How and when can economic skills enhance cooperation?

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Abstract

Conventional wisdom has it that economic training and education tends to produce less cooperative people – where cooperation means following group-oriented goals. This issue has attracted particular attention in discussions of the current economic crisis where it was asked if increasing marketization of societies has created an environment encouraging amoral selfish behavior of financial intermediaries and other economic agents. We provide some evidence against this claim with the help of an experiment, using an investment game with a public-goods character. Modest guidance of strategic abilities increases the degree of cooperation if the institutional setting permits

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reputation building. We thus conclude that economic practice can enhance cooperation in a socially stable environment.

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D02
D03

1 Introduction

There is a long-standing debate in the social sciences on the effect of economic practice and education on individual pro-social behavior.[1] The basic hypothesis of this discussion is that – if it indeed causes any effect – economic training will make individuals more selfish and will, thus, reduce individual willingness to cooperate. In particular, the development of the current economic crisis has been attributed to the dissemination and social acceptance of selfish behavior, which has been alleged to be "economic" behavior.

This issue – does economic education makes people uncooperative and selfish – has been addressed in a number of field and laboratory experiments. Some find evidence that Economists (or Business people) are more selfish than others (see, for instance, Carter and Irons (1991), Frank, Gilovich, and Regan (1993 and 1996), Marwell and Ames (1981), and Selten and Ockenfels (1998)). Other papers find evidence of just the opposite (Ahmed (2008), Laband and Beil (1999), Stanley and Tran (1998), and Yezer, Goldfarb, and Poppen (1996)). Still, the focus of all these discussions is on long-term exposure to economic thinking, as the individuals of interest had received a university education in economics or business. The adoption of an economically coined attitude towards social interaction as a result of education could not, however, be identified. A selection bias could also be part of an explanation; some think that more selfish people tend towards an economic education or profession (for instance Frank and Schulze (2000) and Frey and Meier (2005)).

This approach thus explains only partly why other social scientists complain about a general "economic imperialism" in social issues and a tendency towards marketization of our society in general. This contagion with "economic" behavior can only be explained if the mere encounter with economic ideas also produces uncooperative and selfish behavior. To provide evidence,

[1] We use the two terms "economic practice" and "economic education" to distinguish between short-term and long-term exposure to economic thinking – practice as some sort of "training" here indicates a short time period.

we need to evaluate the effects of a short-term exposure to economic thinking. We do so by means of an experiment on cooperative behavior.

A public-goods game is a classical way to test the willingness to cooperate in an experimental frame (see, for a classic description, Andreoni (1988) and Keser and van Winden (2000)). In these games all players can contribute to a joint project. As individual contributions benefit all players, while costs are borne by the contributor, the game-theoretical model predicts an underinvestment problem. Laboratory experiments typically show that participants contribute more than the model predicts but less than would be socially desirable. Experiments also show that contributions are nearer the social optimum if the game is repeatedly played within the same group of players. In such a scenario players can build reputation for cooperative behavior.

We use a two-player investment game with a public-goods character to test to factors: the importance of an institutional setup allowing for reputation building and short-term economic training. We ask if players' average contributions move closer to the model prediction or closer to the social optimum if people are guided to learn the model prediction (Nash-equilibrium play) as well as the socially desirable outcome.

We played the game with students from Economics, Business, and other Social Sciences as well as Life-Science programs. To analyze the effects of economic guidance and reputation building, we designed four different treatments based on the original public-goods game. The treatments differed in the pre-play preparation of the participants and in player matching: in the so-called *basic treatment* we only checked if participants had understood the rules of the game; in the *economic-practice treatment* participants learned how to deduce the Nash-equilibrium of the game as well as the socially desirable outcome. Participants played the game 20 times. Both scenarios used both, a so-called *partner design* where player pairs were the same in all 20 rounds, and a *random stranger design* where pairs were matched anew in each round. The game played in the random stranger design can be seen as a repeated one-shot game without the possibility to build reputation while the partner design allowed for reputation building.[2]

The experiment produces results that are – at first sight – astonishing. Economic training actually can improve individual willingness to play cooperatively, that is, to choose contributions that are substantially higher than the Nash-equilibrium level. This effect only occurs, however, if reputation building is possible. If subjects have no chance to build a reputation as being reliable, economic training had no effect – neither positive nor negative – on individual behavior.

Our results thus suggest that it is not the exposure to economic thinking that makes people behave non-cooperatively. The driving effect for cooperative behavior is clearly the possibility to build reputation, as previous work has already suggested. However, in a stable environment, economic training can actually help to improve cooperation as it teaches people to consider the opportunity cost of selfish short-run behavior.

2 Experimental design

We are interested in understanding how short-term economic guidance affects player behavior in an investment game that has public-goods aspects. To that end we have conducted a series of computer-based laboratory experiments with four different treatments using z-Tree (Fischbacher (2007)).

All treatments are based on a bilateral game wherein each player (named A and B) can contribute to a joint project with revenue $R(\alpha, \beta)$ where α denotes A's nonnegative investment and β that of B. Each player receives half of the project revenue. We assume $R(\alpha, \beta)$ to be twice differentiable, increasing, and concave in investments. Contributions are relation specific and

[2] In addition we also tested a fifth treatment in which the participants only learned how to deduct the Nash equilibrium of the game without any information on the first-best investment level (*strategic-treatment*). This treatment was played in *partner design* and the results were very similar to the results of the *economic-practice treatment.* The results of the *strategic-* and *economic-practice treatment* only differed in the pace participants learned how to reach the optimal investment level.

exhibit mutual positive externalities on each partner's productivity. That is, $R(\alpha, 0) = R(0, \beta) = 0$, $\frac{\partial R}{\partial \alpha}|_{(0,\beta)} \gg 0$, $\frac{\partial R}{\partial \beta}|_{(\alpha,0)} \gg 0$, and $\frac{\partial^2 R}{\partial \alpha \partial \beta} = \frac{\partial^2 R}{\partial \beta \partial \alpha} > 0$. Investments are costly and costs must be borne by each player privately, where the cost function $c(\cdot)$ is increasing and convex. Given this structure, investments are contributions to a public good within the relationship.

The total profit from cooperation is $\pi = R(\alpha, \beta) - c(\alpha) - c(\beta)$. Accordingly, first-best (cooperative) investments are characterized by $\frac{\partial R}{\partial \alpha} = \frac{\partial c}{\partial \alpha}$ and $\frac{\partial R}{\partial \beta} = \frac{\partial c}{\partial \beta}$.

As revenue from the final product will be split equally between both partners and each partner must cover his investment cost, a free-riding problem arises. Individually rational levels of investment are characterized by $\frac{\partial R}{\partial \alpha} = 2\frac{\partial c}{\partial \alpha}$ and $\frac{\partial R}{\partial \beta} = 2\frac{\partial c}{\partial \beta}$. Due to concavity of the production function, these investment levels are lower than first-best levels.

As other authors have shown (for example the early works of Andreoni (1995), and Ledyard (1995)), players in an experiment can be expected to choose investment levels between the cooperative and the individually rational level. Which investment level they will choose depends on the institutional set-up of the experiment – in our case, the preparation the players have to go through before they start playing. Thus, to make differences resulting from different treatments visible and to avoid focal points in the investment pattern, we designed the production problem such that cooperative and individually rational investment levels were at a considerable distance. The revenue and cost functions we used in all treatments were: $R(\alpha, \beta) = 54.1(\alpha\beta)^{0.29}$, $c(\alpha) = 2\alpha$, and $c(\beta) = 2\beta$. Accordingly, first-best investment levels are given by $\alpha = \beta = 134.87$ and individual rational levels by $\alpha = \beta = 25.89$.

Based on these payoffs we designed four different treatments that share a fundamental structure: Each subject played a 20-round repeated game with simultaneous investment decisions in each round; (interim) payoffs were determined based on the above revenue and cost functions. To provide participants in our experiment with a clear representation of the strategic situation, we reduced the continuous investment problem to a discrete one where participants could choose investment levels of $0, 10, 20 \ldots, 200$ points. Each participant received a table that named his/her payoff based on both players' contributions in this round.[3] In addition, participants had positioning devices – made from colored paper – to support readability of the table. In the discrete game $\alpha = \beta = 130$ points were first-best investments and $\alpha = \beta = 30$ points as well as $\alpha = \beta = 20$ points were individual rational investment levels. In each round participants had a budget of 200 points which could not be transferred between rounds. Participants chose their contribution by use of an input mask. When both players had entered their investment level, the computer program displayed both participants' contribution, revenue and costs as well as both players' payoffs.

Participants' (final) monetary payoffs were based on individual results in 5 randomly drawn rounds of the game which were drawn at the end of the experiment and displayed together with the outcomes and payoffs of all rounds. Each experimental point was valued at 0.05 Euro. All participants received a show-up fee of 2.50 Euro. In sum, the average payoff for a participant was 9.60 Euro including the show-up fee. The average duration of an experiment session was 49 minutes; the average remuneration per hour was 11.75 Euro. The lowest remuneration (including show-up fee) was 4.00 Euro and the highest 13.50 Euro.

The treatments differed in two ways to test the effects of training and reputation (see Table 1): some of the subjects played a so-called *partner design*. Here subjects were matched into random but fixed and anonymous pairs that played 20 rounds of the investment game. Subjects in the partner design knew that they were playing against the same participant every round. Another group of subjects (larger due to the needs of the matching procedure) played a so-called *random-stranger* design. Here pairs were randomly

[3] To avoid framing effects that stress the cooperative nature of the game, we abstained from the usage of cooperation-related wording. The expressions "investment", "partner", and "joint" have not been used throughout the experiment. Instead we used "contribution to a project" and "the other participant". The experiments were conducted in German; all translations in this text are as close as possible to the German original.

matched in each of the 20 rounds out of a fixed and anonymous group of 6 participants. Subjects in the random-stranger design knew that they would face other participants in each round.

	Partner Design	Random Stranger Design
Basic Treatment	48 Participants	72 Participants
Economic-Practice Treatment	46 Participants	120 Participants

Table 1: The four different treatments

Both ways of matching players were combined with two different kinds of pre-play preparation of the subjects: A *basic treatment* and an *economic-practice treatment*.[4] In the basic treatment the reading of the instructions was followed by a computer-aided test of participants' comprehension of the payoff table. This test ensured that participants knew how to read the payoff table but did not hint at strategic considerations. The 20-rounds repeated game started after all participants had completed this test. The economic-practice treatment differed from the basic treatment by a short training following the comprehension test. This practice comprised questions like "What would be your payoff if you contribute 60 points and the other participant contributes 200 points?" followed by the question "Assume you contribute 60 points. Which contribution should the other participant choose to reach the highest pos! sible payoff?" All questions hinted at best responses to develop a possible dynamic towards the Nash-equilibrium. Additionally, three questions centered on cooperative investments were asked: "Assume you could write a binding contract on contributions with the other participant. Which contributions would you choose if you were interested in the highest-possible payoffs for both of you?", "What is your payoff if you and the other participant choose a contribution of 130?" and finally "Assume you expect the

[4]The German instructions are available upon request.

other participant to contribute 130 points. Which contribution would you choose if you were interested in the highest-possible payoff?" The experiment was then conducted as in the basic treatment.

The number of participants was 48 for the basic treatment/partner design pattern (matched into 24 pairs), 46 (23 pairs) in the economic-practice treatment/partner design, 72 in the basic treatment/random-stranger design (participants were grouped into 12 groups of 6 individuals) and 120 (20 groups) in the economic-practice treatment/random-stranger design.

We conducted the experiments with students at Philipps-Universität Marburg. Most of the 286 subjects majored in business administration or economics (67%); the others mostly majored in social sciences or humanities, with a small fraction from the life sciences. Fifty-two percent of the subjects were female and 9% were not native German speakers. A minority of the subjects (16%) had been exposed to game theoretic thinking before or had participated in experiments (5%).

3 Results

Previous work suggests that the majority of investments should range between the Nash-equilibrium and the cooperative contribution. We augment this hypothesis by the claim that reputation and training will shift the distribution of investments towards the cooperative level. To get a first idea if this enlarged hypothesis is sound, we begin with a descriptive statistics of the outcomes. Figure 1 shows the relative frequencies of different investment levels in all rounds ordered by treatments.

We see that reputation has the expected effect. In the two random-stranger treatments the majority of investments is distributed around the Nash-equilibrium levels with a clear peak at the Nash equilibrium at 30 points. In contrast, the two partner treatments that allow for building a reputation as a reliable partner show two peaks: One around the individually rational (Nash-equilibrium) investment level and one around the first-best (cooperative) investment level

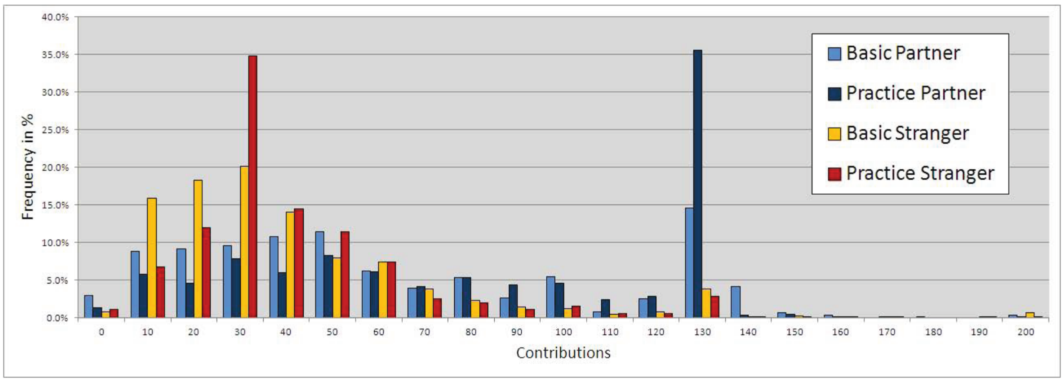

Figure 1: Relative frequencies of individual contributions

at 130 points. Economic training thus has an effect on individual behavior when reputation building is possible.

Table 2 shows that economic training not only induces more investments near the cooperative level, but also increases the general level of investments. While the frequency of very small contributions (0 to 50 points) is smaller in the basic treatment/partner design than in the economic-practice treatment/partner design, the frequency of investments between 60 and 110 points as well as those between 120 and 140 points increased. The shift shows that training not only impacts players who are willing to choose perfectly cooperative behavior but also raises the investments of those players who are rather inclined to play non-cooperatively.[5]

Table 2 and Figure 2 also show that the time structure of investments is impacted by economic practice in the partner design. The last rounds 17-

[5]We have chosen four categories of payments: 0-50 points which are very low and close to the Nash equilibrium; 60-110 points which is clearly bigger than the individually rational contribution but is still significantly smaller than the cooperative investment and therefore considered as non-cooperative; ! 120-140 which is close to or at the first-best level; 150-200 which is an overinvestment that rarely happens. To set the separation between cooperative and non-cooperative behavior at 110 points, is arbitrary. However, we wanted to make sure that behavior only gets labeled as “cooperative” if it really is. Furthermore, the results stay qualitatively unchanged if the line is drawn at 100 points.

Contributions	Rounds					
	1-20	1-4	5-8	9-12	13-16	17-20
Basic Partner Treatment						
0-50	52.9%	40.1%	44.3%	49.5%	59.9%	70.8%
60-110	24.4%	38.0%	35.4%	20.8%	14.6%	13.0%
120-140	21.3%	19.8%	19.8%	29.2%	23.4%	14.1%
150-200	1.4%	2.1%	0.5%	0.5%	2.1%	2.1%
Practice Partner Treatment						
0-50	33.7%	28.3%	24.5%	29.9%	32.1%	53.8%
60-110	26.8%	35.9%	33.2%	26.1%	21.2%	17.9%
120-140	38.7%	34.8%	41.8%	42.9%	45.7%	28.3%
150-200	0.8%	1.1%	0.5%	1.1%	1.1%	0.0%
Basic Stranger Treatment						
0-50	77.2%	53.8%	74.7%	78.8%	86.5%	92.0%
60-110	16.8%	32.3%	18.8%	17.7%	10.8%	4.5%
120-140	4.7%	11.1%	4.5%	3.1%	2.4%	2.4%
150-200	1.3%	2.8%	2.1%	0.3%	0.3%	1.0%
Practice Stranger Treatment						
0-50	80.7%	58.8%	87.1%	86.0%	89.6%	91.0%
60-110	15.2%	30.4%	18.8%	11.5%	8.3%	6.9%
120-140	3.5%	9.6%	2.5%	2.1%	1.7%	1.7%
150-200	0.6%	1.3%	0.6%	0.4%	0.4%	0.4%

Table 2: Aggregated relative frequencies of contributions

20 show in all treatments investments that are near the Nash-equilibrium level. The play here clearly suffers from an endgame effect. Before this endgame effect kicks in, players seem to learn how to play cooperatively as the frequency of first-best investment levels increases from 34.8% in the first four rounds to 41.8%, with 42.9% to 45.7% increase in the later ones.
None of these effects can be seen in the random-stranger treatments. The relative frequencies of contributions around first-best investment levels (reported in Table 2) remain on a very low level throughout all rounds while the frequency of investments around the Nash-equilibrium increases quickly in both treatments to levels beyond 75%. Accordingly, no endgame effect can be found in the random-stranger treatments. Thus, the random-stranger design can be considered as a repeatedly played one-shot game.

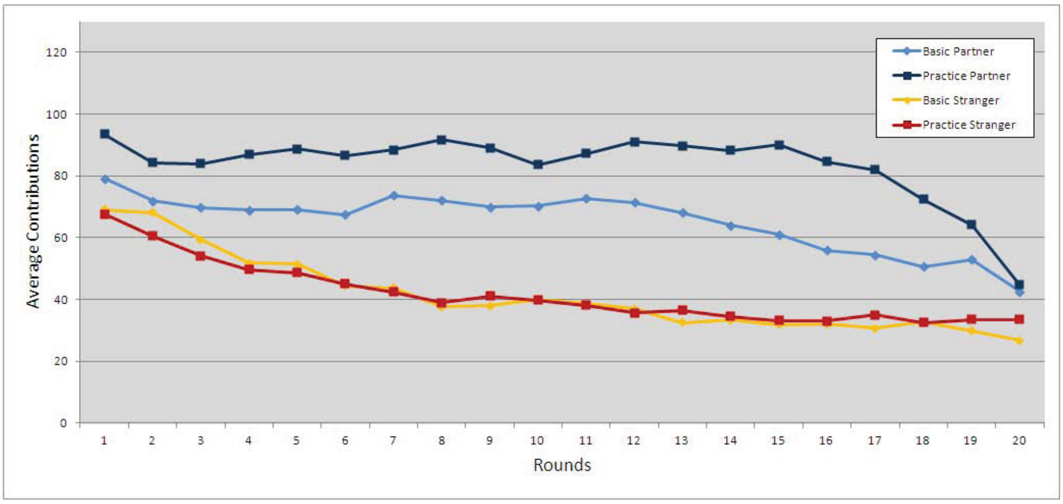

Figure 2: Average contributions over time

The descriptive part of the analysis supports the idea that economic practice enhances cooperative behavior. To analyze the effect in more detail, we formulate a set of hypothesis on the impact of practice and reputation building. We at first concentrate on whether economic practice increases cooperation in the partner treatments. Later we will test whether the matching proce-

dure itself impacts on behavior, that is, if individuals behave differently in the basic partner treatment and the basic random-stranger treatment.
To assess the effect of economic practice in the partner treatment, we test:

> H_0: The average contributions in the *basic treatment* under partner design and the *economic-practice treatment* under partner design have an identical distribution.
>
> H_1: The average contributions in the *basic* treatment under partner design are stochastically smaller than in the *economic-practice treatment* under partner design.

If H_0 can be rejected, we can conclude that the additional questions that point to the Nash-equilibrium and to first-best investment levels impact on individual decision making and enhance cooperative play.
We use a one-sided Wilcoxon rank-sum test to test these hypotheses. As we are not interested in the endgame-effect, we omit round 17 to 20 from the statistical analysis.
The test statistics are given in Table 3.[6]

	Basic (P) vs. Practice (P)	
	Subject	Pair
Test stat.	-2.75***	-1.99**
P-value	0.0030	0.0233

Table 3: Difference in contributions over partner treatments

Critical values are -2.33 for the 1% significance level, -1.64 for the 5%, and -1.28 for the 10% level. We thus find a significant difference between the basic and the economic-practice treatment.
To show that economic practice has an effect on individual behavior only if a reputation for being reliable can be built, we test the same hypothesis for the random-stranger design:

[6]For all tests: ***, **, and * denote significance at 1%, 5%, and 10% level.

H_0: The average contributions in the basic treatment under random-stranger design and the economic-practice treatment under random-stranger design have an identical distribution.

H_1: The average contributions in the basic treatment under random-stranger design are stochastically smaller than in the economic-practice treatment under random-stranger design.

If H_0 could be rejected, we could conclude that economic practice has an impact on individual decision making even without reputation building. However, as expected from the descriptive statistics, we cannot reject the hypothesis. The test statistics are given in Table 4.

	Basic (S) vs. Practice (S)	
	Subject	Group
Test stat.	0.94	0.02
P-value	0.1735	0.4922

Table 4: Difference in contributions over stranger treatments

All results so far suggest that the option for reputation building is a necessary ingredient for cooperative behavior – a platform that allows individuals to learn how to play cooperatively if they are guided in this process. To complete this picture we test the effect of the matching procedure on individual behavior in both treatments. The hypothesis to be tested is:

H_0: The average contributions in the basic treatment (the economic-practice treatment) under partner design and the basic treatment (the economic-practice treatment) under random-stranger design have an identical distribution.

H_1: The average contributions in the basic treatment (the economic-practice treatment) under partner design are stochastically higher than in the basic treatment (the economic-practice treatment) under random-stranger design.

If H_0 can be rejected, we can conclude that the different ways of matching have an impact on individual decision making. The test statistics are given in Table 5.

	Basic (P) vs. Basic (S)		Practice (P) vs. Practice (S)	
	Subject	Pair/ Group	Subject	Pair/ Group
Test stat.	4.56***	2.34***	5.01***	2.97***
P-value	< 0.0001	0.0094	< 0.0001	0.0015

Table 5: Difference in contributions between partner and stranger

As expected, the differences between the two matching procedures are indeed significant for both treatments.

The results show that economic practice can direct people to more cooperative behavior. This effect, however, requires a stable group structure, that is, players must be matched with the same partner for several periods. As there is – due to anonymity – no external enforcement of cooperative behavior even within the stable group, the driving force in the development of cooperative behavior is reputation building. In such a stable social environment economic training is helpful to support people in learning how to play cooperatively faster. Economic training in a socially stable environment thus has a positive value, while it has no effect in unstable environments.

To ensure that the driving forces in our results are reputation and economic practice rather than other hidden forces, we also tested for other aspects. We tested whether gender or occupational aspects impact willingness to cooperate. Our experiment showed neither of these effects. We found no significant differences in the average contributions of economics and business students compared to students from other social sciences or for female and male participants.[7]

[7] As has been mentioned in the introduction, the result with respect to occupation is in line with work by Ahmed (2008), Laband and Beil (1999), Stanley and Tran (1998),

To ascertain that we actually measured the impact of *short-term* training, we also have tested whether students with previous knowledge in game theory or behavioral and experimental economics contributed differently from those without previous knowledge. We found no significant difference in all four treatments.[8]

4 Discussion

This paper has presented experimental results on cooperative behavior in an investment game with a public-goods character. To that end we designed four different treatments of the game to test the impact of economic practice. Scenarios both with practice and without have each been combined with two different matching procedures: (1) a random-stranger matching in which participants have played against a different opponent each round and (2) a partner design where pairwise matchings have stayed together over time.

By combining reputation and training scenarios we have been able to assess whether there is a learning effect at all and if individual willingness to learn depends on institutional parameters. The experiment has provided answers to both questions. Without economic practice most participants chose contributions above but relatively close to the Nash-equilibrium prediction in the random stranger design. The average contribution increased in line with previous experiments (for instance, Croson (1996)) in the partner design without practice.

The inclusion of economic practice that guided people to finding the Nash-equilibrium as well as the socially desirable (first-best) contribution has had different effects in the two matching scenarios. While it did not change behavior in the random-stranger setting, it increased cooperation in the partner

and Yezer, Goldfarb, and Poppen (1996)). With respect to gender Cadsby and Maynes (1998) also found no evidence that men and women differ in their level of cooperation while Anderson, DiTraglia, and Gerlach (2011) found differences in behavior.

[8]The results are summarized in Table 6 in the Appendix.

design. "Increased cooperation" here has two aspects: More people have chosen first-best investments and the average contribution has increased for all participants.

These results suggest that short-term strategic training raises individual awareness of the opportunity cost of exploiting a project partner. These costs are only relevant in a situation with recurring social interactions, e.g., the partner design in the experiment. This idea is in line with Andreoni (1995) who showed that individuals who contribute to a public good do so because they are aware of the chance to realize a better long-term payoff.

We must point out that the training did not direct individuals to selfless and easily exploitable cooperative behavior. The training instructions focused rather on best responses of self-interested rational individuals. They have not encouraged unconditional giving. Obviously, the effect of the training session has been to make individuals aware of the opportunity cost of selfish short-term behavior. Individuals' response has been to realize possible benefits from cooperation and begin to 'nourish' it.

We have found no significant difference in the contribution of students with previous knowledge in game theory or behavioral and experimental economics in comparison to other students in all four treatments. This behavioral similarity shows that the effect we found is actually due to short-term training rather than long-term exposure to economic thinking.

5 Conclusion

Our result supports the view that institutions matter for the outcome of economic interactions. It confirms the known fact that long-term relationships provide incentives for mutually beneficial behavior. On the other hand, we see that people put less emphasis on short-term gains if they understand what is at stake in the long run. This positive motivation is grounded in the better comprehension of the strategic interaction at hand.

One insight from the experiment could thus be that – next to a good institu-

tional design – economic thinking can enhance cooperation in a social group. If people are able to develop a deeper understanding of complex interactions, they can better assess costs and benefits of short-term selfish behavior. ¿From a long-term perspective, economic training will induce better informed decisions that account for the value of cooperation for the individual. In contrast to the public perception of economic education, the experiment suggests that teaching principles of economics does not harm social welfare. It needs, however, to be clear that decisions are made in a social context and that egoistic behavior has an adverse effect on individual reputation; also it is not in the overall individual interest.
This result depends on a crucial assumption: Institutions need to be designed in way that actually allows for reputation building. The basis for more cooperative (and mutually beneficial) behavior – the opportunity cost of short-term gains in form of lost long-term benefits – depends on the actual existence of these long-term benefits. How to design institutions that establish the stable social environment needed for these trade-offs is an issue beyond the scope of experiments on individual behavior.

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Appendix

	Major[a]		Gender[b]		Knowledge[c]	
	Test stat.	P-value	Test stat.	P-value	Test stat.	P-value
Basic Partner	1.08	0.1401	0.55	0.2912	-1.28	0.1003
Practice Partner	-1.25	0.1056	1.43	0.0764	-0.37	0.3557
Basic Stranger	-0.41	0.3400	-0.05	0.4798	-0.82	0.2049
Practice Stranger	-1.13	0.1211	-0.79	0.2153	-1.13	0.1291

Notes: One-sided Wilcoxon rank-sum tests. ***, **, and * denote significance at 1%, 5%, and 10% level.

[a] H_1: Business and economic students contribute stochastically less than other students.

[b] H_1: Male students contribute stochastically less than female students.

[c] H_1: Students without previous knowledge contribute stochastically less than students with previous knowledge.

Table 6: Differences in major, gender, and previous knowledge