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Collective conditionality in decontextualized lab and framed field experiments: on the generalizability of incentive mechanisms across contexts.

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Abstract

Individual subsidy payments that are conditional on a collective threshold is reached could provide a viable resolution to the limited and scattered adoption of agri-environmental contracts aiming at attaining environmental quality targets. Hesitations to apply collectively conditional incentives result in a lack of data, especially from the field, concerning their effectiveness. Based on a threshold public good game (TPGG, Croson & Marks, 2000), two treatments are studied. First, an unconditional subsidy is provided for every unit of the initial endowment contributed to the public good (treatment US). Second, a conditional subsidy is provided proportional to individual contributions only if the group threshold is reached (treatment CS). Two experiments are conducted: one examines the standard decontextualized TPGG in a strictly controlled laboratory environment, and one studies a contextualized TPGG by means of a framed field experiment (Harrison & List, 2004) directly applied to the target population that is naturally immersed in the evoked context. In the lab setting, both subsidy schemes elicit higher average cooperation rates than does the control. In the framed field setting, average contribution rates are sustained well above the threshold and, across all periods, are strictly exceeding those observed in the decontextualized lab experiment. Similar to the findings of Stoop, Noussair & van Soest (2011) farmers seem to cooperate even better than do students, apparently independent of the subsidy scheme applied.

Keywords: agri-environmental contracts, public goods, threshold effects, experimental economics, field experiments.

JEL classifications: Q58, C92, C93.

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INTRODUCTION

The attainment of environmental threshold goods (e.g. improvement of ground water quality) imposes collective efforts on geographically defined communities. Traditional agri-environmental schemes (AES) require individual commitment while neglecting the collective threshold inherent in environmental targets as outlined in the European Common Agricultural Policy. Unsurprisingly, limited and scattered adoption of contracts shows inefficient and unsuccessful in attaining environmental quality targets. Conditional subsidies have been largely avoided in common agricultural policy due to missing data, particularly from the field, on the effectiveness of such collective incentives. Clear evidence on the practical viability of this incentive mechanism is therefore needed.

The context of AES for the attainment of environmental threshold goods is transposed into an economic experiment based on a threshold public good game (TPGG, Croson & Marks, 2000). Collective conditionality imposes a contribution threshold at group level upon which the production of the public good depends. In a TPGG, the provision point mechanism is triggered only once the aggregate of individual contributions to the public good attains or exceeds a pre-set minimum threshold value. An initial decontextualized lab experiment provides data from university students in Southern France (Le Coent, Préget & Thoyer, 2014). The lab results show that the subsidy schemes elicit higher contributions to the public good than what is provided in absence of any subsidy. Both subsidy schemes show to be effective in incentivizing the attainment of the threshold, while there is no significant difference in contributions between the two treatments. Using data of all US and CS treatments conducted in the first sequence, the conditional subsidy schemes seems to be more efficient (ratio of average group contribution to average subsidy paid). Therefore, introducing collective conditionality on the payment of subsidies appears a promising tool for incentivizing the adoption of agricultural practices with threshold dependent positive environmental externalities. In order to prevent an overgeneralization (List, 2006) from the lab results to the AES application, this paper investigates what happens when the lab is moved to the field.

Related Literature

Do behavioral principles as discovered in the lab reproduce in the real world? Generalizability on behavior as discovered in economic experiments conducted in the decontextualized lab is defined as the reproduction of behavioral principles in contextualized field settings (List, 2006). In a review of tests on lab-field generalizability, Camerer (2011) stipulates that there is a shortfall in replicated evidence that reliably supports the absence of generalizability from empirical lab findings to field data. It is thus suggested that replication of empirics for closely matched lab-to-field settings should be assumed until this can be disproven with sufficient statistical power. However, depending on the research question, each setting has different advantages and drawbacks, a one-to-one replication of empirical findings from one context to another may not be ultimate goal. Rather, the inference from the data drawn out of any experiment should always be regarded in its specific context, where data from several different contexts are likely to provide a better picture on the larger question than can be obtained from one experimental setting standing on its own (Levitt & List, 2007). A complementary approach together with a stepwise transition from lab to field or vice versa (List, 2006) can provide several benchmarks as to identify which elements to the design produce behavioral change and to which design elements responses show robust across settings.

Contrasting cooperative, competitive and neutral Prisoner's Dilemma settings, the tendency of participants in the neutral setting is to assume a cooperative frame by default (Engel and Rand). That is, the decisions made under the neutral frame resemble those in the cooperative setting, whereas the competitive frame significantly reduces cooperation relative to neutral ($p=0.011$). Moreover, a 'clean' decontextualized setup may not exist (Engel and Rand, 2014). Participants implicitly project their own story onto 'neutrally' framed experiments. Thus, even carefully designed lab abstractions may produce behavioral patterns from projected frames that the experimenter has no control over. Findings on cooperation behavior from lab-field contrasts (as collected in Camerer, 2011) evoking different degrees of context seem to attest quite strong context dependencies for cooperation (cf. Engel and Rand, 2014; Benz and Meier, 2008; Stoop, Noussair and van Soest, 2010). To that extent a comparison between findings from several context settings may be a large advantage as to identify common mechanisms in decision making and enrich context specific inferences (Levitt and List, 2007). Policy recommendations drawn from field settings benefit from improved precision due to inference over multiple contexts and population segments. Vice versa, laboratory designs can benefit from the field analogy as to improve the precision to modelled incentive mechanisms and imposed decision constraints. The challenge clearly lies in the degree of comparability of conditions created in lab and field (Camerer, 2011). While several years of enrollment under an AES contract leave long periods of reflection between yearly decisions, the lab handles 20 decision rounds within 2 hours. Moreover, information asymmetries due to unknown threshold levels, efficient effort levels and real transaction costs and variation over group composition, may provoke dynamics over time that are left unaccounted for in a natural field setting (Dupraz, Latouche & Turpin, 2007) while they can be controlled for in the laboratory. Usually, exogenous dynamics are set as fixed, reducing the real world uncertainty and ambiguity factors to zero. Taking those effects into consideration, it seems only natural that variation across context produces results that are to a certain extent heterogenous due to the variety in control applied over confounding factors across experimental settings.

METHODOLOGY

List (2006) suggests to progress "slowly toward the environment of ultimate interest" in order to learn about the role of each experimental feature in producing the observed behavior. This study first translates the natural setting into the lab and then moves the lab setting back into the field. Hence, the lab serves as a methodological control for the applied treatments by abstracting from naturally-occurring confounds (List, 2006). The lab-in-field then frames the experiment by relying on an artefactual participant sample that is naturally immersed in the evoked context.

Do subsidy schemes impact cooperation in collectively conditional scenarios differently depending on the context? That is, do the empirics from the decontextualized lab experiment reproduce in a contextualized field setting?

Hypotheses

H1: *In a contextualized lab-in-field setting, based on the findings by Stoop, Noussair & van Soest (2010), cooperation is expected to be more pronounced than in the decontextualized lab.*

H2: *Furthermore, the findings from the lab on the effectiveness of the conditional subsidy scheme are expected to be replicated in the field (cf. Camerer, 2011).*

Treatments

The experiment studies two variations of subsidy schemes, an unconditional and a conditional incentive. The interest lies in the variation of contributions between treatments, next to the standard control TPGG.

- NS, the control threshold public good game and thus *No Subsidy*;

- US, this treatment pays an *Unconditional Subsidy* to all contributors proportionally to their contribution independent of the outcome in terms of public good production;

- CS, this treatment pays a *Conditional Subsidy* scheme only if the threshold is reached by the group.

Groups comprise of 4 members. Each group represents a collective of famers whose exploitation touches upon the same ground water catchment area. Each participant i is endowed with 20 units representing an agricultural surface (i.e. 20 hectares) and must decide how many units to contribute (C_i) to a public account (i.e. to contract under the AES) which benefits all members of the group, but only if the collective threshold (i.e. minimum number of enrolled hectares per catchment zone) is reached. The threshold is set at 50% of the group's total endowment ($0.5 \times 4 \times 20$). A linearly increasing production function as in Isaac et al. (1989) is assumed: the public good production function keeps increasing well beyond the provision point. A set of inefficient Nash equilibria are identified for group contributions below the threshold level, the efficient, and pareto dominating, Nash equilibrium is located exactly at threshold level and the pareto optimal holds for full contribution levels which maximizes overall payoff for all players. A relatively low marginal per capita return (MPCR = 0.3) reflects farmer's conventional perception of a rather low individual spillover from an improvement of environmental quality in return to changing their agricultural practices as outlined in the AES. This may become more intuitive when considering that any environmental improvement happens only gradually over the medium to long term. Farmers' investments in changing their agricultural practices, however, are immediate, so that delayed benefits are offset, at least in the short term, by relatively high transaction and investment costs and potentially lower or less profitable yields. For example, existing AES put in place for species protection run over a period of 5 years and require specific crop, rotation of farm land and adapted machinery to be put in place by farmers but show rather unstable effects on the recovery of the species.

No Subsidy (NS)

$$\pi_i^{NS} = \begin{cases} 20 - C_i & if \sum_{i=1}^{4} C_i < 40 \\ 20 - C_i + 0.3 \sum_{i=1}^{4} C_i & if \sum_{i=1}^{4} C_i \geq 40 \end{cases}$$

Unconditional Subsidy (US)

$$\pi_i^{US} = \begin{cases} 20 - 0.7C_i & if \sum_{i=1}^{4} C_i < 40 \\ 20 - 0.7C_i + 0.3 \sum_{i=1}^{4} C_i & if \sum_{i=1}^{4} C_i \geq 40 \end{cases}$$

Conditional Subsidy (CS)

$$\pi_i^{CS} = \begin{cases} 20 - C_i & if \sum_{i=1}^{4} C_i < 40 \\ 20 - 0.7C_i + 0.3 \sum_{i=1}^{4} C_i & if \sum_{i=1}^{4} C_i \geq 40 \end{cases}$$

This design attempts to replicate the real world context of AES in a stylized experimental setting and is summarized in table 1. The control NS represents a situation in absence of AES. The treatment US represents the traditional AES in which farmers receive an individual subsidy proportional to the agricultural surface enrolled in the scheme. The treatment CS represents the collectively conditional subsidy scheme, that grants an individual subsidy proportional to each farmer's agricultural surface enrolled in the scheme only if the aggregate contracted surface by the community meets or exceeds the threshold that is necessary to ensure an improvement in environmental quality, i.e. provision of the public good. It may appear strange at first sight that agricultural surface is represented as payoff relevant currency. Therefore, try thinking of it this way: by contracting hectares of agricultural surface the farmer incurs transaction costs that are exactly the same and additive for each hectare contracted. Now the subsidy payment reduces these transaction costs from 100% to 70% (and *not* the surface contracted). The return from the public goods account is to be referred to as the environmental externality, the positive spillover that each individual perceives from an improvement of environmental quality (e.g. less sick days, improved well-being, consumption of ground water perceived as more safe than before, etc.).

Table 1. Contextual transposition to lab and to field

Context	**Transposition to the laboratory (decontextualized)**	**Transposition to the field (contextualized)**
Threshold environmental public good such as water quality or biodiversity conservation	Generic version of threshold public good	Framed version of threshold public good
Farmers	Student as participants (University of Montpellier, France)	Farmers as participants (Alsace region, France)
Cost related to the adoption of pro-environmental agricultural practices	Contribution to the public good in tokens	Contribution to the public good in hectares
Absence of agri-environmental scheme: voluntary adoption of pro-environmental agricultural practices in light of threshold but without payment prospect	Standard provision point mechanism without individual subsidy prospect (NS).	Voluntary implementation of AES without individual subsidy prospect (NS).
Traditional agri-environmental scheme: payment to each farmer per ha enrolled independent of other farmers' enrollment rates with respect to the collective threshold.	Subsidy proportional to individual contribution: unconditional subsidy scheme (US). The subsidy partially covers the cost of contributions.	Subsidy proportional to individual contribution: unconditional subsidy scheme (US). The subsidy partially covers the cost of implementation of AES.
Agri-environmental scheme with collective conditionality: payment to each farmer per ha enrolled provided the sum of ha enrolled by all farmers is at least equal to the collective threshold	Subsidy, proportional to individual contribution, dependent on collective attainment of threshold at group level: conditional subsidy scheme (CS). The subsidy partially covers the cost of contributions.	Subsidy, proportional to individual contribution, dependent on collective attainment of threshold at group level: conditional subsidy scheme (CS). The subsidy partially covers the cost of implementation of AES.

necessary for ensuring an improvement of environmental quality.		

Procedure

Participants arriving to the sessions were seated individually and asked to restrain from communication throughout the experiment.

First, in order to control for risk preferences on individual contributions in the different treatments, a series of lottery combinations (adapted from Holt and Laury, 2010) is presented. Participants were informed that, at the end of the experiment, one of the games was randomly drawn and its inherent gamble would determine the earnings for this part of the experiment.

Second, groups of 4 participants each were created through random and anonymous composition, which was maintained throughout both of the following parts of the experiment, sequence 1 and sequence 2. Each sequence consisted of a threshold public good game over 10 periods. One treatment was applied per sequence. Each period, two choices had to be indicated per participant: the estimation of the other three group members' contribution (adapted from Fischbacher & Gächter, 2010) and the respective personal contribution to the public good. At the end of each period, feedback on the aggregate group contribution to the public account and on individual payoff was displayed to each participant. At the end of the 10 periods, the earnings for that sequence were displayed as an aggregate over all the periods of that sequence. Participants had been informed that, at the end of the experiment, one of the two sequences would be chosen at random for payment, in addition to the lottery outcome and the participation fee. Corresponding instructions were distributed separately at the beginning of each part (risk elicitation, sequence 1 and sequence 2). Sequence variations among the three treatments were run in all reverse order combinations of the "between-within" setting for the lab sessions and in the most pertinent combinations, i.e. the subsidy treatments, for the field sessions (table 2).

Participants and earnings

For the laboratory experiments, students were recruited in 2013 and 2014 via ORSEE (Greiner, 2004) and all of the experimental sessions were conducted at the Laboratoire d'Economie Expérimentale de Montpellier (LEEM). The sessions lasted a maximum of 2 hours and the average earning was 15.90€ with a standard deviation of 3€. In addition, a show-up fee was provided comprising of 2€ for students of the same campus the experiment was carried out at and of 6€ for students from a different campus.

For the field experiments, farmers were invited via telephone and e-mail to participate in sessions at three different locations in the French region of Alsace, according to a least distance criterion given their postal address. Contact data had been obtained in 2012 from INSEE (Institut national de la statistique et des études économiques) of which around 350 addresses had been contacted. Facilities were provided by the regional agriculture association (Chambre d'Agriculture d'Alsace) and composed of individual desks (one per participant) each supplied with an iPad from the mobile laboratory of the Laboratoire d'Economie Expérimentale de Strasbourg (LEES). The sessions lasted a maximum of 2 hours and the average earning was 16.38€ with a standard deviation of 2.34€ in addition to the show-up fee of 20€.

FINDINGS

Generally, field group averages show higher contributions rates as compared to the lab (Figure 1). While average group contribution rates in the laboratory evolve slightly above the threshold for about the first 2/3 of each sequence, they decline well below threshold in the very last periods of the sequence.

In the field setting, average group contributions are always above threshold (around 70% of endowment), apart from one group in CS-US treatment that is not cooperating effectively and draws down the average for that treatment.

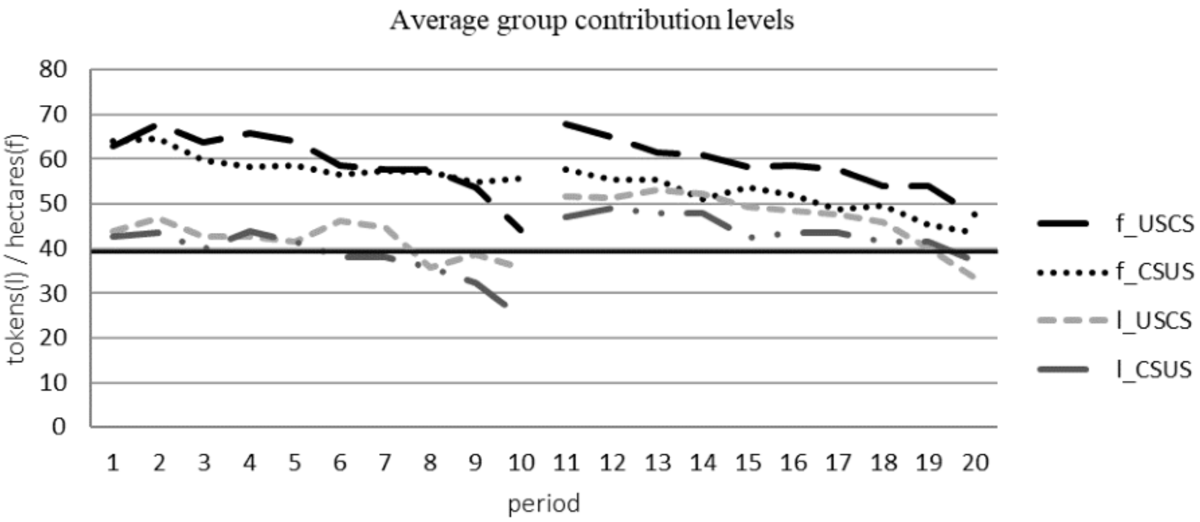

Figure 1. Laboratory (l) and Field (f) experiment average contributions at group level (n=4) per treatment sequence.

Compared to students, farmers indicate more optimistic beliefs over their group members' intentions to cooperate (table 2). The observed difference may be attributed to more perfect stranger matching in the lab. Within either lab or field, however, average reported beliefs over other group members' contributions do not differ significantly between treatments.

Table 2. Average contribution rates and beliefs by treatment in lab and field.

Contribution rates in percent (%) of total endowment

Treatment Sequence	**Average group contribution (%)**		**Average stated belief over others contribution (%)**		**Number of participants**	**Number of groups**
	1	2	1	2	n	N
US-CS	41,84	47,29	32,57	36,74	28	7
CS-US	38,01	44,18	30,89	34,58	32	8
NS-US	25,96	41,12	23,55	32,25	40	10
NS-CS	29,58	53,66	26,16	41,01	40	10
US-NS	41,46	28,42	32,73	22,95	40	10
CS-NS	45,20	18,69	38,14	17,10	40	10
Lab	**37,01**	**38,89**	**30,67**	**30,77**	**220**	**55**
US-CS	59,56	58,53	42,12	41,85	12	3
CS-US	62,37	53,03	46,93	41,73	12	3
NS-CS	71,70	65,30	48,98	49,13	4	1
Field	**64,54**	**58,95**	**46,01**	**44,24**	**28**	**7**

Non-Parametrics

Similar to Stoop et al.'s cooperation contrast between fishermen and students (6) Alsacian farmers seem to be consistently more prosocial than students. Averaging over all groups and periods per treatment of the first sequence (periods 1-10), this different between settings shows highly significant (WMW, $p < 0{,}001$) While in the second sequence the lab-field gap remains pronounced for the US treatment (WMW, $p < 0{,}05$), this difference is not significant any more for the CS treatment groups (periods 11-20). Table 3 summarizes the non-parametric results.

Within the lab setting, average group contributions between the two subsidy treatments show significantly different in the second sequence (WMW, $p<0{,}1$) but not in the first sequence. However, consistently throughout both sequences, the two treatments do show significantly higher contribution rates than do groups in the baseline without subsidy (WMW, $p<0{,}01$).

Within the field setting, average group contributions between the two subsidy treatments show significantly different in the second sequence (WMW, $p<0{,}05$) but not in the first sequence. Based on the preliminary field data, the collectively conditional subsidy shows to be more efficient than the unconditional subsidy, which is congruent to efficiency findings from the lab. Moreover, while in the first sequence (periods 1-10) differences among lab and field group averages are very pronounced (WMW, $p<0{,}01$), the difference for the US treatment groups in the second sequence (periods 11-20) is not significant any more. Thus, this convergence in collaboration behavior over time may be a signal for learning and moderation effects to occur in lab and field alike.

Lab group averages indicate strong free riding in the last periods of each sequence whereas lab-in-field participants (farmers) on average sustain cooperation levels above threshold. The observed difference may be attributed to more perfect stranger matching in the lab.

Table 3a. Non-parametric comparison of average contribution rates at group level by sequence across all treatments in lab and field Sequence 1.

Treatment	Groups	Average group contribution (%) WMW test Sequence 1		
Lab	**N**	**US**	**CS**	**NS**
US	7		--	
CS	8			
NS	10	US>NS***	CS>NS***	
Field				
US	3		--	
CS	3			
Field vs. Lab				
US		f>l***	f>l***	f>l***
CS		f>l***	f>l***	f>l***

Table 3b. Non-parametric comparison of average contribution rates at group level by sequence across all treatments in lab and field Sequence 2.

Treatment	Groups	Average group contribution (%) WMW test Sequence 2		
Lab	**N**	**US**	**CS**	**NS**
US	8		CS>US*	
CS	17			
NS		US>NS***	CS>NS***	
Field				
US	3		CS>US**	
CS	3			
Field vs. Lab				
US		--	--	f>l***
CS		f>l***	f>l***	f>l***

Panel Regression & Average Treatment Effect

In order to arrive at a conclusion of the causal relationship between treatment and results, the average treatment effect (ATE) has to be defined. The ATE is the expected causal effect of the treatment on the outcome for a randomly chosen unit *i* from the relevant population (Ferraro & Miranda, 2014).

Defining μ_0 and μ_1 as the two regression functions for the potential outcomes defined as $\mu_0(x) = E[Y_{it}^0 \mid X_i = x]$ and $\mu_1(x) = E[Y_{it}^1 \mid X_i = x]$, where Y_{it}^0 denotes the control outcome and Y_{it}^1 the treatment outcome, E[.] denotes the expectation operator. Then, by definition, the average treatment effect conditional on observable variables x is given by $\mu_1(x) - \mu_0(x)$ so that: $ATE = E[Y_{it}^1 - Y_{it}^0] = E\left[Y_{it}^1\right] - E[Y_{it}^0]$.

The time subscript is suppressed for X_i as one principal assumption under an estimation of the average treatment effect is that any systematic differences between treated and untreated units are captured only by time-invariant characteristics (Ferraro & Miranda, 2014). Given the dynamic setting of the 10 period TPGG, previous period's contribution and feedback over others' contribution can clearly affect contributions of the subsequent period. Hence, the control variables that are time invariant are used to explain for differences among the treatment groups. By including dummies for the field setting, risk aversion and order effects (i.e. sequence) as controls, these X_i are eliminated as rival explanations in the relationship between the treatment T and the outcome in contribution Y_{it}.

In order to justify OLS regression (Imbens & Wooldridge, 2009) the assumptions of linearity in combination with unconfoundedness must hold. First, the linearity assumption of T on Y given covariates, predicts that the outcome in contributions to the public good is directly related to the treatment applied. In order to assess the pure treatment effect, this assumption is critical. By providing a setting for the field experiment that resembles controlled laboratory conditions, the lab-in-field setting is ideal for isolating the treatment effect from other potentially intervening effects.

Second, the unconfoundedness assumption, requires that conditional on observed covariates there are no unobserved factors that are associated with both the assignment and the potential outcomes. This is controversial: beyond the observed covariates there are no (unobserved) characteristics of the individual associated both with the potential outcomes and the treatment. Unconfoundedness is equivalent to independence of the error term and of the treatment, conditional on covariates. The sample implies a confounding factor that remains stable throughout the time of the experiment: farmers that are present in the same session come from the same region and some of them already knew each other. This factor is assumed to influence their behavior in general but should not be interfering with any of the treatments in particular. Therefore, the geographic proximity of participants is not an underlying cause for inter-period variation so that the treatment effect is not likely to be influenced by this stable condition.

Third, the independence assumption refers to the same underlying distribution that participants in the various treatments are drawn from. It can be described as distribution overlap among the treatment groups: the support of the conditional distribution of X_i, given T = 0 overlaps completely with that of the conditional distribution of X_i, given T = 1. The regression estimator for the ATT assumes that, across each time step, the expected trends of treatment and comparison groups are the same in the absence of the treatment. That is, had there not been treatment variation to the TPGG, all groups in all experimental sessions would be expected to show more or less the same choice pattern on average.

In order to assess the causal effect on a participant assigned to a specific treatment group the average treatment effect on the treated (ATT) is modeled conditional on exposure to treatment (T). The ATT will not equal the ATE if the units exposed to a treatment respond differently to the treatment than the untreated units would respond had they been treated (i.e., heterogeneous responses). Thus, whether a sample selection persists among the farmers has to be verified. Given that each of the three experimental session was conducted in a region geographically distinct from the other two regions, heterogeneity may exist to the extent that crops and thus agricultural practices vary by region. One of the samples had more wine growers, for instance, whereas some participants had experience with organic farming practices. With more data from additional participants it should be possible to control for those individual specific characteristics in the regression analysis. Moreover, this study is conducted on a sample drawn from an artefactual population so that outcomes rest specific to the underlying population characteristics.

Adding a time subscript, t, and by making the additional assumption that treatment effects and time effects are constant, the treatment effect on treated participants is estimated using the ATT, usually defined as: $ATT = E[Y_{it}^1 - Y_{it}^0 \mid\mid T]$. The pairwise combination of treatments from the field experiment does not allow for an extrapolation of expected treatment effects over subjects in a field control group. However, the lab control group remains the benchmark of the regression model for all other treatment variations. Rather, this pairwise combination allows for within-subject comparisons of treatment effects from two distinct treatments. $So\ that\ ATT^1 = E[Y_{it}^1 - Y_{it}^0 \mid\mid T = 2]$ and $ATT^2 = E[Y_{it}^2 - Y_{it}^0 \mid\mid T = 2]$ can be estimated for the same set of i.

Based on the assumptions about the process by which the participants were exposed to the treatment, this model provides an unbiased estimator for the ATT. It should be noted that due to the small sample size, results from the field experiment can be interpreted as initial directions for potential effects of a fully scaled study.

The random effects (RE) estimator yields a weighted average of the between and within estimates. Contrary to the FE model, the time invariant effects are not eliminated and individual-specific effects that are not explicitly controlled for rest in the error term. That is, differences among individuals are considered random (instead if fixed and estimateable) and are accounted for by the individual-specific component of the error term, ϑ_i. For this reason, the RE estimation is chosen over the usually more consistent FE estimator: the time-invariant treatment and field dummies would disappear in an FE estimation. Table 5 summarizes the regression results. However, the RE estimates are consistent only if the individual effect *i* is uncorrelated to the explanatory variables.

Model 1 of the random effects panel regression has individual contributions as its dependent variable. The explanatory variables (summarized in table 4) comprise of previous period's group contribution (first lag, discrete) and the individual's belief over others' contributions in the upcoming period (discrete) as well as several control variables including a field dummy, a sequence dummy, two treatment dummies (keeping the NS treatment as the baseline), a period variable (discrete), and a risk aversion dummy. Error terms account for time and individual specific random effects. Individual observations are clustered by group and the model is specified as follows:

$$\begin{aligned} Contr_{it} = \alpha_0 + \ & \alpha_1 SumC_{i,t-1} + \alpha_2 field_i + \alpha_3 seq_i \\ & + \alpha_4 US_i \ + \ \alpha_5 seq * US_i \\ & + \alpha_6 CS_i \ + \ \alpha_7 seq * CS_i \\ & + \alpha_8 per_{it} \ + \ \alpha_9 seq * per_{it} \\ & + \alpha_{10} belief_{it} \ + \ \alpha_{11} RA_i \ + \ \gamma_i \ + \ \varepsilon_{it} \end{aligned}$$

For model 3 the dependent variable is the sum of group contributions and the estimation is thus performed at group level. Model 2 and model 4 omit the interaction terms.

Regression coefficients show to be rather robust across the four OLS specifications. The lagged group contributions is highly significant and explains for a large part of the variance in each model irrespective of which controls are present. The belief over other group members' contributions in the upcoming period also shows to be an important predictor of individuals' choices for contribution to the group account, i.e. contracting under AES. The treatment dummies show persistently significant although heir coefficients vary slightly depending on covariates included in the estimation. The field dummy shows very significant for individual contributions and not significant anymore for group contributions. This intriguing result is fully attributable to the small sample size of the field experiment, leaving only 6x20 data points for the estimation with group contributions as the dependent variable. The period coefficient being negative and highly significant, which formally attest the decline in contribution levels over periods, in particular for the very last period of each sequence. On their own, the interaction terms do not come out well, however, the combined coefficient shows highly significant in a separate estimation (table 6). The sequence dummy shows to be somewhat significant and considering that, including the interaction terms, *sequence* is represented 4 times in the estimation and is thus attributed a large share of explanatory power. As can already be seen on the graph, treatments in the first sequence produce more pronounced differences in contribution levels between treatments whereas treatments in the second sequence produce generally higher contribution levels within any treatment combination. Moreover, the second sequence CS treatment provokes group contributions that significantly exceed the second sequence US contributions as is confirmed by the pronounced coefficient for the seq*CS interaction term at group level (coef.: 3.4, p<0.05).

Table 4. Interpretation of regression estimators.

Variable	Values	Interpretation
$Contr_{it}$	0:20	C_i
$L.SumC_{it}$	0:80	$\sum_{i=1}^{4} C_{i,t-1}$
$field_i$	0;1	
seq_i	0;1	sequence = 2
US_i	0;1	
CS_i	0;1	
per_{it}	0:10	
$belief_{it}$	0:60	$\sum_{i=1}^{3} C_{i,t+1}$
RA_i	0;1	Risk averse

Table 5. Random Effects Panel Regression. Average contribution rates at individual level clustered by group (1 + 2) and at group level (3 + 4).

	(1)	(2)	(3)	(4)
	Contribution	Contribution	SumC	SumC
L.SumC	0.086***	0.086***	0.693***	0.691***
	(.0082)	(.0083)	(.0225)	()
field	0.823*	0.903**	1.505	1.628
	(.3285)	(.3382)	(.9504)	()
CS	1.365***	1.436***	1.937*	3.671***
	(.4081)	(.2712)	(.9755)	()
US	1.443***	1.287***	3.482***	3.905***
	(.3883)	(.2087)	(.9866)	()
sequence	0.479	0.364*	2.565	1.230*
	(.5232)	(.1665)	(1.4813)	()
period10	-0.176***	-0.182***	-0.588***	-0.860***
	(.0426)	(.0279)	(.1514)	()
sequence*CS	0.146		3.408**	
	(.5164)		(1.3169)	
sequence*US	-0.311		0.823	
	(.6560)		(1.3277)	
sequence*period	-0.0103		-0.477*	
	(.0505)		(.1984)	
RA	-0.517	-0.5069	-0.478	
	(.4470)	(.4483)	(.7039)	
belief	0.153***	0.153***	0.291***	0.294***
	(.0103)	(.0104)	(.0279)	()
_cons	1.736**	1.383***	3.488*	3.967***
	(.5888)	(0.4694)	(1.3584)	()
N groups	244	244	61	61
N obs	4636	4636	1159	1159
Obs per group	19	19	19	19
R^2	0.6152	0.6147	0.8666	0.7235

Robust standard errors in parentheses

Signivicance levels: * p<0.05, ** p<0.01, *** p<0.001

For cases figuring in the first sequence, interaction terms as well as the sequence dummy take the value of zero, reducing the model to:

for ***sequence = 0***

$$
\begin{aligned}
Contr_{it} = \alpha_0 + \alpha_1 SumC_{i,t-1} + \alpha_2 field_i \\
+ \alpha_4 US_i \\
+ \alpha_6 CS_i \\
+ \alpha_8 per_{it} \\
+ \alpha_{10} belief_{it} + \alpha_{11} RA_i + \gamma_i + \varepsilon_{it}
\end{aligned}
$$

For cases figuring in the second sequence, interaction terms as well as the sequence dummy take the value of one, which allows for collapsing the joint coefficients to:

for ***sequence = 1***

$$
\begin{aligned}
Contr_{it} = (\alpha_0 + \alpha_3) + \alpha_1 SumC_{i,t-1} + \alpha_2 field_i \\
+ (\alpha_4 + \alpha_5) US_i \\
+ (\alpha_6 + \alpha_7) CS_i \\
+ (\alpha_8 + \alpha_9) per_{it} \\
+ \alpha_{10} belief_{it} + \alpha_{11} RA_i + \gamma_i + \varepsilon_{it}
\end{aligned}
$$

In order to identify the effect of *sequence=1* on variables figuring in the interaction term *seq**, combined coefficients are estimated for model 1 and 3 using Stata's *lincom* command. Combined coefficients for the attest quite strong order effects with higher average contributions for treatments in the second sequence and stronger decline in contributions over time as compared to the first sequence.

Table 6. Combined coefficients for sequence = 1 (Model 1 + 3).

Model	combined estimator	coefficient	se
(1)	US + seqUS	1.133***	.3905
	CS + seqCS	1.511***	.3344
	period10 + seqPer	-.186***	.0332
(3)	US + seqUS	4.306***	.9294
	CS + seqCS	5.345***	.9613
	period10 + seqPer	-1.065***	.1289

Limitations and further research

One control sequence was run in the field (NS-CS), where no subsidy at all elicits yet almost full cooperation rates (around 90%). It is, however, to be noted that the field control relies on data from one group only (n=4), and that this group showed to be extremely cooperative. More data is to be collected to augment validity and meaning inferred from the field, in particular for the control group.

Alsacian farmers contribute more than do students in the lab. However, with the current experimental setup (i.e. in absence of controlling for environmental preferences) it is difficult to say what their underlying motivation is. In particular, we would be interested in disentangling between two motives:

1. Empathy: Farmers of the same region seem to identify closer with the economic situation of their group members.
2. Environmental: Farmers have stronger relation with nature and thus are more sensitive to the environmental impact of their choices.

Perhaps, the behavior is due to a combination of both "empathy" and "environmental" motives. Nevertheless, a better understanding of the underlying preferences for contributions to a threshold public good that generates positive environmental externalities would be helpful for designing real world interventions. It is clearly noteworthy for the interpretation of our results and the related discussion on generalizability that environmental externalities, in the experimental setting, are represented in form of monetary stakes. Natural field experiments on conditional subsidy schemes would be useful to investigate behavior facing real environmental stakes.

So far, two variations were implemented when moving from lab to field: the artefactual sample and the MAE context. Similar to Stoop et al. a further experiment that provides a decontextualized setting to the artefactual sample would be necessary in order to control for sample and context effects.

CONCLUSION

In the lab, a positive impact of subsidy schemes on facilitating cooperation has been attested. Moreover, the conditional subsidy scheme seems to incentivize higher contribution rates, at least in the second sequence, than do the US and NS treatments.

In the field, we observe contributions that are consistently higher than those of the analogue lab treatment groups and consistently above threshold limit, yielding support for hypothesis 1. The field results also seem to point in favor of the collectively conditional subsidy scheme, yielding support for hypothesis 2. Farmers of the same region are likely to identify closer with the economic situation of their group members and with the environmental impact of their choices.

More field data is needed to draw better inferences (Camerer, 2011; Levitt & List, 2007) upon (1) the role of subsidy mechanisms in facilitating cooperation in an environmentally motivated context and (2) farmers' preferences in order to verify and disentangle prosocial and environmental motives. Moreover, the analysis is to be augmented with natural field data on the artefactual participants stemming from a regional AES implementation.

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