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## Fear of being left alone drives inefficient exit from partnerships: An experiment

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# JENA ECONOMIC RESEARCH PAPERS



# 2014 – 012

## **Fear of being left alone drives inefficient exit from partnerships. An experiment**

by

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# Fear of being left alone drives inefficient exit from partnerships. An experiment.\*

Alexia Gaudeul,<sup>†</sup> Paolo Crosetto<sup>‡</sup> and Gerhard Riener<sup>§</sup>

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## Abstract

We explore in an experiment what leads to the breakdown of partnerships. Subjects are assigned a partner and participate in a repeated public good game with stochastic outcomes. They can choose each period between staying in the public project or working on their own. There is excessive exit as subjects overestimate the likelihood their partner will leave. High barriers to exit thus improve welfare. We observe that exit is driven by failure within the common project but also by pay-off comparisons across options and beliefs about being exploited. Those considerations increasingly matter as we lower exit costs across treatments.

*JEL Codes:* C23, C92, H41

*Keywords:* breakup, collaboration, cooperation, exit, imperfect public monitoring, moral hazard, partnerships, punishment, public good, repeated game, social risk, teams.

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Partnerships are situations in which two or more persons join to pursue a common project. Being together increases the chances of success of the project, whether the project aims at raising children, establishing a business or writing a scientific article. Much has been written about the issue of free riding in such situations: one of the partners may rely on the others to do most of the work while keeping on enjoying its benefits. This issue can lead to inefficient situations where both partners contribute very little. A comparatively small part of the academic literature deals with the dissolution of partnerships and why partners decide to stop working together. Both low contribution levels and dissolution indicate failure in a partnership, but the distinction between those two types of failures is important; it is akin to the distinction between a dysfunctional marriage that keeps on going, and a marriage that ends in a divorce.

This paper deals with the inner dynamics of partnerships, in particular with how success and failure determine the probability a common project will break down. We investigate the effect of events within the project that affect participants beliefs about the level of involvement of others. We vary the relative profitability of joint and individual work to see how this influences beliefs and behavior. We find that subjects over-estimate the probability their partner will leave the partnership. This leads them to preemptively leave the partnership even when they believe the effort of their partner is high enough to make staying more efficient. Inefficient breakdown is not compensated by having more efficient surviving partnerships. Indeed, we find that whether exit costs are high or low does not influence the level of cooperation in surviving partnerships. Finally, we find that exit is ever more unlikely to be followed by a return to cooperation as the cost of exit decreases. We explain this by contrasting two drivers of exit. A myopic cost-benefit analysis takes the situation as given and leads to exit if working alone provides higher utility. A strategic longer-term motivation for exit is aimed at influencing the workings of the partnership. We give evidence that the short-term cost-benefit driver of exit becomes more important as exit costs decrease, which explains why exit is more likely to be final when exit costs are low.

**Experimental design** The questions we are interested in would be hard to study empirically in a real world setting. It would be very difficult and time consuming to chart the contributions of real life partners to their relationship over time, to know how the perceived attractiveness or availability of outside options varies over time or between different partnerships, or even to know what is considered as a success or a failure in a common project. This is why we chose to lead a laboratory experiment on the topic.

We put in place an original experimental design that borrows from the standard public good game (also called voluntary contribution mechanism) in so far as contributions into the public good are pooled and determine the outcome, and from simple models of moral hazard in so far as the amount of investment affects the *probability* a project will be successful and partners cannot monitor the investment made by others. To the difference of the standard designs, our subjects are given the option to participate either in private or in public good production, and then are asked to choose how much to invest in their chosen project. There is no option to invest in both public and individual projects at the same time. The sum of investments in the public good determines the probability of a good outcome, and similarly one's investment in the private good determines the probability it will be successful. Subjects must decide how much to invest in their chosen project based on the commonly known relationship between the amount of investment and the probability of success. The relation between investment and probability of success, while strictly increasing, is not linear but rather concave. This means there is an interior optimal level of investment so that subjects should not invest the maximum possible amount in their chosen project. The endowment which is given to subjects at the start of the game, and which is the maximum they are allowed to invest, is set so that even investing the full endowment cannot guarantee success.

We chose to make outcomes stochastic because this gives meaning to exit: exiting is different from contributing nothing as exiting is a public event while not contributing is a hidden decision. We also think this makes our design closer to the issues arising from collab-

oration in innovative teams, where the output is not in a one-to-one relation with input. Our design reflects salient aspects of team work in innovative and rapidly changing environments, whereby the outcomes of one's effort is difficult to predict. It also reflects modern organizations of collaborative work whereby participants in teams may be located in different places and deal with different aspects of a project, which makes it difficult for them to monitor and judge their partner's commitment to the joint project. In keeping with the mounting precariousness of work relations, individuals in our experiment are able to independently choose each period whether to keep on working in a team or work on their own. We believe that the difficulties in attributing failure to either low effort by others or to bad luck make this game more relevant to real world team situations and more interesting from a psychological point of view than the standard public good game. Individuals who face uncertainty about the level of contribution of their partner and about how their partner will respond to failure may become anxious, feel guilt in case of failure or worry that they are being exploited. Those who are most sensitive to such feelings may thus prefer to switch to private production. Stochasticity in outcomes is also interesting because it reduces some of the incentives and mechanisms driving public good contribution that have been identified in the literature, such as the warm glow effect (Palfrey and Prisbrey, 1997) and the setting of contributions based on the contributions of others (conditional cooperation, cf. Keser and van Winden, 2000; Fischbacher et al., 2001). Repeated game effects are also reduced since contributions are only imperfectly monitored through success and failure.

**Literature** The existing literature on the topic focuses on how ease of exit impacts *how much effort* individuals will devote to the common project and their *likelihood to stay* in it. Its goal is to determine whether having outside options is beneficial or whether it is better for both partners to be tied to the project for its duration.

On the side of the *benefits of allowing exit* from partnerships and of making it easy to exercise the exit option, 1) exit is more efficient

than staying in a dysfunctional partnerships if given present levels of effort in the partnership, both partners would be better off working on their own (MacLeod, 1993). 2) The threat of exit is necessary to enforce cooperation if supervision is ineffective or too costly (Lin, 1990). Exercising the exit option provides a clear way to tell one's partner that one is contributing nothing to the project for this period. Furthermore, exiting may reduce the payoff of the partner by more than one could do by not devoting efforts to the project (Fujiwara-Greve and Yasuda, 2011). 3) Allowing exit frames the collaboration as the product of the *free will* of both partners and thus promotes intrinsic motivations for cooperation (Yamagishi, 1988). 4) Choosing to stay in the partnership is a sign of trust rather than the product of coercion. Being able to give this sign of trust promotes cooperation because people are generally keen to uphold the trust of others (Bravo and Squazzoni, 2013). 5) Allowing exit may also allows *self-selection*: those who do not trust others and would thus lead inefficient partnerships can work on their own rather than burden others, while those who are content working with others build partnerships that are stable and efficient (Orbell et al., 1984).

On the side of the *negative consequences of allowing exit*, 1) outside options reduce the interdependence between partners. A partner cannot punish the other for perceived low effort by withdrawing contribution for a while because the other can escape punishment by working on his own during this punishment period (MacLeod, 1988; Putterman and Skillman, 1992; Dong and Dow, 1993) 2) allowing exit may frame the partnership as a self-interest competition as partners are encouraged by the availability of an outside option to think of their own self-interest (could I do better outside?), rather than of the interest of the group (how can I make it better within the group?) (Tenbrunsel and Messick, 1999) 3) sanctions, such as exit, lower trust in others or in their motives (Mulder et al., 2006), especially if the sanctions are small (Houser et al., 2008).

*Experimental research* on the benefits of outside options within partnerships is quite sparse. A possible reason for the lack of interest in the outside option is that in the standard implementation of

the public good game agents are endowed with funds that they may allocate in part or in full to the public project, with the remainder for them to keep. In that setting, exit corresponds to the amount that was not invested in the public good. The profitability of exit can be changed by varying the profitability of keeping one's endowment, so that one can compare treatments with different returns on private goods or with different returns on public goods. Previous research shows "a strong negative relationship between the marginal rate of substitution (between private and public goods) and the rate of contribution" (Palfrey and Prisbrey, 1996, p. 413). However the standard implementation of the public good game is deterministic. The sum of contributions is multiplied by a factor greater than one and redistributed equally to all. One can therefore assess the average contributions of others. Introducing an element of uncertainty as in our experiment makes it more difficult for our subjects to judge from the outcome the level of involvement in the public good by the other. Making it possible to publicly exit from the partnership thus expands the range of available options as exit is different from contributing nothing. Exiting gives the others a clear indication that one is not anymore willing to contribute to the public good.

Experimental work on the effect of introducing an outside option in the prisoner's dilemma is surveyed in Kurrild-Klitgaard (2010). Closer to ours are papers that consider public good production. Keser and Montmarquette (2011) give the option between public and private good production under perfect public monitoring, and varies the value of the outside option. Like in this paper, payoffs are not linear in effort. Subjects must choose what project to participate in and then choose how much to invest in their chosen project, whether that is a public project or an individual project. They find that providing an outside option increases investments levels in public projects. However Boun My and Chalvignac (2010), who consider 5-players groups playing a linear public good game with fixed exit payoffs, do not find an effect of outside options on contribution levels. Our experiment differs from previous ones in three aspects. The first is that exit is an individual decision. Defection to an individual project by one agent



does not imply the other agent has to exit as well. The second is that exit is reversible. Exit does not imply either that the agent cannot return to the public project in the next period. This means that our subjects always faced the same range of options each period. Every period was formally comparable to the others in terms of what decisions were to be taken. The third difference is that subjects cannot monitor the contribution of others. Experimental research has found that uncertainty about the contribution of others or about the outcomes of cooperation reduces cooperation levels (Van Dijk et al., 2004; Van Lange et al., 2013, pp. 131–132). Uncertainty also makes the availability of punishment devices less effective in maintaining cooperation and punishments tend to be overused as cooperative partners are almost as likely as non-cooperative ones to be punished (Ambrus and Greiner, 2012; Grechenig et al., 2010; Xiao and Kunreuther, 2010).

## **1 The experiment**

Sessions were run in March 2012 in the experimental laboratory of the Max Planck Institute for Economics in Jena, Germany. A total of 316 participants took part over 10 sessions. Almost all subjects were university students in Jena, 55% of them were female, average age was 24 and 16% were studying economics or business administration. This composition was stable across treatments (Table 8 in Appendix A). Average remuneration was €15 and the experiment lasted about one hour and a half. The experiment was computerized with the Zurich Toolbox for Ready-made Economic Experiments (z-Ttree, Fischbacher, 2007) and subjects were recruited using the Online Recruitment System for Economic Experiments (ORSEE, Greiner, 2004). Translated English instructions are provided in Appendix F. The experimental software and the original German instructions are available upon request.

## 1.1 Main part of the experiment

Each subject  $i$  was anonymously and randomly paired with a partner  $j$  and had to make a succession of decisions over several periods. Subjects had to decide each period  $t$  whether to participate in the public project ( $d_{it} = 1$ ) or in their own individual project ( $d_{it} = 0$ ). Participants were then asked to state their belief about the project choice of their partner, in terms of likelihood that their partner had stayed in the public project ( $d_{jt}^e \in [0, 1]$ ). Subjects then learned the decision of their partner,  $d_{jt}$ , and had to decide what level of effort to exert in their chosen project ( $e_{it} \in [0, 10]$ ). They were then asked to state their belief about the effort choice of their partner ( $e_{jt}^e \in [0, 10]$ ). For consistency, this was elicited whether the partner was in the same project as themselves or not. Belief elicitation was incentivized using a linear scoring rule.

Given their own effort and the effort of their partner, success in the chosen project was determined as follows:

- If the subject had chosen the public project, then his probability of success was  $f(e_{it} + d_{jt} \times e_{jt})$  with function  $f(z) = \sqrt{\frac{z}{22}}$  commonly known and presented as a table for all combinations of integer between 0 and 10. Subjects were also given an opportunity to compute success probabilities for non-integer levels of effort, and were allowed to express effort with a precision up to the second decimal point.
- If the subject chose to participate in his own individual project, then success occurred with probability  $f_I(e_{it})$ , with function  $f_I(e_{it})$  depending on the treatment (more on this later).

Agents were then informed of the success or failure of their chosen project, but not of that of the project chosen by the other (if the other participated in another project), neither of the effort exerted by the other. Agent  $i$  obtained payoff  $v_i$  if the project she was involved in was successful, 0 else.  $v_i$  and  $v_j$  were commonly known by both partners. Payoff for subject  $i$  in period  $t$  was thus  $v_i - e_{it}$  in case of success and  $-e_{it}$  in case of failure.

Subjects were told that their partnership would keep on in the next period with probability 95% and would dissolve otherwise, in which case they would be paired with someone else next period. They were told they would be assigned three partners in succession over the course of the experiment and that they would not be matched with the same person over the course of the experiment, neither with anyone who was matched with someone they were previously matched with (perfect stranger matching). However we drew the number of periods in each partnership beforehand according to the announced process, so that in each sessions the first partnership lasted 8 periods, the second partnership 14 and the last partnership 10.

There were 32 subjects in each of our sessions, except for one session with only 28 subjects. This means that there were up to 16 partnerships in any period. Denote  $k \in \{1, \dots, 16\}$  a partnership. Success occurred in period  $t$  of partnership  $k$  for individual  $i$  if  $f(e_{it} + d_{jt} \times e_{jt}) > r_{kt}$  if the individual was in the public project and if  $f_I(e_{it}) > r_{kt}$  if the individual was in an individual project. Each element of the matrix  $[r_{kt}]$  which determined success or failure was drawn beforehand from a uniform distribution over the interval  $[0, 1]$ , independently across periods ( $t$ ) and partnerships ( $k$ ). The same matrix was used for all sessions. This was done in order to control for possible effects of biased random draws in small samples.

The participants were shown at the end of each period a history box showing their past project and effort choices in the current partnership, as well as whether their chosen project was a success or a failure. They also saw past project choices of their partner in the current partnership. They were given no information about the effort and success of their partner.

## 1.2 Control tasks, questionnaire and determination of payoffs

At the end of the main part of the experiment, participants completed two unannounced and incentivized control tasks to assess their attitudes to risk, to strategic uncertainty and their social value orien-

tation. We used the Strategic Uncertainty and Risk Aversion tasks (*su* and *ra*) of Heinemann et al. (2009) and a short, 6-item version of the Social Value Orientation (*svo*) measure by Murphy et al. (2011). In addition to those incentivized controls, we collected the gender (*female*), age (*age*) and field of education (*educ*) of the participants. We also assessed their degree of trust in others along Gächter et al. (2004) (*trust*, high values indicate more trustful individuals). Appendix G gives more details on the tasks and the questionnaire. For a recent review of the impact of social value orientation and trust on cooperation in social dilemmas, see Van Lange et al. (2013, pp. 132–133). Risk aversion may lead subjects to invest more in order to reduce uncertainty, while strategic uncertainty aversion could lead them to avoid joint work. About gender effects, Kuhn and Villeval (2013) give evidence that women are more attracted to cooperation. Age and education were collected because older people may be more pro-social (List, 2004), while economics students may be more likely to free-ride (Marwell and Ames, 1981; Frank et al., 1993).

A random period was drawn at the end of the experiment and success or failure of the chosen project was determined. Subject  $i$  received 0 or  $v_i$  depending on the result, minus effort  $e_{it}$  exerted in that period. In addition to this, agents were paid for the accuracy of their elicited beliefs that period (linear scoring rule), and also received payments for the control tasks. The final payment also included a €5 participation fee which was enough to cover any possible loss in the experiment.

### 1.3 Treatments

As in Keser and Montmarquette (2011), treatments varied in terms of the incentive to exit and in terms of individual payoffs that participants obtained on successful completion of their project. There were 5 treatments overall, each with two sub-treatments, resulting in 10 sessions. Each session involved 32 subjects except one with only 28 subjects.

### 1.3.1 Incentive to exit

Incentive to exit was varied through the function  $f_I(e_{it})$  determining the probability of success as a function of effort in the individual project. A control treatment was run whereby subjects could not exit the common project (Treatment 0). Treatments 1 to 4 varied expected payoff in the individual project. Treatment 1 gave payoff 0 when in an individual project and was designed to check whether simply having the possibility to exit, however unprofitable, changed the outcome. Probability of success in the individual project in treatments 2, 3 and 4 was  $f(e_{it} + x_i) = \sqrt{(e_{it} + x_i)/22}$  with  $x_i = \{0, 2, 4\}$  the subsidy to individual projects in treatments 2, 3 and 4 respectively. We always had  $x_i = x_j$  in each treatments, that is, both agents obtain the same subsidy to individual production. Treatment 2 was therefore such that  $f_I(e_{it}) = f(e_{it}) = \sqrt{e_{it}/22}$  so that the probability to succeed given a level of effort was the same for the partner left behind in the public project and for the partner who was in an individual project. Treatment 3 was such that  $f_I(e_{it}) = f(e_{it} + 2) = \sqrt{(e_{it} + 2)/22}$  so that being alone in the public project was less efficient than being in an individual project. Finally treatment 4 was such that  $f_I(e_{it}) = f(e_{it} + 4) = \sqrt{(e_{it} + 4)/22}$ . We choose 4 as the highest subsidy to individual production because beyond that point, having two individual projects is socially more efficient than what can be attained even under the first best with a public project.<sup>1</sup>

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<sup>1</sup>Suppose both agents participate in the public project. Total expected welfare generated by the joint project is  $f(e_{it} + e_{jt})(v_i + v_j) - e_{it} - e_{jt}$ . Maximum social welfare when both agents are involved in a common project is then  $f(e^*)(v_i + v_j) - e^*$ , where  $e^* = e_{it} + e_{jt}$  is determined by the first order condition of the maximization problem,  $f'(e^*)(v_i + v_j) = 1$ . Suppose now both agents develop individually. In that case, total expected welfare generated by the two individual projects is  $f(x_i + e_{it})v_i + f(x_j + e_{jt})v_j - e_{it} - e_{jt}$ . Agent  $i$  chooses effort  $e_i^*$  such that  $f'(x_i + e_i^*)v_i = 1$  while  $j$  chooses effort  $e_j^*$  such that  $f'(x_j + e_j^*)v_j = 1$ . Social welfare when both agents are involved in individual projects is then  $f(x_i + e_i^*)v_i - e_i^* + f(x_j + e_j^*)v_j - e_j^*$ . Assuming  $x_i = x_j$  and given the form of the function  $f(z) = \sqrt{z/22}$  that was used in the experiment, we find that maximum expected welfare in joint work is higher than maximum expected welfare in individual work for any  $x_i < 4.54$  when  $v_i = v_j = 20$  (symmetric case).

### 1.3.2 Individual payoffs

For each treatment we ran two sub-treatments, whereby in one case all subjects received payoff 20 upon successful completion of whichever project they were involved in at a specific time (symmetric payoffs), and in the second case one partner obtained payoff 16 and the other payoff 24 (asymmetric payoffs). We ran treatments both with symmetric and with asymmetric payoffs because Nash predictions under payoff asymmetry are dramatically different from those under payoff symmetry (see Section 2). Table 1 summarizes our treatment design:

Table 1: Treatment design: Variation in individual payoffs and subsidy to individual projects.

Treatment Sub-treatment	0	1	2	3	4
Symmetric payoffs $v_i = v_j = 20$	No exit	Payoff 0 when exit	$x_i = 0$	$x_i = 2$	$x_i = 4$
Asymmetric payoffs $v_i = 24, v_j = 16$	No exit	Payoff 0 when exit	$x_i = 0$	$x_i = 2$	$x_i = 4$

**Terms and notations** Before analyzing the Nash equilibrium of the one-stage version of the partnership game, let us introduce some additional terms and notations. We will use the term “*common project*” when both partners participate in the public project, and will use the term “*public project alone*” when a subject is the only one to participate in the public project in a given period. The term “*exit*” will refer to choosing an individual project after a period with a common project within the same partnership. A subject will be said to “*follow exit*” when choosing an individual project after a period when she was alone in the public project. In terms of notations,  $ec_{it}$  denotes agent’s  $i$ ’s effort in the common project at time  $t$  and  $ec_{jt}^e$  denotes agent’s  $i$ ’s expectation about the level of effort devoted by her partner  $j$  in the common project.  $epa_{it}$  is agent’s  $i$ ’s effort in the public project when alone and  $ei_{it}$  is agent’s  $i$ ’s effort in the individual project. Table 6 of Appendix A recapitulates the names of our variables and their meaning.

## 2 Nash predictions

We determine in this part the Nash equilibrium for the one-shot version of the partnership game with outside options.

*Effort:* Agent  $i$  is in partnership with agent  $j$  and such that  $v_i \geq v_j$ . Suppose there is a common project in period  $t$ .  $i$  will maximize her own expected payoff  $f(ec_{it} + ec_{jt}^e)v_i - ec_{it}$ .  $f(\cdot)$  is strictly concave so the first order condition for the maximization of this function is  $f'(ec_{it} + ec_{jt}^e)v_i = 1$  and there is only one value, which we denote  $z_i$ , such that that  $f'(z_i)v_i = 1$ . Since  $f'(\cdot)$  is decreasing in its argument,  $z_i$  will be an increasing function of  $v_i$ . We obtain  $z_i = v_i^2/88$ . There is thus a best response function  $ec_{it}(ec_{jt}^e) = \max[z_i - ec_{jt}^e, 0]$  which determines the optimal choice  $ec_{it}$  by  $i$  for every expected level of effort of  $j$ . Given that  $f'(\cdot)$  is decreasing in its argument,  $z_i$  will be higher than  $z_j$  whenever  $v_i > v_j$ . The Nash equilibrium  $(ec_{it}^*, ec_{jt}^*)$  is such that each player's effort is a best-response to the other player's effort, that is,  $ec_{it}^* = \max[z_i - ec_{jt}^*, 0]$  and  $ec_{jt}^* = \max[z_j - ec_{it}^*, 0]$ . If  $v_i > v_j$  then  $z_i > z_j$  and the Nash equilibrium is such that  $ec_{it} = z_i$  and  $ec_{jt} = 0$ . If  $v_i = v_j$  then any combination of efforts such that  $ec_{it}^* = z_i - ec_{jt}^*$  with  $ec_{it}^* \geq 0$  is a Nash equilibrium.

Similarly, the optimal level of effort in the individual project is  $ei_{it}^* = z_i - x_i$  for treatments 2, 3 and 4. In treatment 1, the optimal level of effort is  $ei_{it}^* = 0$ . Finally, optimal level of effort in the public project alone is  $epa_{it}^* = z_i$ .

*Participation decision:* The decision  $d_{it}$  whether to participate in the public project depends on how expected payoff when staying compares with payoff in an individual project. In treatment 1, it is never optimal to exit, so  $d_{it}^* = 1$ . In treatments 2, 3 and 4, we obtain  $d_{it} = 1$  (participate in the public project) if:

$$(1 - d_{jt}^e) \times (f(epa_{it})v_i - epa_{it}) + d_{jt}^e \times (f(ec_{it} + ec_{jt}^e)v_i - ec_{it}) > f(ei_{it} + x_i)v_i - ei_{it} \quad (1)$$

This can be rewritten given equilibrium levels of effort for individual  $i$  with  $v_i \geq v_j$ :

$$(1 - d_{jt}^e) \times (f(z_i)v_i - z_i) + d_{jt}^e \times (f(z_i)v_i - z_i + ec_{jt}^e) > f(z_i)v_i - z_i + x_i \quad (2)$$

Simplifying, subject  $i$  with  $v_i \geq v_j$  stays if *exit costs*  $d_{jt}^e \times ec_{jt}^e - x_i$  are more than zero:

$$\underbrace{d_{jt}^e \times ec_{jt}^e - x_i}_{\text{exit costs}} > 0 \quad (3)$$

In words,  $i$  chooses the public project as long as the expected effort of her partner times the probability he chooses the public project is more than the subsidy to individual projects.

*Beliefs:* In the symmetric case ( $v_i = v_j$ ), any belief  $ec_{jt}^e \in [0, z_i]$  can be sustained in a Nash equilibrium. Depending on this belief and  $d_{jt}^e$ , one will decide to either choose the individual or the public project.

In the asymmetric case ( $v_i > v_j$ ), then the only belief that can be sustained by  $i$  in equilibrium is that  $ec_{jt}^e = 0$  so that subject  $i$  may exit in treatment 1 and will exit in treatments 3 and 4. Subject  $j$  will have belief  $ec_{it}^e = z_i$  and will choose the individual project subject to  $d_{it}^e(z_j + f(z_i)v_j - f(z_j)v_j) < x_j$ . However, the only reasonable belief  $d_{it}^e$  in treatments 3 and 4 is such that  $d_{it}^e = 0$ . Therefore subject  $j$  will also choose the individual project in any treatment such that  $x_i > 0$ .

We summarize the Nash predictions for effort and project selection in Table 2:

Table 2: Project selection under the Nash equilibrium, by treatment.

Sub-treatment \ Treatment	0	1	2	3	4
Symmetric payoffs $v_i = v_j = 20$	.	$d_{it} = 1$ $d_{jt} = 1$	$d_{it} = 1$ if $d_{jt}^e \times ec_{jt}^e > x_i$ $d_{jt} = 1$ if $d_{it}^e \times ec_{it}^e > x_j$		
Asymmetric payoffs $v_i = 24, v_j = 16$	.	$d_{it} = 1$ $d_{jt} = 1$	$d_{it} \in [0, 1]$ $d_{jt} = 1$ if $d_{it}^e > 0$	$d_{it} = 0$ $d_{jt} = 0$	$d_{it} = 0$ $d_{jt} = 0$



### 3 Experimental findings

#### 3.1 Lower exit costs led to a loss in welfare

Table 3 shows average payoffs across treatments depending on which project the subjects participated in as well as overall:

Table 3: Average payoff in ECU and its components, by treatment.

Treatment	Statistics	payoff common	payoff public alone	payoff individual	average payoff
0	mean (sd)	7.9 (9.5)	.	.	7.9 (9.5)
	N	1920	0	0	1920
1	mean (sd)	8.0 (9.4)	3.7 (9.7)	-1.1 (2.6)	7.9 (9.4)
	N	2004	22	22	2048
2	mean (sd)	8.0 (9.6)	3.7 (9.3)	4.4 (10.0)	7.4 (9.7)
	N	1778	123	147	2048
3	mean (sd)	8.2 (9.3)	2.7 (9.3)	5.8 (10.0)	7.1** (9.7)
	N	1300	189	559	2048
4	mean (sd)	8.1 (9.5)	4.0 (9.9)	7.4 (9.7)	7.2* (9.7)
	N	610	246	1192	2048

*Difference w.r.t. treatment 0: <sup>+</sup>  $p < 0.10$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$   
We report Satterthwaite's  $t$  test for data with unequal variances to test equality in means. Differences are also significant under the Wilcoxon rank-sum test and the Kolmogorov-Smirnov test.*

Lower exit costs led to a loss in welfare because subjects were then more likely to choose an individual project and individual projects had lower success probabilities for about the same average level of effort than common projects (Table 7, Appendix A). Individual projects were chosen in 58% of cases in Treatment 4, compared with 27% in Treatment 3, 7% in Treatment 2 and 1% in Treatment 1. Payoffs were lower on average in individual projects and exiting the common project imposed on the remaining partner a cost as she was then often left alone in the public project. This meant that lower exit costs led to lower social welfare as shown in Table 3.

In an ideal world, subjects would have left the common project only when it was efficient to do so, *i.e.* when their partner exerted effort lower than the subsidy. In that ideal world, allowing exit would have provided an improvement in welfare as partnerships that did not function well due to free-riding would have been replaced by indi-

vidual projects where former partners chose their effort to maximize their own payoff. However this is not what we observed as surviving partnerships in treatments 2, 3 and 4 were not on average more efficient than “forced” partnerships in treatments 0 and 1.

There are several possible interpretations for this result: one is that subjects dislike team work and are therefore ready to sacrifice monetary payoff to avoid it. A possibility is also that subjects overuse exit as a punishment for failure in the belief that they will then be able to return to improved collaboration, or simply because they enjoy punishing others. It may also be that subjects hold overly pessimistic beliefs about the effort invested by others or the decision of others whether to participate.

We first investigate exit patterns (Section 3.2) to show that return to collaboration was ever more unlikely as exit costs decreased across treatments so that exit was probably not meant as a mean to discipline others, or if so, was unlikely to succeed in doing so. We then consider beliefs about effort and participation decision (Section 3.3)

### **3.2 Lower exit costs led more subjects to exit and return was ever more unlikely**

As previously mentioned, while almost all subjects chose the public project in treatment 1, this number decreased down to 42% in treatment 4. Further important statistics about project choice are summarized in Table 4 while Figure 4 in Appendix B gives a graphical representation of the situation of each of our subjects in every treatments over time.

Table 4: Exit and return frequency by treatments.

Treatment	1	2	3	4
% periods with a common project	98%	87%	63%	30%
% choosing individual project at the start	1%	6%	18%	45%
Frequency of exit	1%	3%	5%	10%
% following exit	9%	6%	29%	37%
% returning to public project	68%	42%	17%	9%
% re-establishing common project	59%	39%	13%	7%

The number of periods with common projects gradually declined from treatment 1 to 4 (from 98% to 30%). This was in part due to more subjects choosing individual projects at the outset of a partnership (from 1% to 45%), and in part also due to subjects exiting the common project: only about 1% of periods with a common project were followed by exit by one or both of the partners in treatment 1, compared to 10% in treatment 4. Finally, subjects were also more likely to follow exit in treatments 3 and 4 than in treatments 1 and 2: A subject who was alone in the public project chose an individual project the next period within the same partnership in only 9% of the cases in treatment 4, compared to 37% in treatment 4. Since subjects who were in an individual project were ever less likely to choose the public project in the next period with the same partnership (from 68% in treatment 1 to 9% in treatment 4), the frequency with which periods without common projects were followed by a return to collaboration was only 7% in treatment 4 vs. 59% in treatment 1.

Overall therefore, decreasing exit costs meant subjects were more likely to choose an individual project at the outset and more likely to exit common projects if such were established. They also became more likely to follow exit and less likely to try re-establishing co-operation (returning to the public project) once they left the public project. As a result, the likelihood that a common project would be re-established after breaking down became lower as the incentive to work alone increased.

Those findings are consistent with exit being driven by punishment motives in treatments 1 and 2, since many exit were followed by a return to collaboration in those treatments. However exit was much more likely to be permanent in treatment 3 and even more in treatment 4. This means that exit in those treatments must have been the result either of negative beliefs about one's partner (for those who exit), or of the awareness that one's partner was unlikely to come back in further periods (for those who followed exit). Indeed, following exit was the rational thing to do if one believed one's partner was not going to come back in treatments 3 and 4. Low rates of following exit in treatment 2 suggest that subjects understood that staying in

the public project was at least as good as following exit in this treatment as it facilitated the renewal of collaboration. High rates of following exit in treatments 3 and 4 suggest that subjects were aware of the low frequency of return in those treatments. We will investigate further the cost-benefit vs. punishment aspects of exit in Section 3.4 but first investigate the beliefs of those subjects who exit in Section 3.3.

### 3.3 Subjects were too pessimistic about the likelihood of survival of their partnership

With reference to formula 3 in Section 2, two possible reasons for exiting are that one believes the other does not do enough effort or one fears being left alone in a less efficient public project. We did not elicit counter-factual beliefs, *i.e.* beliefs about effort in the common project when there was no common project, so we first rely on beliefs about effort in the period before exit as a proxy for beliefs in the period when exit occurred. Table 5 shows average  $ec_{jt}^e$  before exit and  $d_{jt}^e$  at the time of exit for each treatment. We compare this with the average level of  $ec_{jt}$  before exit and with  $d_{jt}$  at the time of exit.

Table 5: Statistics upon exit, by treatment.

Treatment	1	2	3	4
Number of exits	15	57	60	61
$ec_{jt}^e$ before exit	5.0	5.3	5.6	5.9
$ec_{jt}$ before exit	4.9	5.0	4.7 <sup>+</sup>	5.4
$d_{jt}^e$ at exit	80.4%	69.5%	62.7%	54.8%
$d_{jt}$ at exit	100.0%*	89.5%***	83.3%**	83.6%***

*Difference w.r.t. beliefs:* <sup>+</sup>  $p < 0.10$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

*We report Satterthwaite's approximate t test for unpaired data with unequal variance to test equality in means. Differences are also significant under the Wilcoxon rank-sum test and the Kolgomorov-Smirnov test.*

Beliefs upon exit about the probability that the other would stay in the public project were consistently lower than reality in all treatments. This is in contrast with beliefs in other periods, whereby subjects generally had realistic assessments of the choice of project of their partner, especially so in treatments 3 and 4 (Table 7 in Appendix

A). Beliefs before exit about the effort of the partner were on average consistent with the actual effort of the partner and higher than the subsidy  $x_i$  to individual projects in each treatment. This means that exit was generally not driven by a belief that choosing an individual project would be more efficient than maintaining a common project, but rather by a belief that one's partner was going to exit in the next period. This fear of being left alone can justify leaving even when  $ec_{jt}^e > x_i$  in treatments 3 and 4. However, exit in treatments 1 and 2 cannot be justified even given pessimistic beliefs about the partner leaving.

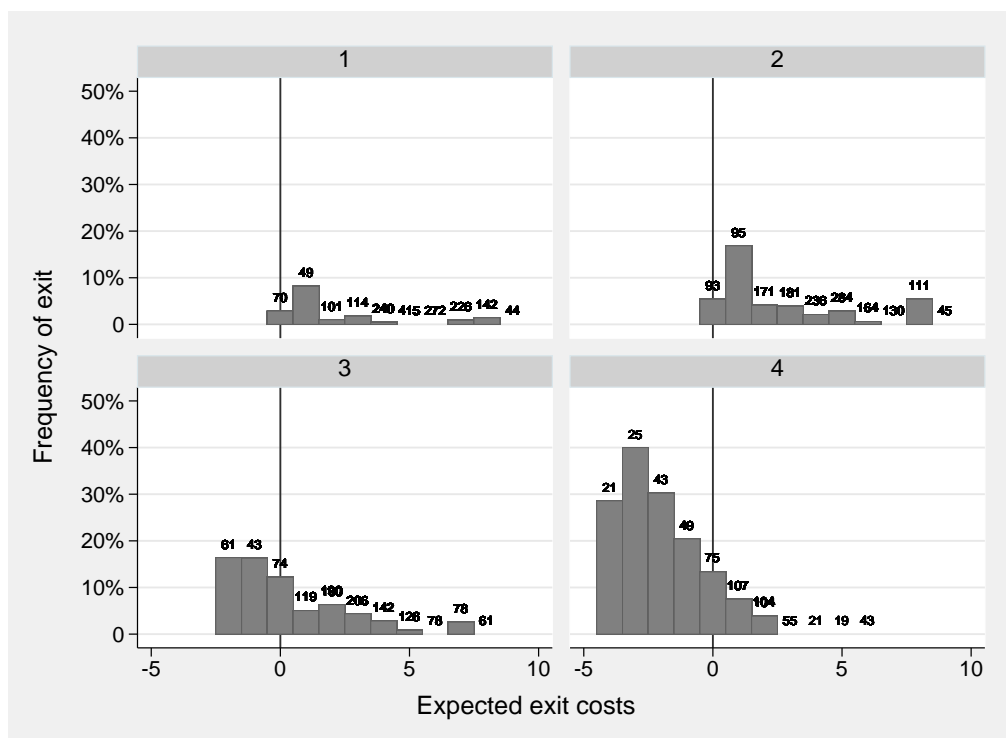
We tested this result for robustness by estimating  $ec_{jt}^e$  at the time of exit from beliefs elicited in phases where individuals both participated in the public project. Section C.1 in Appendix C gives details on how we did so. We assume beliefs are updated over time according to success or failure in the common project. There is an amount of forgetting (depreciation factor) and initial beliefs provide our starting point. We show our estimate of the parameters in the equation determining  $ec_{jt}^e$  in Table 9, Appendix D. Overall estimates show that expected effort increased after successes and decreased after failures. Since most exit followed failures, estimated  $ec_{jt}^e$  at the time of exit (which we denote  $\hat{ec}_{jt}^e$ ) was generally lower than  $ec_{jt}^e$  in the period before exit, but only by a small amount.  $\hat{ec}_{jt}^e$  was therefore still higher than subsidy  $x_i$  in treatments 2, 3 and 4. Not all subjects updated their beliefs the same way after a failure however. Figure 5 in Appendix B shows by how much individual beliefs increased or decreased following failure and success. A significant number of subjects seem to have expected  $ec_{jt}$  to decrease following a success and increase following a failure. We used estimates of individual reactions to success and failure to obtain alternative estimates of  $ec_{jt}^e$  at the time of exit and find those to be equal to  $ec_{jt}^e$  in the period before exit on average. We conclude that exit in treatments 3 and 4 does not appear to have been driven by beliefs about effort, but rather by over-pessimistic beliefs about the intention to exit of one's partner. We explore further the drivers of exit in the next section, in particular for treatments with high exit costs (treatments 1 and 2).

### 3.4 The drivers of exit

We show in this section that exit was driven by failure in the common project in treatments with high exit costs and also by cost-benefit comparisons in treatments with low exit costs.

#### 3.4.1 Exit costs as a driver of exit

Even taking account of their exaggerated fear that their partner would exit, exit is unlikely to have been driven by a cost-benefit analysis in treatments with high exit costs (treatments 1 and 2). Figure 1 shows the frequency of exit as a function of the estimated cost of exit  $d_{jt}^e \times ec_{jt-1}^e - x_i$  (grouped in unit intervals from -4 to +10 ECU) in each treatment. For treatment 1, we assess the cost of exit as  $d_{jt}^e \times ec_{jt-1}^e$ . We see that exit in treatments 1 and 2 occurs even for high expected exit costs, while this is much less likely in treatments 3 and 4 where one sees a more obvious relationship between exit costs and exit frequency. Furthermore, the frequency of exit is more sensitive to variations in exit costs in treatment 4 than in treatment 3, a fact we will come back to and confirm when running regressions for project choice.



$x$  on the horizontal axis denotes the exit cost interval from  $x$  to  $x+1$ . On top of each bar is the number of periods in which subjects with an exit cost in the corresponding interval were in a common project and the height of each bar corresponds to the frequency, shown on the vertical value axis, with which such subjects left the common project in the subsequent period.

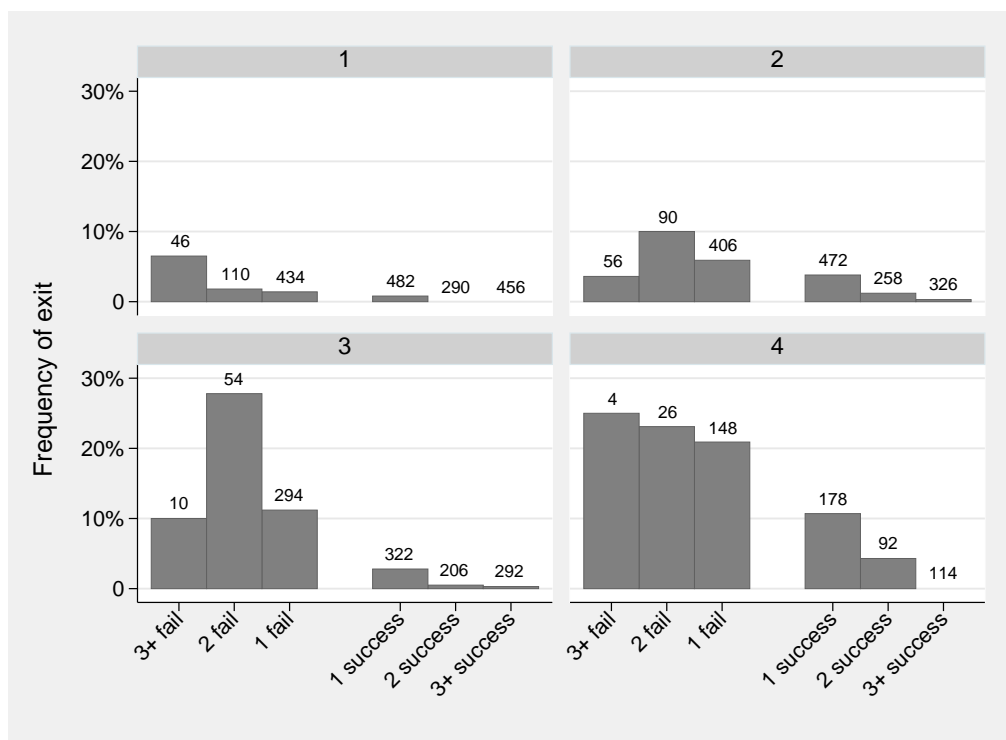
Figure 1: Exit frequency as a function of expected exit costs, by treatments.

Those results are robust even when using formula 1 rather than 3 to estimate exit costs. We do this by limiting ourselves to the sample of subjects for whom individual estimates of  $epa$  and  $ei$  are available because they stayed in the public project alone or led individual projects in at least one period each. We also obtain a very similar figure when using estimates of  $ec_{jt}^e$  as explained page 19 (estimate from NLS regressions with selection correction, column 6 of Table 9, Appendix D and estimates from individual average response to failure and success in the common project).

### 3.4.2 Failure as a driver of exit

While exit was apparently not driven by cost-benefit considerations in treatments 1 and 2, it may have been meant as a punishment for

failure in the common project. We show in Figure 2 the frequency of exit from a common project after only one, only two and more than two consecutive failures or successes within a common project.



Shown on top of each bars is the number of corresponding event patterns that occurred within each treatments in a common project, while the height of each bar corresponds to the frequency, shown on the vertical value axis, with which subjects exited following such an event pattern.

Figure 2: Exit probability following strings of successes and failures, by treatments.

As can be seen, failure increased the frequency of exit from a partnership, while successes decreased it. Repeated failure does not appear to consistently increase exit frequency compared to just one failure however, although repeated success does reduce exit frequency in a consistent way. Since failure generally led to a decrease in expectations about effort of the partner, this raises the question of whether it is punishment or cost-benefit considerations that led to exit in treatments 3 and 4. It is not possible to know from this non-parametric analysis to what extent exit was *indirectly* motivated by failure in so far as it decreased how much effort one expects one's partner to exert / increased fear of the partner exiting *vs.* how far exit was *directly*



motivated by wanting to punish failure.

### 3.4.3 Comparing the strength of different drivers of exit

We therefore need to assess econometrically how far exit was due to myopic payoff considerations and how far it was motivated by punishment. We also include a variable we call “*difference*” which corresponds to the difference between the effort one exerts and the effort one believes one’s partner is exerting. Both those variables were elicited in every period and their difference indicates how different one believes one’s partner is from oneself and the extent to which one believes one’s partner is exploiting oneself or is being exploited (when both are in the common project). This variable allows us to consider the role of “exploitation aversion” (Fehr et al., 2005) in driving exit from the common project.

We ran *reduced estimates* of the decision to participate in a public project as explained in Section C.2 of Appendix C. Results are shown in table 11 of Appendix D. They show that success last period and higher values of the expected gain from staying in the public project both made staying more likely. We concentrate on a breakdown of the results for the population averaged regressions as shown in Table 12 in Appendix D. Indeed, we are not so much interested in the within-subject determinants of exit as in its across subjects determinants so that it would not be appropriate to focus on the effects of deviations across individual means. Results show that cost of exit  $d_{jt}^e \times e\hat{c}_{jt}^e - x_i$  and failure last period are both good predictors of exit for treatments 3 and 4 while exit in treatment 2 is explained by failure last period only. The role of exit costs increases from treatments 2 to 3 and 3 to 4 while past success or failure keeps on playing a role across all treatment. This means that subjects are always more willing to stay in case of success compared to in case of failure even after taking into account the effect of success or failure on their beliefs about their partner. Behavior in treatment 1 is not predicted by our model. It may be that some subjects in that treatment did not understand they would obtain payoff 0 when alone, or they wanted to test their un-

derstanding of the consequences of leaving the partnership, or finally they followed exit by their partner in the belief he knew something about exit that they did not. Finally, subjects in treatment 3 and 4 appear to be sensitive to the belief that their partner exerts less effort than themselves in the common project. This lends credence to the hypothesis mentioned in our literature review whereby allowing exit may frame collaboration as a self-interest competition so that subjects are more likely to leave if they feel they are subjected to distributive injustice.

We ran further *structural estimates* along Taylor (1996) based on formula 1 as explained in Section C.4 of Appendix C. Results are shown in Appendix E and confirm those obtained from the reduced model.

Our results seem to indicate that as exit costs decrease, then a disciplining motivations for exit is complemented with cost-benefit motivations for exit. Indeed, we do observe an increase in the influence of exit costs when exit costs are lower. We do not however observe a reduction in the role of failure ( $sc_{it-1}$ ) across treatments (Table 12 in Appendix D). This finding is robust even when considering experienced subjects only by excluding the first partnership in the experiment (results not shown). Overall therefore, lower exit costs led subjects to be more sensitive to payoff comparisons, but they did not prevent them from punishing failure and rewarding success as would be consistent with wanting to promote cooperation over the long term.

## 4 Discussion

Subjects seem to have left partnerships in excess of what would be justified from the consideration of costs and benefits alone. This indicates that they exited for other reasons, either as a motivational tool or as an emotional reaction to failure. However exit was not effective as a motivational tool. Subjects did not exert higher effort in the common project upon return of their partner to the common project. This is shown by the fact that variable  $1 - d_{jt}$  was not a significant predic-

tor of effort when running regressions for effort in common projects as explained in Section C.3.1 and shown in Table 13 of Appendix D. Exiting and then returning did not therefore seem to promote cooperation. Further reducing the effectiveness of exit as a motivational tool, only few exit were followed by a return (cf. Table 4). This is because exit by one's partner reduced one's belief that the partner would return. Lower  $d_{jt}^e$  thus reduced expected payoff from staying in public project, making it in turn more likely that subjects would follow exit by their partner. In addition to how exit by one partner drove the other partner to exit, the payoff from staying was consistently underestimated in all treatments. This is because subjects who exited had over-pessimistic beliefs about the likelihood of exit of their partner (Table 5). Their fear of being left alone is a social fear that is akin to betrayal aversion (Bohnet et al., 2008). It led to exit even when one believed staying in would be more efficient. Another fear, that of being exploited (Fehr et al., 2005) was less important overall in driving exit.

Beyond establishing the ineffectiveness of exit as a motivation tool and the negative effects of easing exit on welfare, we also clarified in this paper the relationship between the drivers of exit and exit costs. Exit is both a way to *voice* dissatisfaction (if followed by return) and a way to *escape* a dysfunctional team. Both motivations came into play in our experiment and their relative influence evolved in a way that is reminiscent of Hirschman (1970) who observed that the greater the availability of exit, the less likely voice would be used. To borrow the terms in Yamagishi (1988, p. 531), the first driver of exit is *collectivistic* and the second is *individualistic*. Collectivistic motivation for exit is driven by wanting to make the partnership work better by influencing the behavior of one's partner. Individualistic motivation for exit is driven by payoff considerations, taking the behavior of the other as given. We could also call one set of motives "*optimistic*" about the ability of one's partner to respond to pressure towards cooperation and the other "*pessimistic*". Some people have a high sense of self-efficacy in common projects and thus undertake to influence others by their actions (Kerr and Kaufman-Gilliland, 1997), including

by exiting. Other people do not believe in the possibility for others to change behavior and thus take present expected behavior as the basis for their decision to exit.

As suggested by Yamagishi (1988), we found that whether one motivation or the other was prevalent depended on how easy or difficult exit was. Making exit more attractive by increasing the return on individual work did not only make exit more frequent, but also changed the motivations for exit. This is because if exit is easy, then not only is the investment in improving the functioning of the relationship likely to be lost as the partnership may irrevocably dissolve, but the meaning of exit also becomes ambivalent, as it is not clear if it is meant as a punishment or if it means one is not ready to work together anymore. Easy exit thus lowers one's ability to influence the action of others in public projects and therefore makes partners focus on comparisons of profits between individual and public projects, taking the behavior of the other as given. Harder exit makes people more confident that the partnership will not break down and leads them to attempt to influence the behavior of their partner over several periods in the direction of their own interests, possibly using (temporary) exit as an instrument.

## 5 Conclusion

We examined in detail in this paper the causes and consequences of individuals stopping collaboration with their team partners. We set up an original experiment that combined aspects of the repeated public good game with aspects of the repeated prisoner's dilemma. Outcomes of team work were stochastic which made it difficult for subjects to evaluate the benefits of continuing team work compared to working on their own.

We varied the cost of exit across treatments and observed that exit was driven both by the difference in expected payoff between the two options and by whether team work was successful last period. Lower exit costs led subjects to exit more often and to be more sensitive

to payoff comparisons across individual and team work and between themselves and their partner. The behavior of our subjects kept on being consistent with them wishing to reward success by staying in the team and punishing failure by leaving. However, such behavior was inefficient especially in treatments with low exit costs as subjects who exited were unlikely to reestablish team work later on. Overreacting to a negative outcome of collaboration by exiting was therefore unlikely to have an effect on future collaboration. Furthermore, subjects underestimated the pay-off from staying, in large part because they had an exaggerated fear of being left alone in the collaborative project. This led to lower overall welfare when exit was easy. Indeed, most exit in treatments with low exit costs occurred when staying in would have been more efficient.

Further work would consist in controlling for the exaggerated fear that one's partner would leave the collaborative project. This could be done in two ways, either by preventing one of the partners from leaving the collaborative project, or by having both partners leave the collaborative project even if only one decides to do so. This later design would avoid the issue whereby subjects wish to preempt exit in order to avoid being left alone. In both alternative designs, a subject would rationally exit only if he believes his partner exerts low effort. Both alternative designs would however break down the symmetry in terms of the action space of both subjects in every period of the game. Each treatment would differ in two respects, the payoff function when leaving and the payoff function when being left. Our design has the advantage of maintaining comparability across treatments in terms of the payoff when staying in the public project.

The organization of team work in open-source projects would be an ideal source of empirical data to further examine individual motivation to work in teams. Two types of licenses are prevalent in the open-source community, the GPL and the BSD. The BSD gives more freedom for developers to exploit source code for their own purposes while the GPL does not allow a developer to withhold contributions from others. A conclusion from our study is that the GPL, which imposes high exit costs, promotes efficiency by fostering trust between

developers who need not fear that their partners might leave and set up alternative projects on their own. However, open source development differs from what is modeled in our experiment in that there is accumulation of value (code base) over time in software development. Furthermore, developers usually have the choice between different teams rather than between being in a team or alone.

Finally, we did not find significant individual determinants of the decision to work in teams or individually. It would therefore be interesting to investigate further the psychological determinants of favoring one or the other type of work, such as one of the big five personality traits (Wilfling, 2013, Chapter 5) or levels of identification with the group (Täuber, 2009, Study 3). It is however likely that our design does not provide a rich enough team work environment for psychological measures to predict behavior of our subjects. Introducing communication, the need to reach agreement or the possibility to give feedback would go some way towards giving individual characteristics more chances to influence the progression of team work.

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## A Variables

Table 6: Description of variables

NAME	DESCRIPTION
$d_{it}$	participation <b>d</b> ecision by individual $i$ at time $t$ , = 0 if in private project, 1 else.
$d_{jt}$	participation <b>d</b> ecision by other individual in group of individual $i$ at time $t$ , = 0 if in private project, 1 else.
$d_{jt}^e$	individual $i$ 's <b>e</b> xpectation about the participation <b>d</b> ecision of the other individual in his group, in terms of probability of his being in the public project.
$ec_{it}$	<b>e</b> ffort in <b>c</b> ommon project by individual $i$ at time $t$ .
$ec_{jt}$	<b>e</b> ffort in <b>c</b> ommon project by the other individual in group of individual $i$ at time $t$ .
$ec_{jt}^e$	individual $i$ 's <b>e</b> xpectation about the <b>e</b> ffort in the <b>c</b> ommon project of the other individual in his group at time $t$ . This is observed only if $d_{it} = d_{jt} = 1$ .
$ei_{it}$	<b>e</b> ffort in <b>i</b> ndividual project by individual $i$ at time $t$ .
$epa_{it}$	<b>e</b> ffort in <b>p</b> ublic project by individual $i$ at time $t$ when <b>a</b> lone (i.e. when $d_{it} = 1$ and $d_{jt} = 0$ ).
$difference_{it}$	difference between one's effort and expected effort of the partner, = $ec_{it} - ec_{jt}^e$ when in common project, = $ei_{it} - ei_{jt}^e$ when in individual project, = $epa_{it} - ei_{jt}^e$ when in public project alone.
$sc_{it}$	variable indicating <b>s</b> uccess in the <b>c</b> ommon project, = -1 if failure, = 1 if success, = 0 if there is no common project.
$si_{it}$	variable indicating <b>s</b> uccess in the <b>i</b> ndividual project, = -1 if failure, = 1 if success, = 0 if the subject is not in an individual project.
$spa_{it}$	variable indicating <b>s</b> uccess in the <b>p</b> ublic project when <b>a</b> lone, = -1 if failure, = 1 if success, = 0 if the subject is not alone in the public project..
$P_i$	Vector of <b>p</b> ersonal characteristics of $i$ .
$x_i$	= $(x_i, treatment\ k_i, v_i, female_i, age_i, economist_i, ra_i, su_i, svo_i, trust_i)$
$treatment\ k_i$	value of the subsidy to the private project in the treatment to which $i$ was assigned. $x_i$ is undefined for treatments 0 and 1.
$v_i$	dummy variables indicating the <b>t</b> reatment to which $i$ was assigned, = 1 if $i$ was assigned to treatment $k$ , = 0 else, $k = \{0, \dots, 4\}$ .
$female_i$	<b>v</b> alue received on successful completion of the project for $i$ .
$age_i$	= 1 if <b>f</b> emale, 0 else
$economist_i$	<b>a</b> ge of individual $i$
$ra_i$	Dummy variable equal to 1 for subjects studying business administration or economics, 0 else.
$su_i$	index of <b>r</b> isk <b>a</b> version, taking value from 0 (most risk loving) to 10 (most risk averse). See Section G.1.
$svo_i$	index of aversion to <b>s</b> trategic <b>u</b> ncertainty, taking value from 0 (most uncertainty loving) to 10 (most averse to strategic uncertainty). See Section G.1.
$trust_i$	index of <b>s</b> ocial <b>v</b> alue <b>o</b> rientation, see Section G.2. index of <b>t</b> rustfulness, see Section G.3.
$T_t$	Vector of <b>t</b> ime varying characteristics, = $(start_t, per_t, phase_{kt})$ .
$start_t$	Dummy variable = 1 if period $t$ is the <b>s</b> tart of a new partnership, 0 else. $start_t = 1$ for $t = \{1, 9, 23\}$ , 0 else.
$per_t$	<b>P</b> eriod within the partnership, = 1 at the start of a partnership, = 2 in the second period, and so on.
$phase\ k_t$	Dummy variable = 1 if at period $t$ the subject is in the $k^{th}$ partnership, $k = \{1, 2, 3\}$ .

Table 7: Summary statistics, by treatment

TREATMENT	STATS	$d_{it}$	$d_{jt}^e$	$ec_{it}$	$ec_{jt}^e$	$ei_{it}$	$epa_{it}$	% success common project	% success individual project	% success public alone
0	mean	1.00	1.00	5.39	5.46	.	.	67%	.	.
	sd	0.00	0.00	2.46	2.18	.	.	47%	.	.
	N	1920	1920	1920	1920	0	0	1920	0	0
1	mean	0.99	0.93	5.57	5.92	1.05	5.08	68%	0%	41%
	sd	0.10	0.15	2.47	2.29	2.57	3.25	47%	0%	50%
	N	2048	2048	2004	2004	22	22	2004	22	22
2	mean	0.93	0.83	5.32	5.62	7.42	5.57	66%	58%	48%
	sd	0.26	0.27	2.73	2.51	2.75	3.15	47%	50%	50%
	N	2048	2048	1778	1778	147	123	1778	147	123
3	mean	0.73	0.67	5.76	6.11	4.85	5.67	70%	52%	43%
	sd	0.45	0.38	2.65	2.13	3.24	3.44	46%	50%	50%
	N	2048	2048	1300	1300	559	189	1300	559	189
4	mean	0.42	0.41	5.61	6.36	5.01	6.55	69%	62%	52%
	sd	0.49	0.40	2.72	2.03	3.43	3.09	46%	49%	50%
	N	2048	2048	610	610	1192	246	610	1192	246

Table 8: Summary individual statistics, by treatment

TREATMENT	STATS	FEMALE	AGE	ECONOMIST	RA	SU	SVO	TRUST
0	mean	0.60	24.50	0.12	5.70	4.80	0.41	0.03
	sd	0.49	3.84	0.32	1.37	2.19	0.22	1.23
	N	60	60	60	60	60	60	60
1	mean	0.55	25.06	0.13	5.69	5.17	0.37	0.01
	sd	0.50	5.79	0.33	1.94	2.62	0.28	1.44
	N	64	64	64	64	64	64	64
2	mean	0.56	24.16	0.17	5.80	4.41	0.37	0.05
	sd	0.50	6.27	0.38	1.63	2.59	0.25	1.28
	N	64	64	64	64	64	64	64
3	mean	0.48	24.35	0.20	5.70	4.44	0.36	-0.08
	sd	0.50	4.16	0.41	1.81	2.39	0.22	1.44
	N	64	63	64	64	64	64	64
4	mean	0.55	24.08	0.17	5.48	4.91	0.36	0.00
	sd	0.50	3.40	0.38	1.60	2.74	0.25	1.41
	N	64	63	64	64	64	64	64

## B Figures

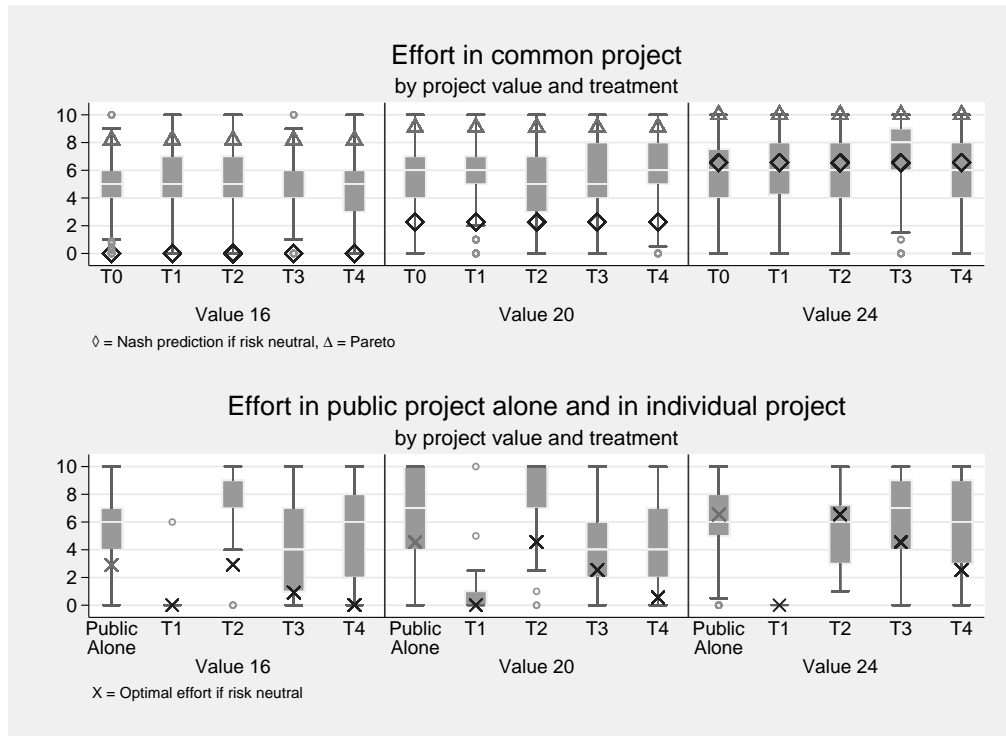


Figure 3: Effort choice, by type of project and project value.

Figure 3 represents the distribution of individual effort for each subject in our experiment, by treatment and value for the project. Superimposed on the figure for common projects is the effort predicted under a Nash equilibrium (Diamonds), and the welfare maximizing effort (Triangles), assuming when otherwise not prescribed that effort is allocated as a proportion of the value derived from the project.

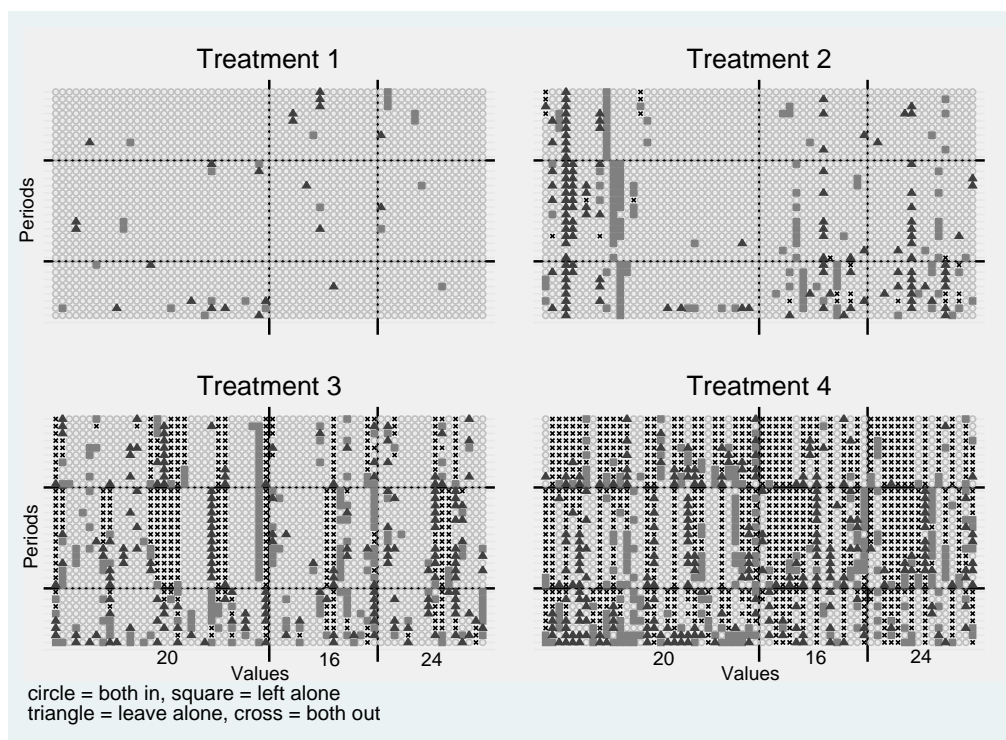


Figure 4: Project choice by individuals over time, by treatment

Figure 4 represents project choice by individuals over time. Each sub-figure corresponds to one treatment. Each point on the horizontal axis is an individual, and each point on the vertical axis is a period, starting with period 1 on the bottom up to period 32 at the top. The shape of the point represents the state of the partnership they are involved in: circle=they are in a common project, square=they are in the public project alone, triangle=they are the only one who chose an individual project, cross=both are in individual projects. Subjects in the symmetric sub-treatment are in the first half to the right of each sub-figures. The second half shows subjects in the asymmetric sub-treatment, first subjects who obtained value 16 and then those who obtained value 24. Horizontal lines in the background separate different phases in the experiment, corresponding to re-matching of subjects in pairs.

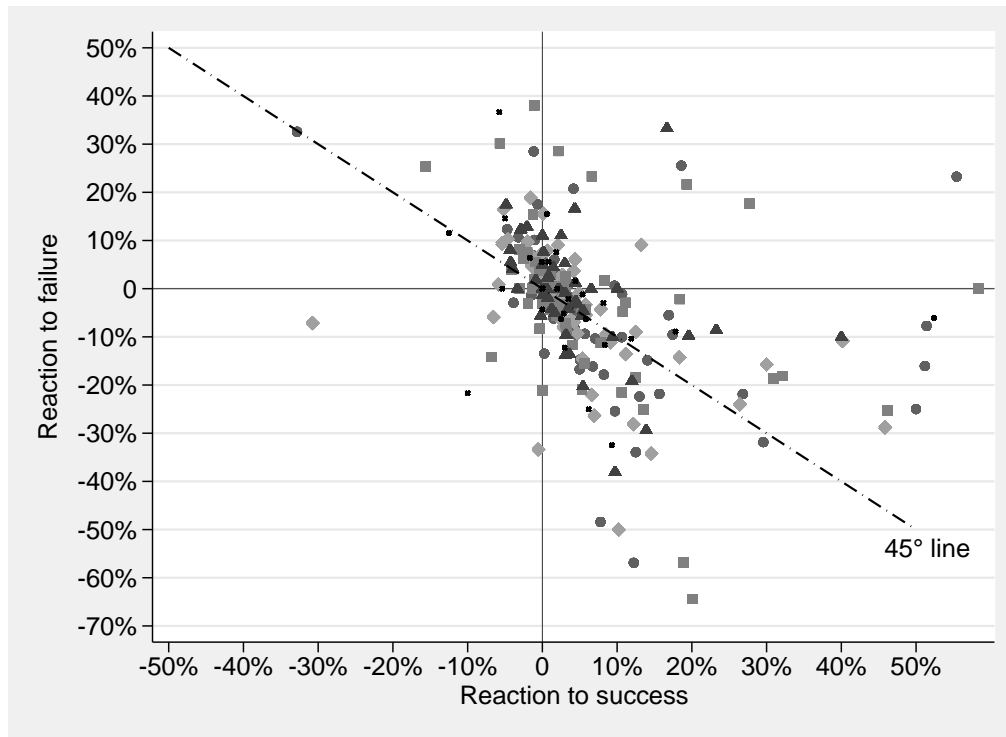


Figure 5: Evolution of beliefs about partner's effort as a function of failure or success last period.

Figure 5 shows individual average percentage increases and decreases in  $ec_{jt}^e$  as a function of whether the common project was a success or a failure last period. The shape of the point represents the treatment the individual was in: circle=Treatment 0, lozenge=Treatment 1, square=Treatment 2, triangle=Treatment 3, cross=Treatment 4.



## C Steps in the estimation procedure

### C.1 Step 1: Estimating expected effort

An issue given our belief elicitation mechanism is that we do not know  $ec_{jt}^e$  if at time  $t$  there was no common project ( $d_{it} = 0$  and/or  $d_{jt} = 0$ ). We therefore need to reconstruct  $ec_{jt}^e$  for those missing periods. Beliefs about the effort of the other in the common project will be assumed to follow a dynamic process for  $t > 1$ :

$$ec_{jt}^e = \alpha + \rho \times ec_{jt-1}^e + \beta \times sc_{it-1} + p \times P_i + c \times start_t + u_i + \epsilon_{it} \quad (4)$$

with  $\rho < 1$ . Initial expectations  $ec_{j0}^e$  will be taken to be equal to the first elicited beliefs about effort of the other. Since subjects most often started out in a common project, this is usually beliefs in period 1. Beliefs evolve depending on whether the common project was successful last period ( $sc_{it-1} = 1$ ), unsuccessful ( $sc_{it-1} = -1$ ) or did not take place ( $sc_{it} = 0$ ). We expect that  $\beta$  will be positive (a success is a good signal about the effort of the other, assuming of course that success will not lead the other to decrease his effort next period). Expectations are carried over from phase to phase with possibly adjustments upwards or downwards with the start of a new phase ( $start_t$ ), which is reflected in parameters  $c$  in equation 4. Expectations will also depend on personal characteristics, including the value of the project to the other ( $v_i$  is a proxy for  $v_j$  as  $v_j = 40 - v_i$ ). Subjects ought to think that those with lower value for the project will contribute less, so the parameter on  $v_i$  should be negative.

Included in our regression are all observations for which  $ec_{jt}^e$  and  $ec_{jt-1}^e$  were observed, so our selection variable is  $selection_{it} = 1$  if  $d_{it} = d_{it-1} = 1$  and  $d_{jt} = d_{jt-1} = 1$ , 0 else. We assume selection depends on the following variables:

$$selection_{it} = 1[\alpha + \gamma \times d_{jt}^e + \delta \times sc_{it-1} + p \times P_i + c \times (phase_{kt}, per_t) + u_i + \epsilon_{it} > 0] \quad (5)$$

with  $\epsilon_{it}$  distributed normally.

We start with estimates of equation 4 by restricting  $\rho$  to being equal to 1, which gets rid of issues with this auto-regressive term. The test for selection effect suggested in procedure 3.2 in Wooldridge (1995) is not significant (column 1 of Table 9) but corrected estimates as per procedure 4.2 in Wooldridge (1995) show that the null of no selection bias is rejected at the 5% level (column 2 of Table 9). Unlike Wooldridge (1995) we include the mean  $\bar{x}_i$  of time-varying variables  $x_{it}$  in the regressions along  $x_{it}$  rather than the full vector  $(x_{i1}, x_{i2}, \dots, x_{i32})$ . This is because there are too many periods and too few individuals to be able to estimate that many parameters reliably. In a second step, we let  $\rho$  be a free parameter and apply a first difference instrumental variables estimator along procedure 3 in Semykina and Wooldridge (2013) (columns 3 and 4 of Table 9). This consists in estimating equation 4 in differences while correcting for sample selection in the same way as in Wooldridge (1995) and using  $ec_{jt-2}^e$  as an instrument for  $\Delta ec_{jt-1}^e$  as suggested in Anderson and Hsiao (1981). In that case, the selection variable is  $selection_{it} = 1$  if  $d_{it} = d_{it-1} = d_{it-2} = 1$  and  $d_{jt} = d_{jt-1} = d_{jt-2} = 1$ , 0 else, as we need to observe  $ec_{jt-2}^e$ , our instrument. The selection equation is as in equation 5. We find here that the  $IMR_t$  are not jointly significant (Wald test).

Finally, we apply the NLS estimator as per procedure 1 in Semykina and Wooldridge (2013), whereby we estimate the following equation for  $t > 1$ :

$$\begin{aligned}
 ec_{jt}^e = & \rho^{t-1} \times ec_{j0}^e + \beta \times \sum_{j=1}^{t-1} \rho^{j-1} \times sp_{it-j} + \frac{1 - \rho^{t-1}}{1 - \rho} \times (\alpha + p \times P_i + \gamma \times ec_{j0}^e) + \dots \\
 & \dots + c \times \sum_{j=1}^{t-1} \rho^{j-1} \times start_{t-j} + \varphi_t \times IMR_{it} + \epsilon_{it2}
 \end{aligned} \tag{6}$$

The selection variable is now  $selection_{it} = 1$  if  $d_{it} = 1$  and  $d_{jt} = 1$ , 0 else.  $IMR_{it}$  is the inverse Mills ratio obtained from estimating this equation 5 in period  $t$ . Results are shown in columns 5 and 6 of Table 9. We find that the IMRs are jointly significant.

All standard errors are obtained through “panel bootstrap”, which “involves re-sampling cross-sectional units (and all time periods for each unit sampled) and using the bootstrap sample to approximate the distribution of the parameter vector” (Semykina and Wooldridge, 2010). Bootstrapping is “an alternative method for estimating the standard errors when the theoretical calculation is complicated” (Guan, 2003). Panel bootstrap obtains a consistent variance–covariance matrix of the estimators in the presence of heteroskedasticity and serial correlation for  $N \rightarrow \infty$  and  $T$  fixed.

## C.2 Step 2: Reduced estimates for the decision to take part in the public project

Based on the results of the estimation of  $ec_{jt}^e$  from equation 6, we obtain reconstructed beliefs  $\hat{ec}_{jt}^e$ . We then use those reconstructed beliefs to estimate the following reduced form selection equation for  $treatment_i \geq 1$ :

$$\begin{aligned} d_{it} = & 1[\alpha + \theta \times d_{jt}^e + \eta \times \hat{ec}_{jt}^e + \lambda \times (d_{jt}^e \times \hat{ec}_{jt}^e - x_i) + \pi \times sc_{it-1} + \dots \quad (7) \\ & \dots + \Delta \times difference_{it-1} + \delta \times d_{it-1} \times d_{jt-1} \times difference_{it-1} + \dots \\ & \dots + p \times P_i + c \times (phase_{kt}, per_t) + u_i + \epsilon_{it} > 0] \end{aligned}$$

Since  $x_i$  is not defined for treatment 1, we set it equal to 0 for the purpose of this regression. Parameters in the decision equation can be estimated with a conditional fixed-effects logit model. According to equation 3, the decision to exit in treatments 2, 3 and 4 depends on whether the cross term  $d_{jt}^e \times \hat{ec}_{jt}^e$  exceeds the subsidy to individual projects  $x_i$ . The probability to participate in the public project may also depend directly on how much effort individual  $i$  believes the other participant devotes to the common project ( $\hat{ec}_{jt}^e$ ) and on the expected participation decision of the other individual  $d_{jt}^e$ . We also consider the direct impact of success or failure last period in the public project,  $sc_{it-1}$ , and of a variable we call “*difference*” which is the difference between one’s effort and the effort one expects one’s partner

is exerting. Expectations about effort were elicited in every period whether one was in the same project as one's partner or not, but this variable ought to play a role mainly when both subjects participate in the common project which is why we differentiate its overall effect and its effect when there was a common project in the previous period.

Results are shown in Table 11, first with a pooled OLS regression, as before with bootstrapped standard errors for the parameter estimates (column 1). There is significant auto-correlation in the idiosyncratic error of our model, so we specify AR(1) within-panel error correlation structure in a population-averaged model (column 2). The column 3 shows results for a fixed-effect model, and column 4 for a random-effects model. A Hausman test rejects the hypothesis that the individual-level effects are adequately modeled by a random-effects model.

### C.3 Step 3: Correcting estimates of effort for selection effects

#### C.3.1 Step 3a: Effort in the common project

Effort in the common project will be assumed to evolve according to the following equation:

$$ec_{it} = \alpha + \sigma \times \hat{ec}_{jt}^e + \phi \times sc_{it-1} + \tau \times (1 - d_{jt-1}) + p \times P_i + c \times (phase_{kt}, per_t) + u_i + \epsilon_{it} \quad (8)$$

so that effort depends on the expected effort of the other. We use  $\hat{ec}_{jt}^e$  in the regressions instead of  $ec_{jt}^e$  so as to be able to obtain estimates of  $ec_{it}$  also for periods out of a common project. Experiments on the public good game show that  $\sigma$  in this setting is positive – conditional cooperation –, but temptation to free-ride might be greater in an environment where the outcome is stochastic so that the partner's effort is not observable. We consider also the direct effect of success in the previous period  $sc_{it-1}$ . Indeed, while a rational agent would vary his effort only as a function of his beliefs about the effort of their

partner, success (or failure) may also have a psychological effect, such that for example a subject may want to “compensate” failure in the previous period by increasing effort and *vice-versa*. We also include  $1 - d_{jt-1}$  to determine if one’s partner returning to the common project has a positive effect on effort levels within the common project, as ought to be the case if punishment by leaving the common project is meant to have an incentive effect rather than being a purely punitive action. The selection variable is  $selection_{it} = 1$  if  $d_{it} = 1$  and  $d_{jt} = 1$ , 0 else, as this determines whether  $ec_{it}$  is observed. We include all regressors of equation 7 in the selection equation. Results are shown in columns 1 and 2 of Table 13. The test for selection effect suggested in procedure 3.2 in Wooldridge (1995) is not significant (column 1) but corrected estimates as per procedure 4.2 in Wooldridge (1995) show that the null of no selection bias is rejected at the 1% level (column 2).

### C.3.2 Step 3b: Effort in the individual project and in the public project when alone

Effort in the individual projects will be determined through the following formula for  $treatment_i \geq 1$ :

$$ei_{it} = \alpha + \varphi \times si_{it-1} + \lambda_k \times treatment\ k_i + p \times P_i + c \times (phase_{kt}, per_t) + u_i + \epsilon_{it} \quad (9)$$

where  $\lambda_k$  is the constant term for treatment  $k = \{2, 3, 4\}$ . We included those dummies as a matter of course within  $P_i$  in other regression equations, but we wish to make a point about the value of the treatment parameter in this part. Since we expect that individuals exert optimal effort  $ei_{it} = 0$  in that treatment, then we expect  $\alpha = 0$  as well. We also expect that there will be only partial crowding out of own effort by the subsidy  $x_j$  associated to each treatment  $j > 1$  so that  $\lambda_j - \lambda_{j+1} \leq x_{j+1} - x_j$ . We include  $si_{it-1}$  as a dependent variable by the same argument as that mentioned in the  $ec_{it}$  regressions. As before, we obtain unbiased estimates of  $ei_{it}$  by including the inverse Mills ratios derived from the non-selection equivalent of equation 7

for each period. We also run separate regressions to estimate effort in the public project when alone, correcting as before for selection effect. Results are shown in columns 3 and 4 for  $ei$  and columns 5 and 6 for  $epa$  in Table 13.

#### C.4 Step 4: Structural estimates for the decision for the decision to take part in the public project

Having obtained estimates of  $ec_{it}$ ,  $epa_{it}$  and  $ei_{it}$  in the previous section we can finally estimate a structural form of the project selection equation for  $treatment_i \geq 1$ :

$$d_{it} = 1[\alpha + \beta \times (EV_{Pit} - EV_{Iit}) + \varphi \times sc_{t-1} + \dots \dots + \Delta \times difference_{it-1} + \delta \times d_{it-1} \times d_{jt-1} \times difference_{it-1} + u_i + \epsilon_{it} > 0] \quad (10)$$

with

- $EV_{Pit} = d_{jt}^e \times (f(\hat{ec}_{it} + \hat{ec}_{jt}^e) \times v_i - \hat{ec}_{it}) + (1 - d_{jt}^e) \times (f(\hat{epa}_{it}) \times v_i - \hat{epa}_{it})$  the expected value of choosing the public project, taking into account the possibility that the other individual might not participate, in which case effort in the public project is  $epa_{it}$ .
- $EV_{Iit} = f_{iI}(\hat{ei}_{it}) \times v_i - \hat{ei}_{it}$  the expected value of the individual project, where  $f_{iI}(\hat{ei}_{it})$  is the expected probability of success for individual  $i$  if he exerts effort  $ei_{it}$  in the individual project given the treatment to which he was assigned.

Results are shown in Table 14, first with a pooled OLS regression, as usual with bootstrapped standard errors for the parameter estimates (column 1). There is significant auto-correlation in the idiosyncratic error of our model, so we specify AR(1) within-panel error correlation structure in a population-averaged model (column 2). The column 3 shows results for a fixed-effect model, and column 4 for a random-effects model. A Hausman test rejects the hypothesis that the individual-level effects are adequately modeled by a random-effects model.

## D Regressions

Table 9: Table of regression results for  $ec_{jt}^e$ 

<i>dependent variable</i>	1. FE TEST selection effect $ec_{jt}^e$	2. POOLED OLS w/ selection correction $ec_{jt}^e$	3. FD-IV w/o selection correction $ec_{jt}^e$	4. FD-IV w/ selection correction $ec_{jt}^e$	5. NLS w/o selection correction $ec_{jt}^e$	6. NLS w/ selection correction $ec_{jt}^e$
$ec_{jt-1}^e$	1	1	0.2664*** (5.54)	0.2601*** (4.69)	0.8098*** (28.34)	0.8099*** (28.26)
$sc_{it-1}$	0.2315*** (6.43)	0.2276*** (6.18)	0.1501*** (4.94)	0.1744*** (5.26)	0.3534*** (8.91)	0.3712*** (8.14)
$start_t$	0.1907* (2.39)	0.2324* (2.20)	0.1873* (2.44)	0.2559** (3.21)	0.1200 (1.56)	0.0369 (0.44)
$v$		0.0011 (0.41)			0.0121 (1.51)	0.0123 (1.55)
<i>treatment 1</i>		-0.0080 (-0.44)			0.0657 (1.15)	0.0617 (1.14)
<i>treatment 2</i>		-0.0497+ (-1.89)			-0.0041 (-0.08)	-0.0234 (-0.42)
<i>treatment 3</i>		-0.0211 (-0.65)			0.0639 (0.96)	0.0245 (0.35)
<i>treatment 4</i>		0.0157 (0.24)			0.1737* (2.31)	0.1002 (1.32)
<i>female</i>		-0.0019 (-0.13)			-0.0416 (-0.95)	-0.0481 (-1.06)
<i>age</i>		-0.0016 (-0.95)			-0.0039 (-1.02)	-0.0042 (-0.98)
<i>economist</i>		0.0228 (0.85)			-0.0255 (-0.38)	-0.0260 (-0.40)
<i>ra</i>		-0.0008 (-0.16)			0.0032 (0.20)	0.0037 (0.24)
<i>su</i>		-0.0021 (-0.76)			0.0005 (0.06)	0.0002 (0.02)
<i>svo</i>		0.0092 (0.28)			0.0275 (0.35)	0.0334 (0.37)
<i>trustful</i>		-0.0042 (-0.67)			-0.0313+ (-1.74)	-0.0318+ (-1.72)
$ec_{j0}^e$					0.0723*** (3.87)	0.0719*** (4.25)
<i>constant</i>		-0.1512 (-1.51)			0.3269 (1.45)	0.3362 (1.58)
<i>IMR</i>	0.1440					
$\chi^2(IMR_t)$		46*		31		54**
<i>N</i>	6962	6962	6513	6495	7353	7353
<i>Subjects</i>	281	281	275	274	294	294
<i>df</i>	3	47	3	32	17	48
<i>Wald test</i> $\chi^2$	45***	125***	42***	78***	87152***	131850***

*z* statistics in parentheses, bootstrapped standard errors, 200 replications.

+  $p < 0.10$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Table 10: Table of regression results for  $ec_{jt}^e$ , NLS by treatments

<i>dep. variable</i>	NLS w/ SELECTION CORRECTION				
	TREATMENT 0	TREATMENT 1	TREATMENT 2	TREATMENT 3	TREATMENT 4
	$ec_{jt}^e$	$ec_{jt}^e$	$ec_{jt}^e$	$ec_{jt}^e$	$ec_{jt}^e$
$ec_{jt-1}^e$	0.8068*** (21.78)	0.8137*** (5.41)	0.7779*** (10.19)	0.7715*** (6.04)	0.6756*** (4.79)
$sc_{it-1}$	0.4818*** (6.62)	0.2472** (2.84)	0.5274*** (4.44)	0.2866** (3.07)	0.1126 (1.28)
$start_t$	0.0307 (0.26)	0.0677 (0.46)	0.1042 (0.51)	-0.0185 (-0.10)	0.1523 (0.38)
$v$	-0.0035 (-0.25)	0.0276 (0.70)	0.0156 (0.70)	0.0212 (0.63)	-0.0131 (-0.39)
<i>female</i>	-0.0439 (-0.57)	-0.2089 (-1.00)	-0.0328 (-0.25)	0.0396 (0.25)	0.1287 (0.68)
<i>age</i>	-0.0171 (-1.32)	0.0003 (0.03)	-0.0028 (-0.21)	-0.0075 (-0.39)	0.0074 (0.26)
<i>economist</i>	0.1832+ (1.76)	-0.2958 (-1.07)	-0.2084 (-1.24)	0.2204 (0.87)	0.1571 (0.49)
<i>ra</i>	-0.0262 (-0.63)	0.0284 (0.55)	0.0093 (0.20)	0.0280 (0.56)	-0.1309+ (-1.65)
<i>su</i>	-0.0083 (-0.41)	-0.0013 (-0.05)	0.0107 (0.31)	0.0256 (0.68)	-0.0211 (-0.46)
<i>svo</i>	-0.1108 (-0.57)	0.2171 (0.97)	-0.1346 (-0.55)	0.0440 (0.13)	-0.1066 (-0.25)
<i>trustful</i>	-0.0414 (-1.12)	0.0064 (0.15)	-0.0192 (-0.32)	-0.0806 (-1.34)	-0.0561 (-0.73)
$ec_{j0}^e$	0.0373 (1.14)	0.0891 (0.89)	0.1024* (2.29)	0.0827 (1.18)	0.1912+ (1.80)
<i>constant</i>	1.3921* (2.44)	-0.1873 (-0.25)	0.1064 (0.19)	0.1470 (0.17)	1.7710 (1.49)
$\chi^2(IMR_t)$	.	11.83	44.02+	43.03+	37.85
<i>N</i>	1860	1942	1722	1251	578
<i>Subjects</i>	60	64	64	59	47
<i>df</i>	13	44	44	44	44
<i>Wald test</i> $\chi^2$	32878***	34345***	22575***	21578***	17206***

z statistics in parentheses, bootstrapped standard errors, 200 replications.

+  $p < 0.10$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$



Table 11: Determinants of the decision to take part in the public project

<i>dependent variable</i>	(1) POOLED $\partial d/\partial x$	(2) PA $\partial d/\partial x$	(3) FE $d$ (see note 1)	(4) RE $\partial d/\partial x$ (see note 2)
$d_{jt}^e$	0.0798 (0.99)	-0.0092 (-0.13)	0.4599 (0.27)	0.0534 (0.74)
$e\hat{c}_{jt}^e$	-0.0035 (-0.32)	-0.0066 (-0.64)	0.1230 (0.41)	-0.0005 (-0.05)
$d_{jt}^e \times e\hat{c}_{jt}^e - x_i$	0.0369* (2.49)	0.0484*** (3.74)	0.7547* (2.30)	0.0361** (2.70)
$sc_{it-1}$	0.0437*** (7.08)	0.0292*** (4.70)	0.5226*** (5.39)	0.0316*** (6.22)
$difference_{it-1}$	-0.0015 (-0.38)	-0.0025 (-0.83)	0.0245 (0.66)	0.0009 (0.43)
$d_{it-1} \times d_{jt-1} \times difference_{it-1}$	-0.0102* (-2.13)	-0.0086* (-2.13)	-0.1777*** (-3.53)	-0.0094** (-3.13)
$per$	-0.0045*** (-4.46)	-0.0060*** (-5.03)	-0.0937*** (-4.56)	-0.0050*** (-5.09)
$phase\ 2$	0.0038 (0.31)	0.0133 (1.17)	0.2098 (0.87)	0.0098 (0.83)
$phase\ 3$	-0.0024 (-0.18)	0.0067 (0.55)	0.1577 (0.67)	0.0045 (0.37)
$v$	-0.0029 (-0.81)	-0.0034 (-1.13)	.	-0.0027 (-1.02)
$treatment\ 2$	-0.1091* (-2.50)	-0.1097* (-2.23)	.	-0.0604+ (-1.91)
$treatment\ 3$	-0.1141** (-2.60)	-0.1110* (-2.42)	.	-0.0770* (-2.03)
$treatment\ 4$	-0.0785 (-1.21)	-0.0550 (-0.89)	.	-0.0570 (-0.99)
$female$	-0.0195 (-0.90)	-0.0210 (-0.87)	.	-0.0299 (-1.49)
$age$	0.0004 (0.16)	0.0003 (0.13)	.	0.0004 (0.21)
$economist$	0.0017 (0.07)	0.0051 (0.18)	.	-0.0005 (-0.02)
$ra$	-0.0019 (-0.22)	-0.0015 (-0.16)	.	-0.0032 (-0.49)
$su$	-0.0077+ (-1.69)	-0.0085+ (-1.68)	.	-0.0069 (-1.56)
$svo$	-0.0269 (-0.60)	-0.0318 (-0.78)	.	-0.0162 (-0.43)
$trust$	-0.0024 (-0.34)	-0.0016 (-0.25)	.	-0.0020 (-0.34)
$N$	7254	7254	3999	7254
$Subjects$	234	234	129	234
$df$	20	20	9	20
$ll$	-1783.3		-984.6	-1421.7
$Mc\ Fadden's\ Pseudo\ R^2$	46.6%			30.3%
$Wald\ test\ \chi^2$	506.7***	300.4***	311.6***	456.8***
$Lagrange\ multiplier\ test$	2.6	2.4	0.8	2.3
$Pr(\hat{d} = 1 \mid d = 1)$	96.0%	96.4%	88.5%	96.3%
$Pr(\hat{d} = 0 \mid d = 0)$	64.6%	61.6%	68.1%	62.1%

$z$  statistics in parentheses, bootstrapped standard errors, 200 replications.

+  $p < 0.10$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Marginal effects are obtained by the delta method.  $\partial d/\partial x$  for factor levels is the discrete change from the base level.<sup>1</sup>In FE, we report parameter estimates for the logit model. FE regressions exclude 105 subjects who chose  $d = 1$  for every period.<sup>2</sup>In RE, marginal effects are computed assuming that individual effects  $u_i$  are equal to 0. The Lagrange multiplier test is obtained by doing a Wald test of zero coefficient for the added regressor  $(x'\hat{\beta})^2$ .

Table 12: Determinants of the decision to take part in the public project, population-averaged regressions, by treatment.

<i>Dependent variable</i>	(1) TREATMENT 1 $\partial d/\partial x$	(2) TREATMENT 2 $\partial d/\partial x$	(3) TREATMENT 3 $\partial d/\partial x$	(4) TREATMENT 4 $\partial d/\partial x$
$d_{jt}^e$	0.0471 (0.66)	0.0213 (0.20)	-0.0859 (-0.45)	-0.3193 (-1.13)
$ec_{jt}^e$	0.0076 (0.68)	-0.0150 (-0.71)	-0.0115 (-0.37)	-0.0149 (-0.36)
$d_{jt}^e \times ec_{jt}^e - x$	-0.0029 (-0.25)	0.0209 (0.92)	0.0872* (2.29)	0.1403** (2.88)
$sc_{it-1}$	0.0040 (0.94)	0.0304+ (1.90)	0.0458** (2.99)	0.0421+ (1.92)
$difference_{it-1}$	(omitted) (.)	-0.0246 (-1.47)	0.0028 (0.54)	-0.0015 (-0.25)
$d_{it-1} \times d_{jt-1} \times difference_{it-1}$	-0.0006 (-0.46)	0.0143 (1.08)	-0.0202* (-2.12)	-0.0216* (-2.06)
$per$	-0.0007 (-0.80)	-0.0008 (-0.40)	-0.0081* (-2.53)	-0.0205*** (-5.14)
$phase\ 2$	0.0083 (1.00)	0.0248 (1.55)	-0.0199 (-0.65)	0.0563 (1.22)
$phase\ 3$	0.0060 (0.78)	0.0427 (1.35)	0.0043 (0.14)	-0.0392 (-0.80)
$v$	0.0017 (0.72)	-0.0064 (-0.88)	-0.0149 (-1.37)	-0.0024 (-0.16)
$female$	0.0026 (0.16)	-0.0291 (-0.41)	-0.0555 (-0.94)	0.0074 (0.09)
$age$	0.0021 (0.72)	0.0064 (0.63)	-0.0068 (-0.54)	0.0027 (0.20)
$economist$	-0.0011 (-0.08)	-0.0210 (-0.28)	-0.0125 (-0.16)	0.0319 (0.27)
$ra$	-0.0021 (-0.44)	0.0026 (0.16)	0.0086 (0.37)	-0.0150 (-0.34)
$su$	0.0017 (0.42)	-0.0097 (-1.01)	-0.0150 (-1.29)	-0.0045 (-0.18)
$svo$	0.0084 (0.30)	-0.0502 (-0.52)	0.0476 (0.42)	-0.1342 (-0.73)
$trust$	-0.0007 (-0.21)	-0.0121 (-0.70)	0.0110 (0.63)	-0.0211 (-0.64)
$N$	1984	1984	1829	1457
$Subjects$	64	64	59	47
$df$	17	17	17	17
$Wald\ test\ \chi^2$	25.9+	51.5***	81.2***	87.3***
$Lagrange\ multiplier\ test$	1.2	0.0	0.0	0.7
$Pr(\hat{d} = 1 \mid d = 1)$	99.9%	99.3%	93.9%	81.3%
$Pr(\hat{d} = 0 \mid d = 0)$	0.0%	9.8%	63.2%	79.4%

*z* statistics in parentheses, bootstrapped standard errors, 200 replications.

Marginal effects are obtained by the delta method.  $\partial d/\partial x$  for factor levels is the discrete change from the base level.

The Lagrange multiplier test is obtained by doing a Wald test of zero coefficient for the added regressor  $(x'\hat{\beta})^2$ .

+  $p < 0.10$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Table 13: Tables of regression results for effort in common projects, individual projects and public projects alone

	1. FE TEST SELECTION	2. POOLED OLS W/ CORRECTION	3. FE TEST SELECTION	4. POOLED OLS W/ CORRECTION	5. FE TEST SELECTION	6. POOLED OLS W/ CORRECTION
<i>Dependent var.</i>	<i>ec</i>	<i>ec</i>	<i>ei</i>	<i>ei</i>	<i>epa</i>	<i>epa</i>
$\hat{e}c_{jt}$	0.0184 (0.17)	0.0159 (0.15)				
$sc_{it-1}$	-0.0463 (-1.28)	0.0267 (0.66)				
$si_{it-1}$			-0.0793 (-1.05)	-0.0507 (-0.66)		
$spa_{it-1}$					-0.1415 (-1.18)	-0.2438+ (-1.79)
$1 - d_{jt-1}$	-0.0122 (-0.09)	-0.1724 (-1.15)				
<i>per</i>	-0.0268** (-3.04)	-0.0175 (-1.38)	-0.0184 (-0.84)	-0.0183 (-0.44)	-0.0133 (-0.34)	-0.0802 (-0.73)
<i>phase 2</i>	-0.1885+ (-1.81)	-0.1414 (-1.22)	-0.2917 (-0.99)	-0.4158 (-1.07)	0.5976 (1.57)	-0.7633 (-0.85)
<i>phase 3</i>	-0.2151+ (-1.85)	-0.1847 (-1.29)	-0.1722 (-0.63)	-0.0024 (-0.01)	0.6970+ (1.67)	-2.1407+ (-1.82)
<i>v</i>		0.1487*** (3.63)		-0.0911 (-0.98)		0.1123 (1.29)
<i>treatment 2</i>		-0.1100 (-0.33)		5.0235*** (4.68)		0.8980 (0.94)
<i>treatment 3</i>		-0.3963 (-1.15)		3.9041*** (4.03)		0.3828 (0.41)
<i>treatment 4</i>		-0.5017 (-0.99)		3.4538*** (3.41)		1.2555 (1.23)
<i>female</i>		0.3828 (1.39)		-0.0497 (-0.08)		0.7901 (1.52)
<i>age</i>		-0.0239 (-1.19)		-0.1304+ (-1.94)		-0.0811 (-1.06)
<i>economist</i>		-0.0286 (-0.08)		0.0653 (0.08)		-0.7007 (-1.31)
<i>ra</i>		0.1067 (1.24)		0.6900*** (3.58)		-0.3257 (-1.63)
<i>su</i>		-0.0753 (-1.58)		-0.4524** (-2.63)		0.1435 (1.08)
<i>svo</i>		1.1044+ (1.93)		-1.6173 (-1.36)		1.4063 (1.54)
<i>trustful</i>		0.0338 (0.34)		-0.0168 (-0.07)		-0.1886 (-1.32)
<i>constant</i>		-0.2342 (-0.10)		4.7767+ (1.73)		6.7190* (2.26)
<i>IMR</i>	0.1643		-0.1066		0.0386	
$\chi^2_{31}(IMR_t)$		54.25**		36.32		64.65***
<i>N</i>	5493	5493	1252	1252	509	509
<i>Subjects</i>	234	234	129	129	138	138
<i>df</i>	7	54	5	50	5	50
<i>Wald test</i> $\chi^2$	22.2**	419.9***	5.3	334.0***	9.2+	197.2***

z statistics in parentheses, bootstrapped standard errors, 200 replications.

+  $p < 0.10$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

## E Structural regressions

Table 14: Structural determinants of the decision to take part in the public project

<i>Dependent variable</i>	(1) POOLED $\partial d/\partial x$	(2) PA $\partial d/\partial x$	(3) FE $d$ (see note 1)	(4) RE $\partial d/\partial x$ (see note 2)
$EV_P - EV_I$	0.0594*** (18.04)	0.0549*** (15.44)	1.0084*** (14.55)	0.0534*** (15.52)
$sC_{it-1}$	0.0472*** (8.46)	0.0337*** (5.94)	0.6043*** (6.83)	0.0326*** (6.91)
$difference_{it-1}$	-0.0028 (-0.62)	-0.0031 (-1.13)	0.0223 (0.56)	0.0008 (0.40)
$d_{it-1} \times d_{jt-1} \times difference_{it-1}$	-0.0093+ (-1.67)	-0.0086* (-2.22)	-0.1814** (-3.28)	-0.0094** (-3.19)
$per$	-0.0049*** (-4.71)	-0.0060*** (-5.20)	-0.0971*** (-4.62)	-0.0051*** (-5.04)
$phase\ 2$	0.0083 (0.65)	0.0157 (1.21)	0.2174 (0.91)	0.0120 (1.07)
$phase\ 3$	-0.0090 (-0.69)	-0.0004 (-0.03)	0.0058 (0.03)	-0.0010 (-0.09)
$v$	-0.0011 (-0.26)	-0.0017 (-0.46)	.	-0.0052+ (-1.77)
$treatment\ 2$	0.1491*** (3.33)	0.1358*** (3.33)	.	0.1928*** (5.42)
$treatment\ 3$	0.1631*** (4.14)	0.1314** (3.08)	.	0.1953*** (5.22)
$treatment\ 4$	0.1950*** (4.28)	0.1548** (3.17)	.	0.2129*** (5.01)
$female$	-0.0280 (-1.16)	-0.0316 (-1.24)	.	-0.0410+ (-1.78)
$age$	0.0020 (0.66)	0.0017 (0.63)	.	0.0010 (0.44)
$economist$	-0.0009 (-0.04)	0.0002 (0.01)	.	-0.0123 (-0.50)
$ra$	-0.0077 (-0.85)	-0.0068 (-0.74)	.	-0.0061 (-0.81)
$su$	-0.0025 (-0.50)	-0.0034 (-0.63)	.	-0.0037 (-0.80)
$svo$	0.0036 (0.08)	-0.0017 (-0.04)	.	-0.0009 (-0.02)
$trust$	-0.0019 (-0.27)	-0.0020 (-0.28)	.	-0.0024 (-0.33)
$N$	7254	7254	3999	7254
$Subjects$	234	234	129	234
$df$	18	18	8	18
$ll$	-1877.4		-996.8	-1446.4
$Mc\ Fadden's\ Pseudo\ R^2$	43.7%			29.1%
$Wald\ test\ \chi^2$	544.6***	276.8***	256.4***	425.6***
$Lagrange\ multiplier\ test$	4.12*	2.86+	0.14	1.10
$Pr(\hat{d} = 1 \mid d = 1)$	95.8%	96.4%	80.2%	96.5%
$Pr(\hat{d} = 0 \mid d = 0)$	61.3%	58.5%	75.6%	58.1%

$z$  statistics in parentheses, bootstrapped standard errors, 200 replications.

+  $p < 0.10$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

Marginal effects are obtained by the delta method.  $\partial d/\partial x$  for factor levels is the discrete change from the base level.<sup>1</sup>In FE, we report parameter estimates for the logit model. FE regressions exclude 105 subjects who chose  $d = 1$  for every period.<sup>2</sup>In RE, marginal effects are computed assuming that individual effects  $u_i$  are equal to 0. The Lagrange multiplier test is obtained by doing a Wald test of zero coefficient for the added regressor  $(x'\hat{\beta})^2$ .

Table 15: Structural determinants of the decision to take part in the public project, population-averaged regressions, by treatment.

<i>Dependent variable</i>	(1) TREATMENT 1 $\partial d/\partial x$	(2) TREATMENT 2 $\partial d/\partial x$	(3) TREATMENT 3 $\partial d/\partial x$	(4) TREATMENT 4 $\partial d/\partial x$
$EV_P - EV_I$	0.0066 (0.96)	0.0208* (2.34)	0.0829*** (10.64)	0.0996*** (10.36)
$sc_{it-1}$	0.0060 (1.54)	0.0275+ (1.84)	0.0474*** (3.38)	0.0543** (2.76)
$difference_{it-1}$	-0.0027 (-0.68)	-0.0264 (-1.40)	0.0025 (0.48)	-0.0038 (-0.70)
$d_{it-1} \times d_{jt-1} \times difference_{it-1}$	0.0019 (0.46)	0.0183 (1.15)	-0.0206* (-1.99)	-0.0226* (-2.14)
$per$	-0.0005 (-0.62)	-0.0003 (-0.13)	-0.0082** (-2.60)	-0.0222*** (-5.84)
$phase\ 2$	0.0108 (1.16)	0.0339+ (1.90)	-0.0127 (-0.38)	0.0721 (1.51)
$phase\ 3$	0.0042 (0.59)	0.0505+ (1.67)	0.0077 (0.24)	-0.0573 (-1.27)
$v$	-0.0022 (-0.71)	-0.0060 (-0.89)	-0.0140 (-1.47)	0.0109 (0.70)
$female$	0.0065 (0.48)	-0.0280 (-0.38)	-0.0611 (-1.08)	-0.0030 (-0.04)
$age$	0.0024 (1.04)	0.0061 (0.62)	-0.0090 (-0.73)	0.0077 (0.58)
$economist$	-0.0067 (-0.64)	-0.0167 (-0.20)	-0.0246 (-0.37)	0.0049 (0.04)
$ra$	-0.0054 (-0.92)	0.0040 (0.22)	0.0030 (0.18)	-0.0412 (-1.16)
$su$	0.0028 (0.77)	-0.0101 (-1.25)	-0.0110 (-1.02)	0.0073 (0.32)
$svo$	0.0111 (0.66)	-0.0525 (-0.54)	0.0752 (0.60)	-0.0586 (-0.30)
$trust$	-0.0012 (-0.42)	-0.0110 (-0.66)	0.0111 (0.66)	-0.0216 (-0.60)
$N$	1984	1984	1829	1457
$Subjects$	64	64	59	47
$df$	15	15	15	15
$Wald\ test\ \chi^2$	45.2***	44.3**	95.1***	89.95***
$Lagrange\ multiplier\ test$	0.01	0.68	0.16	0.01
$Pr(\hat{d} = 1 \mid d = 1)$	100.0%	99.2%	94.7%	82.3%
$Pr(\hat{d} = 0 \mid d = 0)$	0.0%	5.6%	58.1%	76.6%

$z$  statistics in parentheses, bootstrapped standard errors, 200 replications.

Marginal effects are obtained by the delta method.  $\partial d/\partial x$  for factor levels is the discrete change from the base level.

The Lagrange multiplier test is obtained by doing a Wald test of zero coefficient for the added regressor  $(x'\hat{\beta})^2$ .

+  $p < 0.10$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

## **F Instructions for treatment 3, asymmetric payoffs**

Welcome and thank you for your participation! You can earn money in this experiment, how much depends on your decisions and on the decisions of the other participants. It is therefore very important that you read these instructions thoroughly and completely.

Please note that you are not allowed to talk with other participants during the entire experiment. If you have a question, please raise your hand and we will then come to you and answer your question. We ask that you do not express your question(s) aloud before we come to you, and that you ask your question in a low voice so others cannot hear you. We will have to exclude you from the experiment if you violate these rules.

PLEASE ENSURE THAT YOUR MOBILE PHONE IS TURNED OFF NOW!

### **General procedure**

1. The experiment lasts about 110 minutes.
2. There will be three rounds in which you will keep performing the same tasks for a given number of periods.
3. In each of the 3 rounds you will be paired with a different participant in this session (your pair). This person will not have been paired with you or with anyone you were paired with in previous rounds.
4. Each round is divided into periods. The number of periods in each round is determined by a random process, so that the round ends with probability 5% after each period.
5. One period will be selected randomly at the end of the experiment and will determine your earnings. Your earnings will be calculated in ECU (Experimental Currency Units). 1 ECU corresponds to 0.50€. At the end of today's session, your ECU

earnings will be converted into euros and will be paid to you in cash.

6. You will receive 10 ECU (equivalent to 5€) for participating. If you make losses in this experiment, those will be deducted from this payment. However, your total earnings are guaranteed never to be negative.
7. You will be asked to fill out a questionnaire before you receive your payment.
8. You will also be asked to take a few additional decisions unrelated to the main part of the experiment, for which you will be paid.

## **Procedure in each period**

In each period you must make the decision whether to participate in project K or in project G1. Your pair is facing a choice as well whether to participate in project K or in project G2. These projects will be described in the following pages. After this decision, you and your pair determine how much you wish to invest in the project you selected.

### **Suppose you participate in project K and your pair participates in Project K as well:**

If both you and your pair participate in Project K, then the probability of success of project K depends both on the investment of your pair as well as on your investment. You and your pair may invest any sum between 0 and 10 ECU each. The probability the project is successful increases with the sum of your investments according to the following formula:

$$\text{Probability of success} = \frac{\sqrt{(\text{your investment} + \text{investment of your pair})}}{\sqrt{22}}$$

The resulting probabilities can be seen in the table below (investments are shown only for whole numbers in the table, but you can choose investments between whole numbers). For a given investment by yourself (in columns), you can see how the likelihood of success for the project varies as a function of your pair's investment (in rows).

		Your investment										
		0	1	2	3	4	5	6	7	8	9	10
Investment of your pair	0	0%	21%	30%	37%	43%	48%	52%	56%	60%	64%	67%
	1	21%	30%	37%	43%	48%	52%	56%	60%	64%	67%	71%
	2	30%	37%	43%	48%	52%	56%	60%	64%	67%	71%	74%
	3	37%	43%	48%	52%	56%	60%	64%	67%	71%	74%	77%
	4	43%	48%	52%	56%	60%	64%	67%	71%	74%	77%	80%
	5	48%	52%	56%	60%	64%	67%	71%	74%	77%	80%	83%
	6	52%	56%	60%	64%	67%	71%	74%	77%	80%	83%	85%
	7	56%	60%	64%	67%	71%	74%	77%	80%	83%	85%	88%
	8	60%	64%	67%	71%	74%	77%	80%	83%	85%	88%	90%
	9	64%	67%	71%	74%	77%	80%	83%	85%	88%	90%	93%
	10	67%	71%	74%	77%	80%	83%	85%	88%	90%	93%	95%

Each row correspond to a given investment by your pair, each column corresponds to a given investment by yourself. The probability of success of the project for a given combination of investment by you and your pair is shown at the intersection of the corresponding row and column.

Your payment is calculated as follows:

- If project K is successful, then you receive 24 ECU and have to pay your investment. If project K is NOT successful, then you get 0 ECU and still have to pay your investment.
- If project K is successful, then you pair receives 16 ECU and has to pay their investment. If project K is NOT successful, then your pair gets 0 ECU and still has to pay their investment.

Note that your payment in case of success is different from that for your pair!



## **Suppose you participate in project K and your pair participates in Project G2:**

If you participate in project K and your pair participates in project G2, then the probability of success of project K depends only on your investment. You can invest any amount between 0 and 10 ECU.

The probability of success of project K increases with your investment as per the following formula:

$$\text{Probability of success} = \sqrt{\text{your investment}} / \sqrt{22}$$

The resulting probabilities can be seen in the table below:

Your investment										
0	1	2	3	4	5	6	7	8	9	10
0%	21%	30%	37%	43%	48%	52%	56%	60%	64%	67%

The column headings in the table above refers to your investment, the percentages refer to the corresponding probability of success for the project.

Your payment is calculated as follows:

- In case of a success of project K, you receive 24 ECU minus your investment. Otherwise you get 0 ECU and still have to pay your investment.
- In case of a success of project G2, your pair receives 16 ECU minus their investment. Otherwise, your pair gets nothing and still has to pay their investment. Project G2 has the same characteristics as project G1, except your pair receives 16 ECU in case of success and project G2 can only be chosen by your pair. In the same way, your pair cannot choose project G1. Your payment is independent of the success of project G2.

## **Suppose you participate in project G1:**

If you chose project G1, then the success of project G1 depends only on your investment. You can invest any amount between 0 and 10 ECU. The probability of success of project G1 increases with your investment as per the following formula:

$$\text{Probability of success} = \sqrt{\text{your investment}+2} / \sqrt{22}$$

The resulting probabilities can be seen in the table below:

Your investment										
0	1	2	3	4	5	6	7	8	9	10
30%	37%	43%	48%	52%	56%	60%	64%	67%	71%	74%

The column headings in the table above refers to your investment, the percentages refer to the corresponding probabilities of success for the project.

Your payment is calculated as follows:

- If project G1 is successful, you receive 24 ECU minus your investment, otherwise you get nothing and still have to pay your investment.
- The payment for your pair is independent of the success of project G1. It only depends on the success of the project selected by your pair in this period and on his investment in that project.

## Overview of the choices to make

In each period you have to:

- Choose either project K or project G1.
- Then choose how much you want to invest (between 0 ECU and 10 ECU) to the project you selected.

In each period you will be asked:

- How likely it is that your pair chooses project G2. If you guess your pair's decision exactly you receive 5 ECU extra. If your guess is off by  $x\%$  you get  $5-(x/10)$  ECU. If you guess is off by more than 50% then you get nothing from your guess.
- What investment you expect your pair to make in their selected project. If you guess the exact amount invested by your pair,

you receive 5 ECU extra. If your guess is off by  $x$  ECU then you get  $5-x$  ECU. If your guess is off by more than 5 ECU then you get nothing from your guess.

Only one of the guesses (choice of project or investment) will be used to determine your earnings.

## Earnings

Your earnings will be the sum of

1. 10 ECU for participating,
2. PLUS 24 ECU or 0 ECU depending on if your chosen project in the period chosen for payment was successful or not,
3. MINUS your investment in your chosen project in the period chosen for payment,
4. PLUS a payment up to 5 ECU for your guess in either the choice of project or the investment of your pair in their chosen project in the period chosen for payment.
5. PLUS a payment for a few additional decisions unrelated to the main part of the experiment.

## Notes

- You can invest any numbers of ECU between 0 and 10 ECU with a precision up to the second decimal place. You will be given the opportunity to calculate the resulting probabilities of success for your chosen project on your computer.
- You will be provided with information about your project choices and the choices of your pair, and whether your chosen project was successful, for each past period in a given round.
- Reminder: Your payment for a given period is the value of the project you selected in that period (either 24 ECU in case of

success or 0 ECU in case of failure) MINUS your investment in that project.

## **G Additional tasks and questionnaire**

### **G.1 Aversion to risk and strategic uncertainty**

The two following tasks are taken from Heinemann et al. (2009). In the risk task, subjects were presented with ten lottery comparisons, whereby lottery A offered a safe payoff ranging from 1 to 10 ECU and lottery B offered 0 ECU with probability half and 10 ECU with probability half. Measure *ra* of risk aversion is the number of times the subjects chose the safe payoff (lottery A), rather than lottery B. This does not impose the restriction that if A was chosen when it gave  $x$  ECU then it ought to be chosen when it gives any safe payoff  $y > x$ . Imposing such a restriction would make the choices expressed by 28 participants invalid.

In the strategic uncertainty task, subjects were put in groups of four and presented with ten lottery comparisons, whereby lottery A offered a safe payoff ranging from 1 to 10 ECU and lottery B offered 0 ECU if less than 3 group members chose lottery B as well in that choice instance, and 10 ECU if 3 or all group members chose lottery B in that choice instance. Measure *su* of risk aversion is the number of times the subjects chose the safe payoff (lottery A), rather than lottery B. Imposing the restriction that if A was chosen when it gave  $x$  ECU then it ought to be chosen when it gives any safe payoff  $y > x$  would make the choices expressed by 21 participants invalid.

### **G.2 Social value orientation**

Subjects were asked to allocate payoff between themselves and another participants in the session, while they did not know what role (Decider or Recipient) they would be assigned to. There were 6 vectors of allocations to choose from, each with 9 elements, whereby subjects were asked, for each vector, which of the 9 possible allocation

they preferred. Vectors were as follows:

		1	2	3	4	5	6	7	8	9
1	Payoff for me	5.0	5.4	5.9	6.3	6.8	7.2	7.6	8.1	8.5
	Payoff for other	10.0	8.9	7.9	6.8	5.8	4.7	3.6	2.6	1.5
2	Payoff for me	8.5	8.7	8.9	9.1	9.3	9.4	9.6	9.8	10.0
	Payoff for other	1.5	1.9	2.4	2.8	3.3	3.7	4.1	4.6	5.0
3	Payoff for me	5.0	5.4	5.9	6.3	6.8	7.2	7.6	8.1	8.5
	Payoff for other	10.0	9.8	9.6	9.4	9.3	9.1	8.9	8.7	8.5
4	Payoff for me	8.5	8.5	8.5	8.5	8.5	8.5	8.5	8.5	8.5
	Payoff for other	8.5	7.6	6.8	5.9	5.0	4.1	3.3	2.4	1.5
5	Payoff for me	10.0	9.4	8.8	8.1	7.5	6.9	6.3	5.6	5.0
	Payoff for other	5.0	5.6	6.3	6.9	7.5	8.1	8.8	9.4	10.0
6	Payoff for me	10.0	9.8	9.6	9.4	9.3	9.1	8.9	8.7	8.5
	Payoff for other	5.0	5.4	5.9	6.3	6.8	7.2	7.6	8.1	8.5

Choices made by the subjects were summarized in one measure as per Murphy et al. (2011) with

$$svo = \arctan\left(\frac{\text{mean}(\text{payoff for other}-5)}{\text{mean}(\text{payoff for me}-5)}\right)$$

Higher *svo* indicates higher concern for others, up to perfect altruism (maximization of the payoff of the other). Lower *svo* indicates less altruistic behavior, up to perfectly competitive behavior (maximizing the difference between one's payoff and the one by the other). *svo* may takes values between -0.28 and 1.07 given the range of choices provided.

### G.3 Questionnaires on trustfulness

The following questions are taken from the fairness, trust and helpfulness questions in the General Social Survey of the National Opinion Research Center at the University of Chicago.

1. Do you agree with the following three statements:

- (a) In general, one can trust people.
- (b) Nowadays, one can't rely on anybody.
- (c) When dealing with strangers, it's better to be cautious before trusting them.

The possible answers on a four point rating scale are “disagree strongly”, “disagree somewhat”, “agree somewhat”, or “agree strongly”.

2. Would you say that most people...

- (a) would try to take advantage of you if given the opportunity...
- (b) or would try to be fair to you?

3. Would you say that most people...

- (a) try to be helpful...
- (b) or follow only their own interests?

We computed an index of trustfulness from the answer to those questions, using a single-factor measurement model whereby answers to questions 1a, 1b and 1c are modeled as ordered logit and answers to questions 2 and 3 are modeled as logit. The index ranged from  $-3.49$  (least trustful) to  $3.63$  (most trustful).