882. Given the statistical power we have in this experiment, this predicted effect size is too small to be detected by a comparison of means. Nonetheless, actual mean discount factor amongst Receivers in stage 2 is 0.875, and thus rather close to the predicted value.

VARIABLES	(1)	(2)	(3)
	Mean discount factor in stage 2		
Partner characteristic	Educ. intervention completed	All	More patient half of Senders
Unrestricted estimates			
Partner preferences	0.128***	0.120***	0.196***
	(0.031)	(0.043)	(0.047)
Own preferences	0.879***	0.855***	0.807***
	(0.031)	(0.045)	(0.051)
p value sum = 1	0.496	0.701	0.379
Restricted estimates			
Partner preferences	0.126***	0.132***	0.200***
	(0.030)	(0.030)	(0.044)
Partner characteristic	Watched documentary	-	Less patient half of Senders
Unrestricted estimates			
Partner preferences	0.147***	-	0.100***
	(0.031)		(0.029)
Own preferences	0.868***	-	0.910***
	(0.031)		(0.026)
p value sum = 1	0.117	-	0.802
Restricted estimates			
Partner preferences	0.139***	0.129***	0.088***
	(0.030)	(0.030)	(0.026)
p-values			
Influence equal acr. groups			
Unrestricted	0.593	-	0.043
Restricted	0.677	-	0.012
Observations	188	188	188
Subjects	188	188	188

Table 5: **(How) do subjects reevaluate their own preferences?** The second line displays the estimates of β_2 (the Senders' influence on the Receivers' preferences). These estimates are unrestricted. The second line from the bottom displays the estimates from the same regressions with the added restriction that $\beta_1 + \beta_2 = 1$ (that is the Receiver's current preferences are a convex combination of his own past preferences and the Sender's past preferences, up to an additive constant).

5 Conclusion

We have presented an experiment in which communication between randomly paired subjects about financial decisions does not lead to a case of the blind leading the blind, but to genuine improvements in the quality of decision making (measured by subjects' ability to choose as they would if they had a complete understanding of their opportunity set (Ambuehl et al., 2016)).

We have shown that the improvements reflect conceptual learning rather than mimicry of others who know better. The beneficial effects of communication are especially pronounced when relatively unsophisticated individuals interact. Skill transmission appears to work best between subjects at comparable levels of comprehension, perhaps because they address each others questions and concerns at a more appropriate level and pace. Subjecting one member of each pair to an effective financial education intervention, however, did not improve the untreated peer's decision making, except through pure mimicry. Finally, we have shown that discussing with others leads subjects to assimilate their time preferences to their partner's even in private decisions. This effect is particularly large for impatient subjects who are paired with a more patient partner.

Our findings suggest how the design and administration of interventions might be improved. They may harness the power of communication, and do so most effectively by having communication occur between people are on similar levels of understanding. Simultaneously, our findings caution against targeting interventions at influencers and relying on social diffusion. In particular, it may be ill-advised to provide rules of thumb that are appropriate only for particular segments of the population if the treated then share these rules with those for whom the rules are inappropriate, but who might be prone to mimicking without comprehension.

In this experiment we have studied a decision setting in which communication is helpful, rather than a case of the blind leading the blind. Confidence may play an important role. In our setting, confidence and actual competence are highly correlated. It is possible that when the two are less closely related, the effects of communication may work through confidence, rather than through actual ability, resulting in unproductive, or counterproductive communication, as Linnainmaa et al. (2016) document in the case of professional financial advisors. In further research, it will be interesting to find broader patterns that predict, for a given domain, whether one should expect communication to lead to a case of the blind leading the blind, or to be geuninely helpful.

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