Peer Effects and Risk-Taking Among Entrepreneurs: Lab-in-the-Field Evidence*

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February 19, 2018

Abstract

We study how social interactions influence entrepreneurs' risk-taking decisions. We conduct two risk-taking experiments with young Ugandan entrepreneurs. Between the two experiments, the entrepreneurs participate in a networking activity where they build relationships and discuss with each other. We collect data on peer network formation and on participants' choices before and after the networking activity. We find that participants tend to make more (less) risky choices in the second experiment if the peers they discuss with make on average more (less) risky choices in the first experiment. This suggests that even short term social interactions may affect risk-taking decisions. We also find that participants who make (in)consistent choices in the experiments tend to develop relationships with individuals who also make (in)consistent choices, even when controlling for observable variables such as education and gender, suggesting that peer networks are formed according to unobservable characteristics linked to cognitive ability.

Keywords: risk aversion, entrepreneur, peer effect

JEL classification: D03, D81, M13

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*We thank Charles Bellemare, Luc Bissonnette, Vincent Boucher, Bernard Fortin, and two anonymous referees for useful comments that greatly improved the quality of this paper. This study was carried out with financial and scientific support from the Partnership for Economic Policy (PEP) (www.pep-net.org) and funding from the Department for international Development of the UK Aid and the government of Canada through the International Development Research Centre. We are especially grateful to the team of PEP-researchers (PIERI-12451) lead by Juliet Ssekandi, who allowed us to join her evaluation project to collect experimental data. We also thank Benjamin Kachero and Samuel Galiwango for their extraordinary assistance in the field. We are grateful for the support provided by the PEP Research Director of Experimental Impact Evaluations Maria Laura Alzua, and by the PEP Scientific Advisor John Cockburn. This research benefited from collaborations with the Department for Children and Youth at the Ministry of Gender, Labor and Social Development (MGLSD), UNICEF-Uganda and Enterprise Uganda. We thank the Fonds de recherche du Québec - Société et culture and the Social Sciences and Humanities Research Council for our scholarships.

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1 Introduction

Risk plays a fundamental role in economic decision-making. For instance, evidence suggests that entrepreneurship is associated with a higher than average tolerance toward risk (Cramer et al., 2002; Ekelund et al., 2005; Ahn, 2010). Risk preferences may also affect businesses' success rates conditional on entry (Caliendo et al., 2010). But do individuals make risk-taking decisions solely according to their own risk preferences, or are there other important determinants of these choices? In this paper, we study the role of social interactions on risk-taking among groups of entrepreneurs. Using an original experimental design, we find a significant impact of conformity on risk-taking. Our findings suggest that even short-term social interactions are sufficient to affect entrepreneurs' risk-taking behaviors.

Entrepreneurs face more risk-taking decisions than paid employees in their daily life, which makes them a particularly interesting population to study the determinant of risk-taking. To focus on this population, we conducted lab-in-the-field experiments on risk-taking within workshops organized for young entrepreneurs in Uganda. Conducting these experiment in a developing country allows to incentivize participants with large amounts relatively to their income.[1] The workshops included a networking activity where entrepreneurs develop new relationships and converse with each other. We collected detailed information on who participants conversed with during this activity. The entrepreneurs also participated in two risk-taking experiments: one before and one after the networking activity. These two experiments are adaptations of the well-known Holt and Laury (2002) multiple choice lotteries designed to measure risk aversion. The two experiments, combined with data on the peer network formation, provide an innovative experimental design that allows us to capture the causal effect of social interactions on entrepreneurs' choices with respect to risk.

We find significant social conformity effects: Participants tend to make more (less) risky choices in the second experiment if their peers made on average more (less) risky choices in the first experiment. This suggests that social interactions may counterbalance individual risk preferences. Given some risk preferences, an entrepreneur could become more (less) inclined to take risk following a relatively short discussion with an

[1]For example, as we state latter in the paper, the highest possible payoff in one of our experiment is $10,000$ Ugandan shillings, which represents more than 16 hours of work at Uganda's 2012-13 median wage.

entrepreneur who is more (less) risk tolerant. In the second experiment, part of the participants were assigned to an experiment that included an ambiguity component (i.e. uncertainty on the exact probabilities linked to the lotteries' outcomes). As pointed out by Klibanoff et al. (2005), the uncertainty on the probabilities in the lotteries gives more room for subjective expectations to affect decisions. It is possible that social influence affect these subjective beliefs differently than attitude toward pure risk.[2] We also distinguish between preferences to conform with *successful* peers (who made the choice that led to the highest payoff given the lotteries' results) from preferences to conform with *unsuccessful* peers (who made the choice that led to the lowest payoff given the lotteries' results). Under pure risk, we find that participants tend to conform with successful peers, but not with unsuccessful ones. However, when the experiment includes an ambiguity component, we find that participants tend to conform with their peers regardless of the outcome.

Our design allows us to control for homophily, which is commonly a challenge in the estimation of peer effects. Homophily is the tendency of individuals to develop relationships with people similar to themselves. This behavior creates a correlation between one's peer variable (e.g. peers' average outcome) and his own choice even in the absence of peer effects, leading to identification issues. Attanasio et al. (2012) present evidence that individuals form social networks according to similarities in risk attitudes. However, in their context, as opposed to ours, individuals form networks with the objective of pooling risk. Thus, it is not necessarily the case that this behavior will also occur in our context. Nevertheless, individuals could still develop relationships according to some factors that also affect risk preferences. In other words, the peer network formation may be endogenous. There is a large and expanding literature that seeks to control for endogenous networks (for example, see Goldsmith-Pinkham and Imbens, 2013; Arduini et al., 2015; Qu and Lee, 2015; Boucher, 2016; Hsieh and Lee, 2016). However, controlling for endogeneity necessarily requires strong assumptions.[3]

[2] A paper investigating how risk attitudes may change with and without ambiguity is Cohn et al. (2015). They find that ambiguity causes no differences in how their treatment (showing participants a graph of stock market boom or crash) affects risk attitude. They interpret this finding as evidence that their treatment affects pure risk preferences, and not subjective expectations.

[3] For example, Goldsmith-Pinkham and Imbens (2013) assume that there exist two unobserved types of individuals and that those of the same type have a greater probability to become peers. Together with other distributional assumptions, this allows them to write the joint likelihood of the observed outcomes and peer network.

Our design allows us to identify peer effects in the presence of homophily under weaker assumptions. We use choices made in the two experiments to control for time-invariant individual characteristics through a first-difference approach. Assuming that individuals develop relationships based on these time-invariant characteristics is sufficient to rule out that the relationship between one's choice and those of her peers is caused by homophily. Furthermore, we can directly test for homophily effects. The choices made in our first experiment cannot possibly result from peer effects, because this experiment takes place before the networking activity. Therefore, the observed similarities between individuals' choices and those of the future peers they have not yet met can be used to identify homophily effects. We find no evidence of homophily according to characteristics that affect risk choices.

We also study the impact of social interactions on the consistency of individuals' choices. Indeed, in multiple choice lotteries experiments, some combinations of choices are inconsistent with standard risk preferences. We therefore test for homophily effects according to characteristics that affect the consistency of choices. We find that participants who make (in)consistent choices tend to develop relationships with individuals who also make (in)consistent choices. We finally test for social learning peer effects that would cause individuals to make more consistent choices if the peers they met made more consistent choices. We find no evidence of such social learning effects.

We contribute to the literature on the determinants of risk-taking, as well as the literature on peer effects and risk-taking. Firstly, there is a growing literature that suggests risk attitude vary across contexts (Barseghyan et al., 2011) and over time (Baucells and Villasís, 2010).[4] Understanding the factors that drive these variations is of particular importance to understand decisions about becoming an entrepreneur. Evidence suggests that family dynamics are important in shaping individuals' preferences toward entrepreneurship. Dunn and Holtz-Eakin (2000) find that parental entrepreneurial experience is a stronger predictor of entrepreneurship than individual or parental wealth. This correlation may result from both *nature* and *nurture* factors, but evidence suggests nurture factors play a larger role (Lindquist et al., 2015). The social context outside of the family can also shape individuals' attitudes toward risk and

[4]Risk attitude may also be affected by emotional states such as joviality, sadness, fear and anger (Conte et al., forthcoming), or by stress (Cahlíková and Cingl, 2017).

entrepreneurship, or their beliefs or confidence about the expected returns of starting a business. For instance, having entrepreneurial peers could create non-monetary benefits of running a business (Giannetti and Simonov, 2009). Nanda and Sørensen (2010) find that individuals are more likely to become entrepreneurs if they work with peers who have previously been entrepreneurs. They argue that past workers' experience may spill over to their coworkers by influencing their entrepreneurial skills, knowledge or motivation. Our paper explores the complementary idea that entrepreneurs' risk attitude may also spill over to others through peer effects. Secondly, our paper contributes to the expanding literature on peer effects on decisions made under risk. Bursztyn et al. (2014) study peer effects on the purchase of financial assets in a field experiment conducted at a financial brokerage. They find evidence of peer effects driven by both social learning (i.e. learning from peers) and social utility (i.e. utility that results directly from a peer's possession of an asset). Ahern et al. (2014) conduct an experiment about peer effects on risk aversion among MBA students and find significant peer effects. Gioia (2016) conducts a lab experiment and finds that the intensity of peer effects on risk-taking is determined in part by group identity: when peers are matched according to interest, the influence they exert on each other is greater. This suggests that peer effects might be important in our conftext, as our participants all share a common entrepreneurial identity. Our paper adds to these literatures by being the first (to our knowledge) to isolate the causal effect of interactions with peers on risk-taking decisions within a sample of entrepreneurs. Our paper further distinguishes itself in that it suggests that risk-taking can be influenced by peers in the very short run, following a networking activity a few hours only.

Another related paper is Lahno and Serra-Garcia (2015), who conduct a laboratory experiment to investigate whether participants' decisions about risk are influenced by their peers. They find that peer effects on risk-taking seem to be driven by a desire to conform with peers' choices. They argue that this implies that policymakers who seek to influence behaviors related to risk-taking (e.g. decisions to purchase insurance or acquire or repay debt) could publicly inform others about choices made by the population. This implication is particularly relevant for our paper, as we study real entrepreneurs. Our participants are people who need to finance their business projects with loans (this is discussed in detail in the next section). A policymaker could easily inform entrepreneurs about borrowing or insurance choices made by other entrepreneurs (for example, in an activity organized for them such as our workshops). He could also decide to make certain choices public in order to encourage specific behaviors (e.g. posting only the names of entrepreneurs who choose to insure their business). The policymaker could finally create networking activities aimed at discussing risk-taking decisions. These activities may generate social conformity effects that would push behaviors toward the average behavior, reducing excessive risk-taking and increasing risk tolerance for excessively risk averse individuals.

The next section describes our experimental design and data. Section 3 models participants' risk choices and presents the estimation of the social conformity effects. Section 4 models participants' consistency of choices and estimates social learning peer effects. Section 5 tests for homophily effect, and Section 6 concludes.

2 Experimental Design and Data

2.1 The Workshops

We contributed to the organization of six two-day workshops, along with the Partnership for Economic Policy,[5] a group of local researchers and UNICEF Uganda. The workshops took place in early 2014 in several locations in Uganda.[6] Their primary aim was to evaluate and improve financial literacy among young Ugandan entrepreneurs. The workshops included training in finance and business planning, as well as a networking activity where entrepreneurs could share their knowledge with each other. Within each workshop, we ran two experiments on risk-taking: one before and one after the networking activity.

Entrepreneurs were recruited using U-report, a free Short Message Service (SMS) platform created and managed by UNICEF to engage Ugandan youth into policymaking and governance. In 2014 the platform counted around 200,000 subscribers across Uganda.[7] The first contact was an SMS message asking, "Are you an entrepreneur

[5] www.pep-net.org.

[6] Four workshops took place in the districts of Wakiso, M'bale, Gulu and M'barara. The other two workshops took place in the capital city of Kampala.

[7] The average age was 24 years old and 23% were female. Interested readers can visit www.unicef.org/uganda/voy.html for more information about the U-report platform.

below 35 years old?"[8] If the answer was affirmative, a second SMS message was sent: "Would you be interested in obtaining a credit loan from the Youth Venture Capital Fund?" This question aimed at selecting only entrepreneurs who were considering a business loan. If the answer was affirmative again, the potential participant received a phone call from a recruiter. The recruiter asked whether the potential participant was available for a two-day workshop near his/her home. Interested individuals were invited to the workshop, and the potential participant either accepted or rejected the invitation.

In total, 540 entrepreneurs participated in one of the workshops. Upon arrival, participants completed a survey about their sociodemographic characteristics, registered using their full name and were attributed an identification number. All subjects then participated in an initial risk-taking experiment, which we describe in the next subsection. After this experiment, subjects proceeded to the networking activity, which included a lunch and a discussion time. All participants in a given workshop were in the same room for both the lunch and the discussion time, which together lasted three to four hours. We provided them no information on what would happen after the networking activity. They did not know they would play a second experiment at this time, so they had no incentive to seek information from their peers that would guide them in their choices for the second experiment. This strategy allows us to observe interactions occurring naturally without guidance from the experimenter. Throughout the activity, participants wore a tag indicating their full name and identification number. They had to write the name and identification number of at most seven participants with whom they had spent the most time chatting, thus allowing us to record their peer network. They also had to identify each relationship as either an extended family member, a friend from before the workshop, or a person they met at the workshop. Once all participants had registered this information, a random sample of half the participants in each workshop (258 in total) was chosen to participate in a second risk-taking experiment, also described in the next subsection.[9] The procedure of the random selection was to hide a label - either blue or red- inside each participant's tag prior to the workshop. After the networking activity, participants were asked to look at

[8] We sent a total of $2,278$ text messages in large cities.

[9] Participants who were not selected for the second experiment received training in finance and business planning that was also part of the workshop, but which we do not address in this paper.

their attributed color, and those with a given color had to play the second experiment. The first day of the workshop then ended, participants were paid the amount they had won in the experiments, and then returned home. The second day of the workshop included training in finance and business planning, which are outside the scope of this paper.

Two points are important to note. First, we provided no indication regarding what participants should discuss during the activity. They were completely free to discuss, or not to discuss, the experiment they had played. This makes the social interaction effects that we estimate latter in this paper more authentic: they occur naturally in a setting that resembles the real world. It is of course likely that some participants have not revealed any information that may affect their peers' choices for the second experiment. Thus, the peer effects we will present may understate the peer effects that would arise in a full information setting in which participants would precisely know choices made by their peers the first time. Second, although the targeted participants declared being interested in a credit loan, we believe it is unlikely that participants were concerned about potential effect of their choices on the loan. We, as well as the other organizers of the workshops, were not offering loans ourselves, and there was no link between us and the institutions that could granted this loan. Participants who would decide to get a loan would have to contact the institution of their choices by themselves.

2.2 The Risk-Taking Experiments

All subjects participate in the first risk-taking experiment, which takes place before the networking activity. The experiment is an adapted version of the well-known Holt and Laury (2002) experiment designed to measure risk preferences. It consists of nine games in which participants must choose between two lotteries: a safe lottery or a risky lottery, with the risky lottery having more variability between the potential payoffs. Each game is presented to participants in the form of a big transparent box containing 40 large white and black balls. The white balls represent low payoffs and black balls represent high payoffs. The proportion of black balls is low in the first game and increases in each subsequent game. Participants also receive a paper questionnaire that provides them with the exact proportion of the two colors in each box. Participants are told that there are no good or wrong answers so that they do not feel that the experimenter is monitoring them. They are finally told that after all decisions are made, only one box (only one of the nine games) will be selected at random, with one ball selected at random from inside that box. They will then be paid according to this ball's color and the choice they made in the corresponding game. Each within-workshop experiment is split in three or four sessions (depending on the number of participants in the workshop), and the lottery and ball that are selected are specific to each session. This is done to create variation in the amount won across participants conditional on choices made. Decisions are made individually and participants are not allowed to consult each other. Appendix B provides additional details about how the experiment is presented to participants.

Table 1 presents the two possible payoffs for each lottery. The amounts are substantial. For example, $10,000$ Ugandan shillings (UGX), the highest possible payoff, represents more than 16 hours of work at Uganda's 2012-13 median wage.[10]

Table 2 shows the probability that the high payoff ball is picked for each game. It is low in the first game and increases for each game, so that the incentive to choose the risky lottery increases in each game. The last column shows the difference in expected payoffs between choosing the safe lottery and choosing the risky lottery. The combination of choices made by an individual is informative of his preferences. For example, a risk-neutral individual should choose the safe lottery in games 1 to 4, and then switch to the risky lottery in games 5 to 9. Our main variable of interest — the number of safe choices – is the number of games in which the individual chooses the safe lottery. It ranges from 0 (all risky choices) to 9 (no risky choices). A risk-neutral individual should therefore make four safe choices, because he would choose the safe lottery from games 1 to 4.

In theory, a participant should not switch his choice more than once. That is, if a participant chooses the safe lottery in game k and the risky lottery in game $k+1$, it would be inconsistent to switch back to the safe lottery in game $k+2$. In practice, in

[10]The median monthly earning in Uganda was about 110,000 UGX in 2012-13 for a paid employee, with the average work week comprised of approximately 41 hours. Because a month comprises 4.35 weeks on average, the average hourly earning is about 617 UGX per hour (see page 12 of the Uganda National Household Survey of 2012-13 [UBOS, 2014]).

Table 1: Game payoffs (in UGX)

	Return	
	Low	High
Safe lottery	4,000	6,000
Risky lottery	1,000	10,000

Table 2: Probability of high payoff in each game

Game	Probability of high payoff	Expected payoff difference: safe - risky (in UGX)
1	1/10	2,300
2	2/10	1,600
3	3/10	900
4	4/10	200
5	5/10	-500
6	6/10	-1,200
7	7/10	-1,900
8	8/10	-2,600
9	9/10	-3,300

our experiment as in other studies, some participants do switch more than once.[11] This could be the result of a participant misunderstanding the experiment or having difficulty calculating the expected outcomes of each lottery. As pointed out by Andersen et al. (2006), it could also result from participants being indifferent between choices of lotteries, which requires preferences to be weakly convex rather than strictly convex. Still, in the following sections, we will refer to a second outcome of interest: the consistency of choices (i.e. consistent with strictly convex preferences), a dummy variable that equals one if the participant switches no more than once, and zero otherwise.

In the second experiment (after the networking activity), within each workshop, each participant is randomly assigned to one of two subgroups. This creates 12 subgroups in total. Some subgroups replay the original experiment. The other subgroups play three different versions of the experiment, where we introduce an ambiguity component. For these groups, in the second experiment, a small proportion of the balls are wrapped in opaque bags so that participants cannot see whether they are black or white. The proportion of balls of unknown color in the low, medium and high ambiguity groups are 5%, 10% and 15% respectively and remain fixed in all nine games.

[11] For example, see Holt and Laury (2002) and Jacobson and Petrie (2009).

Participants are not provided any information about the distribution of the colors of the hidden balls. As for the balls that are not hidden, the proportions of white and black balls remain as described in Table 2. Following Klibanoff et al. (2005), the uncertainty on the exact share of high payoff balls leaves more room for subjective expectations to affect decisions, so that social influence may affect more (or less) strongly these subjective beliefs than attitude toward pure risk. As we will see in Section 3, we will test whether there are any difference in peer effects when individuals face ambiguity. The lottery and ball that are selected for this second experiment are specific to each subgroup. Appendix B provides details on all the experiments.

2.3 Data

Table 3 summarizes the data collected from the sociodemographic questionnaire, peer network questionnaire and the two risk-taking experiments' results. The average number of safe choices in the first experiment is 4.61 and slightly increases to 4.81 in the second experiment. The standard deviation of the differences in participants' number of safe choices in the two experiments is 1.81. This indicates that the number of safe choices varies upward and downward between the two experiments, even though the aggregate change is relatively small. The proportion of participants who make consistent choices in the first experiment is 54% and increases to 69% in the second second experiment. This increase could, among other things, be the result of playing the game a second time or of social learning effects.

On average, participants identify 4.52 peers who they met at the workshop and 1.76 peers who they knew before the workshop. Although we do not distinguish between these two types of peers in our main results, Appendix A shows that the significance of the peer effects we estimate in Section 3 mainly results from interactions between peers who have met at the workshop, ruling out the concern of social interactions that could have occurred before the networking activity (this is discussed in Section 3.3).

Table 4 decomposes the averages of our two outcomes of interest, for both experiments, for each type of second experiment played. The first column presents the first experiment's outcomes for those who did not play a second experiment, while the other four columns present the outcomes for the two experiments for those who played a second experiment with no, low, medium or high level of ambiguity.[12] The average number of safe choices increases in the second experiment for all experiments with ambiguity, although there is no obvious trend relating to the level of ambiguity. As for the proportion of participants who made consistent choices, it increases in all experiments, with no clear trend regarding the level of ambiguity.

Table 5 presents the bounds of risk aversion parameters that are implied by the observed choices assuming a constant relative risk aversion (CRRA) utility (i.e. $U(x) = x^{1-r}/(1-r)$). This allows us to compare our results with Holt and Laury (2002) and the literature that followed. Note that CRRA utility is consistent with an individual's observed choices only if he made consistent choices (i.e. switched no more than once). Nevertheless, the table presents the proportion of each number of safe choices for all participants (both those who made consistent and inconsistent choices) and relate this number to risk aversion assuming the x safe choices are made for the x first games. The third column shows the proportion for all participants in the first experiment, while the fourth column shows this proportion only for participants in the second experiment who played a game without ambiguity. This is also done by Holt and Laury (2002), who argue that inconsistent choices may simply result from errors around the "true" switching point of the individual. We find that a high concentration of choices in the $r \in (-0.1, 0.56)$ range (50% in the fist experiment and 60% in the second). Figure 1 compares our cumulative distribution of CRRA risk aversion measures to those of Holt and Laury (2002). Since we offer substantial payoffs, we compare our results with their high payoff experiment. Note that it is not possible to bound upward the value of r for individuals who only made risky choices, so the cumulative distribution does not reach 100%. Our participants are clearly less risk averse than those of Holt and Laury (2002). This could be due to a sorting effect: risk tolerant individuals can be more prone to becoming an entrepreneur. We therefore also compare our results to those of the high payoff treatment of Bellemare and Shearer (2010), who conduct similar experiments on workers who face substantial income risk. Consistently with sorting, our results are closer to theirs. The cumulative distributions are near equal below a risk aversion of 0.2. Among individuals with higher risk aversion, our participants are

[12] Average outcome values may systematically differ even in the first experiment, since the ambiguity level varied across workshops. As the workshops were held in different cities, participants may tend to differ in their risk attitude across workshops.

Table 3: Summary statistics

	Mean	SD	Min.	Max.	Obs.
Number of safe choices (0 to 9)					
1st experiment	4.61	1.86	0	9	540
2nd experiment	4.83	1.91	0	9	258
Difference between 2nd and 1st	0.26	1.81	-6	7	258
Consistence of choices (0 or 1)					
1st experiment	0.54	0.50	0	1	540
2nd experiment	0.69	0.46	0	1	258
Difference between 2nd and 1st	0.16	0.56	-1	1	258
Experiments' payoffs (in UGX)					
1st experiment	5,025	3,193	1,000	10,000	540
2nd experiment	4,852	3,184	1,000	10,000	244
Number of peers					
Met at the workshop	4.52	2.35	0	7	540
Family, friends, other	1.76	2.15	0	7	540
Age	26.63	4.41	17	50	540
Male	0.82	0.38	0	1	540
Education level					
Primary	0.14	0.34	0	1	540
Secondary	0.30	0.46	0	1	540
Technical	0.30	0.46	0	1	540
University	0.26	0.44	0	1	540
City					
Kampala 1	0.17	0.37	0	1	540
Kampala 2	0.14	0.35	0	1	540
Wakiso	0.17	0.37	0	1	540
M'bale	0.19	0.39	0	1	540
Gulu	0.19	0.39	0	1	540
M'barara	0.15	0.35	0	1	540
Ambiguity level in 2nd exp.					
None	0.19	0.40	0	1	258
Low	0.33	0.47	0	1	258
Medium	0.30	0.46	0	1	258
High	0.17	0.38	0	1	258

slightly more risk averse than theirs.

After the second experiment, we asked participants to identify the main reason why they changed their choices between the two experiments (if they did change their choices). Table 6 presents the frequency of each possible answer among participants who reported having changed their choices. Almost 42% answered that the discussions they had with their peers during the networking activity had changed their mind. This suggests that participants discussed the experiment and choice strategies during the networking activity, even though we did not instruct them to. It also suggests that they influenced each others in these discussions.

Table 4: Summary statistics by type of 2nd experiment played

	None	**No amb.**	**Low**	**Med.**	**High**
		Mean value of outcome			
Number of safe choices (0 to 9)					
1st experiment	4.65	4.36	4.76	4.64	4.29
2nd experiment	-	4.38	5.06	5.00	4.60
Difference between 2nd and 1st	-	0.02	0.29	0.36	0.31
Consistence of choices (0 or 1)					
1st experiment	0.55	0.48	0.54	0.49	0.62
2nd experiment	-	0.74	0.65	0.71	0.69
Difference between 2nd and 1st	-	0.26	0.11	0.22	0.07
Number of observations	282	50	85	78	45

Table 5: Implied risk aversion parameter from a CRRA utility function (only experiments without ambiguity)

Number of safe choices	Range of relative risk aversion for $U(x) = x^{1-r}/(1-r)$	Proportion of choices First exp.	2nd exp. (no amb.)
0	$r < -1.68$	0.02	0.04
1	$-1.68 < r < -0.94$	0.03	0.02
2	$-0.94 < r < -0.47$	0.05	0.08
3	$-0.47 < r < -0.1$	0.14	0.10
4	$-0.10 < r < 0.23$	0.27	0.30
5	$0.23 < r < 0.56$	0.23	0.30
6	$0.56 < r < 0.89$	0.10	0.04
7	$0.89 < r < 1.29$	0.08	0.04
8	$1.29 < r < 1.85$	0.05	0.04
9	$1.85 < r$	0.03	0.04
Number of obs.		540	50

Figure 1: Comparison of CRRA risk aversion measures

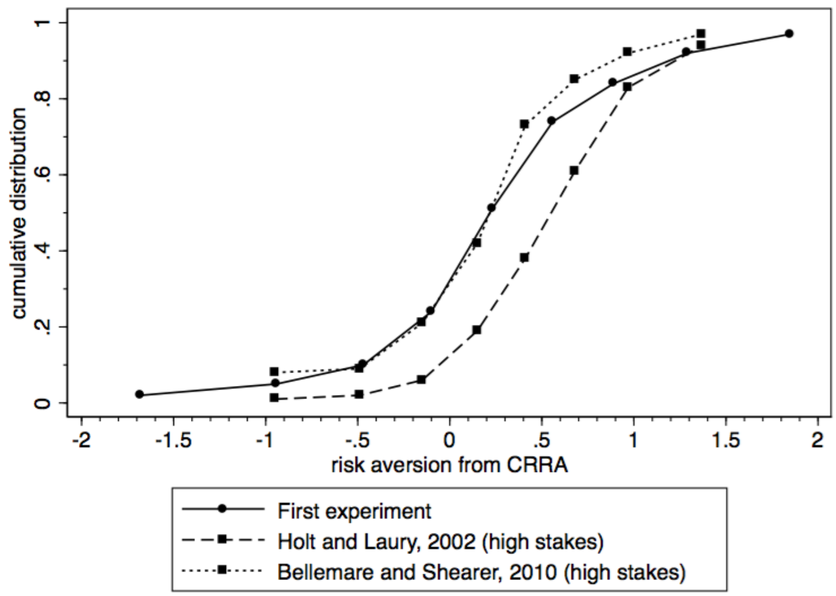

Table 6: Self-reported reasons for changing choices in the 2nd experiment

Why did you change any of your choices?	**Freq.**	**Percent**
I did not understand the first time	18	12.08
The game was different	49	32.89
Discussions with others changed my mind	62	41.61
I lost the first time	20	13.42
Total	149	100

3 Social Conformity and Risk-Taking Decisions

3.1 The Empirical Models

Participants' choices involve choosing between safe and risky lotteries. We therefore let the choice variable be y_{ir}, the number of safe choices individual i made in experiment $r \in \{1, 2\}$, where $r = 1$ is the first experiment (before the networking activity) and $r = 2$ is the second (after the networking activity). We model utility as a trade-off an individual faces: making choices according to his own characteristics and underlying preferences, or according to his or her peers' choices. As in Brock and Durlauf (2001b), Brock and Durlauf (2001a), Bisin et al. (2006) and Boucher (2016), we use a quadratic utility function to model this trade-off. The utility function penalizes the individual more if he chooses a value of y_{i1} that is further from his characteristics, as well as if he chooses a value that is further from average choices of his peers. In the first experiment, however, individuals do not face the trade-off because they do not know their peers' choices, so participants simply choose y_{i1} according to their own characteristics:

$$U_{i1}(y_{i1}) = -\frac{1}{2}(y_{i1} - \alpha_1 - \mathbf{x}_i\boldsymbol{\beta} - \eta_i - \epsilon_{i1})^2, \tag{1}$$

where $\mathbf{x}_i$ is a vector of individual i's observed characteristics. The parameters $\boldsymbol{\beta}$ and η_i are respectively the effect of the individual's observed and unobserved characteristics and may therefore capture the individual's idiosyncratic risk preferences. Thus, we allow for these preferences to be specific to the individual and to be a function of individual characteristics. This is consistent with the literature, which finds differences in risk preferences across individuals (for example, see Croson and Gneezy (2009), who find gender-based differences in risk preferences). Both $\mathbf{x}_i$ and η_i are constant over time (i.e. $\forall r \in \{1, 2\}$). The error term ϵ_{i1} is specific to i and to the first experiment. It allows for shocks, such as stress or other emotions, which might temporally affect choices under risk (see Cahlíková and Cingl (2017) and Conte et al. (forthcoming). The error term also acknowledges that we do not directly observe risk preferences, but rather an imperfect measure of it.[13] Thus, in the spirit of Baucells and Villasís (2010),

[13] Preference elicitation methods other than Holt and Laury (2002) lotteries could lead to different measures (Anderson and Mellor, 2009).

the number of safe choices y_{i1} could be the result of both risk preferences and a random error component. Note that this utility function does not intend to measure individuals' values of a structural parameter of risk preferences.[14] The first-order condition is:

$$y_{i1} = \alpha_1 + \mathbf{x}_i\boldsymbol{\beta} + \eta_i + \epsilon_{i1}. \tag{2}$$

In the second experiment ($r = 2$) after the networking activity, participants face a trade-off between staying true to their own characteristics and conforming with their peers' choices. We model social conformity using two specifications: homogeneous peer effects, where participants partly conform with the average behavior of their peers, and heterogeneous peer effects, where participants may conform differently with different peers according to the first experiment's results.

Before we present our modelization of peer effect, it is important to clarify what we mean by social conformity. Our model is designed to capture a tendency to make choices that are closer to peers' average choices. Although we refer to these peer effects as social conformity effects, we cannot completely rule out that they capture other types of peer effects than a pure preference to conform. For example, participants could have no preferences to conform, but still be influenced by peers' average choices through learning effects. Peer effects could also possibly arise from a taste for competition among peers. Still, throughout this section, we refer to the peer effects we find as social conformity effects because we believe this is the most convincing mechanism in our setting. In Section 4, we explore a separate social learning effect affecting the consistency of choices and find no significant effect. In our view, this makes social learning effects on risk preferences less convincing as well. Regarding potential competition effects, we estimate in Section 3.3 separate peer effects depending on the first experiment's results and argue that these estimations provide no evidence of competition effects. Nevertheless, it is worth keeping in mind that the effects we present in this section could possibly also capture a tendency to conform to peers' average choices resulting from social learning or competition.

[14]As seen in Section 2.3, using utility functions integrating these parameters such as a CRRA only allows to bound the parameters. This lack of point identification would greatly complexify the identification of additional social interaction parameters. In our view, our utility function is the simplest estimable empirical model that acknowledges that utility is a trade-off between the individuals' own characteristics and their peers' choices.

3.1.1 Homogeneous peer effects specification

We assume that individual i in the second experiment maximizes the following utility function:

$$U_{i2}(y_{i2}) = -\frac{1}{2}(y_{i2} - \alpha_1 - \alpha_2 - \alpha_2^g - \mathbf{x}_i\boldsymbol{\beta} - \delta W_i - \eta_i - \epsilon_{i2})^2 - \frac{\theta}{2}\left(y_{i2} - \frac{1}{n_i}\sum_{j \in N_i} y_{j1}\right)^2 \tag{3}$$

where n_i is i's number of peers and N_i is his set of peers. The first part on the right-hand side is the private component of the utility function and the second is its social component. Utility is decreasing with the distance between the individual's choice and the average choice of his peers. We allow for the possibility that playing the experiment a second time affects risk choices in some way through the parameter α_2. We also include α_2^g, a dummy variable specific to the ambiguity-level fixed effect $g \in \{none, low, medium, high\}$ (recall from the last section that participants in the second experiment are randomly assigned to games with different ambiguity levels). We thus allow for each of these four games to have a different effect on the utility that results from choices. We set the reference category to $g = none$ so that $\alpha_2^{none} = 0$. W_i is the individual's payoff from the first experiment (divided by 1,000), so that δ may capture wealth effects.[15] The parameter θ is the social conformity effect, modeled as a preference to conform with peers' average behavior. A value of θ of zero would imply that individuals are not affected by their peers' choices. A negative value would mean that utility increases with the distance between the individual's choice and the average choice of his peers (implying anti-conformity preferences). A value of $\theta = 1$ would mean that the individual attributes the same weight to the private component than to the social component of the utility function. Finally, a value of θ that would tend toward infinity would mean that the individual only cares about imitating his peers. Since any value of θ is theoretically plausible, we do not constraint its value in our estimations below. We allow this parameter to differ depending on whether the participant faces ambiguity or not, so that we have:

[15] The results we will present are robust to using the logarithm of the payoff instead, or to not controlling for the payoff.

$$\theta = \begin{cases} \theta_{na} & \text{if} \quad g = none, \\ \theta_a & \text{otherwise.} \end{cases} \tag{4}$$

Therefore, θ_{na} is the social conformity effect of participants who participate in the exact same experiment the second time, whereas θ_a is the social conformity effect for those who participate in one of the games that includes ambiguity.[16] Note that the time-invariant effects α_1, $\mathbf{x}_i\boldsymbol{\beta}$ and η_i enter the private component of the utility function in the same manner that in the first experiment. This implies we can use choices from the first experiment to control for these time-invariant effects. We do this by substituting equation (2) into equation (3), which yields:

$$U_{i2}(y_{i2}) = -\frac{1}{2}(y_{i2} - y_{i1} - \alpha_2 - \alpha_2^g - \delta W_i - \epsilon_i)^2 - \frac{\theta}{2}\left(y_{i2} - \frac{1}{n_i}\sum_{j \in N_i} y_{j1}\right)^2 \tag{5}$$

where $\epsilon_i \equiv \epsilon_{i2} - \epsilon_{i1}$. Note that this strategy of substituting the first experiment's first order condition in the above equation writes off $\mathbf{x}_i\boldsymbol{\beta}$ and η_i. This corresponds to using a first-difference approach in the private part of the utility function to control for the individual's fixed unobserved effect η_i. Thus, y_{i1} may capture the effect of the individual's risk preferences on choices.[17] Following this strategy, taking the first-order condition leads to the following estimable empirical model, in which the error term does not include the individual's fixed effect η_i:[18]

$$y_{i2} = \frac{1}{1+\theta}\left(\alpha_2 + \alpha_2^g + y_{i1} + \delta W_i + \frac{\theta}{n_i}\sum_{j \in N_i} y_{j1} + \epsilon_i\right). \tag{6}$$

[16] Separate peer effect estimates for all levels of ambiguity (*low*, *medium*, *high*) are available upon request. We do not find a systematic link between the magnitude of the peer effect and the ambiguity level, possibly because separate estimates are not enough precise.

[17] While the second experiment introduces ambiguity, it is in large part similar to the first one given the low fraction of the balls with unknown color (see Section 2.2). Participants' choices should therefore still be in large part linked to the choices they made in the first experiment.

[18] If the individual has no peers ($n_i = 0$), the utility function simplifies to $U_{i2}(y_{i2}) = -\frac{1}{2}(y_{i2} - y_{i1} - \alpha_2 - \alpha_2^g - \delta W_i - \epsilon_i)^2$ and the first-order condition becomes $y_{i2} = \alpha_2 + \alpha_2^g + y_{i1} + \delta W_i + \epsilon_i$. Only one individual in our sample did not report having any peers. As we will see below, we estimate the model using nonlinear least squares, which allows to estimate this individual's first-order condition jointly with those of other individuals. Furthermore, all the results we present are robust to removing this individual.

Equation (6) provides an empirical model we can estimate. Note that the model allows to separately identify the effect of playing a second time from conformity effects. This is because the conformity effects are attributed to variations in peer choices, which are specific to each individual, while the effect of playing a second time is common to all individual and is thus captured by the constant.[19] Importantly, the model also allows us to bypass usual empirical challenges in the estimation of peer effects. First, the peer variable ($\frac{1}{n_i}\sum_{j\in N_i} y_{j1}$) is predetermined, ruling out endogeneity issues and the reflection problem described by Manski (1993), which arises when the dependent variable and the peer variable are simultaneously determined. Second, the model implicitly controls for homophily (i.e. the tendency individuals have to develop relationships with people similar to themselves). Homophily is usually a concern in the estimation of peer effects. Individuals may match according to observable variables (e.g. gender, age, education), which is generally not a problem because these variables' effects can be controlled for. A more important concern is the possibility of homophily according to unobserved characteristics that might affect the variable of interest. In our model, this would mean that individuals with similar values of η_i would tend to become peers. This would imply a correlation between y_{ir} and the average outcome of i's peers even in the absence of peer effects. Fortunately, our first-difference approach in the private component of the utility function cancels out η_i in equation (5). Our identification strategy relies on the assumption that individuals do not choose peers based on their values on ϵ_{i1} and ϵ_{i2}. There may be homophily based on unobserved characteristics that affect risk choices in both experiments (η_i), but we assume the remaining error term ϵ_i is independent of peers' average outcome. Finally, note that the model implies a coefficient restriction because θ appears twice. It is possible to test this restriction by allowing the two parameters to differ and by testing their equality, which we do in Section 3.3.

[19]The constant α_2 could in principle also capture some form of peer effect: the effect of having met peers, regardless of these peers' choices. Our estimate of the social conformity peer effect θ is meant to exclude this effect, as social conformity is driven by comparison with peers' choices.

3.1.2 Heterogeneous peer effects specification

We now allow for heterogeneous peer effects between *successful* peers and *unsuccessful* peers. We define being successful (unsuccessful) as having made the choice that led to the highest (lowest) payoff given the game and the ball that were picked at random in the first experiment. Let N_i^s be the set of peers of i who were successful in the first experiment and N_i^u be the set of peers who were unsuccessful. Additionally, let n_i^s and n_i^u be the respective numbers of i's peers in these two groups (so $n_i = n_i^s + n_i^u$). Our model with heterogeneous peer effects becomes:

$$\begin{aligned} U_{i2}(y_{i2}) = &- \frac{1}{2}(y_{i2} - y_{i1} - \alpha_2 - \alpha_2^g - \delta W_i - \epsilon_i)^2 \\ &- \frac{\theta^s n_i^s}{2n_i}\left(y_{i2} - \frac{1}{n_i^s}\sum_{j\in N_i^s} y_{j1}\right)^2 - \frac{\theta^u n_i^u}{2n_i}\left(y_{i2} - \frac{1}{n_i^u}\sum_{j\in N_i^u} y_{j1}\right)^2, \end{aligned} \tag{7}$$

where θ^k is the social conformity effect for the peer group $k \in \{s, u\}$, modeled as a preference to conform with this group's average behavior. The relative importance of each group is weighted by the proportion of peers in each category n_i^k/n_i. The first-order condition is:

$$y_{i2} = \frac{n_i}{n_i + \theta^s n_i^s + \theta^u n_i^u}\left(\alpha_2 + \alpha_2^g + y_{i1} + \delta W_i + \frac{\theta^s}{n_i}\sum_{j\in N_i^s} y_{j1} + \frac{\theta^u}{n_i}\sum_{j\in N_i^u} y_{j1} + \epsilon_i\right), \tag{8}$$

which we use as an empirical model for estimation. The marginal effect of peers' average number of safe choices in the group k (i.e. $\frac{1}{n_i^k}\sum_{j\in N_i^k} y_{j1}$) is given by $\theta^k n_i^k/(n_i + \theta^s n_i^s + \theta^u n_i^u)$.[20] Notice that the marginal effect of *succesful* and *unsuccessful* peers' average number of safe choices (which we label as ME^s and ME^u) can be rewritten respectively as:

$$ME^s(p_i^s) = \frac{p_i^s \theta^s}{1 + \theta^s p_i^s + \theta^u(1 - p_i^s)} \qquad \text{and} \qquad ME^u(p_i^s) = \frac{(1 - p_i^s)\theta^u}{1 + \theta^s p_i^s + \theta^u(1 - p_i^s)}, \tag{9}$$

where p_i^s is the proportion of i's peers who where *succesful* in the first experiment. Therefore, these marginal effects vary across individuals according to their proportion

[20] To see this, first add (n_i^k/n_i^k) in front of the term $\frac{1}{n_i}\sum_{j\in N_i^k} y_{j1}$ in equation 8.

of peers belonging to each group. Also, the marginal effect of peers' average number of safe choices in one group decreases with the size of the peer effect of the opposite group. Equation 9 shows that the size and statistical significance of each of our estimates of ME^s and ME^u, which we present in the next subsection, will depend on the size and statistical significance of both peer effect estimates ($\hat{\theta}^s$ and $\hat{\theta}^u$).

Finally, note that the proportion of peers in each group could potentially be endogeneous. This would for instance be the case if participants were concerned with selecting the "right" peers in their peer group, for example to please the experimenter. We therefore test whether participants tend to systematically favour making successful peers. We compare (1) the average proportion of successful peers declared by participants who played the second experiment (i.e. $1/N \sum_{i=1}^{N} n_i^s/n_i$) to (2) the proportion of participants who were indeed successful in the first experiment. A t-test reveals that the two proportions are close (0.74 and 0.69, respectively) and not significantly different from each other. The test does not reject the null hypothesis that they are the same at a 10% significance level. Thus, we do not find convincing evidence that participants systematically choose successful peers in their network.

3.2 Estimation and Results

We estimate our two specifications (equations 6 and 8) using nonlinear least squares (NLS). NLS relies on the assumption that the expected value of the error term, conditional on explanatory and predetermined variables, is zero. Thus, it relies on weaker assumptions than other nonlinear methods, such as maximum likelihood estimation, that rely on distributional assumptions.[21] NLS minimizes the sum of the squares of the residuals, assuming the predicted value of y_{i2} is given by a function $g(\boldsymbol{\omega}, \mathbf{y}_{i1})$ where $\boldsymbol{\omega}$ is the vector of all parameters entering the model and $\mathbf{y}_{i1}$ is the vector of choices made by i's peers in the first experiment (i.e. containing all y_{j1} for which $j \in N_i$). NLS therefore choses the value of $\boldsymbol{\omega}$ that minimizes the objective function $\sum_{i=1}^{N}(y_i - g(\boldsymbol{\omega}, \mathbf{y}_{i1}))^2$, where

$$g(\boldsymbol{\omega}, \mathbf{y}_{i1}) = \frac{1}{1+\theta}\left(\alpha_2 + \alpha_2^g + y_{i1} + \delta W_i + \frac{\theta}{n_i}\sum_{j \in N_i} y_{j1}\right) \tag{10}$$

[21]See chapter 5 of Cameron and Trivedi (2005) for explanations on nonlinear estimators.

for our homogeneous specification and

$$g(\boldsymbol{\omega}, \mathbf{y}_{i1}) = \frac{n_i}{n_i + \theta^s n_i^s + \theta^u n_i^u} \left(\alpha_2 + \alpha_2^g + y_{i1} + \delta W_i + \frac{\theta^s}{n_i} \sum_{j \in N_i^s} y_{j1} + \frac{\theta^u}{n_i} \sum_{j \in N_i^u} y_{j1} \right) \tag{11}$$

for our heterogeneous specification. Table 7 presents the results for both specifications. We use the sandwich estimator of variance to calculate standard errors. Column (a) shows the estimates for the homogeneous peer effects specification. The peer effect θ_{na} (for those who participated in the same experiment the second time) is 0.783 and is significant at the 10 percent level. As discussed in Section 2.2, if peer effects affect more (or less) strongly participants' subjective expectations than attitudes toward pure risk, we would expect to find different estimates depending on whether or not participants played a game with ambiguity. Among those who played an experiment with ambiguity, we find a lower social conformity effect ($\hat{\theta}_a = 0.627$). This effect is more precisely estimated and significant, possibly because of the higher number of participants who played an experiment with ambiguity. A Wald test (not shown) does not reject the null hypothesis that the peer effects with and without ambiguity are the same for any reasonable level of significance. Thus, our results provide no evidence that peer effects arise more strongly by impacting subjective expectations. Column (a) of Table 8 presents the estimate of the marginal effect of peers' average number of safe choices, which equals $\hat{\theta}/(1+\hat{\theta})$. It equals 0.439 for individuals who played the same experiment the second time and 0.385 for those who played a experiment with ambiguity. We use the delta method to calculate their standard errors.[22] Both marginal effects are significant at a 1 percent level.

Column (b) of Table 7 presents the estimates of the peer effects from the heterogeneous specification. For those who participated in the same experiment (without ambiguity) the second time, we find that participants tend to conform with their peers who were successful the first time. Conversely, we find a negative but not statistically significant conformity effect from peers who were unsuccessful, and reject the null hypothesis that social conformity effects from successful and unsuccessful peers are equal.

[22] The estimate of the standard error of the marginal effect in the homogeneous specification equals $1/(1+\hat{\theta})^2 \hat{\sigma}_\theta$, where $\hat{\sigma}_\theta$ is the estimate of the standard error of $\hat{\theta}$.

On the contrary, for participants who played a different game with ambiguity in the second experiment, we find positive social conformity effects from the two peer groups and do not reject that the two are equal. Furthermore, a Wald test (not shown) rejects that the peer effects from *unsuccessful peer* with and without ambiguity are equal with a p-value of 0.012, suggesting that peer effects may arise differently in ambiguous environment. In the presence of ambiguity, individuals may simply conform with their peers' choices regardless of the outcome.

The marginal effects from both peer groups' average number of safe choices, which are given by equation 9 vary across individuals since they depend on the proportion of peers belonging to each group. Column (b) of Table 8 present the estimates of the average marginal effects from the average number of safe choices made by *successful* peers (AME^s) and by *unsuccessful* peers (AME^u). The standard errors are again calculated using the delta method.[23] For individuals who played a game without ambiguity, the average marginal effect from *successful* peers' average choices is 0.539 and is significant at a 5 percent level. The negative average marginal effect from *unsuccessful* peers' average choices is less important, in part because it pushed down by the positive and important peer effect from *successful* peers. For individuals who played an experiment with ambiguity, marginal effects are both positive and statistically significant. Overall, our findings suggest a significant impact of conformism on risk-taking decisions. We also find that having won a higher payoff in the first experiment tends to make individuals more willing to take risks.

3.3 Additional Tests and Estimations

As mentioned in Section 2.3, some participants already knew each other before the workshop. Thus, even though most peers met each other at the workshop for the first time (participants have on average 4.54 peers they met at the workshop and 1.76 peers they knew before), one may be worried that our results are largely driven by these few individuals, and that the peer effects we find might only occur among these. We test for this possibility by estimating an empirical model similar to our heterogeneous specification from equation (8), except that "successful" and "unsuccessful" types of

[23]From the delta method, the variance of $[\hat{AME}^s \ \ \hat{AME}^u]'$ equals JVJ', where V is the variance-covariance matrix of $[\hat{\theta}^s \ \ \hat{\theta}^u]'$, and J is the Jacobian matrix of $[\hat{AME}^s \ \ \hat{AME}^u]'$.

Table 7: Peer effects on the number of safe choices - Nonlinear least squares estimation

	Hom. effects (a)	Het. effects (b)
peer effect -	0.783*	
no ambiguity θ_{na}	(0.459)	
peer effect -	0.627***	
ambiguity θ_a	(0.184)	
peer effect (successful peers) -		1.207**
no ambiguity θ_{na}^s		(0.594)
peer effect (unsuccessful peers) -		-0.935
no ambiguity θ_{na}^u		(0.734)
peer effect (successful peers) -		0.387**
ambiguity θ_a^s		(0.164)
peer effect (unsuccessful peers) -		1.261**
ambiguity θ_a^u		(0.496)
second exp. effect α_2	1.122*	1.439**
	(0.594)	(0.602)
1st exp payoff effect δ	-0.200***	-0.223***
(in thousands of UGX)	(0.069)	(0.073)
p-value $H_0 : \theta_{na}^s = \theta_{na}^u$		0.05
p-value $H_0 : \theta_a^s = \theta_a^u$		0.09
Number of observations	258	258
Ambiguity fixed effects α_2^g	Yes	Yes

*** $p \leq 0.01$; ** $p \leq 0.05$; * $p \leq 0.1$

Table 8: Average marginal effects from the nonlinear least squares estimations

	Hom. effects (a)	Het. effects (b)
Avg. y_{j1} - no ambiguity	0.439*** (0.144)	
Avg. y_{j1} - ambiguity	0.385*** (0.070)	
Avg. y_{j1} (successful peers) - no ambiguity		0.539** (0.168)
Avg. y_{j1} (unsuccessful peers) - no ambiguity		-0.209 (1.146)
Avg. y_{j1} (successful peers) - ambiguity		0.154** (0.061)
Avg. y_{j1} (unsuccessful peers) - ambiguity		0.250*** (0.058)
number of observations	258	258

Standard errors are calculated using the delta method
*** $p \leq 0.01$; ** $p \leq 0.05$; * $p \leq 0.1$

peers are replaced by "pre-existing" and "new" types of peers. Table 11 from Appendix A presents separate peer effect estimates from these two types of peers. Column (a) is the homogeneous peer effects specification – exactly the same as column (a) from our main results presented in Table 7, whereas column (b) shows heterogeneous peer effects from "pre-existing" and "new" peers, where "pre-existing" peers refer to those the individual already knew before the workshop and "new" peers refers to those met at the workshop. The results show that the significance of our peer effect estimates is mostly driven by interactions that occurred among peers who met the first time at the workshops.

In order to explore the idea that some subsamples of participants can be more influenced than others, Table 12 in Appendix A provides additional estimations of our homogeneous peer effect specification (equation 6) made on subsamples of our participants. Columns (a) and (b) estimate the model only for unsuccessful and successful participants respectively. Potentially, participants who were unsuccessful could seek more information from their peers with the objective of being successful the second time. Also, if peer effects capture competition effects beside conformity, we would expect them to differ according to the results of the first experiment. Column (c) estimates the model only for those who stated that the discussion with other had changed their mind, and column (d) estimates it only on those who made consistent choices. Unfortunately, the smaller number of observations in these subsamples leads to imprecise estimates, especially for the effect of those who played a game without ambiguity. The estimate of the peer effects for those who played an experiment with ambiguity are still significant and in line with the estimate from our main specification. Note that, while the estimate of peer effect without ambiguity might seem high, it is not inconsistent with reasonable values. The resulting estimated marginal effect is 0.732, which is larger than the marginal effect from our main results (0.439) but not inconsistent. Overall, our results present no evidence that the peer effects we estimate are driven solely by some distinguishable subsample of our participants, or that they arise from competition effects.

Finally, as mentioned previously, the social conformity effect θ in our empirical model appears twice. It is therefore possible to test the implied coefficient restrictions. Allowing the two coefficients to differ changes our homogeneous and heterogeneous specifications respectively to

$$y_{i2} = \frac{1}{1+\theta^A}\left(\alpha_2 + \alpha_2^g + y_{i1} + \delta W_i + \frac{\theta^B}{n_i}\sum_{j\in N_i} y_{j1} + \epsilon_i\right) \tag{12}$$

and

$$y_{i2} = \frac{n_i}{n_i + \theta^{sA} n_i^s + \theta^{uA} n_i^u}\left(\alpha_2 + \alpha_2^g + y_{i1} + \delta W_i + \frac{\theta^{sB}}{n_i}\sum_{j\in N_i^s} y_{j1} + \frac{\theta^{uB}}{n_i}\sum_{j\in N_i^u} y_{j1} + \epsilon_i\right). \tag{13}$$

Table 13 in Appendix A presents the two NLS estimations, as well as the tests of coefficient restrictions implied by our main specifications (equations 6 and 8). While some restrictions are not rejected at any reasonable level of confidence, others are rejected. In the homogeneous specification, the restriction for the parameters with ambiguity is rejected, as is the restriction on the peer effects from successful peers with ambiguity in the heterogeneous specification. However, the estimate of the coefficients themselves are mostly consistent with our main results regarding their sign and amplitude. The estimates of the effects from successful peers without ambiguity are positive and high, while those from unsuccessful peers are negative and high. The sign and amplitude of the coefficients with ambiguity are also consistent for the peer effects from unsuccessful peers. The exception is the estimates of the coefficients of the peer effects from successful peers with ambiguity: one is negative and the other positive. The negative coefficient is however very imprecise relative to its size. Overall, while these tests formally reject some of our model's coefficient restrictions, we believe that the structure we impose helps uncovering more precise estimates of the peer effects.

4 Social Learning and Consistency of Choices

4.1 The Empirical Models

We now investigate whether participants learn from their peers who made consistent choices. We assume participants make some effort to understand how to make good choices. This implies a different model underlying participants' choices than the one described in Section 3. Let the latent variable e^*_{ir} be the effort that an individual i puts into understanding experiment $r \in \{1, 2\}$. Participants have to reach some minimal level of understanding, normalized to 0, to make consistent choices. This leads to the standard latent variable framework:

$$e_{ir} = \begin{cases} 1 & \text{if} \quad e^*_{ir} \geq 0, \\ 0 & \text{otherwise,} \end{cases} \tag{14}$$

where e_{ir} is the consistency of choices that results from putting enough effort into understanding the experiment. In the first experiment ($r = 1$), they choose the effort that maximizes the following utility:

$$V_{i1}(e^*_{i1}) = (c_1 + \mathbf{x}_i \boldsymbol{\gamma} + \mu_i + \psi_{i1}) e^*_{i1} - \frac{{e^*_{i1}}^2}{2}, \tag{15}$$

where $\mathbf{x}_i$ and μ_i are the individual's fixed observed and unobserved characteristics, respectively, and ψ_{i1} is an error term. The first portion of the right-hand side represents the individual's perceived benefit from exerting effort, while the second portion represents the increasing cost of effort. The perceived benefit of effort depends on individual characteristics. For example, a low-skill person (low $\mathbf{x}_i$ or μ_i) may not see why he should try to calculate anything, and instead prefer to pick lotteries at random.[24] The first-order condition is:

$$e^*_{i1} = c_1 + \mathbf{x}_i\boldsymbol{\gamma} + \mu_i + \psi_{i1}. \tag{16}$$

In the second experiment - after the networking activity - participants may now have learned from their peers who made consistent choices the first time. Let m_i be the number of i's peers who made consistent choices in the first experiment. In the second experiment, individual i chooses effort e^*_{i2} in order to maximize:

$$V_{i2}(e^*_{i2}) = (c_1 + c_2 + c_2^g + \mathbf{x}_i\boldsymbol{\gamma} + \mu_i + \epsilon_{i2})e^*_{i2} - \frac{{e^*_{i2}}^2}{2} + \lambda m_i e^*_{i2}, \tag{17}$$

where c_2 is a constant that adds to the first experiment's constant. It might (among other things) capture a learning effect of doing the experiment a second time or a fatigue effect. We again add ambiguity dummies c_2^g specific to the level of ambiguity $g \in \{none, low, medium, high\}$ in the second experiment. The reference category is set to $g = none$ so that $c_2^{none} = 0$. The individual's perceived utility is affected by his peers through social learning effects. The m_i peers who understood the experiment the first time may make it easier for i to understand the experiment because he can learn from them. We can see this as a reduction in the cost of effort needed to understand the experiment. As in the last section, we let the peer effect λ differ for those who participated in a treatment that included ambiguity the second time, so that:

$$\lambda = \begin{cases} \lambda_{na} & \text{if} \quad g = none, \\ \lambda_a & \text{otherwise.} \end{cases} \tag{18}$$

The first-order condition is:

$$e^*_{i2} = c_1 + c_2 + c_2^g + \mathbf{x}_i\boldsymbol{\gamma} + \lambda m_i + \mu_i + \epsilon_{i2}, \tag{19}$$

[24] Conversely, individual characteristics could be seen as affecting the cost of effort instead of its perceived benefit: a high-skill person may find it less costly to provide sufficient effort to understand the experiment.

which provides an empirical model we can estimate. Once again, the peer variable m_i is predetermined, which rules out the reflection problem of Manski (1993). It also rules out the multiple equilibriums problem that arises in binary outcome models where the dependent variable and the peer variables are simultaneously determined (Brock and Durlauf, 2001a).

4.1.1 Naive Specification

Contrary to Section 3, the latent variable framework implies we cannot use the first-difference approach to remove equation (19)'s time-invariant observed or unobserved variables. Thus, if there is homophily according to μ_i, m_i should be correlated with the error term. Nevertheless, as a benchmark, we first ignore homophily concerns and use equation (19) as our empirical model assuming $E(\mu_i + \epsilon_i | m_i, \mathbf{x}_i) = 0$.

4.1.2 Difference-in-Differences Specification

Homophily and peer effects may both create similarities in peers' choices in the second experiment. However, in the first experiment, only homophily may create these similarities. We can therefore use the choices in the first experiment to separately identify the two effects.

We use a specification analogous to a difference-in-differences (DID) estimation. In a standard DID setting, a control group and a treatment group are observed both before and after a treatment occurs. The variation in the outcome of interest that occurs between the two periods for reasons other than the treatment can be controlled for using the variation in this outcome among the control group. The additional variation that is specific to the treatment group is then attributed to the treatment effect.

In our setting, the number of peers who made consistent choices (m_i) is analogous to the DID treatment variable. As in a standard DID estimation, individuals with different values of m_i may on average have different levels of understanding about the experiment, even before social interactions occur, because of homophily. The variation in the outcome that occurs between our two experiments for reasons other than social interactions can also be controlled for using a dummy variable that equals 1 if $r = 2$ and 0 otherwise. The additional variation that arises in the the second experiment as a function of m_i can then be used to identify peer effects. Specifically, we estimate the following model:

$$e_{ir}^* = c_1 + \mathbf{x}_i\boldsymbol{\gamma} + \tilde{\lambda} m_i + \mathbb{1}(r=2)\left[c_2 + c_2^g + \lambda m_i\right] + \mu_i + \epsilon_{ir}, \tag{20}$$

where $\mathbb{1}(r=2)$ equals 1 if $r=2$ and 0 otherwise. The correlation between m_i and μ_i that comes from homophily is present in the two experiments and is thus captured by $\tilde{\lambda}$. Besides homophily effects, the estimate of $\tilde{\lambda}$ captures any relationship between μ_i and m_i that arises for reasons other than the social interactions occurring after the first experiment. Thus, λ excludes the effect of homophily and captures the peer effects, which only arise in the second experiment.

4.2 Estimation and Results

We estimate our two specifications using probit estimations. Table 9 presents the estimated average marginal effects. We include in $\mathbf{x}_i$ age, sex and education, as well as fixed effects for the six locations in which the experiments took place. Column (a) presents the naive specification (equation 19) and column (b) presents the DID specification (equation 20). The number of observations in column (b) is greater because we use the choices from the first experiment to control for homophily. The standard errors are clustered by individual, but the results are robust to using the sandwich estimator of variance without clustering.[25]

The naive peer effect estimates show a significant relationship between an individual's consistency of choices and his number of peers who made consistent choices in the first experiment. However, this relationship is significant only for participants who participated in an experiment without ambiguity the second time. The relationship may, however, include both a peer effects and a homophily effect.

Our DID estimation yields a significant homophily effect. An individual's probability of making consistent choices in the first experiment is 3.9 percentage points greater, on average, for each peer who made consistent choices, even if participants have not yet discussed with each other. The additional effect of the number of peers

[25] We avoid clustering by the six locations (on top of the locations' fixed effects), because clustering with too few clusters leads to a downward-biased variance matrix estimate, and thus to over-rejection. However, small cluster sizes may also lead to a biased estimate of the variance matrix. See Cameron and Miller (2015) for a discussion on problems that arise with few clusters or with small clusters.

Table 9: Peer effects on consistency of choices -
Average marginal effects of a probit estimation

	Naive (a)	DID (b)
peer effect - no ambiguity λ_{na}	0.113**	0.044
	(0.049)	(0.049)
peer effect - ambiguity λ_a	0.025	-0.020
	(0.023)	(0.026)
homophily effect $\tilde{\lambda}$		0.039***
		(0.014)
2nd exp. effect c_2		0.011
		(0.204)
observable characteristics		
age	-0.001	-0.004
	(0.006)	(0.004)
male	0.066	0.149***
	(0.076)	(0.048)
education: secondary	0.337***	0.173***
	(0.104)	(0.062)
education: technical	0.233**	0.170***
	(0.110)	(0.061)
education: university	0.324***	0.233***
	(0.106)	(0.062)
Number of observations	258	798
Number of individuals	258	258
Ambiguity fixed effects c_2^g	Yes	Yes
District fixed effects	Yes	Yes

*** $p \leq 0.01$; ** $p \leq 0.05$; * $p \leq 0.1$

who made consistent choices in the second experiment — the social learning effect of having met and discussed with these peers — is not significant. Therefore, we can see that neglecting the role of homophily would have led us to interpret the relationship between one's consistency of choices and those of her peers as peer effects.

One drawback of this estimation is that our estimate of the homophily effect cannot easily be interpreted as an effect on the probability of developing a relationship. The next section explores homophily in greater details.

5 Testing for homophily

As mentioned previously, our models from the last two sections both control for homophily. Nevertheless, because homophily is interesting in itself, we now explore it in greater details. Homophily according to observable characteristics can be tested for by looking at whether individuals tend to be peers with others who share these observable characteristics. Furthermore, because we observe behaviors before social interactions occur, we can also test for homophily on unobservable characteristics that affect the outcome. We do so by testing for correlations in outcomes between future peers who have not yet met. This correlation cannot possibly come from peer effects and should therefore be attributable to homophily.

Let the network tie d_{ij} be equal to 1 if individual i states that individual j is his new friend and 0 otherwise. We allow the network to be directed, meaning that d_{ij} is not necessarily equal to d_{ji}. As suggested by Bramoullé and Fortin (2010), we let the probability that $d_{ij} = 1$ depend on the absolute distance between i and j's variables (which capture homophily effects) and on both i and j's variables. We model individual i's decision to state that j is one of his friends by the following rule:

$$d_{ij}^* = \delta_0 + \mathbf{x}_i \boldsymbol{\delta}_1 + \mathbf{x}_j \boldsymbol{\delta}_2 + y_{i1}\delta_3 + y_{j1}\delta_4 + |\mathbf{x}_i - \mathbf{x}_j|\boldsymbol{\rho}_x + |y_{i1} - y_{j1}|\rho_y + \upsilon_{ij} \tag{21}$$

$$d_{ij} = \begin{cases} 1 & \text{if} \quad d_{ij}^* > 0, \\ 0 & \text{otherwise.} \end{cases} \tag{22}$$

We call $\boldsymbol{\rho}_x$ the vector of homophily according to observable characteristics effects and ρ_y the effect of homophily on unobservable characteristics (that affect y_{i1}). Importantly, the outcome variables (y_{ir} and y_{jr}) are those of the first experiment ($r = 1$) before social interactions occur, so that ρ_y may not capture peer effects. Depending on the specification, we let the outcome variable be our risk aversion measure or the consistency of choices. We also estimate a model that includes both variables.

We estimate this model using a probit estimation. Because this is a model of peer network formation, we remove observations where peers stated that they already knew each other before the workshop. It is important to note that this model has some limitations in explaining some features of the network formation. It assumes that the probability that i and j become peers is independent of other links formed in the network. Thus, this model may not explain clustering (i.e. the stylized fact that two individuals who share a peer in common have a higher probability of becoming peers with each other). One should consult Chandrasekhar (2016) for a review of econometric models that are more consistent with stylized facts. Nevertheless, this simple model allows us to test for the existence of homophily effects. Table 10 presents the average marginal effects for the three specifications: column (a) uses the consistency of choices as the outcome, column (b) rather uses our measure of risk aversion, and column (c) uses both. Regardless of the specification used, we find no evidence of homophily according to observable variables. We also do not find evidence of homophily according to unobserved characteristics that affect the number of safe choices, as shown in columns (b) and (c). However, consistently with our results from Section 4, we do find significant homophily effects according to unobserved characteristics that affect the consistency of choices, in both columns (a) and (c). Specifically, we find that the probability that individual i becomes peer with individual j is lower by 0.46 percentage points if one of them made consistent choices while the other did not. These findings suggest that participants develop relationships according to some characteristics linked to cognitive skill that are not easily observable.

6 Conclusion

In this paper, we combine information on the formation of a network of entrepreneurs with observations from a field experiment on choices under risk before and after social interactions occur. This design allows us to estimate social conformity effects while controlling for homophily. We find that entrepreneurs tend to conform with their peers' choices, which suggests that social interactions play a role in risk-taking decisions.

Interestingly, Herbst and Mas (2015) compare peer effects on workers' output estimated in the lab to those estimated in the field in a meta-analysis. They find that peer effects estimates in the lab generalize quantitatively. If their results also apply in the context of peer effects on risk-taking, our results imply that a policymaker could influence entrepreneurs' real life risk-related choices, such as decisions about loans or

Table 10: Average marginal effects of a probit estimate - dependent variable: friendship (friends who already knew each other before the workshop are excluded)

	(a)		(b)	(c)	
Absolute value of the difference between individual variables					
Consistency of choices	-0.0046	**		-0.0046	**
	(0.0020)			(0.0020)	
Number of safe choices			-0.0000	-0.0001	
			(0.0006)	(0.0006)	
Age	-0.0003		-0.0003	-0.0003	
	(0.0003)		(0.0003)	(0.0003)	
Male	0.0000		0.0000	0.0000	
	(0.0000)		(0.0000)	(0.0000)	
Education	-0.0000		-0.0000	-0.0000	
	(0.0006)		(0.0006)	(0.0006)	
Individual's variable					
Consistency of choices	0.0021			0.0022	
	(0.0020)			(0.0021)	
Number of safe choices			0.0008	0.0008	
			(0.0005)	(0.0005)	
Age	0.0002		0.0002	0.0002	
	(0.0002)		(0.0002)	(0.0002)	
Male	0.0000		0.0000	0.0000	
	(0.0000)		(0.0000)	(0.0000)	
Education	-0.0000		0.0000	-0.0000	
	(0.0006)		(0.0006)	(0.0006)	
Potential peer's variable					
Consistency of choices	-0.0020			-0.0020	
	(0.0020)			(0.0021)	
Number of safe choices			-0.0005	-0.0005	
			(0.0006)	(0.0006)	
Age	0.0002		0.0002	0.0002	
	(0.0002)		(0.0002)	(0.0002)	
Male	0.0000		0.0000	0.0000	
	(0.0000)		(0.0000)	(0.0000)	
Education	-0.0000		-0.0000	-0.0000	
	(0.0006)		(0.0006)	(0.0006)	
Number of obs.	47,664		47,664	47,664	

Notes:
1 - Dummy variables for the district in which the experiment took place are also included in the regression but are not shown.
2 - Standard errors are clustered by "two potential peers" identifiers.
*** $p \leq 0.01$; ** $p \leq 0.05$; * $p \leq 0.1$.

insurance, by making other entrepreneurs' choices public. He could also influence risk-taking behaviors by organizing networking activities aimed at discussing risk-taking decisions. Social conformity effects may push behaviors toward the average behavior, reducing excessive risk-taking behaviors and increasing risk tolerance for excessively risk averse individuals.

We also find that participants who make (in)consistent choices in the experiments tend to develop relationships with individuals who also make (in)consistent choices, even when controlling for observable variables such as education or gender, suggesting that peer networks are formed according to characteristics linked to cognitive ability, but not not easily observable. This has implications for researchers seeking to estimate peer effects when the network is potentially endogeneous: if the outcome of interest relates to cognitive skills (e.g. educational achievement), estimated peer effects on this outcome may capture homophily also, as does our naive specification of peer effects on the consistency of choices.

The social interactions and homophily behaviors captured in our experiment are authentic; we do not influence the network formation or the discussions participants have. Furthermore, the peer effects we estimate result from a three to four hour-long networking activity. Our finding that these few hours of free discussion time are enough to influence one's choices, at least in the short run, complement other findings in the literature that suggest that long-lasting social relationships play a role in shaping individuals' risk attitudes in the long run (e.g. Dohmen et al., 2012). Our results also raise the issue of the direction of the causal relationship between risk preferences and the decision to start a business. If individuals who start a business enter a social world of entrepreneurs who tend to have higher risk tolerances, entry into entrepreneurship might cause more risk-taking. Cramer et al. (2002) raise the possibility of reverse causality, finding a negative effect of risk aversion on entry into entrepreneurship but questioning the causality of the relationship. Brachert and Hyll (2014) find that entry into entrepreneurship is associated with an increased willingness to take risks and argue that this entry may cause a change in risk attitudes for several reasons; our evidence suggests that social interactions with other entrepreneurs could be one of these reasons.

A limitation of our study is that the results shed no light on how our estimated peer effects may perpetuate in the long run. While it is conceivable that the effect of a one-time-only social interaction may disappear in the long run, real life social interactions are often repeated daily, so the repeating peer effects may possibly shape long run risk-taking decisions. This is however far from obvious: the effect of social interactions could move individuals' choices away from their preferences only temporarily and may dissipate in the long run, as is the case with other behavioral effects (see Erev and Haruvy, 2013). We leave the question of whether or not repeated social interactions generate peer effects that perpetuate in the long run for future research.

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A Additional estimations

Table 11: Peer effects on the number of safe choices -
heterogeneous effects between pre-existing and new peers -
Nonlinear least squares estimation

	Hom. effects (a)	Het. effects (b)
peer effect -	0.783*	
no ambiguity θ_{na}	(0.459)	
peer effect -	0.627***	
ambiguity θ_a	(0.184)	
peer effect from pre-existing peers -		-0.109
no ambiguity θ_{na}^p		(0.317)
peer effect from new peers -		2.047*
no ambiguity θ_{na}^n		(1.118)
peer effect from pre-existing peers -		0.905*
ambiguity θ_a^p		(0.497)
peer effect from new peers -		0.575***
ambiguity θ_a^n		(0.200)
second exp. effect α_2	1.122*	1.565***
	(0.594)	(0.509)
1st exp payoff effect δ	-0.200***	-0.206***
(in thousands of UGX)	(0.069)	(0.064)
p-value $H_0 : \theta_{na}^p = \theta_{na}^n$		0.08
p-value $H_0 : \theta_a^p = \theta_a^n$		0.54
Number of observations	258	258
Ambiguity fixed effects α_2^g	Yes	Yes

*** $p \leq 0.01$; ** $p \leq 0.05$; * $p \leq 0.1$

Table 12: Peer effects on the number of safe choices (estimated on subsamples) - Nonlinear least squares estimation

	(a)	(b)	(c)	(d)
peer effect -	0.639	0.776	0.120	2.725*
no ambiguity θ_{na}	(1.082)	(0.577)	(0.208)	(1.532)
peer effect -	0.552*	0.489***	0.608*	0.529***
ambiguity θ_a	(0.326)	(0.188)	(0.326)	(0.198)
second exp. effect α_2	1.867	0.318	1.539**	-0.900
	(1.494)	(0.702)	(0.699)	(0.996)
1st exp payoff effect δ	-0.375*	-0.078	-0.268***	-0.154
(in thousands of UGX)	(0.199)	(0.072)	(0.092)	(0.101)
number of observations	70	188	62	136
Ambiguity fixed effects α_2^g	Yes	Yes	Yes	Yes

*** $p \leq 0.01$; ** $p \leq 0.05$; * $p \leq 0.1$

Columns (a) and (b): estimation only for unsuccessful and successful participants respectively. **Column (c)**: estimation only for participants who stated that discussion with others had changed their mind. **Column (d)**: estimation only for participants who made consistent choices in the first experiment.

Table 13: Test of coefficient restrictions - Nonlinear least squares estimation

	Hom. effects (a)	Het. effects (b)
peer effect -	0.172	
no ambiguity θ_{na}^{A}	(0.639)	
peer effect -	0.823**	
no ambiguity θ_{na}^{B}	(0.396)	
peer effect -	0.058	
ambiguity θ_{a}^{A}	(0.230)	
peer effect -	0.793***	
ambiguity θ_{a}^{B}	(0.180)	
peer effect (successful peers) -		0.657
no ambiguity θ_{na}^{sA}		(0.702)
peer effect (successful peers) -		1.329**
no ambiguity θ_{na}^{sB}		(0.528)
peer effect (successful peers) -		-0.263
ambiguity θ_{a}^{sA}		(0.263)
peer effect (successful peers) -		0.518***
ambiguity θ_{a}^{sB}		(0.193)
peer effect (unsuccessful peers) -		–1.383*
no ambiguity θ_{na}^{uA}		(0.775)
peer effect (unsuccessful peers) -		–1.546*
no ambiguity θ_{na}^{uB}		(0.872)
peer effect (unsuccessful peers) -		0.886*
ambiguity θ_{a}^{uA}		(0.491)
peer effect (unsuccessful peers) -		1.530***
ambiguity θ_{a}^{uB}		(0.463)
Tests of coefficient restrictions		
p-value $H_0 : \theta_{na}^{A} = \theta_{na}^{B}$	0.307	
p-value $H_0 : \theta_{a}^{A} = \theta_{a}^{B}$	0.004	
p-value $H_0 : \theta_{na}^{sA} = \theta_{na}^{sB}$		0.217
p-value $H_0 : \theta_{a}^{sA} = \theta_{a}^{sB}$		0.003
p-value $H_0 : \theta_{na}^{uA} = \theta_{na}^{uB}$		0.828
p-value $H_0 : \theta_{a}^{uA} = \theta_{a}^{uB}$		0.051
Number of observations	258	258

*** $p \leq 0.01$; ** $p \leq 0.05$; * $p \leq 0.1$

Note: Both estimations control for second experiment effect (α_2), first experiment payoff and ambiguity fixed effects (α_2^g).

B Details about the experiments

Upon arrival to the workshop, participants answered a questionnaire about their socio-demographic characteristics. They were then gathered in a room for the first experiment. An instructor explained the instructions and verified participants' comprehension by asking a series of questions. When he thought everyone understood, he took the box representing the first lottery and put it in front of the group. The box contained black balls (representing a high payoff) and white balls (representing a low payoff). He briefly explained again the composition of the box and asked participants to write down their first investment choice on a decision sheet. The online appendix provides the exact instructions provided to participants, as well as the decision sheet on which they had to write their choices. The boxes in these decision sheets indicate the exact proportion of each ball and their associated payoffs. When participants were done writing their choice, the instructor took the box representing the second lottery and briefly explained the composition of the box, before participants recorded their second choice of lottery. Then the instructor went on with the third lottery and onward. All choices were made individually and in silence. Once everyone had finished recording their choices, one of the nine lotteries was randomly chosen by drawing from a bag of balls numbered from 1 through 9. Then, a single ball was randomly drawn from the selected lottery and participants were payed according to the choice recorded on their decision sheet.

Approximately 50% of participants were then randomly chosen to participate in a second experiment. Selected participants were randomly divided into two groups, with each group participating in an experiment with a different level of ambiguity (including none, low, medium and high). Only two ambiguity treatments were conducted at each workshop. Table 14 shows the number of participants assigned to each ambiguity level at each workshop. Note that there are more participants assigned to the *low* and *medium* levels. This comes from a confusion that arose in the organization of one of the workshops. Specifically, the participants of the "Kampala 2" workshop should have been assigned with *none* and *high* levels of ambiguity, but were mistakenly assigned with *low* and *medium* instead. This, however, does not invalidate our results, as we control for these differences in ambiguity levels in our estimations.

Participants assigned to *none* participated in the same experiment as the first experiment. Those assigned to treatments with ambiguity were presented a box that contained, in addition to white and black balls, balls that were wrapped in opaque bags, so that their color was unknown. The decisions sheets for the low, medium and high ambiguity treatments, as well as the exact instructions that were read and provided in written form to participants, are presented in figures the online appendix.

Table 14: Assignment of participants to the second experiment

			Ambiguity level in second experiment				
District		1st exp. only	None	Low	Medium	High	Total
Kampala 1	Obs.	53	0	0	18	19	90
	%	59%	0%	0%	20%	21%	100%
Kampala 2	Obs.	44	0	18	15	0	77
	%	57%	0%	23%	19%	0%	100%
Wakiso	Obs.	46	0	24	21	0	91
	%	51%	0%	26%	23%	0%	100%
M'bale	Obs.	50	24	0	0	26	100
	%	50%	24%	0%	0%	26%	100%
Gulu	Obs.	50	26	27	0	0	103
	%	49%	25%	26%	0%	0%	100%
M'barara	Obs.	39	0	16	24	0	79
	%	49%	0%	20%	30%	0%	100%
Total	Obs.	282	50	85	78	45	540
	%	52%	9%	16%	14%	8%	100%