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Abstract

We study the effects of peers on risky decision making among adolescents in the age range of 13 to 15 years. In a field experiment, we randomly allocated school classes to two social interaction treatments. Students were allowed to discuss their choices with a natural peer – either a friend or a randomly selected classmate – before individually making choices in an incentivised lottery task. In the control group, adolescents made choices without being able to discuss them with a peer. In addition, we collected information on existing peer networks. This novel design allows us to separate two channels of peer influence, assortative matching on preferences and the effect of social interaction on choices. We find that friends and classmates are matched on socio-demographic characteristics but not on risk preferences. In contrast, social interaction strongly increases the similarity of teenagers' risky choices. A large fraction of peers align their choices perfectly.

JEL codes: D03, D80, G02, C91, D81

Keywords: peer effects; assortative matching; social interaction; risk and loss aversion

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1 Introduction

Individuals often interact with others when they make important economic choices. A large body of literature indicates that peer effects matter in decisions, ranging from academic performance to investment choices (Sacerdote, 2001; Falk and Ichino, 2006; Card and Giuliano, 2013; Bursztyn et al., 2014; Dahl et al., 2014; Beshears et al., 2015). However, clean identification of peer effects is notoriously difficult, even in controlled laboratory and field experiments. Moreover, knowledge about the mechanisms that produce peer effects is still limited. In this paper, we propose a new experimental strategy that allows us to study both assortative matching when peer groups are formed and the effect of social interaction when choices are made. While assortative matching reflects peer selectivity with respect to similar characteristics, social interaction effects may for example arise from the exchange of information relevant to the choice or through social utility considerations. These aspects have so far been studied in isolation.

Our experimental strategy combines two components: the random assignment of *existing* peer groups (henceforth, *natural peers*) to social interaction conditions and the collection of network information to establish the degree of assortative matching. We apply this experimental design to risky choices of adolescents which have long been thought to be particularly prone to peer effects (Arnett, 1992).[1] Our key findings are: First, natural peers (classmates and friends) are not matched with respect to risk preferences. Second, in line with Bursztyn et al. (2014)'s results for adults, social interaction has a large effect on adolescents' risky choices.

A fundamental challenge to the identification of peer effects is unobserved and endogenous selection into social relationships. Laboratory experiments on social interaction address this concern by randomly allocating subjects to peer groups. In previous experimental studies following this approach, social interaction usually takes place between random, anonymous peers (e.g. in Lahno and Serra-Garcia, 2015), or between participants with no common history (e.g. in Falk and Ichino, 2006). Manski (2000), among others, criticised this approach, pointing out "(...) the groups whose interactions are observed are formed artificially for the sake of the experiment. This raises obvious questions about the credibility of extrapolating findings from experimental settings to populations of interest." Carrell et al. (2013) present empirical evidence supporting this view. They show that social interaction patterns can vary substantially in spite of a random assignment process due to endogenous friendship formation after the assignment.

In this paper, we randomly allocate natural peers, i.e., peers who know each other previous to the experiment, into controlled social interaction conditions at the school class-level. Within the classroom, we allow for social interaction, i.e. communication between pairs of students. Hence, the first innovation of our design lies in the *random* exposure of natural, i.e. non-artificial peers that have formed stable relationships, to social interaction (as opposed to random matching of unacquainted subjects in the lab).

[1] Adolescence is a period of increasingly independent decision-making, in which the influence of (more risk averse) parents on choices weakens and the influence of same-age peers becomes stronger. Gardner and Steinberg (2005) argue that peer pressure in risk-taking is larger in adolescence than in adulthood. Psychologists suggest that "risk taking [in adolescence] is the product of competition between the socio-emotional and cognitive-control networks, and adolescence is a period in which the former abruptly becomes more assertive (i.e., at puberty) while the latter gains strength only gradually, over a longer period of time" (Steinberg, 2007). Hence, increased susceptibility to peer influences may arise from the heightened role of socio-emotional networks in the adolescent brain (see Galvan et al., 2006, 2007; Steinberg, 2008; Van Leijenhorst et al., 2010; Reyna et al., 2011). Card and Giuliano (2013) highlights social interaction effects in risky behaviours such as sexual initiation, smoking, marijuana use, and truancy (for similar results, see also Arnett (1992); Brown et al. (1986); McPhee (1996)).

While this strategy addresses the criticism of artificial peer groups, it raises new questions. If peers are known to each other before the experiment, could sorting into classes or assortative matching of friends lead us to overestimate the effect of social interaction? Non-experimental approaches often use data on *endogenous* peer relations to account for selective peer interaction. These studies face the challenge that "true social interaction effects are difficult to distinguish from unobserved background factors that are correlated across friends" (Card and Giuliano, 2013). A similar point is made by Manski (1993) and Moffitt (2001). This also applies to the natural peers in our experiment. Subjects are randomly exposed to peer interaction in our experiment, but they have been previously assigned to classrooms and have already formed friendships (or not). In consequence, unobserved background factors may have led to assortative matching of friends and classmates.

This concern is addressed by our second innovation: We combine the experimental data with information on existing peer relations in the control group that we collect from the same subjects. First, we analyse whether subjects match assortatively on risk preferences, our object of interest, which may lead to a strong positive correlation of choices in the control and treatment conditions.[2] Second, random exposure to social interaction in the treatment conditions allows us to distinguish between peer similarity that arises through assortative matching and the impact of social interaction in the choice situation.

The identification strategy that we propose is widely applicable beyond the classroom; it is suitable in any settings with existing peer groups of limited size, such as the workplace, a village or neighbourhood, or family networks. It can also be applied to study social or intertemporal preferences, and resulting behaviours such as redistribution preferences and voting behaviour in controlled lab or field environments. For example, Sutter et al. (2010) emphasize the importance and heterogeneity in adolescents' social preferences, while Lührmann et al. (2016) analyse heterogeneity in adolescents' intertemporal choices. The advantage of the experimental setup is that it simulates real-world peer interaction and helps understand whether peer influence arises from the set of peers an individual has formed ties with, or from the social interaction dynamics between peers.

Our design has the following additional features. We conduct a between-subjects experiment with bilateral, simultaneous peer interaction. The between-subjects design avoids experimenter demand effects that appear when choices are repeated under different interaction conditions (see Zizzo (2010) for a detailed discussion of such demand effects). We use a simultaneous setting where peers discuss the task, as opposed to the more frequently used sequential design with a first and a second mover, effectively closing down feedback between peers (see, e.g. Bursztyn et al. (2014); Harbaugh et al. (2002)). The latter design allows identification of who influences whom in a peer group, and thus addresses the reflection problem (Manski, 1993). In a sequential design, the second mover faces a simple binary choice — whether to follow the first mover. In our simultaneous setting, peers interact in a natural way, i.e. with feedback, so that we can study not only differences in choice *similarity*, but also in the *degree* of risk (and loss) taken in a social interaction situation.

We conduct the experiment among adolescents in the age range of 13 to 15 years that attend the lower

[2] Assortative matching on demographic and socio-economic characteristics has been widely documented for many peer relationships, but matching on preferences has received little attention.

tier of the German high school system.[3] Adolescents in this age group are close to graduation and about to face many important (financial) decisions involving risk. In this crucial period, they decide whether to continue education, engage in vocational training or directly start their working career.

We randomly allocated 12 classes from 5 schools to three controlled social interaction conditions: In control classes, teenagers made choices in the absence of social interaction, i.e. they choose alone and without discussing the task with a peer. In the two treatments conditions, students first communicate with a natural peer; afterwards, they make individual choices regarding risk and loss. Choices are made using lists of lotteries with pure and mixed prospects, as in Tanaka et al. (2010). Risks are perfectly correlated across individuals to avoid strategic hedging; a single draw determines the state of nature and chosen lottery for the entire class.

In the two treatment conditions, we vary the degree of peer closeness in the stable environment of the classroom; in the first treatment arm, teenagers are randomly paired with a classmate; in the second treatment arm, teenagers pair with a friend. To sum up, our design creates random variation in *whether* and *with whom* subjects can interact in a choice situation involving risk and potential loss.

The following three key findings emerge. First, we find no evidence of assortative matching on risk preferences, neither on risk nor loss aversion. As expected, however, friends (and classmates) match on observable characteristics such as age and gender (as well as on family background characteristics). Overall, we find large variation in risk preferences among natural peers (i.e. friends' and classmates).

Our second finding is that peers strongly influence each other's risky decisions when they are allowed to interact before making choices. Students exposed to peer communication display a strong choice similarity in both treatment arms – i.e. regardless of whether they interact with friends or random classmates –, and in both types of lotteries – those with positive and mixed payoffs, compared to students in the control group. For a metric of how much choices differ between experimental conditions, we use prospect theory to estimate risk and loss aversion parameters in all conditions, treating choices *as if* they were taken without peer influence.[4] We find that communication reduces the difference in risk and in loss aversion within pairs by about 53% respectively 61%. Increased similarity mainly arises from perfect alignment of lottery choices. 59% of those paired with random classmates choose the same switchpoints in lotteries with positive payoffs, and 67% make identical choices in mixed lotteries. Qualitatively similar effects are found when *friends* are allowed to discuss their choices before making decisions – similarity in risk aversion more than doubles (the within-pair difference drops by -53%), and differences in loss aversion decrease even more strongly, by 61%. Again, this is mainly due to a higher fraction of identical choices.

Third, a large majority of adolescents are risk *and* loss averse in all treatment conditions. This is important in the context of the literature on adolescents' risk choices, as previous studies have focused on excessive risk taking but have neglected teenagers' reactions to loss. Social interaction with a random classmate results in more loss aversion.

In summary, our data allow us to draw important conclusions on the nature of peer influences. We find that even in the stable peer network of the classroom and also among friends, a large heterogeneity in

[3] About 53% of the secondary school age population in Berlin attend this school type (Statistisches Bundesamt, 2016).

[4] We discuss the interpretation of the estimated parameters in the different experimental conditions in Section 3.4. Additionally, we present all results in terms of adolescents choices of switching-points.

risk preferences prevails. This lack of assortative matching on risk preferences has implications for the design of interventions aimed at leveraging peer effects to improve adolescents' outcomes, or reducing the adverse effects of peer groups. Assigning individuals to peer groups is unlikely to yield effects through aligning preferences. This view is consistent with findings by Carrell et al. (2013) who show that peer group manipulation may be undone by subsequent selective subgroup formation.

The remainder of the paper proceeds as follows. In Section 2, we discuss the relevance of our experimental design and findings with respect to the existing literature. The experimental design is discussed in the context of our application in Section 3. We present the results from our experiment in Section 4. We discuss the implications of our findings in the concluding Section 5.

2 Relation to the literature

Our study contributes to a body of literature that documents how typical patterns of risky choices develop during childhood and adolescence (Reyna and Ellis, 1994; Harbaugh et al., 2002; Levin and Hart, 2003; Levin et al., 2007). Most find that risk aversion increases with age after adolescence in both experimental and self-reported survey measures (see, e.g. Gardner and Steinberg (2005); Dohmen et al. (2011)). We find that teenagers in our sample share one common feature: a large majority are risk *and* loss averse, i.e. display $\sigma < 1$ and $\lambda > 1$ (Kahneman and Tversky, 1979; Tversky and Kahneman, 1992). 59.4% of adolescents in the control group are risk averse, and the large majority of 79.7% are loss averse. We compare the risky choices of our sample of teenagers from families with relatively low socio-economic status (SES) with those of undergraduate students at a top-ranked university in Germany using the same choice task. Teenagers are significantly less risk and loss averse than undergraduate students. Our results are consistent with previous results regarding risk aversion among teenagers, and present new results on their reaction to downside risks. Glätzle-Rützler et al. (2015) focus on but find no evidence of *myopic* loss aversion in individual decision of youth between age 11 and 18.

Even among teenagers, only a small fraction is risk loving or risk neutral, so that the source of their risk taking is still open to debate. Recent studies by Alan et al. (2017) and Zumbühl et al. (2013) emphasise the importance of intergenerational transmission of risk preferences as the source of heterogeneity in risk preferences. Tymula et al. (2012) suggest that lower levels of ambiguity aversion in adolescents lead to higher risk taking.[5] We explore the impact of peer interaction on adolescents' risky choices in a school-based field experiment.

In a study of risky choices among adults, Bursztyn et al. (2014) consider peer effects among friends and family members regarding the choice of a risky investment product. They experimentally restrict the nature of their social interaction, but abstract from the endogenous nature of the pre-existing peer relations they study. We bridge the gap between experimental studies that often rely on artificial peers, and non-experimental approaches who face the challenge of endogenous natural peer groups. We introduce controlled experimental variation in social interaction among randomly paired individuals and collect data

[5] In parallel, recent evidence from neuroscience shows that brain maturity is reached later than previously thought, i.e. in early adulthood up to age 25, so risk taking in ambiguous situations may provide important learning opportunities and thus foster neurodevelopment (Steinberg, 2007).

on networks among natural peers. We separate assortative matching from social interaction effects and thus add to the scarce evidence on assortative matching based on risk preferences. Attanasio et al. (2012) find evidence of assortative matching on risk preferences, but in a group lending context where sorting into groups by risk preferences is economically beneficial (for risk pooling arrangements).[6]

In a recent study, Ahern et al. (2014) showed in a sample of MBA students that peer effects lead to a convergence of risk attitudes over the course of one academic year. We find different results.[7] In our study, individuals in the control group do not interact during the risk task, so we use their choices to obtain an estimate of assortative matching on (or realised convergence of) risk preferences. We use a dyadic approach similar to Fafchamps and Gubert (2007) with symmetric undirected matches to model matches among friends or classmates. We find no evidence of assortative matching on risk preferences – neither on risk nor loss aversion. Unsurprisingly, classmates (and friends) do match on observable socio-demographic characteristics such as age and gender (and family background characteristics). Shaner (2015) provides similar evidence of at best weak assortative matching on time preferences among spouses, i.e. a correlation of 0.09 for spousal discount factors.

Our findings of a social interaction effect towards increased similarity in risk aversion (but not in loss aversion) accords with the results of previous studies among adults. Several underlying mechanisms have been suggested. Bursztyn et al. (2014) designed an experiment that distinguishes between two of the underlying mechanisms, social preferences and social learning. Social preferences ("Keeping up with the Jones") arise when one's utility from making a risky choice depends directly on another individual's choices.[8] Relatedly, Fafchamps et al. (2015) find that in a repeated game, subjects try to keep up with the winners.[9] Lahno and Serra-Garcia (2015) provide evidence consistent with conformity as a reason for increased choice similarity. It arises from anchoring effects of peers' choices to which individuals conform due to relative payoff concerns (Festinger, 1954). Bursztyn et al. (2014) describe the second channel, social learning, as a situation where the individual interprets her peer's intended choice as a quality signal about the investment product. Dahl et al. (2014) also find strong dynamics in social learning among peers. They emphasise its multiplier effect by documenting snowballing spillovers across individuals in the context of a paid paternity leave scheme.

The risky choices in our study are closest to the risky investment choice in Bursztyn et al. (2014). The authors use a design in which information exchange is unidirectional. In their experiment, second mover adults who receive information about their (first mover) friends' investment choice are significantly more likely to buy the same asset. Investment take-up increases from 42% to 71% (respectively 93%), yielding effect sizes that are similar to those found in our study: close peers choose similar lotteries, and a large fraction makes identical choices. Social learning effects in the Bursztyn et al. (2014) study are greatest when the first (second) investor is financially sophisticated (financially unsophisticated). We investigate social learning in a simultaneous setting, and exploit the fact that we obtain two measurements of financial

[6]Lahno et al. (2015) find in a sample of villagers from Uganda that differences in individual risk attitudes are larger among peers in interpersonal conflict, in particular among kin.

[7]One reason for the differences in the results may be that in Ahern et al. (2014), peer influences may interact with learning on the course, as financial risk is one of the major components of the MBA curriculum.

[8]In Bursztyn et al. (2014), social utility is identified through learning about one's peer's actual (rather than intended) investment, as first movers make investment choices but receipt of the chosen investment product is randomised.

[9]See Trautmann and Vieider (2012) for a discussion of social utility aspects regarding behaviour under uncertainty.

competency, one that is publicly known (the math grade) and one that is not (the financial literacy score we measure). We find that randomly paired classmates with a high value of the measure that is not publicly known ('private') seem less prone to peer influence, i.e. less likely to coordinate their choices. Peer differences predict the likelihood of coordination only if they are visible publicly, while we find no such effect for peer differences in the private measure. These differential effects are suggestive of social learning effects between peers.[10]

Previous studies have also documented increased choice similarity (among adults) between the first and second mover, but they have been silent about the impact of peer interaction on the nature (or level) of risk taking.[11] In our simultaneous design with multiple choices, we can examine whether risk and loss aversion change when adolescents makes choices after social interaction. The effects we find depend on the type of peer: communication with a less close peer leads to a *higher* degree of choice alignment and results in *more* loss averse (and weakly more risk averse) choices. The percentage of loss averse choices among randomly paired, interacting classmates is 92.6% and significantly higher than when teenagers decide alone (79.7%). In contrast, we do not find evidence of increasingly loss averse choices in social interaction with friends (76.2% loss averse choices).

Finally, we re-examine Carrell et al.'s hypothesis that the nature of the peer relation matters (Carrell et al., 2013) and find evidence in its favour. We experimentally vary the degree of peer closeness between randomly paired classmates, and self-selected friends into discussion groups of two. Social learning appears to be an important driver of peer effects among classmates, but not among friends – leading to mixed results regarding peer-induced risk and loss taking.

3 Experiment

We conducted the experiment in 7th and 8th grade classes in lower tier schools in Berlin in June 2014.[12] First, students were given an incentivised experimental task involving lottery choices. Second, they completed individual questionnaires on cognitive ability, and socio-demographic characteristics.[13]

3.1 Experimental setup and task design

We used a between-subjects design to avoid experimenter demand effects that may arise in a within-subject design from repeating the same lottery choices under different conditions. Before the visits, we randomly allocated each class to the control group or one of the two treatment arms. In control classes

[10]Bursztyn et al. (2014)'s field experiment is designed to isolate two channels: social learning and social utility. We consider assortative matching as a third channel.

[11]For the analysis of the mechanisms through which peer effects arise, sequential designs which allow the experimenter to control the feedback mechanism, and the peer signals that the second-mover receives, are important. They are also useful to understand who influences whom as they address the reflection problem. However, to understand the nature of individual risky choices that result from peer interaction, a simultaneous design seem more appropriate, as it replicates the natural interaction between peers, and allows for dynamic feedback.

[12] These schools belong to the lower track of the German high school system whose students have on average lower socio-economic status (Dustmann, 2004). Graduation from this track is typically followed by vocational training.

[13]All participating schools were part of a larger survey on financial literacy among high school students that was conducted in June and July 2014 throughout Germany. Therefore, the questionnaire also contains information on financial knowledge and teenagers' finances that are not used in this paper. A subsample of 4 classes in 3 schools received three sessions of a financial literacy training several months prior to our visit. All results are robust to including dummies for the financial literacy training as an additional variable in the regressions.

(short: *CONTROL*), students completed the task on their own without any communication among students. In the treatments, *RANDOM* and *FRIENDS*, students were allowed to discuss their choices with one partner who was seated next to them. After this pre-play discussion, they made individual choices. In classes in the *RANDOM* condition, students drew a random number at the beginning of the session. Classmates with the same number were seated together. In the *FRIENDS* condition, we asked students to self-select a friend.[14]

The experimental task consisted of 18 lottery choices on two separate decision sheets (A and B) which were adapted from Tanaka et al. (2010). Each of the eleven choices on sheet A was between two lotteries with positive payoffs. The first lottery option on the left hand side of the sheet (L) is the same across choices on sheet A, while we increase the high payoff in the second lottery (R) across choices. The 7 choices on sheet B are mixed lotteries, i.e. including gains and losses, with the first lottery's (L) expected value deteriorating from row to row (choices 1, 2, 3, 5), or the second lottery (R) improving from row to row (choices 4, 6, 7). All choices involved 50:50 lotteries. This design enables us to assign parameters of prospect theory utility (cf. Tversky and Kahneman, 1992). Due to timing[15] and complexity constraints, we abstracted from probability weighting.[16] The two decision sheets (A and B) can be found in appendix A.7. The order of the sheets was randomised within each class. In the treatment groups, we randomised across pairs such that interacting students received identically ordered decision sheets.

3.2 Procedures

In this section, we describe the procedures used to implement lottery choices, match peers, and identify friends in this anonymised study. All procedures were designed to ensure participation and trust in the experimenter.

Instructions and experimenters: In each session, the instructions for the lottery task were read out aloud in front of the class before the incentivised task started. In all classed, the instructions were given by the same experimenter (one of the authors), supported by an aide. Sessions lasted one lesson, of which the instructions and the lottery task usually took roughly 15 minutes each. The instructions can be found in appendix A.6.

Transaction costs and participation constraint: Students received an initial endowment of €3.10 to ensure an overall positive payoff regardless of their choices, so that the participation condition is satisfied and to ensure trust. The endowment was placed in front of each student on his desk at the beginning of the session. Trust was further enhanced by teachers being present in the class during the experiment and witnessing payment announcements and payouts.

[14] We framed this as pairing up with a friend by stating: "Choose a friend with whom you would like to discuss your choices and sit together in pairs. You are allowed to discuss your decisions with your partner quietly, but you decide only for yourself!" Following common practice for partner tasks in German high schools, students formed groups simultaneously. 18 students ended up with no partner (due to uneven class size or lack of parental consent of either partner) and were excluded from the analysis.

[15] Experimental sessions had to be conducted within a 45-minute school lesson.

[16] Typically, probability weighting functions are inversely S shaped and intersect unweighted probability around $p = \frac{1}{3}$ (Wakker, 2010). For several estimates of probability weighting functions (e.g. Tversky and Kahneman, 1992; Camerer and Ho, 1994; Wu and Gonzalez, 1996) deviations of $p = 0.5$ from its decision weight are relatively small. Identifying probability weighting would have required participants to fill out at least one more decision sheet (Tanaka et al., 2010) and a considerably more complicated and time consuming mechanism for determining lottery realizations. Thus, we decided against obtaining such a measure in this study.

Task comprehension: Before participants proceeded with the incentivised task, they had to answer four test questions which can be found at the beginning of appendix A.7. The experimenters checked each participant's responses and allowed only students with correct answers, i.e., those who understood the task, to proceed to making their choices. If a participant had made a mistake, the experimenter would explain the calculation of payoffs again. The lottery task was completed using pen and paper.
Payment method and incentivisation: Upon task completion, the experimenters collected students' responses. Once the task was completed by all, one participant was selected to draw one random card, thereby determining which of the 18 choices would be paid out. Another participant was selected to draw one of 10 colored chips (5 red and 5 blue) from a bag to determine whether the high or the low payoff had been selected for payout.[17] High and low payoffs were colour-coded in blue and red on the sheets to connect students' lottery choices to the draw, and probabilities were visualised as 5 red and 5 blue chips at the top of each sheet. Both chance draws were applied to all students in the class, so that risks were perfectly correlated across students to minimise the scope for mutual insurance. All payments were made in class at the end of the session. Participants earned on average €4.10 with a standard deviation of €1.81.
Identification of peers: Each student received a booklet of lottery task sheets, and a survey questionnaire. Both carried the same unique ID number per student. An adhesive sticker with the same ID number was attached to the booklet's front page. The pairing in *RANDOM* was documented through the exchange of these stickers between assigned partners. Pairs were formed before the the task began. In *FRIENDS*, we follow the same strategy except that students self-select their friend as a partner, and exchange stickers. Similarly, we collect network information in the *CONTROL* classes. Students in these classes identified their closest friend in the classroom through a sticker exchange. Like in the friends treatment, students had to mutually agree to the exchange. School and class information is recorded by storing all surveys from one session, i.e. one class, in the same labeled envelope.

3.3 Sample

Our sample consists of 235 teenagers from 12 classes in 5 schools. All participants included in the sample provided parental consent to participate in the study. 17 participants made inconsistent choices.[18] 5 participants made incomplete choices. 18 participants could not be matched with a partner.[19] Exluding these subjects leaves us with 198 observations for the statistical analysis.
Individual characteristics are balanced across experimental conditions, as shown by the χ^2 tests presented in Table 1, supporting the validity of our randomization.[20] They include the school grade (7th or 8th), age (in months), gender, and numeracy (measured as the last math grade relative to the average class grade). We also include a measure of cognitive ability (or fluid intelligence), which we measure using four of Raven's progressive matrices (Raven, 1989). These are chosen to reflect the variance in cognitive

[17]For example, if B1 and a red chip were drawn, and the participant had chosen lottery L, the payoff was €-0.40.
[18]12 teenagers on decision sheet A, 7 on decision sheet B, two of these on both.
[19]Reasons for failure to match are: i) odd class size; ii) the partner did not fill out (all) choices, iii) behaved inconsistently or iv) failed to hand in a consent form (We allowed students to hand in their consent forms late but in these cases, earnings remained with the teachers until the consent form was handed in).
[20]A similar table for the whole sample can be found in appendix A.1.

ability among German teenagers following Heller et al. (1998).

Finally, we obtain socio-demographics and family background variables such as the log of household size, a dummy capturing migrant background, and the number of books at home.[21] Since we randomised at the class level, we cluster standard errors accordingly (Moulton, 1986).

Table 1: Descriptive Statistics

	TOTAL	CONTROL	RANDOM	FRIENDS	p-value
students	198	64	54	80	
classes	12	4	3	5	
schools	5	3	3	4	
Grade	7.768	7.750	7.741	7.800	0.670
Age	14.105	13.935	14.114	14.238	0.151
Male	0.595	0.594	0.558	0.620	0.775
Math grade (rel.)	0.024	-0.031	0.116	0.009	0.747
Financial literacy	7.480	7.344	7.093	7.850	0.359
Cognition score	0.788	0.672	0.759	0.900	0.400
Low cognition	0.823	0.859	0.870	0.762	0.180
Single parent	0.301	0.379	0.319	0.231	0.166
Household size	3.931	3.855	4.061	3.909	0.288
Migrant background	0.628	0.694	0.542	0.629	0.263
Number of books in hh	2.505	2.705	2.688	2.227	0.207
< 25 books at home	0.449	0.391	0.389	0.537	0.122
Contact intensity	2.505	2.828	2.229	2.408	0.012**
Frequent contact	0.414	0.578	0.296	0.362	0.004***

Note: Math grade (rel.) is math score subtracted by the class mean; cognition score corresponds to the number of correct Raven matrices out of 4; low cognition is a dummy variable indicating 0 or 1 correct responses in the Raven matrices; migrant background is a dummy taking value 1 if student checked box "other language spoken at home"; frequent contact is a dummy taking the value 1 if the respondent replied to have contact with the partner at least once per week; p-values are from χ^2 tests of sample balancedness; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

At the end of the survey, pairs (those in the treatment conditions as well as participants who exchanged stickers in the control group) were asked about the frequency with which they meet up after school.[22] As intended in our design, the degree of contact is higher among those who were asked to identify a friend as peer (in *CONTROL* and *FRIENDS*) than those in *RANDOM* who stated their degree of contact with a randomly allocated classmate. We find that the friends pairs in CONTROL and FRIENDS report a weakly significantly higher contact intensity than the classmate pairs in RANDOM (MWU test; $p - value = 0.0696$).

3.4 Assignment of loss and risk aversion parameters

We determine students' individual utility parameters assuming a prospect theory value function of the form

$$v(x) = \left\{ \begin{array}{cc} x^\sigma & \text{for } x \geq 0 \\ -\lambda(-x)^\sigma & \text{for } x < 0 \end{array} \right\}$$

[21]This variable is also used in PISA surveys as a proxy of important family inputs into education. See Hanushek and Woessmann (2011).

[22]The phrasing of the question can be found in appendix A.8.

where σ is the concavity parameter measuring the degree of risk aversion and λ denotes the loss aversion parameter, i.e., the kink of the value function at payoffs of 0. We insert each payoff x into the value function $v(x)$, and weight it with the objective probability of 0.5. An individual's switching point in choices over lotteries with positive payoffs (sheet A) determines a range for her risk aversion parameter σ. For example, when person i switched in row 4 from option L to option R, this is consistent with any $\sigma_i \in [0.8, 1]$. We assign the midpoint of the range as σ_i, e.g. in the previous example $\sigma_i = 0.9$.[23] Later switches to the second lottery imply lower σ, i.e., higher risk aversion.

Then, we assign λ from participant's choices on list B conditional on their σ_i. For example, if a participant switched in row 4 on sheet A and was assigned $\sigma_i = 0.9$, and switched to option R in row 4 on sheet B, she is assigned the midpoint $\lambda_i = 2.03$ of the respective range of $\lambda \in [1.70, 2.35]$. As a consequence there are $11 \times 7 = 77$ possible combinations of (σ_i, λ_i) in our dataset.[24] Later switches to option R on sheet B imply higher loss aversion λ.

In the control group, σ and λ represent preference parameters. In the treatment conditions, peer influence may lead to a mismatch between choices and implied preference parameters that we would falsely interpret as a difference in preferences. Therefore, we present all results in terms of the direct choices, i.e. switching points. To obtain a metric of the magnitude of social interaction effects, we also present the results in terms of estimated σ and λ (or differences thereof), i.e. treating choices *as if* they were taken without peer influence.

4 Results

4.1 Descriptive results

We first focus on risk and loss preferences in the control group in which adolescents take individual and independent decisions without peer interaction (see Table 2). We apply prospect theory as described in Section 3.4 to derive preference parameters from observed choices.[25] We find that adolescents are, on average, risk and loss averse (Kahneman and Tversky, 1979; Tversky and Kahneman, 1992), i.e., they are characterised by $\sigma < 1$ and $\lambda > 1$ ($p < 0.01$ in one-sided tests; see last column). We find a mean risk aversion parameter $\hat{\sigma}$ of 0.828, and mean loss aversion $\hat{\lambda}$ of 2.579.[26] Looking across the distribution of parameters, we find that 59.4% of teenagers in the control group are risk averse, and the large majority of 79.7% are loss averse.

The prevalence of risk aversion among adults and adolescents has been established in previous studies (Glätzle-Rützler et al., 2015; Tanaka et al., 2010; Tymula et al., 2012; von Gaudecker et al., 2011). Fewer studies provide evidence on the degree of loss aversion, and none of them investigate adolescents (Abdellaoui et al., 2008; Etchart-Vincent and L'Haridon, 2011). An exception is Glätzle-Rützler et al.

[23] If a participant always chose option L or option R, there is no bound for σ from below and above. In this case we calculate the "midpoint" using the same interval width as in the neighboring interval. That is, for a person i who always chose option L, we assign $\sigma_i = 0.05$, and for person j who always chose option R, we assign $\sigma_j = 1.625$.

[24] Appendix A.2 lists all choices and their associated σ parameter values, and an exemplary table for assigning λ parameter values.

[25] For a summary of teenagers' choices, see Table 13 in appendix A.3.

[26] We find adolescents to be risk and loss averse also in the full sample. Average parameter values are $\bar{\sigma} = 0.77$ and $\bar{\lambda} = 2.86$. We reject the hypothesis that individuals are not risk-averse (loss-averse) at $p < 0.01$ (one-sided tests).

Table 2: Distribution of estimated preference parameters in the absence of social interaction (CONTROL)

	mean	median	5th	25th	75th	95th	% risk- (loss-) averse	p-value[a]
σ	0.828	0.650	0.150	0.500	1.125	1.625	0.594	0.004***
λ	2.579	1.985	0.458	1.548	2.499	7.490	0.797	0.000***

Note: [a]: p-values from t-tests of H_0: $\sigma \geq 1$ or $\lambda \leq 1$ respectively; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3: Mean estimated risk parameters in the social interaction conditions

				p-values		
	CONTROL	RANDOM	FRIENDS	RAN vs CON	FRI vs CON	RAN vs FRI
σ	0.828	0.761	0.727	0.245	0.350	0.681
λ	2.579	3.407	2.724	0.039**	0.906	0.021**
% risk-averse	0.594	0.630	0.700	0.692	0.185	0.397
% loss-averse	0.797	0.926	0.762	0.048**	0.623	0.014**

Note: Full sample included; p-values are from Mann-Whitney U tests; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

(2015) who study (and do not find evidence for) *myopic* loss aversion, i.e., a combination of loss aversion and mental accounting, among adolescents.[27] To our knowledge, this study is the first to provide evidence for adolescents. Two-thirds of adolescents are loss-averse; the average degree of loss aversion ($\bar{\lambda} = 2.579$) is similar to previous estimates for adults, and for university students who were given a nearly identical choice task. For more information on risk preference estimates in other studies or populations, please see appendix A.4.

Due to our simultaneous choice design, we can examine the impact of social interaction on the *level* of risk- and loss taking, using the 11 choices between lottery pairs. This would not be possible in a (one-shot) sequential peer design which poses only a take-it-or-leave-it offer to the second mover. Since the choice independence assumption does not hold under peer interaction, we henceforth report the estimated parameters as a metric to characterise choices rather than deep preference parameters. Additionally, we report choices directly in terms of the switching points in the lotteries with positive (A) and mixed payoffs (B). All results hold when we look at raw choices.

Table 3 describes adolescents' risky choices in the social interaction conditions, and tests for differences in choices in comparison to the control group. We find that teenagers make less risky choices in both treatments as evidenced by the lower σ parameters and the higher percentage of adolescents that make risk-averse choices in these groups (see columns labeled *RANDOM* and *FRIENDS*). However, the difference to the control group is not statistically significantly different from zero (see last three columns for p-values from Mann-Whitney U tests). Choices display higher mean λ in both treatments (and a higher proportion of loss averse choices in RANDOM). The difference in *lambda* is large and statistically significant under social interaction with randomly matched classmates (*RANDOM*; MWU test, $p = 0.021$), but not statistically significant in the *FRIENDS* group. The loss aversion parameter is higher in each of the reported percentiles of the distribution in *RANDOM* than in the control group (see Table 2 above and

[27] They choose a design which does not allow the identification of λ.

Table 4, Kolmogorov-Smirnov test, $p = 0.084$), and leads to a higher percentage of loss averse adolescents – over 90%, which is an increase of 12.9 percentage points or 16.2%.

Table 4: Distribution of estimated risk parameters in the social interaction conditions

	mean	median	5th	25th	75th	95th	% risk- (loss-) averse	p-value^a
Interaction with a randomly matched classmate (RANDOM)								
σ	0.761	0.600	0.150	0.350	1.125	1.625	0.630	0.001***
λ	3.407	2.094	0.879	1.875	3.592	10.517	0.926	0.000***
Interaction with a friend (FRIEND)								
σ	0.727	0.650	0.100	0.550	0.900	1.625	0.700	0.000***
λ	2.724	1.981	0.791	1.541	2.975	10.371	0.762	0.000***

Note: ^{a}: p-values from t-tests of H_0: $\sigma \geq 1$ or $\lambda \leq 1$ respectively; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

In the following, we consider two channels through which peers may influence risky choices: first, we test whether friends and classmates sort or match assortatively on their risk and loss preferences (Section 4.2). Second, we separate assortative matching (that influenced peer group formation) from the experimentally induced social interaction between peers during the risk choice task. We discuss outcomes in terms of similarity of lottery choices between peers in Section 4.3. In Section 4.4, we provide additional evidence on the mechanisms behind the social interaction effects, and discuss extensions of our experiment.

4.2 Assortative matching on risk and loss preferences

We first test for assortative matching – that is, we use the control group data to test whether lottery choices are similar among peers even though subjects made them independently from each other. Two mechanisms may lead to similarity in preferences: First, adolescents likely exhibit assortative matching in their choice of friends. Similarly, there may be selection in the sorting of adolescents into classes (due to geographic proximity, family background, and so forth), either by parental or school influence on class assignment (see, inter alia, Jackson and Rogers (2007) for evidence on social sorting processes). Secondly, attending the same class or friendship may result in convergence in risk preferences over time (Ahern et al., 2014). The objective of this paper is not to differentiate betweeen friends' assortative matching (or selective sorting into classes) and subsequent convergence in preferences as relations deepen. Rather, our aim is to separate the effect of direct social interaction in the choice situation (as introduced in the treatment conditions) from the peer sorting processes which took place before our experiment and which affect all experimental conditions. In the remainder of the paper, we hence use the term "assortative matching" to characterise the cumulative effect of the matching, sorting and convergence between peers that take place before the experiment.

Based on previous evidence for young adults, we expect preferences among peers to have converged over the course of their joint school time.[28] In consequence, we expect stronger similarity in preferences among friends. Additionally, friends may match assortatively, potentially including matching on risk and loss preferences.

[28] Ahern et al. (2014) find convergence in risk aversion among MBA students which overall increases average risk aversion over the course of one study year. McPhee (1996) showed that adolescents are more likely to exert peer pressure on friends than on acquaintances.

We base our test on the dyadic regression approach outlined in Fafchamps and Gubert (2007) (see also Arcand and Fafchamps (2011); Attanasio et al. (2012)). This regression model uses each possible pair of subjects in a sample – in our case, the control group – as an observation. The dependent variables are binary indicators for whether the subjects that form the pair are (i) friends or (ii) classmates. The explanatory variables are constructed using the values of the estimated risk parameters, σ and λ of the subjects that form the pair. As individual and family background characteristics of adolescents are likely determinants of school choice, class assignment and the formation of friendships, we additionally control for these variables (z and y, respectively). We also include class fixed effects, μ.

In the control group, where no peer interaction took place, dyads are defined as follows. We elicit (mutual) friends in the control group using the same procedure (the exchange of stickers with ID numbers) as in the *FRIENDS* treatment. Since friendship pairs are formed jointly by exchanging stickers, they are by construction undirected, so the matches m_{ij} and m_{ji} are symmetric. Classmate dyads are naturally formed as class membership is known.

Let $m_{ij} = 1$ if two subjects (i) declare their friendship or (ii) are classmates, and zero otherwise. Since $m_{ij} = m_{ji}$, we collapse identical pairs and adapt a symmetric dyadic approach. We specify the following model:

$$\begin{aligned} m_{ij} = \beta_0 &+ \beta_1|\sigma_i - \sigma_j| + \delta_1|\lambda_i - \lambda_j| + \eta_1|z_i - z_j| + \zeta_1|y_i - y_j| \\ &+ \beta_2|\sigma_i + \sigma_j| + \delta_2|\lambda_i + \lambda_j| + \eta_2|z_i + z_j| + \zeta_2|y_i + y_j| + \mu_i + \epsilon_{ij} \end{aligned} \tag{1}$$

where σ and λ denote elicited preference parameters, and ϵ_{ij} is the idiosyncratic error. The specification includes absolute values of sums and differences of each explanatory variables within the pair.[29] Sums model the likelihood that pairs of individuals with particular characteristics are more likely to be friends. Differences allow us to test for assortative matching on time-invariant characteristics (most of the variables that we include in **z** and **y** are fixed, e.g gender).[30] Positive values of the parameters β_1 or δ_1 indicate negative assorting in risk and loss preferences (individuals characterized by larger differences are more likely to be friends), and vice versa. Parameter estimates for η_1 and ζ_1 indicate matching on individual and family background characteristics. We estimate the dyadic model using a logit estimator and cluster standard errors at the class level, following Attanasio et al. (2012).

We first consider selection into friendships. There are 335 dyads.[31]. The results are shown in Table 5.

We find no evidence for assortative matching on risk or loss preferences among friends (see column 1). Controlling for individual and family background characteristics, such as age, gender and numeracy (measured by the math grade relative to average class performance), we find strong evidence for positive assortative matching, consistent with the matching literature. Yet again, risk or loss preference parameters are not among the matching characteristics. The same holds for numeracy, while students match on age and gender (see column 2). Since all teenagers enter a pair by experimental design (unless class size

[29]We use absolute values as the match is undirected.

[30]If preferences are time-variant (as adolescents' σ and λ may be), differences may additionally reflect convergence in those dimensions through previous contact. As discussed at the beginning of this section, we do not distinguish between these possibilities. Thus, we test joint hypotheses that comprise these mechanisms.

[31]The number of possible pairs depends on the class size as friends and classmates can by our design only be matched within, not across classes.

is uneven or an adolescent in a pair did not give consent, in which case one teenager remains unmatched and is not part of the analysis sample), there is little source for endogenous participation choice. As expected, we find none of the terms involving sums of characteristics to affect the probability of a friend matching, confirming the viability of our design and rejecting the hypothesis of endogenous participation choice.

In column 3, we add family background characteristics such as log of household size, whether they live with a single parent, have migrant background, or have (relatively) low socio-economic status. The latter is proxied by a dummy that takes the value of 1 if there are no more than 25 books in a teenagers' home.[32] We find very similar parameter estimates for the matching variables age and gender. Additionally, adolescents are more likely to match on single parent and socio-economic status.[33] The results with respect to assortative matching on risk and loss preferences remain qualitatively unchanged as we add regressors moving from columns (1) to (3).

Next, we consider matching among classmates; see Table 6. Again, we find no evidence of pairing by preferences for risk or loss but we do find evidence for matching on age and gender: positive assortative matching on age arises because we have classes from two different cohorts in our sample, thus age differences between classes are more pronounced than within classes. Negative assortative matching on gender can be explained using conditional probabilities: given that classes are small, a boy will match more likely with a female within the classroom than across different classrooms, since the distribution of gender across *CONTROL* classes is affected relatively less by removing one male from the reference group. Apart from these mechanical results, we find no evidence of sorting or systematic matching effects.

In summary, we do not find evidence of assortative matching on risk or loss preferences, suggesting that friendships are neither formed nor more likely sustained if these preferences are similar. We also do not find matching on preferences for risk or loss among classmates.

Result 1: Choice similarity in the absence of social interaction

a Neither friends nor classmates assortatively match on loss or risk aversion.

b Classmates are matched by observable characteristics, i.e., age and gender, which however results from mechanical sorting of classes.

c Friends assortatively match on age and gender, and on living with a single parent and family socio-economic status.

4.3 Social interaction in risky choice situations

Peer effects may not only arise from assortative matching but also from direct social interaction. We introduce controlled variation in social interaction (in the form of pre-decision peer communication) in

[32]The number of books is a standard question in PISA that captures important family inputs into a teenager's education, or lack thereof (Hanushek and Woessmann, 2011). The types of schools where we conducted our study tend to have lower SES students to begin with. We capture the variation in socio-economic status among them by the number of books at home.

[33]In a robustness check, we re-estimate specifications 2 and 3 on a pooled control (*CONTROL*) and friends matched treatment (*FRIENDS*) to check whether both groups have a similar matching on observables. We find very similar parameter estimates as for the control group alone, so friends match on the same characteristics in both groups (see Table 16 in appendix A.5).

Table 5: Logit regressions of probability of being friends on Dyad Level Explanatory Variables within *CONTROL*

	(1) $P(friends = 1)$	(2) $P(friends = 1)$	(3) $P(friends = 1)$
DIFFERENCES			
σ	0.0647	0.288	0.416
	(0.212)	(0.550)	(0.890)
λ	0.00834	-0.0542	-0.0642
	(0.115)	(0.183)	(0.161)
Math grade (rel.)		0.130	0.157
		(0.291)	(0.261)
Age		-0.613**	-0.761**
		(0.263)	(0.304)
Male		-2.517***	-2.526**
		(0.972)	(1.110)
Household size			-0.158
			(0.175)
Single parent			-1.751***
			(0.601)
Migrant background			-0.0728
			(0.286)
< 25 books at home			-0.357***
			(0.0887)
SUMS			
σ	0.216	0.151	0.212
	(0.280)	(0.300)	(0.426)
λ	0.0186	0.0748	0.0982
	(0.0637)	(0.129)	(0.123)
Math grade (rel.)		0.0629	-0.00710
		(0.163)	(0.221)
Age		0.0353	0.0804
		(0.0658)	(0.133)
Male		-0.334*	-0.348
		(0.203)	(0.285)
Household size			0.0889*
			(0.0463)
Single parent			0.460*
			(0.251)
Migrant background			-0.0466
			(0.360)
< 25 books at home			-0.216
			(0.328)
Constant	-3.213***	-2.956	-4.128
	(0.600)	(2.963)	(5.332)
Observations	335	335	335

Note: Math grade (rel.) refers to the math grade of each individual subtracted by the class average; robust standard errors clustered at the class level in parentheses;
*** p<0.01, ** p<0.05, * p<0.1

Table 6: Logit regressions of probability of being classmates on Dyad Level Explanatory Variables within *CONTROL*

	(1) $P(classmates = 1)$	(2) $P(classmates = 1)$	(3) $P(classmates = 1)$
DIFFERENCES			
σ	-0.637	-0.699	-0.688
	(0.742)	(0.583)	(0.498)
λ	0.0106	0.00426	0.0146
	(0.0454)	(0.0715)	(0.0429)
Math grade (rel.)		-0.108	-0.107
		(0.140)	(0.111)
Age		-0.612***	-0.615***
		(0.156)	(0.166)
Male		0.163***	0.163***
		(0.0557)	(0.0560)
Household size			-0.0117
			(0.0420)
Single parent			0.0859
			(0.0725)
Migrant background			0.0385
			(0.0835)
< 25 books at home			-0.0466
			(0.130)
SUMS			
σ	-0.0485	0.00422	-0.00239
	(0.454)	(0.495)	(0.453)
λ	-0.0214	-0.0121	-0.0214
	(0.0350)	(0.0444)	(0.0700)
Math grade (rel.)		0.0223	-0.00352
		(0.0793)	(0.143)
Age		0.189	0.207
		(0.335)	(0.384)
Male		-0.101	-0.110
		(0.120)	(0.151)
Household size			0.0371
			(0.112)
Single parent			-0.0760
			(0.216)
Migrant background			-0.00896
			(0.250)
< 25 books at home			-0.0797
			(0.263)
Constant	-0.667	-5.273	-5.888
	(1.551)	(7.967)	(9.997)
Observations	1,378	1,378	1,378

Note: Math grade (rel.) refers to the math grade of each individual subtracted by the class average; robust standard errors clustered at the class level in parentheses; robust standard errors clustered at the class level in parentheses; *** p<0.01, ** p<0.05, * p<0.1

our experiment. In the two treatment conditions, we pair each teenager with a randomly allocated classmate (treatment *RANDOM*) or a friend (treatment *FRIENDS*) and allow them to communicate. After they communicated, each subject makes lottery choices individually. We acknowledge the dynamic nature of peer influences by allowing for bilateral interaction and simultaneous decision-making under random exposure to natural peers. This comes at a cost: We are not able to identify the direction of peer influences, as would be possible in a sequential design. However, our design does preserve the innate uncertainty about the actions of one's peer that is characteristic for many choice situations, and allows for dynamic interaction. The latter is ruled out in sequential designs.

Our design helps us better understand the mechanisms of peer influences, as we can separate assortative matching from (dynamic) social interaction. Since we found assortative matching on socio-economic characteristics (though not on risk preferences) among classmates and friends, we identify the impact of peer communication on adolescents' risk choices using the following approach: we compare the choice similarity of paired teenagers in the treatment groups with similarly matched pairs in the control group where no communication was possible.

In the *RANDOM* treatment, we construct counterfactual pairs by randomly drawing classmates from the control group sample without replacement. Measures of similarity are given by within-pair differences, denoted by $\Delta_{ij}(\sigma)$ and $\Delta_{ij}(\lambda)$, and by comparing decisions on sheets A and B, or both, again within pairs. We test for social interaction effects by regressing these measures of similarity on a treatment dummy, which allows us to account for multi-level clustering by pairs and classes by using the wild bootstrap (see Cameron et al. (2008) and for a survey, Cameron and Miller (2015)) to compute p-values. This approach is particularly suitable for our situation with a small number of clusters (classes).

Table 7 shows the results. We find that communication reduces the difference in risk aversion by 0.22 ($p = 0.090$), a reduction of 53% (relative to the overall mean). The difference in loss aversion is reduced by 1.40 ($p = 0.011$), a reduction of 61%.

The increase in similarity mainly stems from perfect alignment of lottery choices. 41% of pairs in the treatment group choose the same switchpoint in the lotteries with positive payoffs on sheet A ($p = 0.027$) relative to 19% identical choices in the control group, and 42% make identical choices in the mixed lotteries (sheet B) ($p < 0.001$). 56% of subjects in *RANDOM* make identical choices in both decision sheets, while none do so in the control group.

Similar social interaction effects manifest when friends (rather than classmates) are allowed to discuss their choices (see Table 8). As the counterfactual, we use the similarity of friends' risky choices in the control group, identified through a mutual exchange of ID stickers, the same method that we used in the *FRIENDS* treatment.

Similarity in the risk aversion parameter is 0.22 lower but this effect is not statistically significant ($p = 0.076$). Differences in the loss aversion parameter decrease by 1.80 ($p = 0.007$), which corresponds to a decrease of 57% (relative to the mean). Again, this is mainly due to the increase in identical choices within peer pairs. The fraction of pairs who align their choices perfectly in the lotteries with positive payoffs increases by 29 percentage points ($p = 0.022$), and by 47 percentage points in the mixed lotteries ($p = 0.003$). 42.5% of individuals make perfectly aligned choices in both lottery types, as opposed to 3%

Table 7: Similarity of risk parameters across matches in *RANDOM* vs. *CONTROL*

	Estimate	p-value	relative difference	mean value in *CONTROL*
$\Delta_{ij}(\sigma)$	-0.2151	0.090*	-0.53	0.4086
$\Delta_{ij}(\lambda)$	-1.4027	0.011**	-0.61	0.9073
Identical choice of i and j in lotteries with				
positive prospects	0.4051	0.027**	2.16	0.1875
mixed prospects	0.4167	<0.001***	1.67	0.2500
both	0.5556	0.035**	^{a}	0.000

Note: i and j refer to matched participants. $\Delta_{ij}(\sigma)$ and $\Delta_{ij}(\lambda)$ refer to the absolute difference in σ respectively λ between two matched participants; results are obtained by a regression of $\Delta_{ij}(\sigma)$ respectively $\Delta_{ij}(\lambda)$ on the treatment dummy in a sample covering CONTROL and RANDOM groups. p-values were obtained using the clustered wild bootstrap procedure by Cameron et al. (2008) with 2000 replications. The same procedure was followed for dummies capturing whether matched participants made identical choices on sheet A (the lotteries over positive prospects), sheet B (the lotteries with mixed prospects) or both sheets %(*ident.choice*).*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. ^{a}: no value reported due to zero denominator.

Table 8: Similarity of risk parameters across matches in *FRIENDS* vs. *CONTROL*

	Estimate	p-value	relative difference	mean value in *CONTROL*
$\Delta_{ij}(\sigma)$	-0.2164	0.076*	-0.57	0.3820
$\Delta_{ij}(\lambda)$	-1.8043	0.007***	-0.86	2.0943
Identical choice of i and j in lotteries with				
positive prospects	0.2938	0.022**	1.88	0.1563
mixed prospects	0.4688	0.003***	1.67	0.2813
both	0.3938	<0.001***	12.6	0.0313

Note: i and j refer to matched participants. $\Delta_{ij}(\sigma)$ and $\Delta_{ij}(\lambda)$ refer to the absolute difference in σ respectively λ between two matched participants; results are obtained by a regression of $\Delta_{ij}(\sigma)$ respectively $\Delta_{ij}(\lambda)$ on the treatment dummy in a sample covering CONTROL and FRIENDS groups. p-values were obtained using the clustered wild bootstrap procedure by Cameron et al. (2008) with 2000 replications. The same procedure was followed for dummies capturing whether matched participants made identical choices on sheet A (the lotteries over positive prospects), sheet B (the lotteries with mixed prospects) or both sheets. Mean values for these measures are obtained from a random match of classmates in *CONTROL* without replacement. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

in the control group.

Result 2: Choice similarity in the social interaction conditions

a Similarity in risk preferences increases among peers who are allowed to communicate (among friends and classmates).

b Social interaction increases the fraction of paired teenagers who make identical decisions (in both treatments).

4.4 Additional mechanisms and confounding factors

Several mechanisms may explain choice similarity among peers under social interaction. In this part of the paper, we explore one of them: social learning. We develop our testable hypotheses based on the idea that social learning depends on the choice competency of the individual and her peer. In our study, we observe private information of own and a public signal of peer quality. As in Bursztyn et al. (2014), we can only establish correlations, not causality, because these signals are not randomly assigned.

Our study contains a financial literacy score derived from 12 questions[34] and a numeracy measure (the last math grade). Both might serve as signals of peer quality. However, math grades (and performance) are publicly observable in the classroom whereas our test of financial literacy was conducted during the same classroom session and subjects received no feedback on their peers' test results.[35] Therefore, numeracy is a publicly observed signal of peer quality, while the financial literacy score summarises private information.

Under social learning, peers' willingness to coordinate choices should increase with the public signal, and one's willingness to coordinate (with any peer) should decline in one's private information. Since peer interaction is bilateral in our simultaneous setting, choice alignment can be avoided through choosing not to communicate or through cheap talk. If social learning matters, individuals who receive a negative peer signal should decide not to change their choices towards that of the peer. The same holds for individuals with high financial competency, as a high own quality may reduce the need for social learning, thus muting the scope for peer influence. We use perfect choice alignment in all lottery choices as a conservative measure of peer influence, and examine this hypothesis.

In Table 9, we investigate the determinants of perfect alignment of choices across both sheets in each treatment condition. We include measures of (own) financial literacy and numeracy and peer differences in these measures. As financial competency summarises private information, peer differences in this measure should not affect peer similarity; we find that indeed it does not. In contrast, we find that peer differences in (publicly observed) numeracy reduce the probability of alignment, suggesting social learning. Moreover, high own financial literacy is associated with a decrease in the likelihood of perfectly aligned choices, as hypothesised above, suggesting that own competence makes teenagers less prone to peer influences.

However, Table 9 also shows that these effects are statistically significant (and economically important) only in the *RANDOM* treatment, so whether social learning takes place appears sensitive to the type of peer. We find no correlation between numeracy or financial sophistication (or peer differences in these) in the *FRIENDS* treatment, although interacting friends also display strong increases in the fraction of identical choices. The sensitivity of peer influence to peer closeness has also been highlighted by Carrell et al. (2013). Our results suggest that social learning and information exchange play a stronger role in social interaction between less close peers. Given that we cannot rely on exogenous variation in financial competency and since our sample size shrinks when we split the data by treatment condition and degree of similarity, the robustness of the differential results between these two peer groups should be tested in further research. A suitable design for such a test could be to introduce experimental variation in private and public signals between rounds in repeated lotteries with mixed and positive prospects.

[34]Our measure of financial literacy replicates their interest compounding and risk diversification measure, but provides a more comprehensive measurement of financial literacy using ten questions in which adolescents are asked to choose between differently risky assets, trade off between risk and liquidity, and so forth. See appendix A.8 for further details.

[35]We use their (undisclosed) literacy score as an independently measured rather than self-assessed measure of their financial competency and assume that it summarises individuals' private information on their "quality".

Table 9: Determinants of perfect choice alignment

	RANDOM	*FRIENDS*
Financial literacy	-0.239 (1.99)**	0.068 (0.91)
Δ(Financial literacy)	-0.076 (0.34)	-0.110 (1.51)
Math	0.236 (0.88)	-0.060 (0.34)
$\Delta(Math)$	-0.706 (2.11)**	0.269 (1.51)
Male	-0.260 (0.55)	-0.590 (1.78)*
$\Delta(Male)$	0.525 (1.01)	-0.564 (1.25)
Constant	2.512 (2.28)**	-0.317 (0.45)
Observations	46	74

Note: Estimates are clustered at the class level. t statistics in parenthesis;
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

To summarise, peer interaction increases choice similarity in both, gains and mixed lotteries, and regardless of the type of peer. In addition, the fraction of peers who make identical choices increases substantially. When peers are less close, those with higher own financial literacy are less likely to coordinate their choice with their peer, while peer similarity in publicly observed numeracy increases the likelihood of aligned peer choices (although neither classmates nor friends match assortatively on numeracy). In contrast, we find that the likelihood of identical choices between friends varies only by gender – with female pairs being more likely to coordinate. Finally, interaction with a random classmate leads to more loss averse choices. While lottery choices of classmate pairs who make non-coordinated choices are similar to those in the control group, statistically significantly more loss averse choices are made among classmates who coordinate. Hence, we find social learning effects that are sensitive to peer closeness.

Social learning is not the only possible mechanism behind peer effects in social interaction. Bursztyn et al. (2014) shows that social utility, i.e. "Keeping up with Joneses" type effects also matter in shaping peer effects among adults. Social utility effects among adolescents could also take the form of a social insurance motive: random classmates might make very loss averse choices for fear of ending up loosing more of their initial endowment relative to their peer. This motive may be stronger among classmates than friends due to differences in the level of trust. However, further research is needed to shed light on the mechanisms behind social interaction effects.

Result 3: Social learning

a Peer's alignment of choices decreases with private information on one's financial competency.

a Perfect alignment is more likely as similarity in publicly observed financial competency increases, consistent with social learning effects.

b Choice alignment among friends is neither related to a privately nor publicly observed measure of financial competency.

5 Conclusion and discussion

In this paper, we presented a new experimental strategy to improve the understanding of peer effects. We randomised subjects from a sample of natural peers into experimental conditions with and without social interaction, and collected data on pre-existing peer relations. Our experimental design thus combines the identification strategies of the experimental and non-experimental literatures on peer effects to the advantage that it allows us to separate two components of peer influence – assortative matching and social interaction effects.

We applied this strategy to study peer effects in risk taking among adolescents. Our sample of natural peers are classmates, and among them we observe who are friends. In the two treatment conditions, we allow for social interaction between randomly paired classmates and pairs of pre-existing friends, respectively; in the control condition, there is no interaction. We find that adolescent friends assortatively match on socio-economic characteristics like age, gender and on family background characteristics. However, we find no evidence that they match on their preferences regarding risk or loss. This finding is in line with evidence on spouses from Shaner (2015). She finds at best weak assortative matching on preferences in couples.

Our main finding is that peer communication, i.e. social interaction in the choice situation, strongly affects incentivised risky choices. Adolescents in both treatment groups – randomly paired classmates and mutual friends – display increased choice similarity in lotteries (both with positive as well as mixed prospects) when they are allowed to communicate in pairs before they make their choices. A large fraction of choices in a pair are perfectly aligned. In summary, peer effects do not arise from assortative matching with the set of peers an individual has formed ties with but rather from social interaction dynamics between peers. This result challenges the common presumption that well-documented and strong assortative matching patterns in observables would extend also to the (usually unobserved) domain of preferences.

Future research is needed to understand the mechanisms behind the strong social interaction effects that we separately identified in our study. One potential driver of increased choice similarity among pairs is social learning (see, for example, Bursztyn et al. (2016)). We explore this mechanism by exploiting the fact that our data contain both private information on, and a public signal of, financial competency. High own financial competency appears to make teenagers less prone to peer influence, i.e., less likely to coordinate choices. A positive public signal of financial competency, in contrast, increases the likelihood of choice coordination among peers. A second mechanism would be a social utility channel, i.e., the utility an agent receives from her payoffs might depend on the relationship with other agents. A natural extension of our design would introduce *experimental* variation in private and public information on task competency to investigate social learning effects in more detail. Our design could also be adapted so that information between peers flows only in one direction, as in Bursztyn et al. (2014) who induce experimental variation in the visibility of peer choices to identify social learning, and in a peer's choice realisation to identify the social utility channel.

A third potential mechanism worth exploring may be the role of trust established in pre-existing peer relations. Friendship is a peer relation built on trust while being classmates is not. Lack of trust may affect risky choices in a social interaction situation.[36] Trust may explain why we find that social interaction effects among randomly paired classmates and among friends are different. We find evidence in favour of the social learning hypothesis only for randomly paired classmates but not for friends. Also, classmates make more loss averse choices under peer influence but do not seem to systematically change their risk taking, while we do not find this result for friends. These findings are consistent with higher levels of trust among friends than among classmates.

Our findings can be summarized as follows. First, more than half of adolescents are risk averse and over two-thirds loss averse. They displayed less risk aversion than (a comparison sample of) university students in the same choice task. This confirms existing evidence on how risk aversion varies with age. Additionally, we find weakly declining loss aversion by age. To our knowledge, we are the first to document loss aversion among adolescents. Second, adolescents' risky choices are strongly influenced by their peers. Our third and main result is that the mechanism behind these peer effects is not assortative matching on risk preferences but social interaction. Fourth, we find evidence that is suggestive of social learning. Finally our results are consistent with, but non-conclusive regarding other mechanisms underlying these strong social interaction effects. A natural extension of our experimental framework would introduce experimental variation in social learning, social utility and trust to better understand the mechanisms driving social interaction between (adolescent or adult) peers.

[36]There is a debate about whether risk preferences and trust attitudes can be separated in laboratory experiments. Houser et al. (2010) argue that trust decisions are not tightly connected to a person's risk attitudes and thus can be studied separately using experimental tasks such as Berg et al. (1995)'s trust game.

A Appendix

A.1 Additional summary statistics

Table 10 reports descriptive statistics of the whole sample and separately for each treatment. The last column reports p-values from χ^2 tests. Our results are robust to restricting the sample to subjects with consistent choices[37] and those who have a partner in the treatment conditions.

Table 10: Descriptive Statistics Full Sample

	TOTAL	CONTROL	RANDOM	FRIENDS	p-value
students	235	77	59	99	
classes	14	5	4	5	
schools	7	4	4	4	
Grade	7.783	7.779	7.729	7.818	0.417
Age	14.117	13.937	14.077	14.281	0.138
Male	0.586	0.636	0.544	0.571	0.520
Math grade (rel.)	-0.000	-0.000	-0.000	0.000	0.957
Financial literacy	7.489	7.442	7.102	7.758	0.312
Cognition score	0.804	0.714	0.763	0.899	0.258
Low cognition	0.817	0.831	0.881	0.768	0.187
Single parent	0.296	0.357	0.327	0.234	0.199
Household size	4.072	3.973	4.111	4.128	0.116
Migrant background	0.634	0.707	0.566	0.614	0.232
Number of books in hh	2.532	2.699	2.679	2.319	0.250
$<$ 25 books at home	0.447	0.390	0.390	0.525	0.119
Contact intensity	2.513	2.711	2.264	2.495	0.005***
Frequent contact	0.409	0.532	0.305	0.374	0.018**

Note: rel. math is math score subtracted by the class mean; stan. math is math score subtracted by class mean and divided by class standard deviation; cognition corresponds to number of correct Raven matrices out of 4; low cognition is a dummy variable indicating 0 or 1 correct responses in the Raven matrices; migrant background is a dummy taking value 1 if student checked box "other language spoken at home"; high contact is a dummy taking the value 1 if the respondent replied to have contact with the partner at least once per week; p-values are from χ^2 tests of sample balancedness; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

[37]We encouraged consistent choices by explaining to participants why it only makes sense to switch once from option L to option R, and asked for a summary of choices at the bottom of each sheet (see appendix A.7). We allowed participants to disregard the choice summary and determined payouts from the binary choices at the top. Participants often left out (parts of the) summary at the bottom of the decision sheets: of the 235 original observations, 50 participants left out items of the summary on sheet A and 41 left out items of the summary on sheet B. Therefore, we only analyse the binary choice reponses and disregard the summary information.

A.2 Assigning parameter values

Tables 11 and 12 display the lotteries on the two decision sheets, including their payoffs and the differences in expected payoffs. Additionally, Table 11 demonstrates how σ is assigned according to the row of the switch to the right option. Using a value of $\sigma = 0.65$ as an example, Table 12 demonstrates how λ is assigned (switch on sheet A to the right option in the 6th row).

Table 11: Assigning σ parameters

	Option L		Option R		$\Delta_{LR}(EV)$	σ range		Assigned σ
Switch	Low	High	Low	High		Min	Max	
1	1.00	2.00	0.20	2.41	0.20	1.50	n/a	1.63
2	1.00	2.00	0.20	2.56	0.12	1.25	1.50	1.38
3	1.00	2.00	0.20	2.80	0.00	1.00	1.25	1.13
4	1.00	2.00	0.20	3.09	-0.14	0.80	1.00	0.90
5	1.00	2.00	0.20	3.29	-0.24	0.70	0.80	0.75
6	1.00	2.00	0.20	3.54	-0.37	0.60	0.70	0.65
7	1.00	2.00	0.20	3.87	-0.53	0.50	0.60	0.55
8	1.00	2.00	0.20	4.31	-0.76	0.40	0.50	0.45
9	1.00	2.00	0.20	4.93	-1.07	0.30	0.40	0.35
10	1.00	2.00	0.20	5.85	-1.53	0.20	0.30	0.25
11	1.00	2.00	0.20	7.33	-2.27	0.10	0.20	0.15
Never						n/a	0.1	0.05

Note: Switch in first rows corresponds to choosign option R in all rows; σ for switch in row 1 and for never assigend by extrapolating distance from neighboring category's midpoint to respective category

Table 12: Assigning λ parameters ($\sigma = 0.65$)

	Option L		Option R		$\Delta_{LR}(EV)$	λ range		Assigned λ
Switch	Low	High	Low	High		Min	Max	
1	-0.40	2.50	-2.10	3.00	0.60	0.21	n/a	0.06
2	-0.40	0.40	-2.10	3.00	-0.45	1.40	0.21	0.80
3	-0.40	0.10	-2.10	3.00	-0.60	1.70	1.40	1.55
4	-0.40	0.10	-1.60	3.00	-0.85	2.26	1.70	1.98
5	-0.80	0.10	-1.60	3.00	-1.05	3.69	2.26	2.97
6	-0.80	0.10	-1.40	3.00	-1.15	4.79	3.69	4.24
7	-0.80	0.10	-1.10	3.00	-1.30	9.14	4.79	6.97
Never						n/a	9.14	9.69

Note: Switch in first rows corresponds to choosign option R in all rows; λ for switch in row 1 and for never assigend by extrapolating distance from the two neighboring category's midpoints to respective category

A.3 Risky choices and estimated parameters

Table 13 shows the distribution of switching points in the three treatment conditions. We find that while on average individuals switch later on sheets A and B in both treatments, most of the effect comes from late switchers delaying their switch as the median remains similar. The last column reports p-values from t-tests comparing the mean switching point on the first (second) choice list to the average switching point under risk (loss) neutrality for each treatment. Switching points on list A (B) are significantly different from risk (loss) neutrality according to these tests.

Table 13: Descriptive Statistics for risky choices

	mean	median	5th	25th	75th	95th	p-value
Switching point on sheet A (gains lotteries)							
CON	5.359	6.0	1.0	3.0	7.5	11.0	0.000***
RAN	6.037	6.5	1.0	3.0	9.0	11.0	0.000***
FRI	5.888	6.0	1.0	4.0	7.0	11.5	0.000***
Switching point on sheet B (mixed lotteries)							
CON	3.859	4.0	1.0	3.0	4.0	7.0	0.000***
RAN	4.463	4.0	2.0	3.0	5.0	8.0	0.000***
FRI	3.950	4.0	2.0	3.0	5.0	8.0	0.000***

Note: Full sample included; p-values from t-tests of H_0: switch A = 3.5 or switch B = 2 respectively; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Using the assumption of prospect theory, we estimate preference parameters from the choices in the lotteries with positive and mixed payoffs. The estimates represent estimates of loss and risk-aversion for teenagers in the control group as these decide independently of their peers. As we show in this paper, the assumption of independent choices is violated in the social interaction treatments. Therefore, our estimates of σ and λ do not represent preference parameters but rather *as if* estimates under the independence assumption. Furthermore, these parameters are identified from the two decision sheets as combinations (σ, λ), hence they are identified conditional on each other. For this reason, we present our results in terms of switching points, i.e. risky choices, as well as changes in the estimated σ and λ parameters. Our results do not depend on the assumptions made in the estimation of σ and λ.

A.4 Comparison with risk preference estimates from a sample of university students and from other sources

A.4.1 Replication of lottery choices in a sample of university students

We replicated the task we posed to the control group in our experiment with university students. The students completed a computerised lab task using the same lottery choices (with identical payoffs except for rounding to one decimal). Students were paid according to an individual chip and choice draw, compared to a joint in-class draw in our experiment. In a sample of 64 students at LMU Munich, we find risk aversion among university students at the mean ($\sigma = 0.613$; see lower panel of Table 14) that is similar to the parameters found in the previous literature. We test for differences in risk aversion between teenagers of relatively low SES and university students (who tend to be of high SES), and find teenagers to be statistically significantly less risk-averse (two-sided Mann-Whitney test, $p = 0.004$).
Correspondingly, we find that teenagers switch to the more risky lottery B earlier (average switching point: 5.359) than university students (switching point: 6.845, MW test, $p = 0.003$), and that a larger percentage of young adults is risk-averse (χ^2 test, $p = 0.023$). [38]

[38] This result is consistent with the findings of Tymula et al. (2012) who report increasing risk aversion between adolescence and adulthood. Contradicting evidence is provided in Glätzle-Rützler et al. (2015) who found the degree of risk aversion to be stable across age between childhood and adolescence. However, since the two study populations differ in their socio-economic status, we cannot interpret our findings solely in terms of age.

Table 14: Descriptive Statistics for individual parameters: adolescents and university students

	mean	median	5th	25th	75th	95th	% risk- (loss-) averse	p-value[a]
Adolescents' risk preferences (CONTROL)								
σ	0.828	0.650	0.150	0.500	1.125	1.625	0.594	0.004***
λ	2.579	1.985	0.458	1.548	2.499	7.490	0.797	0.000***
University students' risk preferences[b]								
σ	0.613	0.550	0.050	0.350	0.750	1.625	0.775	0.000***
λ	2.949	2.023	0.671	1.540	3.141	10.019	0.775	0.000***

Note: [a]: p-values from t-tests of H_0: $\sigma \geq 1$ or $\lambda \leq 1$ respectively; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
[b]: Risk preferences are elicited using the same lottery choices (rounded to one decimal).

A.4.2 Comparison with other studies

The prevalence of risk aversion among adults and adolescents has been established in previous studies (Glätzle-Rützler et al., 2015; Tanaka et al., 2010; Tymula et al., 2012; von Gaudecker et al., 2011). Tanaka et al. (2010), for example, report $\bar{\sigma}_{south} = 0.59$ ($\bar{\sigma}_{north} = 0.63$) for farmers in south (north) Vietnam, while Glätzle-Rützler et al. (2015) estimate mean risk aversion to be 0.57 among children and adolescents aged between 10 and 18 in Austria. Our results show a similar degree of risk aversion at the median, but a slightly lower mean degree of risk aversion compared to these studies.

For loss aversion, Tanaka et al. (2010) report $\bar{\lambda} = 2.63$, and Tversky and Kahneman (1992) provide an estimate of 2.25. Comparing our parameter values to those of adults in Tanaka et al. (2010) using t-tests, we find no statistically significant differences in loss aversion λ.

A.5 Robustness checks for assortative matching results

In this section, we repeat the dyadic regressions from section 4.2, but pool the control group and the treatment condition *FRIENDS*. We do in order to check whether friends or classmates in both groups assort with respect to the same characteristics.

In Table 15, we report robustness checks for the dyadic regressions regarding the probability of being friends based on Fafchamps and Gubert (2007) whose results are shown in Table 5. We include the observations from the *FRIENDS* treatment in columns (2) and (4), and repeat the results for the control group in columns (1) and (3). Naturally, in the interacting pairs of friends in *FRIENDS* the preference parameters are endogenous to the interaction therefore we cannot include them as explanatory variables. The results indicate that the correlation between age, gender, and family characteristics, and being friends is robust to including the observations from the *FRIENDS* treatment.

In Table 16, we report robustness checks for the dyadic regressions regarding the probability of being classmates shown in Table 6. Again, we include the observations from the *FRIENDS* treatment in columns (2) and (4) (with columns 1 and 3 reporting estimates for the control group alone). The results indicate that the correlation between age and being classmates is robust to including the observations from the *FRIENDS* treatment. Gender is no longer a statistically significant matching characteristic. However, we explained previously that this effect was somewhat mechanic in a class of restricted size. Pooling control and *FRIENDS* condition relaxes this constraint so we conclude that our results are robust.

Table 15: Logit regressions for the probability of being friends on Dyad Level Explanatory Variables within *CONTROL* and *FRIENDS*

	$P(friends = 1)$			
	(1)	(2)	(3)	(4)
	CONTROL	CONTROL & FRIENDS	CONTROL	CONTROL & FRIENDS
DIFFERENCES				
σ	0.288		0.416	
	(0.550)		(0.890)	
λ	-0.0542		-0.0642	
	(0.183)		(0.161)	
Math grade (rel.)	0.130	-0.0400	0.157	0.0169
	(0.291)	(0.148)	(0.261)	(0.147)
Age	-0.613**	-0.449**	-0.761**	-0.445**
	(0.263)	(0.205)	(0.304)	(0.223)
Male	-2.517***	-2.181***	-2.526**	-2.197***
	(0.972)	(0.479)	(1.110)	(0.525)
Household size			-0.158	-0.126*
			(0.175)	(0.0755)
Single parent			-1.751***	-0.960**
			(0.601)	(0.387)
Migrant background			-0.0728	-0.385
			(0.286)	(0.276)
$<$ 25 books at home			-0.357***	-0.340*
			(0.0887)	(0.199)
SUMS				
σ	0.151		0.212	
	(0.300)		(0.426)	
λ	0.0748		0.0982	
	(0.129)		(0.123)	
Math grade (rel.)	0.0629	0.0269	-0.00710	0.0252
	(0.163)	(0.0547)	(0.221)	(0.0599)
Age	0.0353	-0.0568	0.0804	-0.0775**
	(0.0658)	(0.0347)	(0.133)	(0.0352)
Male	-0.334*	-0.352***	-0.348	-0.321***
	(0.203)	(0.0832)	(0.285)	(0.0871)
Household size			0.0889*	0.0564
			(0.0463)	(0.0466)
Single parent			0.460*	0.507***
			(0.251)	(0.177)
Migrant background			-0.0466	-0.126
			(0.360)	(0.0846)
$<$ 25 books at home			-0.216	0.0165
			(0.328)	(0.0747)
Constant	-2.956	0.262	-4.128	0.965
	(2.963)	(0.870)	(5.332)	(0.968)
Observations	335	705	335	705

Note: Columns 2 and 4 contain observations from *FRIENDS* treatment; Robust standard errors clustered at the class level in parentheses; math grade (rel.) refers to the math grade of each individual subtracted by the class average; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 16: Logit regressions for the probability of being classmates on Dyad Level Explanatory Variables within *CONTROL* and *FRIENDS*

	$P(classmates = 1)$			
	(1)	(2)	(3)	(4)
	CONTROL	CONTROL & RANDOM	CONTROL	CONTROL & RANDOM
DIFFERENCES				
σ	-0.699		-0.688	
	(0.583)		(0.498)	
λ	0.00426		0.0146	
	(0.0715)		(0.0429)	
Math grade (rel.)	-0.108	-0.118	-0.107	-0.111
	(0.140)	(0.0748)	(0.111)	(0.0808)
Age	-0.612***	-0.382***	-0.615***	-0.385***
	(0.156)	(0.143)	(0.166)	(0.128)
Male	0.163***	0.0117	0.163***	0.0133
	(0.0557)	(0.0900)	(0.0560)	(0.0864)
Household size			-0.0117	-0.0461
			(0.0420)	(0.0345)
Single parent			0.0859	-0.155
			(0.0725)	(0.267)
Migrant background			0.0385	-0.313
			(0.0835)	(0.324)
$<$ 25 books at home			-0.0466	-0.0226
			(0.130)	(0.0740)
SUMS				
σ	0.00422		-0.00239	
	(0.495)		(0.453)	
λ	-0.0121		-0.0214	
	(0.0444)		(0.0700)	
Math grade (rel.)	0.0223	0.00450	-0.00352	-0.00639
	(0.0793)	(0.0255)	(0.143)	(0.0416)
Age	0.189	0.0617	0.207	0.0714
	(0.335)	(0.206)	(0.384)	(0.224)
Male	-0.101	0.0121	-0.110	0.00191
	(0.120)	(0.119)	(0.151)	(0.102)
Household size			0.0371	0.0280
			(0.112)	(0.0546)
Single parent			-0.0760	0.0389
			(0.216)	(0.143)
Migrant background			-0.00896	-0.0506
			(0.250)	(0.152)
$<$ 25 books at home			-0.0797	-0.0613
			(0.263)	(0.213)
Constant	-5.273	-2.601	-5.888	-2.689
	(7.967)	(5.813)	(9.997)	(6.470)
Observations	1,378	3,269	1,378	3,269

Note: Columns 2 and 4 contain observations from *FRIENDS* treatment; Robust standard errors clustered at the class level in parentheses; math grade (rel.) refers to the math grade of each individual subtracted by the class average; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

A.6 Instructions (translated from German)

Part 1 – Risk Game

Welcome to today's survey. Our **survey** consists of **two parts**. Please raise your hand if you did not hand in a parents' consent!

[***CONTROL***

In part 1 it is very important that you make your decisions on your own. There are no right or wrong decisions. We would only like to know what you prefer. As soon as we have finished the explanation, we ask you to be **absolutely quiet for 5 minutes**.]

[***RANDOM***

In part 1 you can discuss your decisions with another person in the classroom. We prepared a bag with numbers. Each number exists twice. Please draw a number and, as soon as everyone has drawn, look for the person that has drawn the same number and sit together in pairs. Please take a pen with you! You are allowed to discuss your decisions with your partner quietly, but you decide only for yourself! If no one else has drawn your number, you can fill in the first part alone! In the lower part of your questionnaires you find red stickers with your questionnaire number. Please remove the sticker from your own questionnaire and put it on your partner's questionnaire, so we know who you worked with!]

[***FRIENDS***

In part 1 you can discuss your decisions with another person in the classroom. Please stand up and take a pen with you! Choose a friend with whom you would like to discuss your choices and sit together in pairs. You are allowed to discuss your decisions with your partner quietly, but you decide only for yourself! [with odd number of participants: the person that does not find a partner can fill in the first part alone!] In the lower part of your questionnaires you find red stickers with your questionnaire number. Please remove the sticker from your own questionnaire and put it on your partner's questionnaire, so we know who you worked with!]

We will now go through the **first part of the survey**. We will have breaks in between, so you can ask questions. Please raise your hand in case you have a question.

Part 1

In the first part you can earn money. The money will be paid out to you at the end of this lesson. For your participation you receive 3.10 Euros right now.

The amount of money you will have in the end depends on your decisions and on chance. **There are no right or wrong answers!** It is important that you fully understand the rules. In the first part you have to make 18 decisions. We will randomly select one of the decisions and actually pay it to you at the end of the lesson. You will not know which decision this will be before you have finished the part 1! Therefore, you have to think carefully about all your decisions, because every decision might be drawn.

One of you will draw a chip from this bag later on. In this bag there are 5 blue and 5 red chips. You have to decide how much money you receive if a blue chip or a red chip is drawn. Part 1 consists of two sheets with several decisions. You always have to decide between two options – option L (for left) and option R (for right).

Let's start with the first sheet! Here is an example. In the upper part of the sheet you can see how many chips of each color are in the bag. We take a look at the first decision you have to make.

	Option L				Option R
A1	**€2.00** for 🔵 **€1.00** for 🔴	☐	or	☐	**€2.41** for 🔵 **€0.20** for 🔴

If you choose option L, you receive 2.00 Euros if a blue chip is drawn and 1.00 Euro if a red chip is drawn. If you choose option R, you receive 2.41 Euros for a blue chip, but only 0.20 Euros for a red chip. If you prefer option L in A1, tick the left box. If you prefer option R, tick the right box. **Please do only tick one box per line!**

The other decision on the first sheet are similar. You have to make a decision for each line.

In the second line you again decide between L and R. In this line, however, you can win either 2.56 Euros or 0.20 Euros in option R. As you can see here, the amount of money you can win if a blue chip is drawn increases for option R. Option L always stays the same in this sheet.

After you have made all your decisions in a sheet, you have to summarise your decisions in the bottom part of the sheet.

Assuming that you want to coose option L for all decisions, you should tick the left box in each line. Then, you state that you choose option L for lines A1 to A11 in the bottom part of the sheet and cross out the line for option R.

It works the same way if you only want choose option R. You cross out the line for option L to indicate that you never want to choose L.

Assume that you choose option L for lines A1 to A4 and then option R from line A5 on. Then you write A4 into the empty box in the upper line and A5 in the lower line.

Please pay close attention now!

Most people only switch from option L to option R **once** at some point between the first line and the last line. As soon as you have ticked a box on the right side, you should stay with option R for all following lines.

Why is this the case?

Let's look at the second and third line. If you have chosen right in a previous line, it does not make sense to switch back to the left side in the following lines. Option L is equal in both lines. In option R the amount for a red chip is the same in both lines, but in the third line you receive 2.80 Euros for a blue chip. This is more than in the second line, where you only receive 2.56 Euros for a blue chip. Therefore, option R is better in the third line than in the seond line. Thus, if you alreday choose right in the second line, you should all the more choose option R is the third line! It is always the same if you compare two consecutive lines – the further you go down, the better option R gets. Therefore, you should tick option R if you have already chosen option R earlier!

Is this clear to everyone?

Sheet 2 is similar to sheet 1, only the numbers are different. If a red chip is drawn, you loose money in both options. In case this happens, you have to return money to us. Additonally, sometimes also the left option changes from line to line.

Let's look at the second sheet!

	Option L		or		Option R
B1	**€2.50** for ● (blue) **€-0.40** for ● (red)	☐	or	☐	**€3.00** for ● (blue) **€-2.10** for ● (red)

The decisions work the same way as in the first sheet. You decide how you want to receive or pay money for the chips. If you choose option L, you receive 2.50 Euros if a blue chip is drawn, but you have to return 0.40 Euros to us if a red chip is drawn. It works the same way with option R: for a blue chip you receive 3.00 Euros. If a red chip is drawn, you have to return 2.10 Euros to us.

As in sheet 1, we ask you to make a decision for each line. As before, you have to transfer your decisions into the summary in the end. As in sheet 1, cross out the lower line if you only want to shoose option L. Cross out the upper line if you only want to choose R.

If you choose option L to line B3, for example, and option R from line B4 onwards, then please indicate it in the bottom part of the sheet.

It works the same as in the first sheet!

Most people switch from left to right only once. As soon as you have ticked a box on the right side, you should stay with option R in all following lines.

Why is it like that?

Let's look at the third and the fourth line. It does not make sense to switch back to the left side in the lower lines if you have already chosen right before. Option L is equal in both lines. In option R the payment for a blue chip is equal in both lines. In line 3, however, you loose 2.10 euros for a red chip, which is more than in the fourth line, where you only loose 1.60 Euros. Thus, option R is definitely better in the fourth line than in the third line. If you have already chosen option R over option L in the third line, then you should all the more tick option R in the fourth line. It is always like that if you compare two consecutive lines. Thus, if you have chosen option R once, you should also choose option R in all following lines.

Is this clear to everyone?

Now we have to explain to you how you get your money. In the end of part 1 you have made 18 decisions, 11 in the first sheet and 7 in the second sheet.

When all of you have finished part 1, one of you can draw a card from these 18 cards. There is one card for each of your decisions – A1 to A11 and B1 to B7. The card that will be drawn counts for all of you. If you draw A1, the decision A1 is actually paid out **to all of you**.

Then one of you draws a chip from the bag. This chip determines the payment for all of you. If you draw A1 and blue, the ones who have chosen option L in A1 get 2.00 Euros and the ones who have chosen option R get 2.41 Euros. If you draw red, some get 1.00 Euro and others get 0.20 Euros.
As each of your 18 decisions can be drawn, you should think carefully for each line about whether you want to choose option L or option R!
Before you are allowed to start with part 1, you have to fill in the comprehension check on the first page. Please work on your own! We will go through the rows and check whether you have filled in the correct answers. Please do not start before we have given you [*DISCUSSION*: and your partner] the start signal. Remember that we have already given you 3.10 Euros!
[*TREATMENT*: You are allowed to discuss your decisions with your partner, but you choose for yourself and the money is only for you. Therefore, you do not have to make the same decision.]
[*CONTROL*: It is very important that you make the decisions on your own. This takes only a few minutes in part 1. Please be absolutely quiet now.]
Please raise your hand as soon as you have finished part 1, so we can collect it.

Part 2

Those of you who are done can start with part 2. Part 2 is a **questionnaire about yourself**. We are very interested in your own opinion, so please answer all questions on your own – without talking to your neighbours. Please try to answer every question as good as you can.
You have time for this until [5 minutes to the end of the lesson]. Do not take too long for a single question – at most one minute.
[Towards the end of part 2 in *CONTROL*: In the bottom of your sheets you find red stickers with your questionnaire number. We would like to look at your decsions and the decisions of one of your friends later. Please choose a friend and exchange your stickers!]
[5 minutes to end] You have three minutes left to answer the remaining questions. Please answer the questions on the sticker in any case, as we are specifically interested in these questions!
Thank you for your participation!

A.7 Decision sheets

Part I –
Your decisions around money

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Comprehension Check

Here we ask you to answer four questions to check whether you have understood the rules. Imagine that the following decision has been drawn and that you have decided as ticked here:

Z6	**€1.50** for 🔵 **€0.50** for 🔴	☐	or	☒	**€2.50** for 🔵 **€0.10** for 🔴

1. How much money do you **receive additionally** if 🔵 is drawn? . . . Euros
2. How much money do you **receive additionally** if 🔴 is drawn? . . . Euros
3. Inculding our thank-you, how much money do you have **in total in the end** if 🔵 is drawn? . . . Euros
4. Inculding our thank-you, how much money do you have **in total in the end** if 🔴 is drawn? . . . Euros

Which option do you choose?

Your payment for both options is determined by the draw of the chip from the bag, which contains 5 red and 5 blue chips:

🔴🔴🔴🔴🔴 🔵🔵🔵🔵🔵

	Option L				Option R
A1	**2.00 Euros** for 🔵 **1.00 Euros** for 🔴	☐	or	☐	**2.41 Euros** for 🔵 **0.20 Euros** for 🔴
A2	**2.00 Euros** for 🔵 **1.00 Euros** for 🔴	☐	or	☐	**2.56 Euros** for 🔵 **0.20 Euros** for 🔴
A3	**2.00 Euros** for 🔵 **1.00 Euros** for 🔴	☐	or	☐	**2.80 Euros** for 🔵 **0.20 Euros** for 🔴
A4	**2.00 Euros** for 🔵 **1.00 Euros** for 🔴	☐	or	☐	**3.09 Euros** for 🔵 **0.20 Euros** for 🔴
A5	**2.00 Euros** for 🔵 **1.00 Euros** for 🔴	☐	or	☐	**3.29 Euros** for 🔵 **0.20 Euros** for 🔴
A6	**2.00 Euros** for 🔵 **1.00 Euros** for 🔴	☐	or	☐	**3.54 Euros** for 🔵 **0.20 Euros** for 🔴
A7	**2.00 Euros** for 🔵 **1.00 Euros** for 🔴	☐	or	☐	**3.87 Euros** for 🔵 **0.20 Euros** for 🔴
A8	**2.00 Euros** for 🔵 **1.00 Euros** for 🔴	☐	or	☐	**4.31 Euros** for 🔵 **0.20 Euros** for 🔴
A9	**2.00 Euros** for 🔵 **1.00 Euros** for 🔴	☐	or	☐	**4.93 Euros** for 🔵 **0.20 Euros** for 🔴
A10	**2.00 Euros** for 🔵 **1.00 Euros** for 🔴	☐	or	☐	**5.85 Euros** for 🔵 **0.20 Euros** for 🔴
A11	**2.00 Euros** for 🔵 **1.00 Euros** for 🔴	☐	or	☐	**7.33 Euros** for 🔵 **0.20 Euros** for 🔴

Your choices

I choose option L for questions	A1	to	
I choose option R for questions		to	A11

Which option do you choose?

Your payment for both options is determined by the draw of the chip from the bag, which contains 5 red and 5 blue chips:

🔴🔴🔴🔴🔴 🔵🔵🔵🔵🔵

,

	Option L				Option R
B1	**2.50 Euros** for 🔵 **-0.40 Euros** for 🔴	☐	or	☐	**3.00 Euros** for 🔵 **-2.10 Euros** for 🔴
B2	**0.40 Euros** for 🔵 **-0.40 Euros** for 🔴	☐	or	☐	**3.00 Euros** for 🔵 **-2.10 Euros** for 🔴
B3	**0.10 Euros** for 🔵 **-0.40 Euros** for 🔴	☐	or	☐	**3.00 Euros** for 🔵 **-2.10 Euros** for 🔴
B4	**0.10 Euros** for 🔵 **-0.40 Euros** for 🔴	☐	or	☐	**3.00 Euros** for 🔵 **-1.60 Euros** for 🔴
B5	**0.10 Euros** for 🔵 **-0.80 Euros** for 🔴	☐	or	☐	**3.00 Euros** for 🔵 **-1.60 Euros** for 🔴
B6	**0.10 Euros** for 🔵 **-0.80 Euros** for 🔴	☐	or	☐	**3.00 Euros** for 🔵 **-1.40 Euros** for 🔴
B7	**0.10 Euros** for 🔵 **-0.80 Euros** for 🔴	☐	or	☐	**3.00 Euros** for 🔵 **-1.10 Euros** for 🔴

Your choices

I choose option L for questions	B1	to	
I choose option R for questions		to	B7

A.8 Additional Measurements

The following is a translation of the financial literacy questions that were asked in part 2 of the study. The financial literacy score is constructed by adding up the number of correct responses. A maximum of 13 points can be reached. For the verbal responses in situation 1, we had two research assistants independently judge the correctness of the response. Contradictory judgements were then settled by one of the authors.

Situation 1

Martin can buy tomatoes individually or in crates. 1 kilo of tomatoes individually cost 2.75 Euros. One crate with 10 kilos costs 22 Euros. Martin thinks: "The crate with tomatoes offers a better cost-benefit-ratio than the individual tomatoes."

Give one reason for this:

Buying one crate of tomatoes (10 kilos) could be a bad idea for some people. What could be the reason for this?

Situation 2

Imagine your grandmoter would like to save money. Potentially she needs to use the money on short notice, if her car breaks down and she needs to pay the repair. Which of the following investments would you recommend to her? (Please only mark one answer)

- ☐ Real estate (e.g. a house or apartment)
- ☐ Stocks
- ☐ Savings book

Situation 3

Chrstina wants to invest her savings and to take as little risk as possible. Which of the following investments would you recommend to her? (Please only mark one answer)

- ☐ Real estate (e.g. a house or apartment)
- ☐ Stocks
- ☐ Savings book

Situation 4

Anna wants to take up a new mobile plan. She can choose from two plans. Each month, she never sends more than 100 text messages, talks for a maximum of 100 minutes on the phone and surfs at most

100 minutes online. She has to pay the phone costs from her own money. Which alternative would you recommend to her?

- ☐ **Alternative A** (flatrate contract): For 40 Euros a month Anna can phone, text and surf online without limit.
- ☐ **Alternative B** (prepaid card to top-up): Anna gets a SIM card for free. With this plan, she can talk for 10 cents a minute, surf for 10 cents a minute and send texts for 10 cents per text.

Situation 5

Which of the following statements is correct? If you buy a stock of company B...

- ☐ ... you own a par of company B.
- ☐ ... you have lent money to company B.
- ☐ ... you are liable for the debt of company B.
- ☐ I don't know.

Situation 6

Marie has 100 Euros in her savings account. She gets 2% interest a year. Marie asks you, how much money she will have in her account after five years, if she leaves the full amount in the account for the whole time. What do you think?

- ☐ More than 102 Euros
- ☐ Exactly 102 Euros
- ☐ Less than 102 Euros
- ☐ I don't know.

Situation 7

If you buy 5 stocks from a single company, you get a safer return (profit) than if you invest the same amount of money into 5 different stocks (e.g. into an investment fund). Right or wrong?

- ☐ Right
- ☐ Wrong
- ☐ I don't know.

Situation 8

(Please mark the possibility that seems the most important to you) Buying something on credit...

- ☐ ... allows me to afford everything today that I want.
- ☐ ... gives me the freedom to decide myself what I can afford when.

☐ ... means getting into debt now in order to buy something now that I cannot afford right now.

Situation 9

Which ones are one-off costs and which are recurring costs in sports?

Tennis racket	☐one-off	☐recurring	costs
Monthly fee at the sports club	☐one-off	☐recurring	costs
10er ticketbook at the swimming pool	☐one-off	☐recurring	costs

Situation 10

Why do companies advertise?

☐ To sell their products better.

☐ Because the employees like to see their company on TV, in magazines etc..

☐ They produce funny advertisements in order to thank their customers.

☐ I don't know.

Furthremore, participants were asked about the degree of contact they have with the person they exchanged stickers with using the following question:

Degree of Contact

You and your best friend in your class (your current discussion partner), how often do you see each other outside of school?

☐ Daily

☐ Several times a week

☐ Once a week

☐ Less than once a week

☐ Never

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