

Assortative Matching and Social Interaction: A Field Experiment on Adolescents' Risky Choices *

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Abstract

We propose a novel empirical strategy to distinguish two sources of peer effects in risky choices: i) assortative matching in the formation of the peer relationship, and ii) social interaction effects arising in the choice situation. We combine network information on existing peer relationships with controlled randomised exposure to social interaction conditions. We apply this strategy in a field experiment on risky decision making among adolescents between ages 13 and 15. School classes were randomly allocated 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 a dyadic analysis, we find that friends and classmates are matched on socio-demographic characteristics but are not assortatively matched 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

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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; Duflo and Saez, 2003; Bursztyn et al., 2014; Dahl et al., 2014; Beshears et al., 2015). Yet, knowledge about the mechanisms behind peer effects is still limited. We propose a new experimental strategy that allows us to distinguish two sources of peer effects, an assortative matching (or sorting) channel that drives the formation and assimilation of peer relations and a social interaction channel, i.e., peer dynamics that arise in a specific choice situation.¹ These aspects have so far been studied in isolation.

Our experimental strategy combines 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. This approach has two main benefits. First, separating the sources of peer effects is informative for the design of policy interventions aimed at reducing adverse peer influence. If peer effects arise through assortative matching, interventions changing the distribution of peers in the classroom or workplace may be effective in improving behavioural outcomes. If, in contrast, peer effects arise to through social interaction, changing the choice architecture or designing interventions that change interaction patterns may be more successful in reducing adverse peer influence.

Second, our approach bridges the gap between experimental and non-experimental studies of peer influence, potentially avoiding estimation bias that could arise in either type of study. Experimental studies have focused largely on the social interaction channel, with interaction usually taking 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), drawing the criticism that “(...) 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” (Manski, 2000). Carrell et al. (2013) lend empirical support to this view, showing that social interaction patterns can vary substantially in spite of random assignment due to endogenous friendship formation after the assignment. We avoid this criticism through *random* exposure of natural peers, i.e., peers who have formed stable relationships prior to the experiment, to social interaction conditions at the class-level (as opposed to random matching of unacquainted subjects in the lab).

A crucial feature of our design is the combination of experimental data with information on existing peer relations in the control group. This non-experimental component allows us to study whether (endogenously formed) natural peer relations are assortatively matched (or sorted) based on similar risk preferences or other characteristics. Similar network information is often used in non-experimental approaches that 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).²

To summarize, subjects are randomly exposed to social interaction in our experiment, but they have

¹While assortative matching reflects endogenous peer formation based on similar individual characteristics, social interaction effects may for example arise from the exchange of information relevant to the choice or through social utility considerations.

²Similar points are made by Manski (1993), Moffitt (2001) and Lyle (2007).

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, leading to similarity in peers' choices even in the absence of ongoing social interaction. We separate these two effects.

We apply this experimental design to risky choices of adolescents which have long been thought to be particularly prone to peer effects (Arnett, 1992).³ We conduct the experiment among adolescents in the age range of 13 to 15 years that attend the lower tier of the German high school system.⁴ 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. Thus, our design creates random variation in *whether* and *with whom* subjects can interact in a choice situation involving risk and potential loss.

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 among friends or classmates. In our analysis of social interaction effects, we compare the choice similarity of paired teenagers in the communication treatments with similarly matched pairs in the control group that were not allowed to communicate.

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

³Adolescence 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) highlight 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)). In contrast, Eisenberg et al. (2014) find strong peer effects among first year College students in social activities like binge drinking but no evidence of peer influence in other risky behaviours such as illicit drug use, gambling, and sexual activity.

⁴About 53% of the secondary school age population in Berlin attend this school type (Statistisches Bundesamt, 2016).

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).

The following three key findings emerge. First, we find large variation in risk preferences among natural peers (i.e., friends and classmates) but in a dyadic analysis we do not find evidence of assortative matching on risk preferences – neither on risk nor loss aversion. At the same time, classmates (and friends) do match on observable socio-demographic characteristics such as age and gender and on family background characteristics. These findings seem to contrast with existing evidence on assortative matching with respect to risk preferences among adults, most prominently couples, e.g. Dohmen et al. (2012)⁵, but also in other social groups such as risk-sharing groups (Attanasio et al., 2012) and MBA classmates (Ahern et al., 2014). That we do not find assortative matching on risk preferences might be explained by the fact that we consider a younger population of adolescents, and friends and classmates rather than couples. Couples likely get larger economic benefits from having similar risk preferences so that we expect more assortative matching or assimilation. In line with this interpretation, Serra-Garcia (2018) shows that couples with heterogeneous risk preferences are more likely to divorce, while Di Falco and Vieider (2018) find that assimilation in risk preferences within a couple is strong while assortative matching is less important. Relatedly, Attanasio et al. (2012) find evidence of assortative matching in a group lending context where sorting by risk preferences is economically beneficial (for risk pooling arrangements). Also, Ahern et al. (2014) find that risk attitudes get more similar over the course of one academic year in a sample MBA classmates but the setting is one in which peer influences may interact with learning on the course as financial risk is one of the major components of the MBA curriculum.

Second, we find 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.⁶ 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.

⁵The evidence on assortative matching of couples may not extend to other preferences, as Shaner (2015) find only a rather weak correlation of time preferences among spouses.

⁶We discuss the interpretation of the estimated parameters in the different experimental conditions in Section 2.4. Additionally, we present all results in terms of adolescents choices of switching-points.

Previous studies of social interaction effects in risky choices have focused on adults (Bursztyrn et al., 2014; Fafchamps et al., 2015), and they abstract from the endogenous nature of pre-existing peer relations which we capture in the assortative matching channel. We find similar social interaction effects towards increased similarity in risk aversion among adolescents. Additionally, we analyse social interaction effects regarding losses, and find increased similarity in loss aversion.

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 experimental design allows us to draw important conclusions on the nature of peer influences, and distinguish between the assortative matching and social interaction channel. We find that in the stable peer network of the classroom and also among friends, a large heterogeneity in 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 existence of strong social interaction effects suggests that interventions designed to encourage informed and independent decision-making among adolescents may be a more effective policy avenue to limit adverse peer influence.

The remainder of the paper proceeds as follows. The experimental design is discussed in the context of our application in Section 2. We present the results from our experiment in Section 3. In the concluding Section 4, we discuss the implications of our findings and highlight the fact that our novel experimental design can be used to separate assortative matching and social interaction in the analysis of peer effects in other settings.

2 Experiment

We conducted the experiment in 7th and 8th grade classes in lower tier schools in Berlin in June 2014.⁷ First, students were given an incentivised experimental task involving lottery choices. Second, they completed individual questionnaires on cognitive ability, and socio-demographic characteristics.⁸

2.1 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.⁹ Before the visits,

⁷These 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.

⁸All 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.

⁹This design precludes observing initial conditions which would be helpful to identify heterogeneous social interaction effects. Yet, due to the usually close proximity of schools to the children's home locations, it is virtually impossible to

we randomly allocated each class to the control group or one of the two treatment arms.¹⁰ In control classes (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.¹¹ 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.¹²

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 timing¹³ and complexity constraints, we abstracted from probability weighting.¹⁴ 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.

2.2 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 classes, 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

observe school children prior to any social interaction. It would be necessary to observe them at primary school entry at the very latest.

¹⁰We conducted this lab-in-the-field experiment in collaboration with the charity My Finance Coach. Teachers who were interested in the charities’ financial education courses in schools were asked if they were willing to take part in the experiment. Randomization of classes to control and treatment groups was implemented through a web interface designed by the research team.

¹¹Paired students were seated next to each other at a desk during the experiment, and the desks of the pairs were separated. Pairs were allowed to communicate for about 10 minutes after instructions had been given. Then, the experimenters asked each subject to make their own choices. Paired students could copy each other’s choices but would not know the choices that subjects in other pairs made.

¹²We 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.

¹³Experimental sessions had to be conducted within a 45-minute school lesson.

¹⁴Typically, 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.

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.

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.¹⁵ 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 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.

2.3 Sample

Our sample consists of 235 teenagers from 12 classes in 5 schools, with between 14 and 20 students per class. All participants included in the sample provided parental consent to participate in the study. 17 participants made inconsistent choices.¹⁶ 5 participants made incomplete choices. 18 participants could not be matched with a partner.¹⁷ Excluding these subjects leaves us with 198 observations for the

¹⁵For example, if B1 and a red chip were drawn, and the participant had chosen lottery L, the payoff was €-0.40.

¹⁶12 teenagers on decision sheet A, 7 on decision sheet B, two of these on both.

¹⁷Reasons 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).

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.¹⁸ 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 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.¹⁹ Since we randomised at the class level, we cluster standard errors accordingly (Moulton, 1986).

Table 1: Descriptive Statistics

	TOTAL	CONTROL	RANDOM	FRIENDS	<i>p</i> -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	0.279	0.328	0.319	0.218	0.286
Household size	3.973	4.000	4.061	3.897	0.500
Migrant background	0.372	0.306	0.458	0.371	0.263
< 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.²⁰ 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; $p = 0.0696$).

¹⁸A similar table for the whole sample can be found in Appendix A.1.

¹⁹This variable is also used in PISA surveys as a proxy of important family inputs into education. See Hanushek and Woessmann (2011).

²⁰The phrasing of the question can be found in Appendix A.8.

2.4 Assignment of loss and risk aversion parameters

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

$$v(x) = \begin{cases} x^\sigma & \text{for } x \geq 0 \\ -\lambda(-x)^\sigma & \text{for } x < 0 \end{cases}, \quad (1)$$

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 $\sigma_i = 0.9$.²¹ 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.²² 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.

3 Results

3.1 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 2.4 to derive preference parameters from observed choices.²³ 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 2.579.²⁴ Looking across the distribution of

²¹If 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 $\sigma_j = 1.625$.

²²Appendix A.2 lists all choices and their associated σ parameter values, and an exemplary table for assigning λ parameter values.

²³For a summary of teenagers' choices, see Table 13 in Appendix A.3.

²⁴We 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).

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.

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; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

A large body of research 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). The prevalence of risk aversion among adults and adolescents has been firmly established (Glätzle-Rützler et al., 2015; Tanaka et al., 2010; Tymula et al., 2012; von Gaudecker et al., 2011). Moreover, several studies show that risk aversion increases with age in both experimental and self-reported survey measures (see, e.g. Gardner and Steinberg (2005); Dohmen et al. (2011)). Fewer studies provide evidence on the degree of loss aversion (Abdellaoui et al., 2008; Etchart-Vincent and L’Haridon, 2011), and none of these investigate adolescents with the exception of Glätzle-Rützler et al. (2015); they study (and do not find evidence for) *myopic* loss aversion, i.e., a combination of loss aversion and mental accounting, among adolescents. Our study appears to be the first to provide evidence for adolescents. We find that two thirds of adolescents are loss-averse; the average degree of loss aversion ($\bar{\lambda} = 2.579$) is similar to previous estimates obtained with adult samples. In Appendix A.4, we provide a more detailed comparison of our findings with risk preference estimates obtained in studies of adults and with one study of undergraduate students at a German university that used the same choice task as in the present paper.

Previous studies have also documented increased choice similarity (among adults) between the first and second mover in social interaction conditions, but they have been silent about the impact of peer interaction on the nature (or level) of risk taking. 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.²⁵

Since the choice independence assumption does not hold under social 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 differ-

²⁵Sequential designs which allow the experimenter to control the feedback mechanism, and the peer signals that the second-mover receives, are, however, important to understand who influences whom as they address the reflection problem. Simultaneous designs, which replicate the natural interaction between peers and allow for dynamic feedback, are in contrast suited to understand how peer interaction changes the riskiness of individual choices.

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

	CONTROL	RANDOM	FRIENDS	<i>p</i> -values		
				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; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

ence 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 λ 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 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%. In summary, the effects we find depend on the type of peer: 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).

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

	mean	median	5th	25th	75th	95th	% risk- (loss-) averse	<i>p</i> -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 for peer matching or sorting (among friends or classmates). Our focus is particularly whether friends and classmates display assortative matching regarding their risk and loss preferences (Section 3.2).

Second, we separate assortative matching (that influenced peer group formation or assimilation among peers before the experiment) 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 3.3. In Section 3.4, we provide additional evidence on the mechanisms behind the social interaction effects, and discuss extensions of our experiment.

3.2 Assortative matching in 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 may exhibit assortative matching in their choice of friends. Yet, assortative matching may be much weaker than among spouses who face large economic incentives to match closely, e.g. due to resource sharing within the household and joint economic decision-making. 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 between friends’ assortative matching (or selective sorting into classes) and subsequent convergence in preferences as relations deepen, i.e. assimilation. 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.²⁶ 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.²⁷ In consequence, we expect stronger similarity in preferences among friends. Additionally, friends may match assortatively, potentially including matching on risk and loss preferences.

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_c .

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

²⁶Note that we find less social contact among randomly matched classmates than among friends, so self-selection is meaningful: 36% of self-selected friends report that they meet outside of school at least once a week; among randomly paired classmates this proportion is only 29%.

²⁷Ahern 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.

$m_{ij} = m_{ji}$, we collapse identical pairs and adapt a symmetric dyadic approach. We specify the following model:

$$m_{ij} = \beta_0 + \beta_1|\sigma_i - \sigma_j| + \delta_1|\lambda_i - \lambda_j| + \eta_1|z_i - z_j| + \zeta_1|y_i - y_j| + \beta_2|\sigma_i + \sigma_j| + \delta_2|\lambda_i + \lambda_j| + \eta_2|z_i + z_j| + \zeta_2|y_i + y_j| + \mu_c + \epsilon_{ij} \quad (2)$$

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.²⁸ 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).²⁹ 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.³⁰ 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 is uneven or an adolescent in a pair did not provide parental 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.³¹ 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.³² The results with

²⁸We use absolute values as the match is undirected.

²⁹If 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.

³⁰The number of possible pairs depends on the class size as friends and classmates can by our design only be matched within, not across classes.

³¹The 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.

³²We 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).

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. To summarize, 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.

3.3 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 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.

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

	(1)	(2)	(3)
	$P(\text{friends} = 1)$	$P(\text{friends} = 1)$	$P(\text{friends} = 1)$
DIFFERENCES			
σ	-0.0371 (0.132)	0.310 (0.582)	0.345 (0.858)
λ	0.00275 (0.122)	-0.0519 (0.195)	-0.0904 (0.160)
Math grade (rel.)		0.251 (0.249)	0.255 (0.215)
Age		-0.688*** (0.194)	-0.805*** (0.305)
Male		-2.615*** (1.011)	-2.708** (1.196)
ln(household size)			-0.331 (0.699)
Single parent			-1.276* (0.658)
Migrant background			-0.107 (0.308)
< 25 books at home			-0.154 (0.130)
SUMS			
σ	0.129 (0.247)	0.0850 (0.254)	0.200 (0.374)
λ	0.0135 (0.0662)	0.0616 (0.137)	0.0985 (0.114)
Math grade (rel.)		0.0430 (0.143)	0.00297 (0.158)
Age		0.0499 (0.0679)	0.0725 (0.104)
Male		-0.345* (0.194)	-0.381 (0.241)
ln(household size)			0.0634 (0.216)
Single parent			0.486* (0.295)
Migrant background			0.143 (0.307)
< 25 books at home			-0.217 (0.372)
Constant	-2.934*** (0.586)	-3.203 (2.909)	-3.684 (3.593)
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)	(2)	(3)
	$P(\text{classmates} = 1)$	$P(\text{classmates} = 1)$	$P(\text{classmates} = 1)$
DIFFERENCES			
σ	-0.634 (0.750)	-0.691 (0.595)	-0.693 (0.545)
λ	0.0215 (0.0220)	0.0114 (0.0512)	0.0136 (0.0353)
Math grade (rel.)		-0.101 (0.0750)	-0.101 (0.0665)
Age		-0.571*** (0.177)	-0.576*** (0.186)
Male		0.165*** (0.0361)	0.166*** (0.0382)
ln(household size)			-0.00446 (0.0599)
Single parent			0.105 (0.0701)
Migrant background			0.0210 (0.102)
< 25 books at home			-0.114 (0.178)
SUMS			
σ	-0.0514 (0.464)	0.0230 (0.491)	0.0208 (0.450)
λ	-0.0306 (0.0513)	-0.0166 (0.0579)	-0.0164 (0.0690)
Math grade (rel.)		0.0260 (0.104)	0.0142 (0.128)
Age		0.175 (0.333)	0.189 (0.364)
Male		-0.0675 (0.174)	-0.0745 (0.199)
ln(household size)			0.0300 (0.0834)
Single parent			-0.0288 (0.185)
Migrant background			0.0431 (0.247)
< 25 books at home			-0.0368 (0.203)
Constant	-0.634 (1.642)	-5.002 (7.984)	-5.604 (9.047)
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

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

	Estimate	<i>p</i> -value	relative difference	mean value in <i>CONTROL</i>
$\Delta_{ij}(\sigma)$	-0.2151	0.090*	-0.53	0.4086
$\Delta_{ij}(\lambda)$	-1.4027	0.011**	-0.61	0.9073
Identical choice of <i>i</i> and <i>j</i> 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	0.025**	^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.

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 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 arise 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% in the control group.

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

	Estimate	<i>p</i> -value	relative difference	mean value in <i>CONTROL</i>
$\Delta_{ij}(\sigma)$	-0.2164	0.076*	-0.57	0.3820
$\Delta_{ij}(\lambda)$	-1.8043	0.008***	-0.86	2.0943
Identical choice of <i>i</i> and <i>j</i> 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Result 2: Choice similarity in the social interaction conditions

- a** Similarity in risky choices 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).

3.4 Additional mechanisms and confounding factors

Several mechanisms have been suggested as explanations for social interaction effects. Bursztyn et al. (2014) conduct a field experiment among friends and family members regarding the choice of a risky investment product, emphasising two mechanisms through which peers affect risky choices – social learning and social preferences. Bursztyn et al. (2014) describe 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.³³ Social preferences (“Keeping up with the Jones”) arise when one’s utility from making a risky choice depends directly on another individual’s choices.³⁴ Relatedly, Fafchamps et al. (2015) find that in a repeated game, subjects try to keep up with the winners.³⁵ 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; Bernheim, 1994).

³³Lerner and Malmendier (2013) find reverse peer effects among MBA students regarding entrepreneurship decisions. A higher share of entrepreneurial peers decreases entrepreneurship due to gradual learning of one’s entrepreneurial ability during peer exposure.

³⁴In Bursztyn et al. (2014), social utility is identified through learning about one’s peer’s chosen (rather than intended) investment, as first movers make investment choices but receipt of the chosen investment product is randomised.

³⁵See Trautmann and Vieider (2012) for a discussion of social utility aspects regarding behaviour under uncertainty.

In the following, we explore social learning based on the idea that it depends on the choice competency of the individual and her peer. In our study, we observe subjects' private information of their own quality 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. In a recent study, Ambuehl et al. (2018) conduct a social interactions experiment in the lab in which they exogenously vary financial information. They cannot address potential confounding effects through the assortative matching channel, but find stronger learning effects among communicating peers with low financial competence

Our study contains a financial literacy score derived from 12 questions³⁶ 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.³⁷ 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. Mean loss aversion among perfectly aligned pairs is 7.32 compared to 4.93 in pairs whose choices differ ($p=0.0011$; two-sided t -test); we do not find statistically significant differences in their mean risk aversion.

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 ("math") and peer differences in these measures.³⁸ 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

³⁶Our comprehensive measure of financial literacy based on 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.

³⁷We use their (undisclosed) literacy score as an external rather than self-assessed measure of their financial competency and assume that it summarises individuals' private information on their "quality".

³⁸There might be a concern that numeracy is not fully observable by classmates. We believe it is likely that math grades are public knowledge but we do not have data to support this claim. We should note, however, that we re-estimated the regressions reported in Table 9 with cognitive ability and obtained results that are almost identical (not reported).

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.

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 choices 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 being made among classmates who coordinate. Overall, we find that in a sample of adolescents, social learning effects are sensitive to peer closeness. This result is consistent with findings from related studies in psychology and economics.

As we noted in the introduction, there is a large literature on risk-taking among adolescents in psychology much of which focuses on excessive risk-taking with regard to sexual behaviour and substance abuse. Also, Gruber (2001) examines a wide array of adolescent risky behaviours such as smoking, drinking and other substance abuse as well as reckless driving and teen pregnancies, and finds a high variability in these behaviours which is not observable among adults. He suggests that these observations are consistent with either more “emotional (hot affective) decision-making by youth” or with “multiplier effects through peer influence”. Recent studies provide evidence that excessive risk-taking in these domains does not stem from adolescents having risk-seeking preferences, but rather from a larger risk tolerance in ambiguous situations – in which the likelihood or payoffs to “winning” and “losing” are unknown, see Tymula et al. (2012). This higher ambiguity tolerance has been linked to adolescence as a phase of increasing independence characterised by the encounter of unfamiliar situations. At the same time, the neurological development of the brain continues in adolescence, with improving executive function and impulse control, and eventually leading to more rational behaviour. Together with some resolution of ambiguity through learning, this might lead to a reduction of risky behaviours (Tymula et al., 2012). Relatedly, Andreoni et al. (2019) provide evidence that executive functions among children, and soft skills among adolescents, are negatively associated with risk taking.

Peers have also been shown to play an important role. Bursztyn et al. (2014) argue that social utility (i.e. “Keeping up with Joneses”) is important in shaping peer effects among adults. Social utility effects among adolescents could also take the form of a social insurance motive: classmates might make very loss averse choices for fear of ending up losing more of their initial endowment relative to their peers. This motive may be stronger among classmates than friends due to differences in the level of trust. Bursztyn

Table 9: Determinants of perfect choice alignment

	<i>RANDOM</i>	<i>FRIENDS</i>
Financial literacy	-0.239 (1.99)**	0.068 (0.91)
$\Delta(\text{Financial literacy})$	-0.076 (0.34)	-0.110 (1.51)
Math	0.236 (0.88)	-0.060 (0.34)
$\Delta(\text{Math})$	-0.706 (2.11)**	0.269 (1.51)
Male	-0.260 (0.55)	-0.590 (1.78)*
$\Delta(\text{Male})$	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;

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

and Jensen (2015) analyse adolescents’ educational investments under experimental conditions that vary the observability of performance and the nature of the peer group to which this information is revealed. The hypothesis is that when “effort is observable to peers, students may try to avoid social penalties by conforming to prevailing norms”. They find that peer social concerns can increase or decrease educational investments, i.e. effort, depending on the peer group setting and observability of performance. Blakemore (2018) argues that for adolescents, the social risk of being rejected by peers outweighs other potentially negative outcomes of decisions, such as threats to one’s health or the prospect of getting caught. Finally, Lieber and Skimmyhorn (2018) find that choice observability matters for the emergence of peer effects in financial decision making among adults.

Further research is certainly needed to shed light on the mechanisms behind such social interaction effects. Nevertheless, in light of the existing evidence, one interpretation of our findings is that if social risk is crucial for teenagers’ decision-making, then this may lead to excessive risk-taking in some domains and loss-averse behaviour in others. That is, in situations that are about having had an experience or taking part in an activity (sex, drugs, parties etc.), a teen may engage in the behaviour earlier or more intensively than without peer pressure in order to fit in and not be “left behind”. In situations like ours where performance is observable to the direct peer and partially observable to others in the class (as in Bursztyn and Jensen (2015)), this may lead to more loss-aversion in order to not be the one with the lowest pay-off.

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.

4 Summary of findings and discussion

In this paper, we presented an experimental strategy that separates two sources of peer effects, assortative matching (the formation of peer relations before an economic choice is made) and social interaction effects (which arise in the choice situation). 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 friends.

We find that adolescent friends assortatively match on socio-economic characteristics like age, gender, and family background. However, we find no evidence that they match on their preferences regarding risk or loss. This result suggests that well-documented and strong assortative matching patterns in observables might not extend to the (usually unobserved) domain of preferences. Yet, it is in line with recent empirical evidence for spousal relationships which finds strong assimilation effects over time but weak assortative matching in terms of risk and time preferences (Shaner, 2015; Di Falco and Vieider, 2018).

Our second 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. We conclude that 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.

More research is needed to understand the mechanisms behind the strong social interaction effects that we observed in our study. We highlight three potential channels. First, increased choice similarity among pairs might reflect social learning (see, for example, Bursztyn et al. (2014)). 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. In contrast, a positive public signal of financial competency increases the likelihood of coordination among peers. Our data allow also us to re-examine the hypothesis that the nature of the peer relation matters (Carrell et al., 2013) and find evidence in its favour.

Social learning appears to be an crucial driver of peer effects. Together with findings by Angrist and Lang (2004), this result has an important implication for policies aimed at reducing adverse peer group effects. A policy that disturbs existing peer groups, say by busing students from one residential area to another as in the intervention studied by Angrist and Lang, might not be very effective as it reduces primarily those peer group effects that are due to assortative matching but not those that arise from social interaction. Results on peer interactions from a recent literature in psychology (e.g., Reniers et al. (2017)) might be useful in designing interventions that target this channel.

A related second mechanism might be social utility effects – 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. 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 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. A third potential mechanism worth exploring is 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.³⁹ 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 (but not friends) make more loss averse choices under peer influence but do not seem to systematically change their risk taking. These findings are consistent with higher levels of trust among friends than among classmates.

A noteworthy feature of our data is that half of these 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. 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.⁴⁰

The experimental strategy we propose in this paper is widely applicable beyond the classroom; it is suitable in any setting 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. 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. Natural extensions 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.

³⁹There 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.

⁴⁰In 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).

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 choices⁴¹ 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; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

⁴¹We 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 responses 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

Switch	Option L		Option R		$\Delta_{LR}(EV)$	σ range		Assigned σ
	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 assignend by extrapolating distance from neighboring category's midpoint to respective category

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

Switch	Option L		Option R		$\Delta_{LR}(EV)$	λ range		Assigned λ
	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 assignend 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	<i>p</i> -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, $p = 0.023$).⁴²

⁴²This 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 3.2, 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*

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

	$P(\text{classmates} = 1)$			
	(1)	(2)	(3)	(4)
	CONTROL	CONTROL & RANDOM	CONTROL	CONTROL & RANDOM
DIFFERENCES				
σ	-0.699 (0.583)		-0.688 (0.498)	
λ	0.00426 (0.0715)		0.0146 (0.0429)	
Math grade (rel.)	-0.108 (0.140)	-0.118 (0.0748)	-0.107 (0.111)	-0.111 (0.0808)
Age	-0.612*** (0.156)	-0.382*** (0.143)	-0.615*** (0.166)	-0.385*** (0.128)
Male	0.163*** (0.0557)	0.0117 (0.0900)	0.163*** (0.0560)	0.0133 (0.0864)
ln(household size)			-0.0117 (0.0420)	-0.0461 (0.0345)
Single parent			0.0859 (0.0725)	-0.155 (0.267)
Migrant background			0.0385 (0.0835)	-0.313 (0.324)
< 25 books at home			-0.0466 (0.130)	-0.0226 (0.0740)
SUMS				
σ	0.00422 (0.495)		-0.00239 (0.453)	
λ	-0.0121 (0.0444)		-0.0214 (0.0700)	
Math grade (rel.)	0.0223 (0.0793)	0.00450 (0.0255)	-0.00352 (0.143)	-0.00639 (0.0416)
Age	0.189 (0.335)	0.0617 (0.206)	0.207 (0.384)	0.0714 (0.224)
Male	-0.101 (0.120)	0.0121 (0.119)	-0.110 (0.151)	0.00191 (0.102)
ln(household size)			0.0371 (0.112)	0.0280 (0.0546)
Single parent			-0.0760 (0.216)	0.0389 (0.143)
Migrant background			-0.00896 (0.250)	-0.0506 (0.152)
< 25 books at home			-0.0797 (0.263)	-0.0613 (0.213)
Constant	-5.273 (7.967)	-2.601 (5.813)	-5.888 (9.997)	-2.689 (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		or		Option R
A1	€2.00 for ● €1.00 for ●	<input type="checkbox"/>		<input type="checkbox"/>	€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 choose 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 second line. Thus, if you already choose right in the second line, you should all the more choose option R in 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 ●	<input type="checkbox"/>		<input type="checkbox"/>	€3.00 for ●
	€-0.40 for ●				€-2.10 for ●

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 choose 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 decisions 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

Prof. Dr. Joachim Winter
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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 ●	<input type="checkbox"/>	or	<input checked="" type="checkbox"/>	€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. Including our thank-you, how much money do you have **in total in the end** ... Euros
if ● is drawn?
4. Including our thank-you, how much money do you have **in total in the end** ... Euros
if ● is drawn?

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			or		Option R	
A1	2.00 Euros for <input type="checkbox"/> 1.00 Euros for			<input type="checkbox"/>	2.41 Euros for 0.20 Euros for	
A2	2.00 Euros for <input type="checkbox"/> 1.00 Euros for			<input type="checkbox"/>	2.56 Euros for 0.20 Euros for	
A3	2.00 Euros for <input type="checkbox"/> 1.00 Euros for			<input type="checkbox"/>	2.80 Euros for 0.20 Euros for	
A4	2.00 Euros for <input type="checkbox"/> 1.00 Euros for			<input type="checkbox"/>	3.09 Euros for 0.20 Euros for	
A5	2.00 Euros for <input type="checkbox"/> 1.00 Euros for			<input type="checkbox"/>	3.29 Euros for 0.20 Euros for	
A6	2.00 Euros for <input type="checkbox"/> 1.00 Euros for			<input type="checkbox"/>	3.54 Euros for 0.20 Euros for	
A7	2.00 Euros for <input type="checkbox"/> 1.00 Euros for			<input type="checkbox"/>	3.87 Euros for 0.20 Euros for	
A8	2.00 Euros for <input type="checkbox"/> 1.00 Euros for			<input type="checkbox"/>	4.31 Euros for 0.20 Euros for	
A9	2.00 Euros for <input type="checkbox"/> 1.00 Euros for			<input type="checkbox"/>	4.93 Euros for 0.20 Euros for	
A10	2.00 Euros for <input type="checkbox"/> 1.00 Euros for			<input type="checkbox"/>	5.85 Euros for 0.20 Euros for	
A11	2.00 Euros for <input type="checkbox"/> 1.00 Euros for			<input type="checkbox"/>	7.33 Euros for 0.20 Euros for	

Your choices

I choose option L for questions to

I choose option R for questions to

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			or		Option R	
B1	2.50 Euros for <input type="checkbox"/> -0.40 Euros for <input type="checkbox"/>			<input type="checkbox"/>	3.00 Euros for <input type="checkbox"/> -2.10 Euros for <input type="checkbox"/>	
B2	0.40 Euros for <input type="checkbox"/> -0.40 Euros for <input type="checkbox"/>			<input type="checkbox"/>	3.00 Euros for <input type="checkbox"/> -2.10 Euros for <input type="checkbox"/>	
B3	0.10 Euros for <input type="checkbox"/> -0.40 Euros for <input type="checkbox"/>			<input type="checkbox"/>	3.00 Euros for <input type="checkbox"/> -2.10 Euros for <input type="checkbox"/>	
B4	0.10 Euros for <input type="checkbox"/> -0.40 Euros for <input type="checkbox"/>			<input type="checkbox"/>	3.00 Euros for <input type="checkbox"/> -1.60 Euros for <input type="checkbox"/>	
B5	0.10 Euros for <input type="checkbox"/> -0.80 Euros for <input type="checkbox"/>			<input type="checkbox"/>	3.00 Euros for <input type="checkbox"/> -1.60 Euros for <input type="checkbox"/>	
B6	0.10 Euros for <input type="checkbox"/> -0.80 Euros for <input type="checkbox"/>			<input type="checkbox"/>	3.00 Euros for <input type="checkbox"/> -1.40 Euros for <input type="checkbox"/>	
B7	0.10 Euros for <input type="checkbox"/> -0.80 Euros for <input type="checkbox"/>			<input type="checkbox"/>	3.00 Euros for <input type="checkbox"/> -1.10 Euros for <input type="checkbox"/>	

Your choices

I choose option L for questions to

I choose option R for questions to

A.8 Additional Measurements

The following is a translation of the financial literacy questions that were asked in part 2 of the study. These are a combination of standard financial literacy questions, and questions that we developed for and validated in a separate school experiment in 2013 (see Lührmann et al. (2018)). The four standard financial literacy questions follow the design by Lusardi and Mitchell (2014). They elicit numeracy regarding interest compounding, inflation, risk diversification and the definition of a share. Furthermore, we use two financial literacy questions from the 2012 PISA assessment of financial literacy. These capture the value of money (PISA, 2012). Additional questions elicit whether students are able to identify liquid and low risk assets, whether they are able to distinguish between fixed and running costs (e.g. in a mobile plan), the concept of debt, and firm's advertising motives. 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 grandmother 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

Christina 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 | <input type="checkbox"/> one-off | <input type="checkbox"/> recurring | costs |
| Monthly fee at the sports club | <input type="checkbox"/> one-off | <input type="checkbox"/> recurring | costs |
| 10er ticketbook at the swimming pool | <input type="checkbox"/> one-off | <input type="checkbox"/> 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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