

WORKING PAPERS SES

Peer Effects on Perseverance

**Berno Buechel,
Lydia Mechtenberg,
and
Julia Petersen**

**N. 488
IX.2017**

Peer Effects on Perseverance*

Berno Buechel^a, Lydia Mechtenberg^b, and Julia Petersen^b

September 8, 2017

Abstract

Successful performance – be it in school, at the job, or in sports activities – requires perseverance, i.e., persistent work on a demanding task. We investigate in a controlled laboratory experiment how an individual's social environment affects perseverance. We find evidence for two kinds of peer effects: being observed by a peer can serve as a commitment device, while observing a peer can be informative. In particular, we show that successful peers affect perseverance positively if they communicate their success in a motivating way and negatively otherwise, while perseverance is unaffected by unsuccessful peers. Our experimental results suggest that peers affect perseverance indirectly, via influencing self-confidence. We turn to field data from an educational setting and find that students seem to be able to harness the power of peer effects, by selecting into groups that help them reach their goals.

Keywords: Self-control; Peer Effects; Social Networks; Experiment

JEL-Classification: C91; D90; I21; J24

^a University of Fribourg, Department of Economics, Bd. de Pérolles 90, 1700 Fribourg, Switzerland. Email: berno.buechel@unifr.ch. Web: www.berno.info

^b University of Hamburg, Department of Economics, Von-Melle-Park 5, 20148 Hamburg, Germany. Email: lydia.mechtenberg@wiso.uni-hamburg.de, petersen.julia@t-online.de. Tel.: +49-30-25491-446.

* We thank Ugo Bolleta, Bryan Graham, Jean-Robert Tyran, the participants of the workshop on Behavioral Public Economics at the University of Vienna, 2014, and the participants of the 'Economics and Leadership' conference in Groningen 2017. Special thanks go to the WISO lab crew: Olaf Bock (leader), Thais M. Hamasaki, Jan Papemeier, Fenja Pauls, and Merlinde Claudia Tews; and to Willi Flemming and Lorenz Rumberger for supreme research assistance in this particularly demanding experiment.

1 Introduction

1.1 Motivation

Some people are more successful than others. Be it in school, at the job, or in sports activities, there are tasks which require painful effort at some point. While many people let themselves get distracted or fully give up, others stay focused and keep working on the successful completion, i.e. they *persevere*. Clearly, perseverance is to some extent a matter of personality. However, individual behavior is also shaped by the social environment, which includes peers, teachers, parents, and coaches. Personal interactions can have two very distinct effects on individual perseverance.

On the one hand, others who know about the task at hand and interact with the individual after the task can induce social pressure. This is the typical view that peer effects work through *being observed* by peers. On the other hand, *observing peers* before the own task can affect an individual's ability to persevere. More experienced peers, as well as mentors and coaches, may foreshadow the value of perseverance and affect the confidence of the one aspiring to successfully complete a task.¹ It is important to understand the conditions for positive and negative peer effects on perseverance because they can have severe consequences. Applications range from students' educational success, over performance of sports athletes, to success within organizations.

In this paper, we test both categories of peer effects on perseverance in a controlled laboratory experiment and we illustrate the importance of peers for self-control – which is necessary for perseverance – in a field study. We find that both types of peer effects are present in some way. Peer pressure to justify the own performance after the task induces participants to try for a longer period before giving up. Communication with a successful peer before the task, on the other hand, can either induce participants to give up immediately or to persevere, depending on the content of the communication. This last result means that a successful peer can have a crucial effect on the perseverance of a less experienced individual, while unsuccessful peers are rather ignored, according to our data.

¹ As an illustration of the two categories of peer effects, we may consider a leisure time runner who aspires to complete a marathon. Joining a group of runners might have a positive effect for the ability to persevere for multiple reasons. Committing to regularly practicing together and having announced the own plan to participate in a certain marathon to the group clearly fall into the category of peer effects through peer pressure. Learning about the challenges that running a marathon implies and increasing the own self-confidence by observing that others have succeeded to finish a marathon clearly falls into to the category of peer effects through learning. Yet, to choose a group of runners, it is important to know which effect is at work, under which conditions they are working, and when there are actually negative peer effects.

We provide one of the first experimental studies on how peers influence individual perseverance by their own success or failure.² We find that the effect depends on the way peers communicate about the difficulty of the task. In the field study, we illustrate the relation between own self-control and the social network one maintains in an economically relevant environment.

1.2 Two Categories of Peer Effects

Perseverance, which is required to successfully complete a demanding task, necessitates self-control, i.e. the strength to resist temptations to procrastinate. For instance, a student might want to study hard in the evening, in order to be well prepared for class the next day and to eventually perform better in the exam by the end of the semester. However, when evening actually arrives, the now immediate psychological costs from studying become much more salient than the still relatively far-away benefits, and the student is severely tempted to go out instead. More generally, if a disproportionally high weight is attached to whatever costs or benefits would accrue immediately, then plans implying that investments precede benefits will be likely to fail due to the temptation to avoid immediate costs. Since performance plans typically are of this kind, a present bias can impede perseverance and hence performance. This link is studied in the literature on present-biased preferences.³

The vast majority of the studies on peer effects suggest that being observed by peers can enhance performance, e.g. due to peer pressure.⁴ In contexts of perseverance this means that peers could serve as a commitment device: By imposing some social cost on a person whom they observe giving in to a temptation, they can help that person not to give in. An example for such a strategy in the educational context is a group of students who agree to meet at 8 am at the library and who would not appreciate any breach of this agreement. Their agreement is a commitment to start early with studying on that day. Considering peers as commitment devices is an interesting variation of the general idea that commitment devices can help overcome self-control problems.⁵

² The only other study we are aware of is the one by Gerhards and Gravert (2017).

³ For theoretical contributions see, e.g., Strotz (1955), Thaler and Shefrin (1981), Ainslie and Haslam (1992), Laibson (1997), O'Donoghue and Rabin (1999), Gul and Pesendorfer (2001), Bénabou and Pycia (2002), and Fudenberg and Levine (2006). Recent experimental contributions on time preferences include Ariely and Wertenbroch (2002), Andersen et al. (2008), Benhabib, Bisin and Schotter (2008) and Andreoni and Sprenger (2012).

⁴ See, e.g. Hoxby (2000), Hoxby and Weingarth (2005), Zimmerman (2003), Falk and Ichino (2006), Mas and Moretti (2009), and Calvo-Armengol, Patacchini and Zenou (2009). The latter combines theory and empirics to investigate peer effects in an educational setting.

⁵ See Ariely and Wertenbroch (2002), Houser et al. (2010), and Burger et al. (2011).

The second category of peer effects on perseverance is elaborated in the theory of Battaglini, Bénabou and Tirole (2005), hereafter: BBT, which provides us with our main hypotheses. They stipulate that observing one's peers - rather than being observed by them - can affect own performance. The reason is that the behavior of others can be informative about how much the task at hand would challenge one's own self-control.

In BBT, self-control is the ability to persevere on a task (that may be anything from not drinking alcohol to preparing for an exam or working on a paper). Individuals have imperfect knowledge about their own self-control, but know that levels of self-control are positively correlated within their peer group. Hence, observing how peers react to temptations provides individuals with additional information about their own level of self-control. This, in turn, affects their *self-confidence* and consequently the degree to which they themselves exhibit self-controlled behavior in the future.⁶ Intuitively, observing how peers can handle similar challenges to their willpower can be encouraging or discouraging ("if he can do it, then so can I" or "if not even he can do it, then I do not even have to try"). Hence, the theory of BBT is complementary to the vast majority of the literature on peer effects which assumes that being observed by – rather than observing – one's peers affects own performance.

1.3 Our Contribution

In a large lab experiment with overlapping generations, we address the question whether observing or being observed affects own perseverance and performance. Hence, we test the major claims that BBT and the empirical literature on peer effects make about causality against each other. For this purpose, we match subjects who face a challenge to perseverance with subjects who faced the same challenge before and whose success or failure is observed by the former. We let the matched pairs talk to each other in bilateral free text chats about the challenge. Some of the subjects who chat *after* the task anticipate being observed by and chatting with the peer, and some do not. The former setting is the *commitment treatment* in which *being observed* by peers may function as commitment device. Subjects who chat *before* the task can be encouraged or discouraged to persevere later on by *observing* an experienced peer. We find that knowing in advance that one has to justify one's own performance toward one's peers prevents giving up immediately but does not necessarily have long-lasting effects on perseverance. Hence, there seems to be a commitment effect as stipulated by the empirical literature on peer effects. However, we also find effects on those unpracticed peers who observe

⁶ Relatedly, Bénabou and Tirole (2002, 2003) and Mechtenberg (2009) theoretically study effects of communication on self-confidence of subjects with incomplete self-knowledge. These studies find both positive and negative effects on self-confidence, i.e. learning from a peer can be both encouraging and discouraging.

how their advanced peers explain their own success. More specifically, both motivating and demotivating messages from the advanced peer affect own perseverance if and only if the peer had been successful in her own task. A peer's success or failure in itself, unmediated by communication, has no effect on perseverance.

In our lab experiment, communication is the channel through which information that is assumed to be public in BBT is made available. Communication addresses in particular information on how, according to the experienced peer's belief, the subjective difficulty of the task at hand – or, alternatively, the individual level of self-control – is distributed in the population. Hence, our lab experiment combines the approach of BBT with a communication channel similar to the one discussed in Bénabou and Tirole (2002, 2003) and Mechtenberg (2009). The presence of communication channels is also the main feature of our experimental design that essentially distinguishes our lab experiment from the one conducted by Gerhards and Gravert (2017). As opposed to them, we do not find that observing one's peers – without the mediation of communication – has any effect on perseverance; in our lab experiment, it is the interaction between listening to communication and observing success that is effective.

While we implement *exogenous* bilateral relationships of students in the lab to identify directions of causality in peer effects on perseverance, it is an *endogenous* social network of students that we investigate in our field study. The move to an endogenous network is the logical next step in our analysis since in reality, the total net peer effects on perseverance and the resulting performance depend on whether subjects connect to those peers who have positive rather than negative effects on their perseverance. BBT assume such rationality, but real network formation decisions might look different. First, other motives, like homophily on observable characteristics, play a role (Currarini et. al., 2009) and, second, it is realistic to assume that there is also a stochastic element in who connects with whom (Jackson and Rogers, 2007) . Whether subjects form helpful rather than harmful social networks in the presence of a task that challenges their self-control is an empirical question.

In our field study, we behaviorally elicit individual self-control, talent, and performance; and we reconstruct the social network of our students with survey methods.⁷ We show that more self-controlled students are better connected to their peers, have better access to highly talented peers and write better exam grades than students with low self-control. Our findings are consistent with BBT and the empirical test of their theory in Battaglini, Diaz, and Patacchini

⁷ Communication is less salient in our field study, but is indirectly present there, too, since we elicit real-world networks of students that obviously include communication channels. In our field study, we measure individual time-consistency rather than perseverance to elicit self-control.

(2015). In particular, we find evidence for their prediction that individuals with low self-control rationally remain unconnected to their peers while individuals with sufficiently high self-control connect to them. In addition, we find that connectedness and performance are positively correlated, which is in line with both the theory and the empirical results of Calvó-Armengol, Patacchini and Zenou (2009). Specifically, we find that it is connectedness to *talented* friends that correlates with performance. We conclude that students seem to be able to harness the power of peer effects, by selecting into groups that help them reach their goals.

To our knowledge, we are the first to combine within one field study the behavioral measurement of self-control with the measurement of subjects' social network. Moreover, we are the first to combine a lab experiment with a field study to investigate peer effects on perseverance.

2 Lab Experiment

We first introduce the experimental design and implementation. Then we turn to the results.

2.1 Experimental Design

We first describe the task environment and then the overlapping-generations structure and the different treatments.

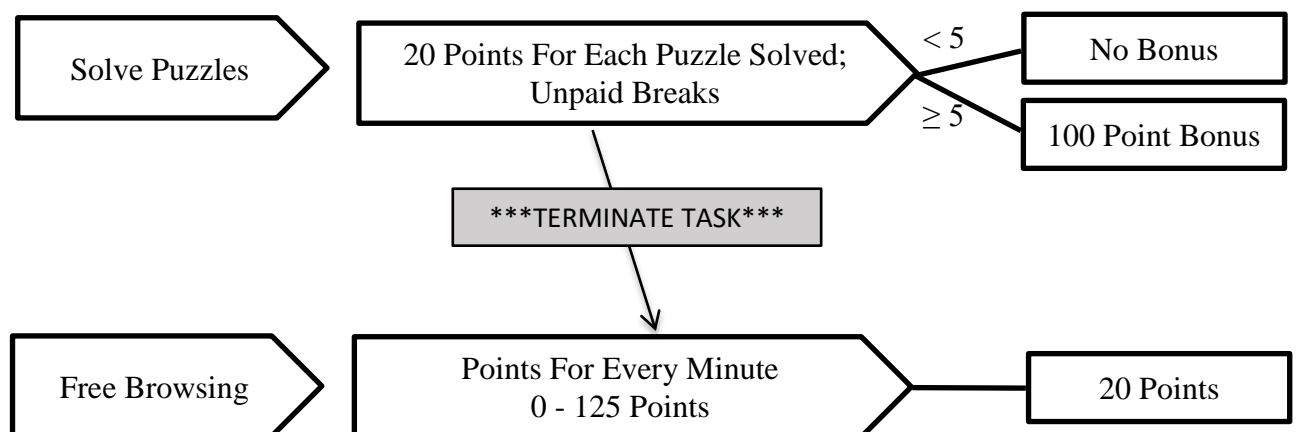
THE TASK ENVIRONMENT To measure perseverance, we implemented the combination of a real-effort task with access to the internet (see, e.g., Corgnet et al. 2015). In order to create the necessary variation in perseverance, we chose a real-effort task that is (1) depleting, to require an effort to persevere, (2) meaningless, to reduce average intrinsic motivation, and (3) novel to our subject pool, to exclude experience from previous real-effort experiments. Subjects had to identify, among a number of meaningless strings of letters (like abCOCaCaZAgbCBZ) any such strings that contain the letter combination “abc” (in all combinations of small and capital letters) *if* that is followed by a vowel. (For such tasks and their depleting effect, see Baumeister et al., 1998, and many others as surveyed in Hagger, Wood, & Stiff, 2010.)

The task was divided into ten subtasks, each consisting of 99 strings displayed on the computer screen, with five input fields for the five strings that had to be identified. A subtask was counted as correctly solved if at least four of the five strings were identified correctly. The entire task was counted as correctly solved if five out of the ten subtasks were solved. We chose these standards (after piloting the real-effort task) to allow approximately one third of our subjects to solve the entire task correctly. For the correctly solved task, a bonus of 100 points was paid.

Additional points were paid for each subtask that was correctly solved. The difficulty of the task was increasing in that more and more letters a, b, and c appeared in the strings, while still only five strings had to be identified. The points awarded for correct subtasks were fixed to 20. Subjects had 50 minutes to solve the task, but they could also give up on it whenever they wished. Those who wished to give up could click the terminate-task button always visible on the task screen and spend the remainder of their time browsing the internet.

Browsing the internet is a common diversion at the work-place and hence a realistic obstacle to perseverance. However, it is to be expected that the laboratory setting activates the social norm to exclusively concentrate on the experimental task. Moreover, access to the internet is typically blocked in lab experiments to enhance control. Therefore, granting access to the internet may well create a demand effect; subjects might deduce from the design that browsing the internet would be encoded as lack of perseverance. Thus, simply granting access to the internet might not be sufficient to induce any usage of it. To prevent the demand effect and to overcome the social norm of “no surfing in the lab”, we paid a small bonus of 20 points for terminating the real-effort task and paid “surfing points” for browsing the internet afterwards, thereby providing a possible rationale for giving up the task. Browsing the internet in minute t yielded $t/10$ points to someone who terminated the task. For instance, if a subject clicked the “terminate-task” button in minute 12, she earned 1.2 points in minute 12; 1.3 points in minute 13, and so on. Hence, who only browsed the internet and did not even start working on the task received 125 points in total. Generally, clicking the “terminate task” button at time T yielded $125 - \frac{1}{20}T^2$ points in total. 100 points correspond to 5 Euros. The payoff structure of the task environment is depicted in Figure 1. The respective amount of points possible to gain for continuing the task and for terminating it and surfing for the rest of the time was continuously announced on the experimental screen in z-tree.

Figure 1: Overview of Payoff Structure



The payoff structure is such that the following holds in virtually any case: If one solves at least five of the ten subtasks correctly, i.e., if one is eligible for the task bonus, then it pays off to continue on the task until the end, that is, to never click the “terminate task” button. If one solves less than five of the ten puzzles correctly, then it pays off to quit the task, that is, to click the “terminate task” button. In this case, the best choice is to terminate right after the beginning of the task. We did *not* communicate during the task whether the subject had reached the threshold for the bonus (this would strongly determine performance). Also, we took great care to explain the payoff structure and the choice between continuing and terminating the task in the instructions, using Figure 1. Subjects were given an example of a subtask in the instructions for them to assess their own confidence in their performance. Also, in the course of their work at the task we elicited their beliefs about whether or not they solved the last subtask correctly. Hence, we could validate that their termination decision was optimal, given their beliefs, which is true for more than 90% of the participants. (More precisely, of those who believed that they had solved five subtasks or more correctly, no one gave up. Of the remaining participants, most (84%) gave up at some point.)

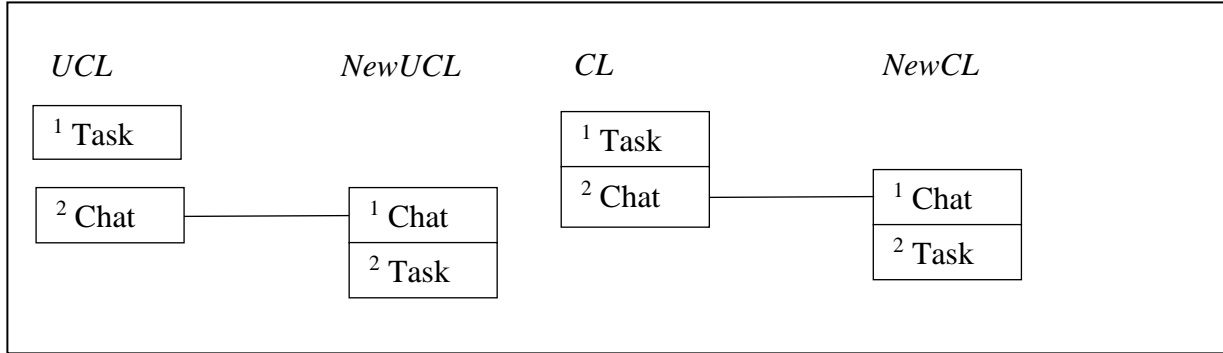
To familiarize subjects with the website that they could browse after task termination, we explicitly stated in the instructions that taking occasional breaks from the task and surfing during the breaks are allowed. However, we did not pay any points for browsing the internet during a break. Breaks could be scheduled freely by clicking the break button and switching to a specific website created by us for a deliberate amount of time.⁸ Moreover, prior to each subtask, subjects had to go on a website to search a password with which they could unlock the next subtask in z-tree. The website offered a broad variety of pictures and articles in many categories such as politics, sports, diets, celebrities, cartoons, and news; and during the unlocking of each subtask, subjects were made aware of the possible diversions offered on the website.

TREATMENTS We conducted four different treatments in a between-subjects design, two *newcomer* treatments and two *leader* treatments. We now use the “leader” and “newcomer” terminology to easily refer to a more experienced peer versus an unexperienced peer. Subjects were invited in subsequent cohorts, with a leader cohort preceding a newcomer cohort; a cohort comprised twelve subjects, half as many as were cubicles in the lab. All treatments had two parts, a *task part* and a *chat part*. The task part was spent in the task environment as described above. In the chat part, subjects in a newcomer treatment were randomly matched one-to-one

⁸ Note that breaks need not signify lack of perseverance but might be part of an optimal working schedule since the task is depleting.

with the subjects of the preceding leader treatment and could chat with their matched partner for three minutes. The treatment structure is depicted in Figure 2.

Figure 2: Treatments. Committed leaders (CL), newcomers with committed leaders (NewCL), uncommitted leaders (UCL), and newcomers with uncommitted leaders (NewUCL)



The chat part A newcomer matched with a leader from the precedent cohort in the first (chat) part of a newcomer treatment learned the failure or success and the degree of perseverance of their matched partner, i.e., whether or not their matched partner had earned the bonus of 100 points and whether or not she had terminated the task. This was communicated to the newcomers on their computer screen and remained visible there throughout the next (task) part. In addition, newcomers could communicate with the matched leader via a chat window during the chat part. The chat opportunity lasted for three minutes. Chat format was free text; there were no prohibitions, except that anonymity must be kept. We asked the leaders from the preceding cohort to address the following three questions: 1) *Please estimate the fraction of participants in percent that manage the task, i.e. who reached the bonus of 100 points.* 2) *Does it pay off to strive for the bonus or should the task be quit immediately?* 3) *Please, explain your recommendation.* Question 1, in particular, was asked to measure the *similarity* that a leader assumed between herself and the average person, regarding performance on the task. A successful (unsuccessful) leader who believes more than half of the other subjects to be successful (unsuccessful) as well deems herself rather *similar* to the average person; a successful (unsuccessful) leader who believes that no other is successful (unsuccessful) as well deems herself *exceptional*. We assume that a newcomer – who has no experience with the task – tends to perceive herself as an average person with regard to the task environment. Thus, the answer that her matched leader gives to question 1 provides her with information about how similar she herself is to her matched leader. Since BBT implies that only similar peers have effects on subjects' perseverance, we expect the answers given by the leaders to question 1 to

set the conditions for any effects of their success or failure on the perseverance of the matched newcomers.

Leader treatments In the commitment treatment “*committed leaders*” (short: *CL*) subjects are leaders: They first do the task part and then the chat part. During the task part they know the content of the second part, i.e., that they will be matched with newcomers (**after** carrying out the task), that these newcomers will learn their success or failure and their perseverance, and that they will chat for three minutes. By contrast, in the other leader treatment “*uncommitted leaders*” (short: *UCL*) they do not anticipate the details of the chat part since instructions for that are only given after completion of the task part. Hence, subjects do not anticipate that they will be matched with newcomers of the subsequent cohort and that information about their own perseverance and performance will be provided to their matched partners.

Newcomer treatments In the “*newcomers with committed leaders*” treatment (short: *NewCL*) subjects are newcomers: They first do the chat part and then the task part. They are matched with leaders from CL (committed leaders) in the chat part. In the other newcomer treatment “*newcomers with uncommitted leaders*” (short: *NewUCL*) subjects are matched with leaders from UCL (uncommitted leaders) during the chat part.

IMPLEMENTATION In total, we have $N=336$ participants which amounts to 84 subjects (i.e., observations) per treatment. Despite the logistic complexities of this overlapping generations experiment and the IT reliability challenge to let participants seamlessly switch between experimental software and a web browser, there was only one case of a system failure within a regular session.⁹ Cohorts are overlapping: always two consecutive cohorts were chatting with each other after the first of the two had finished the task and before the second started with it. Hence, for logistic reasons we conducted multiple sequences of sessions per day. We distributed these sequences of sessions across different days and made sure that treatments do not differ much in the days and time slots at which they were run. The experiment was run at the WISO-laboratory of Hamburg University. A different room outside the computer lab was reserved for reading of instructions and a subsequent quiz to guarantee understanding of the decision and payoff structure. Another room outside the lab was reserved for payment (in cash and in private). Communication other than required in the chat part of the experiment was prohibited. In the time span between the end of the task and leaving (in treatments *NewCL* and *NewUCL*) or chatting with the subsequent cohort (in treatments *CL* and *UCL*), subjects answered a questionnaire. This questionnaire elicited age and sex of the subject and other control variables. The chat data were transcribed and coded by a research assistant unaware of

⁹ This reduces the number of observations in *NewCL* to 83.

the purpose of the study and the different treatments. The reliability of the coding was checked and confirmed by having another, equally unaware research assistant, coding the same chat protocols. The coding of the three questions that had to be addressed in the chat into answer categories was straight-forward.

2.2 Results

Before we turn to the effects of observing peers and to the effects of being observed by peers, we first provide some summary statistics for the main variables.

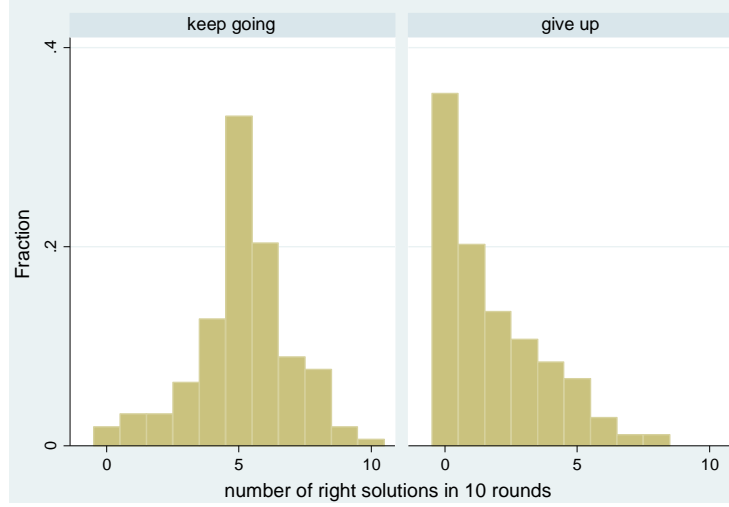
2.2.1 Some Summary Statistics

The main outcome variables are the number of subtasks solved correctly (*Puzzles*) and the dummy whether the bonus is reached or not (*Bonus*).

Table 1 reports summary statistics of these main outcome variables and also reports the payoff earned in EUR (*Payoff*) and the minutes participants stayed in the task before clicking the “terminate task” button (*MinutesTask*). The average number of correctly solved puzzles is below four, but still roughly every third participant (34%) reached the bonus of 100 points, which requires to solve five or more. There was indeed one person who solved 10 puzzles correctly, each time believed that the puzzle is solved correctly, and received the bonus of 100 points. (This leads to the maximal number of points of $10 \cdot 20 + 10 \cdot 1 + 100 = 310$, which yields the maximal payoff of EUR $15.50 + 8.00 = 23.50$.) The average Payoff was around 16 EUR.

In Table 1 there are separate outcome statistics for those participants who gave up, i.e. clicked the “terminate task” button at some point, and those who stayed in the task, reported in the last two columns. Separating these two groups, we observe that the bonus was reached by 73% of those who stayed in the task. The histogram below shows that those who stayed in the task solved typically five or six puzzles correctly. Very few managed to solve nine or ten tasks. Most of those who gave up solved none or less than three tasks. Table 2 reports the timing of giving up, i.e. the minutes before clicking the “terminate task” button, separated by the number of puzzles solved. Those who solved three or four puzzles gave up after 44 of 50 minutes, which indicates that they strived for the bonus until they realized that they would not reach it. Participants who solved two puzzles or less gave up earlier.

Figure 3: Histogram of Puzzles by Giving Up Decision



Since each participant was asked after each subtask whether she believed that she had solved it correctly, we can analyze the participants' beliefs about the number of tasks they had solved correctly. The correlation of this belief with the actual amount solved correctly (*Puzzles*) is 0.88. Looking at the difference between these two variables, it turns out that a majority of participants, 72%, has exactly accurate beliefs, while 24% overestimate the amount, and 4% underestimate it. Hence, beliefs are quite accurate, but there is bias consistent with overconfidence. The bias is however weak since most overconfident participants overestimate the amount solved by one or two.

We now first analyze the effects of observing peers and communication, i.e., we focus on the newcomer treatments. Afterwards, we turn to the effects of being observed, i.e., we focus on the commitment treatment.

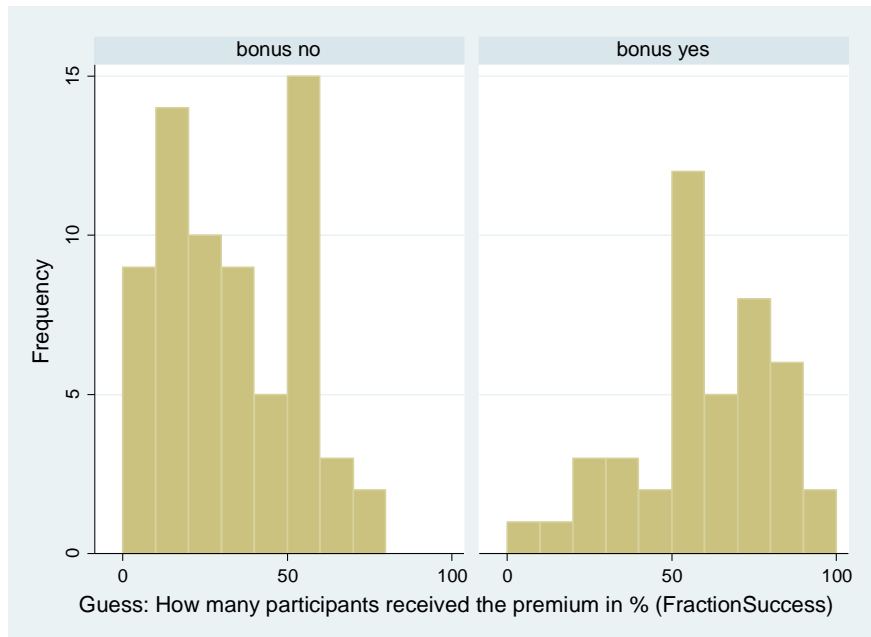
2.2.2 Effects of Observing Peers and Communication

Consider the newcomer treatments, *NewCL* and *NewUCL*. From the perspective of the newcomers, no treatment variable changes between these two treatments. Hence, when analyzing the newcomers' reaction to what they observe from their matched leaders, we pool the data from *NewCL* and *NewUCL*.

The main aspect observed by a newcomer is whether her matched leader did or did not receive a bonus (captured by the dummy variable *BonusPeer*). This piece of information is highlighted during the chat and on each subtask in the newcomer treatments. Moreover, in the chat the leaders are asked to provide a guess about the fraction of participants that was able to receive

the bonus. The answers range from 0% to 100%, and we call this continuous variable *FractionSuccess*. As the histograms in Figure 4 show, leaders who do not receive the bonus more often guess that the number of participants receiving the bonus is small, but there is still a considerable variation across leaders such that our subjects in the newcomer treatments get very different signals about the difficulty of the task and their personal likelihood of being able to receive the bonus.

Figure 4: Histogram of *FractionSuccess*, i.e. the leader's estimate concerning how many participants manage to reach the bonus, differentiated by whether the leader has received the bonus herself (right panel) or not (left panel)



FractionSuccess provides information on the leader's belief about how likely it is that the newcomer can succeed. Combined with the leader's success or failure, *FractionSuccess* provides information on the peer's belief on how similar she is to general population and hence to the newcomer. If it follows from *FractionSuccess* that most will succeed or fail like the peer, this signals high similarity. Moreover, in the chat leaders gave explicit recommendations whether to give up or to strive for the bonus, and we elicited how difficult the task is described as. These two variables *Recommendation* and *Difficulty* are highly correlated with the variable *FractionSuccess*, but coarser measures since they are binary, respectively ordinal.

In the following we regress the main outcome variables (*Puzzles* and *Bonus*) on *BonusPeer* and *FractionSuccess*, which are the two most important variables of observing and communicating with a peer. They cover the objective information provided by the experimenter that the peer (leader) has or has not received the bonus, as well as the subjective message of how hard it is to reach the bonus. Since it must be expected that these two variables interact with each other, we also include the interaction term. The outcome variable *Puzzles* is a count variable which

we estimate by a Poisson regression. The outcome variable *Bonus* is a binary variable, for which we use a logistic regression.

We first illustrate the results by plotting the predicted outcome values by different levels of *FractionSuccess* for both subjects with matched leaders who received a bonus and subjects with matched leaders who did not. We obtain the predicted number of puzzles and the predicted probability of a bonus for values of *FractionSuccess* that range from 0 to 100 with increments of 5.

Figure 5: Marginsplots of Puzzles and Bonus by *FractionSuccess* and *BonusPeer*

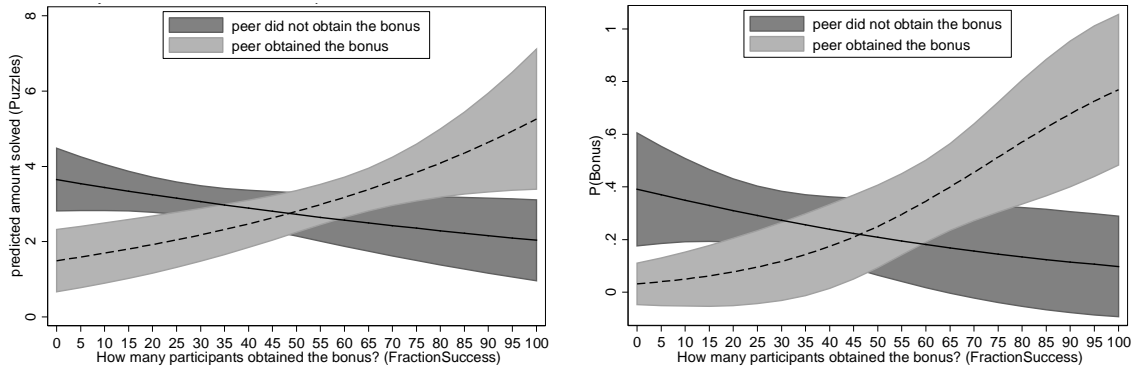


Figure 5 shows how the outcome variables are increasing in *FractionSuccess* for a matched leader who received a bonus and rather decreasing in *FractionSuccess* for a matched leader who did not. The graphs indicate that the best performance is predicted for those who observe that their matched leader obtained the bonus *and* who receive the message that a very high fraction were able to reach the bonus. Lowest performance is obtained if the matched leader received a bonus but reports that only very few are able to do so. It seems that a message from a peer who has been successful can be both particularly encouraging and particularly discouraging.

To investigate this interaction effect and to test for significance of the difference between having a peer with bonus and a peer without bonus, we run a sequence of regressions each time modifying the variable *FractionSuccess* by subtracting a constant from it. More precisely, we generate the variables *FractionSuccess*0, *FractionSuccess*10, *FractionSuccess*20, ..., *FractionSuccess*100, with *FractionSuccess**k* defined as *FractionSuccess* minus *k*. The results of these regressions are all identical apart from the coefficient of *BonusPeer*. Consider the interaction term, which is the product of the binary variable *BonusPeer* and the continuous variable *FractionSuccess**k*. When the variable *FractionSuccess**k* is zero, the interaction term is always zero such that the coefficient of *BonusPeer* picks up the whole effect. We use this fact to provide an interpretation for the effects. All coefficients of *BonusPeer*, which are the only

coefficients that differ from the ones in Table 3, are reported in Table 4. Hence, the entries in FS0 of Table 4 correspond to the columns (1'), (2'), and (3) of Table 3.

First, we observe that significance levels are very similar across model specifications. Thus, the results seem to be robust. Second, the coefficients are significant for low values and for high values of *FractionSuccess* but not in between. Moreover, the sign of the effects changes from low to high values. We provide an interpretation for each outcome variable, starting with *Puzzles*.

EFFECTS ON PUZZLES Pure coefficients of the Poisson model cannot be interpreted easily due to the log link function. Hence, we use the incidence rate ratio (IRR) interpretation. (The significance levels remain roughly the same comparing the robust and the IRR – option). At the lower end of the distribution, IRR coefficients are significant for *FractionSuccess* in the range from zero to thirty. For example, the IRR at *FractionSuccess*₂₀ = 0,592 (see column 'FS20') is the estimated rate ratio comparing whether the matched leader received the bonus, i.e., was successful, or not, while holding the message *FractionSuccess* at the level of 20%. Those whose matched leader was successful are expected to have an incidence rate for *Puzzles* 0,592 times that of subjects whose peer was not successful, which is a decrease of 41%. Thus, a low *FractionSuccess* is bad news, in particular if the matched leader has received the bonus; it seems to transport the message “I did it but it was so hard, you should not try it”. Low *FractionSuccess* of a matched leader who did not receive the bonus is in principle bad news as well, but much less so. The statement that only few reached a bonus does not seem highly credible if it comes from a person who did not manage to receive the bonus. Maybe subjects believe that their unsuccessful peer did not even try and only wants to justify her own behavior.

The IRR-coefficients at the upper end are significant for *FractionSuccess* in the range from 70 to 100 (with very high significance for 80-100). For instance, holding *FractionSuccess* at a high level of 80%, i.e., considering *FractionSuccess*₈₀, the IRR coefficient is 1.786 (column 'FS80'): Those who receive the news that their matched leader has received the bonus are expected to have an incidence rate for *Puzzles* 1,786 times that of subjects that receive the opposite news, which corresponds to an increase of almost 79%. This is the good-news effect; it seems to transport the message “I could do it *and* I am similar to the average person, then so can you”. It seems encouraging to hear that many of the subjects are able to receive the bonus from a person who did it as well.

EFFECTS ON BONUS and PAYOFF Turning to the outcome variables *Bonus* and *Payoff*, we observe that the significance levels for the different *FractionSuccess* values are very similar to Poisson estimates of the outcome variable *Puzzles*. Effects are significant at the upper and

the lower end of *FractionSuccess* with the same direction of the effect for each outcome variable. For instance, if the peer received a bonus and reported that only 20% were successful (i.e. at *FractionSuccess*20) the *Payoff* decreases by EUR 2.01. At the other end of the spectrum, the *Payoff* if one's matched leader received a bonus and reported that 80% were successful increases by EUR 2.38, while the average payoff is EUR 15.73. Hence, there is a sizeable effect, not only on the number of puzzles solved and on the likelihood to receive a bonus, but also on the *Payoff*.

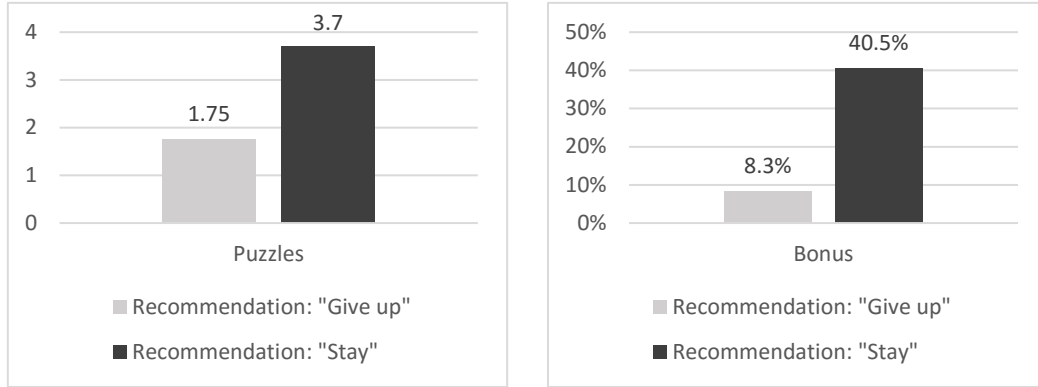
ROBUSTNESS These results remain valid when we include variables from the questionnaire. As a test of robustness, we rerun the regressions including all variables from the questionnaire that survived the stepwise reduction procedures. The magnitude and significance levels are virtually the same.¹⁰

As another test of robustness we exchange the variable for the message *FractionSuccess* with the binary variable *Recommendation*, which is one if the matched leader recommends to stay in the task and zero if she recommends to give up at some point. Table 5 reports the main outcome variables in dependence of whether the matched leader has recommended to give up or to stay in the task. The table gives an indication that recommendation to stay has generally a positive effect on outcome. This strongly indicates that it is indeed perseverance which is mainly required to succeed in the task. It also confirms the strong interaction between the content of the chat and the success of the peer. Recommendations of peers who received the bonus heavily affect outcome variables. In particular, the most encouraging leaders are those who have received the bonus and recommend to strive for the bonus. We have the binary variable for the recommendation which is either “stay in the task” or “quit at some point”.¹¹ Testing for significance (by a Fisher exact test and a Wilcoxon ranksum test) confirms: Recommendation to stay in the task leads to significant increase of *Puzzles* and a significant increase of the fraction of bonus reached, given that the matched leader has obtained the bonus ($p < 0.05$).

¹⁰ The magnitudes are even a bit higher and the p-values are even a bit smaller when additional variables are included. These results can be requested from the authors.

¹¹ The coding of the chat for the recommendation entails three categories: “stay in the task”, “work first on the task and quit later”, and “quit immediately”. The binary variable recommendation simply distinguishes between the first and the two latter categories.

Figure 6: Recommendation from a peer who has reached the bonus positively affects both the number of correctly solved subtasks (*Puzzles*, left panel, $p < 0.05$) and whether the bonus has been reached (*Bonus*, right panel, $p < 0.05$).



Summarizing we find both a good news effect and a bad news effect. Thereby, the signal about the peer's performance alone is not sufficient since it is put in relation to what the peer communicates. Good news is obtained from a successful peer who communicates in an encouraging way. Bad news are obtained from a successful peer who communicates in discouraging ways.

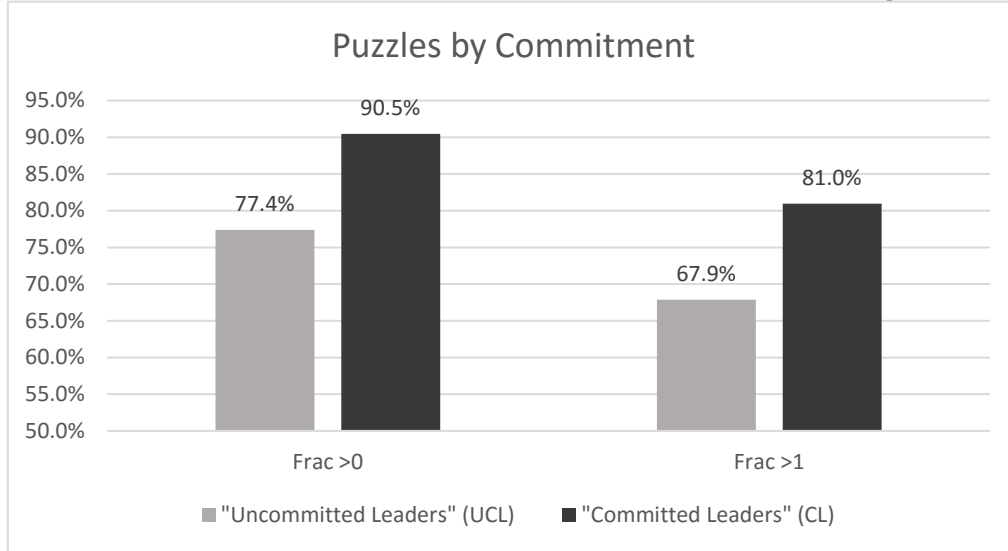
2.2.3 Effects of Being Observed by Peers

We now compare the commitment treatment *CL*, in which subjects know that their performance will be shown to a peer and that they are going to chat with this peer, to the other treatments. Table 6 reports summary statistics of the variable *Puzzles* for *CL* and for all the other treatments.¹² Two-sample Wilcoxon rank-sum (Mann-Whitney) tests reveal: *Puzzles* is significantly higher under *CL* than under both newcomer treatments, and significantly higher under *CL* than under all other treatments pooled. The effect of commitment on *Puzzles* is not so strong at the mean or the median as it is at the lower end of the distribution. While the null hypothesis is not rejected for *CL* vs. *UCL* (the uncommitted-leaders treatment) when using the rank-sum test (with $p=0.2643$), it is rejected when considering the fraction of participants who solved at least one, respectively at least two, puzzles. Fisher exact tests reveal that a fraction of subjects who solved at least one (or at least two) puzzles correctly is significantly higher under *CL* than under *each* of the three other treatments on the 5% significance level, as well as higher under *CL* than under all other treatments pooled, of course. Hence, subjects who know that their

¹² For the newcomer treatments, we have seen that there are positive and negative effects, depending on the peer. These effects may well cancel each other out such that no treatment effects may be apparent when comparing the newcomer treatments with the *UCL* treatment.

result will be observed by a peer and that they have to justify themselves in a chat, tend to solve more puzzles on average and less often solve zero or only one puzzle.

Figure 7: Fraction who solved at least one subtask, respectively at least two subtasks, is higher in the committed leader treatment than in the uncommitted leader treatment (Fisher exact tests: $p < 0.05$).



It seems that the fact that there will be an observer to whom the subject has to justify her performance motivates the participants to try the puzzles at least for a while. If subjects manage to solve five puzzles this behavior pays off with a bonus of 100 points. Otherwise, this is not so clear because at the same time the subjects could have earned points for surfing in the internet. In particular, subjects who solve two, three or four puzzles can neither accumulate much points in the surfing mode nor receive the bonus of 100 points. Correspondingly, we do not find that the commitment treatment significantly increases the likelihood of receiving the bonus (not reported here). In the commitment treatment the fraction is 36% and in the other treatments 33%. Hence, it is not surprising that the difference of the commitment treatment *CL* to the other treatments in the number of puzzles solved does not materialize in higher payoffs. The average payoffs are 15.81 for *CL* and 15.70 EUR for all other treatments (these numbers are not repeated in the appendix).

2.3 Summary of Experimental Results

We find that knowing in advance that one has to justify one's own perseverance toward one's peers prevents giving up immediately but does not necessarily have long-lasting effects on perseverance. Hence, there seems to be a commitment effect as stipulated by the empirical literature on peer effects. However, we also find effects on those who observe how their advanced peers explain their own success or justify their own failure. More specifically, both

motivating and de-motivating messages from the advanced peer affect own perseverance if and only if the peer had been successful in her own task. A peer's success or failure in itself, unmediated by communication, has no effect on perseverance. Our findings provide evidence both for the direction of causality assumed by the empirical literature on peer effects and the direction of causality suggested by BBT.

3 Field Study

By randomly matching peers in our lab experiment, we have assured that who interacts with whom is exogenous. In reality, however, people actively form (friendship) ties and hence the network is endogenous. Moreover, in the field we can study less artificial tasks that are economically more relevant. Hence, we did a field study that complements the lab experiment.¹³

3.1 Hypotheses

Individuals who achieve their goals are typically more self-controlled than those who set the same goals but fail to achieve them. We use this insight to implement a behavioral measure of self-control in our experiment. In theory, individuals learn from their experience and adjust their goal-setting behavior to their perceived level of self-control.¹⁴ Consequently, a behavioral measure of self-control can best be obtained from goal achievement at early stages of this learning process, i.e., from observing how well individuals not yet fully informed about their own self-control relate to their early goals. Hence, we (behaviorally) equate high self-control with a high level of early goal achievement.

We are now in a position to formulate our first two hypotheses:

H1 *Performance increases in self-control, i.e., in early goal achievement.*

H2 *Individuals with high self-control invest more effort into achieving their goals than individuals with low self-control.*

Our main question is how self-control interacts with one's position in a social network and thereby with peer effects. BBT argue that only sufficiently self-controlled individuals profit from the interaction with peers; but they profit more from peers who are similar to them in

¹³ A more detailed description of this field study can be found in an earlier working paper version that was entitled "Peer Effects and Student's Self-Control".

¹⁴ For a theory on learning self-control in a dynamic setting, see Ali (2011).

terms of self-control. This is because teaming up is helpful only if “good news” about the level of self-control of the other team members is sufficiently likely. Hence, if social network formation is endogenous and individuals link strategically (cf. Jackson and Wolinsky, 1996), then the individuals with higher levels of prior self-control will choose to team up with each other whereas those with low levels of prior self-control will stay alone. Thus, two testable predictions of the BBT model are, first, that only sufficiently self-controlled individuals have a strictly positive number of links to others, and, second, that self-control is correlated among direct friends.

The BBT model does not account for human capital. However, it is easy to see how an extended reasoning along its lines can be used to bridge the gap between the literature on self-control and the literature on how human capital of peers can affect achievement: Since self-controlled people tend to persist in their performance plans, they tend to acquire more human capital. Consequently, they become more valuable peers with regard to the peer effects on achievement that can be expected from them. If indeed only sufficiently self-controlled individuals connect with others, as the BBT model suggests, then only sufficiently self-controlled individuals will enjoy positive peer effects on their performance. Hence, we hypothesize:

H3 *Performance increases in the number of talented friends.*

H4 *People with high self-control (a) are more likely to connect to others, (b) have a higher overall number of friends, and (c) have a higher number of talented friends.*

3.2 Study Design and Implementation

3.2.1 Procedure

We conducted our field study in a large undergraduate Microeconomics course at the University of Hamburg in the summer semester of 2013.¹⁵ In total, 117 first-year students regularly participated in the lecture and the tutorials.

Our experiment was conducted in the following five waves:

1. Survey on aspired course grades and current study time
2. Survey on intermediate goals

¹⁵ In Hamburg, like in most German cities, courses in the summer semester are taught from beginning of April to mid July.

3. Measurement of intermediate goal achievement
4. Elicitation of students' social network
5. Exam

Students could not anticipate upcoming waves, and they were not told before the beginning of the fourth wave that they were part of a field study.¹⁶ Rather, they were told that some faculty wanted to analyze students' learning conditions and study behavior. Moreover, the experimenters who actually entered classes were not involved in teaching the lecture or tutorial courses.¹⁷ The teaching assistants were informed about who would enter their classes on which days and were told that the interventions they witnessed were part of a study on student behavior.¹⁸ They have never been informed about the fourth wave of the experiment or about the purpose of the first three waves before the end of the semester. Furthermore, they were instructed not to provide possible interpretations of our study to the students. Apparently students accepted our explanation of the interventions in their classes and never approached an experimenter or teaching assistant on this issue.

THE FIRST WAVE In the first wave, we conducted a survey in each tutorial, asking which grades students aspired to and how many minutes they on average studied per week for this specific course.

THE SECOND WAVE The second wave was mainly dedicated to induce students to set goals for themselves at a relatively early stage of the course, since their behavior with respect to these goals allows us to measure students' self-control at a later point in time. This wave was conducted in the tutorials. Each teaching assistant announced that some organizational issues were to be settled within the ongoing session of the tutorial course. Two intermediate goals were elicited.

Intermediate goal with partial commitment A senior student who was not informed about the purpose or content of our study entered the tutorial course and truthfully explained to the students that they could enroll in a non-compulsory one-day micro workshop. Moreover, he mentioned that enrolment was not obligatory for participants but that each student who, though

¹⁶ We received consent of the department to conduct this study.

¹⁷ The experimental team that actually entered the classes to conduct the experiment consisted of two of the three authors of this paper. The third author, Lydia Mechtenberg, committed both not to act as an experimenter in class and not to receive the data set in a non-anonymized form, since she was the lecturer of the Microeconomics course. Exams were graded by the teaching assistants who did not know the purpose of the study and never received access to the data set.

¹⁸ In the second wave, they announced the modalities of handing in the mid-term problem set as a natural part of their teaching job. These modalities were designed for experimental purposes about which, however, the teaching assistants were not informed.

enrolled, would not show up for the workshop would have to pay a fee of 3 EUR. By contrast, participation, both with and without enrolment, was free of charge. It was also made clear that payment of the fee would be asserted.¹⁹ Hence, students were offered a pre-designed goal (participating in the workshop) together with a commitment device (the fee). Then, the senior student distributed enrolment forms that again mentioned the fee and provided the link to a website where online enrolment for the workshop was possible. Finally, he collected the enrolment forms and left the class.

Intermediate goal without commitment The teaching assistant truthfully explained to his students that they had to submit a compulsory midterm assignment in order to be eligible for the final exam. This assignment – a standard problem set – was identical for all students. The quality of the solution was not decisive for students' admission to the final exam; but their solution had to fulfill the minimum requirement of being a recognizable attempt to solve all problems of the problem set. The teaching assistant explained to his students that they had to choose between two submission deadlines, an early and a late one, which were neutrally framed. He distributed forms on which the two deadlines were specified and the students were asked to individually choose their preferred deadline. Hence, students were offered the possibility of regulating their own future learning behavior by choosing the goal of submitting early. Students were told that their choice was not binding but helpful for organizational purposes. They were also told that assignments handed in after the late deadline would not be considered and excluded the students from the possibility of taking the exam. This information was provided online, too, and was repeated several times in the lecture to make sure that all students enrolled would understand it. To provide a (weak) incentive for students to choose the early deadline, a weakly informative online feedback about the quality of their solution of the midterm assignment was promised to students who submitted prior to the early deadline. However, we instructed the teaching assistants not to provide the correct solutions of the midterm assignments in their online feedback. No feedback was provided to students who submitted prior to the late but after the early deadline. However, any student always – also after the early deadline – had the opportunity to approach their teaching assistant after the tutorials or during the office hours to get any information about any exercise they needed help with.

THE THIRD WAVE The organizing team of the workshop collected the data about students' enrolments and their actual participation and passed them on to the experimental team. The workshop took place on a weekend day after the third wave of the experiment. In its course,

¹⁹In fact, teaching assistants and the experimental team collected the fee within the three weeks after the workshop and, if necessary, immediately after the exam.

additional exercises related to the content of the micro course and prepared by the organizing team were discussed. The teaching assistants collected all data about students' choices of a submission deadline and registered whether students' actually submitted before the chosen deadline. Again, all data were passed on to the experimental team.

THE FOURTH WAVE The main purpose of the fourth wave was to elicit students' networks. We conducted a final survey in which students were asked to name the peers with whom they had joined to prepare themselves for the exam (outside the official lectures and tutorials). These lists of names allow us to reconstruct the social network of learning relationships within class.²⁰ For each of the listed friends some further survey questions specified the kind of relationship, e.g., since when they know each other. We also asked how they perceived their own, their peers', and other students' degree of self-control.²¹

THE FIFTH WAVE Students wrote the final exam either at the beginning or at the end of the semester break. Thereby, they could score between 0 and 90 points.²²

3.2.2 Key Measures

MEASURING PERFORMANCE, TALENT AND EFFORT We use the score in the final exam to measure performance, since it fully determines the overall course grades. We also obtained the scores (and grades) from a previous math course as a control variable for talent since a proficiency in math was important both for solving the preparatory problem sets and for doing well in the exam.

MEASURING SELF-CONTROL We constructed an indicator variable, *self control*, that measures whether a student behaved in a time-consistent way, i.e., achieved her own intermediate goals. A student's two intermediate goals are to submit the midterm assignment prior to the deadline that she herself had previously chosen and to participate in the workshop in case that she had registered for participation. Among the 63 students who signed up for the micro workshop, a share of 73% (46 students) attended it. Early midterm assignment was planned by 107 students, but only a share of 73% (78 students) actually delivered early. The binary variable *self control* becomes one if the student both submitted prior to her self-chosen deadline and did not fail to attend the workshop if enrolled. This variable captures self-control

²⁰ Literally, the social network is directed because some students are mentioned by others who do not mention them. However, it seems more reasonable to consider learning relations as bilateral and to believe that two students have learned together even if only one of them reports this, e.g., because the other learning partner was absent on the day on which we conducted the final survey. We use the two direct networks to test for robustness of the results.

²¹ Students reported their perceived degree of self-control on a scale between 0 and 10.

²² Students who failed to pass at the early date were automatically eligible to write a new version of the exam at the end of the semester break.

since it assigns low self-control to students who cannot resist the temptation to procrastinate when preparing the midterm assignment or the temptation to spend their week-end in a more agreeable way than by attending a workshop that they had planned to attend.²³ The basic assumption underlying the construction of the indicator variable is that, as implied by economic theory, the economic concept of self-control can be equated with time-consistent behavior. An important alternative interpretation would be that *self control* captures an advantage in human capital of those students who attended the workshop and received feedback after an early submission of their midterm assignment. To test whether this is true, we conducted a Wilcoxon rank-sum test comparing the performance of those who attended the workshop and received the feedback with those who did not. The result is displayed in Table 9. There is no evidence that workshop attendance and feedback affected performance. Hence, we can be confident that *self control* measures time inconsistency only.

MEASURING STUDENTS' SOCIAL NETWORK POSITION We created two different variables: *friends* and *degree*. The binary variable *friends* takes the value one if the student is at all connected to others, i.e., is part of a study group, and zero otherwise. The variable *degree* counts the student's direct links to others, i.e. the number of learning partners.²⁴ We computed both variables from the undirected social network. In order to check for robustness of our results we also use variables derived from the directed network.

DIFFERENTIATION OF PEERS To further substantiate friends' peer effects we categorize them as talented or untalented according to their math score. The count variable *talented friends* counts the number of links to fellow students who have a math score weakly above 58 points, while the count variable *untalented friends* counts the number of links to fellow students who have a math score weakly below 55 points. The chosen threshold makes use of the fact that no student achieved a math score between 55 and 58; and it splits all friends in two categories of equal size.

Descriptive statistics of our most important variables are provided in Table 7.

²³ In principle, we can also construct two separate indicator variables for time-consistent behavior concerning each of the two intermediate goals. While many of our results are qualitatively unaffected, there is simply less variation in these two measures.

²⁴ Further network statistics, such as measures of centrality, are difficult to interpret since the network is quite sparse, consisting of many isolates and small components.

3.3 Main Results

3.3.1 Determinants of Performance

Table 8 displays the results on how individual characteristics, including self-control, general connectedness to other students and specific connectedness to highly talented students affect performance. In all models, we tested for the presence of omitted variables with a specification-error test as well as for multicollinearity and rejected both. Moreover, ordinary least square (OLS) estimates were calculated with robust standard errors. We additionally used a two-step FGLS (feasible generalized least square) method to account for heteroskedasticity and thus to gain efficiency.

SELF-CONTROL AND TALENT Both individual self-control and talent have a positive effect on performance. Pairwise correlation of the dependent variable and these regressor variables yields a correlation coefficient of 0.66 (with a p-value smaller than 0.001) for talent and performance, while the correlation coefficient of self-control and performance is 0.30 (with a p-value of 0.003). The effect of talent is significant at the 1% level across all model specifications. Self-control is statistically significant, albeit to a lesser degree. Being highly self-controlled is associated roughly with an eight points higher micro score, which is substantial since the maximal score is 90 and the median score is 58. The standard deviation of *math score*, our control variable for talent, is 21 (reported in Table 7); an increase of one standard deviation in *math score* leads to a rise of 12 points in the micro score. These results fully support our hypothesis H1 that self-control positively affects performance.

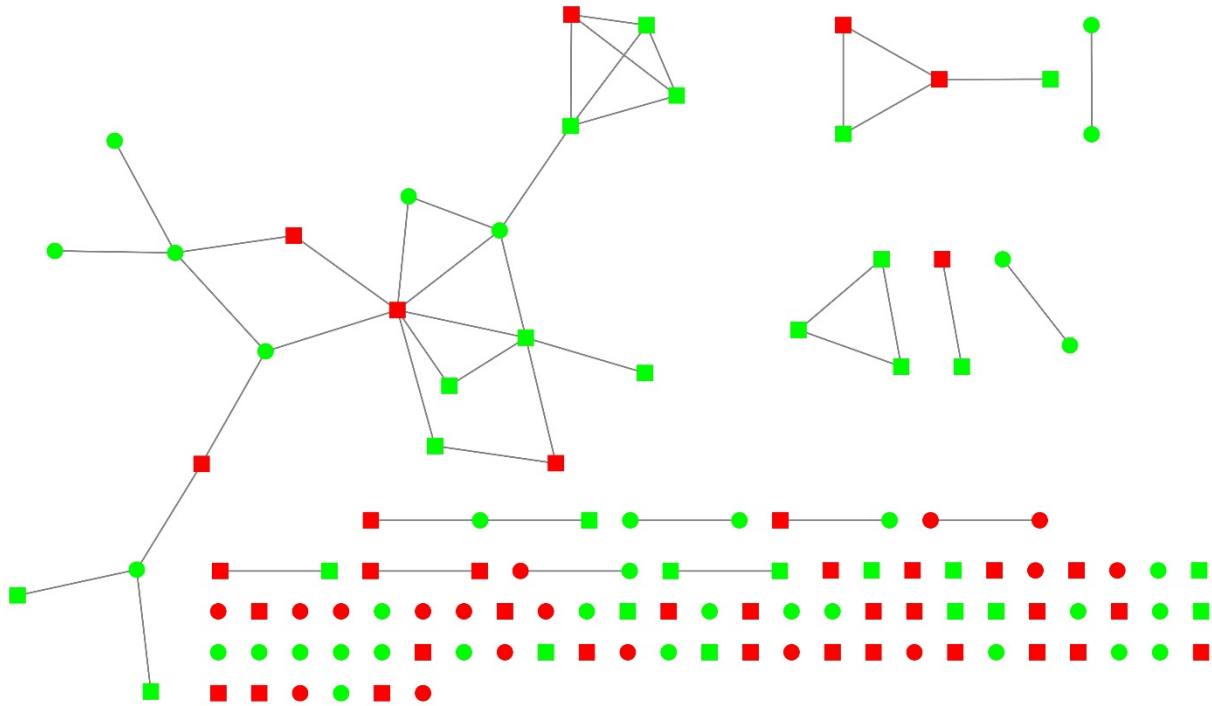
SELF-CONTROL AND PEER EFFECTS The number of friends in general, measured by *degree*, has no significant effect on performance when controlling for talent and self-control (see columns 2 and 3 in Table 8). However, increasing the number of *talented* friends, i.e., friends with a high math score, significantly improves performance, which is fully in line with our hypothesis H3. If we control for the number of talented friends, the effect of self-control becomes weaker. Within the OLS regression the effect of self-control is of the same dimension but insignificant. In the FGLS model, the effect of self-control remains significant when controlling for the number of talented friends. Since overall model diagnostic statistics favor the FGLS model, we conclude that both self-control and being connected to many talented peers significantly improve performance. Overall, we find strong evidence in support of our hypotheses H1 and H3.

3.3.2 The Interaction of Self-Control and Peer Effects

SELF-CONTROL AND STUDENTS' SOCIAL NETWORK

Figure 1 displays the social network of our students and provides a first impression of the relationship between self-control and connectedness. Importantly, most students who have at least one friend also have a high level of self-control, whereas the majority of those who are unconnected to any friends also exhibit low self-control. To be more precise, only 39% of the many isolates have a high level of self-control. In contrast, 71% of the students who have at least one friend also exhibit high self-control. This observation already suggests that network formation among students could well be fully in line with the BBT model, i.e., that only sufficiently self-controlled individuals profit from teaming up with similarly self-controlled peers.

Figure 8: Students' learning network and their self-control. Light-green (dark-red) stands for a high (low) level of self-control. Circle (box) indicates a female (male) student.



To test this hypothesis we first consider the variable *friends*, i.e., the binary variable measuring whether a student is connected to any friends at all (*friends* = 1). Indeed, students with high self-control are much more likely to have connections to peers (Pearson $\chi^2 = 12.159$ with p -value < 0.001 and Fisher's exact test yields p -value = 0.001).²⁵ To control for other individual characteristics, we run a probit regression with *friends* as a dependent variable, using self-control and further individual characteristics as covariates. A Hosmer-Lemeshow specification

²⁵ This result is not reported in the tables.

test shows that all the models are well specified. Table 11 displays the results. Self-control and talent both have a statistically significant positive impact on the likelihood of being connected in different model specifications. Indeed, the predicted probability of being connected is 21% higher for individuals with high self-control than for those with low self-control, holding all other covariates constant.

We also tested whether self-control tends to increase the number of friends a student is connected to. Since the dependent variable *degree*, the number of friends, is a nonnegative integer, we use Poisson and negative binomial models. Results are shown in Table 11. Since overdispersion is present, negative binomial models provide the best fit. Our results support our findings from the previous binary outcome model. Highly self-controlled students have a higher number of friends; and this effect is significant at the 5% level in both models. Specifically, a discrete change from having low self-control to having high self-control is associated with 0.64 additional friends. To check for robustness, we use the directed network and estimate the effect of self-control on outdegree and indegree, i.e. the number of friends a student nominated in the survey, respectively how often the student was nominated. Both measures from the directed network show a significant effect of similar size.²⁶ Overall, our estimates consistently show that highly self-controlled students are more likely to have friends and also tend to have more friends than students with low self-control. This is clear evidence for parts (a) and (b) of our hypothesis H4.

To test whether highly self-controlled students are also more likely to have highly talented friends, we ran two Poisson regressions with *talented friends* and *untalented friends* as response variables. Since the equidispersion property is not violated, models fitting the Poisson distribution are used. Results are reported in column 1 of Table 12. On average, the number of highly talented friends is increasing in self-control. This effect is positive and statistically significant, which supports part (c) of our hypothesis H4. The size of the effect is also substantial: Being highly self-controlled is associated with 0.35 additional talented friends.

We also tested whether self-control affects the number of lowly talented friends, i.e., the number of friends with a low math score. As can be seen from the second column of Table 12, we do not find any effect of self-control on the number of lowly talented friends. Hence, while a highly self-controlled student is more likely than a student with low self-control to have friends at all, the former also seems to be more selective in choosing her friends than the latter. Since students with more talented friends perform better in the exam, these results imply that self-control facilitates positive peer effects on performance: More self-controlled students have better

²⁶ These results are not reported in the tables.

access to highly talented peers and thereby accomplish more than their less well-connected peers. This is another channel through which self-control affects performance. Overall, we conclude that there is supportive evidence for all parts of our hypothesis H4 showing a strong link between a student's connectedness and her self-control. By contrast, we do not find any evidence in favor of our hypothesis H5, i.e., that self-control is correlated among direct friends. This, however, seems due to the fact that testing this hypothesis requires a dramatic reduction in sample size since a significant part of our students does not have any friends at all. Hence, we conclude that our results are sufficiently in line with the BBT model to provide support for it.²⁷

3.3.3 Gender Differences in Self-Control and Peer Effects

Since we conducted our field study in an educational setting where gender differences typically play an important role, we also investigate whether and how female students differ from male students in self-control, peer effects and performance.

Table 13 and Figure 9 display the results regarding self-control. Female students are significantly more self-controlled than their male counterparts: females' likelihood of being highly self-controlled is 38% higher than males', holding other covariates constant. Moreover, comparing the probabilities of being highly self-controlled between female and male students for different levels of talent, as we do in Figure 9, reveals, first, that the predicted values of being highly self-controlled are higher for female students for all levels of talent. Second, men's fitted probability of being highly self-controlled increases much more strongly in talent than women's, i.e. the men's slope is steeper. One possible explanation could be that women partly use self-control as a substitute for talent while men's self-control and talent are complements. To test whether this notable advantage over their male peers gives female students a head-start in the exam, we conducted a Wilcoxon rank-sum test which is displayed in Table 9. Moreover, we controlled for gender when examining determinants of students' performance (see all model specifications in Table 8). As can be seen from both tables, our findings do not indicate that women outperform men. If anything, men perform better than women. Hence, we conclude that the advantage of female students in terms of self-control must be counterbalanced by some disadvantage either in other determinants of performance or in the way in which self-control is used to increase performance. A second Wilcoxon rank-sum test which is displayed in Table 9

²⁷ Note that a very specific prediction of the BBT model is the claim that only sufficiently self-controlled individuals profit from connecting to others *at all*. This prediction could not be generated by a theory of homophile network formation, other than the prediction that self-control be correlated among friends. Hence, since we validate the former claim, we provide evidence in support of the BBT model.

reveals that women seem to scribe somewhat lower math scores than men, but this is not significant. Hence, we cannot conclude that women have a disadvantage in talent. Regarding their social network, neither are female students significantly less likely to have friends, nor do numbers of friends differ significantly between the sexes, as can be seen from Table 10 and Table 11. This is already an interesting observation, since one could expect the (more self-controlled) female students to be more likely than the male students to be connected to friends at all, given that self-control turned out to be an important determinant of connectedness. However, if we look deeper into the way in which female and male students form their social network, an interesting gender difference emerges that possibly counteracts the women's advantage in self-control: We find a compositional difference in how female and male students form their network. Female students seem less selective than male students in choosing their friends since there is a strong negative impact of being female on the number of talented friends which is significant at the 1% level (cf. Table 12). In particular, female students have 0.4 fewer highly talented friends compared to male students.

Hence, female students, although more self-controlled on average than male students, seem to use their self-control less efficiently to get access to highly talented peers and hence profit less from positive peer effects on performance.²⁸

3.4 Summary of Field Study Results

In the field study, our main contribution is to show that there exists a significant and sizable relation between connectedness to peers and self-controlled behavior. More self-controlled students are more likely to be connected and tend to have both more friends in total and more highly talented friends than their less self-controlled counterparts. Moreover, the connection to highly talented peers is an additional channel through which self-control positively affects performance. We also find gender differences: Female students are more self-controlled than their male counterparts, but do not outperform the latter although they seem to be equally talented. Our main finding of a positive relation between connectedness to peers and self-controlled behavior is in line with the theory provided by BBT.

²⁸ This finding seems to be very much in line with the empirical results of Lalanne and Seabright (2013) and the experimental findings of Friebel et al. (2013). Lalanne and Seabright (2013) find that the presence of influential individuals in a cohort of employees improves men's wages much more strongly than women's. Friebel et al. find that men's network formation is more reactive to short-term benefits (i.e., payoffs) than women's. Obviously, there are other possible explanations, for example that women perform worse than equally talented men in competitive environments (see, e.g., Gneezy, Niederle and Rustichini (2003), Niederle and Vesterlund, (2007), and Jurajda and Munich (2011)).

4 Discussion and Conclusion

In our lab experiment, we investigated the two causal channels through which peers might influence individual perseverance. One channel (“being observed by peers”) is stipulated by the empirical literature on peer effects, the other channel (“observing peers”) is stipulated in a theoretical model of self-control (BBT). Importantly, our findings on the interaction of communication and observed success corroborate the approach of BBT who argue that some assumed similarity between one’s own self-control and the self-control of one’s peers is necessary for one’s own perseverance to be affected by the peer’s success or failure. In our lab experiment, such beliefs are formed through communication when the advanced peers express their beliefs about how widespread their own success or failure may be among the experimental subjects (and, thus, how similar they believe themselves to be to the average student). Our results confirm the prediction of BBT that a peer’s success enhances own perseverance the more, the more likely it is that her type – i.e., her inner resource of self-control – is similar to one’s own: Only successful peers who expressed the belief that more than half of the experimental subjects were successful, too, had a significant stimulating effect on the perseverance of the subjects matched with them. Intuitively, their success could be interpreted as the message “Since I am an average person, and I did it, you can do it, too”. However, our experiment also shows that, unpredicted by BBT, subjects can be affected negatively by their peer’s success if they are told that their peer believes herself to be the exception rather than the rule. In this case, BBT would predict no effect rather than a negative effect. Psychologically, we believe our finding to be intuitive: The success of an – according to herself – exceptionally self-controlled peer may be interpreted as the message “I did it, but since I am an exceptional person, you do not even have to try”. Hence, our experiment is a first step to bridge the gap between BBT and the literature on how communication affects self-confidence. It confirms the important role that Bénabou and Tirole (2002, 2003) ascribe to the way advanced peers communicate with beginners whose self-confidence is still under formation; and it shows that – and how – one and the same fact (the success of an advanced peer) can be turned into a motivating or a de-motivating message.

Surprisingly, we find that a peer’s failure, even if justified by this peer herself as likely to be widespread among the experimental subjects, has no significant effect on own perseverance. At first glance, this seems contrary to BBT, too, who would predict a negative effect. However, BBT assume *objective public* information about the similarity between self-control levels among peers whereas we, in our lab experiment, implement *subjective private information* (through peer-to-peer communication) about such similarities. Hence, it seems that in our lab

experiment, only successful peers are taken to be credible in their communication about how similar they believe themselves to be to the average person. Therefore, the results of our lab experiment can be used to test the predictions of BBT about the effects of successful (highly self-controlled) peers on own perseverance but may reveal nothing regarding BBT's predictions about the effects of unsuccessful peers.

BBT assume that network formation is guided by rationality, i.e., peers connect to each other if getting “good news” from these peers is sufficiently likely and getting “bad news” sufficiently unlikely. One consequence of this assumption is that individuals of very low self-control tend to be unconnected, and that individuals of high self-control connect to each other. The main result of our field study is consistent with this prediction. However, one possible alternative interpretation of the higher connectedness among self-controlled students would be that students' social network (which might be exogenous to some extent) affects the degree to which students behave in a self-controlled manner. For instance, having friends with whom study activities are coordinated might serve as a commitment device that helps students to exhibit self-controlled behavior. In our lab experiment, we also found evidence for such a commitment effect. Hence, we conclude that both channels of causality can be active; they might even complement each other and lead to a virtuous circle between self-control and positive peer effects.

References

- Ainslie, G., and N. Haslam (1992): Hyperbolic Discounting. In: *Choice over Time*, ed. by G. Loewenstein and J. Elster, 57-92. Russell Sage Foundation, New York.
- Ali, S. (2011): Learning Self-Control. *Quarterly Journal of Economics* 126(2), 857-893.
- Ameriks, J., Caplin, A., Leahy, J., and T. Tyler (2007): Measuring Self-Control Problems. *American Economic Review* 97(3), 966-972.
- Andersen, S., Harrison, G., Lau, M., and E. Rutström (2008): Eliciting Risk and Time Preferences. *Econometrica* 76(3), 583-618.
- Andreoni, J., and C. Sprenger (2012): Risk Preferences are not Time Preferences. *American Economic Review* 102 (7), 3333-3356.
- Ariely, D., and K. Wertenbroch (2002): Procastination, deadlines, and performance: self-control by pre-commitment. *Psychological Science* 13(3), 219-224.
- Battaglini, M., Bénabou, R., and J. Tirole (2005): Self-control in peer groups. *Journal of Economic Theory* 123, 105-134.
- Battaglini, M., Díaz, C., and E. Patacchini (2017). Self-control and peer groups: An empirical analysis. *Journal of Economic Behavior & Organization* 134, 240-254.
- Bénabou, R., and M. Pycia (2002): Dynamic Inconsistency and Self-Control: a Planner-Doer Interpretation. *Economics Letters* 77 (3), 419-424.
- Bénabou, R., and J. Tirole (2002): Self-Confidence and Personal Motivation. *Quarterly Journal of Economics* 117 (3), 871-916.
- Bénabou, R., and J. Tirole (2003): Intrinsic and Extrinsic Motivation. *Review of Economic Studies* 70(3), 489-520.
- Benhabib, J., Bisin, A., and A. Schotter (2010): Present Bias, Quasi Hyperbolic Discounting, and Fixed Costs. *Games and Economic Behaviour* 69(2), 205-223.
- Burger, N., Charness, G., and J. Lynham (2011): Field and Online Experiments on Self-Control. *Journal of Economic Behavior and Organization* 77(3), 393-404.
- Calvo-Armengol, A., Patacchini, E., and Y. Zenou (2009): Peer Effects and Social Networks in Education. *Review of Economic Studies* 76, 1239-1267.
- Cameron, A., and P. Trivedi (2005): *Microeconometrics: Methods and Applications*, Cambridge University Press, New York.
- Corgnet, B., Hernán-González, R., and E. Schniter (2015). Why real leisure really matters: Incentive effects on real effort in the laboratory. *Experimental Economics*, 18(2), 284-301.

- Currarini, Sergio, Matthew O. Jackson, and Paolo Pin (2009): An economic model of friendship: Homophily, minorities, and segregation. *Econometrica* 77(4), 1003-1045.
- Falk, A., and A. Ichino (2006): Clean Evidence on Peer Effects. *Journal of Labor Economics*, 24(1), 39-58.
- Friebel, G., Lalanne, M., Richter, B., Schwardmann, P., and P. Seabright (2017): Women Form Social Networks More Selectively and Less Opportunistically than Men. SAFE Working Paper No. 168.
- Fudenberg, D., and D. Levine (2006): A Dual-Self Model of Impulse Control. *American Economic Review* 96(5), 1449-1476.
- Gerhards, L., & C. Gravert (2017). Because of you I did not give up -- How peers affect perseverance. Mimeo, University of Hamburg.
- Gneezy, U., Niederle, M., and A. Rustichini (2003): Performance in competitive environments: Gender differences. *Quarterly Journal of Economics* 118(3), 1049-1074.
- Gul, F., and W. Pesendorfer (2001): Temptation and Self-Control. *Econometrica* 69(6), 1403-1435.
- Houser, D., Schunk, D., Winter, J., and E. Xiao (2010): Temptation and Commitment in the Laboratory. IEW Working Papers 488, Institute for Empirical Research in Economics – University of Zurich.
- Hoxby, C. (2000): Peer Effects in the Classroom: Learning from Gender and Race Variation. National Bureau of Economic Research Working Paper 7867.
- Hoxby, C., and G. Weingarth (2005): Taking Race Out of the Equation: School Reassignment and the Structure of Peer Effects. Mimeo, Harvard University.
- Jackson, Matthew O., and Brian W. Rogers (2007): Meeting strangers and friends of friends: How random are social networks? *The American Economic Review* 97.3, 890-915.
- Jackson, M., and Wolinsky, A. (1996): A strategic model of social and economic networks. *Journal of Economic Theory* 71(1), 44-74.
- Jurajda, S., and D. Münich (2011): Gender Gap in Performance under Competitive Pressure: Admissions to Czech Universities. *American Economic Review* 101(3), 514-518.
- Laibson, D. (1997): Golden Eggs and Hyperbolic Discounting. *Quarterly Journal of Economics* 112(2), 443-478.
- Lalanne, L., and P. Seabright (2013): The Old Boy Network: Gender Differences in the Impact of Social Networks of Remuneration on Top Executive Jobs. Centre for Economic Policy Research Discussion Paper 8367.
- Mas, A., and E. Moretti (2009): Peers at Work. *American Economic Review* 99(1), 112-145.

Mechtenberg, Lydia (2009): Cheap Talk in the Classroom: How Biased Grading at School Explains Gender Differences in Achievements, Career Choices and Wages. *The Review of Economic Studies* 76(4), 1431-459.

Niederle, M., and L. Vesterlund (2007): Do women shy away from competition? Do men compete too much? *Quarterly Journal of Economics* 122(3), 1067-1101.

O'Donoghue, T., and M. Rabin (1999): Doing it now or later. *American Economic Review* 93(2), 186-191.

Strotz, R. (1955): Myopia and Inconsistency in Dynamic Utility Maximization. *Review of Economic Studies* 23(2), 165-180.

Thaler, R., and H. Shefrin (1981): An economic theory of self-control. *Journal of Political Economy* 89(2), 392-406.

Zimmerman, D. (2003): Peer Effects in Academic Outcomes: Evidence from a Natural Experiment. *Review of Economics and Statistics* 85(1), 9-23.

Appendix: Tables

A Tables of Lab Experiment

Table 1: Summary Statistics of Lab Experiment

variable	N	mean	sd	min	max	p50	mean if stayed (N=157)	mean if given up (N=178)
Puzzles	335	3.37	2.52	0	10	4	5.15	1.80
Bonus	335	0.34	0.47	0	1	0	0.73	0.00
MinutesTask	335	39.55	14.73	0.05	49.98	49.55	49.98	30.34
Payoff	335	15.73	3.19	8.10	23.50	15.20	17.12	14.5

Notes: Calculation is based on data from laboratory experiment conducted in 2015. The variable *Puzzles* is a count variable capturing the number of subtasks solved correctly. The variable *Bonus* is binary and equal to 1 if the bonus was reached; zero otherwise. The variable *MinutesTask* captures the time elapsed until the “terminate task” button is pushed. When it is never pushed, then *MinutesTask* is fifty, which is measured by the computer as 49.98. Variable *Payoff* captures the experimental payout in EUR. “Given up” means that the “terminate task” button is pushed at some point, “stayed” means that it was never pushed.

Table 2: MinutesTask by Puzzles if Given Up

Puzzles	0	1	2	3	4	5	6	7	8	Total
Mean	17.29	27.83	36.88	43.74	43.91	45.01	41.66	46.41	46.90	30.34
Std. Dev.	12.05	10.91	10.68	7.21	7.31	4.38	5.09	0.11	4.03	15.08
Freq.	63	36	24	19	15	12	5	2	2	178
Rounds	0	1	2	3	4	5	6	7	8	Total
Mean	17.29	27.83	36.88	43.74	43.91	45.01	41.66	46.41	46.9	30.34

Notes: The variable *MinutesTask* captures the time elapsed until the “terminate task” button is pushed. This table only reports the observations in which this button was pushed at some point (“Given Up”). *Puzzles* is a count variable capturing the number of subtasks solved correctly.

Table 3: Basic Regressions

	(1) Poisson Puzzles	(1') IRR Puzzles	(2) Logit Bonus	(2') Odd Ratios Bonus	(3) OLS Payoff
BonusPeer	-0.892* (0.476)	0.410*** 0.125	-2.956** (1.358)	0.052** 0.071	-3.586** (1.455)
FractionSuccess	-0.006 (0.005)	0.994 0.004	-0.018 (0.013)	0.982 0.014	-0.025 (0.018)
FractionSuccess x BP	0.018** (0.008)	1.019*** 0.006	0.064*** (0.024)	1.066*** 0.026	0.075*** (0.027)
constant	1.294*** (0.157)		-0.444 (0.443)		16.367*** (0.657)
N	110	110	110	110	110
χ^2	6.282	11.594	7.566	8.869	
P	0.099	0.009	0.056	0.031	0.060
Pseudo R^2	0.021	0.021	0.065	0.065	
F					2.543
R^2					0.058

Notes: Dependent variables are *Puzzles* in models (1) and (1'), *Bonus* in models (2) and (2'), and *Payoff* in model (3). Robust standard errors are reported in parentheses. Variable *FractionSuccess* is the fraction of participants who receive the bonus as estimated by the matched peer of the observed subject. The variable *BonusPeer* is equal to 1 if the matched peer has received the bonus and zero otherwise. "FractionSuccess x BP" stands for the interaction term between *FractionSuccess* and *BonusPeer*. *, **, *** indicate significance at the 10%, 5% and 1% level.

Table 4: Effect of BonusPeer at Different Levels of FractionSuccess

	FS0	FS10	FS20	FS30	FS40	FS50	FS60	FS70	FS80	FS90	FS100
Puzzles											
Poiss. IRR	0.410*** 0.125	0.493*** 0.127	0.592** 0.127	0.712* 0.127	0.856 0.132	1.028 0.153	1.236 0.201	1.486** 0.286	1.786** 0.414	2.147*** 0.594	2.581*** 0.838
Bonus											
Logit OR	0.052** 0.071	0.098** 0.114	0.186* 0.178	0.353 0.277	0.668 0.441	1.263 0.777	2.391 1.586	4.524* 3.565	8.561** 8.22	16.202** 18.772	30.661** 42.07
Payoff											
OLS	-3.586** (1.455)	-2.841** (1.223)	-2.095** (1.011)	-1.35 (0.836)	-0.605 (0.724)	0.14 (0.707)	0.885 (0.79)	1.631* (0.947)	2.376** (1.149)	3.121** (1.375)	3.866** (1.616)

Notes: Dependent variables are *Puzzles* in the first row, *Bonus* in the second row, and *Payoff* the third row. The columns stand for the effect of the bonus of the matched peer when her estimated fraction of subjects who reach the bonus is 0, 10, ..., 100. As detailed in the main text, we obtain these effects by using the variable *FractionSuccess_k*, which is defined as *FractionSuccess* minus constant *k*, and considering the coefficient for *BonusPeer*. The coefficients in the first row report the incident rate ratios (IRR) of the Poisson regressions that correspond to model (1') in Table 3. The coefficients in the second row report the odd ratios of the logit regressions that correspond to model (2') in Table 3. The coefficients in the third row report the marginal effects of the ordinary least squares (OLS) regressions that correspond to model (3) in Table 3. Robust standard errors are reported in brackets. *, **, *** indicate significance at the 10%, 5% and 1% level.

Table 5: Outcome Variables by Recommendation and Bonus of the Matched Peer

Bonus of Peer Recommendation by Peer	Any		No Bonus		Bonus	
	Give Up	Stay	Give Up	Stay	Give Up	Stay
N	68	53	56	16	12	37
Puzzles	2.81	3.47	2.94	3.04	1.75	3.7
Bonus	26.47%	37.74%	30.36%	31.25%	8.33%	40.54%

Notes: Means of outcome variables *Puzzles* and *Bonus*. Columns distinguish cases by whether the matched peer has reached the bonus and by the peer’s recommendation. The coding of the chat for the recommendation entails three categories: “stay in the task”, “work first on the task and quit later”, and “quit immediately”. The binary variable *Recommendation* simply distinguishes between the first (“Stay”) and the two latter categories (“Give Up”).

Table 6: Puzzles by Commitment

Puzzles	N	mean	Sd	p50	Frac >0	Frac >1
“committed leaders” (CL)	84	3.83	2.37	4	90.5%	81.0%
Others (UCL, NewCL, NewUCL)	251	3.22	2.56	3	76.9%	63.8%
“uncommitted leaders” (UCL)	84	3.38	2.54	4	77.4%	67.9%
“newcomers with committed leaders” (NewCL)	83	3.13	2.54	3	79.5%	63.9%
“newcomers with committed leaders” (NewUCL)	84	3.13	2.62	4	73.8%	59.5%
Total	335	3.37	2.52	4	80.3%	68.1%

Notes: The variable *Puzzles* captures the number of subtasks solved correctly. The columns “Frac > 0” and “Frac > 1” report the fraction of observations that solved more than zero, respectively more than one, subtasks correctly. Rows distinguish observations by treatments.

B Tables of Field Study

Table 7: Summary Statistics of Field Study

Variable	# of obs.	# of obs. if var =1	Mean	Std. dev.	p50	Min	Max
micro score (performance)	96	-	56.59	20.70	58	0	88
math score (talent)	100	-	48.19	20.80	51	0	82
self control (1=yes)	117	63	0.54	0.50	1	0	1
gender (1=female)	117	52	0.44	0.50	0	0	1
friends (1= at least one friend)	117	55	0.47	0.50	0	0	1
degree	117	23	0.94	1.33	0	0	7

Notes: Calculation based on data collected in spring and summer 2013. Sample includes first year economics students. Variables *micro score*, *math score* and *effort* are metric. The construction of *self control* is described in detail in section 3.2. The variable *gender* is binary and 44% of the students are female. The variable *friends* is binary as well. 55 students reported to have studied with at least one friend for the micro course. The count variable *degree* gives the number of friends a student has.

Table 8: Self control, Talented Peers and Performance

	(1) OLS micro score	(2) OLS micro score	(3) FGLS micro score	(4) OLS micro score	(5) FGLS micro score
self control (1=yes)	8.896* (4.737)	8.553* (4.932)	7.974** (3.734)	7.125 (4.854)	6.831* (3.760)
math score	0.580*** (0.110)	0.579*** (0.111)	0.578*** (0.086)	0.580*** (0.105)	0.580*** (0.084)
gender (1=female)	-4.251 (4.413)	-4.008 (4.484)	-4.089 (3.300)	-2.111 (4.426)	-2.230 (3.376)
degree		0.574 (1.194)	1.097 (1.126)		
talented friends				4.340** (1.938)	4.067** (1.962)
constant	23.805*** (6.385)	23.300*** (6.471)	23.142*** (5.489)	21.807*** (6.225)	22.188*** (5.337)
Observations	88	88	88	88	88
adj. R^2	0.422	0.417	0.436	0.441	0.460
F-statistic	22.743	17.276	17.836	18.837	19.540
p value F-statistic	0.0000	0.0000	0.0000	0.0000	0.0000

Notes: Dependent variable is the score obtained in Introductory Microeconomics exam. Robust standard errors are reported in parentheses. *, **, *** indicate significance at the 10%, 5% and 1% level. Feasible generalized least square (FGLS) is calculated with a skedasticity function that includes math score and self control.

Table 9: Wilcoxon Rank-Sum (Mann-Whitney) Tests

performance by gender	observations	rank sum	expected
male	55	2915.5	2667.5
female	41	1740.5	1988.5
combined	96	4656	4656
z value	1.838		
p value of z	0.0661		
talent by gender			
male	61	3310.5	3080.5
female	39	1739.5	1969.5
combined	100	5050	5050
z value	1.626		
p value of z	0.1040		
performance by early submission and workshop attendance			
yes (both criteria fulfilled)	31	1980	1829
no (only one or none criterion fulfilled)	86	4923	5074
combined	117	6903	6903
z value	-0.933		
p value of z	0.3509		

Notes: Performance is measured as the score obtained in Introductory Microeconomics exam. Talent is measured as the score obtained in the Math exam. Results on performance and talent indicate, if anything, higher levels for men. There is no difference in performance with regard to workshop attendance and early submission of midterm assignment.

Table 11: Self-Control on General Connectedness

	(1) poisson degree	(2) negative binomial degree	(3) negative binomial degree
math score	0.006 (0.008)	0.006 (0.007)	0.006 (0.008)
gender (1=female)	-0.286 (0.264)	-0.318 (0.279)	-0.318 (0.272)
self control (1=yes)	0.627** (0.271)	0.641** (0.296)	0.641** (0.270)
constant	-0.539 (0.561) (0.395)	-0.517 (0.366) (0.409)	-0.517 (0.589) (0.491)
η_{sc}			0.639** (0.255)
Observations	100	100	100
Wald-statistic	8.307	8.209	9.036
p value Wald-statistic	0.040	0.042	0.029
dispersion parameter α		0.526	
p value α		0.0014	
loglikelihood	-143.9276	-138.8002	-138.8002
AIC	295.8553	287.6004	287.6004
BIC	306.2760	300.6263	300.6263

Notes: Dependent count variable degree is defined as number of friends. Robust standard errors are in parentheses for the first and third model. The second model was calculated without robust standard errors to retrieve the dispersion parameter α . Overdispersion is present. η_{sc} : average marginal effect of degree with respect to self control. *, **, *** indicate significance at the 10%, 5% and 1% level.

Table 10: Self-Control on General Connectedness

	(1)	(2)	(3)
	probit	probit	probit
	friends (1=yes)	friends (1=yes)	friends (1=yes)
math score	0.020*** (0.007)	0.020*** (0.007)	0.018* (0.009)
gender (1=female)	-0.077 (0.310)	0.081 (0.535)	-0.333 (0.728)
self control (1=yes)	0.593* (0.303)	0.673* (0.357)	0.602** (0.306)
gender x self control		-0.243 (0.632)	
gender x talent			0.005 (0.014)
constant	-1.143*** (0.395)	-1.173*** (0.409)	-1.038** (0.491)
η_{math}	0.0067*** (0.002)		
η_{sc}	0.215** (0.109)		
Observations	100	100	100
pseudo R^2	0.131	0.132	0.132
F-statistic	15.368	15.429	15.893
p value F-statistic	0.0015	0.0039	0.0032
loglikelihood	-59.9514	-59.8717	-59.8730
AIC	127.9028	129.7434	129.7459
BIC	138.3235	142.7692	142.7718

Notes: Dependent variable friends is binary and equal to 1 if the student named at least one learning partner; zero otherwise. Robust standard errors in parentheses. η_{math}/η_{sc} : average marginal effect of friends w.r.t. math score/ self control. *, **, *** indicate significance at the 10%, 5% and 1% level.

Table 12: Self-Control and (Un-)talented friends

	(1) poisson talented friends	(2) poisson untalented friends
math score	0.005 (0.008)	0.007 (0.011)
gender (1=female)	-0.941*** (0.337)	0.131 (0.357)
self control (1=yes)	0.864** (0.360)	0.341 (0.346)
constant	-1.279** (0.550)	-1.272 (0.775)
η_{gender}	-0.391*** (0.136)	
η_{sc}	0.375*** (0.141)	
Observations	100	100
Wald-statistic	15.911	1.547
p value Wald-statistic	0.001	0.671
loglikelihood	-85.142	-99.701
AIC	178.284	207.403
BIC	188.704	217.823

Notes: Dependent variable is the number of (un-)talented friends and is a count variable. Talented friends are friends that have a math score weakly above the median of the cohort, untalented friends have a math score below the median of the cohort. Robust standard errors in parentheses. Overdispersion is not present and both models are fitted using Poisson distribution. η_{gender} / η_{sc} : average marginal effect of dependent variables w.r.t. gender and self control. *, **, *** indicate significance at the 10%, 5% and 1% level.

Table 13: Gender and Self-Control

	(1) probit self control (1=yes)	(2) probit self control (1=yes)	(3) probit self control (1=yes)
math score		0.024*** (0.008)	0.026*** (0.010)
gender (1=female)	0.623*** (0.240)	1.154*** (0.320)	1.397* (0.796)
gender x talent			-0.006 (0.017)
constant	-0.174 (0.157)	-1.391*** (0.461)	-1.498*** (0.571)
η_{math}		0.0078*** (0.002)	
η_{gender}		0.375*** (0.085)	
Observations	117	100	100
F-statistic	6.754	15.663	16.439
p value F-statistic	0.0094	0.0004	0.0009
loglikelihood	-77.2923	-56.3700	-56.2932
AIC	158.5847	118.7400	120.5863
BIC	164.1090	126.5555	131.0070

Notes: Dependent variable self control is binary and equal to 1 if the student behaved in a time consistent manner concerning early goal achievement; zero otherwise. Robust standard errors in parentheses. *, **, *** indicate significance at the 10%, 5% and 1% level. $\eta_{math}/\eta_{gender}$: average marginal effect of self control w.r.t. math score and gender.

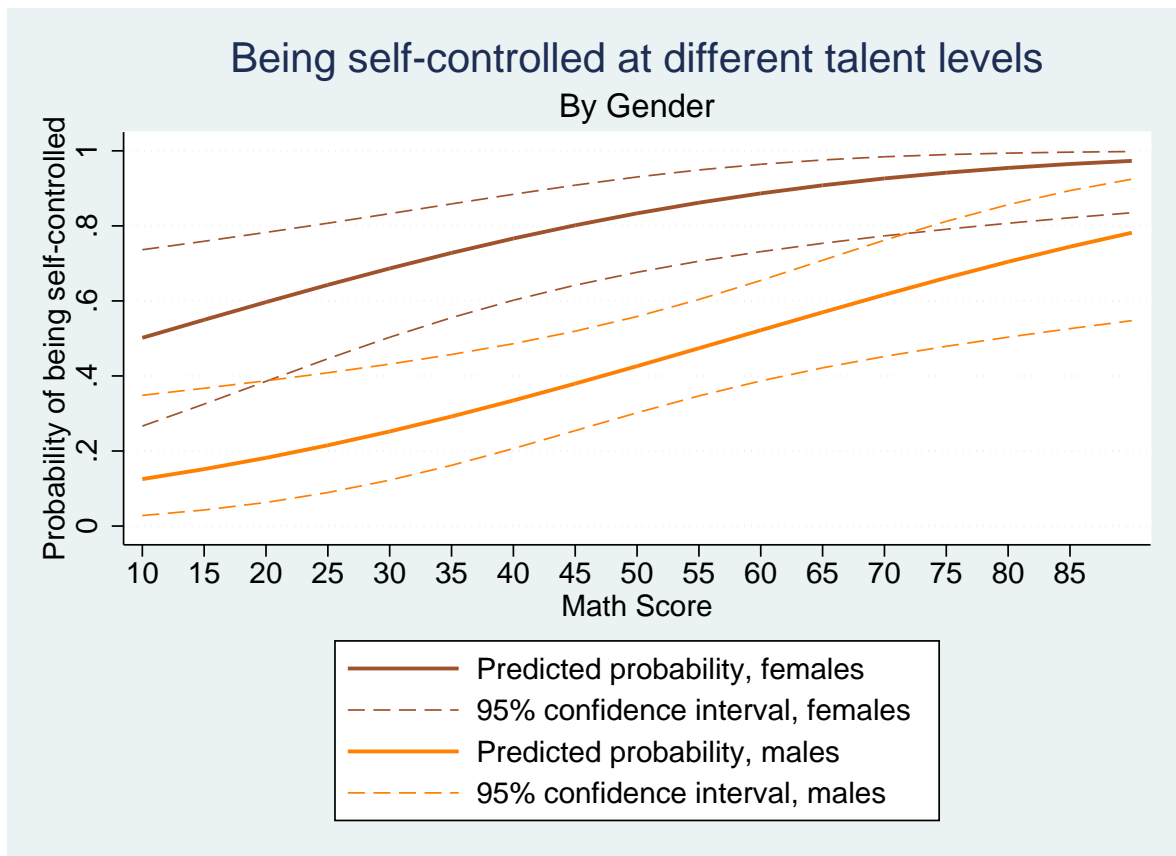


Figure 9: Self-control by Gender

Authors

Berno BUECHEL

University of Fribourg, Department of Economics, Bd. de Pérolles 90, 1700 Fribourg, Switzerland.

Phone: +41 26 300 8250; Email: berno.buechel@unifr.ch Website: <http://www.unifr.ch/amabe/en/team/berno-buechel>

Lydia MECHTENBERG

University of Hamburg, Department of Economics, Von-Melle-Park 5, 20148 Hamburg, Germany.

Email: lydia.mechtenberg@wiso.uni-hamburg.de Tel.: +49-30-25491-446

Julia PETERSEN

University of Hamburg, Department of Economics, Von-Melle-Park 5, 20148 Hamburg, Germany.

Email: petersen.julia@t-online.de Tel.: +49-30-25491-446

Abstract

Successful performance – be it in school, at the job, or in sports activities – requires perseverance, i.e., persistent work on a demanding task. We investigate in a controlled laboratory experiment how an individual's social environment affects perseverance. We find evidence for two kinds of peer effects: being observed by a peer can serve as a commitment device, while observing a peer can be informative. In particular, we show that successful peers affect perseverance positively if they communicate their success in a motivating way and negatively otherwise, while perseverance is unaffected by unsuccessful peers. Our experimental results suggest that peers affect perseverance indirectly, via influencing self-confidence. We turn to field data from an educational setting and find that students seem to be able to harness the power of peer effects, by selecting into groups that help them reach their goals.

Citation proposal

Berno Buechel, Lydia Mechtenberg, Julia Petersen. 2017. «Peer Effects on Perseverance». Working Papers SES 488, Faculty of Economics and Social Sciences, University of Fribourg (Switzerland)

Jel Classification

C91, D90, I21, J24

Keywords

Self-control, Peer Effects, Social Networks, Experiment

Working Papers SES collection

Last published

483 Imhof D.: Econometric Tests to Detect Bid-rigging Cartels: Does it Work?; 2017

473 Deuchert E., Huber M., Schelker M.: Direct and indirect effects based on difference-in-differences with an application to political preferences following the Vietnam draft lottery (first revised edition); 2017

484 Imhof D.: Simple Statistical Screens to Detect Bid Rigging; 2017

485 Buechel B., Mechtenberg L.: The Swing Voter's Curse in Social Networks; 2017

486 Buechel B., Klössner S., Lochmüller M., Rauhut H.: The Strength of Weak Leaders. An Experiment on Social Influence and Social Learning in Teams; 2017

487 Denisova-Schmidt E., Huber M., Leontyeva E., Solovyeva A.: Combining Experimental Evidence with Machine Learning to Assess Anti-Corruption Educational Campaigns among Russian University Students;

Catalogue and download links

<http://www.unifr.ch/ses/wp>

http://doc.rero.ch/collection/WORKING_PAPERS_SES

Publisher

Université de Fribourg, Suisse, Faculté des sciences économiques et sociales
Universität Freiburg, Schweiz, Wirtschafts- und sozialwissenschaftliche Fakultät
University of Fribourg, Switzerland, Faculty of Economics and Social Sciences

Bd de Pérolles 90, CH-1700 Fribourg
Tél.: +41 (0) 26 300 82 00
decanat-ses@unifr.ch www.unifr.ch/ses