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Learning and Peer Effects

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ABSTRACT

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Peer effects are possibly very important for educational performance but hard to identify. This paper confirms the existence of peer effects in a learning process with data from an experiment. The experimental approach circumvents key econometric problems which greatly restrict the analysis of educational peer effects with administrative or survey data. The experimental setting offers some insight into the mechanisms of peer interaction. The results show that prospective cooperation has a motivational effect. There is no evidence with respect to an optimal group composition. The benefit from the pair treatment is largely independent of the characteristics of the partner.

Keywords: learning, peer effects, experiment, economics of education

JEL-Classfication: C90, C92, I20

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1 Introduction

It is a long-standing hypothesis that the performance of a student depends on the behaviour and characteristics of his fellow students. The existence and properties of these peer effects influence the welfare implications of educational policies which affect the (self-)selection of students into different learning groups (Arnott and Rowse, 1987). Therefore, Rothschild and White (1995) describe education as a customer-input-technology. However, the identification of peer effects in field data is very difficult. Econometricians invest great effort to identify quasi-experimental evidence on peer effects from administrative or survey data. The ideal data set would observe the same individual at the same time in the same environment but with a different peer. Such a data set is not available. Hence, researchers actually face a choice. Either they take non-experimental data from surveys or administrations and turn them into quasi-experimental evidence as far as possible. Almost all contributions in the literature have followed this part. Or they generate data in a real experiment with key characteristics of a real world learning process.

In this paper I take this second path which is complementary to the econometric approach. The experiment circumvents four problems which typically restrict the analysis of peer effects with field data:

(i) Students are not randomly assigned to their peer groups. Parents, schools or administrations decide where students enrol. Such a selection process precludes the identification of a plausible counter-factual result.[1]

[1] Gould et al. (2005), Hoxby (2000), and Ammermüller and Pischke (2006) for example rely on differences in the compositions of individual classes. These authors estimate what would happen if a student was in a different class. Hanushek et al. (2003) and McEwan (2003) use variation within peer groups over time to identify peer effects. Stinebrickner and Stinebrickner (2006), Foster (2006), Zimmerman (2003) and Sacerdote (2001) investigate

(ii) Teacher behaviour and other environmental characteristics can change with the peer group composition. The same teacher may teach the same topic in a different way, if the average ability or the ability distribution changes in a class (Meier, 2004)[2].

(iii) Measures for peer effects have to be exogenous. The reflection problem (Manski, 1993) could be ignored if an independent, and relevant, measure for each student was available. Yet most datasets do not have such a variable.

(iv) Even with a satisfactory dataset the mechanisms of peer effects are difficult to identify. Survey or administrative data largely provide a 'peer group composition effect'. This effect does not explain *how* students influence each other and what happens if students do not cooperate at all.

The experiment in this paper covers a learning process. The subjects face a task on which they can improve over time. Some subjects can improve with a randomly assigned partner (pair treatment), others have to do it alone (single treatment). The assignment to the treatments is random as well. Environmental conditions do not change over time and there is no teacher. Hence, any difference in final performance between both treatment groups at the end of the experiment derives from the differences in the treatment, i.e. the peer effect.

Beside this identification of a peer effect, the experiment allows an insight into the mechanisms of peer interaction. Firstly, a peer can provide an 'instructional' advantage. The

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peer effects in universities in a different way. They use the random assignment of students into university dormitories as indicators for social tie formation and its subsequent impact on educational performance.

2 Arguably, such an effect is part of a peer effect. One could distinguish between a direct peer effect, where students directly influence each other, and an indirect one, where students influence each other via the teacher.

subjects explain the task to each other, for example. This instructional argument is a standard assumption in models of educational production[3]. The advantage typically differs with the composition of the peer group. High ability students are assumed to provide a greater advantage, for example. The anticipation of this advantage can provide motivation even if the motivational mechanism does not exist[4]. Secondly, a partner can motivate even if the learning technology of single and pairs do not differ significantly. People do not want to appear as being a lazy, for example. This aspect has largely been ignored in the literature on the economics of education. Falk and Ichino (2006) find experimental evidence for positive peer effects in a "real task", non-cooperative production environment. In their case the peer effect stems from the mere presence of another person in the room.[5] A large psychological literature emphasizes the role of peers as benchmark for the development of academic self-concepts. An academic self-concept captures the self-assessment of individual capabilities for a specific

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3 Lazear (2001) focuses on disadvantage from fellow students via the interruption of instruction, but this does not change the logic of the argument. Lazear's approach provides a connection between peer group composition and peer group size. In his model educational output is maximized if each student is alone with a teacher.

4 A motivational factor can derive from the fact, that the abilities of the partners are complementary inputs. In this case, the marginal gain from investing effort is lower in the single treatment groups, where the "partner" has an effective ability of zero. An anonymous reviewer provided this helpful comment. For the motivational aspect of prospective educational technology improvements see also the model set-up in De Fraja and Landeras (2006)

5 Falk and Ichino relate their approach to the "social facilitation paradigm", a research topic in the psychological literature (e.g., Zajonc, 1965, Cottrell et al., 1968, or more recently Feinberg and Aiello, 2006).

academic subject[6]. Again, this motivational function can vary with the peer group composition.

Evidence for the second, purely motivational mechanism is provided by a pre-treatment performance test. The subjects know if they will cooperate or not but cooperation has not yet taken place. A second performance test after the treatment provides information about the instructional benefit. The performance in the second test, controlled for performance in the first test, provides information about the benefit from the actual cooperation.

The results show a significant peer effect. Subjects with a partner performed better in both tests. The results provide evidence for the motivational mechanism since the subjects with a prospective partner are already better in the first test than those subjects in the single treatment group. I also find evidence for the instructional effect but this evidence is not entirely robust. The results provide only little evidence with respect to an optimal group composition. The benefit from the pair treatment is independent of the characteristics of the partner.

The paper is organized as follows. The next section describes the experimental design. Section 3 contains the hypotheses and section 4 presents the results. Section 5 discusses the results and section 6 concludes.

6 Festinger (1954) initiated this literature, and Marsh (1987) captured it as the Big-Fish-Little-Pond-Effect. A student thinks he is good in, say, math, just because all others are rather bad. Köller (2004) provides a summary of this psychological literature.

2 The Experiment

2.1 Design of the experiment

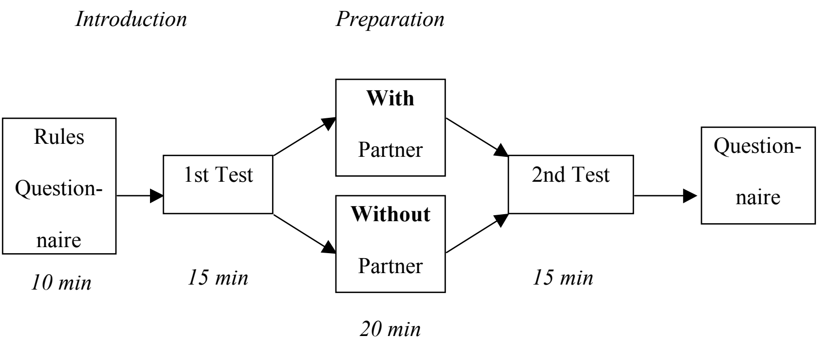
The objective of the experiment is to identify if and how learning partners affect the performance of a subject in a learning process. Two different mechanisms of peer effects are investigated: Firstly, a motivational mechanism can exist. A partner induces a subject to provide more effort before cooperation takes place. Secondly, partners support (or distract) each other directly once they cooperate. This direct benefit is called the instructional mechanism. Both mechanisms may depend on the composition of the peer group, i.e. the characteristics of the partner.

The implementation of the peer effect design is rather straightforward (see figure 1). The subjects faced a learning process for a specific task. The task was a largely unknown logical puzzle called Kakurasu which will be described in greater detail below. The participants received some general rules (see appendix) and then faced a first test about the task. The test measured how many puzzles the subjects solved in 15 minutes. I use the test to identify motivational differences between the treatment groups.

After the first test, subjects could prepare for 20 minutes for a second test. In this preparation period the treatment took place. Some subjects had to prepare alone (single treatment), others with a partner (pair treatment). The subjects learned at the beginning of the experiment about their treatment and the prospective partner. During the preparation period students got some further puzzles on which they could work.

After the preparation period the second test started. Again, the test measured how many puzzles the subjects solved in 15 minutes. However, the puzzles were more difficult in this test. Questionnaires were handed out before and after the experiment.

Figure 1: The design of the experiment



The existence of peer effects is confirmed if the treatment groups differ significantly in the performance in the second test. The experiment allows distinguishing between the different mechanisms of peer interaction. If the treatment groups differ in the first test, a prospective partner induces higher effort, even if he does not provide support. In this case the motivational mechanism works. If the differences between treatment groups are significant even after controlling for individual performance in the first test, we have evidence for benefits from actual cooperation (the instructional mechanism). The composition of a learning group matters if the performance of a subject in the pair treatment group increases or decreases with some exogenous characteristics of the partner.

It is clear that the motivational and the instructional mechanisms interact. Improved prospective marginal productivity from cooperation can induce higher motivation. On the other hand, higher marginal productivity from higher motivation can induce a higher benefit from cooperation. The latter implies that a simple differences-in-differences estimation is inappropriate for an identification of the instructional mechanism. The former implies that motivation has several sources. Prospective marginal productivity is a plausible source but not necessarily the only one. The experimental set-up cannot distinguish between these different sources. A feasible experimental set-up would not be compatible with the professional standards on deception in experimental economics (see below).

2.2 The Task

In the experiment the participants can learn a solution strategy for a logical puzzle called Kakurasu (see appendix[7]). This type of puzzle consists of a matrix in which the correct fields have to be marked. Numbers at each line and column of the matrix allow deriving the logically correct solution.

The task satisfies several criteria. Subjects can learn the logic of the puzzle within reasonable time. The puzzle can vary in difficulty by increasing or decreasing the size of the matrix. Unlike the famous Sudoku the puzzle is largely unknown. Hence many subjects start from zero[8].

The matrices in the first and second tests are of different size (4x4 and 5x5 respectively). This variation is used to avoid floor or ceiling effects in the learning process. The experiment has been pre-tested. Here, the matrices were of identical size in both tests, to show if subjects actually improve. Using 5x5 matrices for puzzles in the first test caused a floor effect. Only few people actually managed to solve a single puzzle. On the other hand, 4x4 matrices for puzzles in the second test are too easy to solve. Many participants did not improve from test 1 to test 2 (a ceiling effect). In the preparation period, the subjects got 4x4, 5x5 and 6x6 puzzles.

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[7] A detailed description of the puzzle can be found at www.janko.at (in German). The owners of the website created the meaningless name for the puzzle.

[8] Similar puzzles exist on dedicated homepages on the internet. Hence, it is likely that some participants have an advantage even though they do not know the puzzle.

2.3 The Procedure

The first experiment was conducted on the 5th of December in 2006 with 85 Swiss students at a public high school (Kantonsschule) in Kreuzlingen in the Canton of Thurgau in Switzerland. A second experiment took place on 26th February, 2007 with 61 students from a similar school in Romanshorn in the same canton. The Canton groups students according to performance into different schools. Both schools roughly capture the top 15% of the students in their hometown.

The students were recruited from the top three grades (age 15-18) through the school's intranet. Students responded via E-Mail and provided information about their grade and sex. Each participant got 20 Swiss Francs (about 12.40 € or 16.25 US$ in December 2006) for their participation. 48 participants were assigned to the single treatment and 96 to the pair treatment group. The subjects were assigned randomly to the different groups. Each subject in the pair treatment group got a randomly assigned partner, but only from the same class level and sex. Due to missing partners at the experiment, three pairs were formed with subjects from different grades. Table 1 shows the composition of single treatment and pair treatment groups.

All subjects did the experiment at the same time to ensure that students could not communicate solution hints to following students. In Kreuzlingen, the subjects did the experiment in five different rooms in the school. Two rooms were filled with single learners, three rooms with the pair treatment group. The differences across rooms within a specific treatment group are insignificant. In Romanshorn, the students were separated in two different rooms according to their treatment. All the students received their instructions in standardized oral and written form from the author of this paper. Each room contained either 20 or 40 persons. One person per 20 participants was in charge of the technical details (i.e. two persons were in each large room). These supervisors received instructions about the procedure of the experiment but not about the puzzle. The participants were explicitly told that the overseer could not answer questions with respect to the puzzle.

Table 1: The distribution of the subjects into single treatment and pair treatment groups

	Single treatment group			Pair treatment group		
Grade	Male	Female	Sum	Male	Female	Sum
2	11	10	21	27	18	45
3	8	7	15	19	10	29
4	7	6	13	16	6	22
Sum	26	23	49	62	34	96

2.4 Discussion of the Design

The design of the experiment is without precedent in the economic literature. I did not find anything similar in other disciplines like sociology, psychology or education. The closest relation is the one provided by Falk and Ichino (2006). These authors just observe peer effects in a simple production task with fixed wages. The experiment in this paper covers a learning process and distinguishes between different mechanisms of peer effects.

The random assignment of subjects into different treatment groups ensures that self selection does not play a role. All subjects face the same problem. Within the pair treatment the groups are of identical size. While verbal interaction between partners was allowed during the preparation period, all subjects faced the same conditions during the tests and the analysis is based on the results of these tests.

The results of the experiment are likely to change with the task, the subject pool or the pay-out formula. The floor and ceiling effects identified in the pre-tests support this presumption. A change in the result does not imply that the design is inappropriate. The fixed payment keeps the set-up as simple as possible. If financial incentives were the only source of motivation the results in the first test should not differ within and across treatment groups. Since students know each other and cooperate face to face in the preparation period, financial incentives could induce some of them to promise post-experimental rewards or punishments to elicit the desired level of cooperation. Furthermore, the results from Gneezy and Rustichini (2000) show that the impact of financial incentives on performance in tasks with a great cognitive demand is not trivial. Hence, a much larger experimental setup would be necessary which would extend beyond the objectives of this paper. Finally, Deci and Ryan (1985) claim that human beings' "need for competence" is a source of intrinsic motivation equivalent to basic needs for autonomy and social relatedness. This assumption implies that subjects with different marginal productivity will provide different test results. The experiment allows to investigating if it is too simplistic.

The experimental design identifies the instructional and the motivational benefits derived from peers. All subjects were informed at the beginning of the experiment about their treatment and – in the pair treatment – their prospective partner. I use econometric analysis to distinguish between the instructional and the motivational mechanism. Using the performance in the first test as a control variable eliminates the motivational differences between the treatment groups. Any resulting difference between the treatment groups derives from the availability of a cooperation partner. A different way to identify the instructional mechanism is also possible: Two treatment groups (single and pairs) in which subjects learn about their treatment only after the first test. The econometric analysis provides a much more parsimonious identification (in terms of sample size) for a result which is unlikely to differ significantly.

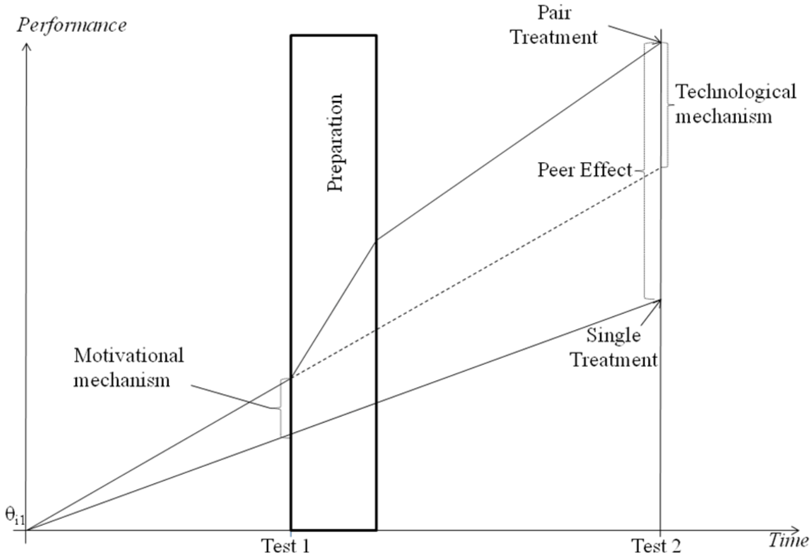
These alternative treatment groups would not add any insight into the sources of motivation. The experiment can identify motivational differences between the treatment groups. It cannot necessarily distinguish if this motivation derives from having a partner per se (e.g. for reasons of social control) or from the anticipation of instructional benefits and the resulting increased marginal productivity. Note that the performance in the first test of subjects in this new treatment group is not necessarily the same of those in the single treatment with ex-ante information about the treatment. The latter group knows that they will never have a partner in the experiment, while the subjects in the former one may correctly anticipate cooperation in the preparation period. Strictly speaking, two other treatments would be necessary: One single treatment in which students are deceived with the information that they will have a partner in the preparation period and one pair treatment in which students are deceived with the information that they will have no partner in the preparation period. Such a treatment is obviously not compatible with the standards of experimental economics.

3 Behavioral Expectations with Positive Peer Effects

Figure 2 illustrates the expectations in a stylized way. The lower straight line shows how subjects improve over time in the single treatment group. The subjects in the pair treatment benefit from cooperation, if peer effects are positive. Therefore, their marginal productivity is greater throughout the experiment. In the preparation period the curve is particularly steep because here the instructional benefit from cooperation kicks in. Before the preparation period (i.e. in Test 1) subjects are motivated by the prospective cooperation. In Test 2 the subjects reap the rewards from the cooperation. The dotted line indicates the outcome if motivation is not caused by the instructional benefits from cooperation[9].

[9] Of course, the instructional and the motivational mechanism are not necessarily mutually exclusive.

Figure 2: The two mechanisms of peer effects and their impact on performance



4 Results

I present OLS estimations in the subsequent econometric analysis. The performance is measured by counting the number of solved puzzles. Therefore, Poisson regressions or negative binomial regressions seem to be plausible alternatives, depending on the dispersion. The analysis has been conducted with all three methods and differences are reported whenever they appear. A comparison between the results indicates that OLS regressions are the most conservative method with respect to significance levels for testing the propositions.

A first estimation model shows a difference in the first test between the treatment groups (Table 2). I estimate output on a treatment dummy for the pair treatment (Model 1 in the table). The results show a significant treatment effect even after controlling for heterogeneity between the groups with respect to sex (0 = female, 1 = male), the school grade (*Grade:* 2, 3, or 4) and a school dummy (all in Model 2).[10]

The prospective cooperation has a positive impact on the subjects. The subjects were asked at the beginning of each test to rate on a scale from one to five how difficult they find the puzzle. Controlling for this item pushes the *p*-level to 0.001. Further controls for marks in math and general preferences for logical puzzles supported this evidence. These latter results are also robust after including further control variables as well as for both male and female participants and the different schools.

Table 2: Estimation of differences between the treatment groups in the first test.

OLS: N=145, dependent variable: *firsttest*; coefficients (robust standard error)		
	Model 1	Model 2
Treatment	1.071 (.476)*	1.203* (.477)
Grade		.014 (.266)
Sex		.786^ (.445)
School		.918* (.428)
Constant	3.408 (.399)	2.576** (.903)
R^2	.0361	.0842
*Significance levels: ***=.001, **=.01, *=.05, ^=0.1*		

However, it is not clear at this point if the motivation is derived from the anticipation of an instructional benefit or if it is ‘purely motivational’ (i.e. independent of prospective

10 No incident of cheating was observed during the test, which could have driven the result. The correlation between the first test score of a subject in the pair treatment and the test score of his prospective partner is .01, which underlines this observation.

productivity). Therefore the next step in the analysis is the identification of the instructional mechanism. A standard differences-in-differences estimation does not clearly distinguish the motivational mechanism from the instructional one. Differences in differences between the treatment groups do not just derive from the instructional mechanism but also from motivational differences in the first period. These differences in the first test can put subjects on a different "growth path"[11].

Hence, the instructional mechanism is identified if subjects in the pair treatment perform better in the second test than single learners, even after controlling for performance in the first test. The first test as a control variable eliminates the motivational differences in the first test. Since the conditions in both tests are equal across treatment groups, any performance difference can only derive from the benefits of cooperation instructional mechanism. Figure 2 provides a graphical illustration for the argument.

Table 3 shows the results. Model 1 provides the overall peer effect. However, this significant effect is driven to some extent by the results in the first test (Model 2). The resulting treatment effect is hardly significant on a 10% level. It is insignificant once differences in the group composition are accounted for (Model 3). In the model, the performance in the first test has a direct impact on the marginal productivity and – via this direct impact – an indirect one on the supply of inputs. Hence, the impact of the first test is non-linear. Model 4 finds a significant treatment effect once a non-linear impact of the first test ($firsttest^2$) is taken into account[12].

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[11] This is why the straight lines in figure 2 are not parallel. A differences-in-differences estimation would require a specific assumption, i.e. that motivation provides an initial mark-up in performance but does not affect marginal productivity otherwise.

[12] The p-value for the treatment effect is .093 if model 4 is estimated with both *firsttest* and $firsttest^2$.

Table 3: Estimation of differences between the treatment groups in the second test.

OLS: N=145, dependent variable: *secondtest*; coefficients (robust standard error)				
	Model 1	Model 2	Model 3	Model 4
Treatment	.946*** (.334)	.441^ (.263)	.427 (.277)	.519^ (.278)
Firsttest		.472*** (.051)	.468*** (.052)	
Firsttest²				.055*** (.006)
Grade			.155 (.159)	.153 (.154)
Sex			-.156 (.266)	-.212 (.269)
School			.266 (.270)	.339 (.261)
Constant	2.429*** (.265)	.820*** (.222)	.369 (.545)	.892 (.542)
R^2	.0508	.4384	.4487	. 4558
*Significance levels: ***=.001, **=.01, *=.05, ^=0.1*				

The final focus is on optimal peer group composition. At first I test for so-called *cognitive* peer effects, i.e. if performance increases in the ability of the partner. The following analysis uses data from the 96 subjects in the peer treatment group. I took several cognitive measures. The score of the partner in the first test (*partnerscore*) does not show any significant effect (Model 1 in Table 4) even as a nonlinear measure[13]. Other independent predictors have been taken from the questionnaires, e.g. the partner's math grade in school (*mathmark*) or his interest in logical puzzles (*sudokupeer*). Only the partner's interest in logical puzzle has a small significant effect (Model 2) but it vanishes once it is controlled for the school mark (p-value: .100). No differences across the different subgroups are observable. There is some evidence for cognitive peer effects in the first test score via the math grade of the partner.

[13] Even if it was significant, it would be affected by Manski's (1993) reflection problem.

These effects are small and insignificant for the final performance. The final questionnaire included questions about the quality of cooperation, how subjects liked their partner, if they were in the same class etc. None of these variables has a significant impact.

Table 4: Estimation of the cognitive peer effect

OLS: N=96, dependent variable: *secondtest*; coefficients (robust standard error)			
Partnerscore	.085 (.062)		
Sudokupeer		.845 (.445)^	.775 (.466)^
Mathpeer			.019 (.020)
Firsttest	.477 (.070)***	.494 (.071)***	.505 (.074)***
Grade	.363 (.208)^	.392 (.205)^	.397 (.204)^
Sex	-.214 (.329)	-.086 (.339)	-.150 (.347)
School	-.065 (.355)	-.097 (.357)	-.090 (.360)
Constant	-.045 (.648)	-.526 (.761)	-.806 (1.151)
R^2	.4060	.4223	.4273
*Significance levels: ***=.001,**=.01,*=.05, ^=0.1*			

Hence, there is no evidence for cognitive peer effects. In fact, the experiment does not allow any conclusion about the dominance of any particular group composition policy.

5 Summary and Discussion

This paper confirmed the existence of peer effects in a learning context with a field experiment and allowed some insight into the mechanisms of peer interaction. The experimental approach circumvents key econometric problems which greatly restrict the analysis of peer interaction with administrative or survey data. The results show that prospective cooperation has a motivational effect. The additional "instructional" benefit from actual cooperation is not significant across all specifications. Furthermore, the benefit from cooperation is independent of the characteristics of the partner.

A simple variation of the experiment may change this outcome. The actual treatment starts 25 minutes after the experiment has begun. Reducing this time decreases the performance in the first test and enhances the relative importance of the treatment period for the success in the second test. Increasing the complexity of the task (e.g. by changing the puzzles in the first test to a 5x5 matrix) can have a similar effect because it takes longer to "digest" the logic behind the puzzle. Evidence from the different schools supports this consideration. The students in one school perform worse in the first test than their colleagues in the other one, i.e. the task is, on average, more difficult for them[14]. Estimating the third model in Table 4 separately for each school documents a significant treatment effect in the school with the lower performance (p-value = .033) and an insignificant one in the better school. Variations in the timing may also provide some evidence about the optimal composition of learning groups.

The experiment identified a motivational mechanism for peer effects which is independent of the marginal productivity. This result does not question the assumption that utility increases in final performance. It indicates that the utility function is more complex than the model assumes. Adjusting pay to performance might attach more weight to the final performance measure and provide stronger evidence for the instructional mechanism. However, as stated above, such an identification strategy is not trivial and would require a much more complex experimental set-up.

An almost unlimited amount of possible further experimental variations exist and some of these variations can have an impact on the results. This is a phenomenon which is not

14 The p-value in a two-sample t test is .0438. See also Table 2.

unknown in the econometric literature on peer effects[15]. It is a nice feature of the experimental approach that future research allows to generate data which provide precise information about the impact of any variation.

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15 Take for example, Table 1 in Ammermüller and Pischke (2006)

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Appendix A: Introductory letter

The participants in the different treatment groups received different introductory letters. The appendix merges both letters. The participants did not know anything about the treatment in the other group. The letter is translated from German.

Dear participant,

You take part in an research project about the learning of logical puzzles. The experiment will go on for about one hour. Thank you very much for your participation.

We will test you twice in the experiment, at the beginning and at the end. In both tests we investigate, how many puzzles you can solve within a specific time.

Instruction for single treatment group:

Please put your solution for each puzzle in the solution sheet only.

Between both tests you have twenty minutes to learn more about the logic of the puzzle. You get a sheet with more puzzles to do so.

Please work alone during the entire experiment.

Instruction for pair treatment group:

Please solve these tests alone.

Please put your solution for each puzzle in the solution sheet only.

Between both tests you have twenty minutes to learn more about the logic of the puzzle. You get a sheet with more puzzles to do so. In **this** period you can cooperate with another participant. The seat number of your partner is on the sheet with your own number.

Instruction for both groups:

We ask you to answer two questionnaires, one at the beginning and one at the end of the experiment. We ensure the anonymity of the results. You will get 20 Francs once you hand in the last questionnaire at the end.

You will find the rules for the puzzle on the next sheet. Please read them carefully. We cannot answer any questions in order to keep the conditions identical for all participants. Please do not worry if you do not understand the rules immediately. It is neither uncommon nor embarrassing.

Yours sincerely.

Appendix B: Rules (Translated from German)

Your task in the test: Solve as many puzzles as possible by marking the correct boxes in the puzzle.

The composition of a puzzle:

We can explain it with the help of the following example:

	5	**6**	**1**	**2**	
3					**1**
7					**2**
1					**3**
2					**4**
	1	**2**	**3**	**4**	

The puzzle is surrounded by numbers. These are very important. The numbers at right boundary and at lower boundary assign values to the different boxes.

Please bear in mind:

Each box has two different values!

1. At the right boundary you find the values for each box in the respective row.

	5	6	1	2	
3	1	1	1	1	1
7	2	2	2	2	2
1	3	3	3	3	3
2	4	4	4	4	4
	1	2	3	4	

All boxes in row one have value 1.

All boxes in row two have value 2.

All boxes in row three have value 3.

All boxes in row four have value 4.

2. At the lower boundary you find the values for each box in the respective column.

	5	6	1	2	
3	1	2	3	4	1
7	1	2	3	4	2
1	1	2	3	4	3
2	1	2	3	4	4
	1	2	3	4	

All boxes in the first column have value 1

All boxes in the second column have value 2

All boxes in the third column have value 3

All boxes in the fourth column have value 4

Here you find the values for each box again. (row value / column value).

	5	6	1	2	
3	1 / 1	1 / 2	1 / 3	1 / 4	1
7	2 / 1	2 / 2	2 / 3	2 / 4	2
1	3 / 1	3 / 2	3 / 3	3 / 4	3
2	4 / 1	4 / 2	4 / 3	4 / 4	4
	1	2	3	4	

All boxes in row one have value 1.

All boxes in row two have value 2.

All boxes in row three have value 3

All boxes in the first column have value 1.

All boxes in the second column have value 2.

All boxes in the third column have value 3

3. The numbers at the *left* boundary show the sum of the values of all marked boxes in the respective *row*.

In this case, the values at the *lower* boundary are the relevant values.

The sum in row one is 3

The sum in row two is 7

The sum in row three is 1

The sum in row four is 2

	5	6	1	2	
3					1
7					2
1					3
2					4
	1	2	3	4	

4. The numbers at the *top* boundary show the sum of the values of all marked boxes in the respective *column*.

In this case, the values at the *right* boundary are the relevant values.

The sum in column one is 5

The sum in column two is 5

The sum in column three is 1

The sum in column four is 2

	5	**6**	**1**	**2**	
3					**1**
7					**2**
1					**3**
2					**4**
	1	**2**	**3**	**4**	

Now you have to mark the boxes, ensuring that both the values on the top and at the left boundary are correct.

Our example has the following solution

	5	6	1	2	
3			X		1
7	X	X		X	2
1	X				3
2		X			4
	1	2	3	4	

Is the solution correct?

In *row one,* the sum of all marked boxes has to be 3. Only the third box is marked, its value is 3 (see lower boundary). Hence, the solution in this row is correct.

In *column one*, the sum of all marked boxes has to be 5. The second and the third box are marked: 2+3=5 (see right boundary). The solution in this column is correct.

Indeed, the marked boxes ensure a correct solution for the values on the top and the left.

Row one:	3 = 3	Column one:	5 = 2+3
Row two:	7 = 1+2+4	Column two:	6 = 2+4
Row three:	1 = 1	Column three:	1 = 1
Row four:	2 = 2	Column four:	2 = 2

For the sums in each **row**, take the marked boxes in this row with the corresponding values at the **bottom**.

For the sums in each **column**, take the marked boxes in this column with the corresponding values at the **right**.

If you do not understand the solution at the moment, it is no problem. Go through the solution once. Or maybe the following test can help you. Good luck.

Appendix C: The Questionnaires (translations from German)

Questions in the first questionnaire:

- Sex (Boxes: male and female)
- How old are you?
- What are your preferred subjects in school? Note up to three
- What are your three best subjects in school?
- What was your math mark in last year's final certificate?
- What was your overall average mark in last year's final certificate?
- Do you like to go to school? (Boxes: yes and no)
- Do you often prepare for exams with other students? (Boxes: yes and no)
- Can you explain difficult topics successfully to others (e.g. solution for difficult mathematical tasks)? (Boxes: yes and no)
- Do you get along with most of your fellow students? (Boxes: yes and no)
- Do you like to solve logical puzzles, e.g. Sudoku? (Boxes: yes and no)
- Are you member in a club? (Boxes: yes and no)
- If yes, what type of club is it?

Questions ahead of each test

- Do you understand the rules of the puzzle? (Boxes: yes and no)
- What do you expect? Will you solve the puzzles easily or with difficulty? (5 Boxes, very easy to very difficult)
- Will your performance in this test be above or below the average performance of the other participants? (5 Boxes, much below average to much above average)

Questions in the final questionnaire:

- Compared with your expectations, did you find the puzzles easier or more difficult to solve. (5 Boxes, much easier to much more difficult)
- Do you expect to perform better than the average? (5 Boxes, much worse to much better)
- How was the quality of cooperation with the partner? (5 Boxes, very good to very bad)
- Was your partner helpful during the preparation? (5 Boxes, very helpful to not helpful at all)
- Did you know your partner before the experiment? (Boxes: yes and no)
- Are you in the same grade? (Boxes: yes and no)
- Are you in the same class? (Boxes: yes and no)
- Does your partner live in your vicinity? (Boxes: yes and no)
- Do you prepare occasionally with him for classes or exams? (Boxes: yes and no)
- How much do you like your partner ? (5 Boxes, very much to not at all)
- Did you like the experiment? (Boxes: yes and no)
- Are you interested in the results of the experiment? (Boxes: yes and no)
- Do you want to participate in similar experiments in the future? (Boxes: yes and no)

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